Audit Firm Software Intensity and Audit Quality

Christian Friedrich
University of Mannheim

W. Robert Knechel
University of Florida & University of Auckland

Marleen Willekens
KU Leuven & BI Norwegian Business School

Victor S. Zuiddam
KU Leuven

ABSTRACT

This study examines the association between audit firm software intensity and audit quality. Despite ongoing discussions about the potential of information technologies to improve financial statement auditing, little is known about the actual impact of audit firm software use on audit outcomes. Using German audit firm level data on the acquisition value of software in use, we find that clients of more software-intensive audit firms have lower absolute discretionary accruals. The effect is less pronounced when firms introduce a new audit methodology but is amplified by audit firm employee training. In an additional analysis, we address whether software intensity may proxy for investments in IT-related human capital (Fedyk et al., 2022) and find that such investments provide incremental explanatory power, while software intensity remains statistically significant. Our results are robust to including client and audit firm fixed effects, using alternative software investment measures, and using a difference-in-difference design. We find similar, somewhat weaker results using restatements as an audit quality proxy. Cross-sectional analyses suggest that our results are stronger with more recent software investment, in audit firms with higher non-audit service revenue, for clients that purchase fewer or no non-audit services from their auditor, and for clients from complex industries.

Data availability: Data used in the paper are available from the authors upon request.

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Key Words: Audit Data Analytics, Audit Information Systems, Audit Software, Audit Support Systems, Audit Technology, Audit Quality.

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I. INTRODUCTION

During the last decade, all major international public accounting firms have intensified their investments in technology in general and software specifically. Such innovation could profoundly change the conduct of an audit (Deloitte, 2014; EY, 2018; KPMG, 2014; PwC, 2018). In response, audit regulators have issued several position papers and alerts, acknowledging the potential for different types of technology to transform financial statement auditing, e.g., blockchain, data analytics, process tracing, and artificial intelligence (e.g., AICPA, 2017; IAASB, 2016). Such investments are not new and cyclical developments in technology and evolving audit methodologies have always had the potential to improve audit quality (Salijeni et al., 2021). What has changed in recent years is the relative scale of technology investments as evidenced by the evolution from analytical procedures to analytical review to data analytics, with parallel developments in the rigor of techniques such as regression analysis, machine learning, and artificial intelligence (Appelbaum et al., 2017, 2018). However, little is known about whether investments in information technology (IT) and software applications by audit firms have improved audit quality in individual engagements (Gepp et al., 2018; Salijeni et al., 2019).[1] In this paper, we examine whether increases in software intensity in audit firms affect the quality of an audit.

Research in information systems has found that the effects of software intensity on business performance are complex and successfully leveraging software intensity requires a mix of

[1] We invoke theory in information systems, but our research question and variable measurement focus on software. As such, we leverage theory from broader information technology to the narrower context of software. We cannot speak to the effects of other information technology (e.g., hardware). We use software intensity, the relative amount of investment in software capital, as our concept of interest (as opposed to absolute software capital) because information systems research suggests that complementary non-technological resources are necessary for effective leveraging of software in practice (Melville et al. 2004). We scale software capital by size using the number of employees because it is independent of accounting choices and reflects the tradeoff of labor for technology.

complementary resources (Melville et al., 2004; Schryen, 2013). There is a high level of uncertainty of software success in auditing because audit engagements are idiosyncratic, which makes it more challenging to assess and develop appropriate software to support the audit process (Knechel et al., 2013). Research has shown that audit partners often promote standardization in the conduct of the audit to facilitate cross-engagement evaluation (Westermann et al., 2015). As a result, there is a need to delicately balance comparability-enhancing standardization and quality-enhancing service delivery to meet idiosyncratic client needs (Knechel et al., 2020). Flexible systems and software may help facilitate this balance as they allow for service individualization and consistent service delivery at the same time (Lehrer et al., 2018).[2] The general lack of prior evidence and competing theoretical arguments make it an empirical question whether audit software intensity has an overall positive impact on audited financial statements.[3]

There is a substantial body of narrowly focused prior literature on the potential effects of software intensity in auditing (Austin et al., 2021; Cao et al., 2015; Eilifsen et al., 2020; Lowe et al., 2018; Salijeni et al., 2019, 2021). Several experiments have investigated the effect of specific technologies or decision aids in laboratory settings, revealing quality and efficiency gains as well as some negative effects (Agoglia et al., 2009; Brazel et al., 2004; Lynch et al., 2009). Large sample archival evidence is limited, however. Banker et al. (2002) and Chang et al. (2011) suggest that software intensity can improve auditor productivity at the audit firm level. However, they do not

[2] Knechel et al. (2020) argue that standardization in "back office" operations can be effective and efficient because those activities often lend themselves to automation. However, standardization in "client facing" operations is more difficult because of the idiosyncratic needs of each client.

[3] We acknowledge the possibility that software intensity may yield efficiency gains instead of or in addition to quality gains. However, efficiency gains from software intensity are challenging to investigate empirically. Additional analyses of audit report lags, which likely reflect scheduling of audit work instead of audit efficiency (Glover et al., 2022), do not reveal effects of audit firm software intensity. Neither do additional analyses of audit fees, which may decrease with efficiency gains but simultaneously increase for audit firms to cover their technology investments (Law and Shen, 2025). Hence, they are not a good measure of efficiency in our setting, which is consistent with recent non-results in Law and Shen (2025). We continue to fail to find results for audit report lags or fees when using our alternative difference-in-difference research design. We present the results of all these analyses in Online Appendix 3.

examine the effect of software on the audit process or audit outcomes. Focusing on employees with AI-related skills, Fedyk et al. (2022) observe that audit firms employing more individuals with AI-related skills have clients with fewer restatements and charge lower fees.[4] Carson and Dowling (2012) show that increased structuring (i.e., standardization) of the audit process through audit support systems leads to lower audit fees but do not address the potential effect of structuring given the idiosyncratic needs of the client. Collectively, this research suggests that software investments can improve audits and reduce the cost of service delivery. However, these papers do not address the potential impact of an audit firm's overall strategy for software intensity.

To construct a measure of audit software intensity, we use data from the largest nine accounting firms in Germany, as German GAAP requires detailed reconciliations of year-on-year changes in intangible assets, including investments in software. We proxy for software intensity by the extent of software with continued use over time.[5] We measure software in use at an audit firm at its historical acquisition cost. We do not use book values because they can be distorted by arbitrary amortization schedules. We scale the investment in software by the number of employees (Chang & Gurbaxani, 2013).[6] We expect higher levels of audit firm software intensity to be associated with higher audit quality (Hypothesis 1). However, the information systems literature notes that major organizational changes may temporarily reduce software effectiveness (Schryen, 2013) and that software requires complementary non-IT resources to fulfill its potential (Melville et al., 2004). Therefore, we also hypothesize that the effect of software intensity on audit quality is

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[4] In a related study, Law and Shen (2025) also find positive audit quality effects, focusing solely on the existence, but not the intensity of AI-related human capital.

[5] This observation is based on the information systems success model from DeLone & McLean (1992) and the Unified Theory of Acceptance and Use of Technology from Venkatesh et al. (2003).

[6] Our audit firm-level data cover the entire firm, including all service lines (e.g., audit, tax, consulting). We cannot determine the allocation of the software or employees across service lines. Conceptually, since audit firms follow a firm-wide strategy, it is unlikely that some service lines heavily invest in software while others may not. Anecdotally, some audit firms explicitly report that new software was for auditing (see Table A2 in the Appendix). Moreover, recent research suggests that software applications may cover assurance and advisory content simultaneously (Austin et al., 2021). Therefore, we expect our measure to capture software intensity of the audit practice.

weaker when there are major changes to the audit firm environment, proxied by the introduction of a new audit methodology (Hypothesis 2), and stronger when there are complementary resources available, proxied by the yearly training cost per employee within an audit firm (Hypothesis 3).

We use absolute performance-adjusted modified Jones discretionary accruals (Kothari et al., 2005) as a proxy for audit quality (Aobdia 2019). Our results suggest that higher audit software intensity is associated with lower absolute discretionary accruals. Moreover, we find that this association is weaker when a new audit methodology is initially introduced, providing support for our second hypothesis. Results also support our third hypothesis, as we find stronger associations of software intensity with audit quality when audit firm employee training is higher.

Because prior research documents a positive association between IT-related human capital and audit quality (Fedyk et al., 2022), our software intensity measure may (partially) proxy for such investments. We test this possibility by incorporating a measure of an audit firm‘s demand for IT-related human capital, which captures investments in in-house IT capabilities for software development. Importantly, in-house capabilities for software development represent a distinct dimension of human capital (hereafter: IT-related human capital) separate from the training measure used to test H3, which captures capabilities to use software as an aid in primary (audit) tasks. Our results indicate that software intensity and IT-related human capital represent distinct investment strategies: both exhibit independent and incremental positive associations with audit quality, and we find no significant interaction effect. This suggests that "making" (IT-related human capital) and "buying" (software intensity) software capabilities contribute incrementally to higher audit quality.

Furthermore, our primary results are robust to a number of tests as they hold for different fixed effects structures, when using high software investment events to create an alternative difference-in-difference research design, and when using alternative test variables. Our results are similar, but statistically weaker, when we use a measure of restatements that is relevant in the German setting as an alternative audit quality proxy.

In cross-sectional analyses, we find that our results are more pronounced for recently acquired software. However, we find that quality benefits only appear in the years *following* the year of implementation. Moreover, our results are stronger in audit firms with higher non-audit revenue and for clients that purchase fewer non-audit services or belong to complex industries. Our results are stronger in the earlier years of our sample period, suggesting that software development has been important in audits for many years. We find no indication of differences between Big 4 and non-Big 4 audit firms, or bigger and smaller offices.

We contribute to the auditing literature in several ways. First, to our knowledge, this is the first archival study to examine the association between audit firm-level software and auditing outcomes using engagement-level data. We provide evidence that, beyond the effect of individual technologies or capabilities, a greater overall commitment of audit firm resources to software yields positive performance effects. Earlier research relied on *perception* of technology use gathered from interviews or surveys or focused narrowly on specific technologies or capabilities (e.g., Christ et al., 2021; Cooper et al., 2019; Law and Shen, 2025). This research has not directly addressed the complex challenge of building firm-wide software resources. Balancing the efforts of standardization and consistent service provision with the need to address idiosyncratic client needs presents distinct challenges and tensions that may not be evident in narrow settings.

Second, we show that technology by itself is not an isolated path to improved audit quality since successful implantation also requires adequate training and non-IT human resources (Ham et al. 2025). More specifically, consistent with a complementarity perspective, we find that software intensity exhibits a stronger positive association with audit quality when audit firms simultaneously invest in non-IT human resources through training (H3). Second, complementing Fedyk et al.

(2022) and Law and Shen (2025), who conceptualize IT-related human capital as a direct source of IT capabilities, we show that investments in software assets and IT-related human capital are distinct strategies that are both independently and incrementally associated with audit quality.

Finally, our results contribute to an understanding of the diffusion and limitations of software in auditing. Making capital investments in an audit firm often involves the tradeoff of short-term partner profit sharing and long-term costs. Our results suggest that software potentially enables auditors to deliver better audits and can inform the current debate among standard setters, regulators, and practitioners about the role of technology in audits, including the use of generative AI (PCAOB 2024). While we do not delve into the latest developments in specific audit technologies, our findings highlight the foundational role of a digital workflow, and the related impact of human resources in implementing and using technology tools, which will continue to evolve in the future.

II. BACKGROUND AND HYPOTHESIS DEVELOPMENT

Software Investments and Audit Services

The main inputs for an audit are the professional staff that comprise the engagement team (Hackenbrack & Knechel, 1997; Knechel et al., 2009) which, in turn, depends on the audit process, the firm's methodology, and related technology (Knechel et al., 2013). In this paper, we focus on the effects of technology by studying the intensity of the software adopted by an audit firm that can be used in the audit process. The information systems literature suggests that it is unclear whether investments in IT will always improve a business process, especially a process that is risky and idiosyncratic as is the case in auditing (McGrath, 1997; Schwartz & Zozaya-Gorostiza, 2003). Research has struggled to establish consistent causal links between IT investments in a business process and subsequent operational improvements (Schryen, 2013). Melville et al. (2004) argue that the creation of value from IT investments may not improve performance unless those investments are paired with investments in non-IT human capital and other complementary non-IT resources (e.g., workplace practices and training). Extending this logic to an audit setting suggests that the potential benefit from investments in IT depends on the firm's audit methodology and training of the audit staff.

Nevertheless, simultaneous investments in software intensity, methodology, and personnel may not lead to the intended benefits in the short term. A misalignment of training and expectations for software use could undermine the intended value of the software. Furthermore, because of the idiosyncratic nature of a client, tension or incompatibility between an auditor's process and a client's own systems may distort the intended use of the software. For instance, Austin et al. (2021) report that client management may restrict an auditor's access to data, potentially limiting the use of advanced data analytics. Moreover, intensive software aimed at enhancing auditor judgment could lead to decreased development of critical thinking, problem-solving skills, and professional skepticism due to distractions, reduced partner presence, or a lack of coaching opportunities (Westermann et al., 2015). Audit software can foster over-reliance on the output of the software (Dowling & Leech, 2014), or an auditor may not exercise adequate skepticism when information provided by systems confirms an auditor's pre-established expectations (Earley, 2015). Another potential problem is that audit software may reveal many potential discrepancies in transaction processing involving issues that are not material but might distract an auditor's attention from more important matters (Salijeni et al., 2019). In combination, these potential risks may negatively influence the quality of an auditor's judgment and outcomes (Barnes, 2004).

Finally, it is generally uncertain whether new software leads to effectiveness gains (better quality), efficiency gains (less resource expenditure for a set quality level), or both (Melville et al., 2004). For example, new technology can create false positive results that auditors need to address with traditional audit procedures, essentially leading to a loss in efficiency (Salijeni et al., 2019).

Furthermore, software contributes to the standardization of processes that may be favored by audit partners (Westermann et al. 2015), but audit quality depends on addressing idiosyncratic client needs, which may limit gains in audit efficiency through standardization (Knechel et al., 2020). Inappropriately high levels of standardization of the audit process potentially reduce the role of skilled professionals (Arnold et al., 2004; Seow, 2011). At the same time, some newer audit technology may be flexible in ways that allow for simultaneous service individualization and consistent service delivery (Lehrer et al., 2018). Audit firms often claim that audit software allows auditors to focus on areas in the audit process where professional judgment is most needed (Cao et al., 2015; Lombardi et al., 2015), suggesting that audit software can lead to better audit quality. In summary, we expect that, if software intensity improves the audit process, quality gains are likely.

Prior Literature and Hypotheses

The audit profession has experienced multiple waves of technological or methodological change, each accompanied by narratives from the profession emphasizing potential enhancements in audit quality (Curtis et al., 2016; Jeppesen, 1998; Salijeni et al., 2019, 2021). However, we know little about whether these claims manifest empirically. Salijeni et al. (2021) argue that these technological changes are reactions to changing socio-economic audit environments and focus on defending the relevance and legitimacy of external auditing. Hence, it is important to examine the effect of audit firm software intensity on the quality of audited financial statements. With the advent of generative artificial intelligence and new generations of data analytics in auditing, this paper addresses how the previous wave of intensified software use, which now forms the backbone of audit methodologies, has influenced the outcomes of audits.

Researchers, practitioners, and regulators are debating the current wave of technological transformation of audit services and the audit profession (e.g., Appelbaum et al., 2017, 2018; Austin et al., 2021; Cao et al., 2015; Eilifsen et al., 2020; Lowe et al., 2018; Salijeni et al., 2019, 2021). Normative research, surveys, and interviews hint at the potential to change the scope, extent, and conceptualization of audit workflows with this transformation (Austin et al., 2021; Salijeni et al., 2021). In practice, all major international public accounting firms have invested heavily in technological developments for years (Deloitte, 2014; EY, 2018; KPMG, 2014; PWC, 2018). These investments accelerated significantly in 2020/2021 because of the challenge of conducting audits during the Covid pandemic. At the same time, Eilifsen et al. (2020) report that the adoption of novel technologies remains limited, a finding that is consistent with more recent interview evidence reported by Kokina et al. (2025). Barriers for adopting novel technologies include concerns about overreliance, independence impairment, explainability and reliability, and data privacy (Austin et al., 2021; Kokina et al., 2025). While these discussions provide insight into the *potential* application of new audit software (see Appelbaum et al., 2017, 2018 for an overview), scholars have pointed out that little is known about whether and how investments in existing audit software affect audit outcomes (Gepp et al., 2018; Salijeni et al., 2019).[7]

Several behavioral studies have examined whether software could make audit procedures more effective, e.g., in the case of fraud brainstorming (Chen et al., 2015), work paper review (e.g., Brazel et al., 2004; Trotman et al., 2015), mode of communication and collaboration (Bamber et al., 1996; Bennett & Hatfield, 2013; Murthy & Kerr, 2004; Murthy, Park, Smith & Whitworth, 2023), or audit support systems (Dowling & Leech, 2014). Results from this literature indicate that software can either be beneficial (Brazel et al., 2004; e.g., Lynch et al., 2009; Murthy & Kerr, 2004; Murthy et al., 2023) or detrimental (Agoglia et al., 2009; Bennett & Hatfield, 2018; Kerr & Murthy, 2009) to audit quality. Complex interactions between software design and contextual factors of the audit task such as auditor attitude, firm culture, and auditor training will influence whether

[7] Recent empirical literature has increasingly investigated the effects of auditors' IT-related human capital (Fedyk et al., 2022; Ham et al., 2025; Law and Shen, 2025). This increasing research activity likely represents a shift in data availability of auditor human capital vs. software, not their relative importance in audit practice.

technology improves quality (Bedard et al., 2003; Dowling, 2009; Dowling et al., 2008). Experimental researchers have also investigated the effect of sophisticated analytical tools for specific audit tasks (Barr-Pulliam et al.; Rose et al., 2017). In one application, Christ et al. (2021) report that combining drones with image-recognition software can improve the quality of inventory audits on animal farms.

Together, these studies confirm the potential of investments in specific software tools at a task-specific level. They do not address whether audit firms have successfully integrated technology and non-IT human resources to balance consistent service delivery with idiosyncratic client needs. Hence, we know little about the overall effect of auditor software intensity on the quality of audit services. A likely reason is the lack of data on the utilization of audit firm software. Studying one accounting firm in-depth, Banker et al. (2002) find positive effects of large IT investments on productivity. Using a large dataset from Taiwan, Chang et al. (2011) confirm this result and add that IT capital contributes more to audit firm productivity growth than human capital. However, neither study analyzes the effects of productivity changes on audit quality at the engagement level. Carson and Dowling (2012) address engagement-level effects and find that fees are lower for clients of auditors with a more structured audit support system. They call for future research to further investigate the association of audit firm software and engagement-level effects. Both professional[8] and research literature support the potential of software to enhance audit services, with only limited evidence of adverse effects, leading to our first hypothesis for software intensity and audit quality:

> **Hypothesis 1 (H1):** *An audit firm's software intensity is positively associated with audit quality at the engagement level.*

[8] For example, BDO 2013/14 reports "With the now completed introduction of our globally standardized auditing software APT, we could realize the planned improvements in the efficiency and *quality* of client service provision in our core business, annual audits." [emphasis added] (careful translation from original German report by one of the authors).

In addition to the overall effects of software intensity on audit quality, our prior discussion suggests that different conditions or circumstances may facilitate or impede the effectiveness of software. Knechel (2007) discusses that transitions in the audit process can create unforeseen obstacles for the conduct of audits based on existing resources and rituals. A major overhaul of a firm's audit methodology may reduce the complementarity of existing software with changing workplace practices. As a result, the potential positive effect of software intensity may be impaired until new practices and software are fully aligned, leading to our second hypothesis:

> **Hypothesis 2 (H2):** *The association of audit firm software intensity and audit quality is weaker when an audit firm introduces a new audit methodology.*

Lastly, we investigate the interaction of audit firm's software intensity with complementary resources available to audit firm employees in the form of internal training. A well-established literature on human capital has established the link between human capital and operational success (Becker 1993; Crook et al. 2011). An organization can shape a firm's human capital through different types of investments that expand the competencies of personnel, e.g., training (Boselie et al., 2005; van Iddekinge et al., 2009). This notion has been acknowledged in auditing research, with recent studies suggesting that auditor human capital gained through continuing professional education (Che et al., 2018) and the extent of auditor training (Friedrich et al. 2024) matter for the conduct and outcome of the audit. For example, Fedyk et al. (2022) present interview evidence suggesting that the primary barrier to the widespread adoption of AI lies in onboarding and training staff. Similarly, Walker and Brown-Liburd (2019), based on interviews with audit partners, find that auditors are increasingly using more complex tools and that training plays a critical role in embedding these investments within the organization. Law and Shen (2025) observe that audit firm demand for general software proficiency increases following the hiring of AI experts. Collectively, this recent literature suggests that effective software use requires appropriately trained audit staff, motivating our third hypothesis:

> **Hypothesis 3 (H3):** *The association of audit firm software intensity and audit quality is stronger when there is increased training available.*

III. SETTING, DATA, AND MODELS

Annual Audits of German Public Entities

We use the German setting for our study due to unique publicly available data related to investments in technology and staffing by audit firms.[9] German national auditing standards (IDW Prüfungsstandards, IDW PS) conform with International Standards on Auditing (ISAs) adjusted for some unique features of the German market, which do not have direct implications for the use and possible effects of audit software in the audit process.[10] Thus, Germany represents a large and representative audit market that allows us to construct a proxy for software intensity for individual audit firms which is, to our knowledge, not available in comparable settings.

Data of German Audit Firms and Variable Definitions

We combine several data sources to construct our variables of interest that are uniquely available in Germany. We manually collect information on audit firm software from each firm's annual financial statement and related notes. We hand-collect employee numbers, data on employee training, and data on audit firm mergers and acquisitions from each audit firm's management report (Lagebericht). These audited reports are a unique feature of German GAAP (Sec. 289 HGB, Sec. 316 Para 1 HGB). Finally, we manually identify changes in audit methodology from each firm's transparency report. Note that all audit firms in our sample provide

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[9] Per 2019 World Bank data, Germany ranked 7th in the world by market capitalization of listed entities, and audits of listed entities are comparable to the audit setting in other developed economies (Porumb et al., 2021). Moreover, Germany is representative regarding digital technologies as in our final sample year (2019), German adults ranked around the OECD median regarding digitalization skills, see Table 1.1 in https://www.oecd.org/content/dam/oecd/en/publications/reports/2019/05/oecd-skills-outlook-2019_c8896fe0/df80bc12-en.pdf.

[10] For an extended discussion of the regulation of annual audits in Germany, see Marten et al., 2020, pp. 167–185.

different lines of service, including statutory audits and non-audit services (e.g., tax, consulting). The data sources cover the entirety of these services but do not consistently disaggregate the data by service line. Therefore, we collect audit firm data aggregated across all service lines.[11]

German GAAP requires private limited liability firms, including audit firms, to produce an annual financial report.[12] As part of this report, firms must capitalize all acquired intangible assets and can choose to capitalize or expense costs of self-developed intangibles (Sec. 248 Para. 2 HGB). Firms must report if they choose to capitalize self-developed intangibles.[13] Sec. 266 Para. 2 HGB requires a firm that meets the size criteria of a medium limited liability firm[14] to disclose at least four different types of intangibles: goodwill, advance payments, other self-developed intangibles, and other acquired intangibles. Many audit firms in our sample choose to label other intangibles (self-developed and acquired) as "software" or indicate in their notes that the majority of "other intangibles" are software.[15] Therefore, we interpret "other intangibles" for all audit firms as software. Firms must provide a detailed reconciliation of the development of intangibles in their notes (Sec. 284 Para. 3 HGB), including historical acquisition cost, current year investments and divestments, current year and accumulated amortization, and book value. To calculate *Software_Intensity*, we use the sum of other self-developed and acquired intangibles measured at historical acquisition cost and scale it by the total number of employees.[16]

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[11] The only report that exclusively covers an audit firm's practice is the transparency report. The only information we collect from this report is whether the firm introduced a new audit methodology.

[12] We access the annual single entity filings of our sample audit firms through the German Federal Gazette (Bundesanzeiger, https://www.bundesanzeiger.de/pub/de/start?2)

[13] Two firms capitalized self-developed intangibles in a few years: BDO 2014/15-2018/19 and EY 2016/17-2018/19. Inferences remain unchanged if we remove those observations.

[14] Sec. 267 HGB: The firm must be above two of the three following thresholds: (1) total assets of m€ 6; (2) revenue of m€ 12; average number of employees of 50.

[15] For the following audit firm years, we could *not* confirm that all or most of their other intangibles are software: KPMG after 2015/16, Mazars after 2017, all observations from Rödl & Partner and Warth Klein Grant Thornton. If we remove those observations, our results are similar to our primary specifications, although we lose significance in Models 1 and 2 of Table 4, Panel A, and in all models in Table 4, Panel C.

[16] An unscaled software variable would likely capture the size of the audit firm because software capital, human capital, and other firm-specific capital are highly correlated. Our approach follows the literature on productivity analysis (Chang and Gurbaxani, 2013). To account for the possibility that audit firms of the same size in terms of employees

<u>Independent Variables</u>

To validate our proxy of software intensity, we conducted semi-structured interviews with representatives from two of the nine firms in our sample. Interviewees described that the most important software for audits is a combination of a big workflow and documentation tool and several focused tools to perform specific audit tasks. The workflow and documentation tool is the digital representation of the audit methodology that is highly customizable and must be used in every audit. Conversely, back-office software, such as customer relationship management systems, "have little impact on the audit" and are typically licensed standard software from third-party vendors. From these interviews it appears that software data included in an audit firm's balance sheet, while possibly incomplete, will reasonably represent software intensity of relevance to public audits. Finally, the interviewees expressed doubt that efficiency improvements from software would be observable and are unlikely to affect fees, supporting our focus on audit quality effects.

Our proxy for audit firm software usage is *Software_Intensity*, which covers all software in continued use by a firm at a point in time. We use the historical acquisition cost of software to construct *Software_Intensity*. Research has shown that day-to-day use of technology is a reliable indicator of successful software implementation.[17] Extensive and continued use of technology is necessary for a persistent impact on performance (Bhattacherjee, 2001; Kim & Malhotra, 2005). Hence, software capital in continued use over time is the most likely to have positive effects. We do not use the book value of software because it is subject to arbitrary amortization and treatment under the tax code so it is unlikely to reflect actual use, i.e., book value may significantly understate the value of a firm's software at a point in time. Furthermore, higher book values will reflect the

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may still have different-sized audit practices because of different service mixes, we alternatively use audit revenue as the denominator to calculate software intensity. Results remain robust, although they become statistically insignificant for Models 1 and 2 in Table 4, Panel A.

[17] For example, see the literature on the Information Systems Success Model (DeLone & McLean, 1992) or the Unified Theory of Acceptance and Use of Technology (Venkatesh et al., 2003).

most recent software investments for which performance effects may not be established or are uncertain.

Table A2 in the Appendix summarizes information about the type of software we capture with our measure. We include audit information systems and workflow software. Audit firms use such software to implement their audit methodology during fieldwork (e.g., BDO LEAD). In later sample years, the disclosure includes more sophisticated applications such as data analytics (e.g., Deloitte 2017/18 and Warth Klein Grant Thornton 2016/17). Finally, it includes back-office systems, such as the S/4 HANA system of KPMG. These software applications are all managed at the national level. Most of this software is applicable to the day-to-day work of auditors, making it likely that they are broadly disseminated and used for most clients. Moreover, while differences in application may exist across audit offices or even individual audits, Fedyk et al. (2022) find that for centrally managed investments in AI-related human capital, the preferable measurement unit is the national level. We match our national-level data on audit firm software to the engagement-level audit quality and control variables.[18]

Our second variable of interest, *New_Audit_Methodology*, is an indicator variable for whether an audit firm reported the first-time, firm-wide use of a new audit methodology. Employing a new audit methodology or significant software application is arguably a major change to the audit environment in which the audit software is embedded. In our sample, audit firms typically use the methodology of the global audit firm network, which typically has a brand name and embeds several software applications. Audit firms must describe the process the audit firm

[18] While we cannot identify the allocation of the software across service lines (e.g., audit, tax, and consulting), audit firms are likely to have an overall strategy for software use such that they make investments in all areas over time. The existence of an overall strategy makes it less likely that some service lines will have radically different levels of investment. Furthermore, some software is service-independent. This is referred to as "back office" investment in Knechel et al. (2020), Proposition 5-1. An example from our sample is KPMG 2019 specifying that "investments in acquired software largely relate to capitalized further developments in the S/4 HANA system" [i.e., the latest SAP business intelligence software]. Research corroborates that assurance and advisory personnel typically use the same software applications (Austin et al., 2021).

uses during the conduct of audits in their transparency reports (Article 13 Sec. 2 (d) Directive (EU) 537/2014). Seven of the firms in our sample introduced a new audit methodology at least once during the sample period (i.e., Mazars and Rödl & Partner did not). Deloitte changed methodology twice, and BDO three times. We code *New_Audit_Methodology* as one, if the brand name and description of the audit methodology changed,[19] a major software application was added or replaced,[20] or the audit firm explicitly reported a methodology change.[21]

Our third variable of interest, *Training_Costs*, is an audit firm's total employee training expense scaled by the number of employees in the audit firm. Employee training is generally intended to improve human capital and is important when a firm is adopting new technology or software. More importantly, employee training is generally audit-firm specific, as is the case for audit software and audit methodology. Hence, it is likely that training increases the complementarities of the audit firm's existing human resources and its software resources.

<u>Dependent Variable</u>

We follow prior research and use accruals-based earnings management as our audit quality proxy. In our setting, other audit quality proxies from prior literature have significant empirical disadvantages. Qualified audit opinions and modified going concern opinions are too rare to provide meaningful analyses, and German regulation does not require disclosure of internal control weaknesses. In a supplemental analysis we also develop a restatement-based measure tailored to the German reporting environment as an alternative proxy for audit quality (see Section IV). Specifically, we use absolute discretionary accruals ($|DA|$) estimated with the performance-

[19] Baker Tilly 2019 reports "For performing annual audits, the audit software Audit Template from Caseware International, adjusted for the worldwide consistent ISA-compliant audit methodology of BTI, was introduced in the middle of 2017. This BTI-specific software called 'Global Focus' […] must be applied for all annual audits of […] consolidated entities that end after December 31, 2018." We code *New_Audit_Methodology* as one in 2019.
[20] Deloitte reports for 2017/18: "For a sound risk assessment and system-supported processing of certain audit areas, the newly developed web-based system 'Audit Online' is employed since the current year."
[21] We exclude methods or software being pilot tested on a small number of engagements.

adjusted modified Jones model (Kothari et al., 2005). We estimate the following OLS model for each industry year with at least ten observations based on the 3,131 observation that have the necessary data for accrual estimation. We define |*DA*| as the absolute value of the residuals from the model:

$$TACC_{it} = \beta_0 + \beta_1(1/Assets_{it-1}) + \beta_2(\Delta Sales_{it} - \Delta Receivables_{it}) + \beta_3 PPE_{it} + \beta_4 ROA_{it-1} + \varepsilon_{it}, \quad (1)$$

where *TACC* is total accruals, calculated as the change in current assets minus the change in cash minus the change in current liabilities plus the change in short term debt minus depreciation and amortization, scaled by lagged total assets; Δ*Sales* is the change total revenues scaled by lagged total assets; Δ*Receivables* is the change in total receivables scaled by lagged total assets; *PPE* is property, plant, and equipment scaled by lagged total assets and *ROA* is return on assets.

Sample Selection and Models

Our sample period begins after the last major rule change in German GAAP that affects accounting for intangibles (Bilanzrechtsmodernisierungsgesetz, or the Accounting Law Modernization Act) which became effective for fiscal years that ended in 2011. We end with fiscal year 2019, the latest data before the COVID-19 pandemic. We manually collect data on statutory auditors, auditor changes, audit fees, and audit report lags from client consolidated financial statements. We retrieve data on client characteristics from Refinitiv Eikon and Bureau Van Dijk Dafne. Industry identification is based on sector classification according to the Frankfurt Stock Exchange which reflects particularities of the German economy.[22]

Our initial sample consists of 3,958 observations from publicly listed audit clients in Germany that have positive total assets and revenues and provide consolidated IFRS-based

[22] In a few cases, we infer industry classification from SIC codes or business descriptions when the sector classification is unavailable.

financial statements.[23] We remove 595 firm-years from the financial services sector, 232 observations with insufficient data for estimating discretionary accruals, and 50 observations where we could not identify the financial statement auditor. We lose another 864 observations because one firm does not provide software data and there is missing data in some years for the other firms. Finally, we drop 105 observations with missing data for calculating control variables. The final sample consists of 2,112 firm-year observations from 367 unique publicly listed client firms, comprising nine of the top ten audit firms in Germany, including the Big 4. We summarize our sample selection procedure in Panel A of Table 1. We present the industry composition and sample distribution by year in Panels B and C, respectively. Panel B of Figure 1 shows the audit firm-years with the necessary data to calculate our software measure.[24] The industry composition is similar to other studies using German data (Friedrich et al., 2023), and the distribution across years is relatively even. Because training costs are voluntarily disclosed, the sample size for analyzing H3 reduces to 1,335 observations.[25]

<<<<< Insert Table 1 about here >>>>>

We test our hypotheses by estimating the following OLS regression model:

$$\begin{aligned}|DA|_{it} = \beta_0 &+ \beta_1 Software\_Intensity_{it} + \beta_2 Ln\_Assets_{it} + \beta_3 Leverage_{it} \\ &+ \beta_4 \Delta Sales_{it} + \beta_5 CFO_{it} + \beta_6 MTB_{it} + \beta_7 Altman\_Z_{it} \\ &+ \beta_8 Big4_{it} + \beta_9 Industry\_Expert_{it} + \beta_{10} Auditor\_Switch_{it} \\ &+ \beta_{11} NAS\_Ratio_{it} + \beta_{12} ROA_{it} + \beta_{13} Lag\_Loss_{it} \\ &+ \beta_{14} |Lag\_TACC|_{it} + \beta_k Fixed\_Effects_{it} + \varepsilon_{it}.\end{aligned} \tag{2}$$

A significant negative coefficient for β_1 supports H1, consistent with lower absolute discretionary accruals. For our test of H2, we estimate equation (2) after adding *New_Audit_Methodology* and

[23] We exclude the accounting firm of Ebner Stolz because of its unique organizational structure. Ebner Stolz only employs CPAs, while a separate legal entity employs all other staff and contracts it to Ebner Stolz. We have no information on the separate entity so we cannot calculate our primary variable of interest.
[24] As Baker Tilly and Deloitte are only available in later years, when the average software intensity is higher than in earlier periods, we repeat our analyses after excluding those two audit firms and find robust results.
[25] Specifically, we lose all observations from Baker Tilly, EY, Rödl & Partner, and Warth Klein Grant Thornton. We also lose Deloitte 2017/18 and 2018/19, KPMG 2014/15, and Mazars 2011-2016.

its interaction with *Software_Intensity*. A significant positive coefficient on the interaction supports H2 that a significant disruption to the audit process of a firm weakens the potential short-term benefit of implementing software. To test H3, we add the variable *Training_Costs* and its interaction with *Software_Intensity* to equation (2). A significant positive coefficient for the interaction supports H3.

We include control variables that prior literature has shown can influence audit quality in general (Reichelt & Wang, 2010) and in the German setting (Ernstberger et al., 2020). All variables are defined in Table A1 in the Appendix. For the audited companies, we specifically control for size (*Ln_Assets*), extent of debt financing (*Leverage*), growth (Δ*Sales*), cash flow from operations (*CFO*), the market-to-book ratio (*MTB*), insolvency risk (*Altman_Z*), return on assets (*ROA*), and losses in consecutive years (*Lag_Loss*). For the audit firms, we control for the Big 4 (*Big4*), industry expertise (*Industry_Expert*), a change in auditors (*Auditor_Switch)*, and the extent of auditor-provided non-audit services (*NAS_Ratio*), Finally, since accruals mechanically reverse, and short-term accruals reverse after one year, we include |*Lag_TACC*|.[26]

We also include industry and year fixed effects to control for unobservable time-invariant industry characteristics and changes over time. For our main tests, we run this baseline model, then add audit firm fixed effects (mechanically dropping *Big4*) to make sure that idiosyncratic characteristics of audit firms do not drive our results, and finally replace industry with client firm fixed effects to control for all time-invariant idiosyncratic client characteristics. We use robust standard errors and cluster by client and audit office.[27] For ease of exposition, we standardize all variables of interest in our regressions to have a mean of zero and standard deviation of one.

[26] We winsorize all variables at the 1 and 99 percent levels.

[27] Ideally, we would cluster by audit firm because variation of our variable of interest is at the audit firm level. However, we only have nine audit firms in our sample which may inflate t-values. Office level clustering yields over 100 clusters.

IV. RESULTS

Descriptive Statistics

Table 2 presents summary statistics for the variables in the model. Absolute discretionary accruals have a mean (median) of 5% (3%) of lagged total assets. We observe that 14% of the clients are audited by an audit firm using a new audit methodology and the average (median) training cost per employee is €10,560 (€9,830), with a modest standard deviation of €2,970. The average (median) amount of audit firm-level software per employee is €3,170 (€3,670), ranging from less than €200 to more than €8,200. This suggests considerable variation across audit firms and time as illustrated in Figure 1. Panel A shows the development of *Software_Intensity* over time. The average software intensity increases each year except for 2014. Panel B depicts the yearly values of *Software_Intensity* separately for each audit firm and highlights audit firm years with methodology changes. Trends clearly differ across audit firms, and no single audit firm is consistently the lowest or highest in terms of software intensity.

<<<<< Insert Table 2 and Figure 1 about here >>>>>

The correlation matrix is presented in Table 3. We find a negative and significant bivariate correlation between absolute discretionary accruals and audit firm software per employee, suggesting that higher levels of audit firm software correlate with higher audit quality. The correlations between software intensity and a new audit methodology (H2) and auditor training (H3) are small, suggesting these variables are not jointly determined. Most other correlations are relatively small, with the strongest correlation of -0.67 between *ROA* and *Lag_Loss*.[28] Multicollinearity is not an issue, as our largest variance inflation factor is 2.83 (for *ROA*).

[28] Note that the correlation between software intensity and the Big 4 dummy is negative, which is inconsistent with the plausible expectation that the Big 4 are more likely to make significant software investments. This correlation is driven by the exceptionally high software intensity of the non-Big 4 firm BDO. If we remove BDO from our sample, the correlation between software intensity and Big 4 becomes positive and significant.

<<<<< Insert Table 3 about here >>>>>

Main Results

Panel A of Table 4 presents the results from estimating equation (2) with |*DA*| as the dependent variable to test H1. The first column presents a pooled-OLS regression with industry and year fixed effects. We find that the coefficient for *Software_Intensity* is negative and significant (-0.002, p<0.10). In economic terms, a one-standard deviation increase in software intensity is associated with a decrease in absolute discretionary accruals of about -0.002.[29] The magnitude of this effect is roughly 25% of the Big 4 effect. We add audit firm fixed effects in Column (2) and replace industry with client fixed effects in Column (3).[30] The negative and significant effect of *Software_Intensity* is robust in both columns (-0.007/-0.011, both p<0.01).

Our results for H2 are presented in Panel B of Table 4. Again, the first column presents a pooled-OLS regression with industry and year fixed effects. The main effect for *New_Audit_Methodology* itself is negative and significant (-0.006, p<0.05), which indicates that introducing a new audit methodology is generally associated with higher audit quality. *Software_Intensity* continues to be significant (-0.003, p<0.01). The interaction of *Software_Intensity* × *New_Audit_Methodology* is positive and significant (0.006, p<0.01). This means that the association we observe between software intensity and discretionary accruals only manifests when audits are conducted under a stable methodology. When an audit firm changes its methodology, any benefits from software intensity are offset by the challenges of introducing the new methodology. Specifically, the interaction coefficient of 0.006 combined with the coefficient

[29] We cannot interpret the coefficients on standardized variables as economic effects after adding audit firm fixed effects because software intensity has substantial variation between audit firms, which audit firm fixed effects remove.
[30] Instead of firm fixed effects, we could run a change model (all variables as year-on-year changes). It results in a loss of observations, which is particularly problematic for H3. Moreover, our test of H2 uses a variable that represents a structural break between two time periods that are longer than one year. Transforming it to a year-on-year change does not allow for a meaningful interpretation. When we repeat our analysis with a changes model for H1 and H3 (untabulated), results remain robust for H1 and are similar, but insignificant for H3.

of -0.003 for *Software_Intensity* suggests that a one-standard deviation increase in software intensity reduces absolute discretionary accruals by -0.003 (37.5% of the Big 4 effect) under a stable methodology but increases absolute discretionary accruals by 0.003 (-0.003 + 0.006; 37.5% of the Big 4 effect) in years when a new methodology is introduced. Our tests for H2 are robust to the alternative fixed effects structures in the second and third column.

Finally, Panel C presents the results for H3. The columns follow the same fixed effects structures as in Panels A and B. The main effect of *Training_Costs* is insignificant in Columns (1) and (3) and marginally significant in Column (2) (0.009, p<0.10).[31] *Software_Intensity* continues to be significant (-0.008, p<0.05). The interaction *Software_Intensity* × *Training_Costs* is negative and significant (-0.005, p<0.05), suggesting that software intensity and staff training have a complementary effect on discretionary accruals. Economically, the coefficient of -0.008 for *Software_Intensity* combined with the coefficient of -0.005 for the interaction term suggests that a one-standard deviation increase in each results in an improvement of -0.013 in absolute discretionary accruals (-0.008 + -0.005, about the same as the Big 4 effect) for clients of audit firms with higher levels of employee training combined with higher levels of software intensity. Again, our results are robust to the fixed effects structures in the second and third column.

<<<<< Insert Table 4 about here >>>>>

Effect of Alternative Investments in Software Capabilities

An important alternative channel through which auditors can increase their software capabilities is the hiring of software specialists (e.g., Fedyk et al., 2022). IT-related human capital may substitute or complement investments in software assets in creating IT capabilities, as IT specialists do not serve as auditors or consultants but rather as developers who either create new

[31] This result suggests that audit quality is marginally lower for higher training levels. To examine this counterintuitive result further, we rerun our analysis with only *Training_Costs* (omitting *Software_Intensity* and the interaction term). In this case, the coefficient for *Training_Costs* is insignificant (untabulated).

software assets (potentially substituting for direct investments in software) or enhance existing ones (serving as complements to, or substitutes for, enhancement investments). To examine whether investments in IT-related human capital affect our analysis and our primary findings are robust to investments in such capabilities, we collect job-posting data from LinkUp and Revelio Labs (see Online Appendix 2 for details). Specifically, we construct the variable *Software_Hires*, defined as the number of software development-related job postings scaled by the total number of job postings within an audit firm-year. We then re-estimate equation (2) after either adding *Software_Hires* or additionally interacting it with *Software_Intensity*.

We lose more than half of our sample observations because of the limited availability of job-posting data, retaining 997 observations. As a result, we rarely observe more than two years for a given audit firm (and for most individual client firms), which is why we include only industry and year fixed effects in this analysis. Table 5 reports the results, with Model 1 adding *Software_Hires* and Model 2 additionally including the interaction between *Software_Hires* and *Software_Intensity*. We find significantly negative associations of both *Software_Hires* and *Software_Intensity* with absolute discretionary accruals, indicative of higher audit quality, but no significant interaction effect. Because both variables are standardized, the coefficient estimates on the main effects suggest similar economic magnitudes, although software assets appear to be slightly more influential. These results indicate that creating software capabilities through investing in either software assets or IT-related human capital independently and incrementally contributes positively to audit quality.[32]

Taken together, this additional evidence indicates that audit firms effectively view software assets and IT-related human capital as distinct investments in their technology capabilities that are

[32] Note that the correlation between *Software_Hires* and *Software_Intensity* is negative and significant (–0.47, untabulated). It suggests that audit firms make a joint investment decision of whether to invest in software assets or human IT capital, and at least partially trade off these two investment types.

both independently and incrementally positively associated with audit quality. Our findings therefore complement earlier research that examines IT-related human capital as a central component of audit firms' software capabilities (e.g., Fedyk et al., 2022; Law and Shen, 2025).

<<<<< Insert Table 5 about here >>>>>

Alternative Research Design

Our variable *Software_Intensity* likely includes measurement error, potentially limiting the precision of our primary research design. To triangulate our results, we develop a fundamentally different research design relying on major changes in software intensity over time. Specifically, we create a stacked difference-in-difference design analyzing short windows around high-investment events, which we identify as all software investments (scaled by employees) that are at least 50% higher than the next highest investment of the same audit firm and above the median of all yearly investments of all audit firms in our sample. Thus, we ensure that these investments are unusually high.

Having identified high-investment events,[33] we select a short-window sample from the year before the investment to two years after the investment. We include all client years of the high-investment auditor as treated observations (*Treat* = 1) and all client years of all other auditors as control observations (*Treat* = 0) until their auditor has a high-investment event. For example, if our window is 2013-2016 and auditor A has a high-investment event in 2015, we exclude 2016 for auditor A. We define the year before and of the investment as the pre-period (*Post* = 0) and the two years after the investment as the post-period (*Post* = 1). We then estimate the following OLS regression with our control variables from equation (2) and firm, year, and auditor fixed effects:

[33] In untabulated analyses, we use these high-investment events to address the possibility that the benefits of prior software intensity dissipate after exceptionally high investments because the benefits of recent software additions may not yet be effectively. Consistent with this possibility, when we interact *Software_Intensity* with an indicator for high-investment events, the interaction coefficient is insignificant.

$$|DA|_{it} = \beta_0 + \beta_1 Treat_{it} + \beta_2 Post_{it} + \beta_3 Treat_{it} \times Post_{it} + \beta_k Controls_{it} + \beta_l Fixed\_Effects_{it} + \varepsilon_{it}. \quad (3)$$

Table 6 presents the results when we either use all high-investment events to create our sample (Model 1) or only high-investment events of Big 4 auditors (Model 2). In both Models, the difference-in-differences coefficient on *Treat* × *Post* is negative and significant (-0.006/-0.006, both $p<0.10$), corroborating our results for H1.[34]

<<<<< Insert Table 6 about here >>>>>

Robustness Tests

We perform several robustness tests. First, we create alternative test variables using a different measure of software intensity. For H1, we replace *Software_Intensity* with *Book_Value_Software_Intensity*, reflecting the book value rather than historical acquisition cost of software. As discussed above, we argue that amortization schedules may not reflect the actual value of software in continued use, but our primary measure implicitly assumes that the value remains constant, rather than declining, over time.[35] Panel A of Table 7 presents our results with our alternative software intensity measure. The coefficient for *Book_Value_Software_Intensity* (-0.002, $p<0.01$) is statistically and economically similar to our primary results.

As an alternative variable to test H2, we create an indicator variable for audit firm years where an acquisition of an IT specialist company was reported in the management report of the audit firm's annual financial statement (*IT_Acquisition*).[36] It is possible that an acquisition of an IT specialist signals significant changes in the IT environment in which the software is embedded. This change may alter or disrupt the effective implementation of existing software. Results reported

[34] To address the possibility that high-software investment events also affect audit efficiency, we re-estimate equation (3) with the natural logarithm of audit report lags or audit fees as the dependent variable. We do not observe any significant results (see Online Appendix 3, Tables OA.2 and OA.4).
[35] In February 2022, the German tax authority advised to amortize most software after one year. Previously, the usual amortization schedule was three years (Wittlinger, 2022).
[36] Note this analysis is conducted using our original software intensity measure.

in Panel B of Table 7 corroborate our main test, i.e., the coefficient for the interaction is positive and significant (0.004, $p<0.10$), although it is insignificant with the most rigid fixed effects structure in the third column. Untabulated tests with an indicator variable equal to one for audit firm years with *either* a new audit methodology or an acquisition of an IT specialist company also corroborate our main results.

To provide an alternative test for H3, we consider the average salaries paid by audit firms since higher wages are likely correlated with higher performing personnel. We calculate *Average_Wage* by dividing the annual personnel expense of the audit firm by the number of employees.[37] Hoopes et al. (2018) show that auditor salaries are positively associated with audit quality, suggesting they are a reliable measure of auditor human capital. However, we only have firm-wide averages, so we are unable to consider office, rank, or service line differences. Results reported in Panel C of Table 7 corroborate our main findings except for the baseline model in Colum (1) without audit or client firm fixed effects.

<<<<< Insert Table 7 about here >>>>>

To test the robustness of our results to alternative audit quality proxies, we specify a restatement measure, *Restatement*, which is an indicator variable capturing whether a client firm filed a corrected annual report (see Online Appendix 1 for details about the German restatement context and variable measurement). Results reported in Table 8 are directionally consistent with our primary results for hypotheses H1 and H3, although we lose significance in most of our models. Nonetheless, they remain economically meaningful. For instance, the coefficient on *Software_Intensity* in Model 1 of Panel A suggests that a one-standard-deviation increase in software intensity decreases the likelihood of a restatement by 0.4 percentage points, roughly 18%

[37] Note this analysis is conducted using our original software intensity measure.

lower than the overall restatement likelihood of 2.18%. This alternative dependent variable has several disadvantages, which is why we do not use it in our primary analyses. Restatements are rare, which provides little variation in our relatively small sample and makes it difficult to detect statistically significant effects. This limited variation is especially problematic for our tests of H2 and H3. Of the 2,112 observations in our sample, we observe only 46 restatements (2.18% of observations).

<<<<< Insert Table 8 about here >>>>>

Next, we remove Rödl & Partner for 2017/18 and 2018/19 from our analysis because they moved their IT department to a separate legal entity, thus removing certain software assets from their balance sheet although they were probably still in use. The results (untabulated) remain robust. Furthermore, if audit firms have self-developed software, they can choose between booking an expense or capitalizing the self-development cost. To control for this possibility, we remove all audit firm years with capitalized self-developed software. Our results (untabulated) remain robust. Finally, in untabulated tests, we replace audit firm fixed effects by audit office fixed effects. Results remain inferentially identical.

Cross-sectional analyses

We conduct several cross-sectional analyses to explore whether the overall positive association of software intensity with audit quality is concentrated in certain subsamples. We run these models with only industry, year, and audit firm fixed effects because client firm fixed effects may remove meaningful variation across clients. Table 9 provides an overview of cross-sectional analyses based on a median split by (1) age of software, (2) total non-audit service revenue of the audit firm, and (3) non-audit service revenue from an individual audit client. We also run a test based on whether a client is in a complex industry. We then estimate equation (2) within each subsample and report the results in two columns in Table 9. Some differences in sample sizes result because we remove the median observation(s) and the first and second cross-sectional variables are measured at the audit firm level.

<<<<< Insert Table 9 about here >>>>>

Age of Software

In Panel A, we partition the sample by *Young_Software*, which is the book value of software divided by the historical acquisition costs of the same software. German GAAP requires a firm to use the same amortization schedules for similar assets, but different firms may use different amortization schedules. *Young_Software* indicates the average age of software in continued use, where smaller (larger) values represent older (younger) software. Younger software gives audit firms access to the latest technological advancements, which may bring the largest improvements to the audit process but may take time to effectively implement. Older software has been in continued use for a long time, indicating software success, with a well-established cadre of employees trained to use the software. Also, older software is probably well-embedded in the conduct of repeated audit engagements.

The median of *Young_Software* is 0.198, indicating that about 80% of the median audit firm's software has been amortized suggesting an accelerated approach to amortization. The first column presents the results for the subsample with below-median values of *Young_Software*, i.e., older software. We do not find significant effects of software intensity. The second column presents results for the subsample with above-median values of *Young_Software*, i.e., younger software. The coefficient of *Software_Intensity* is negative and significant (-0.011, $p<0.01$). Hence, it seems that younger software drives our overall results. Untabulated tests suggest that current year software investments do not affect audit quality, but software investments lagged by one year have a positive effect. Hence, although younger software may help improve audit quality, there seems to be a transition period before new software investments have a positive impact on the audit.

Non-Audit Services

We next consider the effect of the mix of audit and non-audit services (NAS) on the audit *firm* level. Panel B of Table 8 presents results when we partition the sample by the median of *NAS_Revenue*, which is the ratio of an audit firm's annual revenue from NAS to its total annual revenue. We cannot observe to what extent our software measure reflects a benefit to auditing, NAS, or all service lines. Firms with a relatively large proportion of revenue from audits may be more likely to invest in software that is specific to the audit practice, suggesting a large impact of software intensity. Alternatively, a firm with more NAS revenue may have more technical expertise within the firm overall, allowing the firm to develop and utilize better technology. Finally, firms with a more diverse practice may focus on technology that benefits all lines of service, i.e., audit may benefit from a scaling effect.

The median of *NAS_Revenue* indicates that an audit firm earns 59% of its revenue from non-audit services. For firms with a higher proportion of revenue from audit (first column), we do not find significant effects of software intensity. For firms with a higher proportion of revenue from NAS (second column), we observe a negative and significant coefficient for *Software_Intensity* (-0.016, $p<0.01$). Hence, it seems that audit firms with relatively larger NAS practices drive our results. This result is consistent with multi-disciplinary firms being able to better develop and utilize technology resources within the firm, possibly fostered by cross-subsidization between audit and non-audit services.

In Panel C, we consider the mix of audit and NAS at the *engagement* level. We partition the sample by the median of *NAS_Ratio*, which we measure as NAS fees a client pays to its statutory auditor divided by audit fees. Theoretically, non-audit services could increase an auditor's client-specific knowledge through knowledge spillovers or could threaten auditor independence through economic and social bonding. Software is unlikely to have a strong effect on economic and social bonding so a differential effect would stem from the software interacting with knowledge spillovers. If software intensity substitutes for client-specific knowledge that may be obtained from NAS, then a high level of engagement NAS may not influence the effect of software intensity on audit quality. If software intensity complements client-specific knowledge obtained from NAS, then a high level of engagement NAS may increase the effect of software intensity on audit quality.

The median of *NAS_Ratio* is 0.250, i.e., the median client purchases NAS worth 25% of audit fees. The first column presents the results for the subsample with below-median values of *NAS_Ratio*, i.e., less non-audit services. We find a negative and significant effect of *Software_Intensity* (-0.004, p<0.01). For clients with more NAS, we do not find significant results (second column). In untabulated analysis, we find similar results when we split our sample by whether clients purchase any NAS or no NAS at all. Hence, it seems that clients with no or only low-volume NAS drive our overall results. This is consistent with the knowledge spillover effect of NAS substituting for the benefit of software intensity on audit quality.

<u>Complex Industries</u>

Finally, Panel D partitions clients based on whether they operate in a complex industry (Bills et al., 2015). Theoretically, because higher software intensity likely frees up auditor resources to address more complex audit issues (e.g., Law and Shen, 2025), we expect the effects of software intensity to be stronger in more complex industries. Consistent with this logic, the coefficient on *Software_Intensity* is negative and significant for the subsample of complex industry clients (-0.009, p<0.05) but not for less complex industries.

<u>Early and Late Sample Periods</u>

In untabulated tests, we partition our sample into two time periods: 2011-2015 and 2016-2019. While we cannot precisely identify the types of software our measure captures, it is possible that software investments in the earlier period have a greater effect on audit workflow (i.e., automated workpapers) and back-office operations (i.e., scheduling), while investments in the later period may relate to upgraded knowledge support like data analytics. Our tests suggest that the association of software intensity and audit quality in the earlier period is greater than in the later period, but this difference only occurs when we include industry and year fixed effects in the model. Hence, developments in methodologies that utilize broad-based audit software may be the driver of our results.

Type of Audit Firm

Finally, in untabulated tests, we compare Big 4 firms to non-Big 4 firms. We do not find a differential impact of software intensity on the two types of firms. We also do not find a differential impact when we compare large offices to small audit offices. Our primary results also hold for a subsample consisting only of Big 4 clients (untabulated), except for H3, where we lose power due to further limiting the number of observations.

V. CONCLUSION

This study examines the association between audit firm software intensity and audit quality. Recent investments in audit software mark the current wave of possible technological transformation in the audit profession with accompanying narratives of possible audit quality gains. Still, little is known about how audit firm software intensity affects outcomes in financial statement auditing. Our results suggest that clients of more software-intensive audit firms have lower absolute discretionary accruals, consistent with better audit outcomes. We find evidence that major changes to the audit environment reduce this effect, and training increases this effect. Through an additional analysis, we rule out the possibility that our results are driven by investments in IT-related human capital (Fedyk et al., 2022), as we document that audit software intensity provides incremental explanatory power beyond investments in IT-related human capital in explaining audit quality.

Our paper is subject to several limitations that may suggest avenues for future research. A first major caveat of our research is that we can only observe a firm's capitalized software investments; we cannot observe lease costs, rent payments, or other annual expenses for audit firm software. Nor can we differentiate between different types of software or identify the asset value of IT hardware. In addition, we cannot determine to what extent software supports auditing, non-audit services, or all service lines, and we cannot identify whether software use deviates between offices or clients. In spite of these limitations, our results provide a preliminary overview and establish the overall capability of software intensity to enhance audit quality at the engagement level. Future research could develop measures that capture the technology-focused investments of audit firms more accurately, comprehensively, or at a more granular level. Given the limited availability of public data in other settings, researchers could also seek access to proprietary data from audit firms and link these data to publicly available information on statutory audits.

Second, the German setting entails several caveats, most notably the limited availability of relevant audit quality proxies. Moreover, despite its overall economic size, the number of listed German companies and sufficiently large German audit firms is still small compared to the US and China, potentially limiting the power of the empirical analysis. While these caveats and other particularities of the German setting could limit our findings' generalizability, Germany is a major and interconnected global economy and representative of many auditing settings in developed economies. With strong interconnections of audit methodology and software within global audit networks, and given that most of our observations cover auditors from global networks clients, it is unlikely that the use and effects of audit firm software differ substantially between our setting and other audit settings in developed economies.

Third, our accruals measure is an imperfect indicator of audit quality and may capture some variation in the quality of financial statements that is unrelated to auditing, as financial reports are mainly produced by client management and only adjusted for material misstatements by the auditor. We mitigate this concern by using a measure of restatements that is relevant to the German setting as an alternative proxy for audit quality.

Despite these limitations, our study provides an important starting point for advancing our understanding of how audit firm software intensity affects the quality of audited financial statements in the contemporary audit environment. Our empirical results indicate that investments in audit technology and software are associated with higher audit quality. These findings are relevant for practitioners, regulators, and scholars for several reasons. First, our results inform ongoing debates surrounding the increasing use of audit technology. Second, we provide initial evidence that the benefits of technology may diminish during periods of major changes in the audit process, suggesting that practitioners should exercise particular care when managing substantial technological transitions. Third, the positive interaction between software intensity and training underscores the importance of training auditors to use new technologies effectively—an issue that is equally salient for practitioners and for regulators concerned with governing emerging technologies and designing professional requirements for auditors. Fourth, our results highlight the potential audit quality benefits of a multi-disciplinary firm.

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APPENDIX

TABLE A1 Variable Definitions

Variable	Definition
Altman_Z	Altman's (1968) Z-Score
Training_Costs	Total expense for employee training of an audit firm, scaled by total audit firm employees
Software_Intensity	Historical acquisition cost of all currently used self-developed and other acquired software of an audit firm, scaled by total audit firm employees
Auditor_Switch	Indicator variable, equal to one in the first year of the current auditor's tenure with the firm, and zero otherwise
Average_Wage	Total personnel expense of an audit firm, scaled by total audit firm employees
Big4	Indicator variable, equal to one if a firm is audited by a Big 4 auditor, and zero otherwise
Book_Value_Software_ Intensity	Book value of all currently used self-developed and other acquired software of an audit firm, scaled by total audit firm employees
CFO	Cash flow from operations scaled by lagged total assets
DA	Discretionary accruals, calculated as residuals from OLS estimations on industry-years of the performance-adjusted modified Jones model in equation (1).
|*DA*|	Absolute value of *Disc_Acc*
Δ*Sales*	Change in net sales scaled by lagged total assets
Industry_Expert	Indicator variable, equal to one if the sum of all fees paid to the firm's auditor in the firm's industry-year scaled by the sum of all fees paid to any auditor in the same industry-year is greater than 0.3, and zero otherwise
IT_Acquisition	Indicator variable, equal to one if the audit firm reported an acquisition of an IT specialist company in the management report of its annual financial statement, and zero otherwise
Lag_Loss	Last year's loss indicator
|*Lag_TACC*|	Absolute value of last year's TACC
Leverage	Leverage, measured as total debt scaled by total assets
Ln_Assets	Natural logarithm of total assets
Loss	Indicator variable, equal to one if Net Income is negative, and zero otherwise
NAS_Ratio	Sum of non-audit service fees scaled by audit fees
NAS_Revenue	An audit firm's annual revenue from non-audit services scaled by its total annual revenue
New_Audit_Methodology	Indicator variable, equal to one if the audit firm rolled out a new audit methodology to all engagements of its fiscal year, and zero otherwise
MTB	Market-to-book ratio, measured as firm's market value of equity scaled by its book value of equity

Restatement	Indicator variable, equal to one if a firm's annual report of a given financial year has at least two versions in the German Federal Gazette, with at least one version having a correction designation, and zero otherwise
ROA	Return on assets, measured as net income scaled by the average of last year's and current year's total assets
Software_Hires	Number of software development-related job postings scaled by all job postings during an audit firm financial year
TACC	Total accruals, calculated as net income minus cash flow from operations, scaled by lagged total assets
Young_Software	*Book_Value_Software_Intensity/Software/Intensity*

TABLE A2 Examples of capitalized audit software from notes and management reports of audit firm's annual financial statements

Audit Firm Year	Description of audit software acquisition (careful translation from original German report by one of the authors)
BDO 2012/13	We address the ongoing price competition in the audit market and increasing audit quality and efficiency demands by introducing the audit software APT and installing the function for up-front quality control.
BDO 2014/15	Investments in intangible assets relate to investments in software (including self-developed software). […] The self-developed software is an individual software application for a computerized audit workflow embedded in the audit information system (AIS).
BDO 2018/19	Investments in intangible assets relate to investments in software. They include a self-developed software (BDO LEAD). It is a self-developed individual software application for an IT-based job tool.
Deloitte 2017/18	In the past fiscal year, we have introduced an audit analytics platform. The application supports the financial organization of a company and core functions of the governance system: it systematically unearths and visualizes findings in critical processes, control weaknesses, and risks. Companies receive multiple benefits: they achieve higher transparency and gain efficiency, quality, and control certainty.
Deloitte 2018/19	Auditing will soon be even more technology-driven. We work on it globally within our audit transformation initiative. The investment focus is on further developing our audit methodology, training, and innovative tools. […] In 2018/19, we have introduced the audit analytics platform "Spotlight". It searches transactions for anomalies and inconsistencies and visualizes them. Moreover, "Spotlight" supports fact-based risk assements.
PwC 2017/18	Through our targeted software investments, we are well-positioned for annual audits. This contributed to client gains among all companies that tendered their annual audits due to the EU regulation.
Warth Klein Grant Thornton 2016/17	Our investments in intangible assets relate to investments in software and licenses. […] With the introduction of a new audit software and its extensive possibilities, especially in the domain of data analytics, we will further improve our existing services, gain efficiency, and offer new client-specific solutions.

ONLINE APPENDIX 1

Constructing the variable *Restatement*

To construct our variable *Restatement*, we collect all annual reports available in the German Federal Gazette at the beginning of February 2025 and assign them to firms' financial years by the financial year-end mentioned in the filings' titles. If there are several findings with the same financial year, we then order these by their filing data to identify the oldest as the original filing. For the remaining re-filings, we identify whether they are corrections or amendments. Amendments happen frequently as routine additions of information that were not available when the annual report was originally filed, e.g., voting results from the general assembly. To differentiate corrections from amendments, we develop a set of rules using textual characteristics of amendments and restatements we iteratively identified by (1) manually reviewing a random sample of filings for a clearly differentiating feature; (2) classifying all filings that have this feature into the correct category; (3) repeating (1) and (2) with the remaining filings that did not have the previous identified feature(s). The rules are:

1. If the filing contains the words "Gewinnverwendung" or "Ergebnisverwendung" (appropriation of earnings), it is an amendment because the general assembly votes on the appropriation of earnings, which some firms then report as amendments to their annual report.

2. If the filing is shorter than 10% of the original filing (measured as number of characters) and at least 300 characters long, and

    a. if it contains the words "Berichtigung" or "Korrektur" (both synonyms for correction) or "korrigiert" (corrected), it is a restatement because it contains an excerpt of the original filing labeled as corrected.

    b. if it contains a section, figure, or table title that is also included in the original filing, it is a restatement because amendments only add, but do not repeat information

3. If the filing was filed at least 10 days after the original filing, is between 95% and 105% of the length of the original filing (measured as number of characters), and we find at least one match of the filings first, middle and last 100 characters in the original filing, it is a restatement because it has essentially the same structure as the original filing, just with changes to whatever was restated.

4. If the filing was filed at least 20 days after the original filing and we find matches of the filings first, middle and last 100 characters in the original filing, it is a restatement because it repeats essential parts of the original filing.

5. If the filing was filed at least 30 days after the original filing and we find at least one match of the filings first, middle and last 100 characters in the original filing, it is a restatement because it has at least some overlap with the original filing and amendments are, in many cases, filed quickly after the original filing.

6. If 1.-5. do not apply, the filing is an amendment because there is no apparent overlap with the original filing.

ONLINE APPENDIX 2

Constructing the variable *Software_Hires*

To construct our variable *Sofware_Hires*, we first collect all LinkUp data from our sample period where the name of the firm posting the job matches different versions of the names of the audit firms in our sample. We keep all job postings from Germany, which we identify by combining the country and city variables from LinkUp with information from the job url and job title variables from LinkUp. Specifically, we search for job url's and job titles containing German job titles, division abbreviations (e.g., "WP" for "Wirtschaftsprüfung", the German word for audit), and city names. We then manually review the matches for plausibility and reassign the country variable to Germany where appropriate. To differentiate software development-related jobs from all other jobs (including software-related consulting which is not software development), we search job titles with the following, case-insensitive regular expression with R's *regex* syntax and define all search hits as software development-related jobs:

"(IT(?!.{0,4}(Consult|Service|Advi|Prüf|Aud|Bera|Ris|Recht|Secu))|BI|(S|s)oftware|App| Technology Com|(d|D)ata|Server|(A|a)dministrator|Infrastructure En|SQL|Digital|Entwickler| Developer)"

Next, we collect all Revelio Labs data from our sample period where the firm posting the job matches different versions of the names of the audit firms in our sample or where no firm could be identified. We then use the text in the raw description and raw title field in Revelio Labs' raw job postings data to search for regular expression representations of the audit firms from our sample. Similar to the LinkUp data, we then search for German city names and specific job title regular expressions to filter the resulting data for job postings in Germany. To differentiate software development-related jobs from all other jobs (including software-related consulting which is not software development), we use job role classifications from Revelio Labs and identify job postings as software development-related if they belong to one of the following classifications using the variable role_k50:

"Data Analyst", "IT Project Manager", "IT Specialist", "Software Engineer";

or one of the following classifications using the variable *role_k1500*:

"information security engineer", "infrastructure architect", "network consultant", "network consulting engineer", "network engineer", "network security engineer", "security architect".

Finally, we merge both datasets and assign auditor-year groupings by auditor name and auditor financial years from our primary data. Within each group, we count the number of job postings and the number of software development-related job postings, resulting in two counts at an auditor-year level, which we merge back to our primary dataset and use to calculate *Software_Hires* by dividing the software development-related counts by all job posting counts.

ONLINE APPENDIX 3

Using audit report lags as an alternative dependent variable proxying for audit efficiency

TABLE OA.1 Regression Analyses: Effect of Audit Firm Software on Audit Report Lags

Panel A: Effect of audit firm software scaled by number of employees

Variable	Model 1 DV = |DA| Coef. (T-stat)	Model 2 DV = |DA| Coef. (T-stat)	Model 3 DV = |DA| Coef. (T-stat)
Software_Intensity	**0.003** **(0.276)**	**0.012** **(0.456)**	**-0.030*** **(-1.912)**
Ln_Assets	-0.073*** (-8.800)	-0.073*** (-8.770)	0.006 (0.246)
Leverage	0.179* (1.756)	0.182* (1.860)	0.129** (2.141)
ΔSales	0.033 (1.198)	0.035 (1.294)	0.004 (0.238)
CFO	-0.155* (-1.702)	-0.163* (-1.715)	-0.066* (-1.665)
MTB	-0.011** (-2.071)	-0.010** (-2.077)	0.004 (1.531)
Altman_Z	-0.004 (-0.598)	-0.003 (-0.521)	0.002 (0.628)
Big4	-0.078** (-2.154)		
Industry_Expert	-0.009 (-0.456)	-0.013 (-0.652)	-0.005 (-0.379)
Auditor_Switch	0.051** (2.589)	0.056*** (2.823)	0.031* (1.807)
NAS_Ratio	0.006 (0.441)	0.009 (0.675)	-0.009 (-1.258)
ROA	0.113 (0.941)	0.110 (0.870)	-0.103 (-1.563)
Lag_Loss	0.017 (0.697)	0.014 (0.574)	-0.023 (-1.210)
|Lag_TACC|	0.229* (1.680)	0.223 (1.610)	-0.028 (-0.281)
Year Fixed Effects	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	No
Audit Firm Fixed Effects	No	Yes	Yes
Client Firm Fixed Effects	No	No	Yes
N	1,567	1,567	1,544
Adj. R-sq.	0.40	0.41	0.81

Panel B: Effect of audit firm software scaled by number of employees and new audit methodology			
Software_Intensity	0.002 (0.177)	0.013 (0.472)	-0.031* (-1.942)
New_Audit_Methodology	0.004 (0.214)	0.010 (0.679)	0.002 (0.235)
Software_Intensity* × *New_Audit_Methodology	**0.005 (0.370)**	**-0.001 (-0.086)**	**0.004 (0.544)**
Ln_Assets	-0.073*** (-8.765)	-0.073*** (-8.764)	0.006 (0.235)
Leverage	0.179* (1.758)	0.183* (1.866)	0.129** (2.111)
ΔSales	0.033 (1.180)	0.035 (1.310)	0.004 (0.219)
CFO	-0.156* (-1.709)	-0.163* (-1.714)	-0.066* (-1.667)
MTB	-0.011** (-2.069)	-0.011** (-2.098)	0.004 (1.530)
Altman_Z	-0.004 (-0.594)	-0.003 (-0.508)	0.002 (0.609)
Big4	-0.077** (-2.093)		
Industry_Expert	-0.009 (-0.443)	-0.013 (-0.651)	-0.005 (-0.378)
Auditor_Switch	0.052** (2.587)	0.056*** (2.832)	0.031* (1.818)
NAS_Ratio	0.006 (0.439)	0.009 (0.671)	-0.009 (-1.271)
ROA	0.113 (0.944)	0.110 (0.864)	-0.101 (-1.526)
Lag_Loss	0.017 (0.695)	0.014 (0.564)	-0.023 (-1.196)
|Lag_TACC|	0.230* (1.682)	0.222 (1.604)	-0.026 (-0.266)
Year Fixed Effects	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	No
Audit Firm Fixed Effects	No	Yes	Yes
Client Firm Fixed Effects	No	No	Yes
N	1,567	1,567	1,544
Adj. R-sq.	0.40	0.41	0.81

Panel C: Effect of audit firm software scaled by number of employees and auditor training			
Software_Intensity	-0.018 (-0.957)	-0.020 (-0.635)	-0.039 (-1.583)
Training_Costs	-0.019 (-1.180)	0.028 (0.785)	0.044 (1.385)
Software_Intensity* × *Training_Costs	**-0.006** **(-0.463)**	**-0.028** **(-1.060)**	**-0.053**** **(-2.340)**
Ln_Assets	-0.083*** (-10.067)	-0.083*** (-10.065)	-0.011 (-0.303)
Leverage	0.436*** (3.594)	0.436*** (3.553)	0.191 (1.483)
ΔSales	0.032 (0.833)	0.033 (0.839)	0.031 (1.568)
CFO	-0.048 (-0.457)	-0.051 (-0.488)	-0.081 (-1.190)
MTB	-0.022*** (-3.892)	-0.022*** (-3.845)	-0.003 (-0.553)
Altman_Z	0.010* (1.729)	0.010 (1.666)	0.008 (0.874)
Big4	-0.085* (-1.860)		
Industry_Expert	-0.017 (-0.614)	-0.018 (-0.759)	-0.015 (-0.649)
Auditor_Switch	0.060** (2.244)	0.060** (2.274)	0.054** (2.021)
NAS_Ratio	-0.012 (-0.770)	-0.012 (-0.747)	-0.026*** (-2.884)
ROA	-0.051 (-0.355)	-0.049 (-0.343)	-0.243** (-2.024)
Lag_Loss	-0.028 (-1.117)	-0.029 (-1.138)	-0.031 (-1.513)
|Lag_TACC|	0.124 (0.736)	0.123 (0.710)	0.010 (0.065)
Year Fixed Effects	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	No
Audit Firm Fixed Effects	No	Yes	Yes
Client Firm Fixed Effects	No	No	Yes
N	998	998	962
Adj. R-sq.	0.48	0.48	0.81

Notes: Panel A shows the coefficients and t-statistics for the analysis of the effects of total audit firm software levels scaled by audit firm employees (*Software_Intensity*) on the natural logarithm of audit report lags, which are the number of days from client fiscal year end to the auditor's signature on the audit report. Panel B shows results when adding an indicator variable for whether an audit firm reported the first-time firm-wide use of a new audit methodology (*New_Audit_Methodology*) and adding the interaction of that indicator variable with *Software_Intensity*. Panel C shows results when adding an audit firm's total expense for employee training scaled by audit firm employees (*Training_Costs*) and adding the interaction of that variable with *Software_Intensity*. Our variables of interest are all standardized to a mean of zero and standard deviation of one to facilitate interpretation of coefficients. All regression models include standard errors clustered two ways by client and audit office. ***, **, and * represent significance levels of 1%, 5%, and 10% levels (two-tailed), respectively. All variables are defined in Table A1 in the Appendix. Bold text indicates variables of interest.

Table OA.2: Stacked Difference-in-Difference Regression: Effect of Exceptional Audit Firm Software Investments on Audit Report Lags

	Model 1	Model 2
	All Events	Big4 Events Only
	Coef.	Coef.
Variable	(T-stat)	(T-stat)
Treat	-0.018	-0.022*
	(-1.446)	(-1.945)
Post	-0.005	-0.002
	(-1.639)	(-0.657)
Treat X Post	**0.018**	**0.012**
	(0.971)	**(0.878)**
Controls	Yes	Yes
Year Fixed Effects	Yes	Yes
Audit Firm Fixed Effects	Yes	Yes
Client Firm Fixed Effects	Yes	Yes
N	3,254	1,425
Adj. R-sq.	0.85	0.88

Notes: This table shows results from estimating a stacked difference-in-difference regression by estimating our primary model after replacing our dependent variable with the natural logarithm of audit report lags and our software measure with variables *Treat*, *Post* and their interaction. To build our sample of staggered software investment events and define variables *Treat* and *Post*, we identify all audit firm years with software investments, scaled by employees, that are above the sample median and at least 50% higher than the audit firm's own median. This procedure yields five events (EY 2018, KPMG 2015, Mazars 2012, Rödl&Partner 2012, Rödl&Partner 2013) for which we have at least one pre- and one post-period. For each event, we then include the year before and year of the investment as our pre periods (*Post* = 0) and the two years after the investment as our post period (*Post* = 1). We retain clients of all audit firms, define *Treat* = 1 for clients of the event firm (e.g., EY client in 2018), and *Treat* = 0 otherwise. If a non-event audit firm also has an event in the four-year window, we remove all observations of their clients after the event year (e.g., we remove Rödl&Partner clients after 2012 when our event is Mazars 2012). We finally pool the subsamples of all events to build our stacked regression sample. Model 1 shows the coefficients and t-statistics for the difference-in-difference with absolute discretionary accruals as the dependent variable for the full sample. Model 2 shows results for a subsample stacking only the Big4 events (EY 2018, KPMG 2015). All regression models include standard errors clustered two ways by client and audit office. ***, **, and * indicate significance at 1, 5, and 10 percent levels (two-tailed). Bold text indicates variables of interest.

Using audit fees as an alternative dependent variable proxying for audit efficiency

TABLE OA.3 Regression Analyses: Effect of Audit Firm Software on Audit Fees

Panel A: Effect of audit firm software scaled by number of employees

Variable	Model 1 DV = |DA| Coef. (T-stat)	Model 2 DV = |DA| Coef. (T-stat)	Model 3 DV = |DA| Coef. (T-stat)
Software_Intensity	**-0.013** **(-0.566)**	**-0.035** **(-0.920)**	**-0.023** **(-0.603)**
Ln_Assets	0.561*** (32.446)	0.563*** (32.577)	0.362*** (8.435)
Leverage	0.279** (2.407)	0.287** (2.534)	0.110 (0.881)
ΔSales	-0.069 (-1.159)	-0.049 (-0.841)	0.042 (0.943)
CFO	-0.451*** (-3.046)	-0.436*** (-2.959)	-0.205* (-1.784)
MTB	0.016** (2.152)	0.016** (2.232)	0.007** (2.039)
Altman_Z	-0.015 (-1.534)	-0.015 (-1.538)	-0.004 (-0.709)
Big4	-0.031 (-0.522)		
Industry_Expert	0.229*** (3.914)	0.210*** (3.306)	0.135** (2.222)
Auditor_Switch	-0.212*** (-4.363)	-0.217*** (-4.296)	-0.179*** (-4.864)
NAS_Ratio	-0.129*** (-3.126)	-0.129*** (-3.158)	-0.138*** (-3.424)
ROA	-0.285* (-1.711)	-0.259 (-1.529)	-0.131 (-1.103)
Lag_Loss	0.161*** (3.198)	0.165*** (3.225)	0.032 (1.229)
|Lag_TACC|	-0.047 (-0.181)	-0.053 (-0.204)	-0.100 (-0.567)
Year Fixed Effects	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	No
Audit Firm Fixed Effects	No	Yes	Yes
Client Firm Fixed Effects	No	No	Yes
N	2,112	2,112	2,074
Adj. R-sq.	0.84	0.84	0.93

Panel B: Effect of audit firm software scaled by number of employees and new audit methodology			
Software_Intensity	-0.013 (-0.560)	-0.032 (-0.790)	-0.015 (-0.394)
New_Audit_Methodology	-0.033 (-1.259)	0.005 (0.246)	0.019 (0.863)
Software_Intensity **×** ***New_Audit_Methodology***	**0.005** **(0.190)**	**-0.010** **(-0.453)**	**-0.026** **(-1.405)**
Ln_Assets	0.562*** (32.462)	0.563*** (32.549)	0.363*** (8.455)
Leverage	0.278** (2.387)	0.287** (2.531)	0.110 (0.871)
ΔSales	-0.069 (-1.157)	-0.048 (-0.822)	0.045 (0.983)
CFO	-0.452*** (-3.050)	-0.435*** (-2.948)	-0.199* (-1.704)
MTB	0.016** (2.155)	0.016** (2.226)	0.007** (2.037)
Altman_Z	-0.015 (-1.548)	-0.015 (-1.532)	-0.004 (-0.673)
Big4	-0.032 (-0.549)		
Industry_Expert	0.229*** (3.913)	0.210*** (3.308)	0.134** (2.224)
Auditor_Switch	-0.214*** (-4.422)	-0.217*** (-4.314)	-0.179*** (-4.901)
NAS_Ratio	-0.130*** (-3.151)	-0.129*** (-3.165)	-0.138*** (-3.430)
ROA	-0.284* (-1.692)	-0.261 (-1.523)	-0.143 (-1.224)
Lag_Loss	0.161*** (3.185)	0.165*** (3.213)	0.030 (1.179)
|Lag_TACC|	-0.044 (-0.168)	-0.054 (-0.207)	-0.106 (-0.600)
Year Fixed Effects	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	No
Audit Firm Fixed Effects	No	Yes	Yes
Client Firm Fixed Effects	No	No	Yes
N	2,112	2,112	2,074
Adj. R-sq.	0.84	0.84	0.93

Panel C: Effect of audit firm software scaled by number of employees and auditor training			
Software_Intensity	0.053	0.003	0.024
	(1.038)	(0.035)	(0.332)
Training_Costs	-0.071	-0.030	-0.022
	(-1.531)	(-0.480)	(-0.385)
Software_Intensity* × *Training_Costs	**0.052**	**0.029**	**0.063***
	(1.650)	**(0.693)**	**(1.975)**
Ln_Assets	0.581***	0.581***	0.249***
	(23.334)	(23.326)	(3.478)
Leverage	0.302**	0.299*	0.232
	(2.017)	(1.990)	(1.113)
ΔSales	-0.216***	-0.213***	0.060
	(-3.472)	(-3.446)	(1.178)
CFO	-0.706***	-0.700***	-0.073
	(-3.516)	(-3.541)	(-0.422)
MTB	0.012	0.012	0.008
	(0.980)	(0.989)	(1.005)
Altman_Z	-0.002	-0.002	-0.001
	(-0.184)	(-0.231)	(-0.082)
Big4	0.105		
	(0.748)		
Industry_Expert	0.103	0.103	0.078
	(1.543)	(1.439)	(1.197)
Auditor_Switch	-0.206***	-0.206***	-0.152***
	(-3.743)	(-3.777)	(-3.922)
NAS_Ratio	-0.120**	-0.121**	-0.161***
	(-2.296)	(-2.310)	(-2.684)
ROA	-0.136	-0.141	-0.162
	(-0.453)	(-0.472)	(-0.800)
Lag_Loss	0.187***	0.189***	0.028
	(2.853)	(2.917)	(0.921)
|Lag_TACC|	-0.212	-0.232	-0.155
	(-0.649)	(-0.703)	(-0.739)
Year Fixed Effects	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	No
Audit Firm Fixed Effects	No	Yes	Yes
Client Firm Fixed Effects	No	No	Yes
N	1,335	1,335	1,288
Adj. R-sq.	0.85	0.85	0.94

Notes: Panel A shows the coefficients and t-statistics for the analysis of the effects of total audit firm software levels scaled by audit firm employees (*Software_Intensity*) on the natural logarithm of audit fees. Panel B shows results when adding an indicator variable for whether an audit firm reported the first-time firm-wide use of a new audit methodology (*New_Audit_Methodology*) and adding the interaction of that indicator variable with *Software_Intensity*. Panel C shows results when adding an audit firm's total expense for employee training scaled by audit firm employees (*Training_Costs*) and adding the interaction of that variable with *Software_Intensity*.

Our variables of interest are all standardized to a mean of zero and standard deviation of one to facilitate interpretation of coefficients. All regression models include standard errors clustered two ways by client and audit office. ***, **, and * represent significance levels of 1%, 5%, and 10% levels (two-tailed), respectively. All variables are defined in Table A1 in the Appendix. Bold text indicates variables of interest.

Table OA.4: Stacked Difference-in-Difference Regression: Effect of Exceptional Audit Firm Software Investments on Audit Fees

	Model 1 All Events	Model 2 Big4 Events Only
Variable	Coef. (T-stat)	Coef. (T-stat)
Treat	-0.025 (-1.037)	-0.016 (-0.518)
Post	-0.002 (-0.309)	0.014 (0.799)
Treat X Post	**-0.042** **(-0.783)**	**-0.057*** **(-0.899)**
Controls	Yes	Yes
Year Fixed Effects	Yes	Yes
Audit Firm Fixed Effects	Yes	Yes
Client Firm Fixed Effects	Yes	Yes
N	4,346	1,677
Adj. R-sq.	0.95	0.94

Notes: This table shows results from estimating a stacked difference-in-difference regression by estimating our primary model after replacing our dependent variable with the natural logarithm of audit fees and our software measure with variables *Treat*, *Post* and their interaction. To build our sample of staggered software investment events and define variables *Treat* and *Post*, we identify all audit firm years with software investments, scaled by employees, that are above the sample median and at least 50% higher than the audit firm's own median. This procedure yields five events (EY 2018, KPMG 2015, Mazars 2012, Rödl&Partner 2012, Rödl&Partner 2013) for which we have at least one pre- and one post-period. For each event, we then include the year before and year of the investment as our pre periods (*Post* = 0) and the two years after the investment as our post period (*Post* = 1). We retain clients of all audit firms, define *Treat* = 1 for clients of the event firm (e.g., EY client in 2018), and *Treat* = 0 otherwise. If a non-event audit firm also has an event in the four-year window, we remove all observations of their clients after the event year (e.g., we remove Rödl&Partner clients after 2012 when our event is Mazars 2012). We finally pool the subsamples of all events to build our stacked regression sample. Model 1 shows the coefficients and t-statistics for the difference-in-difference with absolute discretionary accruals as the dependent variable for the full sample. Model 2 shows results for a subsample stacking only the Big4 events (EY 2018, KPMG 2015). All regression models include standard errors clustered two ways by client and audit office. ***, **, and * indicate significance at 1, 5, and 10 percent levels (two-tailed). Bold text indicates variables of interest.

TABLE 1 Sample Composition

Panel A: Sample Selection

	(Less)	Firm-year observations
Total observations from operating firms (positive Total Assets and Revenue) with data on consolidated IFRS statements from Thomson Reuters Eikon and Bureau Van Dijk Dafne 2011-2019		3,958
Less financial industry firm-years	(-595)	3,363
Less firms with insufficient data for accrual estimation	(-232)	3,131
Less firms with no information on their auditor	(-50)	3,081
Less firms with non-top10 auditor missing audit firm level software data	(-864)	2,217
Less firms with insufficient data for regression analyses	(-105)	2,112
Final Sample		**2,112**

Panel B: Industry Composition

Industry	N	Percentage	Industry	N	Percentage
Auto	136	6.44 %	*Media*	121	5.73 %
Chemicals	122	5.78 %	*Pharma*	227	10.75 %
Construction	21	0.99 %	*Retail*	154	7.29 %
Consumer Goods	193	9.14 %	*Software*	212	10.04 %
Food	27	1.28 %	*Technology*	199	9.42 %
Industrial Goods	481	22.77 %	*Telecom*	81	3.84 %
Logistics	89	4.21 %	*Utilities*	49	2.32 %

Panel C: Observations Across Years

2011	2012	2013	2014	2015	2016	2017	2018	2019
214	236	220	222	226	220	257	263	254

TABLE 2 Descriptive Statistics

	N	Mean	Median	S.D.	Min	Max
|DA|	2,112	0.05	0.03	0.05	0.00	0.27
Software_Intensity	2,112	3.17	3.67	1.86	0.18	8.28
New_Audit_Methodology	2,112	0.14	0.00	0.35	0.00	1.00
Training_Costs	1,335	10.56	9.83	2.97	0.98	14.52
Ln_Assets	2,112	13.35	13.01	2.25	9.17	18.89
Leverage	2,112	0.57	0.57	0.22	0.10	1.39
ΔSales	2,112	0.06	0.04	0.24	-0.61	1.49
CFO	2,112	0.07	0.08	0.12	-0.52	0.38
MTB	2,112	2.48	1.85	3.01	-9.47	15.85
Altman_Z	2,112	3.94	3.23	3.34	-3.49	21.02
Big4	2,112	0.82	1.00	0.38	0.00	1.00
Industry_Expert	2,112	0.35	0.00	0.48	0.00	1.00
Auditor_Switch	2,112	0.07	0.00	0.26	0.00	1.00
NAS_Ratio	2,112	0.44	0.25	0.58	0.00	3.17
ROA	2,112	0.02	0.04	0.12	-0.58	0.28
Lag_Loss	2,112	0.21	0.00	0.41	0.00	1.00
|Lag_TACC|	2,112	0.07	0.05	0.06	0.00	0.36

Notes: All continuous variables are winsorized at the 1 and 99 percent level. All variables are defined in Table A1 in the Appendix.

TABLE 3 Correlation Matrix

		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
1	*|DA|*																
2	*Software_Intensity*	**-0.07**															
3	*New_Audit_Methodology*	-0.03	**0.05**														
4	*Training_Costs*	**-0.11**	**-0.06**	0.02													
5	*Ln_Assets*	**-0.24**	**0.08**	-0.01	**0.20**												
6	*Leverage*	-0.01	0.01	-0.03	**0.06**	**0.19**											
7	*ΔSales*	**0.06**	-0.02	0.03	-0.04	**-0.06**	**-0.10**										
8	*CFO*	**-0.17**	**0.10**	-0.02	**0.05**	**0.16**	**-0.10**	**-0.04**									
9	*MTB*	0.02	0.00	0.01	0.02	-0.04	**-0.15**	**0.11**	**0.23**								
10	*Altman_Z*	0.01	0.01	0.00	**0.06**	**-0.10**	**-0.54**	**0.13**	**0.24**	**0.44**							
11	*Big4*	**-0.14**	**-0.19**	**-0.06**	**0.61**	**0.35**	**0.11**	**-0.05**	**0.13**	0.02	0.00						
12	*Industry_Expert*	**-0.09**	**0.10**	-0.03	**0.09**	**0.36**	**0.04**	-0.01	**0.11**	0.02	-0.02	**0.34**					
13	*Auditor_Switch*	**0.05**	-0.03	-0.02	**0.05**	**-0.08**	0.03	-0.03	0.00	0.00	-0.01	-0.03	**-0.05**				
14	*NAS_Ratio*	0.02	**-0.09**	**-0.05**	**0.05**	**0.16**	**0.06**	0.00	-0.04	**-0.05**	**-0.07**	**0.13**	**0.05**	**-0.10**			
15	*ROA*	**-0.11**	**0.11**	0.00	0.02	**0.20**	**-0.24**	**-0.06**	**0.64**	**0.21**	**0.33**	**0.05**	**0.09**	-0.02	**-0.05**		
16	*Lag_Loss*	**0.20**	**-0.09**	-0.02	-0.03	**-0.23**	**0.20**	0.00	**-0.44**	**-0.09**	**-0.19**	**-0.06**	**-0.10**	0.03	0.02	**-0.67**	
17	*|Lag_TACC|*	**0.25**	-0.03	0.01	**-0.06**	**-0.23**	**0.09**	**0.05**	**-0.07**	0.03	**-0.05**	**-0.08**	**-0.06**	0.02	0.04	**-0.25**	**0.29**

Notes: All continuous variables are winsorized at the 1 and 99 percent level. All variables are defined in Table A1 in the Appendix. Bold values represent significance levels of 5%.

TABLE 4 Regression Analyses: Effect of Audit Firm Software on Discretionary Accruals

Panel A: Effect of audit firm software scaled by number of employees

Variable	Model 1 DV = |DA| Coef. (T-stat)	Model 2 DV = |DA| Coef. (T-stat)	Model 3 DV = |DA| Coef. (T-stat)
Software_Intensity	**-0.002*** **(-1.725)**	**-0.007***** **(-2.469)**	**-0.011***** **(-3.848)**
Ln_Assets	-0.003*** (-4.058)	-0.003*** (-4.148)	-0.009* (-1.789)
Leverage	0.000 (-0.020)	0.000 (0.001)	0.013 (0.898)
ΔSales	0.009* (1.676)	0.009 (1.620)	0.009* (1.772)
CFO	-0.085*** (-3.335)	-0.085*** (-3.316)	-0.054 (-1.555)
MTB	0.000 (0.293)	0.000 (0.329)	0.001* (1.709)
Altman_Z	0.000 (0.453)	0.000 (0.525)	-0.000 (-0.204)
Big4	-0.008*** (-2.713)		
Industry_Expert	0.003 (1.301)	0.002 (0.956)	-0.002 (-0.630)
Auditor_Switch	0.008** (2.195)	0.008** (2.132)	0.004 (1.033)
NAS_Ratio	0.003* (1.712)	0.003* (1.728)	0.004* (1.819)
ROA	0.080*** (4.075)	0.081*** (4.013)	0.026 (1.596)
Lag_Loss	0.018*** (3.576)	0.019*** (3.667)	0.008* (1.871)
|Lag_TACC|	0.121*** (3.841)	0.121*** (3.779)	-0.048** (-2.068)
Year Fixed Effects	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	No
Audit Firm Fixed Effects	No	Yes	Yes
Client Firm Fixed Effects	No	No	Yes
N	2,112	2,112	2,074
Adj. R-sq.	0.17	0.17	0.32

Panel B: Effect of audit firm software scaled by number of employees and new audit methodology			
Software_Intensity	-0.003*** (-2.602)	-0.009*** (-3.168)	-0.013*** (-4.414)
New_Audit_Methodology	-0.006** (-2.151)	-0.007** (-2.379)	-0.006** (-2.081)
Software_Intensity × New_Audit_Methodology	**0.006*** (3.090)**	**0.007*** (3.633)**	**0.006** (2.437)**
Ln_Assets	-0.003*** (-4.054)	-0.003*** (-4.212)	-0.009* (-1.841)
Leverage	0.000 (-0.028)	0.000 (-0.010)	0.013 (0.907)
ΔSales	0.009 (1.629)	0.008 (1.538)	0.009* (1.674)
CFO	-0.086*** (-3.372)	-0.086*** (-3.353)	-0.056 (-1.598)
MTB	0.000 (0.332)	0.000 (0.401)	0.001* (1.762)
Altman_Z	0.000 (0.404)	0.000 (0.459)	-0.000 (-0.256)
Big4	-0.008*** (-2.609)		
Industry_Expert	0.003 (1.296)	0.002 (1.056)	-0.002 (-0.579)
Auditor_Switch	0.008** (2.167)	0.008** (2.124)	0.004 (1.015)
NAS_Ratio	0.003* (1.682)	0.003* (1.681)	0.004* (1.788)
ROA	0.081*** (4.193)	0.082*** (4.179)	0.029* (1.765)
Lag_Loss	0.018*** (3.586)	0.019*** (3.718)	0.008* (1.952)
|Lag_TACC|	0.122*** (3.922)	0.121*** (3.876)	-0.047** (-2.015)
Year Fixed Effects	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	No
Audit Firm Fixed Effects	No	Yes	Yes
Client Firm Fixed Effects	No	No	Yes
N	2,112	2,112	2,074
Adj. R-sq.	0.17	0.17	0.32

Panel C: Effect of audit firm software scaled by number of employees and auditor training			
Software_Intensity	-0.008**	-0.016***	-0.016***
	(-2.229)	(-2.581)	(-2.879)
Training_Costs	0.002	0.009*	0.008
	(1.040)	(1.665)	(1.564)
Software_Intensity* × *Training_Costs	**-0.005****	**-0.006****	**-0.005***
	(-2.348)	**(-2.263)**	**(-1.875)**
Ln_Assets	-0.003***	-0.003***	-0.008
	(-5.513)	(-5.447)	(-1.153)
Leverage	-0.007	-0.007	-0.045*
	(-0.907)	(-0.969)	(-1.804)
ΔSales	0.007	0.007	0.002
	(1.021)	(1.070)	(0.247)
CFO	-0.037	-0.037	0.014
	(-1.211)	(-1.188)	(0.288)
MTB	0.000	0.000	0.003***
	(0.702)	(0.747)	(3.597)
Altman_Z	-0.001	-0.001	-0.004**
	(-0.985)	(-1.097)	(-2.303)
Big4	-0.012**		
	(-2.173)		
Industry_Expert	0.003	0.002	0.001
	(0.989)	(0.817)	(0.374)
Auditor_Switch	0.013**	0.013**	0.007
	(2.529)	(2.512)	(1.200)
NAS_Ratio	0.007***	0.007***	0.006*
	(2.845)	(2.894)	(1.844)
ROA	0.075**	0.075**	0.052
	(2.566)	(2.561)	(1.415)
Lag_Loss	0.019***	0.019***	0.013**
	(3.602)	(3.604)	(2.073)
|Lag_TACC|	0.093***	0.091***	-0.052*
	(3.285)	(3.209)	(-1.771)
Year Fixed Effects	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	No
Audit Firm Fixed Effects	No	Yes	Yes
Client Firm Fixed Effects	No	No	Yes
N	1,335	1,335	1,288
Adj. R-sq.	0.17	0.17	0.26

Notes: Panel A shows the coefficients and t-statistics for the analysis of the effects of total audit firm software levels scaled by audit firm employees (*Software_Intensity*) on discretionary accruals. Panel B shows results when adding an indicator variable for whether an audit firm reported the first-time firm-wide use of a new audit methodology (*New_Audit_Methodology*) and adding the interaction of that indicator variable with *Software_Intensity*. Panel C shows results when adding an audit firm's total expense for employee training scaled by audit firm employees (*Training_Costs*) and adding the interaction of that variable with *Software_Intensity*. Our variables of interest are all standardized to a mean of zero and standard deviation of one to facilitate interpretation of coefficients. All regression models include standard errors clustered two ways by client and audit office. ***, **, and * represent significance levels of 1%, 5%, and 10% levels (two-tailed), respectively. All variables are defined in Table A1 in the Appendix. Bold text indicates variables of interest.

TABLE 5 Regression Analyses: Effect of Audit Firm Software and Audit Firm's Software-Focused Job Postings on Discretionary Accruals

	Model 1	Model 2
	DV = |DA|	DV = |DA|
Variable	Coef. (T-stat)	Coef. (T-stat)
Software_Intensity	**-0.004**** **(-2.385)**	**-0.003*** **(-1.961)**
Software_Hires	**-0.003*** **(-1.864)**	**-0.002** **(-1.488)**
Software_Intensity × Software_Hires		0.001 (0.916)
Ln_Assets	-0.002*** (-2.935)	-0.002*** (-2.906)
Leverage	0.004 (0.404)	0.004 (0.416)
ΔSales	0.006 (0.859)	0.006 (0.872)
CFO	-0.088*** (-3.000)	-0.087*** (-2.934)
MTB	0.000 (0.125)	0.000 (0.050)
Altman_Z	0.000 (0.616)	0.000 (0.656)
Big4	-0.015* (-1.669)	-0.018* (-1.821)
Industry_Expert	0.004 (1.217)	0.004 (1.101)
Auditor_Switch	-0.002 (-0.327)	-0.002 (-0.341)
NAS_Ratio	0.001 (0.573)	0.001 (0.573)
ROA	0.073*** (3.743)	0.072*** (3.703)
Lag_Loss	0.018** (2.530)	0.018** (2.521)
|Lag_TACC|	0.105** (2.083)	0.104** (2.072)
Year Fixed Effects	Yes	Yes
Industry Fixed Effects	Yes	Yes
N	997	997
Adj. R-sq.	0.17	0.17

Notes: This shows the coefficients and t-statistics for the analysis of the effects of total audit firm software levels scaled by audit firm employees (*Software_Intensity*), audit firm software-focused job postings (*Software_Hiring*), and their interaction (Model 2) on discretionary accruals. *Software_Hiring* is the ratio of software-focused job postings to all job postings in an audit firm's financial year, where we collect job postings from LinkUp and Revelio Labs and classify job postings as software-focused when the job title contains combinations of keywords indicating a software development role (excluding pure software-related client consulting). Our variables of interest are all standardized to a mean of zero and standard deviation of one to facilitate interpretation of coefficients. All regression models include standard errors clustered two ways by client and audit office. ***, **, and * represent significance levels of 1%, 5%, and 10% levels (two-tailed), respectively. All other variables are defined in Table A1 in the Appendix. Bold text indicates variables of interest.

TABLE 6 Stacked Difference-in-Difference Regression: Effect of Exceptional Audit Firm Software Investments on Discretionary Accruals

Variable	Model 1 All Events Coef. (T-stat)	Model 2 Big4 Events Only Coef. (T-stat)
Treat	-0.000 (-0.153)	-0.003 (-0.707)
Post	0.000 (0.799)	0.002 (1.342)
Treat X Post	**-0.006*** **(-1.742)**	**-0.006*** **(-1.766)**
Controls	Yes	Yes
Year Fixed Effects	Yes	Yes
Audit Firm Fixed Effects	Yes	Yes
Client Firm Fixed Effects	Yes	Yes
N	4,346	1,677
Adj. R-sq.	0.42	0.36

Notes: This table shows results from estimating a stacked difference-in-difference regression by estimating our primary model after replacing our software measure with variables *Treat*, *Post* and their interaction. To build our sample of staggered software investment events and define variables *Treat* and *Post*, we identify all audit firm years with software investments, scaled by employees, that are above the sample median and at least 50% higher than the audit firm's own median. This procedure yields five events (EY 2018, KPMG 2015, Mazars 2012, Rödl&Partner 2012, Rödl&Partner 2013) for which we have at least one pre- and one post-period. For each event, we then include the year before and year of the investment as our pre periods (*Post* = 0) and the two years after the investment as our post period (*Post* = 1). We retain clients of all audit firms, define *Treat* = 1 for clients of the event firm (e.g., EY client in 2018), and *Treat* = 0 otherwise. If a non-event audit firm also has an event in the four-year window, we remove all observations of their clients after the event year (e.g., we remove Rödl&Partner clients after 2012 when our event is Mazars 2012). We finally pool the subsamples of all events to build our stacked regression sample. Model 1 shows the coefficients and t-statistics for the difference-in-difference with absolute discretionary accruals as the dependent variable for the full sample. Model 2 shows results for a subsample stacking only the Big4 events (EY 2018, KPMG 2015). All regression models include standard errors clustered two ways by client and audit office. ***, **, and * indicate significance at 1, 5, and 10 percent levels (two-tailed). Bold text indicates variables of interest.

TABLE 7 Regression Analyses: Alternative Test Variables and their Effect on Discretionary Accruals

Panel A: Effect of book value of audit firm software scaled by number of employees			
	Model 1 DV = |DA|	Model 2 DV = |DA|	Model 3 DV = |DA|
Variable	Coef. (T-stat)	Coef. (T-stat)	Coef. (T-stat)
Book_Value_Software_Intensity	**-0.002****** **(-2.767)**	**-0.003****** **(-3.019)**	**-0.004****** **(-3.191)**
Controls	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	No
Audit Firm Fixed Effects	No	Yes	Yes
Client Firm Fixed Effects	No	No	Yes
N	2,112	2,112	2,074
Adj. R-sq.	0.17	0.17	0.32

Panel B: Effect of audit firm software scaled by number of employees and audit firms acquiring IT specialists			
Software_Intensity	-0.003** (-2.451)	-0.007*** (-2.811)	-0.012*** (-4.447)
IT_Acquisition	0.002 (0.591)	0.000 (0.089)	-0.001 (-0.359)
Software_Intensity* × *IT_Acquisition	**0.004*** **(1.895)**	**0.004*** **(1.730)**	**0.004** **(1.627)**
Controls	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	No
Audit Firm Fixed Effects	No	Yes	Yes
Client Firm Fixed Effects	No	No	Yes
N	2,112	2,112	2,074
Adj. R-sq.	0.17	0.17	0.32

Panel C: Effect of audit firm software scaled by number of employees and audit firm wage levels			
Software_Intensity	-0.002 (-1.629)	-0.011*** (-2.791)	-0.016*** (-3.811)
Average_Wage	-0.000 (-0.071)	0.003 (0.556)	0.003 (0.568)
Software_Intensity* × *Average_Wage	**0.000** **(0.025)**	**-0.003*** **(-1.780)**	**-0.005*** **(-1.901)**
Controls	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	No
Audit Firm Fixed Effects	No	Yes	Yes

Client Firm Fixed Effects	No	No	Yes
N	2,112	2,112	2,074
Adj. R-sq.	0.17	0.17	0.32

Notes: Panel A shows the coefficients and t-statistics for the analysis of the effects of total book values of audit firm software scaled by audit firm employees (*Book_Value_Software_Intensity*) on discretionary accruals. Panel B shows results of *Software_Intensity* (see Table 4), an indicator variable for whether an audit firm acquired an IT specialist company (*IT_Acquisition*), and the interaction of those two variables. Panel C shows results of *Software_Intensity*, an audit firm's total personnel expense scaled by audit firm employees (*Average_Wage*), and the interaction of those two variables. Results for control variables are suppressed for brevity and are qualitatively similar to Panel Table 4. Our variables of interest are all standardized to a mean of zero and standard deviation of one to facilitate interpretation of coefficients. All regression models include standard errors clustered two ways by client and audit office. ***, **, and * represent significance levels of 1%, 5%, and 10% levels (two-tailed), respectively. All variables are defined in Table A1 in the Appendix. Bold text indicates variables of interest.

TABLE 8 Regression Analyses: Effect of Audit Firm Software on Restatements

Panel A: Effect of audit firm software scaled by number of employees

Variable	Model 1 DV = Restatement Coef. (T-stat)	Model 2 DV = Restatement Coef. (T-stat)	Model 3 DV = Restatement Coef. (T-stat)
Software_Intensity	**-0.004** **(-1.211)**	**-0.025**** **(-2.167)**	**-0.014** **(-1.031)**
Controls	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	No
Audit Firm Fixed Effects	No	Yes	Yes
Client Firm Fixed Effects	No	No	Yes
N	2,112	2,112	2,074
Adj. R-sq.	0.004	0.007	0.133

Panel B: Effect of audit firm software scaled by number of employees and new audit methodology

Variable	Model 1	Model 2	Model 3
Software_Aud_Rev	-0.003 (-0.893)	-0.025** (-2.168)	-0.013 (-0.943)
New_Audit_Methodology	-0.016** (-2.437)	-0.015* (-1.793)	-0.009 (-1.023)
Software_Intensity* × *New_Audit_Methodology	**-0.003** **(-0.688)**	**0.000** **(0.043)**	**-0.004** **(-0.566)**
Controls	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	No
Audit Firm Fixed Effects	No	Yes	Yes
Client Firm Fixed Effects	No	No	Yes
N	2,112	2,112	2,074
Adj. R-sq.	0.005	0.007	0.132

Panel C: Effect of audit firm software scaled by number of employees and auditor training

Variable	Model 1	Model 2	Model 3
Software_Aud_Rev	-0.018 (-1.404)	-0.039 (-1.539)	-0.025 (-1.136)
Training_Costs	-0.001 (-0.179)	-0.024 (-1.397)	-0.009 (-0.603)
Software_Intensity* × *Training_Costs	**-0.010** **(-1.148)**	**-0.010** **(-0.676)**	**-0.008** **(-0.667)**
Controls	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	No
Audit Firm Fixed Effects	No	Yes	Yes
Client Firm Fixed Effects	No	No	Yes

N	1,335	1,335	1,288
Adj. R-sq.	0.013	0.017	0.26

Notes: Panel A shows the coefficients and t-statistics for the analysis of the effects of total audit firm software levels scaled by audit firm employees (*Software_Intensity*) on restatements. Restatement is a dummy variable coded as 1 if a company issues a correction of a previous annual statutory filing in the German Federal Gazette (Bundesanzeiger), where firms have to disclose their annual statutory filings, and 0 otherwise (see Online Appendix 1 for details). We estimate all models with linear probability models to allow for the inclusion of high-dimensional fixed effects and to facilitate interpretation of the interaction terms. Panel B shows results when adding an indicator variable for whether an audit firm reported the first-time firm-wide use of a new audit methodology (*New_Audit_Methodology*) and adding the interaction of that indicator variable with *Software_Intensity*. Panel C shows results when adding an audit firm's total expense for employee training scaled by audit firm employees (*Training_Costs*) and adding the interaction of that variable with *Software_Intensity*. Our variables of interest are all standardized to a mean of zero and standard deviation of one to facilitate interpretation of coefficients. All regression models include standard errors clustered two ways by client and audit office. ***, **, and * represent significance levels of 1%, 5%, and 10% levels (two-tailed), respectively. For brevity, we suppress control variables (same as in equation (2)). All other variables are defined in Table A1 in the Appendix. Bold text indicates variables of interest.

TABLE 9 Cross-Sectional Analyses: Effect of Audit Firm Software for Different Subsamples

Panel A: Median split by software age

Variable	Below-median *Young_Software* Coef. (T-stat)	Above-median *Young_Software* Coef. (T-stat)
Software_Intensity	**0.005** (**0.698**)	**-0.011***** (**-2.848**)
Controls	Yes	Yes
Year Fixed Effects	Yes	Yes
Industry Fixed Effects	Yes	Yes
Audit Firm Fixed Effects	Yes	Yes
N	1,055	1,036
Adj. R-sq.	0.15	0.20

Panel B: Median split by audit firm non-audit service revenue

Variable	Below-median *NAS_Revenue* Coef. (T-stat)	Above-median *NAS_Revenue* Coef. (T-stat)
Software_Intensity	**-0.004** (**-0.869**)	**-0.016***** (**-2.778**)
Controls	Yes	Yes
Year Fixed Effects	Yes	Yes
Industry Fixed Effects	Yes	Yes
Audit Firm Fixed Effects	Yes	Yes
N	1,026	1,016
Adj. R-sq.	0.20	0.14

Panel C: Median split by non-audit service fees of audit clients

Variable	Below-median *NAS_Ratio* Coef. (T-stat)	Above-median *NAS_Ratio* Coef. (T-stat)
Software_Intensity	**-0.010***** (**-2.680**)	**-0.002** (**-0.317**)
Controls	Yes	Yes
Year Fixed Effects	Yes	Yes
Industry Fixed Effects	Yes	Yes
Audit Firm Fixed Effects	Yes	Yes
N	1,051	1,051
Adj. R-sq.	0.16	0.19

Panel D: Split by whether clients belong to a complex industry

Variable	Non-complex industry Coef. (T-stat)	Complex industry Coef. (T-stat)
Software_Intensity	**-0.005** **(-1.495)**	**-0.009**** **(-2.433)**
Controls	Yes	Yes
Year Fixed Effects	Yes	Yes
Industry Fixed Effects	Yes	Yes
Audit Firm Fixed Effects	Yes	Yes
N	1,433	679
Adj. R-sq.	0.20	0.17

Notes: This table shows the coefficients and t-statistics for the analysis of the effects of total audit firm software levels scaled by audit firm employees on discretionary accruals for different subsample. In Panel A, we split the sample by median software age, where *Young_Software* is the ratio of the book value of total audit firm software to the historical acquisition cost of total audit firm software. Due to depreciation mechanics, higher values represent younger software. The first (second) column contains all observations below (above) the median value of 0.198, i.e., older (younger) software. In Panel B, we split the sample by median non-audit service revenues, where *NAS_Revenue* is the ratio an audit firm's annual revenue from non-audit services to its total annual revenue. The first (second) column contains all observations below (above) the median value of 0.590, i.e., audit firm years with less (more) relative revenue from non-audit services than the median audit firm year. In Panel C, we split the sample by median non-audit service fees received from audit clients, where *NAS_Ratio* is the ratio of a client's non-audit service fees paid to the statutory auditor to audit fees. The first (second) column contains all observations below (above) the median value of 0.250, i.e., audit firm years with less (more) relative non-audit services from their statutory auditor. In Panel D, we split the sample by whether clients belong to one of the following industries, mapping the complex industry definition in Bills et al. (2015) to our industry classification: "construction", "pharma & healthcare", "software", "telecommunications", "logistics", "utilities". Results for control variables are suppressed for brevity and are qualitatively similar to Panel A of Table 4. All regression models include standard errors clustered two ways by client and audit firm. ***, **, and * represent significance levels of 1%, 5%, and 10% levels (two-tailed), respectively. All variables are defined in Table A1 in the Appendix. Bold text indicates variables of interest.

FIGURE 1 Panel A Yearly mean of *Software_Intensity*.

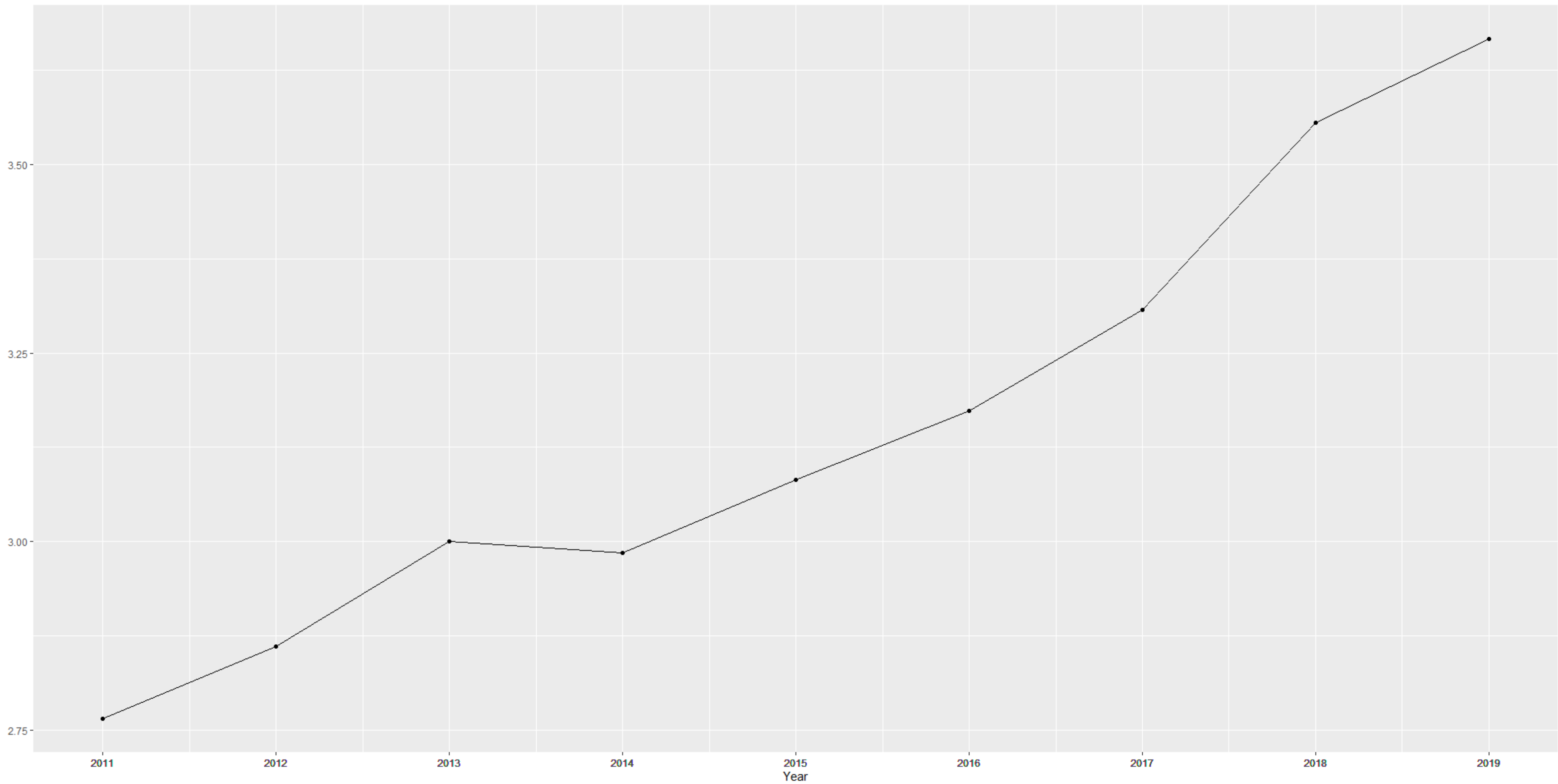
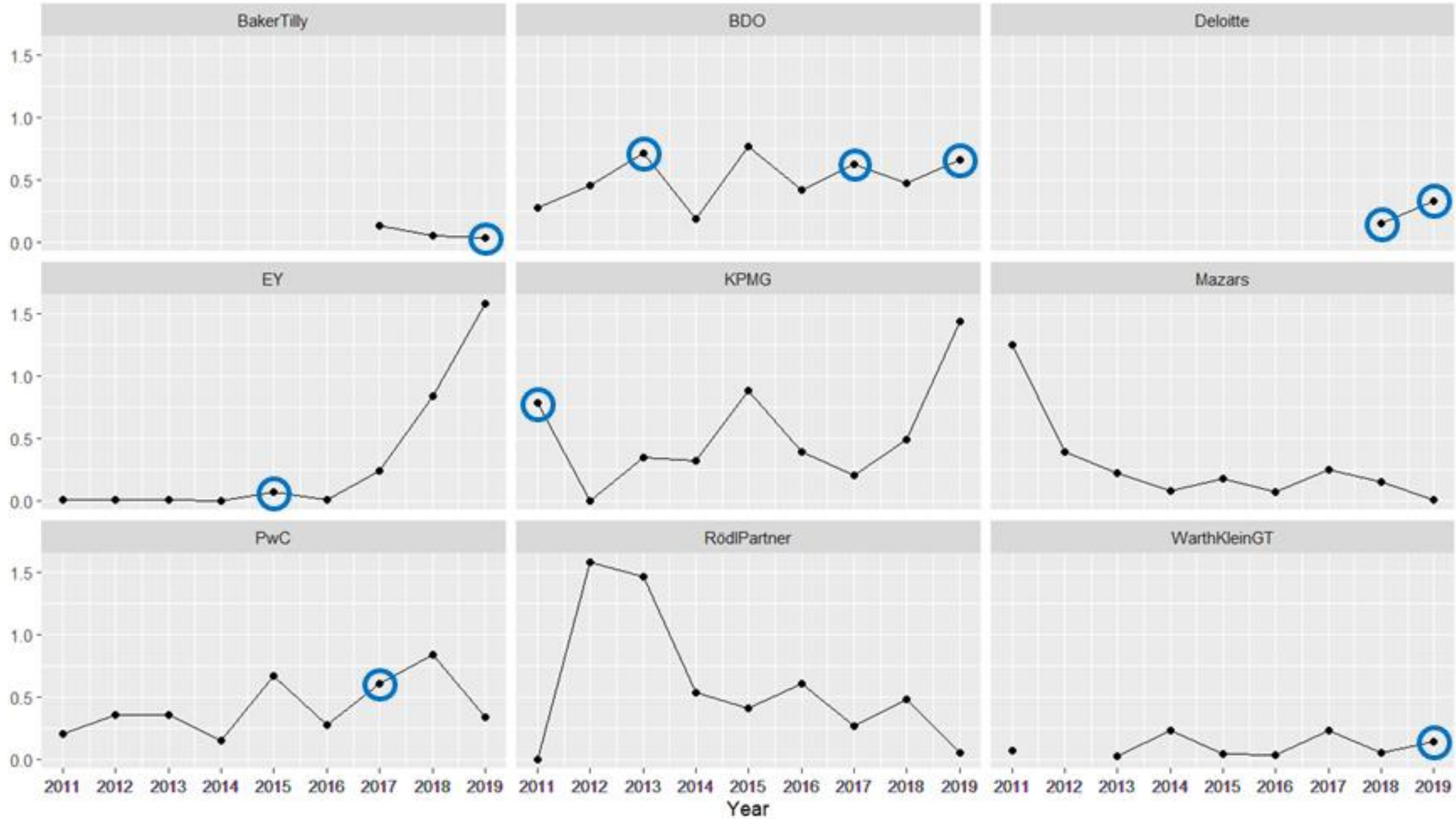


FIGURE 1 Panel B *Software_Intensity* by audit firm and by yeartable



Notes: Panel A shows the yearly means of *Software_Intensity* for our full sample of client firm-year observations. Panel B shows the yearly means for subsamples of client firm-years that share the same auditor. The blue circles represent audit firm-years where a new audit methodology was fully rolled audit (*New_Audit_Methodology* = 1).