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[How Big-4 Firms Improve Audit Quality](#)

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# How Big-4 Firms Improve Audit Quality

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## Abstract

This paper studies whether and how Big-4 firms provide higher quality audits than non-Big-4 firms. Specifically, we first examine a Big-4 effect and then explore three *sources* of the Big-4 effect. To test the Big-4 effect, we use a unique dataset of individual audit partners for a large sample of private companies and employ a novel research design exploiting the fact that auditees may follow the auditor who switches affiliation from a non-Big-4 to a Big-4 firm. Thus, we compare audit quality and audit fees of the same partner-auditee pairs before and after the switch. The results show that the Big-4 effect exists in the private-firm segment. More importantly, we find evidence for three sources of the Big-4 effect. First, Big-4 firms are *able to recruit* non-Big-4 partners who deliver higher audit quality than other non-Big-4 partners in the pre-switch period. Second, *enhanced learning* has taken place after the switch. Third, the increased audit quality can also be attributed to stronger *incentives/monitoring*. These are new findings to the literature.

Key words: Big-4 effect, auditing, audit quality, labor economics, learning, incentives, monitoring, auditor change, private firms, research design

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# How Big-4 Firms Improve Audit Quality

## Abstract

This paper studies whether and how Big-4 firms provide higher quality audits than non-Big-4 firms. Specifically, we first examine a Big-4 effect and then explore three *sources* of the Big-4 effect. To test the Big-4 effect, we use a unique dataset of individual audit partners for a large sample of private companies and employ a novel research design exploiting the fact that auditees may follow the auditor who switches affiliation from a non-Big-4 to a Big-4 firm. Thus, we compare audit quality and audit fees of the same partner-auditee pairs before and after the switch. The results show that the Big-4 effect exists in the private-firm segment. More importantly, we find evidence for three sources of the Big-4 effect. First, Big-4 firms are *able to recruit* non-Big-4 partners who deliver higher audit quality than other non-Big-4 partners in the pre-switch period. Second, *enhanced learning* has taken place after the switch. Third, the increased audit quality can also be attributed to stronger *incentives/monitoring*. These are new findings to the literature.

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## How Big-4 Firms Improve Audit Quality

### 1 Introduction

“The Big-4 effect,” or the idea that Big-4 audit firms provide audits of higher quality than non-Big-4 firms, has been observed in numerous studies of public firms (DeFond and Zhang 2014). *How* it occurs, however, has hardly been analyzed, most likely due to lack of data. We provide new and novel evidence on three potential sources of the Big-4 effect: The ability to recruit higher-quality personnel, increased emphasis on learning, and stronger incentives and monitoring systems.

To test for the Big-4 effect, we use an innovative research design that exploits the fact that when an audit partner switches audit-firm affiliation, some auditees follow the partner (hereafter, the partner-auditee pair). Thus, we hold the pairs of the engagement *partner* and *auditees constant*. By examining audit quality of the same partner-auditee pair before and after the pair switches to the Big-4 firm, we can isolate the audit-firm effect. This is important as the Big-4 effect could be due to auditor-level characteristics (e.g., higher ability) and auditee-level characteristics (e.g., client-risk profiles) as well as audit-firm factors (e.g., more stringent procedures and policies). If the incoming partners deliver higher audit quality for the same auditees after they start working for a Big-4 firm, we interpret the change in quality as a Big-4 effect. As higher quality is likely to be priced, we also examine changes in audit fees.

We explore three mechanisms that Big-4 firms may employ to increase audit quality. First, we consider the ability to recruit higher quality auditors, which has been implicitly assumed but not previously documented in the literature. Specifically, we test whether Big-4 firms are able to recruit partners from the non-Big-4 segment who deliver higher audit quality than other non-Big-4 partners prior to switching. Second, we examine whether the incoming partners increase their formal learning activities after switching to Big-4 firms. As the potential for learning is higher for partners who join large Big-4 offices because these have more expertise and peers to consults, we also test if the increase in audit quality is higher for partners

who join the largest Big-4 offices. Third, we examine the effects of increased monitoring and stronger incentives by analyzing audit-quality measures that reflect monitoring/incentives (not learning) as well as changes in the partners' personal wealth.

Our setting gives access to data that are generally not accessible in most other countries, such as the name of the engagement partners, the number of hours of continuing professional education, and partners' personal income and wealth. In addition, as we can follow partner-auditee pairs as they switch audit-firm affiliation, our Big-4 effect tests are less vulnerable to endogeneity and self-selection, which plague most Big-4 effect research.<sup>1</sup> First, we control for audit partners' time-invariant ability in the Big-4 effect tests because we only compare audit quality (and fees) delivered by the *same* partner before and after the switch to Big-4 firms. It is important to follow the same partner over time because differences among partners influence audit quality (DeFond and Francis 2005; Francis 2011; Knechel, Vanstraelen, and Zerni 2015). Second, we mitigate an important self-selection problem by requiring that the partner audits the *same clients* before and after the affiliation switch (while controlling for changes in the auditees' risk, complexity, and size in the years surrounding the switch year). Thus, we do not need to match clients of the Big-4 firms with those of the non-Big-4 firms on client characteristics (e.g., Lawrence, Minutti-Meza, and Zhang 2011; DeFond, Erkens, and Zhang 2017). Third, we employ fixed auditor-auditee pairs in a sample in which the proportion of auditees following the switching partner is high, implying that it is less likely that our results are influenced by how Big-4 audit firms manage their client portfolios (Johnstone 2000; Bedard, Deis, Curtis, and Jenkins 2008) or the auditees' decision to follow the switching

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<sup>1</sup> Non-randomness is inherent in audit research because clients do not choose their audit firms randomly, individuals do not seek employment randomly across audit firms, audit firms do not toss heads or tails on whom to hire or which clients to accept, and it is unlikely that audit firms assign clients to partners randomly. The different layers of non-randomness make it difficult to draw causal conclusions because the various counterfactual scenarios that could have occurred do not exist – we cannot observe the audit quality of auditee A audited by partner P in audit firm C when it was partner Q in audit firm D who performed the audit.

partner. Fourth, we analyze private firms that are generally considered to be of lower reputation risk to audit firms than public clients (Bell, Bedard, Johnstone, and Smith 2002; Johnstone and Bedard 2003) in Norway, which is a low litigation-risk country (Hope and Langli 2010). Furthermore, none of the partners we analyze become auditors of public clients. Thus, effects of reputation and litigation risks on audit quality and audit fees are reduced.

We use several measures of audit quality that capture different facets of audit quality in order “to take advantage of [the proxies’] strengths and attenuate their weaknesses” (DeFond and Zhang 2014, 276). Our tests allow for both an immediate effect and a more gradual effect on audit quality/fees after the switch. Immediate effects may for instance occur because Big-4 firms have stronger incentives and better monitoring mechanisms, which discipline both partners and their auditees such that audit quality increases. However, a gradual change in audit quality could also happen because learning takes time. For example, the switching partners need time to learn and the Big-4 firms need time to train incoming auditors and implement the firm’s audit technology for the new clients. The effect on audit fees may be gradual because the Big-4 firm may fear losing the incoming partner’s clients if they immediately increase fees.

We document higher going-concern (GC) reporting accuracy, less use of modified opinions, less earnings management, and higher audit fees after the switch. The increase in the GC accuracy and the reduced use of modified opinions is immediate while the increases in earnings quality and audit fees occur gradually. The former effect is consistent with increased incentives and better monitoring and learning, while the latter more likely reflects gradual learning. The increase in the GC accuracy coincides with a lower likelihood of issuing audit reports modified for GC uncertainty. Lower likelihood of modified audit opinions is often interpreted as indication of lower, not higher, audit quality. Our results suggest, however, that fewer modified GC opinions are a mechanism that enables Big-4 firms to increase the accuracy

of the GC reports.<sup>2</sup> We also find that increased earnings quality coincides with fewer audit reports modified for other reasons than GC uncertainty. The results are consistent with Big-4 firms being better able to accurately identify and evaluate financially-troubled firms (Geiger and Rama 2006; Boone, Khurana, and Raman 2010; Myers, Schmidt, and Wilkins 2014) and to provide a fair and faithful representation of the auditees' financial situations, resulting in less use of modified reports.

As to the *sources* of increased audit quality, we first show that Big-4 firms are able to *attract higher-quality inputs*. Specifically, *prior* to moving to the Big-4 firm, the incoming partners provide higher-quality audit work and receive higher fees than partners who do not move up to Big-4 firms. Importantly, even though the Big-4 firms attract partners of higher quality, we document that that these partners deliver *even higher* audit quality after the switch. Next, we show that the incoming partners spend significantly more hours on continuing professional education, consistent with increased learning taking place. We also find that audit quality increases significantly more for the partners who switch to the largest Big-4 offices. We interpret this result as being due to learning as larger offices have more experts and peers to consults, which enables better learning.

Stronger incentives (monitoring) also contribute to increased audit quality. For delayed filings, an audit-quality measure that only relates to monitoring and/or incentives (not learning), we document an immediate increase in audit quality. Because we follow the same partner-auditee pair as they switch to Big-4 firms and nothing has prevented the partner-auditee pair from filing in time prior to the switch, these partner-auditee pairs needed the Big-4 effect to improve their timeliness. Furthermore, we show that the incoming partners enjoy higher

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<sup>2</sup> Better resources and more experts in Big-4 firms can be one of the explanations for why the auditor issues fewer modifications after becoming part of a Big-4 firm. The Big-4 firms may make the auditor better able to hinder earnings management and otherwise provide the auditor with the tools necessary for persuading the auditees to follow the regulations or advice given, which reduce the need to modify the audit report.

compensation and personal wealth after the switch, which leads to greater incentives to keep audit quality high (i.e., there is more at stake or “more to lose”). To the best of our knowledge, these analyses provide new evidence on the sources of the Big-4 effect.

We perform several sensitivity tests to validate our evidence of the Big-4 effect and in particular to address any residual endogeneity that may remain. First, we implement a difference-in-differences test with propensity-score matching. Second, we perform a placebo analysis by testing if switching affiliation from one non-Big-4 firm to another non-Big-4 firm (i.e., lateral switches) causes changes in audit quality and fees. This is important as it mitigates the concern that switching affiliation *per se* may induce changes in quality. Third, to assess whether there is an upward trend in audit quality for the switching auditees, we implement another placebo analysis by artificially picking the year three years prior to the actual switch as the “pseudo-switch year” and repeating the tests (using only observations prior to the actual switch). Fourth, we control for partners’ age, gender, years of experience, level of education, and whether they move from one town to another. Fifth, we document that the auditees that follow the partners are not less risky than other auditees of non-Big-4 firms. Sixth, using multiple measures of financial health, we consider the possibilities that clients are more likely to follow the audit partner if they expect an improvement in their financial health or that the Big-4 firm is more willing to accept the partner’s former client into its portfolio if the Big-4 firm believes there will be an improvement in the company’s financial health. Finally, we analyze switches that occur as a result of audit-firm mergers only. Our inferences are unaffected in these tests.

We believe that this paper offers strong contributions to the literature. First, we add to the labor-economics and auditing literatures on whether employers can pick high-quality employees and subsequently further contribute to the performance of their employees (e.g., Groysberg, Lee, and Nanda 2008; Kim, Morse, and Zingales 2009; Rozenbaum 2014). Second,



to the best of our knowledge, we are the first to empirically document three *sources* of Big-4 firms' higher audit quality. DeFond and Zhang (2014) encourage researchers to focus more on why the Big-4 effect occurs, and we respond to this call. Third, we propose a new research design to test the Big-4 effect by exploiting that the audit partner and their auditees may switch audit-firm affiliation together. The main advantage of this research design is that it allows researchers to hold audit-partner and auditee pairs constant, which enables a better identification of audit-firm factors on audit quality. Our design differs from Jiang, Wang, and Wang (2019) who analyze mergers between non-Big N and Big N audit firms in U.S., but without being able to control for changes in the engagement partner. They document higher audit quality after a merger, but it is an open question whether this is due to the Big-4 audit firm effect or the non-Big N partner being replaced by a more qualified and experienced Big-N partners after the merger.<sup>3</sup> Finally, we find evidence consistent with the Big-4 effect in the private-firm segment, where the results are more scant and mixed than in the public-firm segment (DeFond and Zhang 2014; Langli and Svanström 2014; Vanstraelen and Schelleman 2017). The private segment is pertinent because of its economic significance for the overall economy and in particular for the audit industry (i.e., many private firms disclose audited financial statements either voluntarily or mandatorily).<sup>4,5</sup>

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<sup>3</sup> Other important differences with Jiang et al. (2019) include: (i) Jiang et al.'s control sample consists of auditees of other audit firms. In our design, the control firms are the *same firms* before the switch in audit-firm affiliation. Consequently, the potential effect of unobserved client characteristics is of less concern in our design. (ii) Much larger sample of firms that switch audit-firm affiliation, which increases the power of our tests. (iii) Our ability to analyze three potential sources of increased audit quality. The improvement in audit quality documented in Jiang et al. (2019) can be a result of an improved non-Big4 auditor providing a higher quality audit on the same client or a Big-4 auditor auditing the non-Big4 client after the merger or acquisition (i.e., the client switches from a non-Big4 auditor to a Big-4 auditor). However, our paper provides results of the former channel and go deeper into the reasons for the non-Big4 auditors' improvement such as learning and incentives.

<sup>4</sup> As but one example, disclosure of audited financial statements is mandatory for all private firms exceeding nationally set firm-size thresholds in all EU-countries. We provide details of our institutional setting (Norway) in appendix A.

<sup>5</sup> Private firms are dominant around the world but are less researched than publicly traded companies. To illustrate the economic clout of private firms, Berzins, Bøhren, and Rydland (2008) show that, in the aggregate, Norwegian nonlisted firms have about four times more employees than listed firms, have three times higher revenues, and twice the amount of assets, and that these statistics are representative for most countries in the world. In fact, more

## 2 Literature and Hypotheses

### 2.1 The Big-4 Effect

Numerous studies have investigated, both in substance and in perception, whether Big-4 audit firms provide higher quality audits than non-Big-4 firms. For public firms, there is ample empirical evidence of a Big-4 effect (DeFond and Zhang 2014). In the private-client segment, however, the empirical evidence is mixed and more limited (Langli and Svanström 2014; Vanstraelen and Schelleman 2017).

Most archival audit research has been conducted at the audit-firm or audit-office level, which presumes that audit quality is uniform across audit firm/office. However, as audits are performed by individuals, the Big-4 effect may be even more salient at the partner level. We analyze partner-level data, and we focus on partners who switch from non-Big-4 firm to Big-4 firms.<sup>6</sup> One reason for why partners switch audit-firm affiliation is mergers between audit firms. With the notable exception of the concurrent study by Jiang et al. (2019), the literature analyzing mergers between audit firms primarily focuses on auditees' choice of a new audit firm when their audit firm merges or is dissolved, and not on the Big-4 effect.<sup>7</sup> Jiang et al.

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than 99% of limited liability companies are not listed on a stock exchange. In the U.S., there are about 8 million private firms with paid employees, representing one-half of the nation's GDP (Hope, Thomas, and Vyas 2013, 2017; Hope and Vyas 2017). The primary reason for limited research on private firms in the U.S. is clearly the *lack of data* (i.e., such studies cannot be conducted). In contrast, Europe offers such possibilities. Further, employing Norway as our research setting (a country that has the same audit-related regulation as the EU, thus generalizing beyond the single-country setting), we have the extremely detailed data that allow us to pursue our specific research questions.

<sup>6</sup> In sensitivity analyses we also examine switches in the other direction.

<sup>7</sup> Blouin et al. (2007) analyze auditees' auditor choices following the dissolution of Arthur Andersen in the aftermath of the Enron scandal. Chen, Su, and Wu (2009) examine similar forced audit changes affecting eight audit firms with listed clients in China, but these do not involve Big-N firms. None of the Big-4 acquisitions of non-Big-4 audit firms in our sample is preceded by known audit-firm scandals or other events that force the non-Big-4 audit firms to stop operating as independent non-Big-4 audit firms. Audit firms that are dissolved due to audit failures may have attracted clients with certain characteristics. Chen et al. (2009) show that clients with greater earnings management prior to the switch are more likely to follow their partner to the new audit firm, and that these clients become more aggressive in the second and third years following the switch (but are not different from others in the switching year). This effect is not observed for clients that do not follow. Healy and Lys (1986) analyze the auditor choices of auditees of two non-Big-N that merged with Big-N firms in 1976 and 1978. Chen,

(2019) test for Big-N effects in the U.S. using the auditees of non-Big-N firms that became clients of Big-N firms through 20 acquisitions during the years 1976 – 1995. Consistent with the majority of studies using auditees that are listed, they find evidence of a Big-N effect for some of their audit-quality measures. Our study complements and extends the findings in Jiang et al. (2019) by analyzing private firms from a more recent period (after the introduction of SOX and PCAOB inspections of audit firms), but most importantly, we are able to keep the partner-auditee pairs constant (and also identify *sources* of increased audit quality).

Theoretical predictions support the idea that larger audit firms should deliver higher audit quality than smaller audit firms (DeAngelo 1981; Dopuch and Simunic 1980). For example, the Big-4 firms have better incentive and quality control systems and more experts in auditing, accounting, tax, and valuation (Francis 2011; Knechel, Niemi, and Zerni 2013). Further, the Big-4 firms continually invest in and employ cutting-edge audit methodology for all clients, and the Big-4 firms' resources (broadly defined) are available to auditors of both private and public clients.<sup>8</sup> Therefore, we expect that audit quality should be higher for Big-4 firms compared to non-Big-4 firms, also in the private-client segment. Because higher quality is likely to be priced, we also expect to observe an increase in audit fees. Our first set of hypotheses (all hypotheses are stated in the alternative form) are:

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Su, and Wu (2010) investigate the loss of clients that occurred when a local Chinese audit firm with 46 clients merged with Ernst & Young. Chan and Wu (2011) analyze 59 cases where Chinese audit firms merge, but only three cases involve Big-N firms. Chan and Wu (2011) find that increases in audit-firm size as a result of the merger do not matter for audit independence, but what matters is the size of the audit firms' public clients. The auditees in all these studies are listed on U.S. or Chinese stock exchanges, and only Chen et al. (2009) control for the identity of the partner.

<sup>8</sup> The Wall Street Journal article "Auditing Firms Count on Technology for Backup" (Rapoport 2016) reports that "[t]he Big Four are pouring hundreds of millions of dollars into new technologies, betting they will make audits more accurate and comprehensive, giving investors greater assurance that a company's finances are sound. ... Ernst & Young made an initial capital investment of \$400 million to develop its technologies, which it began using last year." An older quote indicates that Big-N firms always want to be in the forefront of the development of systems and procedures: "Price Waterhouse is *continually* refining its audit approach, adding efficiencies and maximizing the use of new and emerging technologies in order to meet clients' auditing and other professional service needs... Our approach has universal applicability and results in a tailored, effective audit of each entity's financial statements" (Walker and Pierce 1988, 1-2, emphasis added).

*H1a:* When an audit partner switches affiliation from a non-Big-4 firm to a Big-4 firm, the partner will deliver audits of higher quality after the switch compared to before the switch.

*H1b:* When an audit partner switches affiliation from a non-Big-4 firm to a Big-4 firm, audit fees will increase.

It is not clear how long time it would take before audit quality increases after the switch because it may depend on the sources of increased quality (which we discuss in greater detail below). A gradual effect may occur if the increase is due to learning. For example, it may take time for the incoming partners to be fully acquainted with the Big-4 firms' audit methodology or to fully utilize the increased knowledge base that is available to them. Besides, the decision to change audit firm rests with the client. If the client finds that the partner becomes too strict in constraining the use of accruals or starts modifying the audit report for reasons that did not result in modified audit opinions previously, the client may decide to not follow the switching partner. Also, the clients may also elect a non-Big-4 firm if audit fees increase immediately after the switch. Both the partners and auditees are aware of these possibilities. Thus, to avoid losing clients, the effects on audit quality and audit fees may occur gradually. At the same time, the increase in audit quality may occur immediately if the primary source is increased monitoring and/or stronger incentives or if an increase is necessary to meet the Big-4 firms' minimum standard for audit quality. Thus, Big-4 firms may rather risk losing the clients than allow sub-par performance.

## **2.2 Three Sources of the Big-4 Effect**

The question of why some firms and institutions in knowledge-intensive industries perform better than others is of broad interest (Kim, Morse, and Zingales 2009). For example, productive researchers tend to hold positions at prestigious universities, which could be due to a selection effect - that prestigious universities are better able to hire productive researchers.

Alternatively, it could be a firm effect - that better universities are able to fuel productivity through the facilities they possess, the intellectual stimulation that can take place, and their incentive systems. Allison and Long (1990) find most support for the latter, but the causation is not clear. A firm-effect is also documented by Rozenbaum (2014), Groysberg et al. (2008), and Groysberg and Lee (2008). Rozenbaum (2014) finds that performance improves (deteriorates) for financial analysts moving to brokerage houses with more (fewer) resources. Groysberg et al. (2008) find that star analysts who move to firms with lesser capabilities, and those who move without other team members, experience the most pronounced decline in performance, but they find no change in performance for those who move to firms with better capabilities. Groysberg and Lee (2008) show that top performers' past performance is indicative of future performance, but also that the quality of their colleagues significantly affects performance. Analyzing scientists, Oettl (2012) finds that the helpfulness of colleagues is an important determinant of performance. These studies suggest that both the employees' past performance and firm-specific resources are potential determinants of future performance.

Turning to the audit literature, the evidence of *why* the Big-4 effect exists is scarce. Both Causholli, De Martinis, Hay, and Knechel (2010) and DeFond and Zhang (2014) highlight the importance of focusing more on why Big-4 firms deliver higher audit quality and charge higher fees. We examine three potential sources.

*Recruitment policy:* As audit work is conducted by individual engagement partners and staff, one potential source of higher audit quality is that the Big-4 firms have higher-quality personnel. Higher-quality personnel could reflect training and experience that take place while being employed by a Big-4 firm. It could also reflect that Big-4 firms are better able to identify and recruit persons who are more motivated, better educated, and have higher innate abilities. Consequently, we hypothesize that one source of Big-4 firms' high audit quality is their ability to hire high-quality personnel.

Aobdia, Lin, and Petacchi (2015) implicitly assume that audit partners are endowed with innate abilities that drive the quality of their audit work. We argue that if a Big-4 effect exists, Big-4 firms should be able to influence the development and use of these abilities, independent of their innate abilities. The reasons are that Big-4 firms may stimulate or give better opportunities for learning and that they are better at incentivizing both auditees and partners to deliver higher quality audit through increased monitoring or better compensation.

*Learning:* Compared with non-Big-4 firms, Big-4 firms have more resources and incoming partners are exposed to a greater pool of resources. Rozenbaum (2014) and Groyberg and Lee (2008) show that the quality of the human resources that are available in brokerage houses is important for the performance of incoming analysts. From the audit literature, evidence shows that larger audit offices deliver higher-quality audits due to more collective experience, more peers to consult, greater in-house expertise in detecting material misstatements, more valuation and tax experts, and more internal support from legal consultants (Watts and Zimmerman 1981; Francis and Yu 2009; Sundgren and Svanström 2013). As Big-4 firms possess greater resources than non-Big-4 firms, we expect that Big-4 firms are able to further increase the ability and competence of incoming partners by providing facilities that may contribute to increased learning, especially for partners who switch to large offices.

One method for increasing learning is to provide high-quality training. Research has documented a positive association between educational training programs and performance (Russel, Terborg, and Powers 1985; Noe and Schmitt 1986; Delaney and Huselid 1996; Creter and Summey 2003). Within auditing, Bonner and Pennington (1991) state that the association between continuing professional education (CPE) and auditors' knowledge is well established. Regulators also acknowledge the importance of CPE and the EU requires all member states to ensure that auditors participate in CPE programs (EU 2014/56/EC, article 13). As Big-4 firms

are committed to deliver high-quality audits, they may require or incentivize incoming auditors to take more CPE than previously. A possible source of increased learning is therefore increased participation in CPE programs.

*Monitoring/incentives:* A third factor that may contribute to higher audit quality is stronger monitoring/incentives. Rules and regulations are in place to ensure sufficient audit quality, and all Norwegian audit firms are required by law to have internal-control systems and undergo periodic reviews. A periodic review includes an assessment of the firm's internal-control system. For audit firms with private clients only, the periodic reviews take place at a minimum of every sixth year. The inspections are undertaken by members of the Norwegian Institute of Public Accountants (DnR) in cooperation with the Financial Supervisory Authority of Norway (FSAN). By comparison, the inspections are undertaken by the FSAN at least every third year for auditors of public clients. Further, for auditors of public clients that are listed in the U.S., the inspections are undertaken at least every third year by both FSAN *and* the U.S. Public Company Accounting Oversight Board (PCAOB). While the FSAN administers the inspections, both the FSAN and PCAOB write separate reports to the inspected firms (Finanstilsynet 2009). *All* Big-4 firms in Norway have some clients listed on U.S. stock exchanges and are thus subject to PCAOB inspections. In contrast, *none* of the non-Big-4 audit firms are inspected by PCAOB. We expect that the more frequent and more stringent inspections conducted by both FSAN and PCAOB (relative to the DnR-only inspections) create a stronger incentive for a Big-4 firm to maintain sufficient quality in their internal-control system, which includes the engagement-review process.<sup>9</sup> Therefore, Big-4 firms may be more

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<sup>9</sup> It is interesting to note that the PCAOB inspections review not only specific engagements but also the overall internal control system (in line with our arguments in the text). The following is copied from a letter PCAOB sent to Deloitte (Norway) after an inspection: "Review of Quality Control System. In addition to evaluating the quality of the audit work performed, the inspection included review of certain of the Firm's practices, policies, and procedures related to audit quality. This review addressed practices, policies, and procedures concerning audit performance and the following eight functional areas (1) tone at the top; (2) practices for partner evaluation,

stringent in monitoring their new auditors and may provide greater incentives for incoming partners to provide high-quality work. Big-4 firms may also give the incoming partners increased incentives and abilities to be independent as Big-4 firms have a greater pool of clients, making the partner less financially dependent on her specific portfolio.

Wealthier audit firms are perceived to deliver higher quality audits because they have more to lose (DeAngelo 1981), and wealth is an important driver of audit quality (Dye 1993). At the partner level, wealth may play a similar role. The compensation structure of partners is often complex and data in most jurisdictions are not available.<sup>10</sup> Sundgren and Svanström (2017) show that Big-4 partners who are sanctioned by the Swedish Public Oversight Body experience a significantly higher drop in salary after an audit failure compared to partners in non-Big-4 firms, which induce Big-4 partners to supply higher quality audits. In general, Big-4 firms tend to remunerate partners and staff more than non-Big-4 firms.<sup>11</sup> If partners' salary/wealth increases after they switch from non-Big-4 to Big-4 firms, this gives them stronger incentives to provide higher-quality audits. Based on the discussion above, we hypothesize the following:

**H2a:** The Big-4 firms recruit higher quality partners, that is, partners switching to Big-4 firms provide higher-quality audits before the switch than partners who remain with non-Big-4 firms.

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compensation, admission, assignment of responsibilities, and disciplinary actions; (3) independence implications of non-audit services; business ventures, alliances, and arrangements; personal financial interests; and commissions and contingent fees; (4) practices for client acceptance and retention; (5) practices for consultations on accounting, auditing, and SEC matters; (6) the Firm's internal inspection program; (7) practices for establishment and communication of audit policies, procedures, and methodologies, including training; and (8) the supervision by the Firm's audit engagement teams of the work performed by foreign affiliates." ([https://pcaobus.org/Inspections/Reports/Documents/2011\\_Deloitte\\_AS.pdf](https://pcaobus.org/Inspections/Reports/Documents/2011_Deloitte_AS.pdf), downloaded January 2, 2017).

<sup>10</sup> Knechel, Niemi, and Zerni (2013) and Dekyser, Gaeremynck, Knechel and Willekens (2016) analyze compensation data, but they do not provide information on pay differences between non-Big-4 and Big 4 firms as they analyze Big-4 firms only.

<sup>11</sup> *Big4 Partner Compensation – How high is it, really?* (<https://www.big4bound.com/partner-compensation/>, accessed October 17, 2018) and *2017 Audit (Practice & Internal Audit) Salary Guide* (<https://www.morganmckinley.co.uk/article/2017-audit-practice-internal-salary-survey-guide>, visited October 17, 2018).



*H2b*: Improvements in audit quality subsequent to partners switching to Big-4 firms can be attributed to enhanced learning.

*H2c*: Improvements in audit quality subsequent to partners switching to Big-4 firms can be attributed to greater incentives (monitoring).

### **3 Data and Research Design**

#### **3.1 Data**

We start by obtaining the names of the engagement partners from a tax form all clients that are registered as limited-liability firms must file with the Norwegian Tax Administration (NTA) from 2004 to 2012, and a list of *all* licensed auditors in Norway from the Financial Supervisory Authority of Norway (FSAN).<sup>12</sup> On the forms filed with NTA, the clients manually fill in the names of their auditors, and there may be misspelling or omission errors. To the extent possible, we manually match the names of individual auditors in the FSAN list with the names of auditors in the NTA data file. We are able to identify the names of the

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<sup>12</sup> The name of the *audit firm* is easily obtainable through electronic open registers. The name of the *engagement partner*, however, is only *electronically* available in a confidential register that is kept and maintained by NTA. Special permissions are needed to obtain data from NTA. Our permissions give us access to data for the years 2004 – 2012. The name of the partner is also stated in the audit report as the partner is required to sign the audit report. For each firm, it is possible to obtain a pdf-file of the financial statements including the audit report from the Brønnøysund Register Center (BRC) and providers of business-source information. However, given the very large sample size required to identify sufficient switchers, hand collection of the names of partners from pdf-files is not feasible. Note that in order to identify the switching partners, it is necessary to cover the population of client firms (Norwegian audit firms issued more than 234,000 audit reports in 2010).

individual auditors for about 72 percent of *all* limited-liability firms in Norway in the sample period.<sup>13,14</sup>

Next, we merge the data from NTA and FSAN with audit-related information provided by Experian AS and Eniro AS. The audit-related information includes the type of modified opinions, the audit fees, and the identities of audit firms. The accounting data are provided by the Center for Corporate Governance Research (CCGR) at BI Norwegian Business School. All private limited-liability firms are required by law to have their accounts audited (until May 1, 2011, when the very smallest firms were allowed to forgo having an auditor). Experian AS provides data on the dates the firms eventually were notified as having defaulted on debt payments (see Section 3.2 for details).

We identify a total of 77 partners who have switched from non-Big-4 firms to Big-4 firms (EY, KPMG, Deloitte, and PWC). Our test strategy requires that the partners must audit the *same* auditees before and after the switch and that the test and control variables can be computed. These criteria result in a final sample of 68 switching partners.<sup>15</sup>

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<sup>13</sup> Our data do not allow us to identify to what extent the auditor's audit team also switches when the auditor switches affiliation. We do not regard that as a threat to our inferences as the literature shows that the beliefs and preferences of partners can significantly affect the behaviors and attitudes of audit staff and actual audit quality (Ponemon and Gabhart 1990; DeZoort and Lord 1994; Trompeter 1994; Tan, Jubb, and Houghton 1997; Carcello, Hermanson, and Huss 2000; Wilks 2002; Emby, Gelardi, and Lowe 2002; Ayers and Kaplan 2003; Carey and Simnett 2006). For example, Wilks (2002) documents that partners' views influence audit staff' judgments while Peytcheva and Gillett (2011) show that knowledge of superiors' views affects audit-staff reports even when they learn their superiors' views after they reach their independent judgments. Our interviews with former and current partners in Big-4 firms reveal that both incoming partners and their team members are trained in the Big-4 firms' audit methodology.

<sup>14</sup> Appendix A provides a discussion of the audit market in Norway.

<sup>15</sup> Although it would be beneficial to have a larger number of switching partners, in practice most fixed-effects studies rely on a relatively low number of observations given the strict requirements that are imposed. For comparison, Gul, Wu, and Yang (2013) have a sample of 85 switches in their study of auditors' fixed effects in China. Recall that a key feature of our research design is that we keep *both* the audit *partner* and the *client firm* constant, while prior research holds only one constant (e.g., the CEO).

### 3.2 Measures of Audit Quality

We employ several measures of audit quality because no single measure draws a complete picture of audit quality, and different measures focus on different dimensions of audit quality. We use measures that are most applicable for private clients. While we acknowledge that these measures contain measurement errors and have different strengths and weaknesses, the use of multiple measures as well as audit fees helps reduce concerns related to their ability to capture audit quality.

Our first measure of audit quality is the accuracy of going-concern opinions (GC-opinions). GC-opinions relay auditors' judgments on whether there is substantial doubt about the client's ability to continue on a going-concern basis. Even though the responsibility of the auditor is not to predict future bankruptcies (but to assess "the appropriateness of management's use of the going-concern assumption," ISA 570 (6)), a stream of literature analyzes the accuracy of GC-opinions by observing whether a firm declares bankruptcy subsequent to receiving a GC-modified opinion. The measure is intuitively appealing, as more competent auditors should be better at identifying auditees that are likely to face financial distress and thus should be able to issue more accurate audit reports.

It is common to classify GC-opinions as correct or wrong depending on whether the auditee goes bankrupt within 12 months after the issuance of the audit report (Francis 2011; Knechel et al. 2015).<sup>16</sup> However, creditors of the auditee can suffer losses even if the auditee does not declare bankruptcy. Creditors sometimes would rather write off part or all of the existing debts or ask the courts to levy distress, rather than incurring the cost of ordering the company to be wound up in order to eventually regain some of their claims. Consistent with

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<sup>16</sup> Geiger and Rama (2006) find that GC opinions are more accurate predictors of future bankruptcy when the opinions are issued by Big-N auditors rather than non-Big-N auditors. Gaeremynck and Willekens (2003) find that Big-N auditors are better able to identify financially troubled companies that are less troubled than those that go bankrupt.

the definition of failure as the “inability of a firm to pay its financial obligations as they mature” (Beaver 1966, 71), we use a new measure, *public notification of unpaid debt*, as the yardstick for assessing going-concern accuracy. The notification takes form of a *payment remark*.<sup>17</sup>

Any creditor can obtain information about a firm’s payment remarks from credit-rating companies that have permission to use such information. For the creditors, the audit report would be useful if it provides a signal of the likelihood of the auditee’s subsequent payment remarks, as payment remarks imply that the auditee has defaulted on its payment. Thus, instead of classifying the accuracy of the GC-opinions using bankruptcies, we use the occurrence of payment remarks;  $DefaultDebtPay_{it} = 1$  if firm  $i$  receives payment remarks within 12 months after the publication of the annual report for year  $t$ , and 0 otherwise. We define  $GCAccuracy_{it}$  equal to 0 for firm  $i$  in year  $t$  if there is a Type-1 or a Type-2 error in the audit report, and 1 otherwise.<sup>18,19</sup> By using payment remarks, which is a less severe measure of financial distress than bankruptcy, we can test for changes in audit-reporting accuracy using the *same pair* of auditor-auditee before and after the switch (change analyses are not possible using bankruptcies as firms going bankrupt before the switch do not exist after the switch).

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<sup>17</sup> To collect unpaid debt, creditors may engage debt-collection firms. Debtors that do not pay their debt after receiving reminders are registered in the Register of Mortgaged Movable Property. If there is a disagreement between the creditor and the debtor regarding the validity of the claim, no remark is registered. The issuance of a payment remark can be compared to court decisions in the U.S. and the U.K., where firms get a judgment due to unpaid debt. Consistent with this argument, the credit-rating companies Experian and Dun & Bradstreet collect information about judgments in the U.S. and U.K. and payment remarks in Scandinavia, and the information enters the debtors’ credit reports. Banks include information on payment remarks in their default models (Carling, Jacobson, Lindé, and Roszbach 2007). The variable “Unpaid Debt” used by Knechel et al. (2015) is based on payment remarks.

<sup>18</sup> A Type-1 error occurs when the auditor issues an audit report due to GC uncertainty and the client does not receive a payment remark within 12 months after the publication of the annual report. A Type-2 error occurs when the client receives a payment remark within 12 months after the publication of the annual report and the audit report is not modified for GC uncertainty. An increase (decrease) in Type-1 (Type-2) errors is referred to as “conservative” (“aggressive”) reporting (Knechel et al. 2015).

<sup>19</sup> In the tabulated results,  $GCAccuracy$  is based on whether the firm has defaulted on payments or not. Untabulated results yield consistent inferences when we define  $DefaultDebtPay$  equal to 1 if the client has defaulted on more than 5, 10, or 25 percent of total debt (0 otherwise). Defaulting on more than 25 percent of total debt would likely constitute serious financial distress.

Next, we measure how the auditors' reporting decisions are affected using going-concern opinions (*GC*) and the number of modifications (*NumMod*), both defined using information from the audit report. *GC* equals 1 if the audit report expresses concern over the going-concern assumption, and 0 otherwise. *NumMod* equals the number of modifications in the audit report that are not related to *GC* uncertainties.<sup>20</sup> If, for example, the audit report contains comments due to disagreements with the auditee about the use of accounting estimates and weaknesses in the internal control systems, *NumMod* = 2. The advantage of using *NumMod* is that it is a broader measure of competence and independence than *GC*.

We also test for changes in the partner's views on earnings management, a more indirect but widely used proxy for audit quality. Specifically, we use discretionary accruals (Kothari, Leone, and Wasley 2005) and define *EarningsQuality* as the absolute value of the error terms multiplied by (-1); thus, higher values of *EarningsQuality* indicate higher audit quality.<sup>21</sup> See Appendix B for further details on variable definitions.

*GC* and *NumMod* intend to capture a partner's competence and independence, and the literature has usually interpreted higher values as indications of higher independence and higher audit quality. There is some debate in the literature as to how to assess the validity of these proxies. On one hand, Blay, Moon, and Paterson (2016) conclude that a higher propensity

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<sup>20</sup> Modifications are categorized into 20 different types based on ISA 700 (International Standard on Auditing 700 (Revised), Forming an Opinion and Reporting on Financial Statements). The most serious modification is a "negative conclusion" (the financial statements should not be approved or the auditor is unable to conclude). The least serious modification is "clarifications." In between, we have "reservations" (limitations with respect to scope or disagreements with management). Examples of reservations are modifications due to weaknesses in the internal-control system, negative equity, uncertain values of assets or liabilities, disagreement over accounting estimates, inadequate bookkeeping routines, and inability to verify the beginning balances. Examples of clarifications include delayed filing of the financial statements, illegal loans to shareholders, CEOs, or board members, the company being involved in a litigation case, and the company buying its own shares in violation of the Company Act.

<sup>21</sup> Following Hope, Thomas, and Vyas (2013), we estimate discretionary accruals for each industry-year with a minimum of 20 observations. In an untabulated sensitivity analysis we follow Dechow, Hutton, Kim, and Sloan (2012) and estimate discretionary accruals using all the observations in a pooled regression without affecting any inferences.

to issue GC-opinions does not always reflect higher audit quality and Myers et al. (2014) find that non-Big N firms became more conservative while Big-N firms became more accurate in their going-concern reporting after the introduction of SOX. Thus, *GC* and *NumMod* may be more ambiguous measures of audit quality compared to our other measures.<sup>22</sup> Specifically, if Big-4 firms are more accurate than non-Big-4 firms and non-Big-4 firms are more conservative than Big-4 firms, we may observe a *decline* in the use of modified opinions after partners switch to Big-4 firms. However, again we caution about the measurement errors inherent in the commonly employed audit-quality proxies.

### 3.3 Test Methodology

All tests are conducted at the *auditee level* and we require that the switching partners have the *same clients* before and after the switch. We use the following regression, clustering standard errors at the client level, to test for a Big-4 effect (hypotheses 1a and 1b):<sup>23</sup>

$$(1) \quad AQ_{ijt} = \beta_0 + \beta_1 \text{SwitchYear}_{ijt} + \beta_2 \text{FirstYear}_{ijt} + \beta_3 \text{AfterFirstYear}_{ijt} + \Sigma \text{Controls}_{ijt} + FE_{yr} \\ + FE_{ind} + FE_{auditor} + \varepsilon_{ijt}$$

*AQ* is audit quality (*GCAccuracy*, *GC*, *NumMod*, or *EarningsQuality*). We use the same specification when the natural logarithm of audit fees (*LnAF*) is the dependent variable. Our hypotheses predict increases in audit quality and audit fees following the switches.

*SwitchYear*, *FirstYear*, and *AfterFirstYear* are the variables of interest. *SwitchYear* equals 1 for all clients *i* of auditor *j* that have switched affiliation in the switching year (i.e.,

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<sup>22</sup> The usual interpretation of higher likelihood of going-concern modified opinion is higher auditor independence and thus higher audit quality. However, it may also indicate excessive auditor conservatism and thus reduced audit quality (DeFond and Zhang 2014).

<sup>23</sup> Inferences are unaffected if we cluster at the audit-partner level or if we do not cluster standard errors (untabulated). On balance we believe that clustering on client firms makes most sense. The key issue in our setting is that clients stay the same whereas partners change as they join a Big-4 firm. Thus, we have a clear break in terms of partner before and after the switch in terms of learning, incentives, monitoring, compensation, prestige, etc. For the client firm the impact overall is much lower.

$t=0$ ), and 0 otherwise. *FirstYear* equals 1 for all clients  $i$  of auditor  $j$  in the year after the switching year (i.e., for  $t = 1$ ), and 0 otherwise. *AfterFirstYear* equals 1 for all clients  $i$  of auditor  $j$  in the subsequent years after the first year (i.e., for  $t = 2, 3, \dots T$ ), and 0 otherwise. Thus, *AfterFirstYear* captures the “long-term” effect while *SwitchYear* and *FirstYear* capture the short-term effects.

Hypotheses 1a (increased audit quality) and 1b (higher fees) are supported if  $\beta_3 > 0$ ; the effect should materialize in the long run. As the length of the transition period necessary to observe changes is uncertain, we have no predictions regarding  $\beta_1$  and  $\beta_2$  (but assume that the long-run effect will be captured by *AfterFirstYear*).

We include year ( $FE_{yr}$ ), industry ( $FE_{ind}$ ), and most importantly, auditor ( $FE_{auditor}$ ) fixed effects in the regressions.<sup>24</sup> Note that the auditor fixed effects control for the time-invariant innate ability of the individual partners. We further include an extensive set of time-varying control variables (*Controls*) motivated by prior research. As we compare the *same pairs of* auditors and auditees before and after a switch in affiliation, the control variables are associated with the size, risk, and complexity of the auditees that may have changed in the years surrounding the switch. We control for firm size and firm age - the natural logarithm of total assets ( $LnTA$ ), the number of employees ( $LnEmployees$ ), and the age of the client firm ( $LnAge$ ). We measure business complexity using the number of industries the clients operate in ( $NumberIndustries$ ), inventory and account receivables scaled by total assets ( $InvAccRec$ ), and intangible assets divided by total assets ( $Intangibles$ ). We measure financial risk using the debt ratio ( $Leverage$ ), changes in the debt ratio ( $ChgLeverage$ ), and probability of going bankrupt ( $ProbBankruptcy$ ). Operating risk is measured using return on assets ( $ROA$ ), sales growth

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<sup>24</sup> While the year fixed effects capture the effect of each specific year, *SwitchYear* captures the effect of switching related to the switching year, which varies among observations. The same logic goes for *FirstYear* and *AfterFirstYear*. Hence, there are no perfect correlations among these variables. Furthermore, the year fixed effects control for economy-wide effects such as inflation.

(*SalesGrowth*), cash flow (*CashFlow*), and an indicator variable for loss (*Loss*). We proxy for liquidity risk with short-term investments scaled by total assets (*ShortTermInv*) and the ratio of current assets to current liabilities (*CurrentRatio*). To test H2a, H2b, and H2c, we use modified versions of equation (1).

### 3.4 Descriptive Statistics

The first column in Table 1, Panel A provides the number of partners switching to Big-4 firms in each year. While we have identified 77 partners who have changed their firm affiliation from non-Big 4 firms to Big-4 firms, only the 68 switching partners who satisfy the requirements of our research design are included in the sample. The next two columns report the number of clients audited by the switching partners before and after switching affiliation in each year; 14,932 observations before switching, and 16,486 observations after switching.<sup>25</sup> The last column indicates that there are 31,418 observations in total.<sup>26</sup>

Table 1, Panel B presents descriptive statistics for the sample before and after the partners switch from non-Big-4 firms to Big-4 firms. Even though the auditors audit the same clients before and after the switch, some t-tests reject the hypothesis of equality of means, which provides additional support for the inclusion of the time-varying control variables.<sup>27</sup>

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<sup>25</sup> The number of pre-switch observations generally decreases over time. For example, the pre-switch observations in 2010 only include the firm-year observations of switches that took place in 2011 and 2012 (not 2009 and before 2009), while the pre-switch observations in 2006 consist of those of switches that occurred in 2007, 2008, 2009, 2010, 2011, and 2012. Hence, the earlier years will likely have a greater number of pre-switch observations than later years. Similarly, the number of post-switch observations is increasing over time.

<sup>26</sup> We have manually checked which partner and audit firm each of the switching partners' clients has switched to post the switch. Untabulated results show that among the identified 7,449 client firms of the switching partners, 40 clients switched to other Big-4 firms, 375 client firms switched to non-Big 4 firms, and 7,034, which is 94% of the clients, followed the switching partners to the Big-4 firms. Among them, 507 client firms are audited by other audit partners of the Big-4 firms that the non-Big 4 partners have switched to, while 6,527 (87.6%=6,527/7,449) client firms are audited by the same partners after the switch.

<sup>27</sup> After the auditor switches to a Big-4 firm, the clients are larger (*LnTA and TA*), default on debt payment (*DefaultDebtPay*) more often, have lower sales growth (*SalesGrowth*), and have more short-term investments



## 4 Main Results

### 4.1 The Big-4 Effect

Table 2 presents the results from testing changes in audit quality and audit fees before and after a switch in audit-firm affiliation. As explained above, the partners switching affiliation have exactly the same auditees before and after the switch.<sup>28</sup>

We find support for a Big-4 effect on audit quality (H1a). The audit reports become significantly more precise in predicting financial distress (*GCAccuracy* in column 1), the effect occurs immediately after the switch in affiliation (*SwitchYear* and *FirstYear* are significantly positive) and it seems to be long term (as *AfterFirstYear* also is significantly positive). In column 4, we observe significantly higher *EarningsQuality* in the first year after the switch and in the long run (*FirstYear* and *AfterFirstYear* are significantly positive), but no immediate effect (*SwitchYear* is not significant).<sup>29</sup>

The results for *GC* (column 2), the likelihood of issuing GC opinions, and *NumMod* (column 3), the number of modifications included in the audit report, show lower likelihood of issuing modified opinions after the auditors switch to Big-4 firms (the coefficients on the test

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(*ShortTermInvest*). As the clients' sizes increase, it is reasonable that the auditees pay higher audit fees (*AuditFee*). It is also reasonable that the sample firms are older (*LnAge*) following the switch.

<sup>28</sup> In columns 1 and 2, we lose observations because the auditor fixed effects correlate perfectly with the dependent variable. In column 5, we lose some observations because we do not have audit fees for all sample firms. We obtain similar findings when we keep N constant across the five columns.

<sup>29</sup> To assess whether the switching partners have different opinions on income-increasing and income-decreasing discretionary accruals, we define *DAPosi* (*DANega*) as the raw positive (negative) discretionary accruals. Thus, *DAPosi* (*DANega*) captures the magnitude of the income-increasing (income-decreasing) abnormal accruals. We then re-conduct our main test using *DAPosi* and *DANega* as the dependent variable, respectively. As *DAPosi* (*DANega*) is left-truncated (right-truncated) at zero by construction, we use truncated regressions. The sub-sample with positive (negative) discretionary accruals consists of firms with income-increasing (-decreasing) accruals, and negative (positive) coefficients on the test variables indicate higher audit quality after the switch. Untabulated results show that the Big-4 effect is far stronger in the sub-sample of firms with income-increasing accruals (*DAPosi*). The negative and significant coefficients for *FirstYear* and *AfterFirstYear* imply that Big-4 firms significantly curb the auditees' income-increasing accruals. For firms with income-decreasing accruals (*DANega*), we observe a weak positive effect in the long run.

variables are mostly significantly negative).<sup>30,31</sup> The observed effect of lower likelihood of GC-opinion may explain *why* Big-4 firms are more accurate in predicting financial distress. Specifically, Big-4 firms have resources that result in GC opinions being more accurate in predicting future bankruptcies (Geiger and Rama 2006; Boone et al. 2010; Myers et al. 2014) or financially-troubled firms that are liquidated (Gaeremynck and Willekens 2003).<sup>32</sup> Better resources and more experts in Big-4 firms may also explain why the auditor issues fewer modifications after becoming part of a Big-4 firm (the reduction in *NumMod* after the switch in column 3). The Big-4 firms may make the auditor better able to hinder earnings management and otherwise provide the auditor with the tools necessary for persuading the auditees to follow the regulations or advice given, which reduce the need to modify the audit report. Thus, results showing increased accuracy in GC reports and reduced earnings management are consistent with lower likelihood of GC modifications or other types of modified opinions.<sup>33</sup>

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<sup>30</sup> We have followed Francis and Yu (2009) and rerun the regressions for *GC*Accuracy and *GC* using financially distressed firms. The results are similar and are not tabulated for brevity.

<sup>31</sup> The area under the Receiver Operating Characteristic curve for the logistic regression is 0.8136 when *GC*Accuracy is the dependent variable, and is 0.9292 when *GC* is the dependent variables. These statistics suggest that our models have excellent or outstanding discrimination ability.

<sup>32</sup> We measure the accuracy of GC-opinions by aggregating two types of errors: Issuing GC-opinions to firms that do not default on debt in the future (Type-1 errors), and not issuing GC-opinions to firms that later default on debt (Type-2 errors). There are costs involved with both these errors (Carson et al. 2013). Usually Type-2 errors are regarded as more costly since auditors are typically not held liable for too conservative reports, but rather for those that were insufficiently conservative. To gain insight into how the improvements (i.e., decline in errors) in *GC*Accuracy occur, we separately examine Type-1 and Type-2 errors. We redefine the *GC*Accuracy such that it reflects either Type-1 errors or Type-2 errors, and rerun the tests. Untabulated results show that the increased accuracy is due to a significant reduction in the more costly Type-2 errors sample. For Type-1 errors, the coefficients for the test variables are positive, but not significantly positive at conventional levels (i.e., the coefficient for *AfterFirstYear* is significantly positive with a p-value 0.14 using a two-sided test).

<sup>33</sup> All Big-4 firms, but none of the non-Big-4 firms, are subject to PCAOB inspections. Thus, one contributing explanation for our results could be that the Big-4 firms are subject to PCAOB-inspections. Although we consider this effect, if any, part of the Big-4 effect, we test if inferences hold after explicitly controlling for PCAOB inspections. To be precise, we utilize the fact that the PCAOB inspects more audit engagements when an audit firm has more SEC registrants as clients. We construct two variables that capture differences in the Big-4 firms' exposure to PCAOB inspections. The first scales the number of U.S. audits by audit firm *k* by all clients of audit firm *k*. The second measures the Big-4 firms' market share of U.S.-listed firms (computed as the number of U.S. audits by audit firm *k* as a percentage of all U.S. audits performed by all the Big-4 firms in Norway). We then include these controls in the empirical analyses. We find that none of our test variables are significantly affected by the inclusion of these control variables (untabulated), and thus no inferences are affected.

The last column of Table 2 shows that audit fees increase in the first year and after first year, but not in the switching year (H1b). The estimated coefficient of 0.061 on *AfterFirstYear* indicates that the audit fees increase about six percent ( $(e^{0.061}-1) = 0.063$ ) in the long run after the auditor switches from a non-Big-4 firm to a Big-4 firm.<sup>34</sup> This finding adds credence to the notion that audit quality increases; thus, we conclude that H1a and H1b are supported.<sup>35</sup> It is also reassuring that the signs of the control variables are mostly as expected, and that the models seem to fit reasonably well. Overall, the results in Table 2 provide evidence of a Big-4 effect in terms of more accurate GC reporting, higher earnings quality, and higher audit fees.<sup>36</sup>

Analogously to our primary focus on partners who move up to Big-4 firms, we expect that audit quality and fees should decrease for partners who switch from Big-4 firms to non-Big-4 firms. We therefore repeat our tests for partners who move *from Big-4 firms to non-Big-4 firms*. We identify a total of 43 partners who move down to non-Big-4 audit firms. However, auditees usually remain with the Big-4 firm when their engagement partner starts working for a non-Big-4 firm. For that reason, the sample used for these tests is much smaller, less than 4% of the sample size for the switches to Big-4 firms, and thus the issue of low power arises. Consequently, we do not tabulate these results. However, for the two measures that have less ambiguous interpretations (*GCAccuracy* and *LnAF*), the results support our hypotheses: we find reduced precision in going-concern reporting and lower audit fees in the long run. Thus, when the Big-4 resources are no longer present, audit quantity and audit fees decrease.

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<sup>34</sup> Untabulated analyses show that conclusions are unchanged if we use CPI – adjusted audit-fee data.

<sup>35</sup> As many of our observations are from 2008 and 2011, we rerun the analyses using only these years. Conclusions are unchanged. We also rerun the analyses after excluding 2012 observations (as there are no observations in the pre-switch period). No inferences are affected.

<sup>36</sup> While we believe that our approach of testing for a gradual (“phase-in”) effect makes intuitive sense and is in line with what we hear from discussions with practicing auditors, in untabulated sensitivity analyses we only use one test variable, *PostSwitch*. We obtain consistent inferences.

## 4.2 Sources of the Big-4 Effect

We first provide results of whether Big-4 firms are able to *recruit high-quality input* (i.e., the best partners). This has been considered as one of the potential reasons for the Big-4 effect in prior literature but has not been shown empirically.

If the partners shifting to Big-4 firms are of higher quality, compared to partners who remain in non-Big-4 firms, they should deliver audits of higher quality *before* joining Big-4 firms. The expected higher audit quality prior to the switch to Big-4 firms should also correspond with higher audit fees. To examine whether Big-4 firms are able to attract higher-quality inputs, we construct a sample consisting of (1) the auditees of all partners in non-Big-4 firms who have not switched between a Big-4 firm and a non-Big 4 firm and (2) all the auditees of the partners switching to Big-4 firms in the years prior to the switch. We define  $ToBig4Pre = 1$  for the auditees of the partners switching to Big-4 firms in the years prior to the switch, and 0 otherwise. In this sample, we replace the test variables in equation 1 with  $ToBig4Pre$  and run model (1). Table 3 presents the results.  $ToBig4Pre$  is significantly positive for  $GC$ Accuracy,  $EarningsQuality$ , and  $LnAF$ , and significantly negative for  $GC$ . Thus, auditors who subsequently move up to Big-4 firms issue fewer, but more accurate going-concern opinions, earnings quality is higher prior to the switch, and they charge higher audit fees. The results indicate that Big-4 firms are able to attract higher-quality inputs, thus supporting H2a.

We next provide results related to whether and to what extent *learning* in Big-4 firms contribute to the Big-4 effect (H2b). To examine whether the increased audit quality after audit partners switch to Big-4 firms is (partially) due to *learning*, we first analyze the number of hours the auditors spend on CPE using detailed data obtained from FSAN. FSAN has conducted surveys where all auditors are mandated to report the number of CPE hours they undertake on courses related to auditing, ethics, and others in the previous three years. If audit

partners have more CPE hours after the switch, this provides some support that they have been undergoing more training and learning in Big-4 firms compared to their experience in the non-Big 4 firms. We tabulate the hours in Panel A of Table 4. We find that CPE hours on all courses, auditing, and ethics are statistically significantly higher after the switch compared with before the switch. The significantly higher level of CPE hours in the post-switch period indicates that there is increased *learning*, which is one of the sources of increased audit quality.

In Panel B of Table 4, we test whether switching partners who take positions in one of the largest Big-4 offices experience greater increases in audit quality. The idea is that a larger office offers greater opportunities for learning from peers and specialists. Furthermore, as we know that switching partners do not move when they change audit-firm affiliation (see Section 5.7), the ease with which the incoming partners may work at a large office and learn from colleagues varies. To extract exogenous variation in the learning effect, we measure the distance from the incoming partner's home to the Big-4 office in one of the four largest cities in Norway using postal codes, where the distance measures driving time in minutes using Google Maps to account for variations in road quality, speed limits, and the use of ferries. We add  $LargeOffice = \text{Ln}(\text{driving time by car}) \times (-1)$  to the main test, and also add the interaction terms between  $LargeOffice$  and  $SwitchYear$ ,  $FirstYear$ , and  $AfterFirstYear$  in the equation (1). For brevity, we only report the results for these three interaction terms. We find stronger effects for partners switching to a larger Big-4 office, which provides further support for a learning effect.

Next, we compare audit quality of new Big-4 partners with existing Big-4 partners in both the short and long run following the switch.<sup>37</sup> If the increased audit quality after the switch is (partially) driven by enhanced learning and it takes time for partners to learn, it is likely that

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<sup>37</sup> For this analysis, we employ a difference-in-differences methodology with propensity-score matching (and include all control variables plus fixed effects in the regression). For brevity, we only tabulate the relevant test variables.

partners who switch to Big-4 firms provide, compared to existing Big-4 partners, lower audit quality right after the switch and similar audit quality a few years after the switch. To examine the difference in audit quality of the incoming partners after their switch and existing Big-4 partners, we first match the clients of the incoming partners (treatment sample) with clients of the existing Big-4 partners (control sample) based on PSM.<sup>38</sup> *Treat* equals one (zero) for the treatment (control) sample. We then define a variable for long-term effects, *LT*, which is one if the client-year observation is after the first year and zero if the client-year observation is in the switch year or the first year. Note that we only include the client-year observations of incoming partners in the post-switch period. We regress measures of audit quality and audit fees on *Treat*, *LT*, and the interaction term  $TreatLT = Treat \times LT$  including all the control variables and industry and year fixed effects. The coefficient on *Treat* and *TreatLT* are our focus as they capture differences in audit quality/fees between the treatment and control group in the short and long term, respectively. The results in Panel C of Table 4 show that the coefficients on *Treat* are negative and significant, suggesting that the audit quality of switching partners is lower than that of existing Big-4 firms in the short term. In the long term, however, the audit quality of switching partners is similar to or higher than that of existing Big-4 partners. These findings are consistent with learning taking place after the partner joins the Big-4 firm,

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<sup>38</sup> For conducting the PSM analysis, we use all the control variables in the main equation and employ the nearest neighbor approach with a caliper width of 0.2 without replacement based on the propensity score calculated via logit model. We have examined three balance metrics for the matching covariates following DeFond, Erkens, and Zhang (2017): (1) “t-value” for the differences in the means of the treatment and control groups, (2) “*%bias*”, the absolute standardized percentage bias, which is the mean difference between the treatment and control groups divided by the square root of the average treatment and control sample variances, and (3)  $L_1$ , which is the third covariate balance metric based on Coarsened Exact Matching (CEM) approach following Iacus, King, and Porro (2011). The value of  $L_1$  is between 0 and 1, where a value of 0 means that the two empirical distributions of the treatment and control samples exactly coincide and a value of one indicates that the distributions are completely separated (Iacus et al. 2011). All the three imbalance metrics indicate a good match between the treatment and control groups, suggesting that the PSM works well.

and that learning takes time as the effect of learning is stronger in the long run than in the short run.<sup>39</sup>

Audit partners' income and wealth may be associated with their incentives. Partners who switch to Big-4 firms may earn higher income after the switch and thus have stronger incentives to perform well in order to maintain their high level of salary and to avoid audit failures that may result in sanctions. One of the advantages of using our experimental setting (Norway) is the possibility of obtaining data on both compensation and wealth, on which we present the statistics in Panel A of Table 5. We find that both income and wealth are significantly higher after the switch than before the switch, which suggest that the partners' *incentives* to deliver higher quality are likely to be stronger.

To further examine whether the incoming partners have stronger incentives to deliver higher audit quality after the switch, we investigate audit modifications due to delayed filing of the annual report. The General Assembly has to approve the firms' annual statements within six months after the fiscal year end. Within one month of the General Assembly's approval, the firm has to file their financial statements with a governmental register (the Brønnøysund Register Centre). It is easy for the auditor to observe whether the client files its annual report too late. Thus, learning is not an issue for this modification. However, monitoring, incentives, and independence are. We expect that after switching to a Big-4 firm, we should observe an immediate improvement in the partners' ability to ensure that their clients file on time because of better monitoring, stronger incentives, and/or increased independence. We examine whether there is less late filing after the switch by estimating equation (1) where the dependent variable is 1 if there is delayed filing and 0 otherwise. Panel B of Table 5 reports that there is an

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<sup>39</sup> Note that the coefficients on *TreatLT* indicate higher audit quality (for *GC* and *EarningsQuality*) and fees in the long term for the incoming auditees. Possible explanations could be that our sample period is too short to capture the equilibrium long-term effect or that that incoming partners perform better than existing Big-4 partners due to more recent training, stronger incentives, or closer monitoring because they are new.

immediate decrease in late filing, which indicates stronger incentives and/or better minoring of partners in Big-4 firms.

In addition to the above descriptive statistics and tests, it may also be possible to glean some insights into the relative effects of learning versus monitoring by further considering the results in Table 2. That is, it is intuitive to expect that better monitoring and stronger incentives can take effect immediately and lead to an increase in audit quality. The ability to withstand client pressure may also increase immediately because the partner is less dependent upon keeping the client as Big-4 firms have a much larger portfolio of clients. The effect of learning, for instance to start applying a new audit methodology and/or networking and consulting with a larger pool of in-house experts, will likely occur more gradually. We find evidence consistent with both immediate monitoring/incentive effects and gradual learning effects in Table 2. *GCAccuracy* and *NumMod* may be viewed primarily as proxies for monitoring and incentives and both increase significantly already in the switching year. In contrast, we see that *EarningsQuality*, which most likely reflects learning, is significantly higher in *FirstYear* and *AfterFirstYear* than in the switching year (p-values < 0.01), consistent with gradual learning.

Overall, we find evidence suggesting that partners who switch from non-Big-4 firms to Big-4 firms are of higher quality and benefit from the Big-4 firms' rich resources in terms of both enhanced learning and stronger incentives (monitoring). To our knowledge these are new findings in the literature and help us understand the underlying reasons for why we observe a Big-4 effect.<sup>40</sup>

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<sup>40</sup> It is not feasible to completely and directly "horse-race" learning versus incentives; rather we are able to find evidence consistent with both explanations, which we believe is intuitive and importantly provides strong contributions to the literature. For example, Aobdia, Lin, and Petacchi (2015) implicitly assume that audit partners are endowed with innate abilities that drive the quality of their work. We show that Big-4 firm are able to develop the use of these abilities through increased learning and incentives/monitoring.



## **5. Additional Analyses Related to Potential Endogeneity**

As explained, our research design holds constant the partner-auditee pair, thus providing controls for some of the major challenges in examining potential Big-4 effects. The design also mitigates important self-selection and endogeneity concerns. However, our design is not immune against endogeneity and in this section we provide several additional analyses. We believe that the consistency of inferences across all the different tests suggests that endogeneity and self-selection are not likely a significant threat to our findings.

### **5.1 Difference-in-Differences Analyses with Propensity-Score Matching (PSM)**

As an alternative to our primary empirical strategy, we present results based on a difference-in-differences design. Panel A of Table 6 reports the results for the difference in audit quality between auditees of switching partners and auditees of non-switching non-Big 4 partners. *Treat* equals one if the partner-client pairs switch to Big-4 firms and zero if the partner-client pairs remain with non-Big 4 firms. The treatment and control samples are matched on all control variables based on PSM. We include all controls and year and industry fixed effects and cluster standard errors on client firms. For brevity, we report only the interaction terms between *Treat* and *SwitchYear*, *FirstYear*, and *AfterFirstYear*. The results based on the difference-in-differences analyses support those reported in Table 2.

### **5.2 Partner Switches between Non-Big-4 Audit Firms (Placebo Analyses)**

In the previous section, we attribute changes in audit quality and audit fees to changes in the partners' affiliation between non-Big-4 and Big-4 firms. It could be that changes in audit-firm affiliation per se may cause changes. In order to test if the observed effect is a placebo effect due to switches in affiliation, we rerun the tests on a sample that consists of 79 partners

who switch affiliation from one non-Big-4 firm to another non-Big-4 firm.<sup>41</sup> The results for lateral switches among non-Big-4 firms are presented in Table 6, Panel B (we only report the test variables for brevity). For the four audit-quality measures, 11 of the 12 test variables are insignificant, which is reassuring. When audit fees are the dependent variable, we observe lower audit fees after the switch. Lower fees could be due to for instance low balling or economies of scale where the cost reduction is at least partially passed on to the client.

### **5.3 Artificially Picking Switch Year (Placebo Analyses)**

Next, to address the possibility that *unobservable* factors other than actual switches could drive our results, we conduct a placebo test using a “pseudo-event” year. Specifically, we define the pseudo-switch year as three years prior to the actual switch and only retain observations before the actual switch occurs. We do not find any significant changes in audit quality around these “pseudo-event” years (untabulated), which indicates that there is no general upward trend in audit quality among the switching firms.

### **5.4 Client-Selection Bias Based on Client Risk in the Pre-Switch Period**

Measures of audit quality using financial statement information such as discretionary accruals are subject to endogeneity as the measures are the joint product of audit quality and clients’ innate characteristics (DeFond and Zhang 2014). Because we analyze the same pair of partners and clients before and after the switch, our test design is robust to such effects. However, one might argue that there is a potential self-selection effect due to Big-4 firms favoring low-risk clients (Bedard et al. 2008). Empirical evidence from the U.S. suggests that

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<sup>41</sup> Ideally, we would also like to conduct a similar test for lateral switches among Big-4 firms, but this is not possible because we observe very few lateral changes among partners in Big-4 firms. Furthermore, when they switch, they rarely switch together with their clients and thus it is impossible to carry out statistical tests.

Big-4 auditors tend to have less risky clients (Raghunandan and Rama 1999; Johnstone 2000; Johnstone and Bedard 2004).

To gain insight into whether the auditees that follow the switching partner are more or less risky than other auditees, we perform t-tests for equality of means for the control variables used in equation 1 using a sample consisting of the auditees that follow the switching partner in the pre-switch period (i.e., the auditees that are included in Table 2) and the auditees of non-Big-4 partners not switching audit-firm affiliation. The (untabulated) t-tests show that the auditees that follow the partners switching to Big-4 firms are not different from auditees of non-switching non-Big-4 partners in terms of *ROA*, *CashFlow*, and *Loss*, but that they are more risky as measured by *Leverage*, *CurrentRatio*, and *ProBankrupt*. This suggests that our results are not likely to be driven by Big-4 firms' concerns for risk.

We further compare audit quality of client firms that follow and do not follow the switching partners to the Big-4 firms. *Follow* equals one if the clients have followed the switching partners to the Big 4 firms and zero for those that did not switch to Big 4 firms. The results for the interaction terms between the three test variables (*SwitchYear*, *FirstYear*, and *AfterFirstYear*) and *Follow* show that clients following the switching partners have higher audit quality in the post-switch period (untabulated), providing supportive evidence of our inference.<sup>42</sup>

### **5.5 Anticipation of Future Changes in Auditees' Prospects**

One alternative explanation for the findings observed in Table 2 is that clients are more likely to follow the audit partner to the Big-4 firm if they expect an improvement in their

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<sup>42</sup> To control for unknown firm characteristics that are not captured by other features of our research design, we implement firm fixed-effects tests. In our setting, this analysis is limited by the fact that for many client firms, all the observations of the same client have the same values for our outcome variables. We find that for four out of the five measures (with *EarningsQuality* being the exception), our inferences are unaffected (untabulated).

financial health. Another alternative explanation is that the Big-4 firm is more willing to accept the incoming partner's clients if the Big-4 firm believes there will be an improvement in their financial health. To rule out these alternative explanations for the observed decrease in GC opinions, we provide falsification tests using proxies for the company's financial health as the dependent variables.<sup>43</sup> Specifically, we use return on assets, leverage, sales growth, and loss as dependent variables. If *FirstYear* and *AfterFirstYear* load positively in the financial-health models, it would support the alternative story that clients are more likely to follow the partner from the non-Big-4 firm to the Big-4 firm if their financial health is expected to improve. The results are reported in Table 6, Panel C (only test-variables are shown). We find that neither *FirstYear* nor *AfterFirstYear* are statistically significant for any of the four financial-health outcomes.

Related to expectations of future financial performance and the possibility of expanding internationally, we also control for the percentage of foreign operations. No conclusions are affected (untabulated). Finally, we also investigate if some of the firms change company structure from private to public after switching to Big-4 auditors and thus prepare themselves for more public exposure and growth. We find that all sample firms remain private throughout the sample period. In sum, these findings provide further support for the notion that our Table

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<sup>43</sup> As it is impossible to observe counterfactual scenarios in the event of the switches not taking place, one cannot rule out the possibility that the increase in audit quality and fees could have occurred also if the switches had not taken place. Because we analyze a large number of staggered switches that take place during a period of eight years, we view it as unlikely that such effects contaminate our results. Note that if the auditees had the potential to for instance prepare high-quality earnings but did not do so while being auditees of non-Big-4 firms, we view the increase in earnings quality after the switch as part of the Big-4 effect as they needed the affiliation with the Big-4 firm to realize their potential.

2 results are more likely related to an audit-quality effect rather than unobserved changes in clients' prospects.<sup>44</sup>

## **5.6 Analyses of Switches Emanating from Audit-Firm Mergers**

Partners who switch audit-firm affiliation due to mergers are less susceptible to endogeneity concerns because the most important decision is taken by the merging audit firms. Note that the majority of the auditees follow the switching partner. The following rate in our sample (94%) is comparable to the following rate in Jiang et al. (2019) and the high following rate suggests that our results are less likely to be affected by endogeneity related to the auditees' decision to follow or not or portfolio management at the audit-firm level. Untabulated results show that our inferences are unaffected when we repeat our tests on partner-auditee pairs that switch affiliation due to mergers between non-Big-4 firms and Big-4 firms.

## **5.7 Personal Characteristics of Partners Switching to and from Big-4 Firms.**

To assess the effect of potentially omitted correlated variables that relate to the partners for the Big-4 effect documented in Table 2, we compare partners who switch to and from Big-4 firms. We make use of the rich data availability in Norway and obtain detailed data on each partner's gender, age, years of professional experience (measured as the number of years since the auditor first obtained her license as an auditor), and education (whether the auditor holds a bachelor's or a master's degree in accounting and auditing). We first test for significant

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<sup>44</sup> As an alternative to test if the clients that follow the switching partner experience an improvement in financial health, we rerun the financial-health model for those that do *not* follow the switching partner to the Big-4 firm. These firms might not follow because they do not expect an improvement in financial health or because they are not accepted by the Big-4 firm due to the Big-4 firm expecting them to face financial problems in the future. Untabulated results show that none of the test variables are significant at the 0.05 level. At the 0.1 level, only two out of 12 test variables are significant and they provide mixed signals as to future development. These results suggest that future financial development has no impact on the auditees' decision to follow or not follow the switching partner or the Big-4 firm not accepting firms that they expect will face financial problems in the future.

differences between those shifting to/from Big-4 firms, and second we add these variables as additional control variables in our regressions.

We observe that the partner characteristics in the two samples are almost identical. Specifically, the partners switching from non-Big-4 to Big-4 firms are not significantly different in terms of age, year of experience, gender, and education. The mean age is 45.6 (45.6) years and the mean years of experience is 15.4 (16.2) for the partners switching to Big-4 firms (non-Big-4 firms). The proportion of partners with a master's degree in accounting and auditing is 80 and 75 percent, respectively. Of those switching to Big-4 (non-Big-4) firms, 14.1 (12.5) percent are females. Next, we add age, gender, year of experience, and education as additional controls in the regression analyses. The inferences reported above are unchanged (results not tabulated). Finally, using the switching partners' private addresses, we find that none of the switching partners has moved. These results reduce the possibility that the switches are initiated by the partners' decision to relocate.

## **6. Conclusion**

This study applies a new and novel research design, unique data, and a setting of private-client firms to examine the Big-4 effect and the sources of improvement in audit quality. The research design focuses on audit partners who switch affiliation from non-Big-4 firms to Big-4 firms while holding the *pair of auditor-auditees constant*, which alleviates many important concerns of self-selection and correlated omitted variables.

We find evidence that audit quality increases when pairs of auditor-auditees switch affiliation from non-Big-4 firms to Big-4 firms (and that audit fees also increase). There is limited prior evidence on the *sources* of the Big-4 effect. We first show that Big-4 firms are able to *attract higher-quality inputs*. That is, we find that the partners who move up to Big-4 firms provide higher-quality audits in the pre-switch period than do non-switching non-Big-4

audit partners. Next, using novel data we provide evidence suggesting that both *learning* and *incentives (monitoring)* contribute to the quality improvement we observe. These are new findings in the literature.

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## **Appendix A: The Norwegian Audit Market**

The accounting and auditing regulation in Norway is comparable to regulations found elsewhere in Europe. This is due to the European Economic Area (EAA) agreement between EFTA (European Free Trade Association) and the EU (European Union). Norway has signed the EAA agreement, and as part of the agreement, Norway adopts most EU legislation concerning the single market (except for laws regarding agriculture and fisheries).

All Norwegian limited liability firms are required to send full sets of financial statements to the Brønnøysund Registration Center (BRC) (small firms are not required to produce cash-flow statements). Listed firms use IFRS as adopted by the EU, while other firms may choose IFRS or the measurement and disclosure requirements found in the Norwegian Accounting Act (henceforth NGAAP, Norwegian Generally Accepted Accounting Principles). NGAAP is less demanding than IFRS. Among private firms, IFRS is mainly used by firms with publicly traded bonds. In 2011, more than 214,000 listed and non-listed limited liability firms filed their annual reports with BRC. Fewer than 1,000 firms used IFRS. The tax regulation is independent of the accounting regulation and Norway is considered as a low book-tax alignment country (Nobes and Schwencke 2006).

The auditing standards in Norway are based on the International Standards of Auditing (ISA), with a few national adjustments due to special requirements in the company legislation. Until May 1, 2011, *all* limited liability firms independent of size were required to have their financial statements audited. After 2011, most firms with revenue less than NOK 6 million, assets less than NOK 23 million, or with fewer than ten employees are allowed to provide non-audited financial statements. By the end of 2012, 90,568 firms had opted out of auditing. The total number of limited liability firms that filed financial statements by BRC in 2012 was

229,433.<sup>45</sup> Looking at the total market for auditing services, the Big-4 firms' market share is 29.3% in terms of number of clients and 51.6% in terms of audit fees (FSAN 2012).

Auditors may obtain licenses as registered or state-authorized public accountants. A master's (bachelor's) degree, and two years of practice, is required to become a state-authorized (registered) public accountant. In order to be responsible for audits of listed firms and banks and insurance companies, the auditor needs to have a license as a state-authorized public accountant. The engagement partners (but not other licensed auditors) must take at least 105 hours of continued professional education (CPE) over a three-year period in order to remain licensed as an engagement partner. Failure to satisfy the minimum CPE requirements will lead to the license being revoked. It is the Financial Supervisory Authority of Norway (FSAN, in Norwegian: Finanstilsynet), that oversees auditors and the auditing market. In 2011, there were 6,482 licensed auditors and 745 licensed audit firms (FSAN 2012).

Audit firms are subject to periodic reviews (details are provided in Section 2.2). The litigation and reputation risk of auditors is relatively low for private firms in Norway. A detailed discussion is provided by Hope and Langli (2010), who examine all court cases and other legal proceedings against auditors over a 60-year period.

The audit market, at least for non-listed clients, is competitive and transparent. There is a high number of suppliers (more than 700 audit firms in 2010) and fees for audit and non-audit services have been disclosed in the notes to the accounts since 1990. Information about licensed audit firms has been easily available by FSAN. The market is dominated by a large number of small clients. In 2010, the average audit fee was NOK 26,000. The average number of clients per engagement partner was 151 in 2010 and 113 in 2004. A change in the tax law in 2006 incentivized shareholders to transfer their shares to a limited liability firm, and many new

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<sup>45</sup> Changes in the Company Act that made it easier and cheaper to establish a limited liability firm and also to avoid having an auditor, led to a large increase in the number of new firms in 2012, which explains the increase in the number of firms filing annual reports from 2011 to 2012.

limited-liability firms were created. These holding firms are easy to audit, and is part of the explanation why engagement partners have many clients with low fees. After the opt-out rule went into effect in 2011, many small firms have opted out having an external audit and the average audit fees had almost doubled by 2014.

## Appendix B: Variable Definitions

Variable	Variable definition
<i>AfterFirstYear</i>	= 1 for all clients of auditor <i>j</i> in the years after the first year in the post-switch period (i.e., for $t > 1$ ), and 0 otherwise. The switching year is $t=0$ .
<i>AuditFee</i>	= The fee for audit services in NOK 1,000.
<i>Big4</i>	= 1 if a client firm uses a Big-4 audit firm, and 0 otherwise.
<i>CashFlow</i>	= Cash flow scaled by total assets. Cash flow = earnings - total accruals. Earnings = net income after taxes before extraordinary item and taxes on extraordinary items. Total accruals = change in current assets - change in cash - change in short-term debt + change in short-term interest bearing debt + change in dividends + depreciation + amortization - change in net deferred taxes. <sup>46</sup>
<i>ChgLeverage</i>	= Changes in leverage ratio = $Leverage_t - Leverage_{t-1}$ .
<i>CurrentRatio</i>	= Current ratio = current assets / current liabilities.
<i>EarningsQuality</i>	= <i>EarningsQuality</i> is a measure of discretionary accruals using the performance-adjusted Jones model (Kothari et al. 2005). <i>EarningsQuality</i> is the absolute value of the residual from the following regression multiplied by (-1) (subscript <i>i</i> indicates client firms and <i>t</i> indicates time period):  $Accr_{i,t} = \alpha_0 + \alpha_1(1/Assets_{i,t-1}) + \alpha_2\Delta Rev_{i,t} + \alpha_3PPE_{i,t} + \alpha_4ROA_{i,t} + \varepsilon_{i,t}$ <p><i>Accr</i> is total accruals (defined above, see <i>CashFlow</i>) scaled by lagged total assets; <math>\Delta Rev</math> is the annual change in revenues scaled by lagged total assets; <i>PPE</i> is property, plant, and equipment for firm <i>i</i> in year <i>t</i>, scaled by lagged total assets; <i>ROA</i> is the net income for firm <i>i</i> in year <i>t</i> scaled by average total assets.</p>
<i>FirstYear</i>	= 1 for all clients of auditor <i>j</i> in the year after the switching year (i.e., for $t = 1$ ), and 0 otherwise.
<i>GC</i>	= 1 if audit report is modified due to going-concern uncertainty, and 0 otherwise.
<i>GCAccuracy</i>	= 1 if the audit report is correct and 0 otherwise. An audit report is considered correct if (i) the audit report is modified for going-concern uncertainty and the auditee defaults on debt payment within 12 months after the annual account is filed with the Brønnøysund Register Center, or (ii) the audit report is not modified for going-concern uncertainty and the auditee does not default on debt payments within 12 months after the annual account is filed with the Brønnøysund Register Center.
<i>Intangibles</i>	= Intangible assets scaled by total assets.
<i>InvAccRec</i>	= The sum of inventory and accounting receivable scaled by sales.
<i>Leverage</i>	= Leverage ratio = Debt / Total assets.
<i>LnAF</i>	= The natural logarithm of audit fees = $\ln(AuditFee)$ .
<i>LnAge</i>	= The natural logarithm of firm age, age defined as year <i>t</i> less the year of incorporation.
<i>LnEmployees</i>	= The natural logarithm of the number of employees.
<i>LnTA</i>	= The natural logarithm of total assets. Total assets are measured in NOK 1,000.
<i>Loss</i>	= 1 if a client firm has negative net income, and 0 otherwise.
<i>NumberIndustries</i>	= The number of industries the client firm operates in.

<sup>46</sup> *CashFlow*, *ChgLeverage*, *CurrentRatio*, *InvAccRec*, *Intangibles*, *Leverage*, *SalesGrowth*, and *ROA*, are winsorized at the 2% and 98% levels in the regression analyses due to near-zero values in the scaling variables (e.g., Ball and Shivakumar 2005).

<i>NumMod</i>	=	The number of modifications included in the audit report that do not relate to going-concern uncertainty.
<i>DefaultDebtPay</i>	=	1 if a client firm is registered in the Brønnøysund Register Center as having defaulted on debt payments within 12 months after the annual account is filed with the Brønnøysund Register Center, and 0 otherwise.
<i>ProbBankruptcy</i>	=	Probability of bankruptcy, estimated using model 1 in Ohlson (1980).
<i>ROA</i>	=	Return on assets = Net income / average total assets.
<i>Sales</i>	=	Revenues from operations.
<i>SalesGrowth</i>	=	Sales growth = $Sales_t / Sales_{t-1} - 1$ .
<i>ShortTermInv</i>	=	Short term investment scaled by total assets.
<i>SwitchYear</i>	=	1 for all clients of auditor $j$ that have switched audit-firm affiliation in the switching year ( $t=0$ ), and 0 otherwise.
<i>ToBig4Pre</i>	=	1 for auditees of partners switching audit-firm affiliation from non-Big-4 firms to Big-4 firms in the years before the switch takes place (i.e., for $t < 0$ ), and 0 otherwise.

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**Table 1: Number of Audit Partners and Their Auditees Switching Audit-Firm Affiliation and Descriptive Statistics**

Panel A: Number of audit partners and their auditees switching to Big-4 firms by year

Year	#Partners Switching to Big-4 firms		#Observations in the sample		
	Identified	In the sample	Pre-Switch	Post-Switch	Sum
2005	3	3	2,310	20	2,330
2006	3	1	3,374	64	3,438
2007	5	5	4,127	346	4,473
2008	28	25	1,765	3,435	5,200
2009	6	5	1,408	3,414	4,822
2010	5	4	1,453	3,143	4,596
2011	22	20	495	3,095	3,590
2012	5	5	0	2,969	2,969
Total	77	68	14,932	16,486	31,418

Panel B: Descriptive Statistics

	B1: Before the switch to Big-4 firm					B2: After the switch to Big-4 firm					t-value
	Mean	SD	P25	P50	P75	Mean	SD	P25	P50	P75	
<i>AfterFirstYear</i>	0.00	0.00	0.00	0.00	0.00	0.37	0.48	0.00	0.00	1.00	-93.9***
<i>AuditFee</i>	21.08	24.33	8.00	15.00	25.00	25.54	28.92	10.00	18.00	30.00	-14.6***
<i>CashFlow</i>	0.00	0.46	-0.09	0.01	0.16	-0.03	0.52	-0.10	0.00	0.13	4.6***
<i>ChgLeverage</i>	0.05	0.59	-0.08	-0.01	0.06	0.08	0.78	-0.06	0.00	0.05	-4.6***
<i>CurrentRatio</i>	6.77	16.27	0.98	1.56	3.03	8.04	17.91	0.98	1.70	4.00	-6.6***
<i>DefaultDebtPay</i>	0.06	0.24	0.00	0.00	0.00	0.08	0.27	0.00	0.00	0.00	-6.1***
<i>EarningsQuality</i>	-0.22	0.28	-0.27	-0.11	-0.04	-0.19	0.25	-0.23	-0.10	-0.04	-10.2***
<i>FirstYear</i>	0.00	0.00	0.00	0.00	0.00	0.28	0.45	0.00	0.00	1.00	-76.7***
<i>GC</i>	0.09	0.29	0.00	0.00	0.00	0.08	0.26	0.00	0.00	0.00	5.0***
<i>GCAccuracy</i>	0.88	0.32	1.00	1.00	1.00	0.88	0.32	1.00	1.00	1.00	-1.1
<i>Intangibles</i>	0.02	0.05	0.00	0.00	0.01	0.02	0.05	0.00	0.00	0.00	3.8***
<i>InvAccRec</i>	0.19	0.45	0.00	0.08	0.21	0.20	0.51	0.00	0.06	0.20	-1.4
<i>Leverage</i>	0.72	0.63	0.42	0.69	0.87	0.72	0.76	0.33	0.64	0.88	-0.4
<i>LnAF</i>	2.76	0.79	2.20	2.77	3.26	2.95	0.78	2.40	2.94	3.43	-20.5***
<i>LnAge</i>	2.10	0.86	1.39	2.08	2.77	2.26	0.75	1.79	2.20	2.83	-18.3***
<i>LnEmployees</i>	0.91	1.03	0.00	0.61	1.60	0.83	1.05	0.00	0.30	1.49	6.8***
<i>LnTA</i>	8.13	1.61	7.14	8.13	9.15	8.25	1.76	7.23	8.31	9.35	-6.6***
<i>Loss</i>	0.30	0.46	0.00	0.00	1.00	0.36	0.48	0.00	0.00	1.00	-11.4***
<i>NumberIndustries</i>	1.13	0.40	1.00	1.00	1.00	1.14	0.40	1.00	1.00	1.00	-0.10
<i>NumMod</i>	0.22	0.60	0.00	0.00	0.00	0.22	0.59	0.00	0.00	0.00	1.1
<i>ProbBankruptcy</i>	0.43	0.36	0.07	0.36	0.79	0.36	0.35	0.04	0.24	0.65	17.6***
<i>ROA</i>	0.03	0.07	0.00	0.02	0.05	0.02	0.07	0.00	0.01	0.04	11.6***
<i>Sales(mNOK)</i>	10.39	37.42	0.25	2.06	7.83	11.78	55.65	0.09	1.70	7.39	-2.6**
<i>SalesGrowth</i>	0.34	1.71	-0.02	0.00	0.17	0.21	1.43	-0.05	0.00	0.11	7.5***
<i>ShortTermInv</i>	0.24	0.27	0.03	0.14	0.38	0.25	0.28	0.03	0.13	0.41	-3.2***
<i>SwitchYear</i>	0.00	0.00	0.00	0.00	0.00	0.35	0.47	0.00	0.00	1.00	-85.7***
<i>TA(mNOK)</i>	12.57	46.93	1.25	3.38	9.43	17.42	76.29	1.39	4.06	11.50	-6.7***
#Observations	14,932					16,486					

Panel A presents the number of audit partners who switch audit-firm affiliation from non-Big-4 firms to Big-4 firms and the clients of the switching auditors per year. The first two columns provide the number of partners who have switched from non-Big-4 firms to Big-4 firms we have identified and used in the final sample. The next two columns show, in each year, the number of client observations when the auditors audit the same clients before and after the switch in affiliation. The last column (Sum) presents the sum of total client observations per year. Panel B provides statistics of mean, standard deviation (SD), the 25<sup>th</sup>, 50<sup>th</sup>, and 75<sup>th</sup> percentiles of all the variables used

in the main regression analysis. The variables are defined in the Appendix B. Panels B1 (B2) provide descriptive statistics for the years before (after) the auditors switch to Big-4 firms. The last column reports the t-statistics for tests of equality of means before and after the auditors switch affiliation. \* (\*\*) [\*\*\*] indicates significance at the 10 (5) [1] percent level using two-tailed tests.

**Table 2: Regression Results of Changes in Audit Quality and Audit Fees after the Auditor Switches affiliation from non-Big-4 to Big-4 audit firms (H1a and H1b)**

	(1)	(2)	(3)	(4)	(5)
	<i>CGAccuracy</i>	<i>GC</i>	<i>NumMod</i>	<i>EarningsQuality</i>	<i>LnAF</i>
<i>SwitchYear</i>	0.120* (1.80)	-0.025 (-0.28)	-0.047*** (-3.97)	0.005 (1.04)	-0.004 (-0.37)
<i>FirstYear</i>	0.364*** (3.86)	-0.321** (-2.40)	-0.028* (-1.90)	0.020*** (3.03)	0.028** (2.31)
<i>AfterFirstYear</i>	0.414*** (3.25)	-0.751*** (-3.87)	-0.049** (-2.42)	0.024*** (2.76)	0.061*** (3.55)
<i>LnTA</i>	0.074*** (3.17)	-0.152*** (-4.44)	-0.029*** (-8.14)	0.005*** (2.98)	0.146*** (32.79)
<i>LnAge</i>	0.018 (0.49)	-0.165*** (-3.26)	-0.014** (-2.11)	0.039*** (15.89)	0.102*** (15.55)
<i>LnEmployees</i>	-0.302*** (-8.03)	0.092* (1.67)	0.007 (1.31)	0.025*** (10.27)	0.369*** (52.91)
<i>NumberIndustries</i>	-0.112 (-1.50)	0.165 (1.50)	-0.007 (-0.55)	-0.019*** (-4.43)	-0.068*** (-5.12)
<i>ROA</i>	0.997** (2.09)	-0.335 (-0.50)	-0.134 (-1.61)	-0.721*** (-15.83)	-0.312*** (-4.46)
<i>SalesGrowth</i>	-0.042*** (-3.72)	0.055*** (3.87)	0.008*** (3.49)	-0.016*** (-10.52)	-0.008*** (-4.00)
<i>CashFlow</i>	0.052 (1.25)	0.008 (0.15)	0.006 (0.54)	0.025*** (3.68)	-0.032*** (-3.72)
<i>Leverage</i>	-0.714*** (-11.69)	1.034*** (10.30)	0.137*** (9.03)	-0.085*** (-13.43)	0.120*** (11.98)
<i>ChgLeverage</i>	0.242*** (5.69)	-0.365*** (-5.65)	-0.022** (-2.21)	-0.002 (-0.37)	-0.043*** (-5.55)
<i>CurrentRatio</i>	-0.000 (-0.03)	0.007* (1.81)	0.001*** (5.00)	-0.000*** (-3.49)	-0.002*** (-7.89)
<i>InvAccRec</i>	-0.210*** (-5.30)	0.228*** (3.77)	0.015 (1.58)	-0.034*** (-7.47)	0.060*** (6.70)
<i>Intangibles</i>	-0.830** (-2.02)	1.495*** (2.73)	-0.185* (-1.65)	-0.075* (-1.85)	0.232*** (2.69)
<i>ShortTermInv</i>	0.871*** (6.45)	-1.300*** (-6.14)	-0.176*** (-9.92)	-0.079*** (-10.12)	0.078*** (3.94)
<i>Loss</i>	-0.496*** (-8.68)	1.083*** (13.68)	0.095*** (9.49)	-0.047*** (-10.83)	0.028*** (2.96)
<i>ProbBankruptcy</i>	-1.452*** (-12.42)	3.744*** (17.61)	0.106*** (4.91)	-0.012 (-1.36)	-0.020 (-1.07)
<i>Fixed effects:</i>					
<i>Auditor</i>	Yes	Yes	Yes	Yes	Yes
<i>Industry</i>	Yes	Yes	Yes	Yes	Yes
<i>Year</i>	Yes	Yes	Yes	Yes	Yes
<i>Constant</i>	3.552*** (7.11)	-4.653*** (-7.58)	0.522*** (4.38)	-0.217*** (-7.15)	0.858*** (10.11)
Observations	31,331	31,327	31,418	31,325	30,862
Adjusted R <sup>2</sup>			0.119	0.142	0.618
Pseudo R <sup>2</sup>	0.212	0.463			

This table presents results of regressing measures of audit quality and audit fee against test and control variables for auditors who have switched affiliation from non-Big-4 firms to Big-4 firms. The variables are defined in the Appendix B. The z-values (logit) and t-values (OLS) are adjusted for within-cluster correlation at the client-firm level using the Huber-White Sandwich Estimator. \* (\*\*) [\*\*\*] indicates significance at the 10 (5) [1] percent level using two-tailed tests.

**Table 3: Regression Results of Increased Audit Quality (Fee) Due To Recruiting (H2a)**

	(1)	(2)	(3)	(4)	(5)
	<i>GCAccuracy</i>	<i>GC</i>	<i>NumMod</i>	<i>EarningsQuality</i>	<i>LnAF</i>
<i>ToBig4Pre</i>	0.054*	-0.095**	-0.005	0.004**	0.061***
	(1.76)	(-2.31)	(-0.75)	(1.98)	(10.11)
<i>LnTA</i>	0.061***	-0.146***	-0.036***	0.011***	0.172***
	(9.62)	(-15.94)	(-30.20)	(28.64)	(124.44)
<i>LnAge</i>	0.050***	-0.149***	-0.021***	0.018***	0.054***
	(6.36)	(-14.12)	(-11.47)	(34.44)	(31.51)
<i>LnEmployees</i>	-0.218***	0.006	-0.008***	0.008***	0.336***
	(-22.48)	(0.45)	(-4.38)	(14.14)	(158.61)
<i>NumberIndustries</i>	-0.064***	0.029	0.005	-0.009***	-0.001
	(-3.54)	(1.12)	(1.23)	(-8.12)	(-0.31)
<i>ROA</i>	-0.051	-0.887***	-0.277***	-0.489***	-0.026
	(-0.50)	(-6.46)	(-9.98)	(-43.16)	(-1.23)
<i>SalesGrowth</i>	-0.032***	0.027***	0.008***	-0.023***	-0.020***
	(-14.25)	(9.20)	(12.77)	(-74.14)	(-38.78)
<i>CashFlow</i>	-0.062***	0.213***	0.043***	0.036***	-0.063***
	(-6.18)	(17.14)	(12.88)	(22.04)	(-26.91)
<i>Leverage</i>	-0.621***	1.123***	0.188***	-0.096***	0.119***
	(-39.08)	(30.44)	(37.59)	(-59.02)	(38.13)
<i>ChgLeverage</i>	0.241***	-0.498***	-0.047***	0.033***	-0.016***
	(22.07)	(-21.94)	(-13.33)	(21.38)	(-6.75)
<i>CurrentRatio</i>	0.002***	0.003**	0.001***	-0.000***	-0.003***
	(2.63)	(2.25)	(8.10)	(-8.13)	(-19.10)
<i>InvAccRec</i>	-0.161***	0.169***	0.015***	-0.037***	-0.005*
	(-15.29)	(11.67)	(4.96)	(-30.56)	(-1.93)
<i>Intangibles</i>	-0.917***	1.093***	-0.224***	-0.086***	0.346***
	(-9.80)	(9.09)	(-7.29)	(-10.38)	(14.08)
<i>ShortTermInv</i>	0.902***	-0.727***	-0.252***	-0.095***	0.010*
	(28.52)	(-16.57)	(-44.36)	(-48.40)	(1.69)
<i>Loss</i>	-0.556***	0.955***	0.145***	-0.031***	0.072***
	(-43.17)	(54.15)	(44.73)	(-30.84)	(26.72)
<i>ProbBankruptcy</i>	-1.732***	3.575***	0.175***	0.010***	-0.073***
	(-59.06)	(63.42)	(24.23)	(4.33)	(-12.70)
<i>Fixed effects:</i>					
<i>Industry</i>	Yes	Yes	Yes	Yes	Yes
<i>Year</i>	Yes	Yes	Yes	Yes	Yes
<i>Constant</i>	3.250***	-5.235***	0.411***	-0.185***	0.742***
	(39.94)	(-46.04)	(23.34)	(-36.99)	(41.34)
Observations	483,549	483,536	483,551	451,554	474,761
Adjusted $R^2$			0.132	0.170	0.526
Pseudo $R^2$	0.184	0.421			

This table presents results of regressing measures of audit quality and audit fee against test and control variables for a sample consisting of the auditees of non-switching non-Big-4 audit partners and the auditees of audit partners switching from non-Big-4 audit firms to Big-4 firms. For the switching partners, only observations from the years prior to the switch are included. *ToBig4Pre* = 1 for auditees of partners switching affiliation from non-Big-4 firms to Big-4 firms in the years before the switch takes place, and 0 otherwise. The variables are defined in the Appendix B. The z-values (logit) and t-values (OLS) are adjusted for within-cluster correlation at the client-firm level using the Huber-White Sandwich Estimator. \* (\*\*) [\*\*\*] indicates significance at the 10 (5) [1] percent level using two-tailed tests.

**Table 4: Results for Enhanced Learning After the Switch (H2b)**

Panel A: Descriptive statistics of the number of hours partners spend on continuing professional education (CPE)

	Before the switch to Big-4 firms					After the switch to Big-4 firms					t-value
	Mean	SD	P25	P50	P75	Mean	SD	P25	P50	P75	
meanCPEsum	151	34	128	142	167	168	52	136	156	183	2.01**
meanCPEaudit	46	11	39	45	54	56	18	42	53	65	3.27***
meanCPEethics	18	3	16	18	20	22	6	18	21	23	3.70***
meanCPEothers	87	32	66	74	98	91	38	65	77	103	0.62

Panel B: Regression results of the large office effect

	(1)	(2)	(3)	(4)	(5)
	<i>CGAccuracy</i>	<i>GC</i>	<i>NumMod</i>	<i>EarningsQuality</i>	<i>LnAF</i>
<i>SwitchYearLargeOffice</i>	0.053 (1.41)	-0.086 (-1.57)	-0.002 (-0.30)	0.006** (2.11)	0.034*** (6.38)
<i>FirstYearLargeOffice</i>	0.099** (2.16)	-0.258*** (-3.64)	-0.008 (-1.19)	0.002 (0.83)	-0.006 (-1.08)
<i>AfterFirstYearLargeOffice</i>	0.193*** (4.19)	-0.410*** (-4.88)	0.008 (1.21)	0.006** (2.19)	0.008 (1.30)
Observations	31,331	31,327	31,418	31,325	30,862
Adjusted $R^2$			0.119	0.142	0.618
Pseudo $R^2$	0.213	0.465			

Panel C: Regression results for the difference in audit quality and audit fees for clients of switching partners and existing Big-4 partners in the short term and long term after the switch period.

	(1)	(2)	(3)	(4)	(5)
	<i>GCAccuracy</i>	<i>GC</i>	<i>NumMod</i>	<i>EarningsQuality</i>	<i>LnAF</i>
<i>Treat</i>	-0.170*** (-2.67)	0.196** (2.09)	0.032*** (3.16)	-0.011*** (-2.85)	-0.093*** (-8.33)
<i>TreatLT</i>	0.108 (1.16)	-0.386*** (-2.65)	0.005 (0.41)	0.022*** (3.98)	0.087*** (6.26)
<i>LT</i>	0.002 (0.03)	-0.362*** (-3.42)	-0.020** (-2.44)	-0.004 (-1.07)	0.018* (1.75)
Observations	27,921	27,921	27,921	27,727	27,358
Adjusted $R^2$			0.119	0.130	0.546
Pseudo $R^2$	0.157	0.409			

Panel A presents the descriptive statistics on the average number of hours spent on continuous professional education (CPE) by audit partners who have switched firm affiliation from non-Big 4 firms to Big-4 firms. The descriptive statistics include the mean (Mean), standard deviation (SD), and the 25<sup>th</sup>, 50<sup>th</sup>, and 75<sup>th</sup> percentiles. For each audit partner, we calculate the average CPE hours on all courses (*meanCPEsum*), audit course (*meanCPEaudit*), ethics course (*meanCPEethics*), and other courses (*meanCPEothers*). The first (next) five columns report the statistics for the number of CPE hours before (after) partners switch to Big-4 firms. The last column (t-value) reports the t-value of the difference between the means before and after the switch.

Panel B reports regression results of audit quality on *LargeOffice* and interaction variables between *Distance* and three test variables *SwitchYear*, *FirstYear*, and *AfterFirstYear*. *Distance* is based on driving time (minutes) by car to the nearest large city (a large city is defined as one of the four largest cities in Norway).  $Distance = \ln(\text{driving time by car according to Google}) * (-1)$ . All control variables and

industry and year fixed effects are included.

Panel C presents results for regressing measures of audit quality/fee on *Treat*, *LT*, and their interaction term, *TreatLT*. *LT* proxies for a long-term effect and equals 1 if *AfterFirstYear*=1 and 0 if *SwitchYear*=1 or *FirstYear*=1. *Treat*=1 for clients of partners switching to Big 4 firms, and 0 for clients of existing Big 4 partners. Note that only observations of treatment clients after the switch are used in this test. The treatment and control samples are matched on all control variables based on PSM. All control variables and fixed effects on industries are included. \* (\*\*) [\*\*\*] indicates significance at the 10 (5) [1] percent level using two-tailed tests.

**Table 5: Results for Stronger Incentives and Monitoring After the Switch (H2c)**

Panel A: Financial positions pre and post audit partners switch from non-Big 4 firms to Big 4 firms

	PreSwitch	PostSwitch	Diff	t-value
Salary	1,788,396	2,608,853	-820,457	5.26***
Net Wealth	1,476,719	2,639,561	-1,162,842	2.79***
Total Wealth	2,617,068	5,716,343	-3,099,275	7.45***
CPI_adj_Salary	1,729,580	2,387,599	-658,019	4.69***
CPI_adj_NetWealth	1,423,402	2,413,027	-989,626	2.59**
CPI_adj_TotWealth	2,526,640	5,235,957	-2,709,318	7.11***

Panel B: Regression result using audit modification related to delayed filing

	Delayed filing
<i>SwitchYear</i>	-0.194** (-2.09)
<i>FirstYear</i>	-0.003 (-0.02)
<i>AfterFirstYear</i>	-0.258 (-1.54)
Observations	30,816
Pseudo $R^2$	0.138

Panel A presents audit partners' financial positions pre and post they switch from non-Big 4 firms to Big 4 firms. The first two columns present the average salary, net wealth, and total wealth pre ("PreSwitch") and post ("PostSwitch") the audit partners switch firm affiliation. The column "Diff" reports the difference pre and post the switch. The last column ("t-value") reports the t-statistics of the differences. The first three rows report the values unadjusted by consumer price index (CPI) and the last three rows present the values adjusted by CPI on year 2006.

Panel B reports the results of regressing individual deviations from a clean audit report due to delayed filing on the three test variables (*SwitchYear*, *FirstYear*, and *AfterFirstYear*) and the full set of controls using logit. The variables are defined in the Appendix B. The z-values in the parentheses are adjusted for within-cluster correlation at the client-firm level using the Huber-White Sandwich Estimator. \* (\*\*) [\*\*\*] indicates significance at the 10 (5) [1] percent level using two-tailed tests.

**Table 6: Additional Tests**

Panel A: Regression results of differences in audit quality/fee between clients of switching partners and non-Big-4 partners who have not switched firm affiliation.

	(1)	(2)	(3)	(4)	(5)
	<i>GC</i> <i>Accuracy</i>	<i>GC</i>	<i>NumMod</i>	<i>EarningsQuality</i>	<i>LnAF</i>
<i>TreatSwitchYr</i>	0.048 (0.65)	0.032 (0.33)	-0.034*** (-2.72)	-0.000 (-0.06)	0.030** (2.48)
<i>TreatFirstYr</i>	0.208** (2.48)	-0.270** (-2.36)	-0.008 (-0.55)	0.011* (1.96)	0.085*** (6.63)
<i>TreatAfterFirstYr</i>	0.033 (0.37)	-0.426*** (-2.97)	0.003 (0.19)	0.013** (2.50)	0.148*** (9.58)
Observations	62,061	62,061	62,061	61,602	60,568
Adjusted $R^2$			0.105	0.141	0.569
Pseudo $R^2$	0.201	0.407			

Panel B: Regression results of changes in audit quality and audit fees after the auditor switches affiliation from one non-Big-4 Audit firm to another non-Big-4 audit firm (placebo analyses)

	(1)	(2)	(3)	(4)	(5)
	<i>GC</i> <i>Accuracy</i>	<i>GC</i>	<i>NumMod</i>	<i>EarningsQuality</i>	<i>LnAF</i>
<i>SwitchYear</i>	0.020 (0.28)	-0.108 (-1.13)	-0.000 (-0.00)	-0.002 (-0.52)	-0.017** (-1.98)
<i>FirstYear</i>	0.133 (1.40)	-0.179 (-1.31)	-0.005 (-0.30)	-0.000 (-0.00)	-0.025** (-2.15)
<i>AfterFirstYear</i>	0.090 (0.73)	-0.212 (-1.19)	-0.048** (-2.34)	0.001 (0.11)	-0.043*** (-2.78)
Observations	24,965	24,532	25,305	24,151	24,955
Adjusted $R^2$			0.149	0.167	0.625
Pseudo $R^2$	0.227	0.488			

Panel C: Regression results for testing the financial-health model

	(1)	(2)	(3)	(4)
	<i>ROA</i>	<i>Leverage</i>	<i>SalesGrowth</i>	<i>Loss</i>
<i>SwitchYear</i>	-0.002* (-1.65)	-0.001 (-0.09)	-0.016 (-0.46)	-0.005 (-0.62)
<i>FirstYear</i>	0.001 (1.12)	0.003 (0.29)	0.001 (0.03)	-0.001 (-0.10)
<i>AfterFirstYear</i>	0.000 (0.05)	-0.006 (-0.42)	-0.037 (-0.68)	-0.005 (-0.44)
Observations	31,418	31,418	31,418	31,418
Adjusted $R^2$	0.534	0.650	0.028	0.422

Panel A provides results based on a difference-on-differences analysis by comparing the treatment sample (clients of partners switching to Big-4 firms) and the control sample (clients of non-Big-4 partners who have not switched firm affiliation in the sample period). The indicator variable, *Treat*, is equal to 1 for the treatment sample and 0 for the control sample. We construct the interaction terms, *TreatSwitchYr*, *TreatFirstYr*, and *TreatAfterFirstYr*, which are multiplications between *Treat* and the three test variables (*SwitchYear*, *FirstYear*, and *AfterFirstYear*). We re-estimate eq. (1) in the paper by adding these new variables. We only report the results on the three interaction terms for brevity.



Panel B presents results of regressing measures of audit quality and audit fee against test and control variables for a sample consisting of the auditees of audit partners who switch audit-firm affiliation from one non-Big-4 audit firm to another non-Big-4 firm. For brevity, only coefficients and t-values (z-values) for the test variables are reported. The control variables are the same as those in Table 2, but not tabulated for brevity. The variables are defined in the Appendix B. The z-values (logit) and t-values (OLS) are adjusted for within-cluster correlation at the client-firm level using the Huber-White Sandwich Estimator. \* (\*\*) [\*\*\*] indicates significance at the 10 (5) [1] percent level using two-tailed tests.

Panel C presents results of regressing measures of financial-health variables with the same test and control variables as in Table 2, but we only tabulate the test variables for brevity. The variables are defined in the Appendix B. The z-values (logit) and t-values (OLS) are adjusted for within-cluster correlation at the client-firm level using the Huber-White Sandwich Estimator. \* (\*\*) [\*\*\*] indicates significance at the 10 (5) [1] percent level using two-tailed tests.