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Sharing Suppliers and Information Spillovers:

The Case of the Auditor

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by

Daniel Aobdia

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ABSTRACT OF THE DISSERTATION

Sharing Suppliers and Information Spillovers:

The Case of the Auditor

by

Daniel Aobdia

Doctor of Philosophy in Management

University of California, Los Angeles, 2012

Professor John Hughes, Chair

This dissertation provides empirical evidence consistent with auditors transferring some information from one client to another and, as a consequence, with same industry rivals being reluctant to share the same auditor due to information spillover concerns.

In the first essay, using exogenous shocks to the auditing industry, including large auditor mergers and the collapse of Arthur Andersen, I document a reluctance of rivals to engage the same auditor due to information spillover concerns. This reluctance is more evident in concentrated industries where barriers to mobility, proxied by differentiation and capital expenditure levels, are low. More secretive manufacturing firms are also more reluctant to share their auditor with a rival. I also find weak evidence that the concern for information spillovers is

lessened when rivals are dissimilar in terms of sales or when they are headquartered in the same state, where other conduits for information spillover, including employee turnover, are present. Last, I find some evidence that auditors extract rents from clients concerned about information spillovers.

In the second essay, I present evidence consistent with auditors being a conduit for information spillovers. My results indicate that firms sharing the same auditor are more alike in their investment, research and development, advertising and SG&A decisions compared to firms not sharing the same auditor. Causality is established using the collapse of Arthur Andersen as a natural experiment that broke channels for information spillovers for former clients of the auditing firm. Results are still valid after the enactment of Sarbanes-Oxley (SOX), indicating that the core auditing practice of auditors is a conduit for information spillovers. I also find some evidence that information spillovers are more prominent at the auditor office level. Last, additional evidence from patent citations indicates that information spillovers through sharing the same auditor may lead to dissemination of technological innovations among client firms.

The dissertation of Daniel Aobdia is approved.

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Brett Trueman

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2012

DEDICATION

I dedicate this dissertation to my father, Edgard Aobdia, and to my grandmother, Irene Geftman. Both nurtured my interest in academic studies yet passed away before the completion of this work.

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I. PROPRIETARY INFORMATION SPILLOVERS AND AUDITOR CHOICE

I.1. Introduction

A firm with a supplier who also services rival firms runs the risk of information spillovers. Providing suppliers with access to proprietary information is often necessary for suppliers to fulfill their role. Either by design or inadvertently the supplier may become a conduit through which a rival sharing the same supplier may obtain that information. Concerns about the safety of proprietary information could be sufficient to affect supplier choice decisions. A well-known case was the reluctance of Bell Operating Companies to continue with AT&T as an equipment supplier once deregulation allowed AT&T to become a direct competitor (Hughes and Kao 2001). Semiconductor materials and equipment manufacturers were reputed to be reluctant to share information with members of the Semiconductor Manufacturing Technology Consortium (SEMATECH) fearing revelation of proprietary information to competitors (Grindley et al. 1994). The merger of Ernst & Whinney, Coca-Cola's auditor, with Arthur Young, PepsiCo's auditor, led the combined Ernst & Young to resign from the PepsiCo's account (WSJ, February 26 1990). In anticipation of another merger of auditors, Frederick Zuckerman, Chrysler's treasurer, remarked: *"It'd be very awkward to have the same auditor for two large firms […] Clients may feel uncomfortable knowing that their corporate secrets are lying just a* *few files away from papers of their arch rivals"* (Reuters, July 10 1989)[1]. However, formal empirical research on the reluctance of rival firms to share suppliers due to concerns about information spillovers beyond such anecdotes has been slow to emerge, possibly because information spillovers usually do not leave any paper trails (Krugman 1991b).

In this study, I examine the reluctance of same-industry rival firms to share auditors. The audit setting is particularly suitable to my study given that all publicly traded firms are required to be audited. Auditors have access to a wide range of proprietary information to assess the accuracy of their clients' financial reports. In addition, the number of auditors able to service major industry competitors is very limited, making it likely that concerns for spillovers would be considered when making auditor choice decisions. The recent historical evolution of the auditing industry also provides natural experiments that allow me to infer causality. Specifically, I exploit the collapse of Arthur Andersen, at the time one of the Big Five public accounting firms, and the auditing industry consolidation trend over the past 25 years with three large audit firm mergers to infer causality should these events prompt auditor switching decisions consistent with an objective of avoiding information spillovers.

Contributing to a tension in auditor choice decisions is the prospect that economies of industry specialization by auditors, as documented in previous studies, may outweigh concerns of such spillovers. I use this tension to advantage in cross-sectional analyses. In particular, other features of my study include consideration of other conduits for information spillovers such as employee turnover, cross-sectional variation in predisposition of manufacturing clients toward

[1] Additional anecdotal evidence can usually be found at times of auditor mergers. See for example the Asian Wall Street Journal, March 20 2002, The Wall Street Journal Europe, October 20 1997, and The Economist, December 13 1997 for international evidence on the topic

secrecy, potential barriers to imitation within the industry[2] and rent extraction by auditors from clients concerned about spillovers in reaction to a forced change of auditors.

Theoretical studies of negative externalities in the form of information spillovers from sharing a common supplier include Demski, Lewis, Yao, and Yildirim (1999), Hughes and Kao (2001), Baccara (2007), and Bönte and Wiethaus (2008). Demski, et al. consider client information leakage across divisions of a professional firm and incentive mechanisms for efficiently dealing with such spillovers. Hughes and Kao consider the alternative of a vertically integrated firm, facing downstream competition, to spin off an upstream supplier in order to preclude transmission of rival's proprietary information and preserve upstream sales to those rivals. In Baccara's model, a firm chooses between outsourcing production, thereby exposing it to leakage to competitors, and in-house production at the loss of some efficiency. Bönte and Wiethaus consider a choice between increasing the efficiency of a supplier by providing technical knowledge and risking transmission of that knowledge to competitors serviced by the same supplier. The ubiquitous aspect in each case is a tension between information spillovers from sharing a common supplier with rival firms and experiencing some form of inefficiency. Such a tension is clearly present in an auditing setting where the loss of gains to industry specialization constitutes the potential source of inefficiency.

Indeed, a large portion of the theoretical and empirical auditing literature confirms the prospect that gains to industry specialization by auditors is a significant factor in auditor choice decisions. Signaling models by Titman and Trueman (1986) and Datar, Feltham and Hughes (1991) depict the relevance of audit quality to value in initial public offerings; predictions tested

[2] Barriers to imitation are also widely called mobility barriers within the Industrial Organization literature. They correspond to the ease or difficulty of changing positions from one strategic group to another within the same industry

empirically by Balvers, McDonald and Miller (1988), Beatty (1989), and Feltham, Hughes and Simunic (1991). Other studies including Lim and Tan (2007) and Krishnan (2003) find that the use of industry specialists is associated with higher audit quality. Relevant to the leverage that industry specialization might have on audit fees, Craswell, Francis, and Taylor (1995) document that industry specialist auditors charge significant premiums in the Australian market.

Additional considerations that could influence this tension within the cross-section of industries include differences in the intensity of product market competition and in the nature of the knowledge transmitted. Caves and Porter (1977) and Porter (1979) introduce the concept of strategic groups within given industries and the concept of barriers to mobility for firms to compete from one strategic group to another. Peteraf (1993) empirically confirms for the airline industry that rivalry is greater across strategic groups than within groups. These papers indicate that the costs of sharing the same auditor can be reduced when barriers to mobility are high. Notwithstanding product market competition considerations, the tension also depends on the appropriability of the knowledge potentially transmitted. In particular, innovative knowledge often cannot be protected only with property rights (Rajan and Zingales 2001). Levin, Klevorick, Nelson and Winter (1987), Cohen, Nelson and Walsh (2000) and Cohen (2010) explore different appropriation mechanisms besides patent protection in the context of product and process innovations and document that secrecy matters for many manufacturing firms, with large inter-industry variations.

My results provide consistent evidence that in general the top three rivals by sales in each industry are reluctant to share the same auditor. The reluctance is enhanced by the absence of barriers to mobility, the absence of other conduits for information spillovers, and greater concern for secrecy by manufacturing firms. Regarding the collapse of Arthur Andersen, the evidence indicates that when forced to choose a new auditor, the top three rivals were less than half as likely to do so relative to other firms in the same industry. Similarly, my results in the other experiment involving mergers indicate that top rivals switched auditors following mergers placing them with the same auditor almost twice as frequently as a control group where no top rival was brought in by the merger. I also find evidence that auditors were able to charge higher fees after the collapse of Arthur Andersen in industries where the supply of larger audit firms was tight beforehand[3].

Several policy implications follow from these results. The ability of audit firms to extract greater rents following Arthur Andersen's collapse suggests that the short supply of major audit firms provides those firms with hold-up power that might influence whether regulators would allow them to fail should further scandals arise. In turn, this influence, combined with a potentially captive client base, might lead to a moral hazard that lowers audit quality. The present limited supply of major audit firms also tends to undermine the recent intent of the Public Company Oversight Board in exploring mandatory rotation (see PCAOB release 2011-006 in August 2011) since this may induce greater exposure of firms to information spillovers that would otherwise occur with less frequency. Such a change in the tension between information spillovers and industry specialization could result in rivals sharing the same audit firm seeking to restrict auditor access to proprietary information, thereby resulting in lower audit quality in servicing the public interest. It also would strengthen the bargaining power of audit firms, again, contributing to audit firms' hold-up power.

[3] I cannot use auditor mergers in the fee analysis because all auditor mergers took place before 1999 whereas companies began reporting the amount of auditing fees paid only after 2000.

My results complement Asker and Ljungqvist (2010), who provide empirical evidence that rival firms display reluctance to share the same underwriter. Similar to the roles of Arthur Andersen's collapse and major firm mergers in my research design, Asker and Ljungqvist rely on reactions to exogenous shocks to the securities underwriting industry to infer causality in documenting the reluctance of rivals to share underwriters. The underwriting industry is similar to the auditing industry in the nature of the due diligence that they each perform. They differ in the one-shot nature of underwriting compared to the repeated nature of audits and auditor relations, and in a broader access to proprietary information by the auditor. Consequently the auditing relationship arguably makes for a greater sensitivity to information spillovers[4].

Closer to the context of my study, Kwon (1996), following-up on an idea originally formulated by Danos and Eichenseher (1982) finds a negative association between the level of client industry concentration and a measure of the level of auditor dominance within an industry, consistent with firms in concentrated industries being reluctant to share the same auditor[5]. Distinctive features of my study include the use of the natural experiments related to Arthur Andersen's collapse and mergers among large firms to infer causality, indicants of barriers to mobility that might lessen information spillover concerns, and a survey of firms relying on secrecy as a measure of sensitivity to competitors gaining access to proprietary information on

[4] Additional distinctive features of my study compared to Asker and Ljungqvist include an assessment in auditor choice of the role of product market competition and of the appropriability of the knowledge transferred. In particular, I directly tie concerns for information spillovers with auditor choice by finding that manufacturing firms in more secretive industries are more reluctant to share the same auditor

[5] Kwon's results are subject to two important caveats. First, the number of firms within each industry could be an omitted variable as it is related to both industry concentration ratio and the assumed proxies for auditor dominance. Second, as indicated in section 3, it might be harder to detect true direct rivals within industries with more companies, usually less concentrated, than within industries with fewer companies, usually more concentrated, when looking at the entire industry distribution. I abstract from these concerns by looking at the top three competitors within each industry.

innovations, which allows me to directly tie rivals' reluctance to share the same auditor to information spillover concerns.

The remainder of this paper is structured as follows. Section 2 introduces the main motivation and hypothesis development. Section 3 develops the data and proxy variables construction. Main empirical results are presented in Section 4. I also present an analysis of audit pricing in Section 5. Section 6 concludes.

I.2. Motivation and hypothesis development

Auditors have access to a wide range of proprietary financial information from their clients, implying that firms should be concerned about their auditors' other clientele. Anecdotal evidence indicates that auditors have directly transferred some information from one client to another in the past. In a salient example, Grant Alexander & Company, the precursor of Grant Thornton, transferred some information related to the operations of one client to other clients using the services of the former client (see Werner 2009 and Aobdia 2011b for additional details). In addition, Andersen Consulting, the consulting arm of Arthur Andersen at the time, was suspected of transferring proprietary information from Yamaha to Harley Davidson (O'Shea and Madigan 1998). Industry specialization could also become another indirect channel for information spillovers because auditors routinely apply some of the knowledge learnt at one client to another one (McAllister and Cripe, 2008). Aobdia (2011b) also provides empirical evidence consistent with the auditor being a conduit for information spillovers. In particular, his results suggest that information spillovers are not limited to client firms sharing the same auditor office but can also occur when two clients share the same auditor but not the same office.

However, prior auditing literature has also documented the benefits of industry specialization in terms of audit quality with industry specialist auditors being able to charge significant premiums[6]. In addition, given the repeated game nature of the auditing client relationship, evidenced by low auditor switching rates, it is possible that auditors have taken appropriate action in order to limit the risks of information spillovers, at least for their clients who are concerned about them. Last, some industries might include firms interested in sharing the same auditor as their rivals in order to benefit from information spillovers.

Therefore, it remains an empirical question to detect companies' reluctance to share their auditor with competitors due to information spillovers reasons. I state my first hypothesis as follows:

H1: controlling for the need of industry specialization, same industry rivals are more likely to choose different auditors

During the course of a typical audit engagement, auditors have access to detailed company financial information. This information can include terms of trade to main customers in order to verify accounts receivables, suppliers' terms in order to verify accounts payable, product or product line profit and loss information, country-level profit and loss information and plant level cost information. Auditors also conduct plant visits, for example to verify inventory levels, and can have access to plant specific information. In particular, auditing of raw materials and work-in-process inventories, usually located at the heart of the plant, might put auditors in

[6] Note that a higher aggregate fee premium is also consistent with a spillover related explanation where auditors price their spillover capabilities. For example, Francis et al. (2005) document that audit fees are higher for nationally top ranked auditors only when they are also the city-leader. Their result is consistent with auditors pricing their office-level spillover capabilities, consistent with the theoretical predictions of Baccara (2007). Overall, it is unclear, notwithstanding the pricing of information spillovers, whether aggregate audit fees should be higher for expert auditors compared to non-expert auditors given that expertise can translate into a higher hourly billing rate, but overall into a decreased number of hours for the auditor to complete the audit.

contact with potentially sensitive factory process information. During the course of their conversations with company management, auditors might come in contact with additional information related to company strategy, including merger and acquisition plans, product development or marketing plans. Overall, the information acquired is much more detailed than publicly disclosed company financial statements and could be used by a rival to advantage in product market competition. Accordingly, the rivals most able to make good use of this information should be relatively similar to the company. In the empirical tests I define similarity using industry concentration[7] and comparability of rivals' sales. Consequently, I state the first part of the second hypothesis as follows:

H2a: More similar rivals are more likely to choose different auditors

In addition, the information should be less valuable when the rival cannot quickly imitate the product or customer strategy of the company. In particular, barriers to mobility can influence auditor choice, as they may prevent the information transferred by the auditor from one rival in one strategic group to be used by another rival in another strategic group. This indicates that rivals may be less reluctant to share the same auditor when barriers to mobility are high. Barriers to mobility include industry differentiation and capital expenditures (Saloner et al. 2001, Gilbert 1989, Caves and Porter 1977, Sutton 1991). Capital expenditures can act as a barrier to mobility because large capital expenditures need to be planned in advance and take time and effort to replicate. Consequently, I state the second part of the hypothesis as follows:

H2b: Same industry rivals are more likely to choose different auditors when industry differentiation is low or capital expenditure requirements are low

[7] Note that a result where same industry rivals are more likely to choose different auditors in more concentrated industries would also be consistent with firms in concentrated industries choosing different suppliers to avoid the appearance of impropriety regarding antitrust related matters

Next, given the extent of auditors access to their clients' proprietary information, companies that use secrecy as a mechanism to appropriate the profit from their product and process innovations should be more concerned about sharing their auditor with their rivals. In particular, Cohen et al. (2000) and Cohen (2010) outline the importance of secrecy for manufacturing firms to protect the profit from their own product or process innovations and document large inter-industry variations in the degree of protections afforded by patents and secrecy. I state the third part of the second hypothesis as follows:

> H2c: Same industry rivals are more likely to choose different auditors when secrecy as a means to appropriate the profits from product and process innovations is important within the industry

There are other conduits for information spillovers besides a shared auditor. Examples of information spillovers in general include Mattel's litigation with MGA regarding ownership of the Bratz dolls (WSJ, Jan 12 2011); the inventor of the dolls developed the concept while working at Mattel before moving to MGA. Rajan and Zingales (2001) mention that Fairchild semiconductor management left the firm with proprietary information about the microprocessor in order to found Intel corp. Renault recently wrongfully laid off three key employees because the company believed that they had sold economic information about its electric car program[8] (WSJ, March 19 2011). These examples illustrate that firm employees in contact with firm's proprietary information can act as a conduit for information spillovers. In both cases of Mattel with MGA and Fairchild with Intel, the companies were headquartered in the same area. In particular, two companies sharing the same geographic location have access to the same labor

[8] The managers turned out not to have sold any information. However, this incident documents that companies are concerned about the potential spillover of information to competitors, including information of a pure financial nature

pool and to the same intermediate goods suppliers (Jaffe et al. 1993). This leads to increased probability of information spillovers compared to two companies not sharing the same geographic location. Alcacer and Chung (2007) provide evidence that firms seeking information spillovers locate in the same areas as their rivals, while firms avoiding information spillovers locate in different areas. Consequently, two firms sharing the same geographic location should be less concerned about information spillovers coming from a shared auditor as many alternative information spillover conduits are available[9]. I state my third hypothesis as follows:

> H3: Rivals headquartered in the same location are more likely to have a shared auditor than rivals headquartered in different locations

Last, auditors should be able to extract rents in industries where rivals are reluctant to share the same auditor due to information spillover concerns. The situation should be noticeable in periods of tightening auditing supply, including at the time of the collapse of Arthur Andersen. In particular, rivals in industries avoiding sharing the same auditor where supply was already tight prior to the Arthur Andersen collapse should have experienced a higher increase in fees after the collapse than rivals in industries where there was slack in auditing supply or in industries with lowered concerns for information spillover. I state my fourth hypothesis as follows:

> H4: Audit fees increased more after the collapse of Arthur Andersen in industries where the concern for information spillover was higher and auditing supply was scarcer

[9] Note that a mitigating factor to this hypothesis is that the probability of information spillovers could be increased when two firms are covered by the same auditor office in comparison to two firms being covered by two different offices of the same auditor. This premise is empirically confirmed by Aobdia (2011b). Consequently two firms that are located in the same area due to historical reasons but that are still concerned about information spillovers would tend to choose a different auditor more often than two firms located in different areas. This mitigating factor could explain the relatively weak results I find regarding shared states and probability of rivals sharing the same auditor

I.3. Data Construction

I.*3.1 Defining Rivals*

I obtain auditor and industry information from Compustat, having eliminated firms listed on Canadian exchanges (I keep firms where Compustat currency code is in USD), ADRs (adr ratio empty in Compustat), subsidiaries (stko variable equal to 1 or 2 in Compustat) and companies headquartered outside of the United States (using the loc variable in Compustat)[10]. The aim is to identify companies that have the highest probability of being true product market rivals, where information spillovers through a shared auditor are possible, and where auditor choices are independent. For example, a parent is not a competitor to its subsidiary operating in the same industry. The parent-subsidiary choice of auditor is also unlikely to be independent. The Compustat data spans the period from 1985, first year where the NAICS codes become widely available in the database, to 2009.

Rivals are then defined as the three firms with highest total revenues within a given NAICS six digit code, as specified by the Compustat primary code. The choice of NAICS codes over several alternatives, including SIC codes is driven by several considerations. First, SIC codes have been replaced with NAICS codes starting from 1987. This means that more recent industries are poorly depicted by SIC codes. Also, Krishnan and Press (2003) document that NAICS codes lead to more cohesive industries than SIC codes. Last, historical SIC codes are not widely available in the Compustat database prior to 1987. The use of GIC codes, despite its potential superiority (Bhojraj et al. 2003) is problematic for this study as GIC data is not

[10] I kept companies headquartered in jurisdictions listed as tax havens as per the OECD definition (GAO report December 2008). In any case the results are not sensitive to inclusion of firms headquartered in foreign countries in the sample.

available prior to 1994 in Compustat. However, two of the three large audit mergers occurred prior to 1990 (both mergers of Ernst and Young and of Deloitte and Touche took place in 1989). The main advantage of using the most granular level of NAICS codes as compared to SIC codes or less granular levels of NAICS codes is that the probability of identifying true product market competitors is increased. For example, the code 311930 only includes Coke and Pepsi as primary competitors. The drawback of using higher levels of granularity is the exclusion of several industries in some of the tests where the number of competitors is limited and where a control group within the same industry code is required[11].

I focus on the top players, defined by their sales level[12], within each industry code in order to maximize the power of my tests. This focus is comparable to the one in Asker and Ljunqvist (2010) and is due to several reasons. First, due to their larger sizes, top industry players are more likely to have overlapping business lines, customers and geographies, making them true product market rivals. For example, the top three competitors in the NAICS code 721120 (gaming) for 2009 include Las Vegas Sands, MGM Resorts and Caesar Entertainment. A quick analysis of these companies' 10-Ks shows that they operate in the same cities with competing casinos in their main markets of Las Vegas, Atlantic City and Macau, and target the same customer base. On the other hand, non-top three competitors include Trump Entertainment, a company only with properties in Atlantic City, NJ, and Monarch, a casino located in Reno, NV. These two casinos cannot be considered direct product market rivals.

[11] Results are robust to using top two competitors instead of top three competitors and to the use of different industry classifications, including NAICS at the four digit code level and SIC at the four digit code level, when enough cross-sectional variation is available for the tests

[12] I deem sales level to be more appropriate than other measures of size, including assets and market size, as I want to focus on current size and do not want to introduce in the proxy other confounding considerations such as the level of supply chain integration or future market opportunities.

Second, top industry players are also more likely to be concerned about sharing financial proprietary information with competitors given that they are likely to be more complex than smaller competitors, with presence in different markets, geographies and customer types, and might have more information to hide as a result[13]. Last, only large firms, given the amount of auditing fees involved, can put some pressure on their auditor so that the auditor does not get hired by a rival[14].

I.3.2 Empirical proxies

Several empirical proxies are used in order to test the above hypotheses. Proxies for rivals' similarity (H2a) include:

Herfindahl, the industry Herfindahl index, calculated at the NAICS six digit code. I posit that more concentrated industries are more likely to have better defined rivals.

CCR3, the top three firms concentration ratio, defined as the combined sales of the top three players divided by the total sales within the industry. I use *CCR3* as an alternative to *Herfindahl* for the industry concentration ratio, as the Herfindahl index tends to be highly correlated with other explanatory variables. Finding consistent results using *CCR3* as an alternative variable would decrease concerns about multicollinearity.

Deltasale, which measures whether the top three competitors within a given industry are similar in sales or not. I calculate *Deltasale* as the standard deviation of sales among the top three

[13] Given the wide flexibility granted to firms in terms of segment reporting and the possibility to alter segment reporting to the firm's benefit (Fields et al. 2001), more complex firms have increased opportunities to conceal their profits areas when reporting their financial statements compared to simpler firms.

[14] It is interesting to note that some firms may actually have an interest in sharing the same auditor with their rivals in order to benefit from information spillovers, indicating a potential asymmetry of interests across rivals. Focusing on the top 3 players removes this constraint as larger firms are more likely to be leaders in their industry who can put sufficient pressure on their auditors so that the auditors do not get hired by a rival.

players within an industry, normalized by the average sales of these three companies. A lower value of *Deltasale* indicates more similarity of the top three players sizes while a higher value indicates more heterogeneity in the top three players sizes.

Proxies for industry differentiation and capital expenditures (H2b) include:

Logindcapex, defined as the log of the weighted average of capital expenditures in an industry, weighted by each company market share defined by sales, as in Li (2010). I take the log to reduce the skew in the variable. The variable measures typical capital expenditures needed in the industry and proxies for the ease of imitation within the industry.

Indpricecostmargin, defined as in Karuna (2007) and Li (2010). The variable is equal to industry aggregate sales divided by industry aggregate operating costs and is a measure of industry differentiation, with higher values indicating higher industry differentiation and lower values indicating higher substitutability.

The major proxy for the importance placed on secrecy (H2c) is *Secrecy Mfg*. The variable is equal to the average of the secrecy variables for product innovations and for process innovations taken from the Carnegie Mellon survey results as reported by Cohen et al. (2000) in Table 1 and Table 2. Cohen et al. surveyed in 1994[15] 1,478 manufacturing firms conducting research and development and reported results for 34 sub-industry groups within the manufacturing group. I match these sub-industry groups with the NAICS codes and attribute the results of the survey at the sub-industry group level to each matched NAICS code. *Secrecy Mfg*

[15] There might be a potential concern that a single data point in 1994 is not representative enough for the overall sample which spans over 25 years. However, secrecy is likely to be relatively sticky over time. In addition, there is no reason to believe that there would be any bias caused by the use of this variable. At worst there could be too much noise in the estimation of the variable, in which case results would become statistically insignificant. Overall, results are unchanged when excluding this variable from the empirical specifications, as evidenced by columns two and three of Table 3. In addition, the Arthur Andersen collapse test in Table 5 provides direction for causality for this variable.

measures the mean percentage of product and process innovations for which secrecy is considered effective by the companies within the sub-industry group that replied to the survey[16]. *Secrecy Mfg* is set to zero for non-manufacturing firms given that no data is available. In order to control for the absence of results for non-manufacturing firms, I also introduce the dummy variable *Dummy Mfg* that takes the value one for manufacturing firms and zero otherwise.

The major proxy for rivals in the same location (H3) is *Shared State*, an indicator variable that takes the value one when at least two of the three top competitors have their headquarters in the same state. Company state headquarters information is from Compustat[17].

I also use *Meanindage* as a control, with the variable calculated as the average age of firms within a given industry, with the age being calculated from the first day the company becomes available in Compustat. *Meanindage* is a proxy for industry maturity. The direction of *Meanindage* is unclear as more mature industries have better defined competitors but younger industries might have stronger information spillover concerns.

I.4. Empirical Results

I.4.1 Association based results

Sample construction

The analysis focuses on the actual probability that the three top competitors share the same auditor. I compare this probability to the probability that three firms chosen at random

[16] Note that the validity of this variable for accounting research has been successfully tested by Erkens (2011)
[17] Results are unchanged when replacing Shared State with Shared MSA, an indicator variable equal to one when at least two of the three top competitors have their headquarter in the same MSA.

within the industry share the same auditor. In particular, I calculate the benchmark probability distribution by excluding the top three firms in the industry. In order to have meaningful comparisons, I focus on industries with at least six companies, with three companies being the top three players and where at least three other companies are available in order to calculate the benchmark probabilities. I also focus on industries where the proxies defined in section 3.2 are properly defined. Last, I focus on firms where the top three competitors are being covered by top tier auditors, as most large firms use the services of top tier auditors. Top tier auditors include the Big 8 auditors, which ultimately became the Big 4. This leaves a sample of 4,278 industry years.

I use the distribution of the non-top three firms in order to calculate the benchmark probabilities. More details are given in Appendix A with a specific example provided. First, for each auditor i, I calculate its market share p_i as the number of firms covered by auditor i divided by the total number of firms within the given industry, excluding the top three players in the calculation. p_i ,i=1,n denotes the assumed true distribution from which firms are sampled from. Second, assuming further that three firms are drawn from this distribution, with replacement, I then assess the benchmark distribution using the multinomial distribution. The purpose of the benchmark distribution is to compute the benchmark probabilities that none of the three sampled firms, two out of three or three out of three sampled firms share the same auditor. The benchmark distribution is given as follows. The probability that all three randomly chosen firms share the same auditor is given by $\sum_{i=1}^{n} p_i^3$. The probability that exactly two out of three randomly chosen firms share the same auditor is given by $3\sum_{i=1}^{n} p_i^2 - 3\sum_{i=1}^{n} p_i^3$. The probability that none of the firms share the same auditor is equal to one less the sum of the probabilities defined above. Third, I also define the probability that at least two out of three chosen firms (i.e.

$3\sum_{i=1}^{n} p_i^2 - 2\sum_{i=1}^{n} p_i^3$) share the same auditor, as the benchmark probability. The variable *Benchmarkprobability* is then used in a fourth step to calculate *Diff*, which measures the deviation from the benchmark probability of the actual outcome that at least two out of the top three firms in an industry share the same auditor. In other words, *Diff* is equal to an indicator variable that takes the value one when at least two out of the top three firms in an industry share the same auditor, less *Benchmarkprobability*. The purpose of *Diff* is to measure the deviation from the benchmark of the actual probability of two firms or more sharing the same auditor within the same industry. In particular, the *Diff* variable already controls for industry specialization. I regress *Diff* on the variables identified in section 3 in order to test for the hypotheses elaborated in section 2[18].

Table I.1 Panel A presents summary statistics for the variables defined above[19]. Note that *Diff* has a negative mean of (0.123), consistent with the top three players sharing their auditor less than the benchmark probability. Table I.1 Panel B presents correlations among the variables, with numbers in bold for correlations significant at the 5% level or better. *Herfindahl* is highly correlated with *Deltasale* whereas *CCR3* is less correlated with *Deltasale* than *Herfindahl*.

[18] Note that another potential research design could be to regress over several control variables an indicator variable taking the value one when two or more firms within an industry share the same auditor, and add *Benchmarkprobability* as an additional control on the right hand side of the regression. However, besides the fact that I am interested here in explaining the difference between the actual and benchmark probabilities, another issue is that *Benchmarkprobability* is measured with noise. The OLS coefficients would therefore be biased due to the downward bias of the *Benchmarkprobability* OLS coefficient. On the other hand, adding noise in the dependent variable does not bias the results of the regression.

[19] Given that the *Secrecy Mfg* variable is set to zero to more than half of the sample, I also include summary statistics for the secrecy variable restricted to the manufacturing sample of 1,797 firms

Results

In order to test H1, I employ a chi-square test in comparing the benchmark distribution to the actual distribution of industries where none of the three firms share the same auditor, two out of three firms share the same auditor and three out of three firms share the same auditor. Results are presented in Table I.2. I conduct this test every year because the stability of the client-auditor relationship makes the results sticky over time. The distribution of actual probabilities is skewed towards firms sharing their auditors less often than for the distribution of benchmark probabilities. The chi-square statistics are significant at the 5% level and better in all years and the differences are sizeable. For example, the actual probability for the top three competitors to all share the same auditor is of 6.2% when averaging the results across all the sample years, compared to a benchmark probability of 13.9%. The benchmark distribution as presented in Table I.2 dominates the actual distribution in the sense of the monotone likelihood ratio property (MLRP) for every year in the sample, indicating strong dominance results consistently across the sample. Note that the results hold for both before and after adoption of Sarbanes-Oxley (SOX). This indicates that the reluctance of same industry rivals to share the same auditor continued even after auditors were precluded from cross-selling most types of consulting services to their client firms.

I test H2 and H3 by regressing *Diff* on the empirical proxies defined in section 3.2. Univariate results can already be found in Table I.1 panel B in the correlations table. *Diff* is negatively correlated with *Herfindahl* and *CCR3*, indicating that companies are more reluctant to share the same auditor in concentrated industries. These results are consistent with Kwon (1996). *Diff* is also positively correlated with the capital expenditures variable (*Logindcapex*) and with the shared state variable (*Shared State*), consistent with both H2b and H3. I also find that *Diff* is negatively correlated with *Secrecy Mfg*, consistent with H2c. However, *Diff* is negatively correlated to *Deltasale*, perhaps because of the high correlation between *Deltasale* and *Herfindahl*.

I present multivariate results of the tests of H2 and H3 in Table I.3 using OLS. Given that the panel data covers 25 years and given the relative stability over time of auditor client relationships and industry characteristics, I cluster standard errors at the industry level to alleviate any concern of overstated t-statistics. I begin with a regression of *Diff* without any control, to confirm the results of Table I.2. In this regression, the constant is equal to the average of the *Diff* variable. The constant is significantly negative, indicating that top three players are sharing their auditor less often than the benchmark probability[20]. The next two columns introduce additional control variables. Overall, the results are consistent with H2a. As expected, the coefficients on *Herfindahl* or *CCR3* are significantly negative. The coefficient on *Deltasale* loads significantly in the regression with *Herfindahl* as a control, but not in the regression including *CCR3* as a control, providing weak evidence that companies share their auditor more when they are more different in terms of size. The results on H2b are stronger and consistent with the hypothesis. The coefficient on *Logindcapex* is significantly positive, indicating that firms in industries with large capital investments are less reluctant to share the same auditor. The coefficient for *Indpricecostmargin* is also significantly positive, indicating that industry differentiation plays some role in auditor choice. These results are consistent with less reluctance to share the same auditor when intra-industry barriers to mobility are high due to high capital expenditure requirements or to increased industry differentiation. The results are valid regardless of whether *Herfindahl* or *CCR3* were used as the industry concentration variable. The results are

[20] Note that the constant loses its initial meaning in the following regressions including controls, given that the mean of most independent variables is not equal to zero

also consistent with H3. The *Shared State* variable loads positively, significant at 1%. I then introduce *Secrecy Mfg* in the third and fourth columns in order to test H2c. The variable is negative but loads insignificantly, with a p-value of 10.8%. Other variables signs and statistical significances remain unchanged.

Next, I partition the sample between manufacturing firms and non-manufacturing firms. The results are broadly unchanged for the non-manufacturing sample. For the manufacturing sample, I find that the *Secrecy mfg* variable loads negatively, significant at 5% or better, providing evidence that rivals in more secretive industries are less likely to share their auditors. I also find that the coefficient on *Indpricecostmargin* and *Shared State* are insignificant. In untabulated tests, I find that the coefficient for *Indpricecostmargin* is not significantly different between the manufacturing and non-manufacturing samples, indicating that my tests may be suffering from a power issue in the analyses when partitioning the original sample. On the other hand, the coefficient on *Shared State* in the manufacturing sample is significantly smaller from the non-manufacturing sample (difference significant at 1%), indicating that manufacturing firms are still reluctant to share their auditors with their rivals even when their headquarters are located within the same state. One interpretation could be that the location of the plants, not captured by the *Shared State* variable, matters more for those firms than the location of the headquarters.

I assess the economic significance of the results on H2 and H3 from the second OLS regression, except for the *Secrecy Mfg* variable, assessed from the eighth specification (manufacturing sample) due to lack of significance in the second regression. The results are economically significant. For example, an increase of one standard deviation for *Herfindahl* (see Table I.1 for standard deviations) reduces the propensity of firms to share the same auditor by 13.9%. An increase of one standard deviation of *Logindcapex* and *Indpricecostmargin* increases the propensity of firms to share the same auditor by 5.8% and 2.8%, respectively. Sharing the same state increases the propensity of firms to share the same auditor by 8.9%. An increase of one standard deviation of *Secrecy Mfg* decreases the propensity of manufacturing firms to share the same auditor by 4.7%.

I.4.2 Auditor switches sample

Description of the analysis

I conduct similar analyses focusing only on the subsample of firms that switch auditors. In normal situations, a sample of firms switching auditors might suffer from a selection bias, as the switching decision is endogenous. However, it is still worthwhile to test H1 as firms still have to make a decision regarding their future auditor conditional on a switch occurring. The collapse of Arthur Andersen in 2002 also provides an exogenous shock to the industry. Arthur Andersen's former clients had no choice but to switch auditors. I identify 940 auditor switches by top three firms within the sample, including 232 coming from the collapse of Arthur Andersen. The benchmark probability cannot be calculated as before. The benchmark probabilities are calculated as the market share of the remaining auditors who cover other top three rivals. I exclude the auditor from which the firm is switching from in the calculation of the benchmark probability. A concrete example is provided in Appendix B. Specifically, if auditor i is the auditor from which the top three player is switching from, the benchmark probability is given by $\frac{\sum_{j \neq i}^{n} p_j . 1_{j,Top3}}{1 - p_i}$ where the indicator variable $1_{j,top3}$ takes the value 1 if auditor j covers one of the industry top three players. Similar to the association based results, I focus on switches to top tier auditors as most top three players are covered by top tier auditors.

Results

I compare the actual probability that a top three player switches to an auditor covering one of its top three rivals to the benchmark probability defined above. Test results of H1 are presented in Table I.4. Consistent with H1, the actual probability of a top player sharing the same auditor as its rival is significantly lower than the benchmark probability. This is true for both the Arthur Andersen sample and the remainder of the sample. The numbers are economically significant with the actual probability being half of the benchmark probability. This indicates that switching top industry players are reluctant to share the same auditor with their rivals.

I test H2 and H3 in a similar fashion to Table I.3, using an OLS specification with the dependent variable *Diff* equal to the deviation from the benchmark probability of the switching firm switching to an auditor covering its rival or not[21]. The results are presented in Table 5. In the first column, I confirm the results of Table I.4 by regressing the variable *Diff* without any control variable. The constant is significantly negative and corresponds to the difference between the actual probability and the benchmark probability presented in Table I.4. I then introduce control variables in the second and third columns[22]. Most of the results are consistent compared to Table I.3 and confirm my initial results. *Herfindahl* and *CCR3* load negatively, while *Deltasale* loads positively in the second column and *Logindcapex* loads positively in both columns. *Secrecy Mfg* loads negatively, providing stronger results than in Table I.3. The coefficient on *Indpricecostmargin* remains positive but is insignificant, while the coefficient on

[21] *Diff* is equal to the difference between an indicator variable that takes the value one when the switching firm switches to an auditor covering its rival and the benchmark probability
[22] Note that the constant loses its meaning in those specifications as the mean of the independent variables is not equal to zero

Shared State is insignificant or negative with marginal significance, both contrary to expectations.

I then partition the sample between the switching firms outside of the Arthur Andersen collapse and the firms that had to switch auditors due to the collapse of Arthur Andersen. The results are broadly unchanged compared to the first three columns, with statistical significance sometimes reduced, possibly due to the smaller sample sizes.

I.4.3 Auditor mergers sample

Description of the analysis

I use auditor mergers as a source of exogenous shocks to the client auditor allocation. This allows testing for a causal relationship between rivals being concerned about sharing the same auditor and auditor choice. There were only three large auditor mergers in the past 25 years[23]. However, each auditor had a large clientele. Consequently, in several industries, rivals covered by different auditors would have ended up being covered by the same auditor after the merger, unless they switched auditors. On the other hand, rivals in other industries ended up not being impacted by the merger. The potential spillover risk of sharing auditors with a rival increased for overlapping rivals, keeping the benefits of auditor specialization relatively constant, while this risk did not change for non-overlapping rivals. This setup provides a natural control to

[23] Ernst & Whinney merged with Arthur Young in October 1989, Deloitte, Haskins and Sells merged with Touche Ross in December 1989, and Price Waterhouse merged with Coopers Lybrand in July 1998

compare the probability of switching of rivals where an overlap occurred to firms where no overlap occurred[24].

I define rivals as top three players within each industry and only consider firms covered by the auditors that were involved in the merger, by looking at the auditor-client allocation one year prior to the merger. I then analyze whether clients had switched auditors one year after the merger occurred, depending on whether the other auditor involved in the auditor merger covered a top three rival to the company or not. I restrict the sample to industries with three players or more, where the NAICS codes are well defined at the six-digit level. These restrictions yield a final sample of 623 firms, including 112 overlapping firms and 64 switches.

Results

First, in order to test H1, I compare the probability of switching for overlapping rivals compared to non-overlapping rivals in Table I.6 panel A, where rivals are defined as top three within an industry. I conduct analyses at the company level, where each datapoint is a company, and create an indicator variable, switch, that indicates whether the company switched auditors after the merger compared to before the merger. I also create another indicator variable, *Overlap top3 players*, that indicates whether the other auditor involved in the merger covered a top three rival to the company or not. I find evidence that overlapping rivals are much more likely to switch auditors than non-overlapping rivals, with the probability increasing from 8.8% to 17.0%. The differences are statistically significant at the 1% level using a Chi-Square test of differences, and also large, with the probability of switching almost doubling depending on whether the other auditor involved in the merger covers a top three rival or not.

[24] I also confirmed that there was no ruling by the Department of Justice (DOJ) or Federal Trade Commission (FTC) that compelled the newly merged auditor to divest some of its clients due to a dominant position of the merged auditor in specific industries. Such a ruling could have biased the specifications towards me finding the results

I introduce additional controls in Table I.6 panel B[25]. Switching auditors can be a costly decision as the new auditor may not fully acquainted with the firms' operations. The new auditor also needs to move up the learning curve with its new client. Even though the chosen auditor may decide not to include any extra start-up cost in the initial engagement pricing, the increased activity by the new auditor in order to move up the learning curve could still consume increased time and managerial resources at the client firm, yielding non negligible switching costs for the client firms. Consequently, I posit that client firms are less likely to switch auditors when switching costs increase. I proxy for switching costs by including a dummy for a relationship longer than 5 years, *Long Relationship*, and interact this dummy with whether there is an overlap with a rival or not[26]. Results in the second column of Table I.6 panel B are weakly consistent with my predictions. The coefficient on *Overlap top3 players* loads significantly, indicating that firms with a short auditor relationship are more likely to switch auditors when an overlap with a rival occurs. On the other hand, the sum of *Overlap top3 players* and the interaction of *Overlap top3 players* and the long relationship dummy (*Long Relationship*) is insignificant[27]. However, the interaction coefficient does not load significantly in the regression.

In columns 3 and 4 of Table I.6 panel B I also introduce the additional controls defined in section 3.2. The results on the *Overlap top3 players* variable are still robust to inclusion of these control variables. To test rivals similarity, industry differentiation, capital expenditures, secrecy (H2) and shared headquarters location (H3), I also interact these control variables with *Overlap top3 players* in column 5. Results are weak, possibly because the total number of switches is too

[25] The first column of panel B confirms that the results in panel A are robust to the use of a logit specification
[26] The 5 years cutoff date is based on the results of Levinthal and Fichman (1988), who estimate a hazard model of auditor switching and show that the hazard initially increases until a 5 year relationship is reached and then decreases subsequently.
[27] Even though the sum of both coefficients is still positive, untabulated tests show that the p-value for the sum of both coefficients is at 0.39.

limited, especially for the overlapping sample where only 19 switches are available[28]. Most of the interaction coefficients are insignificant. Only the coefficient on the interaction with *Indpricecostmargin* loads negatively, indicating that, consistent with H2b, companies in more differentiated industries are less likely to switch auditors when client overlaps occur. Due to potential non-linearity concerns in the logit specifications, especially regarding the interaction coefficients, I also confirmed the analysis using OLS. The results are presented on the right of Table I.6 Panel B and are consistent with the logit specifications, with a negative coefficient on the interaction of *Indpricecostmargin* and no other coefficient loading significantly in the specifications.

Overall, the merger test provided causal evidence in favor of same industry rivals avoiding to share the same auditor (H1) and additional evidence in favor of industry differentiation as a mitigating factor to H1 (H2b). The main interest of the merger test is that it does not involve the calculation of any benchmark probability.

Comments and robustness tests

One potential concern related to auditor mergers is that some auditor mergers might not have happened in the first place because the audit firms might have feared that some of the same industry clients would have left after the merger, due to concerns of sharing the same auditor with their rivals. Indeed, an analysis of auditor merger rumors in Factiva indicates that several mergers considered among the Big Eight auditors did not happen. For example, a potential merger between KPMG and Ernst & Young in 1998 did not happen due to antitrust issues (see Reuters News, February 13, 1998). However, this concern goes against me finding the results in

[28] The limited number of actual switches precludes any further analysis where the sample would be partitioned even further to companies where switching costs are lower or higher.

the merger test and would bias the test towards the null in case it is valid. In particular, my results indicate that for the mergers that took place, same industry rivals switched auditors more often than the control group when faced with the prospect of having their auditor covering a rival within the industry.

Another potential concern is related to the supply based alternative explanation where the auditor decides to resign from some of its clients due to excess accumulation of risk within given industries. Even though accumulation of risk is more unlikely to happen when looking at large firms, a way to test for this alternative hypothesis is to look at smaller firms within the industry. Smaller firms are not necessarily in direct competition with each other or with larger firms, but could bring similar industry accumulation risks to the auditors at the time of merger. Consequently, the expectation in the case of the risk based explanation is that smaller firms will switch auditors when an overlap with a large firm or with a smaller firm occurs, in a similar fashion to larger firms. This hypothesis does not hold in the information spillover concerns hypothesis. Untabulated tests, using the largest ten firms in each industry, indicate that smaller firms are not likely to switch more than the control group when an overlap with a firm in the top three group or the top ten group occurs. This result is consistent with the information spillover concerns explanation.

I.5. Auditor pricing and the collapse of Arthur Andersen

I.5.1 Description of the analysis

In this section I investigate whether auditors are able to extract rents from the reluctance of their clients to share the same auditor. I use the collapse of Arthur Andersen as an exogenous shock to the auditing supply and posit that the supply tightened more in some industries than in others[29]. In particular, I predict that audit fees increased more after the collapse of Arthur Andersen in industries where the concern for information spillover was higher and auditing supply was scarcer, as the tightening of the audit supply gave increased opportunities to the auditors for rent extraction (H4).

There are three groups of firms that could be considered in the analysis. I present an illustrative example of these groups in Figure I.1 with two industries and three remaining auditors. Group 1 contains firms covered by Arthur Andersen prior to the collapse of the auditing firm. Any comparison of fees for these firms prior and after the collapse would be meaningless, as these firms had to switch of auditor. Many confounding factors could explain any result on fee changes, including fees lowballing prior or after the collapse of the Arthur Andersen, and including potential compensation for increased risk by the new auditor from taking a former Arthur Andersen client. Consequently, I have no prediction regarding the first group and exclude it from the analyses. Group 2 contains firms that are covered by auditors who started covering in the same industry firms previously covered by Arthur Andersen. Again, any analysis of the fees here could be subject to several confounding factors that could explain the results. Evidence of fees increase could not only be explained by rent extraction from the auditor, but also by

[29] I am unable to use auditor mergers as an exogenous shock because companies started reporting audit fee data from 2000, whereas all the auditor mergers took place before 1999

compensation for increased risk taking by the auditor within a given industry. Consequently, I exclude this group from the analyses. Group 3, the group of focus, contains firms that were covered by auditors other than Arthur Andersen prior to the collapse of the auditing firms and whose auditors did not end up covering firms previously covered by Arthur Andersen within the same industry. For this group of firms, I predict that fees increased for firms in industries where supply was tight prior to the collapse of Arthur Andersen, in comparison with industries where supply was less tight. Note that it would be difficult to reconcile positive results with any other alternative explanation besides rent extraction from the auditor due to the reluctance of rival firms to share the same auditor[30].

I.5.2 Data and sample construction

I use the Compustat audit fee database, which includes audit fee data from 2000 to 2006. I only keep firms covered by auditors that did not end up covering firms in the same industry previously covered by Arthur Andersen. The industry is defined at the NAICS 6 digit codes, consistent with prior tests. Depending on the specifications, I focus on either the top three players, the top five players and the top ten players. The reason I change the sample and do not remain fully consistent with my prior analysis of the top three players in earlier sections is that there was a sufficient number of Big 4 auditors to audit top three firms without any overlap. It therefore makes sense to expand the number of firms in an industry to allow the constraint on auditors to play a role in rent extraction.

[30] In untabulated tests, I confirm that results are also robust for the expanded samples including group 2 and group 3 and all firms (group 1 and group 2 and group 3). However, results are easier to interpret when restricting the sample to group 3. Results are also robust to restricting the sample to years 2001 and 2004, in the spirit of a pure difference in differences test comparing fees before the collapse and after. Last there is no reason to expect any selection bias in Group 3 that would go towards me finding the results.

I use several proxies for tightness of supply. The first proxy, applied to the top three, takes the value one when prior to the collapse of Arthur Andersen the top three players were covered by three different top tier auditors, and zero otherwise. The second proxy, applied to the top five, takes the value one when prior to the collapse of Arthur Andersen the top five players within one industry were covered by all five top tier auditors, and zero otherwise. The third proxy, applied to the top ten, takes the value one when prior to the collapse of Arthur Andersen the top ten players within one industry were covered by all five top tier auditors, and zero otherwise. The fourth proxy is equal to the number of top tier auditors covering the top three players within an industry prior to the collapse of Arthur Andersen, divided by three. The fifth and sixth proxies are similar to the fourth, except that they apply to the top five players and top ten players within an industry. For example, the sixth proxy is equal to the number of top tier auditors covering the top ten players within an industry, divided by five.

I.5.3 Results

Table I.7 regresses the natural logarithm of the audit fees on the proxy variable, an interaction between the proxy variable and the years after Arthur Andersen collapse, and several control variables. I predict that the interaction variable should load positively, indicating an increase of the fees after the collapse of Arthur Andersen for firms in industries where supply was tighter prior to the collapse. I also include several control variables, defined the same way as in Francis et al. (2005). *Lta* is the log of assets, *Lseg* is the log of business segments as reported in the Compustat Segments database, *Cata* is the ratio of current assets to total assets, *Quick* is the ratio of current assets less inventories to current liabilities, *De* is the ratio of long term debt to total assets, *Roi* is the ratio of earnings before interest and taxes to total assets, *Foreign* is the proportion of income from foreign operations (defined as pifo/(pifo+pidom) from Compustat), *Opinion* takes the value one when the audit report is not unqualified (auop code other than 1 in Compustat), *Ye* takes the value one when the year end is not December 31st, and *Loss* takes the value one when net income is negative for a given year. I further winsorize at the 1% and 99% percentiles *Logauditfee, Lta, Cata, Quick, De, Roi* and *Foreign*. In order to control for fixed effects, I also include year dummies, Fama French industry group dummies, and dummies for the number of firms within each industry. Overall, the statistical significance of the regressions, evidenced by the high R-square values and the control variables signs and statistical significances, are consistent with Francis et al. (2005) results. I also cluster standard deviations at the company level as most variables in the regression are relatively stable over time for each company.

The results in Table I.7 are consistent with my initial hypothesis. Most proxies for industry tightness load positively in the interaction term, indicating an increase of fees after the collapse of Arthur Andersen for industries where auditing supply was tight prior to the collapse of Arthur Andersen (H4). Results are significant at the 1% level when using the top ten as the primary sample and at the 5% level when using the top five. The results go in the predicted direction but are statistically insignificant when using proxies based on the top three players, possibly because these proxies are not strong enough to capture tightness of industry supply, or because due to their larger size the top three players in each industry still have enough negotiation power to avoid any major fee increase from the auditor.

In terms of economic significance, the average log of audit fee for the top ten in the sample is 6.308, or a total fee of $549,000. If I use the results from the third column of Table I.7, the log would increase to 6.404, or a total fee of $604,000, an economically significant difference.

I.6. Conclusion

In this paper, I presented evidence that top rivals are reluctant to share the same auditor, due to information spillover concerns. I document that the probability of all top three rivals sharing the same auditor being is only slightly below 50% of the benchmark probability. The use of exogenous shocks to the auditing industry, including auditors mergers and the collapse of Arthur Andersen, make a case for spillovers as a causal factor in auditor choice. The observed patterns occur more in industries where top leaders are more similar to each other, in manufacturing industries where secrecy matters, less in industries where differentiation is higher or capital expenditures are higher and less where top players are headquartered in the same state. Last, using Arthur Andersen collapse, I presented evidence that the auditors are able to extract rents from this behavior from their client firms.

The results suggest that the auditing industry might be even less competitive than initially envisioned. Given the reduction of the number of auditors to the Big 4, and the lack of entry in the industry, those results have important policy implications. In particular, my results contribute to the recent debate on mandatory auditor rotation for client firms and suggest that mandatory auditor rotation might increase the reluctance of firms to share proprietary information with their auditors in case they have to share the same auditor with their rivals.

This paper is the first one to show a causal relationship between companies concern of information spillovers and auditor choice. Several interesting research questions remain to be answered, in particular documenting actual information spillovers coming from a shared auditor and studying market implications of the reluctance of rival firms to share the same auditor.

Appendix I.A: Calculation of Benchmark Probabilities - Association Based

Appendix I.A proposes the following example to detail the computation of the benchmark distribution and the variable *Diff*. The figure represents a current client-auditor allocation for a given industry.

A	B	C	D
1		2	
		3	
4	5	6	8
11	12	7	9
		10	13

In this example, there are four auditors, A, B, C and D, and 13 firms composing the industry. Each firm is denominated by its rank in sales, with firm 1 being the firm with the largest sales and 13 the firm with the lowest sales.

First, I assume that the distribution of the non-top three firms is the true distribution from which firms are chosen randomly. There are ten non-top three firms in the industry. Consequently, the distribution is given by: $P_A = \frac{2}{10} = .2$, $P_B = \frac{2}{10} = .2$, $P_C = \frac{3}{10} = .3$, $P_D = \frac{3}{10} = .3$

I then sample randomly three firms from this distribution, with replacement. The probability that two firms out of the three share the same auditor is given by:

$$P_2 = 3\times(0.2^2 + 0.2^2 + 0.3^2 + 0.3^2) - 3\times(0.2^3 + 0.2^3 + 0.3^3 + 0.3^3) = 0.57$$

The probability that three firms out of the three share the same auditor is given by:

$P_3 = 0.2^3 + 0.2^3 + 0.3^3 + 0.3^3 = 0.07$

Consequently the probability that none of the three firms share the same auditor is given by:

$P_0 = 1 - P_1 - P_2 = 1 - 0.57 - 0.07 = 0.36$

The benchmark probability, the probability that at least two firms share the same auditor is given by: $P_{benchmark} = P_2 + P_3 = 0.57 + 0.07 = 0.64$

The variable Diff is equal to: $Diff = 1 - 0.64 = 0.36$ because firms 2 and 3 share the same auditor, auditor C. In case no top three firm shares the same auditor I would have $Diff = 0 - 0.64 = -0.64$

Appendix I.B: Calculation of Benchmark Probabilities – Auditor Switches

Appendix I.B proposes the following example to detail the computation of the benchmark distribution and the variable *Diff*. The figure represents a client-auditor allocation for a given industry. Client 1 is the client that is assumed to switch auditors.

A	B	C	D
1	2	3	4
10	5	6	9
	7	8	12
	11		

In this example, there are four auditors, A, B, C and D, and 12 firms composing the industry. Each firm is denominated by its rank in sales, with firm 1 being the firm with the largest sales and 12 the firm with the lowest sales.

There are 10 firms covered by auditors B, C and D. Consequently, conditional on not choosing auditor A, the probabilities of choosing auditors B, C and D are given by $P_B = \frac{4}{10} = .4$,

$P_C = \frac{3}{10} = .3$, $P_D = \frac{3}{10} = .3$

Auditors B and C cover firms 2 and 3, assumed to be firm 1's direct rivals. Consequently, the benchmark probability is given by $P_{Benchmark} = 0.4 + 0.3 = 0.7$

If firm 1 switches to auditors B or C, then the variable Diff is given by: $Diff = 1 - 0.7 = 0.3$

If firm 1 switches to auditor D, then the variable Diff is given by: $Diff = 0 - 0.7 = -0.7$

Table I.1: Descriptive statistics

Table I.1 Panel A presents descriptive statistics and Table I.1 Panel B presents correlations among variables, with Spearman above the diagonal and Pearson below and significance at the 5% level or better in bold. There are 4,278 industry years in the sample. *Diff* equals one less the benchmark probability when at least two top three players share the same auditor, and zero less the benchmark probability otherwise. *Herfindahl* is the industry Herfindahl index. *CCR3* is the concentration ratio for the top three firms in each industry as defined by sales. *Logindcapex* is equal to the log of the mean industry capital expenditures. *Indpricecostmargin* is equal to industry sales divided by industry operating costs and is a measure of industry differentiation. *Deltasale* equals the standard deviation of the sale of the two three players divided by the mean of the top three players sales and is a measure of firm similarity. *Meanindage* is equal to the industry average of the number of years firms are in the Compustat database. *Shared State* is a dummy variable equal to one when at least two of the three top firms are headquartered in the same state. *Dummy Mfg* takes the value one when firms are classified as manufacturing firms. *Secrecy* is the average of the secrecy product and secrecy variables as in Cohen et al. (2000) Table 1 and Table 2. The variable is only defined for manufacturing firms. *Secrecy Mfg* equals secrecy when firms are manufacturing firms and zero otherwise.

Panel A: Descriptive statistics

Variable	N	Mean	SD	p25	p50	p75
Diff	4,278	(0.123)	0.514	(0.625)	0.144	0.336
Herfindahl	4,278	0.276	0.165	0.165	0.234	0.342
CCR3	4,278	0.715	0.171	0.606	0.732	0.848
Logindcapex	4,278	4.382	1.860	3.179	4.378	5.677
Indpricecostmargin	4,278	1.153	0.483	1.049	1.089	1.154
Deltasale	4,278	0.625	0.375	0.339	0.555	0.858
Meanindage	4,278	13.595	6.341	9.003	12.445	17.041
Shared State	4,278	0.294	0.456	-	-	1.000
Dummy Mfg	4,278	0.420	0.494	-	-	1.000
Secrecy Mfg	4,278	20.990	25.181	-	-	45.420
Secrecy	1,797	49.968	7.821	43.455	50.105	57.190

Panel B: Correlations (Spearman above diagonal, Pearson below)

Variable	Diff	Herfindahl	CCR3	Log indcapex	indprice costmgn	Delta sale	Mean Indage	Shared State	Dummy Mfg	Secrecy Mfg
Diff		**(0.212)**	**(0.230)**	**0.075**	(0.009)	**(0.057)**	**(0.033)**	**0.100**	**(0.032)**	**(0.044)**
Herfindahl	**(0.119)**		**0.953**	**0.154**	**0.068**	**0.746**	**0.050**	**(0.033)**	**0.133**	**0.103**
CCR3	**(0.164)**	**0.821**		**0.128**	**0.054**	**0.565**	**0.075**	(0.021)	**0.109**	**0.087**
Logindcapex	**0.080**	**0.201**	**0.111**		**0.241**	**0.209**	**0.333**	**0.045**	(0.028)	0.008
Indpricecostmargin	0.016	**0.123**	**0.079**	**(0.162)**		**0.096**	**0.042**	**0.068**	**(0.057)**	**(0.034)**
Deltasale	**(0.048)**	**0.833**	**0.556**	**0.222**	**0.097**		**(0.034)**	**(0.046)**	**0.139**	**0.104**
Meanindage	0.004	0.021	**0.059**	**0.346**	**(0.055)**	**(0.042)**		**(0.099)**	**0.275**	**0.280**
Shared State	**0.087**	(0.047)	(0.017)	**0.031**	**(0.030)**	**(0.051)**	**(0.119)**		(0.007)	(0.006)
Dummy Mfg	**(0.038)**	**0.114**	**0.116**	(0.015)	**(0.104)**	**0.130**	**0.258**	(0.007)		**0.953**
Secrecy Mfg	**(0.042)**	**0.092**	**0.098**	0.009	**(0.099)**	**0.105**	**0.269**	(0.008)	**0.980**	

Table I.2: Probability of sharing the same auditor

Table I.2 presents the distribution of the probabilities that no firms out of the top three players, two firms out of the top three players, and three firms out of the top three players share the same auditor, in comparison to the benchmark probability that three randomly chosen firms within the industry, excluding the top three players, share the same auditor. Calculations are performed on a yearly basis, with a chi-square test of differences and p-value. MLRP satisfied indicates whether the benchmark distribution as presented in the table dominates the actual distribution as presented in the table in the sense of the monotone likelihood ratio property (MRLP).

	Number	Actual Probabilities - Top 3 Sharing			Benchmark Probabilities - Top 3 Sharing			Test of differences		MLRP
Year	**Industries**	**None**	**Two**	**Three**	**None**	**Two**	**Three**	**Chi Sq**	**p -value**	**Satisfied**
1985	145	52.4%	46.9%	0.7%	38.0%	51.6%	10.4%	21.673	0.000	Yes
1986	150	54.0%	44.0%	2.0%	38.5%	51.0%	10.5%	21.195	0.000	Yes
1987	169	52.7%	43.8%	3.6%	37.5%	51.9%	10.6%	20.320	0.000	Yes
1988	158	55.1%	41.8%	3.2%	38.8%	51.1%	10.1%	21.008	0.000	Yes
1989	166	53.0%	43.4%	3.6%	33.0%	54.9%	12.1%	33.993	0.000	Yes
1990	174	47.1%	47.1%	5.7%	32.7%	55.4%	11.9%	18.817	0.000	Yes
1991	173	50.9%	44.5%	4.6%	33.1%	55.6%	11.3%	27.119	0.000	Yes
1992	193	49.2%	47.7%	3.1%	32.2%	55.7%	12.1%	32.369	0.000	Yes
1993	193	48.7%	48.7%	2.6%	33.4%	55.2%	11.4%	28.277	0.000	Yes
1994	213	44.1%	52.1%	3.8%	32.6%	55.4%	12.1%	21.375	0.000	Yes
1995	237	47.7%	47.7%	4.6%	32.2%	55.6%	12.3%	31.664	0.000	Yes
1996	237	45.6%	49.4%	5.1%	32.6%	55.1%	12.2%	23.509	0.000	Yes
1997	224	36.6%	56.7%	6.7%	33.0%	55.1%	11.9%	6.088	0.048	Yes
1998	219	37.9%	54.8%	7.3%	27.9%	58.3%	13.9%	15.131	0.001	Yes
1999	206	39.3%	51.5%	9.2%	27.5%	58.8%	13.7%	15.435	0.000	Yes
2000	188	43.6%	48.4%	8.0%	27.9%	57.6%	14.4%	24.759	0.000	Yes
2001	170	44.7%	45.3%	10.0%	29.4%	57.7%	12.9%	19.258	0.000	Yes
2002	166	30.7%	56.6%	12.7%	21.8%	61.2%	17.0%	8.528	0.014	Yes
2003	156	30.1%	60.9%	9.0%	22.2%	60.5%	17.3%	10.681	0.005	Yes
2004	147	29.3%	60.5%	10.2%	21.5%	61.1%	17.5%	8.622	0.013	Yes
2005	139	27.3%	63.3%	9.4%	21.4%	61.4%	17.2%	7.289	0.026	Yes
2006	123	26.0%	65.0%	8.9%	20.9%	60.5%	18.6%	8.196	0.017	Yes
2007	115	28.7%	63.5%	7.8%	22.6%	60.6%	16.8%	7.544	0.023	Yes
2008	111	32.4%	61.3%	6.3%	21.3%	59.8%	18.9%	15.755	0.000	Yes
2009	106	24.5%	68.9%	6.6%	20.3%	60.1%	19.6%	11.440	0.003	Yes
Average		**41.3%**	**52.5%**	**6.2%**	**29.3%**	**56.8%**	**13.9%**			

Table I.3: Drivers of probability of sharing the same auditor

Table I.3 presents an OLS analysis where the dependent variable, *Diff*, is equal to the difference between the actual probability that at least two top three firms share the same auditor and the benchmark probability. Control variables have been defined in Table I.1. Standard deviations are clustered at the industry level. Coefficient values are presented above and the t-statistic below. The results are presented for the full sample, the sample including non-manufacturing firms only and the sample including manufacturing firms only. Significance levels are * 10%, ** 5% and *** 1%.

		Full Sample					Non Mfg Sample		Manufacturing Sample	
Dep. Variable: Diff	**Predicted Sign**	**No Controls**	**Herfindahl**	**CCR3**	**Secrecy Herfindahl**	**Secrecy CCR3**	**Herfindahl**	**CCR3**	**Herfindahl**	**CCR3**
Herfindahl	-		(0.840) ***		(0.846) ***		(0.853) ***		(0.946) ***	
			(5.091)		(5.104)		(3.812)		(3.923)	
CCR3	-			(0.602) ***		(0.604) ***		(0.625) ***		(0.629) ***
				(5.683)		(5.734)		(4.624)		(3.881)
Logindcapex	+		0.031 ***	0.027 ***	0.031 ***	0.029 ***	0.029 **	0.027 **	0.039 **	0.035 *
			2.949	2.678	3.102	2.851	2.384	2.334	2.226	1.963
Indpricecostmargin	+		0.057 ***	0.049 ***	0.057 ***	0.049 ***	0.054 ***	0.046 **	0.292	0.252
			3.366	2.830	3.334	2.840	2.774	2.440	0.931	0.744
Deltasale	+		0.208 ***	0.056	0.206 ***	0.051	0.218 **	0.076	0.196 *	(0.001)
			2.854	1.181	2.828	1.080	2.219	1.212	1.883	(0.016)
Mean Industry age	?		(0.001)	-	-	-	(0.003)	(0.003)	0.002	0.002
			(0.290)	(0.126)	(0.065)	0.036	(0.754)	(0.704)	0.365	0.492
Shared State	+		0.089 ***	0.094 ***	0.089 ***	0.094 ***	0.156 ***	0.158 ***	(0.011)	-
			2.749	2.859	2.763	2.869	3.653	3.640	(0.232)	0.006
Dummy Mfg	?				0.206	0.208				
					1.471	1.520				
Secrecy Mfg	-				(0.004)	(0.004)			(0.006) **	(0.006) **
					(1.612)	(1.612)			(2.135)	(2.181)
Constant		(0.123) ***	(0.237) ***	0.073	(0.238) ***	0.071	(0.218) ***	0.095	(0.223)	0.142
		(7.092)	(4.292)	0.924	(4.281)	0.909	(3.082)	0.928	(0.663)	0.398
N		4,278	4,278	4,278	4,278	4,278	2,481	2,481	1,797	1,797
Number Clusters		359	359	359	359	359	211	211	148	148
Adjusted R-square		-	0.040	0.045	0.042	0.047	0.049	0.058	0.044	0.042
F-statistic		-	7.724 ***	8.143 ***	6.272 ***	7.135 ***	5.695 ***	6.754 ***	4.456 ***	4.097 ***

Table I.4: Auditor Switches - Univariate

Table I.4 presents a comparison to the benchmark probability of the actual probability that a client firm switches to an auditor covering a rival firm, conditional on the firm switching. The benchmark probability is calculated as the market share of the auditors covering other top three players, divided by one minus the market share of the auditor covering the switching top three player. A chi square tests for the equality of the probability numbers.

	Sample		
	All Switches	**Arthur Andersen**	**Non Arthur Andersen**
Actual Probability	34.1%	34.1%	34.2%
Benchmark Probability	68.7%	70.7%	68.1%
Chi Square Value	523.545	149.993	375.354
P-value	<.0001	<.0001	<.0001
Number Firms	940	232	708

Table I.5: Auditor Switches - Multivariate

Table I.5 uses an OLS specification with the dependent variable, *Diff*, equal to the actual probability of the switching top three firm sharing his new auditor with another top three firm, less the benchmark probability. Coefficient values are presented above and the t-statistic below. Results are presented for the overall sample of switching firms, for the sample of switching firms excluding Arthur Andersen, and for the sample of switching firms from Arthur Andersen collapse. Control variables have been defined in Table I.1. Significance levels are * 10%, ** 5% and *** 1%.

Dep. Variable:	**Predicted**		**Overall Sample**			**Excluding Arthur Andersen**			**Arthur Andersen Collapse**	
Diff	**Sign**	**(1)**	**(2)**	**(3)**	**(1)**	**(2)**	**(3)**	**(1)**	**(2)**	**(3)**
Herfindahl	-		(1.407) ***			(1.373) ***			(1.565) ***	
			(9.381)			(8.288)			(4.447)	
CCR3	-			(0.995) ***			(0.958) ***			(1.060) ***
				(8.161)			(6.802)			(4.318)
Logindcapex	+		0.030 ***	0.036 ***		0.022 *	0.029 **		0.067 ***	0.070 ***
			3.002	3.501		1.916	2.462		2.989	3.132
Indpricecostmargin	+		0.043	0.042		0.093	0.091		0.047	0.047
			1.511	1.478		1.269	1.227		1.397	1.418
Deltasale	+		0.404 ***	(0.043)		0.353 ***	(0.081)		0.577 ***	0.052
			4.920	(0.865)		3.891	(1.439)		2.969	0.510
Mean Industry age	?		(0.005) *	(0.006) ***		(0.004)	(0.006) **		(0.006)	(0.008) *
			(1.946)	(2.650)		(1.414)	(2.034)		(1.186)	(1.659)
Shared State	+		-	(0.068) *		0.021	(0.052)		(0.060)	(0.117)
			(0.004)	(1.903)		0.497	(1.269)		(0.745)	(1.515)
Dummy Mfg	?		0.389 **	0.492 ***		0.364 **	0.451 **		0.598 *	0.745 **
			2.535	3.174		2.068	2.533		1.874	2.332
Secrecy Mfg	-		(0.006) **	(0.008) ***		(0.006) *	(0.008) **		(0.010)	(0.012) *
			(2.096)	(2.592)		(1.747)	(2.120)		(1.617)	(1.923)
Constant		(0.346) ***	(0.171) ***	0.435 ***	(0.339) ***	(0.173) *	0.408 ***	(0.366) ***	(0.405) ***	0.249
		(19.570)	(2.761)	4.154	(16.820)	(1.746)	2.892	(9.987)	(3.076)	1.142
N		940	940	940	708	708	708	232	232	232
Adjusted R-square		-	0.150	0.132	-	0.147	0.122	-	0.169	0.166
F-statistic		-	21.716 ***	18.817 ***	-	16.271 ***	13.244 ***	-	6.889 ***	6.728 ***

Table I.6: Auditor Mergers

Table I.6 panel A presents a comparison of the switching rate of firms when auditors merge depending on whether the merger brings an overlapping rival or not. Panel B presents a logit specification where the dependent variable equals one when the firm switches auditors, and zero otherwise. The *Overlap top3 players* variable takes the value one when the other auditor involved in the auditor merger covers another top three player within the same industry, as defined by the NAICS six digits code. Long relationship is a dummy variable equal to one when the client-auditor relationship is above five years. Other variables have been defined in Table I.1. Interaction coefficients are for the overlap variable and the other variables of interest. Significance levels are * 10%, ** 5% and *** 1%.

Panel A

	Company level		
	No Switch	**Switch**	**Total**
No Overlap	466	45	511
	91.2%	8.8%	100.0%
Overlap top 3 players	93	19	112
	83.0%	17.0%	100.0%
Total	559	64	623
	89.7%	10.3%	100.0%

	Statistic	**p-value**
Chi Square Statistic	6.633	0.010

Panel B

	Logit					OLS
	(1)	(2)	(3)	(4)	(5)	(1)
Overlap top3 players	0.749 **	0.824 **	0.717 **	0.721 **	9.413 ***	1.037 ***
	2.530	2.228	2.327	2.338	2.631	3.658
Long Relationship		(0.899) ***	(0.811) ***	(0.821) ***	(0.683) *	(0.050) *
		(2.795)	(2.624)	(2.660)	(1.878)	(1.710)
Overlap x Long Relationship		(0.368)			(0.666)	(0.104)
		(0.569)			(0.877)	(1.493)
Herfindahl			2.313		2.967	0.209
			1.382		1.542	1.508
Overlap x Herfindahl					(2.845)	(0.192)
					(0.591)	(0.442)
CCR3				1.438		
				1.356		
Logindcapex			(0.094)	(0.099)	(0.072)	(0.009)
			(1.423)	(1.531)	(0.988)	(1.297)
Overlap x Logindcapex					0.316	0.038
					1.404	1.645
Indpricecostmargin			0.100	0.105	0.113	0.017 **
			0.758	0.818	0.725	2.369
Overlap x Indpricecostmargin					(8.266) **	(0.874) ***
					(2.340)	(3.193)
Delta Sale			(0.540)	0.263	(0.547)	(0.023)
			(0.620)	0.648	(0.547)	(0.317)
Overlap x Delta Sale					0.442	(0.004)
					0.176	(0.016)
Meanindage			0.007	0.008	(0.008)	-
			0.298	0.344	(0.257)	(0.112)
Overlap x Meanindage					0.069	0.008
					1.124	1.230
Sharedstate			(0.631) *	(0.612) *	(0.809) *	(0.050) *
			(1.817)	(1.770)	(1.864)	(1.734)
Overlap x Sharedstate					0.035	(0.021)
					0.041	(0.288)
Dummy Mfg			(0.626)	(0.720)	(0.426)	(0.040)
			(0.477)	(0.537)	(0.270)	(0.328)
Overlap x Dummy Mfg					(1.543)	(0.239)
					(0.513)	(0.958)
Secrecy Mfg			0.004	0.006	0.004	-
			0.167	0.236	0.141	0.163
Overlap x Secrecy Mfg					0.005	0.001
					0.088	0.286
Constant	(2.338) ***	(1.899) ***	(2.077) ***	(2.983) ***	(2.365) ***	0.092 **
	(14.974)	(9.371)	(4.631)	(3.422)	(4.524)	2.202
N	623	623	623	623	623	623
R-square	0.014	0.048	0.092	0.092	0.123	0.067
Chi square	5.892 **	19.705 ***	37.918 ***	37.848 ***	50.588 ***	
F statistic						3.333 ***

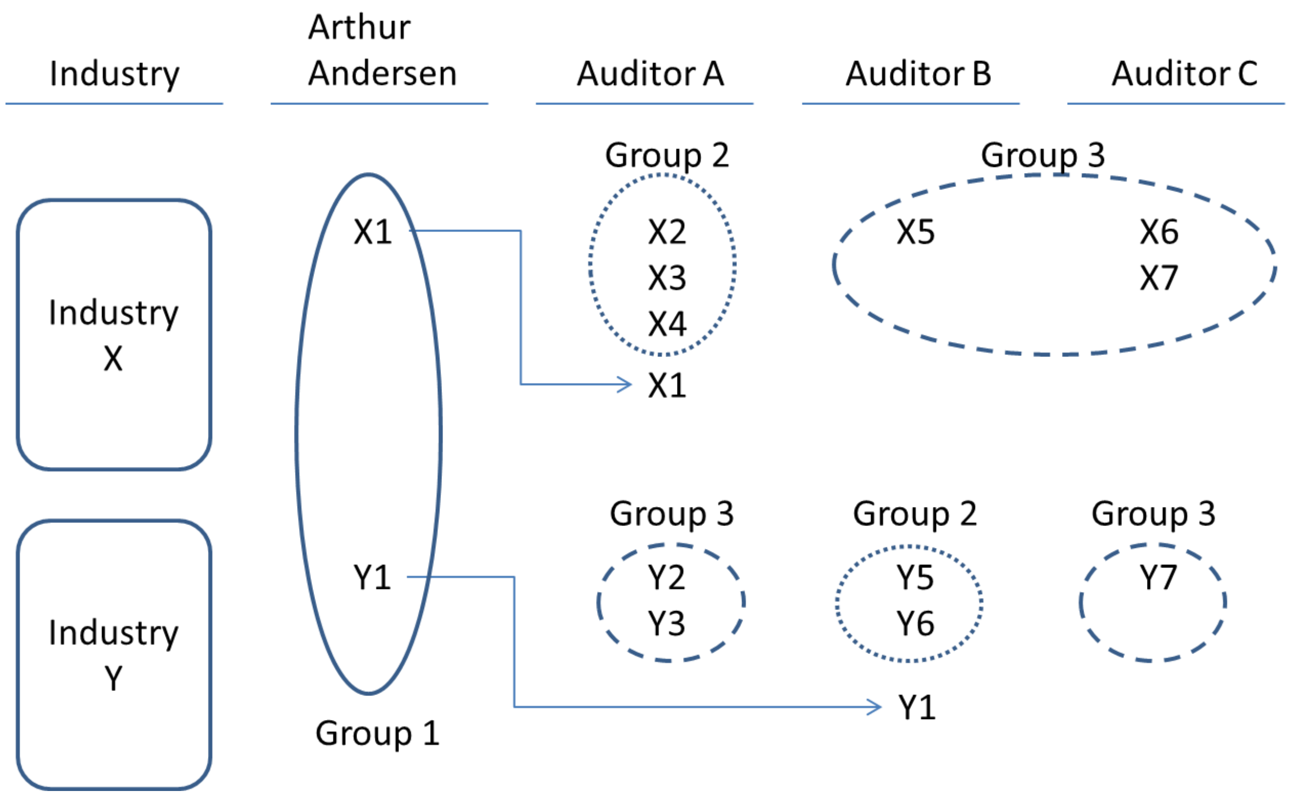
Table I.7: Audit Fee Analysis

Table I.7 presents an analysis where the dependent variable is equal to the natural logarithm of audit fees. The audit fee data is from the Compustat audit fee database, covers years from 2000 to 2006, and is winsorized at the 1^{st} and 99^{th} percentiles. Control variables include *Lta*, the log of assets, *Cata*, the ratio of current assets to total assets, *Quick*, the ratio of current assets less inventories to current liabilities, *De*, the ratio of long term debt to total assets, *Roi*, the ratio of earnings before interest and taxes to total assets and *Foreign*, the proportion of income from foreign operations. All prior control variables are winsorized at the 1^{st} and 99^{th} percentiles. Control variables also include *Lseg*, the log of business segments, *Opinion*, an indicator variable equal to one when the audit report is not unqualified, *Non Dec dummy*, an indicator variable equal to one when the year end is not December 31^{st}, and *Loss*, an indicator variable that equals one when net income is negative for the given year. In the first column, the proxy variable equals one when three top tier auditors were covering the top three industry firms prior to Arthur Andersen collapse. In column two, the proxy equals one when all the five top tier auditors were covering the top 5 firms. In column three the proxy equals one when all five top tier auditors were covering the top ten firms. In the fourth column, the proxy is equal to the number of top tier auditors covering the top three players prior to the collapse of Arthur Andersen, divided by three. Column five and sixth use the number of top tier auditors covering the top five players and top ten players, divided by five. The proxies are interacted with a dummy for after the collapse of Arthur Andersen in 2002. The specifications control for year fixed effects, number of companies within each industry fixed effects, and for fixed effects for the Fama French industry groups. The coefficients are given above and the t-statistic below. Standard deviations are clustered at the company level. Significance levels are * 10%, ** 5% and *** 1%.

	Top 3 Discrete Proxy	Top 5 Discrete Proxy	Top 10 Discrete Proxy	Top 3 Continuous Proxy	Top 5 Continuous Proxy	Top 10 Continuous Proxy
Proxy	0.009	(0.554) *	(0.225) ***	0.010	(0.029)	(0.206) **
	0.215	(1.940)	(5.816)	0.126	(0.277)	(2.573)
Interaction after AA collapse	0.065	0.373 **	0.096 ***	0.074	0.180 **	0.164 ***
	1.557	2.326	2.702	0.999	2.094	2.805
Control Variables						
Lta	0.546 ***	0.550 ***	0.551 ***	0.546 ***	0.549 ***	0.550 ***
	31.290	39.225	51.767	31.138	38.775	50.986
Lseg	0.079 **	0.077 **	0.084 ***	0.079 **	0.078 **	0.086 ***
	2.091	2.389	2.935	2.109	2.423	3.017
Cata	0.636 ***	0.634 ***	0.598 ***	0.637 ***	0.637 ***	0.597 ***
	5.153	6.459	7.545	5.152	6.465	7.457
Quick	(0.059) ***	(0.056) ***	(0.057) ***	(0.059) ***	(0.056) ***	(0.059) ***
	(9.845)	(10.236)	(10.505)	(9.816)	(10.163)	(10.534)
De	0.010	0.021	0.010	0.012	0.028	0.010
	0.109	0.269	0.151	0.124	0.357	0.156
Roi	(0.210) ***	(0.220) ***	(0.208) ***	(0.210) ***	(0.221) ***	(0.203) ***
	(3.716)	(4.940)	(5.711)	(3.695)	(5.037)	(5.584)
Foreign	0.220 ***	0.229 ***	0.233 ***	0.220 ***	0.231 ***	0.233 ***
	7.841	8.685	9.629	7.815	8.803	9.621
Opinion	0.096 ***	0.109 ***	0.120 ***	0.097 ***	0.106 ***	0.121 ***
	4.110	5.337	6.752	4.139	5.222	6.771
Non Dec dummy	(0.160) ***	(0.145) ***	(0.123) ***	(0.160) ***	(0.143) ***	(0.120) ***
	(3.963)	(4.327)	(4.489)	(3.963)	(4.281)	(4.359)
Loss dummy	0.157 ***	0.162 ***	0.191 ***	0.157 ***	0.162 ***	0.191 ***
	4.790	6.031	8.505	4.788	6.024	8.514
Constant	1.978 ***	1.931 ***	1.933 ***	1.981 ***	1.976 ***	2.014 ***
	9.510	10.328	11.395	9.490	10.286	11.523
Number firm dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Industry group dummies	Yes	Yes	Yes	Yes	Yes	Yes
N	5,215	6,768	8,799	5,215	6,768	8,799
Number clusters	1,349	1,731	2,247	1,349	1,731	2,247
Sample used	Top 3	Top 5	Top 10	Top 3	Top 5	Top 10
Adjusted R-square	0.794	0.803	0.808	0.794	0.803	0.807
F-statistic	145.818 ***	177.841 ***	225.405 ***	145.969 ***	178.146 ***	225.412 ***

Figure I.1: Arthur Andersen Collapse – Illustrative Example

Figure I.1 presents an illustrative example of the group definition for the analysis of fees related to the Arthur Andersen collapse. There are two industries, X and Y. Arthur Andersen initially covers firms X1 and firm Y1. Firm X1 moves to auditor A after the collapse, while firm Y1 moves to Auditor B after the Arthur Andersen collapse. These two firms are part of group 1. Group 2 includes firms where their auditor took for client one of the Arthur Andersen firm within their industry. Group 3 includes all other firms.



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II. ARE CLIENT COMPANY SECRETS SAFE WITH THEIR AUDITORS? INITIAL EVIDENCE

II.1. Introduction

Information spillovers are prevalent across firms. Major newspapers regularly mention lawsuits between rival firms because of information leakage originating from an employee transferring from one firm to another. Firms extensively employ non-compete agreements (Garmaise 2011) and often choose different suppliers than their rivals (Asker and Ljungqvist 2010, Aobdia 2011a) because of their concerns for information spillovers. The aim of this study is to investigate whether information spillovers occur among clients engaging the same auditor. Because audits are required and the supply of audit firms of the scale and quality to service public firms is small, my analysis is less likely to suffer from a selection bias in terms of supplier choice than studies involving other classes of suppliers over which firms have more discretion. A caveat to finding spillovers is the long term nature of auditor relationships, akin to a repeated game, raising the possibility that information spillovers go against the auditing firm's long-term interests (Demski, Lewis, Yao, and Yildirim 1999). However, the auditing firm is also composed of individual employees, who may have incentives to leak secrets or may leak them inadvertently, in the same fashion as former employees of firms may leak secrets to their new employer.

The major challenge of any study on information spillovers is that they are difficult to measure, because information spillovers usually do not leave any paper trail (Krugman 1991b). This is especially the case in the auditing industry. Given the existence of confidentiality agreements between the auditor and its clients, any knowledge spillover might constitute a breach of these agreements. Direct transfer of knowledge would also go against the AICPA code of professional conduct, in particular rule 301. Consequently, any leakage of information would have to be done discretely or indirectly, without documentation lest the paper trail be used in court against the auditor. In addition, the client is likely to be unable to monitor spillovers given turnover of audit firm personnel. Another possibility is that the client firm might not wish to discourage information spillovers that have a positive impact on industry-wide revenues. Even in case the client firm has some basis for suit spillovers may still be hard to detect given that auditors are likely to quietly settle to avoid potential reputational loss. Overall, these factors reduce the possibility of finding direct evidence of information spillovers[31].

My empirical strategy is therefore to use several variables that have been used in prior literature to proxy for information spillovers. The aim is to triangulate information spillovers by using these variables and to present evidence consistent with information spillovers. I first study financial variables that proxy for companies' investment, research and development, advertising, and SG&A decisions. Bertrand and Schoar (2003) used these variables to document that executive fixed effects matter when the executives transfer from one firm to another. Their paper can also be interpreted as transferring executives being conduits for information spillovers across firms. I first determine the discretionary part of companies' decisions by taking the residual from a first stage regression as a measure of discretion, and comparing these discretionary decisions across firms in a second stage test. Specifically, for each firm with a given auditor, I randomly select ten firms that share the same auditor during the same year and create ten pairs of firms sharing the same auditor. In order to prevent any concerns of endogeneity, due to the fact that

[31] In Section II.2 I provide direct anecdotal evidence of auditors leaking information from one client to another

same industry rivals are reluctant to share the same auditor (Aobdia 2011a), I choose the ten firms in different industries than the initial firm. I then match each of the ten firms that share the same auditor with a firm in the same industry and with similar sales that does not have the same auditor. This creates ten control pairs of the initial firm and each matched firm as a result. I then compare the similarity of decision results measured in financial terms of pairs having the same auditor to pairs having different auditors. Results indicate that the firms' decisions are closer when they share the same auditor, specifically, as evidenced by SG&A, R&D and advertising expenditures. Results also hold after the enactment of SOX, providing evidence towards the core auditing business of auditors being a conduit for information spillovers.

In order to alleviate any concern of omitted variable that could drive both auditor choice and closeness of financial decisions, I establish causality by using the collapse of Arthur Andersen in 2002 as a natural experiment. Internal conduits within Arthur Andersen for information spillovers were broken for former clients after the auditing firm collapsed, as client firms had to switch auditors. Using a difference in differences methodology, I find that firms' discretionary policies diverged after the collapse of Arthur Andersen when the two firms were initially covered by Arthur Andersen. I also find some evidence that firms decisions diverged more when the firms were covered by the same Arthur Andersen office, suggesting that information spillovers are stronger when client firms are covered by the same auditor office. Results are still consistent with information spillovers for firms sharing the same auditor but not the same office.

Next, I seek to establish a link that directly ties firms sharing the same auditor compared to firms not sharing the same auditor[32]. I therefore conduct an additional test using patents citations. The advantage of patent citations is that they leave a paper trail (Jaffe, Trajtenberg and Henderson 1993) of knowledge use that can be used to infer information spillovers, directly connecting the firm applying for a new patent to the firm that issued the cited patent (Jaffe, Trajtenberg and Fogarty 2002). The potential disadvantage of patent citations for this specific study is that it is unclear whether the auditor can directly transmit technological information from one client to another. However, the auditor has access to detailed patent information from its clients given that patents filing costs are capitalized under GAAP and as a result require periodic impairment tests. In addition, the auditor could still transmit some information of an economic or general technical nature that could be used by other client firms to build upon another client firm's technology or information. Consistent with this hypothesis, I find that the probability that two firms have shared the same auditor when one firm's patent cites the other firm's patent is higher than the probability that a control firm shares the same auditor with the cited firm. The control firm is chosen from the same industry as the citing firm in order to control for auditor industry expertise, and from the same geography and application year in order to control for normal determinants of patent citation. I also find stronger results when both citing and cited firm have their headquarters located in the same metropolitan statistical area (MSA), indicating that information spillovers are more prevalent when clients also share the same auditor office. Last, the evidence still holds after SOX, indicating that the core auditing business is a conduit for information spillovers.

[32] For example, one argument could be that the auditor guides the firms to similar decisions, without any information spillovers

My study contributes to several branches of the literature. First, to the best of my knowledge, it is the first to present empirical evidence of information spillovers through a shared supplier who is not an employee. It also complements Almeida and Kogut (1999) and Song, Almeida and Wu (2003) who document information spillovers from employee transfers. As well, the results support the premise of suppliers being a conduit for information spillovers in theoretical studies by Demski et al. (1999), Hughes and Kao (2001), Bönte and Wiethaus (2008), and Baccara (2007). My evidence corroborates the results of Asker and Ljungqvist (2010) and Aobdia (2011a) who document the reluctance of same industry rivals to share the same underwriter or auditor due to information spillover concerns. In addition, the stronger evidence on intra-office spillovers indicates a direct channel that is not employee turnover for the results of Jaffe et al. (1993) who document geographic localization of knowledge spillovers.

Second, my study also contributes to the professional network literature. In particular, auditors are shown to be another source of information transfer that has an impact on firms' decisions, complementing Bertrand and Schoar (2003) and Fracassi (2011) who report similar results for executive transfers and interlocking directors. A distinction is that, to the best of my knowledge, my study is the first to document that a supplier can link two companies together within a professional network.

Third, my paper also contributes to the auditing literature and the current debate on mandatory rotation. In particular, there exists a large literature on the impact of consulting services on audit quality[33] (e.g Defond, Raghunandan and Subramanyam 2002, Frankel, Johnson and Nelson 2002, Ashbaugh, Lafond and Mayhew 2003, Francis and Ke 2006, Lim and Tan

[33] This literature has suffered from the introduction of SOX in 2002 that prevented auditors from cross-selling most consulting services to their clients except for tax advice services

2008). One argument is that, within the same client, auditors can benefit from knowledge spillovers coming from the provision of non-audit services (Lim and Tan 2008). My results indicate that knowledge spillovers also take place across auditing clients. These results also contribute to the current debate on mandatory auditor rotation. The Public Company Accounting Oversight Board (PCAOB) decided on August 16, 2011, in release 2011-006, to explore potential mandatory rotation of auditors in order to increase auditor independence and audit quality. Mandatory auditor rotation leads to increased auditor switching by the clients that could lead to increased information spillovers across firms. This could upset the balance between a reluctance to share the same auditor due to spillover concerns and efficiencies gained from industry specialization by auditors. The risk of increased information spillovers could potentially have a negative impact on firms' innovation activities, or could result in firms restricting their auditor access to their proprietary information in order to limit the risk. The latter would result in lowered audit quality, going against the initial purpose of mandatory auditor rotation.

The paper is structured as follows. Section 2 introduces the main motivation and hypothesis development. I introduce tests based on accounting variables in section 3 and tests based on patent citation data in section 4. Section 5 concludes.

II.2. Motivation and hypothesis development

Auditors, as suppliers of services, are regularly in contact with a wide range of clients' proprietary information. This information, during the course of a typical audit engagement, can include terms of trade to main customers in order to verify accounts receivable, suppliers' terms to verify accounts payables, product or product line profit and loss information and country-level profit and loss information. Auditors also conduct plant visits to verify, for example, that inventory levels match the level indicated on the company balance sheet, and can have access to plant specific information. In particular, auditing of raw materials and work-in-process inventories, usually located at the heart of the plant, might put auditors in contact with potentially sensitive factory process information. Most notably, during the course of their conversations with company management, auditors might come in contact with additional information related to company strategy, including merger and acquisitions plans and product development and marketing plans. Overall, the information acquired is much more detailed than publicly disclosed company financial statements and could be useful to other firms.

Anecdotal evidence indicates that auditors have in the past directly transferred some of this information from one client to another. In a salient example, Grant Thornton, then Alexander Grant & Company, notified all its clients using the services of another client firm of potential concerns about the operations of the latter firm. In other examples, concerns related to collectability of accounts receivables or potential bribes were raised at one client because relevant information was collected from another client (see Werner 2009). In addition, as noted by practitioners themselves, industry specialist auditors may indirectly transfer some information from one client to another within the same industry as they apply some of the knowledge learnt at the former client to the latter client (McAllister and Cripe, 2008). Note that this indirect knowledge transfer does not go against the AICPA code of professional conduct rule 301 and as a result is unlikely to form a basis for suit.

Consulting services, cross-sold by the auditor, might also become another possibility for the auditors to acquire and leak additional information about their clients. For example, Andersen Consulting was hired by the Japanese motorcycle maker Yamaha in the 1970s. This allowed the consulting firm to develop expertise in both Japanese manufacturing techniques and the motorcycle industry. Andersen Consulting went on to market this expertise to Yamaha's rival Harley-Davidson in the US (O'Shea and Madigan 1998).

Several theoretical papers, including Demski et al. (1999) and Baccara (2007) explore this question with auditors and consultants specifically in mind and show that these suppliers are usually unable to commit not to pass on proprietary information. However, they model one-shot games, whereas the auditor-client relationship, very stable over time, is more of a repeated game nature. It is therefore possible that, consistent with the results of the multi-period theoretical model of Bönte and Wiethaus (2008), auditors have implemented several measures to limit the extent of information spillovers, possibly through specific employee assignments, data monitoring and restrictions on employees' exit employment.

Consequently, it is an empirical question whether auditors are a conduit for information spillovers of their clients. I state my first hypothesis as follows:

H1: Auditors are a conduit for information spillovers among their clients

One question is to determine which groups of firms are most likely to have information spillovers among them. Auditor organizations, especially the Big 4 auditing firms, are very large, including dozens of offices just in the U.S. with thousands of auditors. Consequently, the probability of an interaction between two auditors covering different firms, and the probability that the interaction results in information spillovers, seems quite limited at first. I propose two settings where information transmission may be more likely. First, the same auditor employee could be assigned to audit two different clients at the same time or at different times and could be the sole conduit of information spillovers across his clients. Second, information spillovers could occur if two employees come into frequent contact. In both cases, information spillovers are more likely to happen within the same office. Given the high number of auditing offices within the U.S., and their proximity to their clients' headquarter locations, both conditions are more likely to happen when clients are headquartered at the same location. This is the proxy I will use for shared auditor office[34]. I state my second hypothesis as follows:

> H2: Information spillovers are more likely to be present within the same auditor office, proxied by clients being headquartered at the same location

Another question of interest is to determine whether the auditor is a conduit for information spillovers when performing the core auditing business, or when performing additional consulting services. In particular, the largest auditors used to have large consulting branches. Consulting services were often cross-sold to client firms (e.g Lim and Tan 2008). Consultants often act as a conduit for information spillovers, because they often sell a product that is based on best practices (Baccara 2007). However, consulting activities tend to be one-off relationships, while auditing takes place continuously over time. In addition, the relative ease auditors had to cross-sell consulting services to their clients indicates that important information for consulting was likely to have been transmitted from the auditing side of the business to the consulting side of the business. The Sarbanes-Oxley act (SOX), enacted in July 2002, drastically restricted consulting services to existing auditing clients and required the approval of the client's audit committee for the services still allowed, including tax consulting. Finding evidence of information spillovers after SOX would be consistent with the hypothesis that the auditing business is also a conduit for information spillovers. I state my third hypothesis as follows:

[34] The attestation of the auditor on the form 10-K frequently indicates which Public Accountant office audited the client. I verified on several form 10-Ks that for major metropolitan statistical areas (MSAs) the auditor office auditing the client is from the same MSA. This is not necessarily the case for smaller MSAs. I control for this possibility by removing firms located in smaller MSAs from the sample.

H3: Information spillovers due to sharing auditors continued after enactment of SOX

II.3. Tests using financial decisions

First, I focus on accounting variables as a proxy for financial decisions in order to measure information spillovers among firms covered by the same auditor.

II.3.1 Research design

Pair model

The association based tests use a methodology comparable to Bertrand and Schoar (2003) and the pair model analysis of Fracassi (2011). I first regress selected accounting variables on control variables that have been identified as relevant in prior literature. Both accounting variables and control variables are taken from Bertrand and Schoar (2003) and attempt to measure decisions taken within the organization, including the level of investment, SG&A, R&D, and advertising expenditures. The residual is assumed to be the discretionary part of these decisions, while the predicted value is assumed to be the non-discretionary part of these decisions. Note that this first part of my methodology is comparable to the earnings management and earnings quality literature (see for example Dechow, Sloan and Sweeney 1994), where accruals are regressed on several control variables in order to determine non-discretionary and discretionary accruals. As a result, the methodology suffers from similar caveats as the earnings management and earnings quality literature.

I then compare the discretionary variables across companies using a pair model. Concretely, the unit of analysis is a pair of companies. The dependent variable is equal to the absolute value of the difference of the two firms' residuals. A lower value of this variable indicates that the two firms make more similar discretionary decisions.

I am interested in comparing pairs of firms that share the same auditor with pairs of firms that do not share the same auditor. Consequently, for each firm i, I randomly select ten firms j_1, j_2, …, j_{10}, that share the same auditor during the same year. Aobdia (2011a) finds that same industry rivals are reluctant to share the same auditor. Therefore, given potential endogeneity concerns of choosing firms within the same industry, I randomly select the ten firms j_1, j_2, …, j_{10} from different industries than that of the initial firm i[35]. I choose those firms so that the common shared time at the auditor is the minimum of five years or of the length of the relationship between firm i and the auditor. The aim is to have firms that have had a sufficiently long shared time together in order for information spillovers to be possible. For each of the firms $j_1,\ldots,j_{10}$, I then select a control firm that has a different auditor. The control firm is chosen within the same industry, as defined by the NAICS 4 digit codes, with sales as close as possible to the other firm. I also choose control firms that did not have the same auditor as the initial firm i for a period of at least five years, or for the entire period the initial firm i was covered by its current auditor in case the relationship between the initial firm i and the auditor was less than five years. The purpose is to have control firms that have not shared the same auditor as the initial firm i for a reasonably long period of time. This yields a control sample of ten firms, h_1, h_2,…, h_{10}. I then compare the difference in policies between firm i and firms $j_1,j_2,\ldots,j_{10}$ to the difference in policies between firm i and firms $h_1,h_2,\ldots,h_{10}$.

[35] The reason why I randomly select only ten firms that share the same auditor and not all the firms is to keep the dataset size manageable. Note that this sampling reduces the power of the tests.

First stage regression

$$Investment_{k,t} = \alpha + \beta_1.Logat_{k,t} + \beta_2.Tobinq_{k,t} + \beta_3.Cashflow_{k,t} + YearDummies + Indgroupdummies + \varepsilon_{k,t}$$
$$Otherpolicies_{k,t} = \alpha + \beta_1.Logat_{k,t} + \beta_2.Roa_{k,t} + \beta_3.Cashflow_{k,t} + YearDummies + Indgroupdummies + \varepsilon_{k,t}$$

$\varepsilon_{k,t}$, the residual of the first stage regression for company k for the year t, is the variable of interest that proxies for discretionary decisions of the firm. *Investment* is equal to capital expenditures deflated by beginning PP&E (net), and *Otherpolicies* include SG&A expenditures deflated by beginning assets, R&D expenditures deflated by beginning assets and Advertising expenditures deflated by beginning assets. *Logat* is the log of beginning assets, *Tobinq* is equal to the beginning market value of assets (assets less stockholders' equity plus market value of the stock at the end of the fiscal year) divided by the book value of asset, *Cashflow* is equal to income before extraordinary items plus depreciation deflated by the beginning book value of assets, and *Roa* is equal to income before extraordinary items deflated by the beginning book value of assets. I also include year dummies and Fama French industry group dummies as controls in the first stage regression.

Second stage analysis

$$Abs(\varepsilon_{i,t} - \varepsilon_{j,t}) - Abs(\varepsilon_{i,t} - \varepsilon_{h,t}) \geq 0 ?$$

Specifically, a negative difference in the differences of residuals would indicate that firms sharing the same auditor have closer financial policies compared to the control group where firms do not share the same auditor.

As a robustness check, I also conduct the second stage analysis by using the actual policies, and not their residuals from the first stage regression. The rationale for using the actual policies is that the control pair design already takes care of potential variations of non-discretionary policies due to size or industry.

Natural experiment: The collapse of Arthur Andersen

I use the collapse of Arthur Andersen as a natural experiment that broke the channels for information spillovers for former clients of Arthur Andersen[36]. Even though two firms initially covered by Arthur Andersen might have chosen the same auditor after the collapse of Arthur Andersen, the potential channels for information spillovers within the new auditor are likely to have been partly broken compared to the prior status at Arthur Andersen.

I take advantage of this natural experiment in my research design, using a difference in differences methodology. I take all pairs of firms initially covered by Arthur Andersen. For the control group, I take all pairs including one firm initially covered by Arthur Andersen and one firm initially not covered by Arthur Andersen. I then compare to the control group the absolute value of the difference of the residuals of the first stage regression one year before (year 2001) and one year after the collapse of Arthur Andersen (year 2003) for the pairs of firms both formerly covered by Arthur Andersen. Concretely, I perform the following regression:

$$\left|\varepsilon_{i,t} - \varepsilon_{k,t}\right| = \alpha + \beta_1.Andersenfirm_{i,k} + \beta_2.Andersenfirm_{i,k} \times Y2003 + \beta_3.Sameindustry_{i,k} \\ + \beta_4.Samemsa_{i,k} + \beta_5.Samestate_{i,k} + FixedEffects$$

[36] Note that in contrast to Aobdia (2011a), I cannot use auditor mergers as natural experiments, because client firms had the option to leave the combined auditor after the merger. Aobdia documents that overlapping rivals switched auditors more often, indicating that companies concerned about information spillovers left the combined auditor. This suggests that using auditor mergers as a test would lead to selection bias, potentially compromising the validity of the results. The selection bias is potentially large given switching rates of as much as 17%. This evidence is corroborated by direct survey evidence that indicates that many clients disapproved of the auditor mergers and considered changing auditor (see The Big Eight evaluations. by Fortune 1000 executives 1990 report)

The dependent variable is equal to the absolute value of the difference of the residuals taken one year after or one year prior to the Arthur Andersen collapse event. In practice, a value close to zero of the dependent variable indicates that the two firms' policies are close. Firm i is a firm formerly covered by Arthur Andersen while firm k is either a firm formerly covered by Arthur Andersen or by another auditor. *Andersenfirm* equals one when the two firms were initially covered by Arthur Andersen one year prior to the collapse. I also include a dummy for when two firms are within the same industry as defined by their NAICS 6 digit codes (*Sameindustry*)[37], when they are headquartered in the same MSA[38] (*Samemsa*) and in the same state (*Samestate*) and year fixed effects in the specifications. I also include pair fixed effects in some of the specifications. The variable of interest is the *Andersenfirm* variable and its interaction with the *Y2003* dummy, which takes the value one one year after the collapse of Arthur Andersen. I predict that β_1 is negative as two firms initially covered by Arthur Andersen should have closer decisions than two firms covered by two different auditors. I also predict that β_2 is positive as the decisions of two firms covered by Arthur Andersen prior to its collapse should have begun to diverge due to an absence of information spillovers after the collapse of Arthur Andersen.

II.3.2 Data Construction and First stage regressions

Data is obtained from Compustat. I keep firms with auditor information, data on the first stage regression control variables and investment figures. I also eliminate ADRs and subsidiaries, as ADRs auditors may be located abroad, reducing the potential for information spillovers, and subsidiaries are often very similar to their parent companies, which may bias the results of my

[37] The dummy controls for potential endogeneity for same industry rivals. In any case, results are unchanged when excluding same industry rivals from the specifications

[38] The MSA, or Metropolitan Statistical Area, is a proxy for two firms headquartered in the same location.

tests. The MSA data comes from the U.S. census bureau[39]. I match each company's headquarter location to its MSA using its ZIP code and keep only the firms with non-missing MSAs. This restricts the sample to firms headquartered in the U.S.. I then keep only those firms located in MSAs where at least ten public firms are headquartered for a given year. This reduces the number of MSAs to between 70 and 100 each year, corresponding roughly to the number of auditing offices in the U.S. for each large audit firm. This restriction becomes necessary for my tests of increased information spillovers from sharing the same auditor office (H2), in order to reduce noise in the specifications. It is also confirmed by sampling several form 10Ks that indicate that companies headquartered in large MSAs are covered by an auditing office located in the same MSA, while companies headquartered in smaller MSAs are sometimes covered by auditing offices located in other areas[40]. The final sample is reduced to 96,814 firm years, spanning from 1974 to 2010, used for the first stage regression in the analysis.

Table II.1 panel A presents descriptive statistics and Table II.1 panel B presents the result of the first stage regressions. The first stage regressions, presented in Table II.1 panel B, are in line with prior literature (see for example Fracassi 2011), with comparable coefficients and t-statistics. Even though this technique has no econometric impact on the calculation of the residuals, I clustered standard errors at the firm level as both dependent and independent variables are relatively stable over time within each firm. Note that the explanatory power of the regressions is relatively high, but differs widely depending on the policy. For example the adjusted R-square for the investment regression is 0.17, leaving significant room for discretionary investment, while the adjusted R-square for the R&D regression is 0.52, indicating

[39] See http://www.census.gov/population/www/metroareas/metroarea.html

[40] Public Accountants, in their opinion form included in client firms' 10Ks, often disclose the location of the auditor office that conducted the analysis – This allows determining whether the Public Accountant office that audited the client firm is located in the same area as the client firm

that the independent variables explain an important portion of the regression. Also note that the number of observations differs across regressions because I did not impose any additional restriction on the sample besides not missing investment figures[41].

II.3.3 Pair model results

Initial results

Results are presented in Table II.2. I cluster standard errors at the level of the initial firm i in the specification as each firm i is matched to ten randomly selected firms and therefore the standard errors could be correlated across pairs including the initial firm i. For each of the decisions considered, I also present the results using the residuals of the decision from the first stage regression, and the results using the actual value of the policy without a first stage regression. The reason is that the control firm methodology partially alleviates potential concerns of having to use the residual only because the control firm is chosen within the same industry and with similar sales as the initially randomly selected firm.

Overall, results presented in Table II.2 are consistent with pairs of firms sharing the same auditor having closer decisions than the control pairs of firms not sharing the same auditor. Decisions are closer for R&D, advertising and SG&A. The differences are significant at 1% or better. Results are unchanged when using residuals of decisions from the first stage regression or actual decisions. These results are consistent with the auditors being a conduit for information spillovers (H1). However, results are insignificant for the investment decision (first and second

[41] I replaced missing values of R&D or advertising with zero for the firms in industries, as defined by the NAICS 6 digit codes, that reported positive advertising or R&D expenditures. The aim is to avoid a selection bias on these variables. Results are not sensitive to excluding data points with missing observations.

column). This could be due to a lack of power of the test or investment being a weak variable to proxy for similar decisions across firms.

In terms of economic significance, the difference in decisions between firms sharing the same auditor and firms not sharing the same auditor represents 0.5% to 1.1% of the total decision. For example, in the case of SG&A, the difference of 0.0013 represents approximately 0.5% of the control sample SG&A value, 0.2515. These results are consistent with information spillovers conveyed by the auditor being present without being obviously noticeable either.

Sarbanes-Oxley results

Next, I partition my sample between the years prior to Sarbanes-Oxley and the years after. Results are presented in Table II.3 and are mostly unchanged for the sample of pairs prior to SOX compared to the sample of pairs after SOX. The difference in the differences in investment decisions between the pair and its control is insignificant for both before and after SOX, while the differences in R&D and SG&A decisions are significantly negative for both before and after SOX. Results are slightly weaker for advertising after SOX compared to SOX. Even though the advertising decisions of the pair are closer than those of the control pair, as evidenced by a negative difference in the differences of decisions, the results are insignificant when using the residual of the first regression of advertising, and marginally significant when using the actual advertising decisions. Overall, though, the results indicate that firms' decisions are closer when firms share the same auditor compared to firms that do not share the same auditor. Results mostly hold for the post-SOX period, indicating that the core auditing business is the source of some of the information spillovers across client firms.

II.3.4 Results from the collapse of Arthur Andersen

Initial results

The results above, despite their strength, could be still driven by an omitted variable that drives both auditor choice and companies similarity. For example, Fracassi (2011) documents that companies with interlocking board of directors have more similar financial policies. Interlocking board of directors or another omitted variable could influence both financial policies and auditor choice. Consequently, I need a natural experiment to confirm my results and establish causality of the auditor being a conduit for information spillovers. The collapse of Arthur Andersen provides such an experiment. Results are presented in Table II.4. Panel A presents results for the residuals of the first regression. For all decisions, I find that, consistent with my predictions, financial policies are closer for firms sharing the same auditor, namely Arthur Andersen, than for firms not sharing the same auditor, as evidenced by a negative coefficient on *Andersenfirm* (see column 1 for each policy). Also consistent with my predictions, I find for all policies that the interaction coefficient between *Andersenfirm* and *Y2003* is significantly positive, indicating that the financial decisions of firms initially covered by Arthur Andersen began to diverge after the collapse of Arthur Andersen, compared to the control sample of firms that did not share the same auditor over the period.

I confirm the strength of my results by conducting an even more restrictive difference in differences test. I include pair dummies in all my specifications. Results are presented in the second column for each policy. Due to the inclusion of pair dummies, the coefficient on *Andersenfirm* is not identified. However, its interaction with *Y2003* is significantly positive, confirming the results of Column 1 that indicates that, compared to the control group, firms that were covered together by Arthur Andersen had financial decisions that began to diverge after the collapse of the auditing firm.

Panel B presents the results for the actual decisions and not their residuals. Results are unchanged compared to Panel A and confirm that firms that shared Arthur Andersen prior to its collapse had closer decisions that began to diverge after the collapse of Arthur Andersen, compared to the control group of firms not sharing the same auditor.

Intra-office information spillovers

I test the hypothesis that information spillovers are increased when two clients share the same auditor office (H2) in Table II.5. In particular, I interact the *Samemsa* dummy with the *Andersenfirm* dummy and the *Y2003* dummy. I predict that the coefficient on this interaction should be positive. This would indicate that companies that were covered by Arthur Andersen and that were headquartered in the same MSA had their financial decisions diverging more after the collapse of Arthur Andersen than firms headquartered in different MSA, because the intra-office information spillovers were higher than inter-office information spillovers. Because I use pair fixed effects in my model, I do not need to interact *Samemsa* with *Andersenfirm*, as the interaction is not identified. However, to control for potential changes in company policies after the collapse of Arthur Andersen when firms had their headquarters located in the same MSA, regardless of whether they share the same auditor or not, I need to interact *Samemsa* with *Y2003*. The interaction corresponds to the change in decisions in 2003 compared to 2001 for firms that did not share the same auditor over this period.

Panel A presents the results for the residual of policies. I find a positive coefficient on the interaction $Samemsa \times Andersenfirm \times Y2003$, significant at 5%, for all policies except for the investment policy. This indicates that financial decisions of firms headquartered in the same area that were covered by Arthur Andersen diverged more than firms covered by Arthur Andersen headquartered in different areas, consistent with more information spillovers occurring in the same auditor office (H2). Results are still significant at 1% for the coefficient on the interaction $Andersenfirm \times Y2003$, indicating that information spillovers are not limited to the same auditor office but also occur across different offices within the same auditor. Panel B presents similar analyses for the actual decisions and not their residuals. Contrary to Panel A, the coefficient on the interaction $Samemsa \times Andersenfirm \times Y2003$ is not significant. This could indicate that the results presented in Panel A are relatively weak, or that taking the residuals instead of the actual policies increases the power of the tests because the residuals pinpoint discretionary decisions, while actual policies include a non-discretionary component that creates additional noise in the regressions.

II.4. Tests using patents citations

The evidence presented above is consistent with the auditor being a source of information spillovers (H1). However, I have yet to present a direct link between two firms that are covered by the same auditor. In this section, I take advantage of the patent citation database, the strongest "paper trail" that is available to researchers studying information spillovers.

I note that auditors may not have the technical expertise to directly transmit technical information related to their clients' patents. However, auditors have access to detailed patent information from their clients, given that patent filing costs are capitalized under GAAP, and as a result must develop some patent related expertise, for at least some of their clients with large patent portfolios. In addition, auditors' proximity to the firms' innovations, technology and processes may lead them to transfer information related to those, even though the information might be of a more general nature. It is then a matter of the client benefiting from information spillovers to build upon the initial direction provided by the auditor. I also note that economic information can be used for innovation purposes. For example, Renault wrongfully laid off three of its employees because they had allegedly sold economic information about the company's electric car program (WSJ, March 19 2011) to a potential rival[42]. In this case, it seems that economic information would have been sufficient for the rival to infer the technology used by Renault for its electric car program.

II.4.1 Research design

I adapt the research design of Jaffe et al. (1993), who document the existence of information spillovers at a geographic level using patent citation data. A patent citation indicates that the new patent builds upon existing knowledge from the cited patent. Even though patent citations can be added directly by the inventor and by third parties including the patent attorney or the patent examiner, direct survey evidence indicates that patent citations are a noisy indicator of knowledge flows and communication between the inventor of the cited patent and the inventor of the citing patent (Jaffe et al. 2002). Consequently, a citation represents a "paper trail" for knowledge spillovers (see Hall et al. 2001).

The main idea of the patent analysis is to compare the auditor of the citing patent firms with the auditor of the cited patent firms. For the comparison to be meaningful, the probability that the auditor of the citing firm is equal to the auditor of the cited firm needs to be compared to

[42] The managers turned out not to have sold any information and were exonerated of any suspicion

a baseline probability of matching, taken from a control patent that is not citing the original patent. The role of the benchmark probability is to control for other factors that could impact the probability that the citing and cited patents share the same auditor, including auditor market share movements and auditor industry specialization. I predict that the probability that the citing firm and cited firm share the same auditor is higher than the probability that the control firm originating the control patent and the originally cited firm share the same auditor. If the auditor is a conduit for information spillovers, then firms sharing the same auditor are more likely to cite each other's patents than firms not sharing the same auditor. Mathematically, if A is the firm with a patent citing one of B's patents, and C is a firm that does not cite B's patent, I predict that *Probability(A and B share the same auditor / A cites B) > Probability(C and B share the same auditor / C does not cite B)*. Using the Bayes rule, this inequality is equivalent to *Probability(A cites B / A and B share the same auditor)>Probability(C cites B / C and B do not share the same auditor)*, which might be a more direct way to visualize knowledge spillovers through the auditor.

Several considerations impacted the construction of the benchmark non-citing patent. First, the auditing market and overall auditors' market shares evolve over time, indicating the need for a benchmark patent issued at roughly the same time as the citing patent. Second, I need to control for auditor specialization in my tests. Last, tests of H2 (information spillovers more likely to go through the auditor office) require not only controlling for the existing spatial distribution of technological activity, as in Jaffe et al. (1993), but also controlling for geographic information spillovers that do not go through a shared auditor office. Consequently, for each citing patent, I determine a control patent not citing the original patent within the same technological subcategory[43], that has the same application year, and that comes from the same industry as the citing patent, with the industry defined at the NAICS 4 digit code. In case of multiple matching patents, I choose a matching patent from the same state, in order to control for local information spillovers not coming from the same auditor office, with the control firm's sales as close as possible to the citing patent firm sales.

II.4.2 Data

The patent information and patent citation information comes from the NBER patent project[44] (see Hall et al. 2001 for additional details). The files include all utility patents issued in the US between 1976 and 2006, as well as all citations to these patents. In addition, the assignees for each patent are matched to the Compustat Gvkey codes, allowing me to track the owner of each patent over time and the auditor of the patent's owner. The initial file has 2.8M patent-assignee (a limited number of patents have more than one assignee) and 24.6M citing-cited patent pairs. I merge the patent and citing data to Compustat, reducing the sample to 5.6M citing-cited patent pairs[45], and then build a control patent for each citing patent as detailed above. An important restriction is that control firms must be in the Compustat database, because the auditor information is necessary. I also restrict control firms to have sales similar to the citing firm. In practice, I restrict control firms to have sales between 50% and 200% of the sales of the citing firm. This leaves 2.4M observations. Last, I require Compustat data to have auditor information for all citing, cited and control firms. I also eliminate all self-citations because self-citations

[43] The technological subcategory used is the one defined in the NBER patent project database, and is less granular than the main patent classes defined by the USPTO. In section 4.4, I confirm that most results hold when matching the patents at a more granular level, despite a significant reduction in the sample size.
[44] Data is available at https://sites.google.com/site/patentdataproject/
[45] The reason for the important reduction of pairs is that many patents are developed outside of public firms, including research universities, private firms and individual inventors

introduce a bias in finding the results. This further reduces the sample to its final size of 1,259,202 citing-cited pair observations.

II.4.3 Results

Initial results

Initial results are presented in Table II.6 panel A. In the first three columns, the shared auditor variable equals one when the citing and cited firms or the benchmark and cited firms shared their auditor for at least one year over the past three years prior to the patent application. The first column includes the entire sample. I find that the citing and cited firms share the same auditor 26.10% of the time, compared to the benchmark probability of 23.89%. The difference of 2.21% is statistically significant at the 1% level or better. Note that in order to compute the standard deviations, standard errors were clustered at the citing-benchmark firm pair level, as observations tend to be repeated over time in the sample and firm-auditor relationships are relatively sticky over time.

Recall, that Aobdia (2011a) documents that same industry rivals are reluctant to share the same auditor. Consequently, there might be concerns of endogeneity for citing patents that are in the same industry as the cited patents. I therefore split the sample in two. The second column only includes citing patents in different industries from the cited patents. The results are similar as in the first column. On the other hand, the third column only includes citing patents within the same industry as the cited patent. The results disappear here, consistent with the predictions based on Aobdia (2011a).

Columns 4 to 6 are similar to column 1 to 3 except that the shared auditor variable equals one when the citing and cited firms or the benchmark and cited firms shared their auditor for at least one year over the past five years prior to the patent application. Results are broadly unchanged compared to columns 1 to 3.

I then split the sample within three subperiods, based on the application time of the cited patents. Results, presented in panel B, are consistent over the three subperiods, for both shared auditor variables. Overall, the results presented in Table II.6 are consistent with the auditor being a conduit for information spillovers (H1). In particular I find that firms with patents linked by a citation are more likely to share the same auditor compared to the control group.

Tests of intra-office information spillovers

Table II.7 presents test results confirming increased information spillovers when two clients share the same auditor office (H2). I split the sample into two, one where both citing and cited firms have headquarters located within the same MSA and two, where they are within different MSAs. Note that in order to control for potential information spillovers through the same MSA, I need to have a benchmark firm also located within the same MSA. Also, given some concerns of endogeneity when firms are within the same industry, I restrict my analysis to citing firms in different industries than the cited firms. These additional restrictions reduce the sample to 895,714 observations. Column 1 presents results for when both citing-cited firms are within the same MSA and column 2 for when the firms are within different MSAs. I note that for both columns the citation matching percentage is higher than the benchmark level, indicating that information spillovers occur within the same auditor office and across different offices of the same auditor. However, statistical and economic significance is higher for the sample of firms within the same MSA. A t-test of differences indicates that the difference of the differences is statistically significant, indicating a higher amount of information spillovers for when firms are located within the same MSA when compared to different MSAs. Results are similar for both shared auditor variables and are consistent with the results presented in Table II.5.

Sarbanes-Oxley results

I finally test whether information spillovers occur solely through the consulting arm of the auditor or also through the core auditing business (H3). I split my sample between citing patents that had years of application of 2003 or later, compared to those patents that had years of application before SOX. I also exclude same industry patents, due to potential concerns of endogeneity. Results are presented in Table II.8 panel A. Compared to before SOX, results are unchanged after SOX, indicating that information was still leaked even after the restriction of consulting services and consistent with the core auditing business being a conduit for information spillovers. One caveat for these results is that my sample of patent citations finishes in 2006. This means that only three years were available after SOX was passed to assess whether the auditor was still a conduit for information spillovers. In particular, another interpretation of the results could be that, given that it takes time to develop a new invention, client firms still benefited from information spilled over by the consulting arms of the auditors prior to SOX. In this case, my results could only capture this legacy spillover and not new information spillovers that occurred after SOX was passed. I can still test for new information spillovers by comparing cited patents issued before and after SOX. Results are presented in Table II.8 panel B and are consistent with the results in panel A. One issue, though, is that results after SOX are based on only 762 observations. This indicates that there might be a need to confirm these results in future research.

II.4.4 Robustness tests

Thompson and Fox-Kean (2005) present a critique of the Jaffe et al. (1993) methodology. In particular, they argue that the matching patent should be selected from the same technology class and subclass as the original citing patent. Even though doing so could lead to selection bias, as noted by Henderson, Jaffe and Trajtenberg (2005) in their comment of Thompson and Fox-Kean (2005), I still conduct robustness tests by choosing the matching patent within the same technological class and subclass, as defined by the USPTO, as the original citing patent. Results are presented in Table II.9. The matching sample is significantly reduced, with a final sample of roughly 1/5th of the original sample. This sample reduction is comparable in magnitude to the sample reduction in Thompson and Fox-Kean (2005).

Results are practically unchanged compared to Tables II.6 to II.8. In particular, results still hold for the overall sample and for the samples prior and post SOX, as detailed in Panels A, C and D. However, the analysis on MSAs is less conclusive than in Table II.7. In particular, the results, presented in Panel B, go in the same direction, but are insignificant for firms headquartered in the same MSA, with a p-value of 10.7%.

II.5. Conclusion

This paper has presented initial evidence that auditors are a conduit for information spillovers. I find that firms sharing the same auditor have closer real financial policies. Additional evidence from patents indicates that information spillovers from sharing an auditor is useful in mimicking innovations, even though the auditor may not have sufficient expertise to directly transmit information of a technical nature. Last, I also confirmed that information spillovers continued after the enactment of SOX, consistent with the core auditing business being a conduit for information spillovers, and I also presented evidence that information spillovers increase when two firms share the same auditor office, compared to two firms that share the same auditor but are covered by different offices.

Table II.1: Descriptive Statistics and First Stage Regressions

Table II.1 Panel A presents descriptive statistics for the estimation sample. Table II.1 Panel B presents the first stage regressions. *Investment* equals capital expenditures deflated by beginning PP&E (net), *Randratio* equals R&D expenses deflated by beginning assets, *Adratio* equals advertising expenses deflated by beginning assets, *Sga* equals SG&A expenses deflated by beginning assets. *Logat* is the log of beginning assets, *Tobinq* equals the beginning market value of assets (assets less stockholder's equity plus market value of the stock at the end of the fiscal year) divided by the book value of assets. *Cashflow* equals income before extraordinary items plus depreciation deflated by the beginning book value of assets. *Roa* equals income before extraordinary items deflated by the beginning book value of assets. Standard deviations are clustered at the firm level in Panel B, with significance levels * 10%, ** 5% and *** 1%.

Panel A

variable	N	mean	sd	p25	p50	p75
Investment	96,814	0.374	0.473	0.125	0.229	0.424
Randratio	71,250	0.075	0.134	-	0.018	0.094
Adratio	72,906	0.020	0.046	-	-	0.019
Sga	94,293	0.335	0.316	0.098	0.264	0.474
Logat	96,814	5.099	2.132	3.571	4.955	6.547
Tobinq	96,814	2.010	1.833	1.043	1.385	2.170
Cashflow	96,814	0.029	0.246	0.008	0.082	0.142
Roa	96,814	(0.029)	0.253	(0.040)	0.034	0.084

Panel B

	Investment		Rand		Ad		Sga	
Logat	(0.029)	***	(0.005)	***	(0.001)	***	(0.044)	***
	(24.727)		(10.588)		(2.774)		(31.482)	
Tobinq	0.079	***						
	37.262							
Cashflow	0.144	***	0.067	***	0.047	***	0.778	***
	10.547		3.651		6.207		15.576	
Roa			(0.262)	***	(0.042)	***	(0.884)	***
			(14.557)		(5.762)		(18.166)	
Constant	0.362	***	0.077	***	0.028	***	0.479	***
	30.218		29.511		12.816		54.664	
Year dummies	Yes		Yes		Yes		Yes	
FF ind group dummies	Yes		Yes		Yes		Yes	
N	96,814		71,250		72,906		94,293	
Number Clusters	11,173		8,922		9,730		11,018	
Adj R-square	0.17		0.52		0.19		0.37	
F-statistic	105.40	***	46.57	***	9.40	***	54.12	***

Table II.2: Analysis of Pair Difference

Table II.2 presents a comparison of policies, including Investment, R&D, SG&A and Advertising, between pairs of firms sharing the same auditor and a control group of firms not sharing the same auditor. For each firm i, I randomly select 10 firms that share the same auditor and compare the difference of the policies of these firms with the difference of policies between firm i and control firms having a different auditor that are chosen within the same industry and have similar sales than the initially randomly selected firms. Standard deviations are clustered at the initial firm i level. Both the residual of the policies from the first stage regressions of Table 1 and the actual policies are used in the analysis below. Significance levels are * 10%, ** 5% and *** 1%.

	Residual Investment	Investment	Residual R&D	R&D
Same auditor - Same auditor sample	0.3378	0.3519	0.0875	0.1092
Same auditor - Control sample	0.3374	0.3512	0.0883	0.1104
Difference	0.0003	0.0007	(0.0009) ***	(0.0012) ***
t-statistic	0.73	1.41	(5.92)	(5.67)
Number of observations	731,431	731,431	389,049	389,049

	Residual Advertising	Advertising	Residual SG&A	SG&A
Same auditor - Same auditor sample	0.0319	0.0314	0.2502	0.3149
Same auditor - Control sample	0.0321	0.0318	0.2515	0.3171
Difference	(0.0002) ***	(0.0003) ***	(0.0013) ***	(0.0022) ***
t-statistic	(2.81)	(3.73)	(4.42)	(6.56)
Number of observations	391,849	391,849	691,234	691,234

Table II.3: Analysis of Pair Difference and SOX

Table II.3 presents an analysis similar to Table II.2. The sample is split between observations that took place before SOX and observations that took place after SOX. Standard deviations are clustered at the initial firm i level as in Table II.2. Both the residual of the policies from the first stage regressions of Table II.1 and the actual policies are used in the analysis below. Significance levels are * 10%, ** 5% and *** 1%.

	Residual Investment	Investment	Residual R&D	R&D
Difference before SOX	0.0005	0.0007	(0.0007) ***	(0.0008) ***
t-statistic	0.92	1.12	(4.49)	(3.53)
Number of observations	547,669	547,669	295,368	295,368
Difference after SOX	(0.0001)	0.0009	(0.0013) ***	(0.0024) ***
t-statistic	(0.12)	0.89	(4.01)	(5.06)
Number of observations	183,762	183,762	93,681	93,681

	Residual Advertising	Advertising	Residual SG&A	SG&A
Difference before SOX	(0.0003) ***	(0.0003) ***	(0.0012) ***	(0.0013) ***
t-statistic	(3.08)	(3.24)	(3.47)	(3.32)
Number of observations	277,415	277,415	515,464	515,464
Difference after SOX	(0.0000)	(0.0002) *	(0.0016) ***	(0.0049) ***
t-statistic	(0.14)	(1.94)	(2.92)	(7.47)
Number of observations	114,434	114,434	175,770	175,770

Table II.4: Collapse of Arthur Andersen Analysis

Table II.4 presents a difference in differences analysis. The unit of analysis is a pair of firms. The dependent variable equals the absolute value of the difference in policies between each firm composing the pair. *Sameindustry* equals one when two firms are in the same industry as defined by their NAICS 6 digit code. *Samemsa* equals one when the two firms are headquartered in the same location. *Samestate* equals one when the two firms are headquartered in the same state. *Andersenfirm* equals one when both firms composing the firm initially had Arthur Andersen as a shared auditor. *Y2003* equals one for the year 2003 and is interacted with *Andersenfirm* as the coefficient of interest in the specification. Standard deviations are clustered at the pair level. Both the residual of the policies from the first stage regressions of Table 1 and the actual policies are used in panel A and panel B. Significance levels are * 10%, ** 5% and *** 1%.

Panel A: Residual of policies	Predicted Sign	Investment 1	Investment 2	Research and Development 1	Research and Development 2	Sales General and Administrative 1	Sales General and Administrative 2	Advertising 1	Advertising 2
Sameindustry	?	0.107 ***	(0.032)	0.033 ***	(0.013) **	0.059 ***	(0.011)	(0.008) ***	0.013 ***
		29.12	(1.44)	29.12	(2.08)	23.20	(1.07)	(21.50)	5.18
Samemsa	?	(0.041) ***		(0.005) ***		0.000		0.001 ***	
		(18.84)		(6.43)		0.06		2.82	
Samestate	?	0.072 ***		0.011 ***		0.014 ***		(0.001) ***	
		37.95		20.01		13.48		(3.28)	
Andersenfirm	-	(0.052) ***		(0.013) ***		(0.009) ***		(0.001) ***	
		(39.96)		(34.94)		(13.31)		(6.52)	
Andersenfirm x Y2003	+	0.032 ***	0.032 ***	0.013 ***	0.012 ***	0.012 ***	0.013 ***	0.002 ***	0.002 ***
		23.11	16.32	28.11	16.67	19.64	14.63	9.67	8.87
Year Dummies		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pair Dummies		No	Yes	No	Yes	No	Yes	No	Yes
Clustering		Pair level	Pair level	Pair level	Pair level	Pair level	Pair level	Pair level	Pair level
Adjusted R-squared		0.0442	0.1672	0.0066	0.3735	0.0033	0.5560	0.0035	0.5631
F-statistic		23,001.49 ***	22,563.82 ***	1,844.98 ***	1,551.75 ***	2,547.43 ***	1,917.84 ***	1,501.20 ***	1,293.98 ***
N		2,680,314	2,680,314	1,409,184	1,409,184	2,571,360	2,571,360	1,333,680	1,333,680

Panel B: Actual policies	Predicted Sign	Investment 1	Investment 2	Research and Development 1	Research and Development 2	Sales General and Administrative 1	Sales General and Administrative 2	Advertising 1	Advertising 2
Same Industry	?	0.110 ***	(0.015)	0.025 ***	(0.001)	0.005 *	0.016	(0.008) ***	0.014 ***
		25.94	(0.57)	16.40	(0.11)	1.84	1.37	(22.55)	5.42
Same MSA	?	(0.066) ***		(0.008) ***		0.002		0.002 ***	
		(27.36)		(6.11)		1.38		4.35	
Same State	?	0.086 ***		0.012 ***		(0.002)		(0.001) ***	
		39.40		13.04		(1.49)		(4.30)	
Andersenfirm	-	(0.078) ***		(0.022) ***		(0.015) ***		(0.002) ***	
		(49.41)		(33.43)		(16.92)		(10.87)	
Andersenfirm x Y2003	+	0.059 ***	0.059 ***	0.009 ***	0.006 ***	0.013 ***	0.013 ***	0.002 ***	0.003 ***
		35.31	24.95	16.18	6.93	17.11	12.43	11.49	10.11
Year Dummies		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pair Dummies		No	Yes	No	Yes	No	Yes	No	Yes
Clustering		Pair level	Pair level	Pair level	Pair level	Pair level	Pair level	Pair level	Pair level
Adjusted R-squared		0.0614	0.1459	0.0070	0.6242	0.0026	0.5930	0.0013	0.6257
F-statistic		31,340.46 ***	30,899.73 ***	3,275.04 ***	3,955.58 ***	2,516.19 ***	2,483.99 ***	543.39 ***	433.15 ***
N		2,680,314	2,680,314	1,409,184	1,409,184	2,571,360	2,571,360	1,333,680	1,333,680

Table II.5: Andersen Collapse – Auditor office analysis

Table II.5 presents an analysis similar to Table 4, with the *Samemsa* coefficient being interacted with the *Andersenfirm* and the *Y2003* variables in order to test for differences in information spillover at the auditor office level. Variables have been defined in Table II.4. Standard deviations are clustered at the pair level. Both the residual of the policies from the first stage regressions of Table II.1 and the actual policies are used in panel A and panel B. Significance levels are * 10%, ** 5% and *** 1%.

Panel A: Policy residuals

	Investment		R&D		SG&A		Advertising	
Sameindustry	(0.032)		(0.013)	**	(0.011)		0.013	***
	(1.45)		(2.07)		(1.06)		5.18	
Samemsa x Y2003	(0.017)	***	0.002	*	-		(0.001)	
	(3.97)		1.79		(0.11)		(1.55)	
Andersenfirm x Y2003	0.033	***	0.012	***	0.012	***	0.002	***
	16.09		15.83		13.82		8.23	
Samemsa x Andersenfirm x Y2003	(0.003)		0.008	**	0.012	**	0.003	**
	(0.34)		2.24		2.57		2.15	
Year Dummies	Yes		Yes		Yes		Yes	
Pair Dummies	Yes		Yes		Yes		Yes	
Clustering	Pair level		Pair level		Pair level		Pair level	
Adjusted R-squared	0.1670		0.3730		0.5560		0.5630	
F-statistic	13,540.25	***	934.04	***	1,152.45	***	776.74	***
N	2,680,314		1,409,184		2,571,360		1,333,680	

Panel B: Actual policies

	Investment		R&D		SG&A		Advertising	
Sameindustry	(0.015)		(0.001)		0.016		0.014	***
	(0.57)		(0.10)		1.37		5.42	
Samemsa x Y2003	0.005		0.005	***	0.001		-	
	1.09		3.09		0.33		(0.56)	
Andersenfirm x Y2003	0.059	***	0.006	***	0.013	***	0.003	***
	24.45		6.55		12.17		9.55	
Samemsa x Andersenfirm x Y2003	(0.003)		0.004		-		0.002	
	(0.26)		1.02		0.01		1.61	
Year Dummies	Yes		Yes		Yes		Yes	
Pair Dummies	Yes		Yes		Yes		Yes	
Clustering	Pair level		Pair level		Pair level		Pair level	
Adjusted R-squared	0.1460		0.6240		0.5930		0.6260	
F-statistic	18,540.57	***	2,379.75	***	1,490.75	***	260.32	***
N	2,680,314		1,409,184		2,571,360		1,333,680	

Table II.6: Patent Citation Analysis

Table II.6 presents a patent citation analysis where the analysis level is the number of citations of patents of firms within the Compustat database by firms within the Compustat database. I compare the proportion of citations of firms sharing the same auditor for at least one year during the three or five years prior to the application of the cited patent to the benchmark probability level. Panel A presents results for the whole sample and the sample split by whether citing and cited firms are within the same industry, while Panel B presents results for three subperiods split by the application year of the cited patent. The benchmark is obtained from finding a matching patent to the citing patent that is not citing the cited patent. Standard deviations are clustered at the citing-benchmark pair. Significance levels are * 10%, ** 5% and *** 1%.

Panel A

	Shared auditor within past 3 years			Shared auditor within past 5 years		
	All	**Different industry**	**Same industry**	**All**	**Different industry**	**Same industry**
Number of citations	1,259,202	1,019,174	240,028	1,259,202	1,019,174	240,028
Citation matching percentage	26.10	26.17	25.80	27.72	27.62	28.12
Benchmark percentage	23.89	22.87	28.24	25.36	24.24	30.13
Difference	**2.21**	**3.30**	**(2.45)**	**2.35**	**3.38**	**(2.01)**
t-statistic	2.99 ***	2.61 ***	(0.70)	3.18 ***	2.61 ***	(0.57)

Panel B

	Shared auditor within past 3 years			Shared auditor within past 5 years		
	1985 and before	**1986-1995**	**1996 and after**	**1985 and before**	**1986-1995**	**1996 and after**
Number of citations	270,903	515,461	232,810	270,903	515,461	232,810
Citation matching percentage	20.94	25.24	34.30	22.45	26.30	36.55
Benchmark percentage	19.11	22.31	28.48	20.75	23.36	30.23
Difference	**1.83**	**2.94**	**5.82**	**1.70**	**2.94**	**6.32**
t-statistic	2.14 **	2.21 **	2.85 ***	1.98 **	2.17 **	3.01 ***

Table II.7: Patent Citation Analysis and geographic distribution

Table II.7 presents results similar to Table II.6 with the sample partitioned between whether citing and cited patents firms are headquartered within the same MSA or not. The sample is restricted to citing-cited pairs with firms in different industries. There is a further restriction that the matching patent needs to be in the same MSA as both citing and cited patents when analyzing the same MSA subsample, while the matching patent needs to be in a different MSA as the cited patent when analyzing the different MSA subsample. The benchmark is obtained from finding a matching patent to the citing patent that is not citing the cited patent. Standard deviations are clustered at the citing-benchmark pair. Significance levels are * 10%, ** 5% and *** 1%.

	Shared auditor within past 3 years				Shared auditor within past 5 years			
	Same MSA		Different MSA		Same MSA		Different MSA	
Number of citations	25,557		870,157		25,557		870,157	
Citation matching percentage	28.36		24.96		31.42		26.24	
Benchmark percentage	21.06		22.40		23.25		23.67	
Difference	**7.31**		**2.56**		**8.17**		**2.57**	
t-statistic	2.88	***	1.91	*	3.51	***	1.91	*
t-statistic Same - Different MSA	**1.66**	*			**2.08**	**		

Table II.8: Patent Citation Analysis and SOX

Table II.8 presents results similar to Table II.6 with the sample partitioned between patents that were applied to before SOX and patents that were applied to after SOX. Panel A splits the sample based on the application year of the citing patent, while Panel B splits the sample based on the application year of the cited patent. The sample is restricted to citing-cited pairs with firms in different industries. The benchmark is obtained from finding a matching patent to the citing patent that is not citing the cited patent. Standard deviations are clustered at the citing-benchmark pair. Significance levels are * 10%, ** 5% and *** 1%.

Panel A: Citing patent application year

	Shared auditor within past 3 years				Shared auditor within past 5 years			
	Before SOX		After SOX		Before SOX		After SOX	
Number of citations	934,902		84,272		934,902		84,272	
Citation matching percentage	25.27		36.17		26.50		40.01	
Benchmark percentage	22.49		27.00		23.70		30.15	
Difference	**2.77**		**9.17**		**2.80**		**9.86**	
t-statistic	2.43	**	2.63	***	2.47	**	2.44	**

Panel B: Cited patent application year

	Shared auditor within past 3 years				Shared auditor within past 5 years			
	Before SOX		After SOX		Before SOX		After SOX	
Number of citations	1,018,412		762		1,018,412		762	
Citation matching percentage	26.16		40.29		27.61		42.26	
Benchmark percentage	22.86		30.18		24.23		32.55	
Difference	**3.30**		**10.10**		**3.38**		**9.71**	
t-statistic	2.61	***	2.54	**	2.60	***	2.56	**

Table II.9: Patent citation analysis - Robustness tests

Table II.9 presents an analysis similar to Table II.6, Table II.7 and Table II.8. However, in this table, the benchmark patent is obtained from finding a matching patent that has the same class and subclass than the original citing patent, as in Thompson and Fox-Kean (2005). This significantly reduces the sample size in comparison to Tables II.6 to Table II.8. Panel A presents an analysis similar to Table II.6, while Panel B presents an analysis similar to Table II.7. Panel C and Panel D present an analysis similar to Table II.8. Standard deviations are clustered at the citing-benchmark pair. Significance levels are * 10%, ** 5% and *** 1%.

Panel A: Citation analysis

	Shared auditor within past 5 years		
	All	**Different industry**	**Same industry**
Number of citations	205,024	146,261	58,763
Citation matching percentage	31.08	32.07	28.61
Benchmark percentage	28.05	26.32	32.37
Difference	**3.03**	**5.75**	**(3.76)**
t-statistic	4.32 ***	2.96 ***	(1.06)

Panel B: Geographic distribution

	Shared auditor within past 5 years	
	Same MSA	**Different MSA**
Number of citations	4,486	122,284
Citation matching percentage	34.17	30.78
Benchmark percentage	30.05	25.04
Difference	**4.12**	**5.75**
t-statistic	1.62	2.83 ***
t-statistic Same - Different MSA	**(0.50)**	

Panel C: SOX - Citing patent application year

	Shared auditor within past 5 years	
	Before SOX	**After SOX**
Number of citations	131,702	14,559
Citation matching percentage	30.70	44.42
Benchmark percentage	25.33	35.21
Difference	**5.37**	**9.21**
t-statistic	2.94 **	2.42 **

Panel D: SOX - Cited patent application year

	Shared auditor within past 5 years	
	Before SOX	**After SOX**
Number of citations	146,106	155
Citation matching percentage	32.05	49.03
Benchmark percentage	26.31	35.48
Difference	**5.74**	**13.55**
t-statistic	2.95 ***	2.19 **

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