# Knowledge Transfer in Audit Firms[*]

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Abstract

We utilize the setting of the audit market consolidation in China over the 1998–2013 period to study knowledge transfer in audit firms. We employ a difference-in-difference approach and examine the effect of industry-specific knowledge transfer on audit performance after a merger of two audit firms with different levels of expertise in a particular industry. For clients in an industry audited by both merging audit firms, those audited by the less competent audit firm in that industry belong to the treatment group, while all other clients belong to the control group. We find an economically significant improvement in audit quality (as reflected in a reduction in client misstatements) for the treatment group relative to the control group in the same merged audit firm. In contrast to across-audit-firm comparisons in prior research, our within-audit-firm comparison allows us to eliminate all common effects at the audit firm level and partly attribute the audit performance improvement of the treatment group to industry-specific knowledge transfer after the merger.

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## 1. Introduction

Audit firms are knowledge-intensive organizations (Starbuck 1992), and they can derive competitive advantage by developing and transferring knowledge internally (Gibbins and Wright 1999; Gibbins and Jamal 2001; Gaver and Utke 2019). To perform an efficient and effective audit, auditors must possess knowledge along several dimensions, such as general domain knowledge of accounting and auditing standards, subspecialty knowledge related to specific industries or clients, and general business knowledge (Libby and Luft 1993; Nelson and Tan 2005; PCAOB 2015). While prior research generally finds that auditor competencies, particularly industry specialization, improve audit quality, a criticism of this literature is that it makes strong assumptions about the mechanisms through which auditor competencies improve audit quality (DeFond and Zhang 2014, p. 303). For example, in conducting across-audit-firm comparisons, researchers inevitably assume that an audit firm's industry expertise developed from auditing a particular client necessarily benefits the audits of other clients in the same industry (DeFond and Zhang 2014, p. 303). In this paper, we use the setting of audit firm mergers to more directly examine industry-specific knowledge transfer within an audit firm, and provide evidence on whether and to what extent it leads to audit performance improvement.

We build on a large literature in organizational behavior research that examines organizational learning and knowledge management (e.g., Argote 1999; Argote and Ingram 2000; Argote, Ingram, Levine, and Moreland 2000; Tsai 2001; van Wijk, Jansen, and Lyles 2008; Phelps, Heidl, and Wadhwa 2012).[1] We focus on the transfer of industry-specific knowledge rather than general knowledge because doing so allows us to perform a within-audit-firm comparison that

[1] For evidence on the transfer of auditing or management accounting practices, see, for example, Kurunmäki (2004), Barrett, Cooper, and Jamal (2005), Cooper and Robson (2006), Ahrens and Chapman (2007), and Cruz, Scapens, and Major (2011).

eliminates all common effects at the audit firm level. To investigate the effect of industry-specific knowledge transfer on audit performance, we use the setting of audit firm mergers. This setting has three main desirable features.

First, a merger removes or at least substantially dismantles the organizational boundaries between the merging firms, and thus represents an economic shock to the channels through which knowledge transfer between the merging firms occurs.[2] Post-merger integration often involves the development of information technology (e.g., knowledge databases, group support systems, or intranets) that facilitates knowledge sharing and communication. To achieve unified quality control, the merged firm usually develops audit routines that combine the best practices of the merging firms.[3] Training and gathering organized by the merged firm also provide a platform for audit personnel originally employed by different firms to establish personal relationships, share experience, and interact. Moreover, employment affiliation fosters the development of social ties through homophily (i.e., an affinity for similar others), which, in turn, enhances mutual trust and facilitates the transfer of tacit knowledge (Rogers and Bhowmik 1970; Granovetter 1985).[4]

[2] Existing literature shows that units are more likely to learn best practices from units in the same organization than from units in a different organization (Argote, McEvily, and Reagans 2003). Our maintained hypothesis in this paper is that the formal and informal channels for knowledge transfer between the merging audit firms are stronger after the merger than before the merger. We rely on this time-series variation induced by the merger event to avoid the daunting task of developing empirical measures of all the (observable and unobservable) channels for knowledge transfer.

[3] The Chinese Institute of Certified Public Accountants (CICPA) required merged audit firms to achieve post-merger uniformity of personnel, finance, services, technology standards and information management. The CICPA was also committed to providing special training and instructions to merged audit firms with respect to unified management and quality control.

[4] An auditor's industry-specific knowledge can be both explicit and tacit (Tan and Libby 1997; Vera-Muñoz, Ho, and Chow 2006). While there is conceptual distinction between explicit and tacit knowledge, these two types of knowledge are not separate or discrete in practice and they jointly determine audit performance. Explicit or technical knowledge (or "know-what") can be captured and stored, and is often transferred through formal channels facilitated by information technology. For example, an audit firm can develop a portable set of good practices through experiments, documentation, and subsequent validation (Gendron, Cooper, and Townley 2007), and this set of practices can then be implemented at the audit firm level (Bédard 1989; Power 1996). Tacit or procedural knowledge (or "know-how") manifests itself in terms of intuition, insights, or skills. It cannot be easily articulated or stored, and is typically transferred through personal interactions (Bol, Estep, Moers, and Peecher 2018). The crucial role of procedural knowledge has been well established in organizational behavior research and is expressed in the adage that "the effects of what you do depend on how you do it" (Brockner, Chen, Mannix, Leung, and Skarlicki 2000, p. 138). Knechel

Second, a merger of two audit firms with different levels of expertise in a particular industry results in industry knowledge heterogeneity in the merged audit firm. This heterogeneity allows us to identify a set of recipient units in knowledge transfer (i.e., the treatment group). Because an audit firm often has clients in multiple industries, those units not in this particular industry can serve as the control group. For illustration, consider a merger between two hypothetical audit firms as in Figure 1: firm A specializes in the mining industry; firm B is a non-specialist, and has clients in both the mining industry and the entertainment industry. Firms A and B merge to form firm AB. The question is whether and to what extent the transfer of firm A's knowledge about the mining industry to firm B leads to an improvement in the audit quality for firm B's clients in the mining industry (i.e., the treated clients) after the merger. Firm A's clients in the mining industry and/or firm B's clients in the entertainment industry can serve as the control group, which allows us to perform a difference-in-difference analysis.[5]

Finally, while merger decisions are endogenously determined at the firm level, audit quality is measured at the individual client level rather than the audit firm level. Thus, our empirical tests can exploit across-client variation while removing common factors that affect all units in the merged audit firm (e.g., auditor incentives captured by auditor size) through fixed effects. Our within-merged-audit-firm comparison between the treatment and control groups allows us to partly attribute the audit performance improvement of the treatment group to industry-specific knowledge transfer after the merger.[6] This research design also differentiates our study from

(2000, p. 706) also notes that "this knowledge is rarely documented and often difficult to link to specific assertions or audit risks, it is nevertheless vital for conducting an efficient and effective audit."

[5] Figure 2 depicts a more complicated situation: firm A has clients in two industries: (a) mining, and (b) electronics; firm B has clients in three industries: (a) mining, (b) electronics, and (c) entertainment. Firm A specializes in the mining industry while firm B specializes in the electronics industry. In this case, firm A's clients in the electronics industry and firm B's clients in the mining industry belong to the treatment group, while all other clients belong to the control group.

[6] We do not claim that the improved audit performance for the treated clients is *entirely* driven by knowledge transfer. As discussed later in this section and Section 5, auditor competencies are not independent of their incentives, and audit

existing research on the economic consequences of audit firm mergers (Chan and Wu 2011; Gong, Li, Lin, and Wu 2016; Choi, Kim, and Raman 2017; Jiang, Wang, and Wang 2019), which often compares merged audit firms with other audit firms not involved in mergers and is not able to partly tease out the effect of knowledge transfer on audit quality.

The above three features of this setting enable us to examine the effect of industry-specific knowledge transfer on audit performance after the merger.[7] While technical knowledge in a codified form may be shared relatively easily (Empson 2001), audit performance depends crucially on one's expertise to interpret and apply this knowledge (Ericsson, Charness, Feltovich, and Hoffman 2006; Ericsson, Prietula, and Cokely 2007; Westermann, Bedard, and Earley 2015). Consistent with differences in the style and application of audit routines (Power 2003), individual performance heterogeneity within the same audit firm is well documented in prior research (Lennox and Wu 2018).[8] Thus, it remains an empirical question as to whether and to what extent audit firm mergers lead to audit performance improvement by facilitating knowledge transfer.

We utilize a large sample of audit firm mergers in China over the 1998–2013 period to study knowledge transfer in audit firms. Unlike the U.S. audit market, which is dominated by Big N auditors, China's audit market is quite fragmented among domestic audit firms and the international Big N audit firms. However, there has been a trend toward consolidation in the audit market due in part to China's rapid economic development and regulatory changes (see Appendix

performance reflects the joint competency and incentive effects. We conduct extensive empirical analysis to ensure that our results are not *purely* driven by the incentive effect.

[7] It is also worth noting that the knowledge heterogeneity between firm A and firm B does not automatically translate into knowledge transfer from A to B, especially for tacit knowledge. Knowledge transfer requires A's dissemination and B's assimilation, both of which depend on one's ability, willingness, effort and opportunity (Szulanski 1996). Simon (1973, p. 270) has long noted that "the scarce resource is not information; it is processing capacity to attend to information."

[8] See also Libby (1981), Bonner and Lewis (1990), Libby and Tan (1994), Gibbins and Swieringa (1995), Bonner (2008), Nelson (2009), Gul, Wu, and Yang (2013), Aobdia, Lin, and Petacchi (2015), Ke, Lennox, and Xin (2015), Knechel, Vanstraelen, and Zerni (2015), Li, Qi, Tian, and Zhang (2017), and He, Kothari, Xiao, and Zuo (2018).

A for a detailed discussion of the institutional background). We have a sample of 46 mergers that took place over the 1998–2013 period, in which both merging audit firms had a license to audit listed companies in China.[9]

For each merger, we sort all client companies into the treatment and control groups based on the relative industry expertise of their auditors in the year before the merger. Following prior literature (e.g., Balsam, Krishnan, and Yang 2003; Chin and Chi 2009), an audit firm's industry expertise is measured by its industry market share (based on the number of listed clients in that industry).[10] We require the distance of the industry market share rank between the two audit firms to be at least five to classify one firm as being more competent than the other firm.[11] For clients in an industry audited by both merging audit firms, those audited by the less competent audit firm in that industry belong to the treatment group, while all other clients belong to the control group. One merging audit firm can be more competent in one industry while less competent in another industry than the other merging audit firm (see Figure 2).

We employ a difference-in-difference approach and examine whether the audit quality for the treatment group improves after the merger (three-year post-merger versus three-year pre-merger), relative to that for the control group.[12] Our empirical tests focus on the within-merged-firm variation by including merger fixed effects. This design allows us to examine across-client variation while removing all of the effects at the merged audit firm level. We use client

[9] Our results are robust when we remove the three mergers involving the international Big N (see Section 4.2.2).
[10] Our results are robust with alternative definitions of treated clients that take into account the within-audit firm industry portfolio share or client size (see Section 4.2.1). Archival auditing research commonly uses industry market share to measure expertise and generally finds consistent evidence (DeFond and Zhang 2014). To the extent that industry market share does not accurately reflect expertise, our classification of treated clients based on this variable can lead to an underestimation of the true treatment effect and bias against finding any audit performance improvement.
[11] Our results are robust if we drop this minimal separation requirement (see Section 4.2.1).
[12] The pre-merger data of clients that switch to other auditors after the merger are excluded to ensure that our results are not driven by changing client compositions. Our results are robust when we augment the sample with the pre- and post-merger data of those dropped clients (see Section 4.2.3).

misstatements as our main measure of audit quality (e.g., Chin and Chi 2009).[13] This measure has relatively low measurement error and offers relatively strong evidence of poor audit quality as misstatements are directly under the auditor's influence (DeFond and Zhang 2014).[14]

Using a logistic regression model, we find lower audit quality for the treatment group relative to the control group prior to the merger, and an economically significant improvement in audit quality for the treatment group relative to the control group after the merger. Our results are insensitive to the choice of the control group (i.e., clients in the same industry as the treated clients or not). To ensure that audit personnel movement does not drive our results, we repeat our analyses in a restricted sample in which both the engagement and review partners for a client company belong to the client's audit firm before the merger, and find similar results. In addition, a standard dynamic test shows that treated and control clients exhibit similar trends in audit quality before the merger, supporting the identifying assumption of our difference-in-difference test that treated and control clients follow parallel trends absent industry-specific knowledge transfer.

As DeFond and Zhang (2014) note, an important caveat to any archival research on auditor competencies is that auditor competencies are not independent of their incentives. Greater competencies in supplying high quality audits can increase an auditor's reputation capital, which, in turn, can lead to greater incentives to deliver high quality audits. Similarly, greater incentives

[13] A higher-quality audit in period *t* will reduce the likelihood of an accounting misstatement in period *t*. We use restatement data disclosed in subsequent years to identify a client's misstatement in period *t*. Prior auditing research commonly uses the occurrence of misstatements rather than the discovery of misstatements as a measure of audit quality because misstatements can be uncovered by parties other than the auditor, and the likelihood of misstatement discovery is also affected by the amount of uncorrected misstatements in the first place.

[14] Our main results hold when we use modified audit opinions (MAOs) as an alternative measure of audit quality. Following Wang, Wong, and Xia (2008), we classify unqualified opinions with an explanatory paragraph, qualified opinions, disclaimers, and adverse opinions as modified opinions. We do not use MAOs as our main measure because (1) MAOs are typically used to examine specific questions related to auditor independence (Chan and Wu 2011) and (2) MAOs may also indicate excessive auditor conservatism (DeFond and Zhang 2014). Unlike going-concern opinions only issued to financially distressed clients in the United States (Butler, Leone, and Willenborg 2004), modified opinions in China are sometimes issued to profitable clients with questionable accounting practices (Chen, Chen, and Su 2001). We break down MAOs into accounting-related opinions and uncertainty-related opinions (Ke, Lennox, and Xin 2015; Lennox, Wang, and Wu 2018) and show that our main results hold for both types of MAOs.

to deliver high quality audits can motivate auditors to develop greater competencies. Thus, the improved audit performance documented in our paper can be partly explained by enhanced auditor incentives after the merger. Disentangling the relative magnitude of the competency versus incentive effect is difficult as these two effects are intertwined. Nevertheless, it is important to ensure that our documented results are not *purely* driven by the incentive effect.[15]

We conduct four additional analyses. First, we show that the change in audit performance (as measured by client misstatements) does not occur immediately after the merger, consistent with a gradual process of knowledge development (as in Gaver and Utke 2019) instead of a rapid incentive effect. Second, we restrict the benchmark group to control clients that are more similar to the treated clients in terms of audit quality prior to the merger. Specifically, we include merging audit firm fixed effects (instead of merger fixed effects) in the analysis to examine across-client variation within the same merging audit firm (i.e., firm A or firm B, instead of firm AB). We further remove from the benchmark group those relatively strong units whose audit quality has little room for improvement, and retain only those relatively weak units in other industries whose audit quality can be improved. Our inferences remain unchanged with these alternative benchmark groups. Third, we decompose treated clients into two groups based on their initial audit firm size and show that audit performance improves for both groups. The improved audit quality for treated clients in the relatively big audit firm presents relatively strong evidence of knowledge transfer since it is less likely that the small audit firm can alter the big audit firm's incentives to improve audit performance after the merger. Fourth, we find an increase in audit fees for both the treatment

[15] Prior research commonly focuses on auditor incentives at the audit firm level. For example, larger auditors have stronger incentives to maintain independence because of higher reputation and litigation risk. Because we include merger fixed effects in our analysis, our results cannot be driven by enhanced auditor incentives common to the treatment and control groups in the same merged audit firm. It is possible that the merged audit firm has stronger incentives to improve the audit performance of the treatment group versus the control group (a "bright lights" effect). If these incentives are related to anticipated or realized knowledge transfer, then our documented results reflect the joint incentive and competency effects as noted before.

and control groups after the merger but the difference in fee increases between them is quite small and not statistically significant, suggesting that the audit performance improvement of the treatment group relative to the control group is unlikely to be purely driven by auditor incentives induced by differential reputation or litigation risk.

Two limitations of our study are worth mentioning. First, the audit market in China may be quite different from those in other countries. The economic and regulatory forces underlying China's audit market consolidation may be unique and our documented effect of knowledge transfer on audit performance may hinge critically on these forces. Thus, removing organizational boundaries to create knowledge transfer between different audit firms may not necessarily lead to audit performance improvement in a different economic and regulatory environment. Second, mergers are endogenously determined by audit firms, and can be driven by the perceived benefits of knowledge transfer on audit performance. Thus, the audit performance improvement of the treatment group relative to the control group documented in our study does not necessarily reflect the *causal* effect of audit firm mergers on audit quality changes, and our results may not hold in a randomized merger of audit firms.

The rest of the paper is organized as follows. In Section 2, we contextualize our study within the auditing literature and discuss its contributions. Section 3 explains our sample and research design. Section 4 presents the main results and robustness checks, and Section 5 provides supplemental analyses. Finally, we conclude in Section 6.

## 2. Related Literature

Our study makes three main contributions to the auditing literature. First, it responds to the call of DeFond and Zhang (2014, p. 278) "for more research on the role of auditors' competencies in driving audit quality" and contributes to the literature on knowledge development and transfer

in audit firms.[16] Using audit firm mergers as a shock to the channels through which knowledge transfer occurs, we provide large-sample evidence that industry-specific knowledge transfer leads to economically significant audit performance improvement after the merger. A closely related paper, Gaver and Utke (2019), finds that audit firms in their second or third consecutive year as an industry leader often produce the same level of audit quality as seasoned specialists.[17] Their results based on across-audit-firm comparison suggest that industry-specific knowledge is created within an audit firm during the first two to three years after the audit firm becomes the industry market share leader (due to, for example, client growth or industry entry by clients). Consistent with Gaver and Utke (2019), we find that the change in audit performance after the merger takes more than one year to occur. The advantage of the audit firm merger setting is that it allows us to specifically identify the recipient units in knowledge transfer (i.e., the treatment group) and conduct a within-audit-firm comparison that eliminates all common effects at the audit firm level. Hence, we provide more direct evidence on the economic consequences and speed of knowledge transfer *within an audit firm*, compared with prior research that largely relies on across-audit-firm comparison.

Second, our study contributes to the large body of research that examines whether audit quality is related to auditor industry expertise and auditor size (DeAngelo 1981; Watts and Zimmerman 1986; DeFond and Zhang 2014). The literature on auditor industry specialization assumes that expertise developed from auditing one client benefits audits of other clients in the same industry. We provide evidence supporting that industry-specific knowledge transfer across personnel and clients in merged audit firms leads to an economically significant audit performance

[16] See, for example, Simunic (1984), Trotman (1985), Trotman and Yetton (1985), Danos, Eichenseher, and Holt (1989), Ramsay (1994), Asare and McDaniel (1996), Kennedy, Kleinmuntz, and Peecher (1997), Kinney, Palmrose, and Scholz (2004), Knechel and Sharma (2012), Kadous, Leiby, and Peecher (2013), Causholli, Floyd, Jenkins, and Soltis (2017), and Bol, Estep, Moers, and Peecher (2018).

[17] Related research investigates an auditor's learning curve on a specific client (Gul, Fung, and Jaggi 2009; Cameran, Francis, Marra, and Pettinicchio 2015; Causholli 2016; Cassell, Hansen, Myers, and Seidel 2019).

improvement. Moreover, the endogenous matching of clients and auditors (e.g., Chaney, Jeter, and Shivakumar 2004) is less of a concern in our setting (than in prior literature) because we exploit across-client variation within the same audit firm. Our difference-in-difference approach (as opposed to a cross-sectional analysis) also alleviates the concern that different client characteristics drive the results (Lawrence, Minutti-Meza, and Zhang 2011; Minutti-Meza 2013).[18] Our evidence suggests that industry-specific knowledge sharing in large audit firms enhances their competencies, which, in turn, improve their audit quality.

Third, our study contributes to a recent literature on the evolution of the audit market (e.g., Watts and Zuo 2016; Ferguson, Pinnuck, and Skinner 2018) and responds to the call of Donovan, Frankel, Lee, Martin, and Seo (2014) for more research on the forces that explain changes in audit quality overtime. Most related to our study are Chan and Wu (2011) and Gong, Li, Lin, and Wu (2016) who use the same setting of audit firm mergers in China. Chan and Wu (2011) find that a merger of two firms licensed to audit listed clients results in higher audit quality (reflected as more modified audit opinions) in the *first* year after the merger takes place. They attribute this *immediate* audit quality improvement to changes in audit firms' aggregate quasi rents at stake and independence rather than to any change in their competence that takes more time to occur as we document. Gong, Li, Lin, and Wu (2016) document a significant reduction in audit hours, unaccompanied by a deterioration in audit quality, of merged audit firms. They interpret these results as evidence of economies of scale arising from horizontal mergers. Unlike our within-merged-audit-firm comparison, both of these studies compare the change in audit performance or

[18] For discussions on the Heckman (1979) selection model and the propensity-score matching method, see Angrist and Pischke (2009), Lennox, Francis, and Wang (2012), DeFond, Erkens, and Zhang (2016), Shipman, Swanquist, and Whited (2017), and King and Nielsen (2019). The identifying assumption of our difference-in-difference test requires that treated and control clients follow parallel trends absent industry-specific knowledge transfer. While this assumption is not directly testable, we provide its empirical support based on pre-trends (see Section 4.2.4).

audit effort between merged audit firms and other audit firms without mergers. Thus, their documented results can be driven by a host of audit-firm-level factors. As mentioned before, the advantage of our approach is that it allows us to remove all of the effects at the merged audit firm level (through fixed effects) and attribute our results to industry-specific knowledge transfer within the merged audit firm (through classification of clients within the merged audit firm into the treatment and control groups).

Three related studies examine the economic consequences of audit firm mergers in the U.S. setting. All of them again focus on the comparison between merged audit firms and other audit firms, and none of them is able to attribute the audit performance improvement to knowledge transfer. Choi, Kim, and Raman (2017) examine the 1998 merger of Price Waterhouse and Coopers & Lybrand into PricewaterhouseCoopers (PwC) and document a significant improvement in audit quality at PwC's overlapping offices over that of other Big N local offices. Choi, Kim, and Raman (2017, p. 1075) acknowledge that they are unable to either "reject the null that the change in audit quality at PwC overlapping offices is equal to the change in audit quality at PwC non-overlapping offices" or "identify the precise factor underlying the observed audit quality increase at the overlapping offices." Our larger sample of 46 mergers and within-audit-firm comparison allow us to document lower audit quality for the treatment group relative to the control group prior to the merger as well as an economically significant improvement in audit quality for the treatment group relative to the control group after the merger. Our extensive analysis suggests that knowledge transfer is the potential underlying force driving our results.

Jiang, Wang, and Wang (2019) use the setting of Big N auditors' acquisitions of non-Big N auditors in the United States, and documents an improvement in audit quality for the acquired non-Big N auditors relative to other non-Big N auditors. While they provide strong evidence on

the Big N effect driven by both auditor incentives and general competencies (DeFond and Zhang 2014), their cross-sectional analysis fails to reject the null that the change in audit quality is the same for all acquired clients regardless of the acquiring Big N auditor's competencies in a specific industry. Thus, their results do not speak to the effect of industry-specific knowledge transfer on audit performance. In contrast, our comparison is between treated and control clients in the same audit firm, and our evidence suggests that removing organizational boundaries facilitates industry-specific knowledge transfer and improves audit quality. In addition, our results are not driven by the Big N effect as our inferences are unchanged after removing the three mergers involving the international Big N.

A recent working paper by Christensen, Smith, Wang, and Williams (2018) examines the audit quality effects of small audit firm mergers in the United States. They find a decline in audit quality for the acquiring firms' legacy clients after the merger (compared with that of clients in small audit firms not involved in any mergers and acquisitions). They interpret their results as evidence suggesting that the acquiring firms are distracted from maintaining the audit quality of their existing client base after the merger. Similar to other studies, their across-audit-firm analysis does not allow them to tease out the effect of within-audit-firm knowledge transfer on audit quality, which our approach aims to do.

## 3. Research Methods

### *3.1.Sample and Data*

We collect data on audit firm mergers from the CICPA, audit firms' official websites, and leading financial newspapers. We obtain client companies' financial statement data and audit opinion data from the China Stock Market and Accounting Research (CSMAR) database. To identify misstatements, we collect subsequent restatement data from the "Material Accounting

Errors" section of financial statement footnotes and exclude restatements due to changes in accounting standards or tax rules, mergers and acquisitions, or other issues unrelated to accounting irregularities. Prior misstatements are discovered by corporate officers, auditors, audit committees, or regulators, and are often corrected within a year after the original statement is reported.[19]

Our sample consists of 46 mergers over the 1998–2013 period, in which both merging audit firms had a license to audit listed companies in China.[20] Our sample includes listed client companies over the period 1995–2016 since we use three-year data before and after the merger when available.[21] The sample consists of client companies that are audited by (1) one of the merging audit firms before the merger and (2) the merged audit firm after the merger.[22]

Panel A of Table 1 lists the 46 audit firm mergers by year. Seventeen mergers occurred in 2000, the year in which the China Securities Regulatory Commission (CSRC) and the Ministry of Finance (MOF) issued new regulations that imposed size requirements (with respect to the number of CPAs and total revenue) for audit firms to obtain or retain their license to audit listed companies. The number of mergers in other years ranges from zero to six. Panel B of Table 1 presents the sample breakdown by industry. The whole sample consists of 9,795 client-year observations (1,899 unique clients), 10.87% of which (i.e., 1,065 observations of 213 unique clients) belong to the treatment group (defined below). The Machinery, Equipment, and Instrument industry includes

[19] In our sample, 62%, 81% and 90% of misstatements are corrected within one, two and three years after the original statement is reported, respectively.

[20] The number of audit firms with a license to audit listed companies in China steadily decreases from roughly 100 to 40 over the 1998–2013 period. Those licensed audit firms are relatively big ones in China. There are many more small audit firms registered with the CICPA which are not qualified to audit listed companies and their mergers are not included in our sample.

[21] We use restatement data till the most recent year (i.e., 2018) to identify prior misstatements. To ensure that our results are not affected by under-identification of more recent misstatements in 2015 or 2016, we remove the three mergers in 2012 and two mergers in 2013 in our analysis and our inferences are unchanged (untabulated).

[22] In the case where firm A merges with firm B to form firm AB, A and B are the merging audit firms, and AB is the merged audit firm. We only require a client company be audited by either A or B in the year before the merger and by AB in the year after the merger, and do not impose this requirement over the whole seven-year window because doing so severely reduces the number of observations.

the largest number of client-year observations, accounting for 15.72% of our sample, followed by the Petroleum, Chemical, Plastics, and Rubber industry (9.15%), and the Metal and Non-Metal industry (7.86%). The number (percentage) of the treatment observations in these three industries is 227 (14.74%), 94 (10.49%), and 108 (14.03%) respectively.

*3.2.Research Design*

We employ a difference-in-difference approach and examine whether the audit quality for the treatment group improves after the merger, relative to that for the control group. Specifically, we estimate the following logistic model of audit quality:

$$MISSTATEMENT_{ijt} = \alpha_j + \beta_1 \times POST_{ijt} + \beta_2 \times TREAT_{ijt} + \beta_3 \times POST_{ijt} \times TREAT_{ijt} + Controls + \varepsilon_{ijt}, \quad (1)$$

where *i* indexes client companies, *j* indexes audit firm mergers, and *t* indexes event time (the year of merger is year 0).[23] Our empirical tests focus on the within-merged-firm variation by including merger fixed effects $\alpha_j$. This design allows us to exploit across-client variation while removing all effects at the merged audit firm level. We use misstatements (*MISSTATEMENT*) as our main audit quality measure. This measure has relatively low measurement error and offer relatively strong evidence of poor audit quality as misstatements are directly under the auditor's influence (DeFond and Zhang 2014). $MISSTATEMENT_{ijt}$ equals one if client *i*'s financial statement in year *t* is restated in a subsequent year due to accounting irregularities, and zero otherwise.

For each merger, we include three-year client-level data before and after the merger when available. Data in the merger year is also included since it is the first year in which audit reports are issued in the name of the merged audit firm.[24] $POST_{ijt}$ equals one if client *i*'s year *t* observation

[23] We use a logistic model instead of a linear probability model because the latter may not be appropriate with infrequent (<0.2) or too frequent (>0.8) events (Long 1997). Nevertheless, our inferences are unchanged when we repeat the analysis with a linear probability model (untabulated).

[24] Removing observations in the merger event year does not change our inferences (untabulated).

belongs to the post-merger period (including the merger year), and zero if client *i*'s year *t* observation belongs to the pre-merger period.

We sort all client companies of each merged audit firm into the treatment and control groups based on the relative industry expertise of their auditors in the year before the merger. Following prior literature (e.g., Balsam, Krishnan, and Yang 2003; Chin and Chi 2009), an audit firm's industry expertise is measured by its industry market share (based on the number of listed clients in that industry). We conduct our analysis at the audit-firm level, instead of at the office level for three reasons. First, most audit firms in China over the sample period are relatively small and do not have any branch office. Second, even for bigger audit firms that have branch offices, those branch offices are required to perform audits under the name of the firm (MOF 2010) and have much less authority than the practice offices of the Big N firms in the United States. Third, DeFond and Zhang (2014) note that national-level specialization leverages broad industry-specific knowledge and creates opportunities for knowledge sharing, while office-level or partner-level specialization hinges more on local knowledge or individual ability. Thus, using industry market share at the national-level to measure expertise enhances the power to detect the effect of knowledge transfer on audit performance improvement in merged audit firms.

For clients in an industry audited by both merging audit firms, those audited by the less competent audit firm in that industry belong to the treatment group, while all other clients belong to the control group. We require the distance of the industry market share rank between the two audit firms to be at least five to classify one firm as being more competent than the other firm. One merging audit firm can be more competent in one industry while less competent in another industry than the other merging audit firm (see Figure 2). $TREAT_{ijt}$ equals one if client *i* in merger *j* belongs to the treatment group, and zero if client *i* in merger *j* belongs to the control group.

Our variable of interest is the interaction term $POST_{ijt} \times TREAT_{ijt}$. Its coefficient $\beta_3$ captures the change in audit quality for the treatment group relative to the control group. An improvement in audit quality for the treatment group will be reflected in a negative $\beta_3$, i.e., a reduction in *MISSTATEMENT*. Because we include merger fixed effects in the regression, we essentially compare the treatment group and the control group within the same merged audit firm. This design feature allows us to remove all common effects at the merged audit firm level, and use the treatment effect to capture the effect of industry-specific knowledge transfer on audit performance after the merger.

We include a set of control variables following prior research on misstatements (e.g., He, Kothari, Xiao, and Zuo 2018). Detailed definitions of these variables appear in Appendix B. We control for client size (*SIZE*), leverage (*LEV*), profitability (*ROA*), the incurrence of loss (*LOSS*), and sales growth (*GROWTH*), which are associated with the incidence of misstatements and audit risk (Kinney and McDaniel 1989; DeFond and Jiambalvo 1994). A client's current ratio (*CURRENT*) is included as a control since a higher current ratio indicates a lower degree of audit risk (Chan and Wu 2011). We control for firm age (*AGE*) as old firms in China, after exhausting their IPO proceeds, are more likely to suffer financial distress (DeFond, Wong, and Li 2000). Audit firm tenure (*TENURE*) is also included as a control as in prior studies (Chan and Wu 2011). Following Gong, Li, Lin, and Wu (2016), we control for the presence of modified audit opinions (*MAO*).[25] Finally, we include industry fixed effects in all regressions.[26] Standard errors are clustered by client.

[25] Our inferences are unchanged when we add several additional control variables such as earnings volatility, total accruals, and size squared.

[26] As noted before, the inclusion of merger fixed effects allows us to compare the treatment and control groups within each merged audit firm. We do not include client or year fixed effects due to the low frequency nature of misstatements and the limited time-series and cross-sectional data for each merger.

*3.3.Summary Statistics*

Table 2 presents the summary statistics of all the variables used in the main analysis. All continuous variables are winsorized at the top and bottom one percent to mitigate the influence of extreme values. Misstatements happen for 6.93 percent of all the client-year observations. The relatively low frequency of this outcome variable is consistent with prior research (He, Kothari, Xiao, and Zuo 2018). 57.21 percent of observations belong to the post-merger period, and 10.87 percent of observations belong to the treatment group. The variable *SIZE* is right-skewed, with a mean of 5360.5 million yuan, and a median of 2045.6 million yuan. The mean and median of *LEV* is both around 48 percent, and the mean and median of *ROA* is both around 3.6 percent. 10.4 percent of observations report negative net income. Sales growth exhibits a large variation, with a mean of 19.6 percent, and a standard deviation of 50 percent. Both the mean and median of the current ratio (2 and 1.4 respectively) exceed the common benchmark value of one. An average client has been listed for 9 years. The mean and median tenure for the incumbent audit firm is 6.5 and 5 years respectively. 6.67 percent of observations receive a modified audit opinion.

## 4. Main Analysis and Robustness Checks

In our main analysis, we use the likelihood of clients' earnings misstatements as the proxy for audit quality and test the prediction that the audit quality of the treatment group in a less competent auditor (in terms of industry expertise) improves relative to the control group after the merger with a more competent auditor. We conduct several robustness checks to ensure that our results hold with alternative definitions of treated clients and alternative samples. We also provide empirical support to the parallel-trends assumption.

*4.1.Main Analysis*

Panel A of Table 3 reports the descriptive statistics of *MISSTATEMENT*. The misstatement frequency is 11.18 percent for the treatment group and 7.39 percent for the control group prior to the merger, and the difference between them (3.79 percent) is statistically significant (*t*-stat = 2.92). Thus, audit quality is lower for the treatment group than for the control group in the pre-merger period. This evidence suggests that the industry market share is a reasonable measure of industry expertise and the treatment group identified using this measure likely represents the recipient units in knowledge transfer. After the merger, the misstatement frequency declines to 5.84 percent for the treatment group and to 6.31 percent for the control group, and the difference between them (0.47 percent) is not statistically significant (*t*-stat = -0.44). The difference in misstatement reduction between the treatment and control groups is 4.26 percent and statistically significant (*t*-stat = -2.57).

Panel B of Table 3 presents the logistic regression results of using *MISSTATEMENT* as the dependent variable in Equation (1). Column 1 reports the results of estimating Equation (1) without time-varying control variables for the full sample. The coefficient on the interaction term *POST×TREAT* is -0.6252 and statistically significant at the five percent level (*z*-stat = -2.48). The magnitude and statistical significance of this coefficient become slightly larger (-0.6722 with *z*-stat = -2.71) in column 2 when the full set of control variables is included in the estimation for the full sample. The behavior of the control variables is generally as predicted. Firms with high leverage and low profitability are more likely to misstate their current period earnings. Firms that receive a modified audit opinion are also more likely to exhibit accounting irregularities. Our results are also insensitive when the control group is restricted to clients in the same industry as

the treated clients (in column 3) or to clients in a different industry from the treated clients (in column 4).[27]

The positive, significant coefficient on *TREAT* in all columns suggests that the audit quality for the treated clients before the merger is on average lower than that for the control clients.[28] The sum of the coefficients on *TREAT* and *POST*×*TREAT* is not statistically different from zero (*p*-value ranges from 0.451 to 0.702), suggesting that the audit quality for the treatment group is as good as that for the control group after the merger. In untabulated analysis, we also run the logistic model separately for the pre- and post-merger periods. Consistent with the results in Table 3, the coefficient on *TREAT* is positive and statistically significant in the pre-merger period and is not statistically different from zero in the post-merger period, and the difference between these two coefficients is -0.6685 and statistically significant at the five percent level. These results suggest that closing the knowledge gap between the treatment and control groups that exists before the merger leads to the same level of audit quality for both groups after the merger.

The economic magnitude of the results is gauged from the incremental effect of *POST*×*TREAT* on the likelihood of an earnings misstatement (Puhani 2012).[29] Based on the coefficient estimates in column 2, the marginal effect of *POST*×*TREAT* is -3.35 percent (holding constant *POST* and *TREAT* at 1 and all other independent variables at their mean values).[30] Hence, we find an economically significant reduction in misstatements (nearly one half of the sample mean of *MISSTATEMENT*) for the treated clients relative to the control clients after the merger.

[27] Consider the simple example in Figure 1. The control group in column 2 includes both firm B's clients in the entertainment industry and firm A's clients in the mining industry, while it only includes firm A's clients in the mining industry in column 3, and firm B's clients in the entertainment industry in column 4.
[28] The difference-in-difference approach does not require the level of the dependent variable to be identical between the treatment and control groups as any systematic difference between them will be eliminated in the estimation.
[29] Ai and Norton (2003) point out that in non-linear models the coefficient on the interaction term does not capture the marginal effect. However, Puhani (2012) demonstrates that this critique does not apply in a difference-in-difference model.
[30] This marginal effect is computed using the Stata command prchange (Long and Freese 2005).

These results suggest that industry-specific knowledge transfer after the merger leads to an improvement in audit performance (as measured by the probability of misstatements) for the treatment group relative to the control group within the same merged audit firm.

It is worth noting that there are two forces underlying the documented improvement in audit quality for the treatment group relative to the control group. First, prior misstatements of treated clients are more likely to be uncovered by their auditors after the merger than before the merger. Since these prior misstatements are corrected, the current-period statement is less likely to be misstated due to these prior errors. Second, after the merger, the auditors of treated clients are also more likely to detect errors in their clients' pre-audit statements in the current period and immediately require necessary audit adjustments. Thus, the post-audit statements of treated clients after the merger are less likely to be misstated due to new errors introduced in the current period. While data limitations prevent us from fully disentangle these two effects,[31] both of them lend support to the same conclusion that knowledge transfer leads to audit performance improvement after the merger.

*4.2.Robustness Checks*

We conduct a series of robustness checks. First, we use four alternative definitions of treated clients. Second, we repeat our analysis for two restricted samples. Third, we construct an augmented sample that includes client companies that switched audit firms after the merger. Fourth, we perform a standard dynamic test to test for possible pre-trends.

*4.2.1. Alternative Definitions of Treated Clients*

In the baseline specification reported in Table 3, for each merger, we sort all client companies into the treatment and control groups based on the relative industry expertise of their

[31] Audit adjustment data in China are not available for the period prior to 2006 (Lennox, Wu, and Zhang 2014, 2016; He, Kothari, Xiao, and Zuo 2018; Lennox, Wang, and Wu 2018).

auditors in the year before the merger, where industry expertise is based the auditor's industry market share. For client companies in an industry audited by both merging audit firms, those audited by the less competent audit firm in that industry belong to the treatment group, while all other client companies belong to the control group. We require the distance of the industry market share rank between the two audit firms to be at least five to classify one firm as being more competent than the other firm.

In this section, we remove the requirement that the distance of the industry market share rank between the two audit firms needs to be at least five ($TREAT_1$), and impose alternative requirements on client companies to be classified as being treated. Specifically, $TREAT_2$ equals one when (1) $TREAT_1$ equals one, and (2) the more competent auditor in the merger is an industry expert (i.e., ranked as a top five auditor in terms of industry market share). $TREAT_3$ equals one when (1) $TREAT_1$ equals one, and (2) the within-audit firm industry portfolio share is larger than that of the other audit firm, where the industry portfolio share is computed as the number of listed clients in that particular industry divided by the total number of clients. $TREAT_4$ equals one when (1) $TREAT_1$ equals one, and (2) the total assets of clients in that particular industry audited by the audit firm are larger than those of the other audit firm.

Table 4 presents the regression results with these four alternative definitions of treated clients (which yield 1,766, 831, 1,240, and 1,212 treatment observations, respectively). The full set of control variables as in Table 3 are included but not reported for brevity. Across all columns, the coefficient on the interaction term *POST*×*TREAT* remains negative and statistically significant, the coefficient on *TREAT* remains positive and statistically significant, and the sum of the coefficients on *TREAT* and *POST*×*TREAT* remains statistically insignificant (*p*-value ranges from 0.438 to 0.882).

*4.2.2. Two Restricted Samples*

We repeat our analysis with two restricted samples. First, to ensure that our documented results are not driven by the Big N effect on audit quality (Jiang, Wang, and Wang 2019), we remove the three mergers that involve the international Big N. Second, audit personnel movement between the merging audit firms after the merger can partly contribute to the observed changes in audit performance. To ensure that our results are not entirely driven by this effect, we repeat our analysis for a restricted sample in which both the engagement partner and the review partner for a client company after the merger belong to the client's audit firm before the merger.[32] For this analysis, we collect the names of the engagement and review partners from annual reports, and obtain data on their employment history from the auditor resumes provided by the CSRC.

Columns 1 and 2 in Table 5 present the regression results for these two restricted samples. Consistent with our previous results, the coefficient on the interaction term *POST*×*TREAT* is negative and significant in both columns. These results indicate that our documented improvement in audit quality for the treated clients (relative to the control clients) after the merger are unlikely to be driven by either the Big N effect or audit personnel movement.

*4.2.3. Augmented Sample including Dropped Clients*

Our sample only includes 1,899 client companies of the merging audit firms that remain audited by the merged audit firm after the merger. Clients that switch to other auditors after the merger are not included as knowledge transfer between the merging audit firms would have no

[32] More than 76% of the treatment group (811 treatment observations) is retained in this restricted sample. Assigning audit partners from the more competent audit firm to the clients in the less competent audit firm after the merger is quite uncommon (6% of the treatment group). We use the term "partner" to describe the signing auditor. The two signing auditors' signatures appear on the audit report, with the top signature from the review partner, and the bottom signature from the engagement partner. We cannot hold the audit partners constant over the event window because of mandatory partner rotation in China (Lennox, Wu, and Zhang 2014). We do not impose constant personnel requirement in the main analysis because personnel movement can be viewed as one channel through which knowledge transfer occurs between the merging audit firms.

effect on the audit quality of those clients. For the 46 mergers included in our sample, the total number of client companies is 2,211 before the merger, and 1,899 after the merger. Thus, a relatively small number of client companies (i.e., 14.1 percent) switched auditors after the merger. Based on the industry market shares of the auditors before the merger, 19.3 percent (13.4 percent) of client companies in the treatment (control) group are dropped from the sample due to switching auditors.[33]

We repeat our analysis with an augmented sample that includes client companies that switched audit firms after the merger. We create two dummy variables to indicate dropped client companies. *TREAT_DROP* is a dummy variable that equals one for dropped client companies that would have been classified as a treated client, and *CONTROL_DROP* is a dummy variable that equals one for dropped client companies that would have been classified as a control client. We include in the baseline regression these two dummy variables and their interactions with *POST* (i.e., *POST*×*TREAT_DROP* and *POST*×*CONTROL_DROP*). The augmented sample is essentially divided into four groups: treated clients, control clients, dropped treatment clients, and dropped control clients. In the regression model, control clients are used as the benchmark group. Column 3 in Table 5 presents the logistic regression results. The results show that dropped treatment and control clients both exhibit higher misstatement frequencies than the control clients before the merger. After the merger, the misstatement frequency for both the dropped treatment clients and the dropped control clients reduces.[34] Thus, the audit quality for dropped clients is relatively low

[33] Had no client companies switched auditors, the treatment group would have included 264 client companies, and the control group would have included 1,947 client companies. In the actual sample, the treatment group consists of 213 client companies, and the control group consists of 1,686 client companies.

[34] We note that these results on dropped clients are purely descriptive and can be interpreted in two ways. First, prior misstatements of dropped clients are more likely to be uncovered by their new auditors and these prior misstatements are subsequently corrected. Second, the new auditors of dropped clients are more likely to detect errors in their clients' pre-audit statements and require necessary audit adjustments in the current period. Thus, the post-audit statements of dropped clients after the auditor switch are less likely to contain errors than those before the auditor switch.

(compared with control clients) prior to the merger, but there is no evidence that a different type of clients is dropped from the treatment group versus the control group after the merger. More importantly, the coefficient on the interaction term *POST*×*TREAT* remains negative and significant. Hence, our results continue to hold in this augmented sample that includes dropped clients.

*4.2.4. Testing for Pre-Trends*

To provide further support that the coefficient on the interaction term *POST*×*TREAT* reflects the treatment effect of knowledge transfer rather than a differential time trend between the treatment and control groups, we perform a standard dynamic test to test for possible pre-trends. Specifically, we create two additional dummy variables, *PRE*_1 and *PRE*_2, which equal one for observations that are one or two years before the merger (i.e., Year –1 and –2), respectively. We include in the baseline regression these two dummy variables and their interactions with *TREAT*. Column 1 of Table 6 presents the logistic regression results. The coefficients on the interaction terms *PRE*_1×*TREAT* and *PRE*_2×*TREAT* are both statistically insignificant. These results of similar trends in audit quality across different client companies before the audit firm merger suggest that our inferences of knowledge transfer are unlikely to be driven by potential differential time trends across the treated and control clients absent the audit firm merger.[35]

## 5. Supplementary Analyses

In our main analysis, we classify all client companies in a merged audit firm into the treatment and control groups based on their auditors' industry expertise before the merger. Based on this classification, we attribute the audit quality improvement for the treatment group relative to the control group after the merger to industry-specific knowledge transfer. We acknowledge

[35] We also conduct a placebo test (untabulated) following Jiang, Wang, and Wang (2019). To minimize the loss of observations, we use a four-year window (year *t*–4 to year *t*–1) around the pseudo merger event in year *t*–2. We show that there is no significant change in the audit quality for the treated clients relative to the control clients after the pseudo merger event.

that the improved audit performance for the treatment group (relative to the control group) after the merger can be partly explained by enhanced auditor incentives after the merger. This caveat is not unique to our study and applies to any archival research on auditor competencies because of the interrelation between auditor competencies and their incentives (DeFond and Zhang 2014). To ensure that our results are not *purely* driven by the incentive effect, we conduct four additional analyses. First, we investigate the timing of the audit performance improvement. Second, we restrict the benchmark group to control clients that are more similar to the treated clients in terms of auditor incentives. Third, we decompose treated clients into different groups and examine potential differential treatment effects. Fourth, we look at audit fees.

*5.1.Timing of Audit Performance Improvement*

In the main analysis, the merger year is included as a post-merger year (i.e., *POST* = 1) since it is the first year in which audit reports are issued in the name of the merged audit firm. If our results were purely driven by the incentive effect, the improvement in audit performance for the treatment group (relative to the control group) would occur immediately after the merger (i.e., in the merger year and the three-year period after the merger). We create two dummy variables and use their interactions with *TREAT* to test this prediction: *POST*_0 equals one if the client observation belongs to the merger event year, and *POST*_13 equals one if the client observation belongs to the three-year period after the merger event year. Column 2 of Table 6 reports the results and shows that the effect of audit firm mergers on audit performance (as measured by *MISSTATEMENT*) does not occur in the merger event year and only happens in the three-year period after the merger. Column 3 of Table 6 further includes *PRE*_1 and *PRE*_2 and their interactions with *TREAT* and our inferences are unchanged. Thus, our evidence is consistent with

a gradual process of knowledge development (as in Gaver and Utke 2019) instead of a rapid incentive effect.

*5.2.Alternative Benchmark Groups*

In our main analysis, we compare treated clients with all other clients that are not classified as the treatment group. In the simple example in Figure 1 where firm A (a specialist in the mining industry) merges with firm B (a non-specialist with clients in the mining industry), firm B's clients in the mining industry belong to the treatment group, while both firm B's clients in the entertainment industry and firm A's clients in the mining industry belong to the benchmark group. It could be the case that the audit quality for firm B's clients is generally lower than that for firm A's clients, and the merged audit firm AB has stronger incentives to improve the audit quality for firm B's clients regardless of their industries. To address this concern, we include merging audit firm fixed effects (instead of merger fixed effects) in the analysis to examine across-client variation within the same merging audit firm (i.e., firm A or firm B, instead of firm AB). That is, we compare the audit quality improvement between firm B's clients in the mining industry and firm B's clients in the entertainment industry for the merger depicted in Figure 1. Column 1 of Table 7 presents the regression results for this alternative specification.[36] Consistent with our main analysis, the coefficient on the interaction term *POST×TREAT* is negative and significant.

Further, we remove from the benchmark group those relatively strong units whose audit quality has little room for improvement, and retain only those relatively weak units whose audit quality can be improved. If our results are purely driven by auditor incentives, the audit quality for all relatively weak units (regardless of their industries) should be improved equally after the merger. However, if our results partly reflect the effect of industry-specific knowledge transfer,

[36] The number of observations is slightly reduced in this analysis compared with Table 3 because the value of the dependent variable is all zero for some merging audit firms.

we should observe a larger audit quality improvement for the treatment group than for those relatively weak units in other industries within the same merging audit firm. We identify the relatively weak units as those clients that belong to an industry whose within-audit firm portfolio share is no larger than that of the treated clients. In the example depicted in Figure 2 where audit firm B has three treated clients in the mining industry, two control clients in the entertainment industry, and six control clients in the electronics industry, we remove from the benchmark group those clients in the electronics industry (whose audit quality is relatively high prior to the merger and has limited room for improvement). Thus, we compare the audit quality improvement between firm B's clients in the mining industry and firm B's clients in the entertainment industry.

Column 2 of Table 7 presents the regression results for the analysis with this more restricted benchmark group. The insignificant coefficient on *TREAT* provides evidence that the audit quality is indeed similar for the treatment group and the benchmark group (i.e., other relatively weak units within the same merging audit firm) prior to the merger. The negative, significant coefficient on the interaction term *POST*×*TREAT* suggests that the audit quality improves more for the treatment group relative to the benchmark group. The sum of the coefficients on *TREAT* and *POST*×*TREAT* is negative and significant, suggesting that the audit quality is better for the treatment group than for those relatively weak units in other industries after the merger. We interpret this differential audit quality improvement as evidence of the effect of industry-specific knowledge transfer.

*5.3.Differential Treatment Effects*

In the previous analysis, treated clients are viewed as homogeneous and we document the average treatment effect for all treated clients. In Table 8, we examine whether the treatment effect differs for treated clients in the relatively big audit firm (*TREAT_BIG*) versus treated clients in the

relatively small audit firm (*TREAT_SMALL*).[37] The coefficients on both *POST×TREAT_BIG* and *POST×TREAT_SMALL* are negative and significant, and the difference between them is not statistically different from zero. These results suggest that audit firm mergers lead to similar audit performance improvement for both sets of treated clients. The improved audit quality for treated clients in the relatively big audit firm presents relatively strong evidence of knowledge transfer since it is less likely that the small audit firm can alter the big audit firm's incentives to improve audit performance after the merger.

In untabulated analysis, we also split treated clients into two groups based on (1) whether a treated client is headquartered in the same province as any of the control clients in the same industry within the merged audit firm, (2) whether the merging audit firms are within the same audit firm association,[38] and (3) whether a treated client is a state-controlled company or has any political connection (Fan, Wong, and Zhang 2007).[39] We find no evidence of differential treatment effects for any of these partitions. However, given the relatively small number of treated clients in each partition and the low frequency nature of the dependent variable, these partitioning results should be interpreted with caution.

*5.4.Audit Fees*

The change in audit performance of the treatment group relative to the control group documented in the main analysis reflects a benefit of industry-specific knowledge transfer after the merger. The net effect of knowledge transfer on audit fees is unclear because knowledge

[37] We do not run the model separately for the two subsamples because the small number of treated clients (181 in the big audit firms and 884 in the small audit firms) and the low frequency of the dependent variable significantly reduce the statistical power to detect the effect of knowledge transfer on audit quality.

[38] Bills, Cunningham, and Myers (2016) provide evidence that association member firms in the United States conduct higher-quality audits than nonmember firms. Domestic accounting firm associations are uncommon in China and there is usually little communication among association member firms.

[39] Firth, Rui, and Wu (2011) examine corporate lawsuits in China, and document some court bias in favor of state-controlled companies and politically connected companies.

transfer can entail both additional costs (e.g., integration costs related to information systems or training) and efficiency gains (e.g., reduction in audit hours). However, if previously documented results are totally driven by auditors' stronger incentives to maintain independence because of higher reputation and litigation risk for the treatment group, audit fees should increase more for the treatment group relative to the control group after the merger. Thus, we test whether the change in the audit fee differs between the treated clients and the control clients. For this analysis, the sample is restricted to mergers over the 2001–2013 period because client companies started to disclose audit fees in 2000.

Panel A of Table 9 reports the descriptive statistics of audit fee (*FEE*). For the treatment group, the average audit fee is ¥549,600 before the merger, and ¥722,200 after the merger, and the increase of ¥172,600 is statistically significant (*t*-stat = 4.97). The average audit fee also increases for the control group after the merger (¥178,200 with *t*-stat = 11.03). The difference in audit fee increase between the treatment and control groups is quite small (¥5,610) and not statistically significant (*t*-stat = -0.12).

Panel B of Table 9 presents the OLS regression results of using the natural logarithm of the audit fee (*FEE*) as the dependent variable. Column 1 reports the results using our main specification as in Equation (1). The coefficient on the interaction term *POST*×*TREAT* is -0.0074 and not statistically significant (*t*-stat = -0.30). It remains small and statistically insignificant (-0.0188 with *t*-stat = -0.82) in column 2 when merging firm fixed effects are included in the estimation. These results suggest that the audit performance improvement of the treatment group relative to the control group is unlikely to be purely driven by auditor incentives induced by differential reputation or litigation risk.

## 6. Conclusions

In this paper, we study knowledge transfer in audit firms by utilizing the setting of the rapid audit market consolidation in China over the 1998–2013 period. For each merger, we sort all client companies into the treatment and control groups based on the relative industry expertise of their auditors before the merger. We employ a difference-in-difference approach and examine whether the audit quality for the treatment group improves after the merger (three-year post-merger versus three-year pre-merger), relative to that for the control group. Our empirical tests exploit across-client variation while removing all effects at the merged audit firm level by including merger fixed effects. We find an economically significant improvement in audit quality (as reflected in a reduction in misstatements) for the treatment group relative to the control group in the same merged audit firm. The evidence suggests that industry-specific knowledge transfer in audit firms leads to an economically significant audit performance improvement.

Compared with existing archival research that relies on across-audit-firm comparisons, our empirical approach and setting allow us to identify the recipient units in knowledge transfer and provide more direct evidence on the effect of knowledge transfer on audit performance *within an audit firm*. Our archival approach complements prior studies relying on surveys, interviews or laboratory experiments (Libby, Bloomfield, and Nelson 2002; Bloomfield, Nelson, and Soltes 2016). We echo the statement made by Glaeser and Guay (2017, p. 311) that "the literature is likely to be best served by addressing causal inference in a Bayesian manner, whereby multiple studies, using a variety of research designs and sample selection, are used to update researchers' priors on important topics and theories." Assessing the generalizability of our findings in an alternative audit market or an experimental setting is left for future research.

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## Appendix A: Institutional Background

In this appendix, we describe the development of the audit market in China, and the economic and regulatory forces underlying the audit market consolidation over the 1998–2013 period.

In December 1978, the Communist Party of China led by Deng Xiaoping initiated the program of economic reforms to introduce market principles to China and build "socialism with Chinese characteristics." The opening up of China to foreign investment and the restructuring of state-owned enterprises as joint stock companies generated demand for auditing. China's first audit firm was established in 1980, after which thousands of government-affiliated audit firms mushroomed (Tang 2000). The launch of the Shanghai Stock Exchange in 1990 and the Shenzhen Stock Exchange in 1991 created demand for independent audits. To audit listed companies, audit firms are required to obtain a license from the China Securities Regulatory Commission (CSRC) and the Ministry of Finance (MOF). Responding to investor demand for independent audits, the CSRC and the MOF promulgated a series of reforms to separate audit firms from the government beginning in 1998. These reforms were completed in early 2000. Since then audit firms are independent of the government and operate under competitive market forces (Chen, Chen, Lobo, and Wang 2011).

In the 1990s, most domestic audit firms were small and the audit market was highly fragmented. In 1999, the average number of listed clients for the 106 licensed audit firms was less than ten, and the market share of the 20 largest audit firms was only 49.6 percent in terms of the number of listed clients (CSRC 2001).[40] At that time, many domestic auditing professionals

[40] In the early 1990s, international audit firms were not allowed to directly enter China's audit market, but they were able to form joint ventures with domestic audit firms. Since 1999, international audit firms can directly invest in and own domestic firms. The market share of the international Big N in China was only 3.6 percent in 1999, and this figure grew to 6.9 percent in 2006.

believed that increasing firm size through mergers could strengthen their firms' ability to compete with large international audit firms after China's entrance into the World Trade Organization (China Securities News 2000).

In the late 1990s, an increasing number of large state-owned enterprises were restructured to become joint stock companies, and the government started to impose stringent size requirements for audit firms to obtain an audit license. In 1997, to be eligible to apply for a license to audit listed companies, an audit firm needed to employ more than eight certified public accountants with a qualification from the CSRC to sign audit reports for listed companies. In June 2000, the CSRC and the MOF increased this number to 20 and further required that audit firms must have annual revenue of more than eight million yuan (the Chinese currency), which exceeded the revenue of many audit firms in 1999. Merging with another audit firm could enable small audit firms to meet these requirements.

In the 2000s, the international Big N audit firms aggressively expanded their investment in China.[41] Moreover, international audit firms (mainly the Big N) were selected as auditors for all of the overseas listings of Chinese companies. To protect their domestic market share and to compete for accounting services for large Chinese companies domestically and globally, domestic audit firms had strong incentives to merge with their peers to increase firm size and competencies. Mergers among domestic audit firms were also strongly encouraged and supported by the Chinese government. In May 2007, the Chinese Institute of Certified Public Accountants (CICPA) issued a policy statement directed at developing larger and more competitive domestic audit firms. In

[41] For example, in 2003, Deloitte announced a landmark investment of US$150 million in China as a part of its five-year plan to increase staff and revenue by four to five times (China Securities News 2004). In 2005, Deloitte announced to acquire Beijing Tianjian, a member of the Tianjian Alliance (the biggest domestic audit alliance), and PwC announced its plan to recruit more than 1,000 workers each year in the following five years (China Financial Times 2005).

October 2009, the State Council of China promulgated that the government would support the ten largest domestic audit firms. In November 2009, the CSRC and the MOF issued guidance for application of license to audit Chinese companies listed in Hong Kong.[42] The first batch of eligible firms consisted of the international Big 4 and eight domestic audit firms. Other audit firms could also apply for a license if they had annual revenue of more than 300 million yuan or more than 30 listed clients, and employed more than 400 certified public accountants. Merging with peer audit firms again became an efficient way to meet these license requirements.

In the early 2010s, the audit market consolidation continued and the government furthered its effort to support audit firm mergers. In June 2012, the CICPA issued another policy statement to encourage audit firms to increase their size and competencies (CICPA 2012). In particular, it promised to offer partial membership fee refunds to those audit firms who first became a top 15 firm and to those existing top 15 firms whose ranking improved by more than three positions. It explicitly encouraged audit firm mergers and required merged audit firms to achieve post-merger uniformity of personnel, finance, services, technology standards and information management. The CICPA was also committed to providing special training and instructions to merged audit firms with respect to unified management and quality control.

In summary, unlike the U.S. audit market, which is dominated by Big N auditors, China's audit market is relatively fragmented among domestic audit firms and the international Big N audit firms. China's rapid economic development and regulatory changes over the 1998–2013 period triggered a wave of audit firm mergers. Domestic audit firms merged with their peers to increase

[42] These companies are referred to as H-share companies which are incorporated in mainland China and listed in Hong Kong. Many H-share companies simultaneously issue A shares traded on the Shanghai Stock Exchange or the Shenzhen Stock Exchange. Prior to 2010, H-share companies were required to prepare financial statements audited by Hong Kong auditors, and the international Big N dominated this audit market. Since 2010, the Hong Kong Exchange and Clearing Limited started to accept financial statements prepared under Chinese accounting standards and audited by mainland audit firms.

firm size and to deliver audits demanded by clients in a competitive audit market.[43] Merged audit firms have strong economic incentives to improve their competence to obtain government support and to compete in the audit market. Using this setting, we study whether and to what extent removing organizational boundaries facilitates knowledge transfer between the merging audit firms and leads to audit performance improvement.

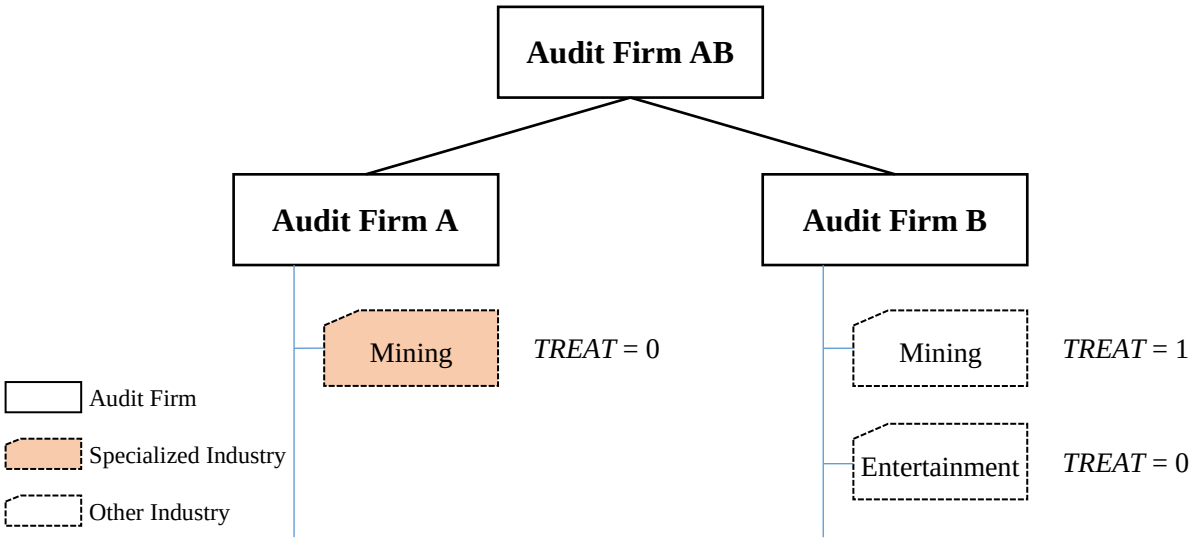
[43] Besides the aforementioned economic and regulatory forces, three factors underlie auditors' incentives to provide high quality audits in China (Lennox, Wu, and Zhang 2016). First, auditors' legal responsibilities and litigation risk were substantially increased by legal reforms in 2002 and 2005 (Firth, Mo, and Wong 2012). Second, audit firms are overseen by the MOF and the CICPA, and regularly inspected by the Inspection Bureau of the MOF. For example, in 2005, the licenses of 18 audit firms were withdrawn by the regulators and a further 60 audit firms were punished with fines and reform orders. Third, audit scandals can result in adverse reputational consequences (He, Pittman, and Rui 2016).

**Appendix B: Variable Definitions**

| Variable | Variable Definition |
|---|---|
| ***Dependent Variables:*** | |
| *MISSTATEMENT* | Equals one if the client's financial statement in the current year is restated in a subsequent year due to accounting irregularities, and zero otherwise. We manually collect restatement data from the "Material Accounting Errors" section of financial statement footnotes and exclude restatements due to changes in accounting standards or tax rules, mergers and acquisitions, or other issues unrelated to accounting irregularities. |
| *FEE* | The audit fee paid by the client in the current year. |
| ***Independent Variables:*** | |
| *POST* | Equals one if the client observation belongs to the post-merger period, and zero otherwise. |
| *TREAT* | Equals one if the client belongs to the treatment group. For each merger, we sort all client companies of each merged audit firm into the treatment and control groups based on the relative industry expertise of their auditors in the year before the merger, where an audit firm's industry expertise is measured by its industry market share (based on the number of listed clients in that industry). For clients in an industry audited by both merging audit firms, those audited by the less competent audit firm in that industry belong to the treatment group, while all other clients belong to the control group. We require the distance of the industry market share rank between the two audit firms to be at least five to classify one firm as being more competent than the other firm. |
| $TREAT_1$ | Equals one if the client belongs to the treatment group after removing the requirement that the distance of the industry market share rank between the two audit firms to be at least five to classify one firm as being more competent than the other firm. |
| $TREAT_2$ | Equals one when (1) $TREAT_1$ equals one, and (2) the more competent auditor in the merger is an industry expert (i.e., ranked as a top five auditor in terms of industry market share). |
| $TREAT_3$ | Equals one when (1) $TREAT_1$ equals one, and (2) the within-audit firm industry portfolio share is larger than that of the other audit firm, where the industry portfolio share is computed as the number of listed clients in that particular industry divided by the total number of clients. |
| $TREAT_4$ | Equals one when (1) $TREAT_1$ equals one, and (2) the total client assets of the more competent auditor in that particular industry are larger than those of the less competent auditor. |
| *TREAT_DROP* | Equals one for dropped client companies that would have been classified as a treated client, and zero otherwise. |
| *CONTROL_DROP* | Equals one for dropped client companies that would have been classified as a control client, and zero otherwise. |
| *PRE_2* | Equals one for observations that are two years before the merger (i.e., Year –2), and zero otherwise. |
| *PRE_1* | Equals one for observations that are one year before the merger (i.e., Year –1), and zero otherwise. |
| *POST_0* | Equals one if the client observation belongs to the merger event year, and zero otherwise. |
| *POST_13* | Equals one if the client observation belongs to the three-year period after the merger event year, and zero otherwise. |
| *TREAT_BIG* | Equals one for treated clients in the relatively big audit firms, and zero otherwise. |
| *TREAT_SMALL* | Equals one for treated clients in the relatively small audit firms, and zero otherwise. |
| *SIZE* | The client's total assets in the current year. |
| *LEV* | Leverage ratio in the current year, computed as total liabilities divided by total assets. |
| *ROA* | Return on assets in the current year, computed as net income divided by total assets. |

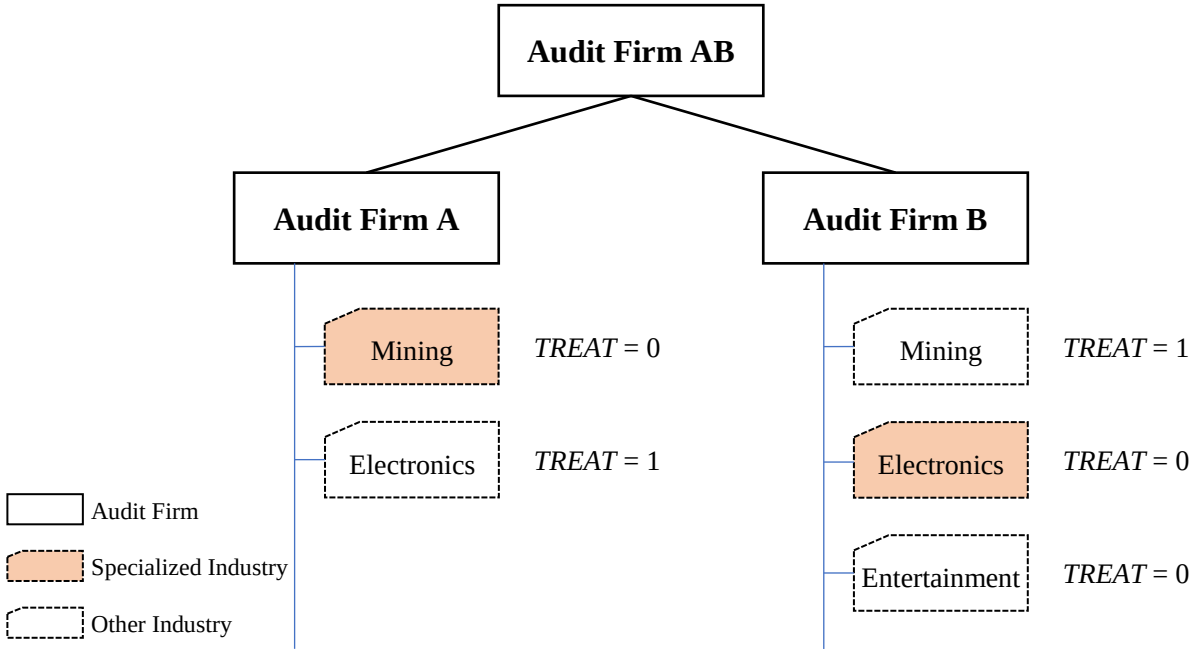
| | |
|---|---|
| *LOSS* | Equals one if the client reports negative net income in the current year, and zero otherwise. |
| *GROWTH* | Sales growth, computed as the percentage change in sales from the prior year to the current year. |
| *CURRENT* | Current ratio in the current year, computed as current assets divided by current liabilities. |
| *AGE* | The number of years that the client has been listed. |
| *TENURE* | The number of continuous years that the client has been audited by the audit firm. |
| *MAO* | Equals one if the client receives a modified audit opinion in the current year, and zero otherwise. Following Wang, Wong, and Xia (2008), we classify unqualified opinions with an explanatory paragraph, qualified opinions, disclaimers, and adverse opinions as modified opinions. |

**Figure 1: A Simple Example**




Notes: This figure depicts a merger between two hypothetical audit firms: firm A specializes in the mining industry; firm B is a non-specialist, and has clients in both the mining industry and the entertainment industry. Firms A and B merge to form firm AB.

**Figure 2: A More Complicated Example**




Notes: This figure depicts a more complicated situation: firm A has clients in two industries: (a) mining, and (b) electronics; firm B has clients in three industries: (a) mining, (b) electronics, and (c) entertainment. Firm A specializes in the mining industry while firm B specializes in the electronics industry. In this case, firm A's clients in the electronics industry and firm B's clients in the mining industry belong to the treatment group, while all other clients belong to the control group.

**TABLE 1: Sample Distribution**

Panel A: Number of Mergers by Year

| Year | 1998 | 1999 | 2000 | 2001 | 2002 | 2003 | 2004 | 2005 | 2006 |
|---|---|---|---|---|---|---|---|---|---|
| N | 2 | 0 | 17 | 1 | 1 | 1 | 0 | 1 | 2 |
| Year | 2007 | 2008 | 2009 | 2010 | 2011 | 2012 | 2013 | | Total |
| N | 3 | 6 | 5 | 1 | 1 | 3 | 2 | | 46 |

Panel B: Number of Client-Year Observations by Industry

| Industry | Full Sample | | Treatment Sample | |
|---|---|---|---|---|
| | N | Percentage (Column) | N | Percentage (Row) |
| Machinery, Equipment, and Instrument | 1540 | 15.72% | 227 | 14.74% |
| Petroleum, Chemical, Plastics, and Rubber | 896 | 9.15% | 94 | 10.49% |
| Metal and Non-Metal | 770 | 7.86% | 108 | 14.03% |
| Wholesale and Retail Trade | 704 | 7.19% | 79 | 11.22% |
| Medicine and Biological Products | 693 | 7.08% | 71 | 10.25% |
| Real Estate | 688 | 7.02% | 71 | 10.32% |
| IT and Computing | 585 | 5.97% | 89 | 15.21% |
| Electronics | 520 | 5.31% | 85 | 16.35% |
| Food and Beverage | 454 | 4.64% | 46 | 10.13% |
| Energy and Water | 421 | 4.30% | 45 | 10.69% |
| Transportation | 398 | 4.06% | 26 | 6.53% |
| Conglomerates | 347 | 3.54% | 18 | 5.19% |
| Textile, Apparel, Fur and Leather | 308 | 3.14% | 24 | 7.79% |
| Public Utilities | 293 | 2.99% | 29 | 9.90% |
| Mining | 265 | 2.71% | 14 | 5.28% |
| Agriculture | 229 | 2.34% | 19 | 8.30% |
| Construction | 211 | 2.15% | 10 | 4.74% |
| Paper and Printing | 208 | 2.12% | 10 | 4.81% |
| Entertainment | 137 | 1.40% | 0 | 0.00% |
| Other Manufacturing | 128 | 1.31% | 0 | 0.00% |
| Total | 9795 | 100.0% | 1065 | 10.87% |

Notes: Our sample consists of 46 mergers over the 1998–2013 period, in which both merging audit firms had a license to audit listed companies in China. Our sample includes client companies over the period 1995–2016 since we use three-year data before and after the merger when available. Our sample focuses on client companies that are audited by (1) one of the merging audit firms before the merger and (2) the merged audit firm after the merger. For each merger, we sort all client companies into the treatment and control groups based on the relative industry expertise of their auditors in the year before the merger, where an audit firm's industry expertise is measured by its industry market share (based on the number of listed clients in that industry). We require the distance of the industry market share rank between the two audit firms to be at least five to classify one firm as being more competent than the other firm. For clients in an industry audited by both merging audit firms, those audited by the less competent audit firm in that industry belong to the treatment group, while all other clients belong to the control group.

**TABLE 2: Summary Statistics**

| Variables | Mean | Std. Dev. | Q1 | Median | Q3 |
|---|---|---|---|---|---|
| *MISSTATEMENT* | 0.0693 | 0.2540 | 0.0000 | 0.0000 | 0.0000 |
| *POST* | 0.5721 | 0.4948 | 0.0000 | 1.0000 | 1.0000 |
| *TREAT* | 0.1087 | 0.3113 | 0.0000 | 0.0000 | 0.0000 |
| *SIZE* (¥m) | 5360.5 | 11171.9 | 1023.9 | 2045.6 | 4729.1 |
| *LEV* | 0.4785 | 0.2372 | 0.3143 | 0.4743 | 0.6293 |
| *ROA* | 0.0363 | 0.0712 | 0.0130 | 0.0367 | 0.0660 |
| *LOSS* | 0.1042 | 0.3056 | 0.0000 | 0.0000 | 0.0000 |
| *GROWTH* | 0.1964 | 0.4981 | -0.0239 | 0.1267 | 0.3020 |
| *CURRENT* | 2.0436 | 2.3432 | 0.9784 | 1.3828 | 2.1603 |
| *AGE* | 9.2178 | 5.3722 | 5.0000 | 9.0000 | 13.000 |
| *TENURE* | 6.5166 | 4.2926 | 3.0000 | 5.0000 | 9.0000 |
| *MAO* | 0.0667 | 0.2493 | 0.0000 | 0.0000 | 0.0000 |

Notes: This table presents the summary statistics of the variables. All continuous variables are winsorized at the top and bottom one percent to mitigate the influence of extreme values. Details on the definition and construction of the variables reported in the table are available in Appendix B.

**TABLE 3: Knowledge Transfer and Accounting Misstatements**

Panel A: Descriptive Statistics of *MISSTATEMENT*

| | *TREAT*=1 | *TREAT*=0 | Difference in Means: (1) vs (2) | |
|---|---|---|---|---|
| | (1) | (2) | Difference | *t*-stat |
| Pre-Merger | 11.18% [N=483] | 7.39% [N=3708] | 3.79%** | (2.92) |
| Post-Merger | 5.84% [N=582] | 6.31% [N=5022] | -0.47% | (-0.44) |
| Difference: Post vs Pre | -5.34%*** | -1.08%** | -4.26%*** | (-2.57) |
| *t*-stat | (-3.16) | (-1.98) | | |

Panel B: Models of *MISSTATEMENT*

| Dependent Variable: *MISSTATEMENT* | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| ***POST×TREAT*** | **-0.6252**** | **-0.6722***** | **-0.6651**** | **-0.7120***** |
| | **(-2.48)** | **(-2.71)** | **(-2.38)** | **(-2.71)** |
| *TREAT* | 0.5017** | 0.5385** | 0.5656** | 0.5486** |
| | (2.32) | (2.54) | (2.35) | (2.35) |
| *POST* | 0.0308 | -0.0512 | -0.0767 | -0.0716 |
| | (0.36) | (-0.50) | (-0.44) | (-0.54) |
| *SIZE* | | 0.0335 | 0.0377 | 0.0524 |
| | | (0.51) | (0.39) | (0.68) |
| *LEV* | | 0.6330** | 1.0319** | 0.2632 |
| | | (2.09) | (2.22) | (0.75) |
| *ROA* | | -1.6349* | -2.6335* | -1.5341 |
| | | (-1.73) | (-1.82) | (-1.50) |
| *LOSS* | | 0.1484 | 0.0994 | 0.1093 |
| | | (0.79) | (0.35) | (0.50) |
| *GROWTH* | | -0.0371 | -0.0349 | -0.1431 |
| | | (-0.37) | (-0.23) | (-1.11) |
| *CURRENT* | | 0.0353 | 0.0290 | 0.0127 |
| | | (1.12) | (0.53) | (0.36) |
| *AGE* | | 0.0783 | -0.0568 | 0.0721 |
| | | (0.55) | (-0.26) | (0.44) |
| *TENURE* | | -0.0021 | 0.0914 | 0.0360 |
| | | (-0.02) | (0.53) | (0.25) |
| *MAO* | | 0.5888*** | 0.3454 | 0.7811*** |
| | | (3.16) | (1.07) | (3.74) |
| Merger Fixed Effects | Yes | Yes | Yes | Yes |
| Industry Fixed Effects | Yes | Yes | Yes | Yes |
| Joint Tests: | | | | |
| *TREAT*+*POST*×*TREAT* = 0 | $Chi^2$ = 0.33 | $Chi^2$ = 0.40 | $Chi^2$ = 0.15 | $Chi^2$ = 0.57 |
| | [*p* = 0.566] | [*p* = 0.529] | [*p* = 0.702] | [*p* = 0.451] |
| N | 9795 | 9795 | 4265 | 6468 |
| Pseudo $R^2$ | 0.1247 | 0.1457 | 0.1859 | 0.1377 |

Notes: Panel A reports the descriptive statistics of *MISSTATEMENT*. The *t*-statistics are reported in parentheses, and the number of observations for each group is reported in brackets. Panel B presents the logistic regression results of using *MISSTATEMENT* as the dependent variable. Column 1 reports the results of estimating Equation (1) without time-varying control variables for the full sample, column 2 includes the full set of control variables for the full sample, column 3 restricts the control group to clients in the same industry as the treated clients, and column 4 restricts the control group to clients in a different industry from the treated clients. Details on the definition and construction of the variables reported in the table are available in Appendix B. For *SIZE, AGE* and *TENURE,* log-transformed values are used in the regressions. *z*-statistics shown in parentheses are adjusted for clustering by client. $^{*}$, $^{**}$, $^{***}$ indicate significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

**TABLE 4: Alternative Definitions of Treated Clients**

| Dependent Variable: *MISSTATEMENT* | (1) *TREAT*=*TREAT*$_1$ | (2) *TREAT*=*TREAT*$_2$ | (3) *TREAT*=*TREAT*$_3$ | (4) *TREAT*=*TREAT*$_4$ |
|---|---|---|---|---|
| ***POST*×*TREAT*** | **-0.4898**$^{**}$ | **-0.6643**$^{**}$ | **-0.7154**$^{***}$ | **-0.5969**$^{***}$ |
| | **(-2.25)** | **(-2.18)** | **(-2.90)** | **(-2.58)** |
| *TREAT* | 0.4095$^{**}$ | 0.6251$^{**}$ | 0.5227$^{**}$ | 0.6934$^{***}$ |
| | (2.08) | (2.20) | (2.34) | (3.31) |
| *POST* | -0.0510 | -0.0850 | -0.0480 | -0.0608 |
| | (-0.49) | (-0.85) | (-0.47) | (-0.59) |
| Controls | Yes | Yes | Yes | Yes |
| Merger Fixed Effects | Yes | Yes | Yes | Yes |
| Industry Fixed Effects | Yes | Yes | Yes | Yes |
| Joint Tests: | | | | |
| *TREAT*+*POST*×*TREAT* = 0 | $Chi^2$ = 0.15 | $Chi^2$ = 0.02 | $Chi^2$ = 0.60 | $Chi^2$ = 0.19 |
| | [*p* = 0.694] | [*p* = 0.882] | [*p* = 0.438] | [*p* = 0.660] |
| N | 9795 | 9795 | 9795 | 9795 |
| Pseudo $R^2$ | 0.1451 | 0.1452 | 0.1456 | 0.1468 |

Notes: This table presents the regression results with alternative definitions of treated clients we remove the requirement that the distance of the industry market share rank between the two audit firms needs to be at least five, and impose alternative requirements on client companies to be classified as being treated. Specifically, we classify the audit firm with a lower industry market share as being less competent in that industry, and define its clients in that industry as the treatment group (*TREAT*$_1$). *TREAT*$_2$ equals one when (1) *TREAT*$_1$ equals one, and (2) the more competent auditor in the merger is an industry expert (i.e., ranked as a top five auditor in terms of industry market share). *TREAT*$_3$ equals one when (1) *TREAT*$_1$ equals one, and (2) the within-audit firm industry portfolio share is larger than that of the other audit firm, where the industry portfolio share is computed as the number of listed clients in that particular industry divided by the total number of clients. *TREAT*$_4$ equals one when (1) *TREAT*$_1$ equals one, and (2) the total assets of clients in that particular industry audited by the audit firm are larger than those of the other audit firm. The full set of control variables (as in Table 3) are included but not reported for brevity. Details on the definition and construction of the variables reported in the table are available in Appendix B. *z*-statistics shown in parentheses are adjusted for clustering by client. $^{*}$, $^{**}$, $^{***}$ indicate significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

**TABLE 5: Alternative Samples**

| Dependent Variable: *MISSTATEMENT* | (1) Removing Mergers Involving International Big N | (2) Restricted Sample without Audit Personnel Movement | (3) Augmented Sample including Dropped Clients |
|---|---|---|---|
| ***POST×TREAT*** | **-0.6715*** ** | **-0.7417** ** | **-0.6655*** ** |
| | **(-2.66)** | **(-2.53)** | **(-2.68)** |
| *TREAT* | 0.5246** | 0.6916*** | 0.5577*** |
| | (2.45) | (2.83) | (2.64) |
| *POST* | -0.0887 | -0.1322 | -0.0654 |
| | (-0.85) | (-1.05) | (-0.64) |
| *POST×TREAT_DROP* | | | -0.7581* |
| | | | (-1.69) |
| *POST×CONTROL_DROP* | | | -0.6119** |
| | | | (-2.39) |
| *TREAT_DROP* | | | 0.7992*** |
| | | | (2.67) |
| *CONTROL_DROP* | | | 0.6016*** |
| | | | (3.10) |
| Controls | Yes | Yes | Yes |
| Merger Fixed Effects | Yes | Yes | Yes |
| Industry Fixed Effects | Yes | Yes | Yes |
| Joint Tests: | | | |
| *TREAT*+*POST×TREAT* = 0 | $Chi^2$ = 0.45 | $Chi^2$ = 0.04 | $Chi^2$ = 0.25 |
| | [*p* = 0.504] | [*p* = 0.840] | [*p* = 0.616] |
| N | 9474 | 7058 | 11480 |
| Pseudo $R^2$ | 0.1473 | 0.1689 | 0.1379 |

Notes: This table presents the logistic regression results for three alternative samples. Column 1 presents the results after removing all mergers involving the international Big N (three cases). Columns 2 presents the results for a restricted sample in which both the engagement partner and the review partner for the client company after the merger belong to the client's audit firm before the merger. We manually verify each partner's employment history from the auditor resumes provided by the CSRC. Columns 3 presents the results for an augmented sample including client companies that switched audit firms after the merger. *TREAT_DROP* is a dummy variable that equals one for dropped client companies that would have been classified as a treated client, and *CONTROL_DROP* is a dummy variable that equals one for dropped client companies that would have been classified as a control client. The full set of control variables (as in Table 3) are included but not reported for brevity. Details on the definition and construction of the variables reported in the table are available in Appendix B. *z*-statistics shown in parentheses are adjusted for clustering by client. *, **, *** indicate significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

**TABLE 6: Timing of Audit Performance Improvement**

| Dependent Variable: *MISSTATEMENT* | (1) | (2) | (3) |
|---|---|---|---|
| ***PRE_2×TREAT*** | **0.0286** | | **0.0364** |
| | **(0.07)** | | **(0.09)** |
| ***PRE_1×TREAT*** | **-0.4376** | | **-0.4262** |
| | **(-1.07)** | | **(-1.05)** |
| ***POST×TREAT*** | **-0.8551**** | | |
| | **(-2.25)** | | |
| ***POST_0×TREAT*** | | **-0.3905** | **-0.5666** |
| | | **(-1.43)** | **(-1.34)** |
| ***POST_13×TREAT*** | | **-1.0520***** | **-1.2266***** |
| | | **(-2.90)** | **(-2.88)** |
| *TREAT* | 0.7194** | 0.5479*** | 0.7215** |
| | (1.98) | (2.58) | (1.99) |
| *PRE_2* | 0.2937* | | 0.2802* |
| | (1.89) | | (1.81) |
| *PRE_1* | 0.4239** | | 0.3967** |
| | (2.45) | | (2.31) |
| *POST* | 0.2454 | | |
| | (1.42) | | |
| *POST_0* | | 0.1840* | 0.4591*** |
| | | (1.87) | (2.68) |
| *POST_13* | | -0.3253** | -0.0414 |
| | | (-2.35) | (-0.21) |
| Controls | Yes | Yes | Yes |
| Merger Fixed Effects | Yes | Yes | Yes |
| Industry Fixed Effects | Yes | Yes | Yes |
| N | 9795 | 9795 | 9795 |
| Pseudo $R^2$ | 0.1471 | 0.1505 | 0.1518 |

Notes: This table presents the logistic regression results with four additional dummy variables. *PRE_1* and *PRE_2* equal one for observations that are one or two years before the merger (i.e., Year –1 and –2), respectively, and zero otherwise. *POST_0* equals one if the client observation belongs to the merger event year, and zero otherwise. *POST_13* equals one if the client observation belongs to the three-year period after the merger event year, and zero otherwise. The full set of control variables (as in Table 3) are included but not reported for brevity. Details on the definition and construction of the variables reported in the table are available in Appendix B. *z*-statistics shown in parentheses are adjusted for clustering by client. *, **, *** indicate significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

**TABLE 7: Alternative Benchmark Groups**

| Dependent Variable: *MISSTATEMENT* | (1) | (2) |
|---|---|---|
| ***POST×TREAT*** | **-0.7767*** ** | **-1.1265*** ** |
| | **(-3.05)** | **(-4.10)** |
| *TREAT* | 0.5387** | 0.3973 |
| | (2.23) | (1.28) |
| *POST* | -0.1067 | 0.1738 |
| | (-1.04) | (1.20) |
| Controls | Yes | Yes |
| Merging Firm Fixed Effects | Yes | Yes |
| Industry Fixed Effects | Yes | Yes |
| Joint Tests: | | |
| *TREAT*+*POST*×*TREAT* = 0 | $Chi^2$ = 0.91 | $Chi^2$ = 5.51 |
| | [*p* = 0.339] | [*p* = 0.019] |
| N | 9713 | 4820 |
| Pseudo $R^2$ | 0.1799 | 0.2156 |

Notes: This table presents the logistic regression results with merging audit firm fixed effects (instead of merger fixed effects). Column 1 uses the full sample, while column 2 restricts the benchmark group to those clients that belong to an industry whose within-audit firm portfolio share is no larger than that of the treated clients. The full set of control variables (as in Table 3) are included but not reported for brevity. Details on the definition and construction of the variables reported in the table are available in Appendix B. *z*-statistics shown in parentheses are adjusted for clustering by client. *, **, *** indicate significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

**TABLE 8: Differential Treatment Effects**

| Dependent Variable: *MISSTATEMENT* | (1) | (2) |
|---|---|---|
| ***POST×TREAT_BIG*** | **-1.3790**** | **-1.4623**** |
| | **(-1.96)** | **(-1.97)** |
| ***POST×TREAT_SMALL*** | **-0.5131**** | **-0.6284**** |
| | **(-1.99)** | **(-2.37)** |
| *TREAT_BIG* | 0.7105** | 0.8337* |
| | (2.01) | (1.87) |
| *TREAT_SMALL* | 0.4983** | 0.4659* |
| | (2.03) | (1.67) |
| *POST* | -0.0524 | -0.1074 |
| | (-0.51) | (-1.04) |
| Controls | Yes | Yes |
| Merger Fixed Effects | Yes | No |
| Merging Firm Fixed Effects | No | Yes |
| Industry Fixed Effects | Yes | Yes |
| Equal Treatment Effect? | | |
| *POST×TREAT_BIG* = *POST×TREAT_SMALL* | $Chi^2 = 1.38$ [$p = 0.240$] | $Chi^2 = 1.16$ [$p = 0.282$] |
| N | 9795 | 9713 |
| Pseudo $R^2$ | 0.1461 | 0.1802 |

Notes: This table presents the logistic regression results after we partition treated clients into two groups: treated clients in the relatively big audit firm (*TREAT_BIG*) versus treated clients in the relatively small audit firm (*TREAT_SMALL*). The full set of control variables (as in Table 3) are included but not reported for brevity. Details on the definition and construction of the variables reported in the table are available in Appendix B. *z*-statistics shown in parentheses are adjusted for clustering by client. *, **, *** indicate significance at the 10%, 5%, and 1% levels (two-tailed), respectively.

**TABLE 9: Audit Fees**

Panel A: Descriptive Statistics of *FEE* (¥000)

| | *TREAT*=1 | *TREAT*=0 | Difference in Means: (1) vs (2) | |
|---|---|---|---|---|
| | (1) | (2) | Difference | *t*-stat |
| Pre-Merger | 549.6 [N=390] | 679.8 [N=2710] | -130.2*** | (-4.37) |
| Post-Merger | 722.2 [N=503] | 858.0 [N=4083] | -135.8*** | (-4.16) |
| Difference: Post vs Pre | 172.6*** | 178.2*** | -5.61 | (-0.12) |
| *t*-stat | (4.97) | (11.03) | | |

Panel B: Models of *FEE*

| Dependent Variable: *FEE* | (1) | (2) |
|---|---|---|
| ***POST×TREAT*** | **-0.0074** | **-0.0188** |
| | **(-0.30)** | **(-0.82)** |
| *TREAT* | -0.1086*** | -0.0315 |
| | (-3.43) | (-0.92) |
| *POST* | 0.0705*** | 0.0660*** |
| | (6.18) | (5.92) |
| Controls | Yes | Yes |
| Merger Fixed Effects | Yes | No |
| Merging Firm Fixed Effects | No | Yes |
| Industry Fixed Effects | Yes | Yes |
| N | 7686 | 7686 |
| Adjusted $R^2$ | 0.5750 | 0.5862 |

Notes: The sample in this table is restricted to mergers over the 2001–2013 period because client companies started to disclose audit fees in 2000. Panel A reports the descriptive statistics of *FEE*. The *t*-statistics are reported in parentheses, and the number of observations for each group is reported in brackets. Panel B presents the OLS regression results of using the log-transformed value of *FEE* as the dependent variable. The full set of control variables (as in Table 3) are included but not reported for brevity. Details on the definition and construction of the variables reported in the table are available in Appendix B. *t*-statistics shown in parentheses are adjusted for clustering by client. *, **, *** indicate significance at the 10%, 5%, and 1% levels (two-tailed), respectively.