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# Do non-audit service failures impair auditor reputation? An analysis of KPMG advisory service scandals in Germany

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## ABSTRACT

Big Four accounting firms increasingly focus on non-audit services. Failures in these services may impair the accounting firm's reputation as an auditor. They may negatively affect the Big Four, its clients, and client stakeholders. From the perspective of critical scholarship, it is vital to understand whether potentially marginalized actors that auditors are meant to protect (e.g., the general public) bear adverse consequences from non-audit failures. Low litigation settings, such as Germany, are of particular interest in this context because they rely on reputation risks to motivate Big Four auditors to provide high-quality services. Accordingly, we analyze two events of observable non-audit service deficiencies of KPMG Germany. We first use an event study and show that KPMG's audit clients suffer negative capital market reactions after the NAS failure events. We then ask whether KPMG, having caused the events, also faces adverse consequences. Moreover, we explore theoretical mechanisms behind the observed capital market reactions. Using the Eisenhardt Method, we deeply engage with extensive quantitative data sets and explore auditor switches, audit pricing, and clients' earnings management. The analyses do not reveal significant negative consequences for KPMG. Earnings management data provides some limited indication that KPMG allows clients more opportunistic accounting choices. Overall, our analysis suggests that reputation may be insufficient to discipline Big Four auditors from acting opportunistically at the cost of less powerful actors in low litigation settings.

## 1. Introduction

The Big Four accounting firms have heavily increased their non-audit services (NAS) revenue in recent years so that it now surpasses audit revenue (Rapoport, 2018; Schmitt, 2019; for Germany: Streicher & Deveci, 2016). Since profit margins are higher for NAS than for audits, the Big Four are focusing on further increasing their NAS revenue (Fröndhoff, 2018). Hence, in addition to possible threats of auditor-provided NAS to auditor independence, there are concerns that the auditing practice will suffer from a shift of focus from auditing to NAS (Donelson et al., 2020). Such concerns are aggravated when turmoil due to a NAS failure demands additional attention from the accounting firm's management (Baugh et al., 2018). Concerns about a shift of focus to NAS may threaten an accounting firm's reputation, in general, and regarding its auditing service. These phenomena have worried "mainstream" accounting researchers for decades (e.g., Abbott & Buslepp, 2022, for reputational spillovers; Tepalagul & Lin, 2015, for NAS and auditor independence) and are just as relevant to the critical accounting research project and its concern with understanding and overcoming marginalization processes (e.g., Gendron, 2018). The Big Four are powerful institutions that, through dominating statutory auditing

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and diversifying their service portfolio, may marginalize public interest, whose defense is part of their role as statutory auditors. Specifically, when their focus on NAS harms the actual (or, through reputation losses, the perceived) quality of their audit services, this implies the firms use their power to create profits at the expense of assurance provided to the general public or less powerful stakeholders (e.g., retail investors). We aim to link these literatures to contribute to building a more comprehensive theory of potential adverse consequences of how auditing is institutionalized in today's praxis by asking the following overarching research question: Do non-audit service failures harm the reputation of the accounting firm's audit services and, if so, who bears potentially adverse consequences?

We study the consequences of two presumable NAS failures of KPMG in a low litigation setting. We identified them from a comprehensive newspaper search for accusations of failure or misbehavior regarding Big Four NAS in Germany. In both cases, KPMG managed the sale of a (mostly) state-owned company, the racetrack and event location *Nürburgring*, and the airport *Frankfurt-Hahn*. KPMG showed lax operations, giving a dubious candidate access to confidential data in the first case (hereafter: *Ring* event) and failing to identify a shell company in the second case (hereafter: *Hahn* event). Finding adverse consequences would indicate reputational spillovers across two service lines of a Big Four brand. We know little about reputational spillovers from NAS to auditing practices in low litigation settings, which promise insights that are not readily observable in high litigation settings. Low litigation settings are of particular interest for the critical accounting research project because they lack disciplinary mechanisms to counterbalance power differences between accounting firms and clients' stakeholders. Specifically, auditors face little financial risk from audit failures, and shareholders or the public have little chance of reimbursement from auditors for damage auditors may (partly) be responsible for.<sup>1</sup>

Our analysis first follows "mainstream" quantitative literature (e.g., [Brown et al., 2013](#); [Weber et al., 2008](#)) to explore whether there are any consequences by studying short-term abnormal stock returns of KPMG audit clients after the two events. We predict and find that accusations of NAS failure lead to negative abnormal stock returns of the accused firm's audit clients. Specifically, we find abnormal one-day stock returns of about  $-0.5\%$  for the average KPMG client at either event ([Table 2](#)). Robustness checks indicate that these results are sensitive to some design choices for the *Hahn* event ([Table 3](#)). To exclude that our findings are random or merely a result of heightened attention, we analyze two pseudo-events as a falsification test. We choose them based on search engine activity taken from Google Trends to measure general attention ([Da et al., 2011](#)). We do not find significant abnormal returns at the pseudo-events ([Table 3](#)).

To explore who eventually bears these adverse consequences, we turn to the Eisenhardt Method ([Eisenhardt, 1989, 2021](#)), thus building theory from multiple cases by deeply engaging with additional quantitative archival data we collect for our two cases. Our setting lends itself to a theory-developing endeavor because there is neither an obvious mechanism behind the short-term reactions we observe nor sufficient prior theory or empirical evidence to make meaningful hypotheses. Specifically, reputation losses could impair the auditor's ability to retain existing clients, gain new ones, and charge high audit fees on the one hand ([Klein & Leffler, 1981](#)). On the other hand, the auditor may implement measures to counteract negative reputation effects, e.g., by providing fee discounts or acquiescing to the client's preferred accounting choices. Alternatively, small changes in reputation may not affect auditor choices because "optimal" auditor-client pairings can have significant economic benefits ([Gerakos & Syverson, 2015](#)). In this case, investor perceptions may still deteriorate.

Following this discussion, we pose two explorative research questions: Do accounting firms experience adverse consequences from apparent reputational spillovers from NAS failures observable in audit markets? To answer this research question, we analyze large-scale quantitative market data on client gains and losses ([Table 4](#)) and audit fees ([Table 5](#)). To our surprise, it seems that accounting firms remain unscathed. We next ask: What mechanisms explain that audit clients face adverse capital market reactions after the accounting firm's NAS failure? We can infer from engaging with the auditor switching and audit fee data that the client-auditor relationship does not seem to deteriorate. However, our analysis of short-term abnormal stock returns indicates that shareholders lose some of their trust in the client's financial reporting. A plausible theoretical mechanism behind these phenomena is that auditors counteract potential reputational harm within the auditor-client relationship by acquiescing more to the client's preferences, i.e., giving up some of their auditor independence. Investors could anticipate such behavior, which could explain the negative abnormal returns. We engage with data on client accruals to explore this theoretical mechanism but find only limited evidence consistent with clients reporting accruals more aggressively ([Table 6](#)).<sup>2</sup> Recent findings from [Donelson et al. \(2020\)](#) are consistent with this limited evidence. They find that Big Four firms acquire consultancies providing audit-unrelated services and shift their culture to commercialism at the expense of audit quality. However, our limited accruals results are not strong enough to rule out alternative explanations. It is possible that economic benefits from auditor-client alignment combined with high transaction costs from auditor switching create incentives for clients to keep their auditor even when the auditor does not counteract small reputation losses. However, and more

<sup>1</sup> In this paper, litigation refers to audit services, not NAS. We explain below in more detail why litigation risk is low and why audit litigation (auditors vs third parties), not NAS litigation (auditors vs their clients), is relevant. Recent discussions in the literature suggest that US auditors are no longer primarily concerned with litigation but with PCAOB inspections ([Westermann et al., 2019](#)). Similar evidence for Germany is missing, and inspection risk seems low (see below). Hence, this alternative perspective does not substantially change our arguments because our setting has a low litigation *and* inspection risk.

<sup>2</sup> We could not use going concern opinions and restatements as alternative proxies for audit quality because both are too rare in our setting. Only 25 observations in our final sample are coded as "Yes" in Audit Analytic's Going Concern variable. The only available database for accounting errors we are aware of are the findings of the German Financial Reporting Enforcement Panel (Deutsche Prüfstelle für Rechnungslegung, DPR). We only have 36 error observations in our final sample, and not all DPR errors relate to materially restated earnings, e.g., errors in disclosure formats or classification of assets or liabilities.

importantly, under both plausible mechanisms, shareholders bear the cost of the malpractice in an accounting firm's non-audit division.

We contribute to the critical accounting research project by advancing theory concerned with how the Big Four as powerful institutions potentially harm less powerful actors through self-interested action. While our analysis centers on reputational spillovers as the primary theoretical mechanism behind such marginalization, we embed it in theories on disciplinary mechanisms in auditing and Big Four commercialism. Of chief concern is our observation that reputation spillovers from NAS deficiencies to the auditing practice do not compensate for a lack of other disciplinary mechanisms (litigation or inspection). Instead, they might even bolster Big Four commercialism in that accounting firms might prefer giving up some auditor independence to avoid losing ground in the audit market. Shareholders seem to bear the cost through declining stock prices. These mechanisms are potentially interesting to regulators in low litigation jurisdictions (or jurisdictions with lax inspections), who are concerned with changing the litigation (inspection) environment to enhance incentives for providing high-quality audit services. Debates may especially be needed as critical scholars have found that brand-name accounting firms enjoy less scrutiny from regulators (Rollins & Bremser, 1997), have successfully lobbied against stricter disciplinary mechanisms (e.g., Citron, 2003; Windsor & Warming-Rasmussen, 2009), and current regulatory settings may be ill-designed to discipline global, powerful organizations (e.g., Malsch & Gendron, 2011; Samsonova-Taddei & Humphrey, 2015).

In addition, we show the value of quantitative analysis for the critical accounting research project. While the negative capital market reactions stem from a reduction in shareholder's audit quality *perceptions*, the potential mechanisms behind our second research question distinguish whether these perception changes coincide with reductions in *factual* audit quality or result from switching costs fortifying individual auditor–client relationships. Such distinctions are difficult to make from qualitative data, especially if the lived experiences of social actors do not fully capture the actions of groups, as in capital or audit markets (Everett et al., 2015). While understanding perceptions and lived experiences of social actors is relevant for critical accounting researchers, understanding the mechanisms behind perception changes is critical to intervene with undesirable consequences, providing for a situation in which a more inclusive use of quantitative research could contribute to the critical accounting project (Maran et al., 2022). If changes in factual audit quality are the explanation, changing incentives for auditors is key, e.g., through stricter disciplinary mechanisms (litigation or inspection). Alternatively, intensifying training or culture in the profession could alleviate audit quality threats from excessive client orientation. For instance, Ricci (2022) shows that progress framing, i.e., prompting auditors to view their service performance as an indicator of achieving the goal of good client service, can alleviate negative audit quality effects from client orientation. If switching costs and economic benefits of auditor–client pairings are the explanation, it may be more important to explore whether sticky auditor–client relationships harm client stakeholders.

We also contribute to the “mainstream” auditing literature concerned with audit quality incentives (insurance vs reputation hypothesis). Our analyses extend the non-results in Baugh et al. (2018) and Ege et al. (2022) using a low litigation setting that allows us to disentangle reputation from insurance effects in a short-term event study. While we find signs of loss of investor trust in the accounting firm's audit practice in the short run, our theory-building endeavor indicates it may stem from short-term perceptions of auditor independence loss rather than from persistent reputational damage to the accounting firm's audit practice. The lack of economic consequences to the accounting firm is consistent with Baugh et al. (2018) and Ege et al. (2022). It suggests that perceptions may differ between different stakeholder groups (shareholders vs audit committees). One potential reason is a commercial orientation of accounting firms favoring client management at the expense of other stakeholders.

Our findings are potentially concerning from the perspective of regulators and users of audited financial statements and point toward a potential audit quality threat in low litigation jurisdictions. To the best of our knowledge, we provide the first low litigation evidence of the overall effects of potential negative reputation signals from NAS on the accounting firm and its audit clients. Research in a pure audit setting in low litigation jurisdictions has shown that adverse effects of failures or misconduct are punished through reputational consequences (Skinner & Srinivasan, 2012; Weber et al., 2008). Our results in the context of NAS deficiencies do not give us confidence that reputation compensates for lack of litigation. These results may inform recent debates within the profession about moving to pure audit firms to improve audit quality. EY has announced plans to split its audit from its consulting practice to create two different firms (Goldstein, 2022), which could alleviate some of the issues suggested by our analyses. Deloitte has denied reports that it considered similar plans, and KPMG and PwC have not taken clear positions on that issue (Reuters, 2022). In light of our results, serious consideration of splitting up auditing and NAS practices could be warranted among the Big Four and other international accounting firms that operate with similar commercialism (Harber & Willows, 2022). We also encourage researchers, regulators, and the profession to debate creative solutions potentially alleviating commercialism in auditing, such as creating an Audit Board as suggested by van Brenk et al. (2022).<sup>3</sup>

The remainder of our paper is structured as follows. Section 2 discusses the theoretical background, develops a hypothesis and two research questions, and lays out our setting. In Section 3, we present our “mainstream” short-term event study. A theory-development endeavor using the Eisenhardt Method follows in Section 4, and Section 5 concludes.

<sup>3</sup> They conceptualize an Audit Board as a quasi-governmental organization conducting annual audits, staffed with a core of experienced audit experts. These experts are complemented by members from the existing accounting firms who take a job at the Board for a certain time period.

## 2. Background, hypothesis and research questions, and setting

### 2.1. Incentives for audit quality and their relation to non-audit services

A fundamental theoretical question in auditing research is how to incentivize audit quality. The literature has established two hypotheses on different theoretical incentives: the insurance and reputation hypotheses (Skinner & Srinivasan, 2012). According to the insurance hypothesis, auditors have an incentive to provide high-quality audits because of the risk of litigation that may result in significant payments to stakeholders of the audit client after an audit failure. This incentive is stronger for larger accounting firms because they can cover greater claims by plaintiffs (Dye, 1993; Lennox, 1999). Therefore, doubts about the auditor's ability to provide such insurance may negatively affect investors' valuation of audit clients (Brown et al., 2013; Dee et al., 2011).

On the other hand, the reputation hypothesis proposes that an auditor can only gain new audit clients, retain existing ones, and charge satisfactorily high audit fees if the auditor generates and maintains a reputation of high trustworthiness and service quality. Therefore, auditors have incentives to avoid any failure that impairs their reputation and harms their overall revenue-generation potential (Davis & Simon, 1992; DeAngelo, 1981).

The "mainstream" auditing literature has collected extensive evidence consistent with both the insurance (Khurana & Raman, 2004; Lennox, 1999; Mansi et al., 2004; Menon & Williams, 1994; Willenborg, 1999) and reputation (Boone et al., 2015; Skinner & Srinivasan, 2012; Weber et al., 2008) hypothesis. This research relies on low litigation contexts or on specific settings in which the effects can be analyzed separately, as they are generally hard to disentangle in Anglo-Saxon jurisdictions, where both effects are theoretically substantial (Brown et al., 2013; Dee et al., 2011; Skinner & Srinivasan, 2012). While it focuses on auditing services, the reputation of accounting firms is often considered a brand-name reputation (Craswell et al., 1995; Ratzinger-Sakel & Schönberger, 2018), i.e., Big Four brand names create a reputation for all services of the accounting firm. In this logic, a decrease in this brand-name reputation harms the perceived value of all accounting firm services, regardless of whether the failure occurred in the accounting firm's non-audit or audit practice (Selnes, 1993; Shapiro, 1983).

Some research in the highly litigious US setting has recently investigated this broader perspective. A group of studies uses severe misconduct in the tax advisory services of KPMG (and the other Big Four, which were not prosecuted), which led to an investigation by the US Department of Justice and was settled in 2005. Brown et al. (2013) conduct an event study with KPMG audit clients and find significant market reactions consistent with the insurance hypothesis. In a similar event study, Incardona et al. (2014) find significant negative abnormal returns for audit clients of the other Big Four, consistent with the perception of the events as a collective scandal spanning all Big Four firms, not only KPMG.

Turning to the reputation hypothesis, Baugh et al. (2018) attempt to isolate reputation effects by comparing the development of KPMG's audit fees (and client gains and losses) from pre- to post-scandal periods to the other Big Four. They do not find support for the reputation hypothesis. However, if the stakeholders perceived the scandal as a collective failure of the Big Four, their result is not surprising in a within-Big Four analysis. Moreover, KPMG accepted responsibility for the misconduct and committed to improving its future behavior. Thus, informed stakeholders could expect KPMG to provide higher-quality services in the future, alleviating adverse reputation effects (Corona & Randhawa, 2018). Taking an even broader view of brand-name reputation, Donelson et al. (2019) analyze client failures out of the auditor's control. They find that auditor bargaining power as an indicator of reputation decreases in cases of clients' non-accounting securities violations (where the auditor is not responsible), but only for other clients of the same auditor office and industry as the scandal client. Analyzing multiple non-audit-related negative news events, Ege et al. (2022) do not find consequences for the auditor (net client losses) or clients (abnormal returns).

In conclusion, recent empirical research has provided some evidence that insurance and reputation effects extend from audit-only settings to a broader brand-name perspective, but results are inconclusive. Moreover, critical scholars have shown that the power of Big Four auditors reduces their exposure to litigation (Rollins & Bremser, 1997) and reputational losses (Maksymov et al., 2020) and that they successfully use impression management to reduce blame for scandals (Dunne et al., 2021) or suspicions of undesirable commercial orientation (Dunne et al., 2022). Thus, in addition to an inherent tension between the litigation and the reputation hypothesis, it is questionable whether the Big Four, as powerful actors in the audit setting, face sufficient institutional incentives to serve the interests of the general public and less powerful stakeholders (e.g., retail investors). With NAS margins driving Big Four profits, commercialism may overcompensate these audit-quality incentives and render potential reputational spillovers from NAS to auditing ineffective (Donelson et al., 2020).

Therefore, we argue that revisiting the potential spillovers of reputation damage in the NAS context using a low litigation setting can improve our understanding of reputation incentives for accounting firms. We are not aware of prior research using low litigation settings to isolate the reputation hypothesis in the NAS context. High litigation settings prevent researchers from drawing conclusions from direct, short-term reactions of investors. The evidence mentioned above from the US tax scandal indicates compelling dynamics that have been attributed to litigation effects spilling over while reputation effects do not. As this is inconsistent with findings in Donelson et al. (2019), existing theories on spillovers of NAS failures on Big Four audit practices do not seem sufficient to describe today's praxis. We argue that a low litigation setting potentially is relevant in seeking to develop a broader theory on spillovers from NAS to auditing because it is less prone to competing theoretical mechanisms.

### 2.2. Hypothesis and research questions

Our theoretical discussion suggests that reputational spillovers from NAS failures may affect the firm's audit practice and its audit clients. A direct way to observe these effects is a short-term event study that can identify immediate changes in investor perceptions.

Earlier research found short-term reactions to a NAS scandal in the US that might support the insurance hypothesis (Brown et al., 2013; Incardona et al., 2014). A low litigation setting is necessary to attribute any event study results to the reputation hypothesis. An additional advantage of a setting with low auditor litigation and similarly infrequent NAS disputes is that, without litigation or disputes, the public is not frequently exposed to Big Four service failures, making press coverage of such failures more salient. Hence, the Big Four are less likely to be resilient against reputational losses from the media covering scandals (Maksymov et al., 2020). Based on these considerations, we hypothesize:

*H1: Stock returns of KPMG clients at events of KPMG NAS failures are more negative than those of a comparable market portfolio.*

If H1 holds, reputational spillovers are not the only possible explanation for adverse investor reactions. Findings in Donelson et al. (2020) suggest that Big Four NAS activities may shift the firms' cultures to commercialism, potentially at the expense of audit quality. NAS failures may aggravate this situation as Big Four management may intensify commercialism to prevent losing NAS market shares. If shareholders infer such audit quality threats from NAS failures, stock prices may drop. Shareholders suffer the loss of abnormal stock returns. Thus, especially from a critical perspective, it is insufficient to interpret negative abnormal stock returns as evidence of reputational spillovers. Instead, we need to understand other consequences of NAS failures beyond the stock market reactions. If there are reputational spillovers, observable consequences for the accounting firm will eventually result. Therefore, we need to engage more deeply with the consequences of our events to answer the following research question:

*RQ1: Do accounting firms experience adverse consequences from apparent reputational spillovers from NAS failures that are observable in audit markets?*

Under the reputation hypothesis, the reputational damage from NAS failures may extend to other groups, e.g., audit committees, and limit KPMG's ability to gain and retain clients and charge satisfactorily high audit fees (e.g., Baugh et al., 2018; DeAngelo, 1981; Klein & Leffler, 1981; Weber et al., 2008; Wilson & Grimlund, 1990). KPMG may discount audit fees to retain or attract clients, counteracting potential negative reputation effects (Boone et al., 2015). As a result, client gains and losses could be unchanged, but audit fees would decline. Taken together, analyzing several observable characteristics of KPMG's audit practice after NAS failures likely provides insights into competing theoretical mechanisms behind the reputation hypothesis.

Alternatively, accounting firms could theoretically avoid adverse consequences. The accounting firm could acquiesce to the client's opinion in audit negotiations to retain clients, potentially leading to aggressive earnings management choices, generally considered as lower audit quality.<sup>4</sup> Such a strategy is consistent with accounting firm commercialism and a reduction of auditor independence. If successful, investors may still react negatively to expected audit quality threats. The described theoretical mechanism could be observable in the audit quality of KPMG clients after the NAS failures.

Finally, switching costs are generally high, and an auditor–client alignment can have significant economic value (Gerakos & Syverson, 2015). If switching costs and alignment benefits exceed reputation damages, the accounting firm may not suffer adverse consequences, and *factual* audit quality may remain stable. Shareholders may still *perceive* the auditor as less competent, consistent with H1 and the reputation hypothesis. Shareholders of other KPMG clients would bear the adverse consequences of the reputational spillover, and no other changes would be observable in KPMG's audit practice after the NAS failures. Put differently, the question is whether lower *perceived* audit quality coincides with lower *factual* audit quality or not, and we aim to answer the following research question:

*RQ2: What mechanisms explain that audit clients face adverse consequences from the accounting firm's NAS failures?*

### 2.3. German setting

Several particularities of the German setting provide advantages and tension for our analyses. Litigation risks are low in Germany because auditor liability is capped and considerable third-party litigation is absent.<sup>5</sup> Auditor oversight is similarly limited. The German Auditor Oversight Board, responsible for overseeing public interest entity audits, publishes its sanctions since 2018.<sup>6</sup> The most severe published auditor sanction is a 32,000 Euro fine. Given apparent low litigation and inspection risks, reputation effects could be particularly pronounced.

<sup>4</sup> Instead, KPMG may increase audit effort and fees to counteract reputational damage by providing better audit quality. Better audit quality is inconsistent with H1. We find no indications of better audit quality.

<sup>5</sup> The liability limit for [grossly] negligent misconduct is [unlimited] € 16 m (before 2021: € 4 m in both cases) for listed companies (Section 323 (2) German Commercial Code – Handelsgesetzbuch (HGB)). For third-party litigation, the German Civil Code (Bürgerliches Gesetzbuch (BGB)) requires the plaintiff to prove that the auditor has acted with intent. Case law provides remote possibilities to sue negligent auditors under very specific circumstances and typically applies a similar liability limit. Cases rely on the fact that audit services may directly serve client stakeholders, e.g., when making investment decisions (Gietzmann & Quick, 1998). NAS typically do not directly serve client stakeholders. Therefore, third-party NAS litigation seems almost impossible.

<sup>6</sup> [https://www.apasbafa.bund.de/APAS/DE/Anlassbezogene\\_Berufsaufsicht/anlassbezogene\\_berufsaufsicht\\_node.html](https://www.apasbafa.bund.de/APAS/DE/Anlassbezogene_Berufsaufsicht/anlassbezogene_berufsaufsicht_node.html) (Accessed September 21, 2022).

Moreover, the importance of statutory audits and, hence, investor sensitivity to auditor reputation in Germany differ from Anglo-Saxon settings. Some arguments support a lower (i.e., auditor reputation is less important and reputation effects are less pronounced) and some a higher (i.e., reputation effects are more pronounced) importance of statutory audits. Weber et al. (2008) argue that the importance of auditing is comparably low, biasing against negative market reactions. They reason that, as banks are often both debt providers and shareholders, bankers closely monitor companies as supervisory board members, which may partly substitute the monitoring role of external auditing. Yet, the portion of DAX (largest German listed companies) shares held by domestic shareholders decreased from 49 % in 2005 to 37 % in 2016 (EY, 2018). This indicates a reduction of shareholders with high monitoring activity, which might lead to more demand for external monitoring. Overall, German stock ownership is concentrated in the hands of institutional and strategic (families and companies) investors, with limited investments by private investors (EY, 2018; Hackethal et al., 2005; La Porta et al., 1999). As institutional investors' primary source of information comes directly from directors via analyst conferences and roadshows, whereas private investors rely mainly on financial reports (Pellens & Schmidt, 2014), auditing might play a less pronounced role.

Although the German two-tier board system<sup>7</sup> theoretically provides comparably high monitoring incentives, monitoring quality in practice may be low. Reasons include that board members often lack financial expertise or personal independence, serve on many boards simultaneously, fulfill board duties part-time next to a full-time job, and meet infrequently (Quick & Schmidt, 2018). Additionally, the large representation of employees may misalign the interests of monitors and shareholders, which makes external monitoring much more important from a shareholder perspective (Elson et al., 2015). Moreover, the dual role of banks as members of the supervisory board and external stakeholders may increase their demand for high audit quality.<sup>8</sup> As private investors might care little about audit quality (Cascino et al., 2014), the large proportion of institutional investors in the German setting may increase the demand for high audit quality. Finally, overall investor protection is low in Germany (Gul et al., 2013). Therefore, we expect that, in practice, the demand for high audit quality and investors' sensitivity to auditor reputation is substantial.

#### 2.4. NAS failure cases

Our analyses require identifying events that encompass unexpected and potentially material information. Moreover, for the event study, we need to find the exact dates on which the information becomes public and exclude confounding events in the days surrounding the event date of interest (MacKinlay, 1997). First, to identify instances of potential NAS failures, we searched LexisNexis and the newspaper database of WISO (containing many national and regional German newspapers) for a combination of "scandal" with either "Deloitte", "EY", "Ernst & Young", "KPMG", or "PwC". We restrict our search to the Big Four to have sufficiently large client portfolios for our analyses. We exclude the financial crisis and search for articles beginning in 2010. While we might miss instances of NAS failure with our restriction to articles that contain "scandal" and explicitly mention the accounting firm, our approach ensures that the press coverage is unambiguously negative and highlights the auditor's role. Of all articles identified with this procedure, we only kept those with a clear accusation of failure or misbehavior when the NAS provision was still ongoing.<sup>9</sup>

With this procedure, we identified two scandals, the *Nürburgring* and *Frankfurt-Hahn airport sale scandal*.<sup>10</sup> For each scandal, we identified the first clear event that accused KPMG (the NAS provider in both cases) of wrongdoing that led to NAS failure. We limit our analysis to these initial events because, in both cases, additional accusations and information followed in close succession, making a short-term event study analysis unattainable for the later events. We made sure that the instances were relevant to the public. Both entailed severe monetary losses for the state of Rhineland-Palatinate. Next to this initial event, we also identify an event when the entire failed NAS relationship finally ended. As we explain in Appendix B, this end of the scandalous NAS period is important for the correct specification of treatment periods when studying our research questions. We now briefly summarize the two scandals.

<sup>7</sup> The executive board is responsible for managing the company. The supervisory board consists of only non-executive members, is independent of the executive board, is responsible for monitoring, and often forms an audit committee from its members. For large companies, employees (often unionists) and shareholders (typically including banks) each get half of the supervisory board seats. Hence, the theoretically high independence of management may increase monitoring quality. Refer to Quick and Schmidt (2018) for more details.

<sup>8</sup> As external stakeholders, they act as creditors and shareholders. Moreover, they often execute proxy votes of private investors (section 135 of the German Stock Corporation Act – Aktiengesetz (AktG)). As supervisory board members, they have to exert due care in monitoring the auditor and assuring appropriate audit quality. For instance, section 117 AktG mandates the supervisory board to monitor financial reports and the audit and report on both at the annual shareholder meeting. The Auditing Reformation Act (Abschlussprüfungsreformgesetz (AREG)) has substantially extended the supervisory board's responsibility and liability concerning the auditor. Section D.III of the German Corporate Governance Codex further emphasizes the responsibility of the supervisory board for the quality of the financial reports and the annual audit.

<sup>9</sup> We excluded four cases. In the first case, KPMG investigated accusations of espionage of employee information by German railway provider *Deutsche Bahn* and was accused of having helped with that espionage earlier. Because KPMG helped resolve the potential misbehavior at that time, we do not interpret it as negative press. In the second case, *LuxLeaks*, two PwC employees blew the whistle on information about tax evasion. However, the press covered PwC for whistleblowing and not for the support of tax evasion. The third case is the *Cum-Ex* tax evasion scheme in Germany. The Big Four appeared as legal counselors in press articles about it but were not accused of low-quality services or misbehavior. In addition, the legal counsel had been years, if not decades, ago. In the fourth case, the *P+S* bankruptcy scandal, KPMG provided a positive restructuring opinion in 2009, which led the state of Mecklenburg-Western Pomerania to support *P+S*. *P+S* went bankrupt in 2012, producing multiple KPMG accusations. However, they happened long after the suspected NAS failures.

<sup>10</sup> Similarities between the two scandals are coincidence, e.g., that KPMG was involved, Rhineland-Palatinate was involved, and the services consisted of managing the sale of a company.

Before KPMG became involved in the sale of the racetrack and event location *Nürburgring*, the public had already discussed the project as a scandal of political mismanagement. The state Rhineland-Palatinate as the main owner of the racetrack had expanded it to an event location and the operating company had turned bankrupt in 2012, which had cost the state around € 500 million and initiated the sale process. KPMG managed the public tender. On November 29, 2013, our initial event (*Ring* event), the press reported that KPMG gave a dubious buyer access to confidential data without requesting mandatory proof of financing (Zerfaß, 2013). Although KPMG's advisory quality became questionable at that point, they kept managing the sale process and finally announced buyers *Capricorn* and *Getspeed* on March 11, 2014, the end of the scandalous NAS period. When they could not pay the second installment and were in financial trouble, it became evident that doubts of KPMG's advisory quality had been appropriate. However, the EU court in Luxemburg confirmed that the KPMG-led sale was executed legally (Deutsche Presseagentur, 2019; Zerfaß, 2014, 2015).

Leading to our second scandal, a strategic realignment of airport *Frankfurt-Hahn* included the decision of majority owner Rhineland-Palatinate to sell most of its shares through a public tender managed by KPMG, which began in 2015. Just one day after they presented the Chinese company *SYT* as the chosen buyer, on June 7, 2016, our initial event (*Hahn* event),<sup>11</sup> press releases doubted the quality of KPMG's background checks and the soundness of *SYT* (Deutsche Presseagentur, 2016a). Those doubts soon proved justified because, by the end of June, journalists from broadcaster Südwestrundfunk found that *SYT* was a shell company, which led to a suspension of the sale just hours later. On November 21, 2016, Rhineland-Palatinate decided to replace KPMG, resulting in the end of the scandalous NAS period (Deutsche Presseagentur, 2016b; Petermann, 2016; Schmitt, 2016).

### 3. Short-term event study

#### 3.1. Sample and models

For each event year, our sample selection starts with all German listed companies with return data available on Refinitiv Datastream.<sup>12</sup> If a company has multiple listed securities, we only keep regular stock carrying voting rights. Then, we manually identify all KPMG clients with available data. We drop penny stocks and require at least 50 days with positive trading volume in the estimation window.<sup>13</sup> Next, we drop observations with no available data on total assets or year-end market value necessary to create our matched portfolio. For each event, we also exclude all companies with a confounding event.<sup>14</sup> Table 1 summarizes our sampling procedure and reports the final sample sizes for both events.<sup>15</sup>

**Table 1**  
Sample selection.

Event	Ring event	Hahn event
KPMG clients with sufficient return data on Datastream	93	98
Less		
Penny stock	0	0
Low trading volume	17	22
Firms with confounding event	11	9
Firms without data to match peer	7	7
Full sample	58	60

Notes: Events are as follows:

*Ring* event: November 29, 2013; KPMG gave a dubious potential buyer access to confidential data.

*Hahn* event: June 7, 2016; First press releases, which doubt the existence of *SYT* and the quality of KPMG's integrity due diligence investigation.

<sup>11</sup> As news broke in the late afternoon, some investors may have reacted the day after. Using an event window of two days instead of just the event day yields similar results in magnitude and significance as our main analyses.

<sup>12</sup> The return for day  $t$  is the sum of the closing price of day  $t$  and the dividend paid on day  $t$  minus the closing price of day  $t-1$ , scaled by the closing price of day  $t-1$ . We require at least 50 returns in the estimation window.

<sup>13</sup> We do not have penny stocks even for high cutoffs (prices below 0.5 Euros). Since the number of days with active trading in the estimation window has no natural cutoff, we repeated our main analyses (Table 2) with 20, 100, and 200 days instead of 50 days. Inferences remain the same.

<sup>14</sup> To identify KPMG clients with confounding events, we screen all news posted on DGAP (Deutsche Gesellschaft für Ad-hoc-Publizität – German Society for Ad-hoc Publicity; <https://www.dgap.de/>; last access September 21, 2022), an online news service for important or mandatory stock market information, in the  $(-5,5)$ -window. We use this larger window to ensure that we identify all confounding events that may have a significant influence lasting until or starting on the event date. We identify a confounding event if the company issues financial statements; announces large deals; reports key performance indicators; announces mergers and acquisitions; announces large capital measures; or appoints a new executive board member.

<sup>15</sup> We constructed two subsamples with potentially closer proximity to the NAS scandal: all KPMG audit clients that also purchase NAS from KPMG (their investors may pay closer attention to KPMG NAS practices) and all KPMG clients from offices that are geographically close to the NAS failure event (their investors may be more aware of local news). Results do not differ consistently from our full sample (see Appendix C).

Our main test of H1 uses the market model of [Markowitz \(1959\)](#) and [Sharpe \(1963\)](#). It assumes that the return  $R_{i,t}$  of a given stock  $i$  on day  $t$  has a perfectly linear relationship with the return of the corresponding market portfolio  $R_{m,t}$  on day  $t$ . We model this relationship with the following OLS regression ([MacKinlay, 1997](#))<sup>16</sup>:

$$R_{i,t} = \alpha_i + \beta_i \cdot R_{m,t} + e_{i,t} \quad (1)$$

As the market portfolio  $R_{m,t}$  is not directly observable, researchers often use a market index as a proxy. However, readily available market indices have a significant disadvantage in our case because they contain many KPMG clients ([Audit Analytics, 2018](#)). Capital market reactions to KPMG clients would strongly influence market index returns; hence, Eq. (1) would overestimate the relationship between  $R_{i,t}$  and  $R_{m,t}$ . Therefore, we create returns of a value-weighted market portfolio  $R_{m,t}$  without KPMG clients for each year as follows: we match a non-KPMG client to each KPMG client by industry and total assets (with replacement). We multiply the returns of each included non-KPMG client with its year-end market value and divide the sum of all weighted returns by the sum of year-end market values.

For each KPMG client  $i$ , we estimate  $\alpha_i$  and  $\beta_i$  of Eq. (1) in an estimation period, starting 255 trading days (about one year) and ending 11 trading days before the event date (i.e., a regression with 245 trading days as observations). Each regression produces estimates for the parameters of Eq. (1),  $\hat{\alpha}_i$  and  $\hat{\beta}_i$ , which we use to calculate abnormal returns  $AR_{i,t}$  of stock  $i$  on day  $t$  as follows:

$$AR_{i,t} = e_{i,t} = R_{i,t} - (\hat{\alpha}_i + \hat{\beta}_i \cdot R_{m,t}) \quad (2)$$

In our main tests of abnormal returns, we use the event day  $t = 0$ . For additional analyses, we also use cumulative abnormal returns  $CAR_{i,\tau,T}$  for firm  $i$  in the event window from  $t = \tau$  to  $t = T$ , which are the sum of the abnormal returns on each day  $t$  in this event window. Under H1, we expect the (cumulative) abnormal returns to be significantly negative.

We use t-tests to estimate whether (cumulative) abnormal returns differ from zero. However, in our setting, event dates are clustered for all sample firms, potentially biasing t-tests, which assume independent observations. Therefore, we further test our hypothesis with the statistic proposed by [Corrado \(1989\)](#), which is robust to event-day clustering and more powerful than alternative robust statistics for short event windows ([Kolari & Pynnönen, 2010](#)). As a robustness test, we use a less powerful portfolio approach, the [Schipper and Thompson \(1983\)](#) model. It differs from analyzing abnormal returns because, instead of estimating abnormal returns for each client  $i$ , it uses only the following equally weighted portfolio returns of all clients:

$$R_t = \frac{1}{N} \sum_{i=1}^N R_{i,t} \quad (3)$$

We use the portfolio return  $R_t$  on day  $t$  to estimate the following OLS regression from 126 trading days before to 126 trading days after the event (i.e., a single regression with 253 trading days as observations):

$$R_t = \alpha + \beta_1 \cdot R_{m,t} + \beta_2 \cdot Event_t + e_t \quad (4)$$

where  $Event_t$  is an indicator variable that takes the value one on the event date and zero otherwise. All other variables are as defined above. Under H1, we expect a significantly negative  $\beta_2$ , indicating an abnormally negative portfolio return on the event date after controlling for market returns.

### 3.2. Results

[Table 2](#) reports the proportion of negative returns and mean returns for market-adjusted returns (the simple difference  $R_{i,t} - R_{m,t}$ ) and abnormal returns (market model in Eqs. (1) and (2)).

We find significant negative market reactions on the event day for both events. Specifically, for the *Ring* event, the average KPMG client experiences an abnormal return of  $-0.514\%$ , and 45 of the 58 KPMG clients (i.e., 78%) in our sample have a negative abnormal return. For the *Hahn* event, the average abnormal return is  $-0.469\%$ , and 42 of the 60 (i.e., 70%) KPMG clients have a negative abnormal return. Market-adjusted returns are more negative. These results are consistent with H1.

We perform several robustness checks to corroborate this conclusion. First, for both events, we report mean abnormal or mean cumulative abnormal returns with  $t$ -statistics based on several different design choices in Panel A of [Table 3](#). The first row for each event shows the  $t = 0$  abnormal return when adding three variables of the Fama and French four-factor model (the fourth factor is  $R_{m,t}$ ) to the right-hand side of Eq. (1). Results remain largely robust. The second row shows the  $t = 0$  abnormal return when replacing  $R_{m,t}$  with the CDAX index.<sup>17</sup> As expected, results become weaker, supporting our idea of constructing a matched portfolio. The third and fourth rows show cumulative abnormal returns for event windows from one day before to one day after the event  $(-1,1)$  and ten days before to ten days after the event  $(-10,10)$ . We find robust results in the  $(-1,1)$ -window for the *Ring* event, consistent with recent accounting research showing that investors act very fast on new information (e.g., [Rogers et al., 2017](#)). Results are robust in the

<sup>16</sup> In robustness tests, we use the [Fama and French \(1993\)](#) multi-factor model, including a momentum factor ([Carhart, 1997](#)). We take the necessary daily data from [Brückner et al. \(2015\)](#).

<sup>17</sup> Other alternatives for  $R_{m,t}$  are all non-KPMG clients (without matching) or all clients of the other three Big Four. We use both alternatives to create value-weighted portfolios and repeat our primary analyses ([Table 2](#)). Results remain largely robust.

**Table 2**  
Market reaction to NAS failure events for German KPMG clients.

	Percent negative	Mean	t-statistic	Corrado-statistic
<i>Ring</i> event (November 29, 2013)				
<i>Market-adjusted return</i>	83 %	$N = 58$ -0.738 %	-2.610***	-1.981**
<i>Market model</i>	78 %	-0.514 %	-1.751**	-2.187**
<i>Hahn</i> event (June 7, 2016)				
<i>Market-adjusted return</i>	82 %	$N = 60$ -1.123 %	-4.837***	-1.901**
<i>Market model</i>	70 %	-0.469 %	-2.215**	-1.477*

Notes: This table presents sample sizes, the proportion of negative returns, mean returns, and test statistics from t-tests and Corrado-tests that the returns are different from zero for two events and three subsamples.

Events are as follows:

*Ring* event: November 29, 2013; KPMG gave a dubious potential buyer access to confidential data.

*Hahn* event: June 7, 2016; First press releases, which doubt the existence of SYT and the quality of KPMG's integrity due diligence investigation.

\*, \*\*, and \*\*\* denotes significance at the 10 %, 5 %, and 1 %-level, respectively, of a one-tailed test that mean returns are negative.

(-10,10)-window for the *Ring* event.

Next, to rule out that overall negative attention to KPMG drives our results rather than changed perceptions of KPMG's service quality, we replace both events with a pseudo-event and repeat our main analysis. To do so, we use Google Trends<sup>18</sup> and identify the first date after the end of the annual meeting season in 2013 and 2016, respectively, on which the use of the search term "KPMG Skandal" (German for "KPMG scandal") spikes in Germany. We identify two clear spikes on March 19, 2014, and February 22, 2017.<sup>19</sup> Panel B of Table 3 presents the results of this falsification test. There are no consistently significant results, and abnormal returns are small.

Finally, Panel C of Table 3 reports the results using the portfolio OLS regression from Eq. (4). Model 1 contains the analysis for the *Ring* event and model 2 the analysis for the *Hahn* event. Effects become considerably weaker and are marginally significant in one-tailed tests for the *Ring* and insignificant for the *Hahn* event. As discussed above, the portfolio OLS regression is less powerful than our main and other robustness tests. It seems that the results of H1 are not strong enough to be identified with tests of low statistical power. The Schipper and Thompson (1983) models for the pseudo-events (models 3 and 4) are insignificant.

In sum, results for the *Ring* event remain largely robust across various research designs, results for the *Hahn* event seem sensitive to some design choices, and the analysis of pseudo-events suggests that the negative abnormal returns from our *Ring* and *Hahn* event are not random.

## 4. Building theory on NAS failure consequences with the Eisenhardt Method

### 4.1. Building theory from two cases

Having established negative market reactions following the NAS failure events, the development of RQ1 and RQ2 above shows that no comprehensive theory exists on other, potentially interdependent consequences of NAS failures in which these negative market reactions are embedded. Developing a clearer theory is of particular interest from a critical perspective because competing theoretical mechanisms leave open who eventually bears the cost of adverse events caused by Big Four firms as powerful organizations. The Eisenhardt Method (Eisenhardt, 1989, 2021) is a well-established approach to elicit theory from deep engagement with multiple cases of phenomena that are not fully understood. Relying on multi-case theory building is the best fit for our data because we lack a robust theory, and our phenomena of interest are only cleanly observable for the two cases we study.

While the Eisenhardt Method is flexible in the cases and data it uses, it has six defining features that fit well with our theory-building endeavor (Eisenhardt, 2021). First, there is little prior theory or empirical evidence. Second, careful case selection is critical, so we used a thorough and systematic search and selection procedure. Third, we rely on pre-existing constructs and measures in our analysis. Fourth, the method emphasizes theoretical mechanisms, focusing on the "why" behind the studied phenomena. We adapt constructs from the auditing literature to a little-studied spillover setting. Fifth, strong theory explores alternative explanations and boundary conditions. The mentioned constructs are alternative explanations to each other, and we design our overarching research question to study the boundary conditions of reputational spillovers as a disciplining mechanism for Big Four auditors. Finally, iterating between theory and data and across cases helps build validity following a replication logic. Using large quantitative data sets for each of our two cases, our analytic approach has the following distinct advantages that help this constant comparison.

We can use several quantitative measures and statistical models to deeply engage with each case from different angles suggested by

<sup>18</sup> Google Trends provides a direct and unambiguous measure of public attention Da et al. (2011).

<sup>19</sup> We use a rolling window of six months, starting with the windows September 1, 2013, to March 1, 2014, and October 1, 2016, to April 1, 2017, and moving forward one month at a time. We required that a spike stays for two subsequent windows and until the next spike appears. As Google Trends reports the relative frequency of a search term, a spike that disappears once the next spike appears might just depict the highest search frequency in a window with an overall very low search frequency.

**Table 3**  
Robustness checks of market reaction to NAS failure events.

Panel A: Alternative (cumulative) abnormal return metrics				
	Ring event (November 29, 2013) Mean (t-statistic)		Hahn event (June 7, 2016) Mean (t-statistic)	
<i>Fama French three factor model</i>	-0.549 %** (-1.748)		-0.318 %* (-1.488)	
<i>Market model with CDAX as market portfolio</i>	-0.279 % (-0.958)		-0.335 %* (-1.586)	
<i>Market model and (-1,1) window</i>	-0.582 %* (-1.605)		-0.230 % (-0.600)	
<i>Market model and (-10,10) window</i>	-2.312 %*** (-2.858)		0.460 % (0.231)	
Panel B: Market reactions at pseudo-events for the full sample				
	Percent negative	Mean	t-statistic	Corrado-statistic
Pseudo-Event 1 (March 19, 2014)		<i>N</i> = 67		
<i>Market-adjusted return</i>	57 %	0.142 %	0.527	-0.056
<i>Market model</i>	57 %	0.141 %	0.523	-0.210
Pseudo-Event 2 (February 22, 2017)		<i>N</i> = 64		
<i>Market-adjusted return</i>	39 %	0.387 %	2.204**	0.703
<i>Market model</i>	47 %	0.164 %	0.926	0.713
Panel C: Schipper and Thompson regression using full sample				
	Model 1 Coefficient (t-statistic)	Model 2 Coefficient (t-statistic)	Model 3 Coefficient (t-statistic)	Model 4 Coefficient (t-statistic)
<i>Intercept</i>	<0.001* (1.782)	0.001** (2.464)	<0.001* (1.808)	0.001*** (3.208)
<i>Return<sub>Matched Portfolio</sub></i>	0.678*** (18.040)	0.641*** (24.499)	0.683*** (19.070)	0.550 (14.296)
<i>Ring event</i>	-0.007* (-1.549)			
<i>Hahn event</i>		-0.005 (-1.009)		
<i>Pseudo-Event 1</i>			0.001 (0.189)	
<i>Pseudo-Event 2</i>				-0.001 (-0.157)
<i>N</i>	253	253	253	253
<i>Adj. R<sup>2</sup></i>	0.563	0.705	0.590	0.446

Notes: Panel A of this table contains mean returns and corresponding test statistics from a *t*-test (in brackets) for alternative return metrics. Panel B presents sample sizes, the proportion of negative returns, mean returns, and test statistics from *t*-tests and Corrado-tests that the returns are different from zero for two pseudo-events. Panel C contains coefficients and *t*-statistics (in brackets) from OLS regressions of Eq. (4) and the *Ring* event, *Hahn* event, pseudo-event 1, and pseudo-event 2 in models 1, 2, 3, and 4, respectively.

Events are as follows:

*Ring* event: November 29, 2013; KPMG gave a dubious potential buyer access to confidential data.

*Hahn* event: June 7, 2016; First press releases, which doubt the existence of SYT and the quality of KPMG's integrity due diligence investigation.

We identify pseudo-events with Google Trends as an event with a spike in the search term "KPMG Skandal" (German for "KPMG scandal") in a rolling window of six months, starting with the windows September 1, 2013, to March 1, 2014, and October 1, 2016 to April 1, 2017. We required that, once a spike appeared, it had to stay for two more consecutive windows and at least until the next spike appears.

\*, \*\*, and \*\*\* denotes significance at the 10%-, 5%-, and 1%-level, respectively, of a one-tailed test that the respective variable of interest is negative, and a two-tailed test otherwise.

the competing theoretical arguments we discussed. Importantly, our discussed theoretical mechanisms would materialize in different measures, essentially making our analysis from different angles an embedded design with multiple analyses in each case.<sup>20</sup> Moreover, starting from the negative investor reaction and following “mainstream” audit research to theorize them as indicators of reputational losses does not seem to capture the entire theoretical mechanism behind the results. Instead, iterative interaction with possible theoretical mechanisms and different quantitative analyses was necessary to develop a coherent theory of what may explain the observed phenomena. This multi-perspective analysis with large data sets shows the value of quantitative analysis for the Eisenhardt Method. The relative strength of the competing theoretical mechanisms is difficult to elicit from qualitative data, especially if the lived experiences of individuals do not fully capture the actions of groups, as in capital or audit markets (Everett et al., 2015). Qualitative data is essential to understanding perceptions of social actors, but understanding the relative strength of mechanisms behind perception changes in large markets is critical to intervene with undesirable consequences. Therefore, Eisenhardt (2021) emphasizes that selecting the right data and approach when using her method is flexible and should be guided by the research question and data availability.

#### 4.2. Results

We perform a series of regression analyses to explore what theoretical mechanisms discussed in developing our research questions fit with our empirical observations. For brevity, we only present parsimonious versions of our OLS models, samples and regression results. Table notes contain technical details, and Appendix D discusses our analytical approach in depth.

To approach RQ1, i.e., whether accounting firms experience adverse consequences from NAS failure events that trigger negative stock market reactions, we first analyze the most consequential step clients could take to avoid reputation spillovers from their auditor: pick a different auditor. To analyze these potential choices, we analyze whether former KPMG clients move to a different auditor after the scandal with the following logistic regression:

$$\text{Auditorchg}_{i,t} = \alpha + \beta_1 \text{LagKPMG}_{i,t} + \beta_2 \text{LagKPMG}_{i,t} \bullet 2014_i + \beta_3 \text{LagKPMG}_{i,t} \bullet 2017_i + \beta_k \text{Controls}_{i,t} + \text{Industry} + \text{Year} + e_t \quad (5)$$

where  $\text{Auditorchg}_{i,t}$  is an indicator variable coded as one if client  $i$  chooses a new auditor for year  $t$ , and zero otherwise.  $\text{LagKPMG}_{i,t}$  is an indicator variable coded as one if KPMG was the auditor of client  $i$  in the year before  $t$ . It captures whether year  $t-1$  KPMG clients are generally more likely to have a different auditor in year  $t$  than in the year before (client losses). We interact this variable with indicators for 2014 and 2017, the first years following our Ring and Hahn event. The interaction terms capture the incremental likelihood to switch auditors for NAS scandal-affected KPMG clients after controlling for the average switching likelihood of all KPMG clients ( $\text{LagKPMG}$ ) and the following additional client characteristics ( $\text{Controls}$ ) that we take from Weber et al. (2008): firm Size, Growth, financial Leverage and changes therein ( $\Delta\text{Leverage}$ ), profitability (ROA), and a Loss indicator. Industry and Year (fixed effects) represent indicator variables for each industry and each year in our sample to control for industry-wide or year-specific differences in auditor switching.

We also analyze whether clients of other auditors are less likely to switch to KPMG with the following logistic regression:

$$\text{Auditorchg}_{i,t} = \alpha + \beta_1 \text{KPMG}_{i,t} + \beta_2 \text{KPMG}_{i,t} \bullet 2014_i + \beta_3 \text{KPMG}_{i,t} \bullet 2017_i + \beta_k \text{Controls}_{i,t} + \text{Industry} + \text{Year} + e_t \quad (6)$$

where the only difference to Eq. (5) is that  $\text{KPMG}_{i,t}$  is an indicator variable coded as one if KPMG is the auditor of client  $i$  in year  $t$ . It captures whether year  $t$  KPMG clients are more likely to have a different auditor in year  $t$  than in the year before (client gains). Again, we interact this variable with indicators for 2014 and 2017, capturing the incremental likelihood to switch to KPMG in NAS scandal-years after controlling for the average likelihood to switch to KPMG ( $\text{KPMG}$ ) and several client characteristics ( $\text{Controls}$ ,  $\text{Industry}$ ,  $\text{Year}$ ). We use 2,888 firm-year observations from 2012 to 2018 in our analysis.

Table 4 presents results from estimating Eq. (5) in Model 1 and from estimating Eq. (6) in Model 2. Results for Model 1 do not indicate any difference in the likelihood of changing auditors for former KPMG clients compared to clients of other auditors. In Model 2, the coefficient of  $\text{KPMG}$  is significantly negative, suggesting that KPMG is less likely to gain new clients in our sample period in general. However, the interactions are insignificant. Therefore, this overall KPMG effect appears unrelated to the NAS failures we investigate. In conclusion, KPMG’s NAS failures do not seem to influence (potential) clients’ choices to select KPMG as their auditor. This is inconsistent with the reputation hypothesis as a disciplinary mechanism for Big Four auditors in low litigation settings. Therefore, it is less likely that the explanation for the negative capital market reactions we observe is that KPMG loses its good reputation with audit clients. Instead, KPMG seems to lose its good reputation with market actors who, as they cannot directly penalize KPMG, penalize KPMG clients by negative market returns.

To follow up on this result, we next explore the possibility that different prices for KPMG audits explain that client preferences for KPMG did not change. We use the following OLS regression to analyze potential pricing differences:

<sup>20</sup> We follow Eisenhardt (2021) and define an embedded design as a case with multiple units of analysis. According to Eisenhardt (1989, p. 542), a “case is analogous to an experiment” in that “each hypothesis is examined for each case”. Thus, we define a case as an event (see Section 2.4) with the necessary conceptual characteristics and empirical data to analyze our hypotheses. Our units are the different perspectives expressed by using different empirical models. Eisenhardt (2021) notes that the number of cases is flexible and depends on data availability, citing several examples that use only two cases and embedded designs with only one case. Our thorough event selection yielded only two available cases, and we cannot increase the number of cases without altering their characteristics.

**Table 4**  
Logistic regression of auditor change.

	Model 1 Client losses	Model 2 Client gains
	Coefficient (z-statistic)	Coefficient (z-statistic)
<i>Intercept</i>	0.056 (0.100)	-0.138 (-0.250)
<i>LagKPMG</i>	0.097 (0.410)	
<i>LagKPMG • 2014</i>	-0.731 (-0.940)	
<i>LagKPMG • 2017</i>	-0.064 (-0.120)	
<i>KPMG</i>		-0.744** (-2.350)
<i>KPMG • 2014</i>		0.029 (0.040)
<i>KPMG • 2017</i>		0.184 (0.260)
<i>Size</i>	-0.212*** (-6.010)	-0.190*** (-5.390)
<i>Growth</i>	0.387*** (3.470)	0.374*** (3.380)
<i>Leverage</i>	0.264 (0.720)	0.297 (0.820)
$\Delta$ <i>Leverage</i>	-0.514 (-1.380)	-0.515 (-1.390)
<i>ROA</i>	-0.022 (-0.040)	-0.079 (-0.130)
<i>Loss</i>	-0.003 (-0.010)	-0.014 (-0.060)
<i>Industry</i>	Yes	Yes
<i>Year</i>	Yes	Yes
<i>N</i>	2,888	2,888
<i>Nagelkerke R<sup>2</sup></i>	0.053	0.058
<i>Likelihood Ratio <math>\chi^2</math></i>	69.01***	76.00***

Notes: Model 1 presents logistic regression results from estimating Eq. (5) and Model 2 presents logistic regression results from estimation Eq. (6). The dependent variable is *Auditorchg*. We winsorize all continuous variables at the first and 99th percentile and compute robust standard errors clustered by client. We define industry based on the 16 sector indices of Frankfurt Stock Exchange and manually backfill missing industries based on SIC codes, if available. \*, \*\*, and \*\*\* denotes significance at the 10 %, 5 %, and 1 %-level, respectively, of two-tailed tests. Variable definitions are in [Appendix A](#).

$$\ln \text{AuditFee}_{i,t} = \alpha + \beta_1 \text{KPMG}_{i,t} + \beta_2 \text{KPMG}_{i,t} \bullet 2014_i + \beta_3 \text{KPMG}_{i,t} \bullet 2017_i + \beta_k \text{Controls}_{i,t} + \text{Industry} + \text{Year} + e_t \quad (7)$$

where  $\ln \text{AuditFee}$  is the natural logarithm of audit fees. Again,  $\text{KPMG}_{i,t}$  is an indicator variable coded as one if KPMG is the auditor of client  $i$  in year  $t$ . It captures whether KPMG clients, on average, pay more or less fees than all other audit clients. We interact this variable with indicators for 2014 and 2017. The interactions capture any incremental fee effect for NAS scandal-affected KPMG clients after controlling for the average fee effect of being a KPMG clients (*KPMG*) and the following *Controls*, which we take from prior literature ([Hay, 2013](#)): *Size*, profitability (*ROA* and *Loss*), inherent risk measures (*Growth*, [Altman's \(1993\) ZScore](#), inventory and receivables, *InvRec*), liquidity (*Leverage* and cash flow, *CFO*), complexity (*MTB*), and auditor characteristics (*Big4*, auditor's market share in the client industry; *IndShare*, and *Assurance*, *Advisory*, and *Tax* NAS provided to the client). We have 2,437 firm-year observations from 2012 to 2018 in this analysis.

[Table 5](#) presents the results from estimating Eq. (7). The main effect of KPMG and the interactions with our treatment years are all insignificant. Based on these results, pricing effects seem unable to explain the negative capital market reactions we observe. Regarding RQ1, we do not find evidence for any direct adverse consequences for KPMG. While our tests of H1 suggest that market actors indirectly penalize KPMG by negative abnormal returns for its clients, we do not find support for any reactions by KPMG's audit clients. This indicates that less powerful actors, such as retail investors, bear the adverse consequences of these failures, while the

**Table 5**  
OLS-regression of the natural logarithm of Audit Fees.

	Model 1 DV = Ln_Audit_Fee
	Coefficient (t-statistic)
Intercept	-1.128*** (-4.724)
KPMG	-0.015 (-0.229)
KPMG • 2014	0.013 (0.210)
KPMG • 2017	0.036 (0.728)
Size	0.462*** (27.949)
ROA	-0.349 (-1.380)
Growth	-0.083** (-2.443)
Leverage	0.319** (2.232)
Loss	0.098* (1.940)
CFO	0.220 (0.917)
MTB	0.013 (1.456)
ZScore	-0.015** (-1.994)
InvRec	0.121 (0.835)
Big4	0.108* (1.859)
Assurance	<0.001*** (4.760)
Advisory	<0.001*** (2.853)
Tax	0.001*** (3.959)
IndShare	0.531*** (4.070)
Industry	Yes
Year	Yes
N	2,437
Adjusted R <sup>2</sup>	0.861
F-statistic	408.30***

Notes: Model 1 presents OLS regression results from estimating Eq. (7). We winsorize all continuous variables at the first and 99th percentile and compute robust standard errors clustered by client. We define industry based on the 16 sector indices of Frankfurt Stock Exchange and manually backfill missing industries based on SIC codes, if available. \*, \*\*, and \*\*\* denotes significance at the 10 %, 5 %, and 1 %-level, respectively, of two-tailed tests. Variable definitions are in [Appendix A](#).

powerful Big Four may not experience direct consequences, although they are responsible for the events.

Why this is the case, i.e., RQ2, is still unanswered. As discussed in [Section 2.2](#), in oligopolistic audit markets, individual auditor–client pairings may entail economic benefits that are more meaningful to clients than potential signals of lower-quality services from the reputational spillover. Hence, the market structure may systematically put the burden of small changes in quality on shareholders. Alternatively, auditors may acquiesce to clients' opportunistic accounting choices, i.e., give up independence, resulting in *factual* audit quality reductions. We explore factual audit quality with our last OLS regression:

$$Accruals_{i,t} = \alpha + \beta_1 KPMG_{i,t} + \beta_2 KPMG_{i,t} \bullet 2014_i + \beta_3 KPMG_{i,t} \bullet 2016_i + \beta_k Controls_{i,t} + Industry + Year + e_t \quad (8)$$

where *Accruals* represents either discretionary accruals (*DiscAcc*), abnormal working capital accruals (*AWCA*), or their absolute values

( $|DiscAcc|$  and  $|AWCA|$ ). We calculate AWCA following DeFond and Park (2001, see Appendix A for details) and estimate  $DiscAcc$  with the performance-adjusted modified Jones Model (Kothari et al., 2005).<sup>21</sup> Again,  $KPMG_{i,t}$  is an indicator variable coded as one if the KPMG is the auditor of client  $i$  in year  $t$ . It captures whether KPMG clients, on average, manage earnings more or less than all other audit clients. We interact this variable with indicators for 2014 and 2016. The interactions capture any incremental earnings management effect for NAS scandal-affected KPMG clients after controlling for the average earnings management effect of being a KPMG clients ( $KPMG$ ) and the following *Controls* (e.g., Francis & Wang, 2008; Mohrmann, 2017): *Size*, *Growth* and increases in sales (*SalesGrowth*), prior year profitability (*ROA*, *Loss*), *Leverage*, *MTB*, *ZScore*, *Big4*, the sum of NAS fees over audit fees (*NASRatio*), and *IndShare*.

Table 6 shows the results of our accruals models with 2,405 firm-year observations between 2012 and 2018. Models 1 and 2 present results when estimating Eq. (8) with signed and absolute discretionary accruals. Models 3 and 4 depict results with signed and absolute

**Table 6**  
OLS-regression of accruals-based earnings management.

	Model 1 DV = $Disc\_Acc$	Model 2 DV = $ Disc\_Acc $	Model 3 DV = $AWCA$	Model 4 DV = $ AWCA $
	Coefficient (t-statistic)	Coefficient (t-statistic)	Coefficient (t-statistic)	Coefficient (t-statistic)
<i>Intercept</i>	0.004 (0.256)	0.092*** (6.957)	0.025 (1.248)	0.125*** (5.229)
<i>KPMG</i>	-0.002 (-0.481)	0.004 (1.092)	0.005 (0.718)	0.002 (0.358)
<i>KPMG • 2014</i>	-0.007 (-0.748)	-0.006 (-0.703)	-0.003 (-0.131)	0.022 (1.530)
<i>KPMG • 2016</i>	0.016** (1.985)	-0.004 (-0.903)	-0.011 (-0.737)	0.004 (0.443)
<i>Size</i>	<0.001 (0.399)	-0.003*** (-3.623)	<0.001 (0.402)	-0.008*** (-4.707)
<i>ROA</i>	0.264*** (5.556)	0.027 (1.070)	0.250*** (3.069)	0.035 (0.700)
<i>SalesGrowth</i>	0.029** (2.562)	-0.008 (-1.150)	-0.063* (-1.826)	0.033* (1.908)
<i>Growth</i>	0.013 (1.630)	0.016*** (3.176)	0.039** (2.002)	0.068*** (8.548)
<i>Leverage</i>	-0.001 (-0.049)	0.001 (0.122)	-0.026 (-1.342)	0.017 (0.919)
<i>Loss</i>	0.005 (0.770)	0.015 (2.956)	0.021** (2.118)	0.026*** (3.018)
<i>CFO</i>	-0.357*** (-9.116)	-0.024 (-1.025)	-0.228*** (-3.875)	-0.074** (-2.131)
<i>MTB</i>	-0.001 (-1.066)	0.001 (1.556)	0.001 (0.759)	0.002 (1.350)
<i>ZScore</i>	0.001* (1.753)	>-0.001 (-0.564)	-0.001 (-0.520)	0.002 (1.540)
<i>Big4</i>	-0.006 (-1.111)	-0.002 (-0.458)	<0.001 (0.053)	-0.001 (-0.220)
<i>NASRatio</i>	-0.010*** (-2.822)	0.004 (1.434)	<0.001 (0.028)	-0.002 (-0.540)
<i>IndShare</i>	0.010 (1.160)	-0.002 (-0.271)	0.007 (0.700)	0.010 (0.862)
<i>Industry</i>	Yes	Yes	Yes	Yes
<i>Year</i>	Yes	Yes	Yes	Yes
<i>N</i>	2,405	2,405	2,405	2,405
<i>Adjusted R<sup>2</sup></i>	0.247	0.121	0.068	0.209
<i>F-statistic</i>	23.49***	10.45***	5.98***	19.13***

Notes: Models 1, 2, 3, and 4 present OLS regression results from estimating Eq. (8) with  $DiscAcc$ ,  $|DiscAcc|$ ,  $AWCA$ , and  $|AWCA|$  as the dependent variable, respectively. We winsorize all continuous variables at the first and 99th percentile and compute robust standard errors clustered by client. We define industry based on the 16 sector indices of Frankfurt Stock Exchange and manually backfill missing industries based on SIC codes, if available. \*, \*\*, and \*\*\* denotes significance at the 10%, 5%, and 1%-level, respectively, of two-tailed tests. Variable definitions are in Appendix A.

<sup>21</sup> Specifically, we estimate the following OLS regression separately for each subsample containing observations of the same industry-year and use the residuals as our variable  $DiscAcc$ . For variable descriptions, see Appendix A.  $TotalAcc_{i,t} = \beta_0 + \beta_1(1/Assets_{i,t-1}) + \beta_2(\Delta SalesRec_{i,t}) + \beta_3PPE_{i,t} + \beta_4ROA_{i,t-1} + \varepsilon_{i,t}$ . In untabulated tests, we use a one-stage OLS regression with Total Accruals as our dependent variable (Chen et al., 2018). Inferences are identical to using  $DiscAcc$  as a dependent variable in our two-stage regressions.

abnormal working capital accruals. The main effect of *KPMG* is not significant in any of the models, suggesting that there are no differences in earnings management between *KPMG* clients and an average client. Turning to our variables of interest, Models 1 and 2 show that, while *KPMG • 2014* is insignificant for both measures, *KPMG • 2016* is positive and significant for signed discretionary accruals but insignificant for absolute discretionary accruals. Our variables of interest are insignificant throughout Models 3 and 4 of Table 6. This suggests that, for the *Hahn* event only, *KPMG* clients might have a tendency to disclose accruals that are overall more income-increasing, but the absolute degree of using accruals for earnings management does not differ from the average client.<sup>22</sup> Hence, we do not find consistent effects on earnings management but some indication that it might become more aggressive. This could be consistent with a strategy of *KPMG* to please clients in an attempt to mitigate negative consequences from NAS failures. Regarding RQ2, we find some limited indication that the adverse consequences for shareholders might stem from *KPMG*'s tendency to give up auditor independence and conduct audits of lower (factual) quality. Nonetheless, we cannot rule out that mere decreases in perceived audit quality without major changes to factual audit quality may drive investor reactions.

## 5. Conclusion

We analyze two events of observable non-audit service deficiencies of *KPMG* Germany to study potential spillovers to the audit practice in a low litigation setting. An event study shows that *KPMG*'s audit clients suffer negative capital market reactions after the events. We then follow the Eisenhardt Method to develop theory on the mechanisms behind those capital market reactions. Specifically, we aim to understand who bears the consequences of the adverse events. Rich quantitative data sets allow us to explore auditor switches, audit pricing, and client's earnings management. The analyses suggest that shareholders of *KPMG* audit clients bear the costs through negative market reactions, while *KPMG*'s position in the audit market for listed companies seems unchanged. Moreover, we mostly find insignificant results regarding accruals-based earnings management of *KPMG* audit clients, a measure of *factual* audit quality. We only find some indication of a tendency of *KPMG* clients to manage accruals more aggressively. If shareholders are aware that *KPMG* clients might behave that way, expectations of aggressive earnings management might contribute to negative market reactions.

Our paper contributes to bridging critical accounting research, studying who benefits and suffers from the actions of powerful institutions in an accounting context, and emerging auditing literature, extending the reputation hypothesis beyond the mere auditing context. The low litigation setting allows us to study how the lack of disciplinary mechanisms other than reputation spillovers and Big Four commercialism represented by NAS interact to create a setting where less powerful actors may suffer adverse consequences from Big Four behavior. Auditors might prefer giving up some independence after NAS failures, and shareholders seem to bear the cost through declining stock prices. This may be relevant for researchers and regulators, as earlier research has shown that reputation may substitute for lack of litigation in a mere audit setting, but our results indicate that it does not substitute for lack of litigation in a setting that considers all services of an accounting firm.

Our paper has some limitations, which indicate future research opportunities. First, some of our robustness tests of the short-term event study indicate that results for the *Hahn* event are sensitive to some design choices. Besides, we are unable to identify subsamples with particularly pronounced abnormal returns. Together, this raises the question of whether all clients and their investors equally share the consequences of NAS failures or whether certain subgroups are more or less susceptible to market consequences. Future research could identify more events or additional cross-sectional variation to study subsamples with presumably larger effects to explore the theoretical and empirical boundaries of the results.

Second, as discussed above, the German setting may bias against strong reputation spillovers for auditors because ownership is generally concentrated in the hands of families, banks, and companies with insider knowledge and monitoring power. Their involvement in boards and audit committees may reduce information asymmetries and power imbalances. Moreover, although our search procedure showed that an extensive search only produced two clean events, the events are of limited scope and may be less severe and prominent than those used in earlier literature. Hence, our events may not be severe enough to influence the decisions of actors with some power. This could explain why short-term capital market reactions, potentially affecting the least powerful investors, are not followed by changes in the audit market, where more powerful actors interact. Therefore, we cannot make firm conclusions about the boundaries of our theory-building endeavor. Future research could identify more severe events of NAS failure in a low litigation environment to provide additional analyses.

Finally, reputation concerns in all areas of an organization become increasingly prevalent, e.g., with the rise of individualized modern societies and social media (Brivot et al., 2017). Organizational and even regulatory focus on reputation management can have profound consequences, e.g., because auditor reporting may become overly defensive and largely uninformative (Power, 2004). These important concerns go beyond our analyses, and we encourage future research to explore possible adverse consequences of reputational risks and reputation risk management for auditors.

## Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

<sup>22</sup> Additional tests reveal that results are somewhat sensitive to using client fixed effects and become stronger if we only use retained *KPMG* clients (i.e., excluding first-year *KPMG* clients) or a different treatment year for the *Hahn* event. See Appendix D for details.

## Data availability

Data will be made available on request.

## Acknowledgements

We acknowledge valuable comments received from participants at the doctoral seminar on Empirical Research on Corporate Governance 2019 in Darmstadt, the 2019 Auditing section midyear meeting in Nashville, the EAA 42nd annual congress 2019 in Paphos, and seminar participants at the University of Malaga 2019. We also thank Raymond Wolf for his contribution to early analyses that led to the development of this paper.

## Appendix A

### Variable definitions

Variable	Definition
<i>Auditorchg</i>	Dummy variable, coded as 1 if a firm is audited by a new auditor for the first time, and 0 otherwise
<i>LagKPMG</i>	Dummy variable, coded as 1 if a firm has been audited by KPMG in the prior year, and 0 otherwise
<i>KPMG</i>	Dummy variable, coded as 1 if a firm is audited by KPMG, and 0 otherwise
<i>Size</i>	Natural logarithm of (1 + total assets)
<i>Growth</i>	Total assets minus lagged total assets, scaled by lagged total assets
<i>Leverage</i>	Total debt scaled by total assets
$\Delta$ <i>Leverage</i>	Leverage minus lagged leverage
<i>ROA</i>	Return on assets, measured as net income scaled by the average of total assets and lagged total assets
<i>Loss</i>	Dummy variable, coded as 1 if net income is negative, and 0 otherwise
<i>LnAuditFee</i>	Natural logarithm of (1 + audit fees)
<i>CFO</i>	Cash flow from operations scaled by total assets
<i>MTB</i>	Market value of equity scaled by book value of equity
<i>ZScore</i>	$1.2 \bullet (\text{current assets} - \text{short-term debt}) / \text{total assets} + 1.4 \bullet \text{retained earnings} / \text{total assets} + 3.3 \bullet \text{EBIT} / \text{total assets} + 0.6 \bullet \text{Market value of equity} / \text{total liabilities} + 0.999 \bullet \text{sales} / \text{total assets}$
<i>InvRec</i>	Inventories + receivables, scaled by total assets
<i>Big4</i>	Dummy variable, coded as 1 if a firm is audited by a Big Four auditor, and 0 otherwise
<i>Assurance</i>	Fees paid to the auditor for other assurance services
<i>Advisory</i>	Fees paid to the auditor other services
<i>Tax</i>	Fees paid to the auditor for tax services
<i>IndShare</i>	Sum of audit fees for all available observations audited by a given auditor in an industry-year, scaled by sum of audit fees from all available observations in an industry-year
<i>TotalAcc</i>	Total accruals, measured as cash flow from operations minus net income, scaled by lagged total assets
<i>1/Assets</i>	Inverse of lagged total assets
$\Delta$ <i>SalesRec</i>	Difference of total revenues minus lagged total revenues and total receivables minus lagged total receivables, scaled by lagged total assets
<i>PPE</i>	Property, plant, and equipment scaled by lagged total assets
<i>AWCA</i>	Abnormal working capital accruals, measured as (current assets – current liabilities + short-term debt – cash) – (lag of (current assets – current liabilities + short-term debt – cash) scaled by sales over lagged sales). The result is scaled by lagged total assets
$ AWCA $	Absolute value of abnormal working capital accruals
<i>DiscAcc</i>	Discretionary accruals, residual from estimating a performance-adjusted Jones Model as described above
$ DiscAcc $	Absolute value of discretionary accruals
<i>SalesGrowth</i>	Sales minus lagged sales, scaled by lagged sales
<i>NASRatio</i>	Sum of Assurance, Consulting, and Tax, scaled by audit fees
<i>Industry</i>	Dummies for a firm's industry based on the 16 sector indices of Frankfurt Stock Exchange. Note that we use only 15 dummies, with the 16th industry serving as the baseline when all 15 dummies are zero
<i>Year</i>	Dummies for fiscal years. Note that we use only six dummies, with the seventh year serving as the baseline when all six dummies are zero

## Appendix B

### Timeline of events and analyses

Fig. B.1 summarizes the timeline of events described above and puts it in context of our analyses to understand which observations fit best in each analysis.

Starting with the *Ring* event, to use an event study to analyze reputation effect of the NAS failure on KPMG's audit practice on November 29, 2013, we need to identify all KPMG audit clients. In Germany, the annual shareholder meeting elects the financial

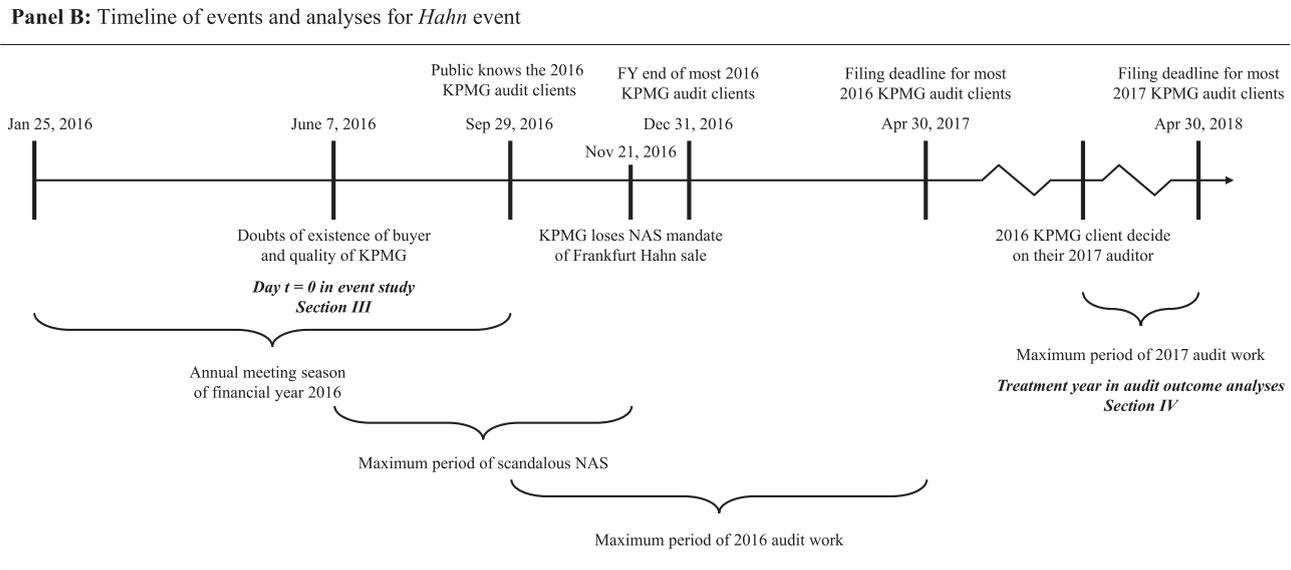
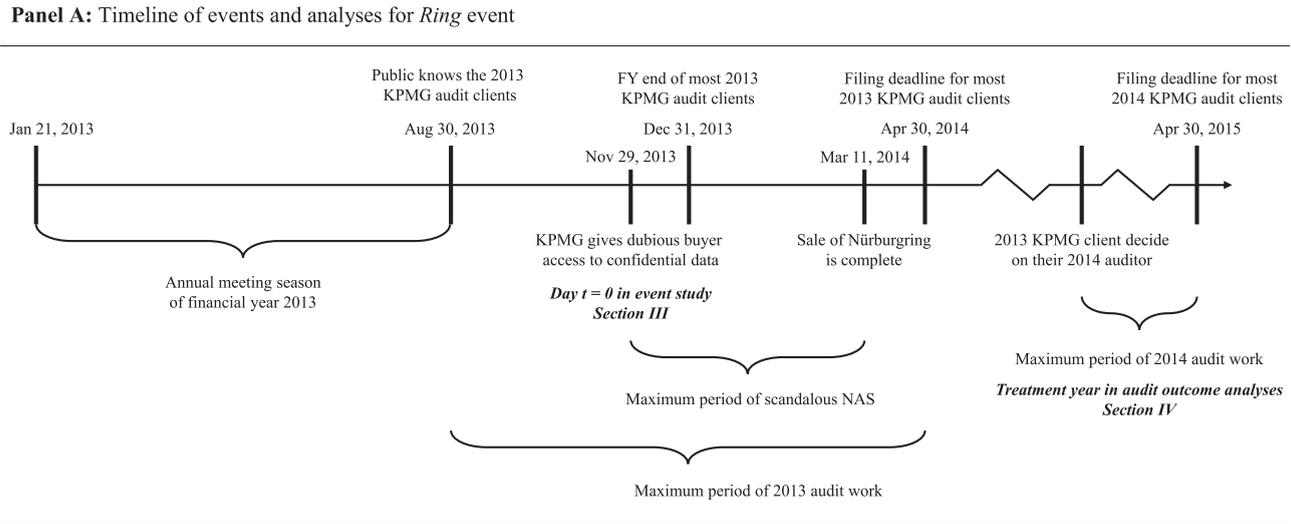


Fig. B.1. Timeline of events.

statement auditor for the current financial year. As shown in Panel A of Fig. B.1, all KPMG clients in our sample held their annual meeting between January 21 and August 30, 2013. Hence, the public knew they were KPMG clients when they potentially reacted to the news on November 29, 2013. Our sample for the short-term event study comprises all 2013 KPMG audit clients.<sup>23</sup> For the models we analyze after the event study in the context of the Eisenhardt Method, we consider two dynamics. First, we estimate whether affected KPMG clients (in this case, the 2013 KPMG clients) choose to change to a different auditor (in this case, the first opportunity is the choice of the 2014 auditor). The first opportunity to change auditors is also the first opportunity to negotiate different audit fees, which may result in changes in audit fees paid for 2014 KPMG audits. Second, changes in the audit process, potentially manifesting more quickly than changes in auditor choice or audit fee negotiations, may lead to changes in audit quality. The earliest possible start of the audit process is when the client selects the auditor at the annual meeting. However, the auditor likely performs most fieldwork after the financial year-end, which is December 31 for most KPMG clients. The auditor has to finish by the filing deadline, which is April 30 for listed clients (Section 325 (4) HGB). For the scandal to influence the audit process, its effects would have to hit substantially before it is completed. In this case, the remaining 2013 audit work, if any, was limited when the scandalous NAS period ended on March 11, 2014. Therefore, the first audit process that is likely to see any influence from the scandal is the 2014 financial statements audit.

Turning to the Hahn event, as Panel B of Fig. B.1 indicates, on June 7, 2016, a considerable part (one-third, or 20 of 60 meetings) of the annual meeting season for 2016 auditor selections was still outstanding. However, auditor–client relationships are sticky, and investors know of the likely election of the auditor from the invitation to the annual meeting, which contains a suggestion and arrives many weeks before the annual meeting. Therefore, we use all 2016 KPMG audit clients for our event study.<sup>24</sup> Accordingly, for our auditor change and audit fee analyses, we consider changes from 2016 KPMG clients to their 2017 auditor and 2017 audit fee negotiations, respectively. However, looking at the audit process and audit quality, as becomes clear in Panel B of Fig. B.1, the proportion of 2016 audit work left after the scandalous NAS period ended was substantial. Most importantly, it ended before the financial year-end of 2016, so that most 2016 audit work was probably outstanding. Therefore, we perform our audit quality analysis for all 2016 KPMG audit clients and use the 2017 KPMG audit clients as an alternative sample in an additional analysis (see Appendix D).

## Appendix C

### Event study results for subsamples

In addition to our full sample, we construct two subsamples with arguably closer proximity to the NAS scandal. Assuming that this proximity increases the strength of the reputation signal, we would expect stronger reactions in these subsamples. The first subsample includes all KPMG audit clients that also purchase NAS from KPMG. Their investors may pay closer attention to other KPMG NAS practices. The number of firms decreases from 58 (60) to 52 (45) for the *Ring* event (*Hahn* event). The second subsample comprises all clients from KPMG offices that are geographically close to the location of the NAS failure event. Investors of geographically closer companies may be more aware of local news. We chose a 250 km radius to achieve a sufficiently large subsample. After all selection criteria, this leaves clients from KPMG offices in Düsseldorf, Essen, Frankfurt am Main, Köln, and Mannheim.<sup>25</sup> The number of firms decreases from 58 (60) to 19 (19) for the *Ring* event (*Hahn* event).

Table C1 reports the proportion of negative returns and mean returns for market-adjusted returns (the simple difference  $R_{i,t} - R_{m,t}$ ) and abnormal returns (market model in Eqs. (1) and (2)) for the two subsamples.

For the NAS subsample in Panel A of Table C1, results compared to Table 2 become stronger for the *Ring* event (average abnormal return:  $-0.871\%$ ) but weaker and statistically insignificant for the *Hahn* event ( $-0.284\%$ ). Results for the office sample in Panel B of Table C1 are slightly weaker for the *Ring* event ( $-0.444\%$ ) but substantially stronger for the *Hahn* event ( $-0.781\%$ ). Overall, despite some instances of stronger effects compared to our full sample, our subsample results are inconclusive.

<sup>23</sup> We assume that the negative reputation effect leads to capital market reactions because investors anticipate negative effects from having a less reputable auditor for the upcoming audit. Alternatively, we may assume that they still use the latest available audited information, in this case, the 2012 financial year, and care about the reputation of the respective auditor. In this case, an analysis of 2012 KPMG clients would be more appropriate. Repeating our main tests with the 2012 KPMG clients does not change our inferences.

<sup>24</sup> Repeating our main tests with all 2015 KPMG clients does not change our inferences.

<sup>25</sup> No clients from KPMG offices in Dortmund and Saarbrücken survived all selection criteria.

**Table C1**  
Market reaction to NAS failure events for German KPMG clients.

Panel A: Market reactions for the NAS sample				
	Percent negative	Mean	t-statistic	Corrado-statistic
<i>Ring event (November 29, 2013)</i>				
<i>Market-adjusted return</i>	85 %	<i>N</i> = 52 -1.066 %	-5.363***	-2.244**
<i>Market model</i>	79 %	-0.871 %	-4.455***	-2.276**
<i>Hahn event (June 7, 2016)</i>				
<i>Market-adjusted return</i>	78 %	<i>N</i> = 45 -0.982 %	-3.792***	-1.509*
<i>Market model</i>	64 %	-0.284 %	-1.158	-0.752
Panel B: Market reactions for the Office sample				
	Percent negative	Mean	t-statistic	Corrado-statistic
<i>Ring event (November 29, 2013)</i>				
<i>Market-adjusted return</i>	89 %	<i>N</i> = 19 -1.154 %	-1.585*	-1.709**
<i>Market model</i>	79 %	-0.444 %	-0.575	-1.898**
<i>Hahn event (June 7, 2016)</i>				
<i>Market-adjusted return</i>	95 %	<i>N</i> = 19 -1.975 %	-6.991***	-2.025**
<i>Market model</i>	68 %	-0.781 %	-3.232***	-1.140

Notes: This table presents sample sizes, the proportion of negative returns, mean returns, and test statistics from t-tests and Corrado-tests that the returns are different from zero for two events and two subsamples. Panel A for a sample of audit clients that also purchase NAS and Panel B for a sample of firms audited by KPMG offices in Düsseldorf, Essen, Frankfurt am Main, Köln, and Mannheim.

Events are as follows:

*Ring event*: November 29, 2013; KPMG gave a dubious potential buyer access to confidential data.

*Hahn event*: June 7, 2016; First press releases, which doubt the existence of SYT and the quality of KPMG's integrity due diligence investigation.

\*, \*\*, and \*\*\* denotes significance at the 10 %, 5 %, and 1 %-level, respectively, of a one-tailed test that mean returns are negative.

**Table C2**  
Robustness checks of market reaction to NAS failure events.

Panel A: Alternative (cumulative) abnormal return metrics				
	<i>Ring event (November 29, 2013)</i>		<i>Hahn event (June 7, 2016)</i>	
	NAS sample	Office sample	NAS sample	Office sample
	Mean	Mean	Mean	Mean
	(t-statistic)	(t-statistic)	(t-statistic)	(t-statistic)
<i>Fama French three factor model</i>	-0.864 %*** (-4.405)	-0.445 % (-0.582)	-0.106 % (-0.427)	-0.768 %*** (-3.012)
<i>Market model with CDAX as market portfolio</i>	-0.623 %*** (-3.139)	-0.174 % (-0.229)	-0.160 % (-0.658)	-0.184 % (-0.750)
<i>Market model and (-1,1) window</i>	-0.565 %* (-1.558)	0.107 % (0.245)	-0.626 %** (-1.810)	-1.123 %*** (-4.009)
<i>Market model and (-10,10) window</i>	-2.901 %*** (-3.597)	-0.845 % (-0.549)	-2.339 %*** (-3.558)	-2.987 %*** (-3.328)
Panel B: Schipper and Thompson regression				
	Model 1	Model 2	Model 3	Model 4
	Coefficient	Coefficient	Coefficient	Coefficient
	(t-statistic)	(t-statistic)	(t-statistic)	(t-statistic)
<i>Intercept</i>	<0.001* (1.806)	<0.001 (1.010)	0.001** (2.089)	0.001* (1.829)
<i>Return<sub>Matched Portfolio</sub></i>	0.709*** (20.521)	0.486*** (11.698)	0.718*** (25.544)	0.490*** (17.742)
<i>Ring event</i>	-0.009** (-2.191)	-0.009* (-1.398)		
<i>Hahn event</i>			-0.004 (-0.833)	-0.006 (-1.085)
<i>N</i>	253	253	253	253
<i>Adj. R<sup>2</sup></i>	0.625	0.721	0.349	0.555

Notes: Panel A of this table contains mean returns and corresponding test statistics from a t-test (in brackets) for alternative return metrics and the two subsamples described in Table C.1. Panel B contains coefficients and t-statistics (in brackets) from OLS regressions of Eq. (4) with the *Ring event* and *NAS* and office subsamples in models 1 and 2, and with the *Hahn event* and *NAS* and office subsamples in models 3 and 4, respectively.

Events are as follows:

*Ring event*: November 29, 2013; KPMG gave a dubious potential buyer access to confidential data.

*Hahn event*: June 7, 2016; First press releases, which doubt the existence of SYT and the quality of KPMG's integrity due diligence investigation.

### Robustness checks

Panel A of [Table C2](#) reports mean abnormal or mean cumulative abnormal returns with  $t$ -statistics based on several different design choices. The first row shows the  $t = 0$  abnormal return when adding three variables of the Fama and French four-factor model (the fourth factor is  $R_{m,t}$ ) to the right-hand side of Eq. (1). The second row shows the  $t = 0$  abnormal return when replacing  $R_{m,t}$  with the CDAX index. The columns show our two subsamples for each event. Results remain largely robust for the Fama and French four-factor model and, as expected, become weaker for the CDAX as the market portfolio. The third and fourth row show cumulative abnormal returns for the  $(-1,1)$ - and  $(-10,10)$ -windows. We find robust results in both windows.

Panel B of [Table C2](#) reports the results from using the portfolio OLS regression from Eq. (4). Effects are robust for the *Ring* event but not for the *Hahn* event.

## Appendix D

### Details of regression analysis

#### Estimation details and sample selection

We closely follow prior literature to build our client switching, audit fee, and earnings management models, which we report in Section 4.2. All estimations use client-year observations as the unit of analysis. [Appendix A](#) contains variable definitions. To reduce the influence of extreme values in continuous variables, we winsorize all continuous variables at the first and 99th percentile (i.e., for each variable separately, we replace values above the 99th percentile with the 99th percentile and values below the first percentile with the first percentile). We compute heteroskedasticity-robust standard errors, i.e., allow values of the variance-covariance matrix' diagonal to vary and use software to estimate these values. We also cluster standard errors by clients, i.e., allow covariances between observations from the same client to differ from zero, again using software to estimate these values.

We start our sample selection for all three types of analyses with 6,072 firm-year observations from all active listed German firms from 2012 to 2018. We start in 2012 to have one full financial year without any possible influence of our events before the *Ring* event late in 2013. For all samples, we exclude all financial firms, because they have different financial reporting formats, which make their data incompatible, and firms for which we have no industry classification (2,481 observations). We drop all observations with non-positive equity (486) and no identifiable auditor (119). Finally, we exclude 70 observations with missing data and 28 firms with only one firm-year in our dataset (no switching observable) from the switching sample, 549 observations with missing data from the fee sample, and 581 observations with missing data from the accruals sample. This leaves us with 2,888 observations in the switching sample, 2,437 observations in the fee sample, and 2,405 observations in the accruals sample.

#### Descriptive statistics

[Table D1](#) presents summary statistics for the three groups of analysis in Panels A, C, and E and correlation tables in Panels B, D, and F. KPMG audits between 15 % and 16 % of all firm-years, a fourth of the 60 % of firm-years audited by any Big Four. Those are evenly distributed across the seven sample years so that around 2.2 % of our observations assume the value of one for our interaction terms. The control variables behave similarly across all three samples.

Bivariate correlations between our variables of interest and *Auditorchg* are insignificant. Correlations with *LnAuditFee* are significantly positive but relatively small. Moreover, the variables of interest also correlate positively with *Size*, which, in turn, correlates positively and strongly with *LnAuditFee*. The only significant correlation between our variables of interest and accruals measures is a small positive correlation between *KPMG • 2014* and  $|AWCA|$ . Accounting literature typically uses variance inflation factors as an indicator of multicollinearity, where small values (especially below 5) indicate low likelihoods of multicollinearity. All variance inflation factors in our regressions are below 3.1.

#### Additional analyses

Because auditor-client pairings and audit fees are sticky, it may take multiple periods until a reputation effect is observable in auditor changes or changes in audit fees ([Boone et al., 2015](#)). However, in our case, we have two events that are somewhat close together in time. Thus, using a two-year treatment period instead of the one-year period from our main tests, for instance, the treatment period for the *Ring* event is 2014–2015 and for the *Hahn* event 2017–2018 (or 2016–2017 in the audit quality analysis). This considerably reduces our control periods and removes a control period of KPMG clients directly before the *Hahn* event in the audit quality analysis. Overall, this biases against results. In untabulated tests, we repeat our analyses with different combinations of treatment and control periods. We also exclude observations from event years altogether in some cases, and use the years thereafter as the treatment period, to exclude potential bias introduced by unobserved turmoil in these years. We find significant results, all indicating potential negative audit quality effects, in the following cases (all untabulated): when defining 2014–2015 and 2016–2017 as separate treatment periods (significant with dependent variables  $|AWCA|$  and *Disc\_Acc*); when defining 2014–2017 as the only treatment period (significant with  $|AWCA|$  and *Disc\_Acc*); when defining 2014–2018 as the only treatment period (significant with  $|AWCA|$  and *Disc\_Acc*); and when defining 2014 and 2017 as the treatment periods and leaving out 2013 and 2016 altogether (significant with *AWCA* and *Disc\_Acc*).

We conduct three more sets of additional tests for our accruals models with single treatment years to explore whether the indication of an effect substantiates in different designs. Inconsistently significant coefficients could simply be a false positive (alpha error) result

**Table D1**  
Descriptive statistics for additional analyses.

Panel A: Summary statistics of the sample used in the auditor change analysis								
	N	Mean	Standard deviation	Min	25th percentile	median	75th percentile	Max
<i>Auditorchg</i>	2,888	0.088	0.283	0.000	0.000	0.000	0.000	1.000
<i>LagKPMG</i>	2,888	0.154	0.361	0.000	0.000	0.000	0.000	1.000
<i>LagKPMG · 2014</i>	2,888	0.021	0.143	0.000	0.000	0.000	0.000	1.000
<i>LagKPMG · 2017</i>	2,888	0.022	0.146	0.000	0.000	0.000	0.000	1.000
<i>KPMG</i>	2,888	0.149	0.356	0.000	0.000	0.000	0.000	1.000
<i>KPMG · 2014</i>	2,888	0.021	0.143	0.000	0.000	0.000	0.000	1.000
<i>KPMG · 2017</i>	2,888	0.021	0.144	0.000	0.000	0.000	0.000	1.000
<i>Size</i>	2,888	12.294	2.444	6.718	10.571	12.038	13.875	18.653
<i>Growth</i>	2,888	0.175	0.492	-0.626	-0.025	0.090	0.239	3.615
<i>Leverage</i>	2,888	0.517	0.213	0.026	0.376	0.529	0.687	0.936
<i>ΔLeverage</i>	2,888	0.036	0.165	-0.441	-0.026	0.019	0.074	0.924
<i>ROA</i>	2,888	0.022	0.125	-0.665	0.005	0.036	0.071	0.304
<i>Loss</i>	2,888	0.211	0.408	0.000	0.000	0.000	0.000	1.000

Panel B: Correlations of the sample used in the auditor change analysis												
	(1)	(2)	(3)	(4)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
1 <i>Auditorchg</i>	-											
2 <i>LagKPMG</i>	-0.030	-										
3 <i>LagKPMG · 2014</i>	-0.028	<b>0.342</b>	-									
4 <i>LagKPMG · 2017</i>	-0.004	<b>0.350</b>	-0.022	-								
5 <i>KPMG</i>	<b>-0.075</b>	<b>0.934</b>	<b>0.334</b>	<b>0.323</b>	-							
6 <i>KPMG · 2014</i>	-0.028	<b>0.328</b>	<b>0.966</b>	-0.022	<b>0.348</b>	-						
7 <i>KPMG · 2017</i>	-0.020	<b>0.325</b>	-0.021	<b>0.934</b>	<b>0.351</b>	-0.021	-					
8 <i>Size</i>	<b>-0.114</b>	<b>0.269</b>	<b>0.087</b>	<b>0.104</b>	<b>0.276</b>	<b>0.087</b>	<b>0.108</b>	-				
9 <i>Growth</i>	<b>0.051</b>	-0.029	-0.020	-0.028	-0.035	-0.014	-0.024	-0.016	-			
10 <i>Leverage</i>	-0.019	<b>0.104</b>	<b>0.041</b>	<b>0.041</b>	<b>0.118</b>	<b>0.048</b>	<b>0.043</b>	<b>0.307</b>	<b>-0.081</b>	-		
11 <i>ΔLeverage</i>	0.001	-0.022	-0.009	-0.018	-0.018	-0.011	-0.015	-0.027	<b>0.548</b>	-0.005	-	
12 <i>ROA</i>	-0.033	<b>0.039</b>	-0.001	0.030	<b>0.042</b>	0.003	0.032	<b>0.214</b>	0.011	<b>-0.075</b>	<b>-0.170</b>	-
13 <i>Loss</i>	0.035	<b>-0.058</b>	0.002	-0.036	<b>-0.066</b>	-0.010	<b>-0.046</b>	<b>-0.244</b>	<b>-0.044</b>	<b>0.040</b>	<b>0.093</b>	<b>-0.651</b>

Panel C: Summary statistics of the sample used in the audit fee and NAS fee analysis								
	N	Mean	Standard deviation	Min	25th percentile	median	75th percentile	Max
<i>LnAuditFee</i>	2,437	5.511	1.393	2.944	4.543	5.288	6.275	9.680
<i>KPMG</i>	2,437	0.160	0.366	0.000	0.000	0.000	0.000	1.000
<i>KPMG · 2014</i>	2,437	0.023	0.151	0.000	0.000	0.000	0.000	1.000
<i>KPMG · 2017</i>	2,437	0.022	0.147	0.000	0.000	0.000	0.000	1.000
<i>Size</i>	2,437	12.646	2.318	6.718	11.041	12.316	14.167	18.653
<i>ROA</i>	2,437	0.027	0.117	-0.665	0.008	0.040	0.072	0.304
<i>Growth</i>	2,437	0.176	0.466	-0.626	-0.014	0.095	0.236	3.615
<i>Leverage</i>	2,437	0.529	0.198	0.026	0.400	0.535	0.687	0.936
<i>Loss</i>	2,437	0.192	0.394	0.000	0.000	0.000	0.000	1.000
<i>CFO</i>	2,437	0.075	0.137	-0.588	0.030	0.081	0.129	0.472
<i>MTB</i>	2,437	2.687	2.539	0.299	1.159	1.877	3.139	14.429
<i>ZScore</i>	2,437	4.419	4.151	-3.965	2.386	3.449	4.941	27.607
<i>InvRec</i>	2,437	0.355	0.184	0.005	0.217	0.349	0.481	0.860
<i>Big4</i>	2,437	0.607	0.489	0.000	0.000	1.000	1.000	1.000
<i>Assurance</i>	2,437	196.245	788.781	0.000	0.000	0.000	42.000	6000.000
<i>Advisory</i>	2,437	146.511	511.852	0.000	0.000	6.000	51.000	4000.000
<i>Tax</i>	2,437	68.340	181.693	0.000	0.000	0.000	42.000	1119.560
<i>IndShare</i>	2,437	0.189	0.243	0.000	0.014	0.081	0.255	0.986

Panel D: Correlations of the sample used in the audit fee and NAS fee analysis																	
	(1)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
1 <i>LnAuditFee</i>	-																
3 <i>KPMG</i>	<b>0.267</b>	-															
4 <i>KPMG · 2014</i>	<b>0.091</b>	<b>0.355</b>	-														
5 <i>KPMG · 2017</i>	<b>0.103</b>	<b>0.345</b>	-0.023	-													
6 <i>Size</i>	<b>0.900</b>	<b>0.248</b>	<b>0.080</b>	<b>0.099</b>	-												

(continued on next page)



**Table D2**  
Robustness checks.

Panel A: Accruals-based earnings management regressions with firm fixed effects				
	Model 1 DV = <i>Disc_Acc</i>	Model 2 DV = $ Disc\_Acc $	Model 3 DV = <i>AWCA</i>	Model 4 DV = $ AWCA $
	Coefficient (t-statistic)	Coefficient (t-statistic)	Coefficient (t-statistic)	Coefficient (t-statistic)
<i>Intercept</i>	0.001 (0.010)	-0.011 (-0.162)	-0.302 (-1.764)	0.268** (2.411)
<i>KPMG</i>	0.002 (0.207)	0.008 (0.932)	0.016 (0.960)	0.015 (1.075)
<i>KPMG</i> • 2014	-0.007 (-0.712)	-0.004 (-0.551)	0.005 (0.195)	0.028* (1.856)
<i>KPMG</i> • 2016	0.014 <sup>a</sup> (1.625)	-0.004 (-0.671)	-0.007 (-0.441)	<0.001 (0.034)
Controls	Yes	Yes	Yes	Yes
Firm	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
N	2,405	2,405	2,405	2,405
Adjusted R <sup>2</sup>	0.401	0.303	0.062	0.384
F-statistic	4.95***	3.56***	1.39***	4.68***
Panel B: Accruals-based earnings management regressions with retained KPMG clients only				
	Model 1 DV = <i>Disc_Acc</i>	Model 2 DV = $ Disc\_Acc $	Model 3 DV = <i>AWCA</i>	Model 4 DV = $ AWCA $
	Coefficient (t-statistic)	Coefficient (t-statistic)	Coefficient (t-statistic)	Coefficient (t-statistic)
<i>Intercept</i>	0.004 (0.244)	0.092*** (6.966)	0.025 (1.272)	0.125*** (5.227)
<i>KPMGret</i>	-0.002 (-0.427)	0.004 (0.975)	0.006 (0.783)	0.004 (0.631)
<i>KPMGret</i> • 2014	-0.011 (-1.054)	-0.004 (-0.441)	0.007 (0.298)	0.018 (1.242)
<i>KPMGret</i> • 2016	0.016** (2.044)	-0.003 (-0.558)	-0.014 (-0.886)	0.004 (0.341)
Controls	Yes	Yes	Yes	Yes
Industry	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
N	2,405	2,405	2,405	2,405
Adjusted R <sup>2</sup>	0.247	0.121	0.068	0.209
F-statistic	23.51***	10.44***	6.00***	19.11***
Panel C: Accruals-based earnings management regressions with 2017 as treatment period for the <i>Hahn</i> event				
	Model 1 DV = <i>Disc_Acc</i>	Model 2 DV = $ Disc\_Acc $	Model 3 DV = <i>AWCA</i>	Model 4 DV = $ AWCA $
	Coefficient (t-statistic)	Coefficient (t-statistic)	Coefficient (t-statistic)	Coefficient (t-statistic)
<i>Intercept</i>	0.004 (0.252)	0.092*** (6.974)	0.026 (1.304)	0.125** (5.233)
<i>KPMG</i>	-0.002 (-0.468)	0.003 (0.745)	-0.002 (-0.327)	0.001 (0.169)
<i>KPMG</i> • 2014	-0.008 (-0.737)	-0.004 (-0.550)	0.004 (0.166)	0.024 (1.576)
<i>KPMG</i> • 2017	0.016** (2.427)	0.003 (0.415)	0.033* (1.693)	0.012 (0.917)
Controls	Yes	Yes	Yes	Yes
Industry	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
N	2,405	2,405	2,405	2,405
Adjusted R <sup>2</sup>	0.247	0.121	0.069	0.209
F-statistic	23.49***	10.45***	6.06***	19.15***

Notes: <sup>a</sup> p-value (two-tailed) = 0.104. Panel A present OLS regression results from re-estimating all models from Table 6 with client firm fixed effects instead of industry fixed effects. Panel B present OLS regression results from re-estimating all models from Table 6 with different definitions of the variables of interest. *KPMGret<sub>i,t</sub>* is an indicator variable that is coded as one if KPMG is the auditor of client *i* in years *t* and *t-1* and zero otherwise. Panel

C present OLS regression results from re-estimating all models from Table 6 with 2017 instead of 2016 as the treatment year for the *Hahn* event. For sake of brevity, we do not report control variables. We use the same control variables as in the respective model in Table 6. We winsorize all continuous variables at the first and 99th percentile and compute robust standard errors clustered by client. We define industry based on the 16 sector indices of Frankfurt Stock Exchange and manually backfill missing industries based on SIC codes, if available. \*, \*\*, and \*\*\* denotes significance at the 10 %, 5 %, and 1 %-level, respectively, of two-tailed tests. Variable definitions are in Appendix A.

because statistical tests are constructed in a way that one in ten tests produces a false positive result (assuming a 10 % significance level). If different designs produce consistent results, they are unlikely due to random false positives. Table D2 summarizes the results.

Panel A presents the results when we re-estimate all of our models and include client firm fixed effects instead of industry fixed effects (i.e., a dummy variable for each individual client as an additional set of control variables). Client fixed effects control for all client-specific effects that are constant over time, even if they are unobservable. Thus, this design creates a within-client comparison. For the *Hahn* event, results remain similar but the formerly significant effect in Model 1 becomes just insignificant ( $p = 0.104$ ). In addition, *KPMG • 2014* becomes positive and significant in the absolute abnormal working capital accruals model, but this effect is not consistent across the other four models.

Panel B shows the results when we repeat our estimations but only include retained KPMG clients that have chosen KPMG as their auditor in the treatment year and have been a KPMG client in the year before. The motivation for this specification is to test whether effects are stronger for those retained clients or are driven by newly acquired clients who have arguably been unaffected by the short-term negative market reactions (at least for the *Ring* event). Additionally, audit quality may systematically differ for first-year audits, which may bias our results for the full sample. *KPMGret • 2014* is insignificant in all models. However, the results for the *Hahn* event (*KPMGret • 2016*) become stronger economically and statistically in the signed discretionary accruals model (Model 1), giving some additional support to our initial results in Table 6.

Panel C of Table D2 displays the results when we use 2017 instead of 2016 as our treatment year for the *Hahn* event, an alternative introduced in our timeline of events (Appendix B). If it takes some time for reputation effects to manifest in the audit process, we might only observe quality effects in 2017. KPMG may have signaled a higher willingness to acquiesce to clients in 2017 engagement negotiations resulting in the non-effect in auditor changes and audit fees. We do not change anything in the specification for the *Ring* event. Results for the *Hahn* event (in this case, coefficients of *KPMG • 2017*) are economically and statistically stronger than in our original specification, supporting the indication of an effect from our main analyses, which may only manifest after the first audit negotiation following the event. Still, given that results are not consistently significant, they should be interpreted with caution.

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