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Introduction

The first chapter of this PhD Thesis explores the effect of expanded audit report rules on the client-auditor relation. The fact that in the U.K. these rules were implemented in several stages allows applying difference-in-differences methodology to analyze the consequences of the new reporting regime. We test whether expanded reporting rules create additional tension between companies and their auditors which leads to an increase in auditor turnover rate. Companies might want to minimise the number of the reported KAMs (i.e., risky areas) as some papers show that receiving too many of them can be perceived by the users of financial statements as a bad signal. At the same time, auditors might prefer to report more KAMs in order to provide high audit quality and minimize potential litigation and reputation losses in cases of detected misreporting. Empirical results suggest that, in line with this idea of the opposing interest of companies and their auditors regarding KAM disclosure, the implementation of the new audit reporting regime lead to an increase in the probability of auditor switch. Cross-sectional analyzes give further support to the idea that KAM disclosure increases tension between auditors and their clients as in the post-implementation period the probability of auditor switch is positively associated with the number of reported KAMs. Finally, there is some evidence of a reduction in the number of reported KAMs following auditor switches, which may indicate a new form of successful opinion shopping.

The second chapter investigates how U.S. auditors respond to shifts in litigation risk driven by two Supreme Court rulings—Tellabs (2007) and Janus (2011)—which altered

legal standards across federal circuits. These rulings created exogenous variation in auditors' legal exposure, allowing us to examine adjustments in audit pricing, reporting conservatism, and audit quality. We consistently find that auditors raise their fees in circuits where litigation risk increased following both rulings. However, the evidence for other auditor responses is mixed: while some findings support the expected effects following Tellabs ruling, others are contradictory. For Janus, we find no meaningful evidence of auditor responses beyond fee adjustments.

The third chapter examines if and how U.S. public companies adjust their ESG practices after receiving penalties for violating ESG-related regulations. We seek to understand whether these firms take steps to improve their ESG standing in order to signal a renewed commitment to ESG principles, rebuild their tainted reputations, and reduce the likelihood of future violations. Consistent with the stated hypotheses, the results indicate that firms with material or repeated violations are more likely to adopt targeted environmental initiatives and experience significant improvements in ESG standing relative to industry peers. Furthermore, we observe greater transparency in ESG reporting, particularly following repeated violations. The findings related to quantitative ESG outcomes — such as waste generation, energy use, or injury rates — are generally weak or statistically insignificant.

Chapter 1

Expanded Audit Reporting and Auditor Switches

1.1 Introduction

This paper examines whether and how the introduction of the extended audit reporting rules—one of the most significant changes in audit reporting in recent years—has influenced client–auditor relationships. Before the introduction of these rules, auditors were responsible solely for issuing a “pass or fail” opinion on their clients’ financial statements, with assessments ranging from clean to adverse opinions and including evaluations of the clients’ ability to continue as a going concern. However, this reporting model was often criticized for its lack of informativeness due to its highly standardized wording. In response to these criticisms, many countries around the world adopted the extended reporting framework, requiring auditors to go beyond the traditional opinion and disclose the most complex and potentially high-risk areas of their clients’ financial statements, known as Key Audit Matters (KAMs), in their reports. Since the introduction of the new reporting rules, numerous academic studies (e.g., Guttierrez et al., 2018; Elamer et al., 2020; Porumb et al., 2021; Lennox et al., 2023) have explored whether the revised reporting framework has met its goal of improving the usefulness of annual report dis-

closures for stakeholders, helping to shape a detailed - though not always unambiguous — picture of the direct consequences of the reform. This paper aims to go further in examining the indirect effects of expanded auditor disclosures and explore whether and how these disclosures have influenced auditor–client relationships.

This research question is motivated by previous studies (e.g., Lennox, 2000; Chen et al., 2015; Amin et al., 2021) showing that issuing an unfavorable audit opinion—such as a modified opinion, a going-concern emphasis, or a report of internal control weaknesses — can strain the auditor–client relationship and increase the likelihood of its termination. This study explores whether the new reporting rules — which require auditors to disclose potentially problematic areas in their clients’ financial statements — have, in the same vein as previous reporting standards, become a new source of tension in auditor–client relationships, increasing the likelihood of auditor switches, or whether, unlike other forms of audit disclosure, they have not generated additional strain between companies and their auditors.

The answer to this question is not immediately evident, as the existing literature does not offer a clear understanding of the incentives that companies and their auditors might have concerning KAM disclosure. Some studies cast doubt on whether the new regulation has achieved its goal of enhancing the informativeness of auditors’ reports, arguing that the KAM section barely contains any previously unknown information that could be of real interest to external stakeholders (e.g., Gutierrez et al., 2018; Lennox et al., 2022). Under this assumption one will not expect companies to be particularly concerned with what auditors disclose as KAMs. Other studies instead provide some evidence that external financial statement users do find the information disclosed within KAM session of auditors report useful (e.g., Christensen et al., 2014; Sirois et al., 2019; Elliot et al., 2020). Under this scenario, it is reasonable to assume that companies would prefer their auditors to refrain from excessive KAM disclosure. When it comes to auditors’ incentives regarding KAM disclosures, the existing literature presents an even more controversial picture. Some papers suggest that reporting more KAMs helps auditors reduce litigation risk, as jurors interpret this disclosure as an effort to warn financial statement users about

potential issues within the company (Brasel et al. 2016; Vinson et al., 2019). Other experimental studies, however, show that excessive KAM reporting not accompanied by other unfavorable audit opinions, can be perceived as negligence on the part of the auditors (e.g., Gimbar et al. 2016; Backof et al, 2022). If this is the case, auditors themselves might prefer to disclose fewer KAMs to avoid negative perceptions which potentially aligns their incentives with those of their clients.

Given the controversy of these findings, it is difficult to form a unequivocal opinion regarding the KAM disclosure-related incentives. Companies and their auditors might (1) have no particular preference regarding KAM disclosure, (2) both be interested in poor KAM reporting, (3) have mismatch between their preferences with companies being strongly interested in reporting minimal amounts of KAMs and their auditors insisting on excessive disclosure. In this paper, I aim to explore which of these scenarios is more likely by investigating whether the introduction of extended audit reporting rules has increased the likelihood of audit switches, which typically occurs when disagreements between companies and their auditors reach a critical point. If reporting on high-risk areas provides valuable information and high-quality auditing requires extensive KAM disclosures, the introduction of extended reporting rules would likely heighten tensions between companies and auditors, making audit switches more probable. Conversely, if extended audit reports offer little valuable information or auditors are disincentivized from extensive KAM disclosures, the new reporting rules should have no effect on the likelihood of audit switches.

I test whether the introduction of KAM reporting rules created additional tension in the relationship between companies and their auditors, using a sample of U.K. companies traded on the London Stock Exchange (LSE). Focusing on the U.K. provides several advantages. The LSE is one of the largest stock exchanges globally, with the market capitalization of all traded companies exceeding 3.5 billion GBP¹, which enables me to obtain a large sample of publicly traded firms. Moreover, the U.K. was an early adopter

¹<https://www.statista.com/statistics/324578/market-value-of-companies-on-the-london-stock-exchange/>

of expanded audit reporting rules, requiring auditors of companies listed on the LSE Main Market to discuss the highest risks in their clients' reporting starting in 2013. The availability of historical data for U.K. companies facilitates an investigation of the long-term consequences of this new reporting regime. Additionally, the phased implementation of the expanded reporting rules in the U.K. allows for a difference-in-differences analysis, helping to partially address endogeneity concerns. Specifically, while companies listed on the LSE Main Market became subject to the new reporting rules in 2013, those on the Alternative Investment Market (AIM) were not required to comply until 2017. This staggered introduction provides me with a natural treatment (Main Market) and a control (AIM) groups.

The analysis is conducted in several steps. First, in a difference-in-differences setting, I test whether enactment of the new audit reporting rules lead to an increase of audit rotation cases. Then, concentrating on the post-implementation period, I investigate whether probability of audit switch is associated with the total number of KAMs a company receives. I also examine whether client-auditor relations are more likely to be terminated when a company receives more KAMs as compared to the industry-year average number and to the number which is ex-ante predicted based on company-specific characteristics. Additionally, I explore if the link between auditor switches and disclosed KAMs depends on the topic coverage of the latter. Lastly, I question whether the change of an auditor is associated with to reduction in the number of disclosed KAMs. This would be the goal of the clients if the termination of auditor-client relations is triggered by extensive KAM disclosure.

The results of the study provide some evidence that introduction of the expanded audit reporting rules did affect client-auditor relation. In particular, difference-in-differences analysis shows that cases of auditor switches became more common among companies that were obliged to adopt the new reporting first. Moreover, post-implementation analysis reveals that the number of reported KAMs is positively associated with the probability of a subsequent auditor change. The results hold for the total number of reported KAMs, as well as for the number of KAMs which are atypical for the industry the client company is

working in and for the number of client-specific unpredicted number of KAMs. Whereas the findings consistently show a positive association between the number of reported KAMs and auditor switches, the topic analysis does not yield strong results: none of the individual KAM subject areas is result to be consistently associated with a higher probability of auditor change. Further exploration shows strong association between the number of reported KAMs and downward auditor changes - cases when Big 4 clients switch to the non-Big 4 counterparts. According to the literature, such switches are typical for the companies in search for a more client-friendly auditors. Notably, further analysis in fact demonstrates that companies switching to lower-tier auditors experience a significant reduction in the number of KAMs reported in the first year of the new audit engagement.

The implementation of the new audit reporting rules provides researchers with a great opportunity to unveil the still underexplored nature of client-auditor relations. The finding of this study clearly indicate that the expanded reporting rules that require auditors to disclose additional information about their clients, bring to light tensions within the client-auditor relations. However, this paper have some limitations which can be potentially addressed by future research. First of all, the incentives of both parties which I use to develop hypotheses are still not empirically tested. Hence, I cannot rule out other forces which could explain my results. This is particularly relevant for the auditors' motivation, as most of the related studies are experimental, leaving us with little understanding of real-life the consequences they the bare for over- or under-reporting of KAMs. Also, the availability of the data does not allow me distinguishing between the cases of client- and auditor-initiated switches. Secondly, even though the results reveal positively strong association between the number of reported KAMs and consequent audit switches, I cannot claim causality of this relation. Using difference-in-differences design in the first part of the analysis partly addresses this point, however, for the post-implementation analysis, there is still a possibility that KAM reporting just highlights the existing disagreement between companies and their auditors rather than creates additional tension. Finally, the paper does not yet provide an in-depth analysis of whether KAM-related

switches result in successful opinion shopping. While the analysis shows a negative association between audit switches and the number of KAMs reported in the first year of an audit engagement, further research is needed to uncover the underlying drivers of this reduction.

This study contributes to the rapidly growing literature on the of extended audit reporting regime by exploring one of the unintended consequences of its introduction. Recent papers in this area examine possible effects of new audit reporting rules on equity (Guttierrez et al., 2018; Reid et al., 2019; and Lennox et al., 2022) and debt markets (Elamer et al., 2020; and Porumb et al., 2021). Some other studies concentrate on the changes of the companies' disclosure choices induced by the KAMs reporting (Andreicovici et al., 2020 and Lynch et al., 2022). In this paper I look at possible consequences of the new audit reporting regime from a slightly different standpoint and investigate whether it has effected interactions between the two parties directly engaged in the financial reporting process. On the one hand, there is a company which prepares its financial statement and makes particular reporting choices. On the other hand, there is an auditor who is supposed to evaluate the adequacy of these choices and fairness of the resulting numbers and point out the riskiest areas of the client's financial statements. To the best of my knowledge, this paper is the first one to study the long-run implications of the extended auditor report on the relations between these two parties. The only exception is Lennox et. al (2022) who also look at any possible association between reported KAMs and the probability of auditor change. However, this research question is not the central one in their paper and, hence, it is not addressed with particular attention. Moreover, the time period analysed by Lennox et al. (2023) consists of only two post-implementation years whereas the effect of the introduction of mandatory KAMs reporting could be deferred in time. Both, auditors and their clients, could use the very first post-adoption years to understand how the new legislation should be applied and which consequences it entails. The current data availability makes it possible to study the consequences of the new audit reporting rules for a much longer time period. My difference-in-difference analysis covers four pre- and four post-implementation years and helps to deal with exogeneity problem

not addressed by Lennox et al (2022). The time period used for the post-implementation analysis spans from 2013 to 2021 which makes the results more compelling. Hence, my study is one the first to analyse the long-running effect of the new regulation on the relations between companies and their auditors.

I also contribute to the literature on the client-auditor interaction. The new regulation is a rare opportunity to shed more light on these relations which, due to the absence of available data, still remain understudied to a great extent. Most of the literature in this field explores the phenomenon of opinion shopping (Chow and Rice, 1982; Lennox et al., 2000; Chen et al., 2016). Under the previous reporting regime where the auditors were required only to report their pass or fail verdicts, looking at the behaviour of the companies that receive a going concern opinion and its consequences was almost the only way to study client-auditor conflicts. Even though the new reporting rules were primarily introduced to enhance external readers' understanding of financial statements and were not meant to change the essence of the work conducted by auditors, they could still affect the way companies interact with their auditors which allows to make an additional step in understanding the nature of these relations. To the best of my knowledge, this paper is the first to study shifts in the patterns of auditor switches induced by the mandatory KAM disclosure. The results show that expanded reporting rules create additional tension in auditor-client relations and that companies do take into consideration KAMs disclosed by their auditors when deciding whether to change the latter.

In the following sections I provide a review of the related literature; develop the hypotheses about the link between KAMs and audit switches; justify research design choices and describe sample selection process; and finally discuss the results of the conducted analyses.

1.2 Regulatory Context and Literature Review

1.2.1 Expanded Audit Reporting Standard

Historically, independent auditor reports, which aim is to communicate the auditor's assessment of whether a company's financial statements are fairly presented, have provided a binary opinion — that the statements are either fairly presented in accordance with accounting standards (an unqualified opinion) or not (a qualified opinion). This approach though has been criticized by practitioners and academics for its limited informativeness, stemming from its standardized format, boilerplate wording, and pass/fail nature (Lennox et al., 2023). In response to these criticisms, regulators worldwide started introducing new audit reporting standards requiring auditors to disclose the key risks identified in their clients' financial reporting. The primary objective of these disclosures was to enhance the informational value of audit reports and provide investors and other users of financial statements with more decision-useful insights. Thus, auditors of public-interest entities in the EU were required to include a section on Key Audit Matters (KAMs) in their reports beginning in June 2016 (EU, 2014). In Australia, similar requirements became effective six months later, in December 2016 (AUASB, 2015), whereas in the U.S., the disclosure of Critical Audit Matters (CAMs) became mandatory for audits of large accelerated filers from December 2020 (PCAOB, 2017).

The U.K, which is the focus of this study, was one of the first countries to adopt the expanded audit reporting standards. The new requirements became applicable to companies with a premium (Main Market) listing on the LSE for fiscal years ending on or after September 30, 2013 (FRC, 2013). The revised auditing standard required auditors to disclose those risks in the client's financial statements that had the greatest effect on the overall audit strategy, the allocation of resources within the audit, and the focus of the engagement team's work. Later, with the introduction of ISA (UK) 701, the scope of the expanded audit reporting requirements was broadened to include companies listed on the Alternative Investment Market (AIM) of the LSE, and the disclosed matters were

formally defined as Key Audit Matters (FRC, 2016). The new standard also provided explicit guidance on how auditors should determine them, emphasizing the need to consider areas of higher assessed risk, significant judgments involving estimation uncertainty, and the impact of major events or transactions during the period.

1.2.2 Literature on Expanded Audit Reporting

The introduction of extended audit reporting requirements in many countries has already attracted considerable academic attention. Early studies in this area examine whether these regulations have achieved their intended objective: enhancing the informativeness of audit reports. To address this question, papers explore how external users perceive the usefulness of KAM disclosures and the extent to which this additional information influences their evaluations of company performance and audit quality.

Research on this issue emerged even before the expanded audit reporting rules came into effect in most of the jurisdictions, with early studies experimentally investigating the information and credibility effects of KAMs. For example, Christensen et al. (2014) provide evidence in support of the new reporting regime by show that KAM disclosures convey valuable information to financial-statement users. In their experiment, participants receive one of three audit-report formats: a standard report, a report with a separate KAM paragraph, or a report in which KAM-related information was incorporated into the management footnotes. The findings suggest that non-professional investors who receive the report containing a distinct KAM section were more likely to adjust their investment decisions based on the disclosed information, supporting the idea that KAMs enhance the decision usefulness of audit reports. In another experimental study, Dennis et al. (2019) also show that KAM disclosures may add value to audit reports. They find that non-professional investors adjust their valuation decisions when an auditor's disclosure is accompanied by a supplemental management disclosure, or when a stand-alone auditor's disclosure regarding material measurement uncertainty is supplemented with visual cues. Other examples of experimental studies that discuss potential consequences of KAMs

introduction are by Sirois et al. (2019) who use an eye-tracking device to show that readers pay more attention to the accounting issues highlighted by KAMs and by Elliot et al. (2020) who show that KAMs can be effective channels of conveying additional information about reporting quality. All these experimental studies suggest that expanded audit disclosure does attract the financial statements' readers attention and influence their investing decisions.

In recent years, the KAM literature has been supplemented by the first empirical studies, with the pioneering works discussed below — Gutierrez et al. (2018), Reid et al. (2019), and Lennox et al. (2023) — focusing on the U.K., one of the first countries to implement this audit reform-

Gutierrez et al. (2018) study the effect of extended audit reports on investors' behavior, audit fees, and audit quality. In order to do that, they implement difference-in-differences research design with treatment group made up of the companies listed on LSE Main Market for which the extended audit report has become mandatory since 2013 and control group comprised of the AIM-listed companies which were not mandated to adopt these regulatory changes until 2017. The authors document that KAMs reporting does not entail any market reaction measured as abnormal returns or trading volumes, neither does it lead to an increase in audit pricing or quality. Gutierrez et al. (2018) conclude that KAM disclosure do not provide external readers with any additional information. The authors point out that usually the risky areas highlighted by auditors in the KAM section overlap with risks that are already discussed within the audit committee's reports. Moreover, the results of the analysis provides some evidence that companies with higher numbers of reported KAMs were already paying higher audit fees even before the extended audit reports became mandatory. Although the study provides no evidence "in favor" of extended audit reports, the authors themselves acknowledge that analysing the first two years of post-adoption period may be not sufficient to capture long-term impact of the new regulation.

Reid et al. (2019) address similar research questions to those covered by Gutierrez et al.

(2018). The authors also study the U.K. setting and investigate whether introduction of extended audit reports leads to an improvement in reporting quality and an increase in audit fees or entails any market reaction. In contrast to Gutierrez et al. (2018), this study provides some evidence of the change in financial reporting quality associated with KAMs introduction. Thus, Reid et al. (2019) document significant decreases in absolute abnormal accruals and the propensity to meet or beat analyst forecasts for the companies adopting extended audit reporting rules. These findings support the idea that mandatory expanded audit disclosure forces companies and their auditors to stick to a more accurate disclosure. Also, Reid et al. (2019) find an increase in earnings response coefficients around the new reporting regime which they interpret as an additional evidence of reporting quality enhancement.

Lennox et al. (2023) is another empirical paper that analyses the consequences of KAM introduction. The authors study disclosure of risks of material misstatements (RMMs) and arrive to the conclusions similar to Gutierrez et al. (2018): concentrating on long-window abnormal returns they find no evidence of the investors' reaction to the disclosed RMMs. The authors try to shed some light on the reasons for such lack of the investors reaction. In particular, they show that RMMs do contain value relevant information (the number of RMMs is negatively associated with the valuation coefficients on earnings and book value and cash flows persistence), but this risk-related information is already known by the investors from sources other than KAMs. Moreover, in line with Gutierrez et al. (2018) and Reid et al. (2020), this paper fails to provide any evidence of association between audit pricing and extended audit disclosure. In one of the additional tests the authors also study the association between the number of RMMs and rate of auditor turnover and find no statistically significant results. However, as mentioned above, this part of the analysis in the paper by Lennox et al. (2023) is complementary and there are still many unanswered questions in terms of the link between KAMs reporting and auditor-client relations which I partly try to address in this paper.

The papers of Gutierrez et al. (2018) and Lennox et al. (2023) discuss the most straightforward and obvious effects of extended audit reports and find no evidence of KAMs

leading to improved reporting quality or adding any incremental informative value. However, some other U.K.-based archival studies investigate more nuanced and less obvious effects and do find statistically significant results. For instance, Porumb et al. (2019) explore whether new audit reporting regime has affected another group of stakeholders which is known to be interested in the riskiness of a company - its lenders. In particular, the authors analyse the informativeness of the expanded audit reports' disclosure for private debt market. Using difference-in-differences research design, they find that adopters of new auditing regime have debt contracts with lower spreads and longer maturities in the post-implementation period.

There are also some recent studies that examine the consequences of the expanded audit report in the U.S., where external auditors are required to complement their pass-or-fail opinions with a detailed discussion of the most complex and subjective areas of the audited financial statements — Critical Audit Matters (CAMs) — since 2019. Burke et al. (2019) investigate market and managerial reaction to these new audit report requirements. In line with Gutierrez et al. (2018) and Lennox et al. (2023), the authors do not find any significant market reaction to the disclosure of the ex-ante expected CAMs in the U.S. setting. However, they do find negative market reaction to the unexpected CAMs which can be seen as an evidence of the disclosure informativeness. In their paper, Burke et al. (2022) develop models to predict expected by the market number of reported CAMs and their topics. Such approach allows to conduct a more accurate analysis of the expanded audit report's informativeness. Also, Burke et al. (2022) document a change in management's disclosure with respect to the areas identified by auditors as CAMs. Thus, their findings support the idea that both, companies and investors, pay attention to the information discussed under the new reporting rules.

1.2.3 Implications for Companies and Auditors

Although the new audit reporting regime is primarily designed to benefit external users of financial statements — and much of the existing research has accordingly focused on

this group — the key parties directly affected by the regulation are companies and their auditors. The former are responsible for preparing the financial statements, while the latter must audit them and, in line with the regulation that is central to this study, identify KAMs. Some recent papers, discussed further in this section, explore different aspects of KAM reporting in relation to each of these two groups separately. Apart from the growing body of literature examining external users’ reactions to KAMs — which can, in itself, influence corporate preferences — some studies explicitly show that companies receiving certain types of KAMs are compelled to adjust their financial reporting practices, even when reluctant to do so (e.g., Andreicovici et al., 2020; Fuller et al., 2021; Lynch et al., 2022). Regarding auditors’ perceptions of KAM disclosure, the literature primarily examines its effects on audit fees, audit quality, and shifts in perceptions of auditors’ negligence. On the one hand, both audit fees and quality could increase, as the introduction of KAMs may have required additional involvement of senior auditors, more extensive reviews, and greater communication between auditors and management—all of which imply higher effort and potentially enhanced audit rigor (Gutierrez et al., 2018). On the other hand, these factors could remain unchanged, since much of the information disclosed as KAMs was already being discussed with audit committees under existing auditing standards, requiring little additional work beyond formalizing those disclosures. Indeed, the FRC explicitly stated that “as the auditor was already required to disclose the information to be included in the audit report to the audit committee, the work effort in preparing it and familiarizing the audit committee with it should be minimal” (FRC, 2013b). The findings of studies examining these effects are as mixed as the underlying expectations: while Gutierrez et al. (2018) and Lennox et al. (2023) focus on the U.K. setting and find no evidence of increased audit fees or quality, other research (e.g., Li et al., 2018; Reid et al., 2020; Almulla & Bradbury, 2022) supports the view that KAM reporting is associated with higher audit pricing and improved audit quality.

Finally, several experimental studies question whether KAM reporting has altered perceptions of auditors’ responsibility for misstatements identified in their clients’ financial statements. Similar to the literature on audit pricing and quality, researchers have de-

veloped and found support for contradictory hypotheses. Some studies claim that, under the expanded reporting requirements, auditors are perceived as less responsible for such misstatements because KAM disclosures effectively warn users that certain areas of reporting involve significant judgments and risks (Brasel et al., 2016; **Asbahr & Ruhnke, 2019**; Vinson et al., 2019; Ma et al., 2020). Conversely, other research finds that KAM disclosures can make auditors appear more responsible, as explicitly identifying high-risk areas may elevate users’ expectations of auditor foresight and control, thereby increasing perceived accountability when misstatements occur (Gimbar et al., 2019; Backof et al., 2022).

Overall, the existing literature suggests that the introduction of expanded audit reporting has affected both parties directly involved in the preparation of financial statements and audit reports - companies and their auditors. The question that then arises, and which this paper seeks to address, is whether the new regulatory environment has also altered the nature of the interaction between these two parties. To the best of my knowledge, this issue has not been carefully addressed in the U.K. or European settings yet. Lin and Yen (2022) examine association between auditor rotation, accruals quality and reported KAMs in Taiwan. The authors look at what happens after a company changes its auditor. They find that an incoming auditor is more likely to mention in her report previously unreported KAMs and that in the cases when the new auditor reports different KAMs, on average, the accruals quality also improves. Differently from Lin and Yen (2022), this paper explores whether and under which circumstances KAM disclosure create additional tension in client-auditor relations and lead to auditor switches.

1.2.4 Literature on Auditor–Client Relations

This study also contributes to the literature on client–auditor interactions, which remains underdeveloped because most aspects of the audit process — including auditor–client negotiations — are largely unobservable to external parties (Francis, 2011; DeFond & Zhang, 2014). Due to these data limitations, prior research in this area has primarily relied on

experimental studies. In the absence of accessible real-world data, this approach enabled researchers to examine how factors such as individual characteristics (e.g., Church et al., 2019), negotiation strategies (e.g., Sanchez et al., 2007; Hatfield et al., 2010; Perreault et al., 2017), environmental context (e.g., Brown-Liburd et al., 2013), and other factors influence negotiation outcomes. As for empirical research, most opportunities arise from the Chinese context, which provides proprietary data on auditor-proposed adjustments to the initial clients reporting. A couple of studies investigate the drivers of more pronounced adjustments - which are shown to significantly improve earnings quality (Lennox et al., 2016). Thus, Lennox et al. (2014) find that mandatory partner rotation leads to larger adjustments in both final year of the leaving auditor and first years of the incoming one. Lennox and Wu (2022) show that the introduction of mandatory internal control audits results in smaller adjustments due to the managers becoming less collaborative and auditor's unwillingness to tear worsen the relations. Lennox et al. (2020) demonstrate that adjustments depend on the equity stakes of the audit partners and on their role within the audit firms: larger ownership stakes motivate earnings quality reviewers to monitor audit quality more closely, whereas the same is not true for engagement partners, who also have incentives to maintain good relations with clients.

Altogether, this is where the empirical literature on the topic largely ends, with only a few exceptions that examine the most negative cases—namely, the termination of the auditor–client relationship, which can be initiated by either party. Some papers explore the possibility that companies may engage in opinion shopping when they are dissatisfied with their incumbent auditors' reports (Chow & Rice, 1982; Lennox, 2000; Chen et al., 2016). A few studies (e.g., Shu, 2000; DeFond et al., 2016) focus on situations where auditors themselves choose to resign in order to reduce engagement risk. Because this branch of research mainly examines cases where clients are dissatisfied with qualified audit opinions—which until recently were the primary form of auditor reporting—the introduction of KAMs provides an opportunity to deepen this line of inquiry and to examine whether this additional reporting also creates disagreements between the parties.

1.3 Hypotheses Development

This paper examines whether the introduction of the expanded audit report increased tension between companies and their auditors and, as a consequence, led to higher probabilities of voluntary auditor switches. I argue that such tension may arise when companies and auditors hold conflicting incentives regarding KAM disclosure. The hypothesis works if two opposing mechanisms operate simultaneously. First, companies should prefer to avoid extensive KAM reporting. This view is supported by studies showing that a larger number of KAMs may be interpreted by external users as warning signals and can pressure companies to adjust their financial reporting choices in response. Second, auditors should favor more extensive KAM disclosure — an idea which aligns with recent experimental evidence suggesting that expanded disclosure helps mitigate their litigation and reputational risks. However, as noted in the literature review, the evidence in the KAM literature is far from consistent, and a number of studies question the validity of these mechanisms.

The section first reviews evidence consistent with the idea that companies and auditors hold opposing incentives with respect to KAM disclosure, thereby supporting the hypothesis that the introduction of the expanded reporting rules prompted an increase in voluntary auditor switches. I also predict a positive association between extensive KAM reporting and the probability of such switches. The discussion then turns to studies that challenge this view, suggesting either that companies may have little incentive to limit the number of KAMs disclosed — if such disclosures provide no additional informative value to external users — or that auditors themselves may prefer to report only a minimal number of KAMs. If these alternative explanations hold, the introduction of the new disclosure requirements would not be expected to generate additional tension within auditor–client relationships. Finally, the section introduces the supporting hypothesis, which extends the main argument by identifying the conditions under which KAM disclosures are most likely to create disagreement between companies and their auditors and lead to the termination of their engagement. This part also considers whether such switches

represent a new form of opinion shopping, whereby companies dissatisfied with extensive KAM reporting seek more lenient auditors and subsequently obtain reports containing fewer KAMs.

1.3.1 Companies' Incentives: Minimizing KAM Disclosures

Some recent empirical studies show that extensive number of reported KAMs is seen as a negative signal by the market. For instance, Almulla and Bradbury (2022) analyze New Zealand data and show that investors become less responsive to the book value of equity disclosed in financial statements when companies receive auditor- or industry-specific KAMs. The authors argue that such an increase in uncertainty about accounting numbers is due to the fact that KAMs do convey relevant information about riskiness of a company. Lennox et al. (2023) find similar results analyzing long-term pricing consequences in the U.K. In particular, they show that during 30 days after the annual report release the valuation coefficients on the reported earnings and assets are significantly smaller for companies that receive more KAMs. Lynch et al. (2022) also analyze companies listed on the London Stock Exchange and, consistent with the idea that KAMs contain information relevant for the investors, document that market discounts deferred tax assets of the companies that receive tax-related KAMs. Burke et al. (2022) look at the market reaction to the expanded audit disclosure in the U.S. and find a decrease in abnormal returns for the companies receiving unpredicted CAMs. Empirical results described in these papers support the idea that investors find KAM disclosure to be useful and adjust their behavior accordingly.

There are also experimental studies, which allow to isolate investors' reaction to reported KAMs from that to any other information contained in the annual reports, that favor the idea that market perceives KAMs as a negative signal. Christensen et al. (2014) conduct an experiment involving 141 business school alumni with prior investing experience. The results suggest that participants are less likely to invest in companies that receive fair value-related CAMs in their audit report and this negative effect of CAM disclosure holds

even if the company itself voluntarily discloses the same information. Another experimental study by Rapley et al. (2021) involves 292 participants from Amazon’s Mechanical Turk and comes to a similar conclusion: individuals are less likely to invest in the companies which receive CAMs as compared to those which do not. Moreover, the authors show that reported CAMs negatively affect inventors’ perceptions of management disclosure credibility. Dennis et al. (2019) conduct another experiment in which they find that very same disclosures provided by managers and auditors impact investors’ behaviour differently. Thus, they discount price/earnings multiples when auditors’ disclosure about material measurement uncertainty is either accompanied by the managers’ discussion of the same issue or contains some visual cues (i.e., direct references to the accounts in financial reports).

Negative market reaction to the reported KAMs found in the above-mentioned studies might incentivize managers to avoid receiving extensive number of KAMs. However, this is not the only reason for which KAMs may be undesirable from companies’ standpoint - some other groups of interest are also found to perceive disclosed KAMs as a bad signal. For instance, Elamer et al. (2020) and Porumb et al. (2021) document that reported KAMs cause negative reaction on the debt market. Elamer et al. (2020) use the ratio of total interest expenses to average total debt of firm as a proxy for cost of debt and show that companies receiving more KAMs bear higher borrowing costs. The authors also document positive association between length and negative tone of audit reports’ KAM section and costs of debt. Porumb et al. (2021) study consequences of KAM disclosure on syndicated loan market and show that number of RMMS which are reported by auditor but not mentioned in audit committees’ report is positively associated with loan spreads. Even in the absence of an immediate market price drop as a reaction to the reported KAMs, managers might simply want to minimize the number of the latter in order to avoid enhanced scrutiny by financial statements’ users. Sirois et al. (2018) conduct an experiment in which they use an eye-tracking technology and find that identical information disclosed in financial statements attracts greater readers’ attention if it is identified as a KAM in the auditors’ report. The increased scrutiny in its turn can force compa-

nies to change their accounting behavior and stick to reporting choices that otherwise could have been avoided. Thus, another experimental study by Gold et al. (2020) shows that when receiving a goodwill-related KAM, managers opt to disclose information which leads to goodwill impairment even though they have strong incentives to avoid it.

Recent empirical studies also provide support to the idea that KAMs disclosure may force management to change the disclosure choices. Thus, Andreicovici et al. (2020) show that U.K. premium listed companies that receive KAMs related to the goodwill impairment for the first time start providing significantly more disclosure about the underlying issue. Moreover, the authors document that companies reverse to less disclosure on goodwill impairment once the related KAM is removed. Fuller et al. (2021) document that more detailed CAM reporting, together with effective audit committee, encourage managers to provide more disclose regarding the risks underlying complex estimates. Lynch et al. (2022) study tax KAMs and show that companies which stop receiving them significantly reduce their effective tax rates in the following years. Based on this finding, the authors conclude that companies tend to increase their tax avoidance once the related KAM is resolved. Drake et al. (2021) study tax-related RMMs received by the U.S. companies and also find that managers adjust their reporting choices depending on whether they receive tax CAMs or not. Thus, when such CAMs are disclosed they become significantly less likely to use tax expense in order to meet analysts' forecasts. Moreover, the authors find that receiving a tax-related CAMs induces upward revisions to the tax liability positions. Taken together all these findings indicate that managers have strong incentives to insist on receiving as few KAMs as possible: reports with many risks of material misstatement may trigger negative debt and equity markets reaction and force a company to opt for specific disclosure choices that it would have preferred to avoid otherwise.

1.3.2 Auditors' Incentives: Extensive KAM Reporting

Auditors, however, may be reluctant to comply with clients' requests to limit extensive KAM reporting if doing so could be perceived as a signal of suboptimal audit quality.

As prior research shows, various indicators of poor audit quality are indeed associated with heightened litigation risk. For instance, Kinney & McDaniel (1994) and Carcello & Palmrose (1994) show that such audit failures as not issuing qualified report to a client that ultimately files for bankruptcy increases auditors' litigation risks. Similarly, Fuerman (1997) and Palmrose & Scholz (2004) document that financial restatements increase the likelihood of lawsuits against auditors, with the latter demonstrating that the effect is particularly pronounced for economic restatements of revenues.

Well aware of the fact that poor audit quality increases their litigation risk exposure, auditors are shown to adjust their reporting behavior to mitigate this risk. Some papers evidence that auditors respond to litigation threats by insisting on certain accounting choices within their clients' reports. For instance, DeFond & Subramanyam (1998) and DeFond et al. (2016) find that auditors under heightened litigation risk tend to insist on more conservative accounting treatments, which allows them to reduce the probability of being named in class-action lawsuits. In a similar vein, Boone et al. (2011) report that abnormal accruals — a proxy for earnings quality — decline as auditor litigation risk increases, suggesting that auditors actively push clients toward higher-quality reporting. Moreover, auditors are shown to use their own audit reports to limit legal exposure. Thus, several studies suggest that, in high-litigation contexts, auditors are more likely to issue a going-concern opinion in order to reduce the probability of a class-action suit against them (e.g., Geiger & Raghunandan, 2001; Geiger et al., 2006).

Analogous to prior findings on other audit-quality variables, associated litigation risks, and the ways auditors mitigate these risks, KAM reporting may similarly play a crucial role in managing auditors' exposure. The new expanded audit reporting rules require auditors to communicate the riskiest areas of the clients' reporting, so withholding this information could itself be seen as poor audit quality—something auditors should avoid if they want to limit their legal exposure, consistent with the prior evidence above. Moreover, because KAMs are now an explicit part of the auditor's report, they may also be used preemptively, much like going-concern or qualified opinions, as a tool to shift responsibility away from the auditor by signaling that the underlying risks were commu-

nicated to users in advance. Recent experimental studies provide support for the idea that auditors view KAM disclosures as a mechanism to mitigate their risks. The findings suggest that both auditors and external judges perceive an external auditor to be less responsible for the revealed financial misreporting when the former reports corresponding risk of material misstatement in advance. Thus, Brasel et al. (2016) conduct an experiment with 528 Amazon’s Mechanical Turk participants who represent jurors and show that the auditors are less likely to be accused of negligence when their audit reports mention related CAMs. Moreover, the authors do not find support for the idea that auditors may be punished for disclosing CAMs unrelated to misstatements. The paper shows that auditors’ costs of non-reporting of the relevant risks are much higher than the costs of over-reporting of not relevant ones which may create additional incentives to report high numbers of KAMs (CAMs). In another experimental setting Vinson et al. (2019) show that in the cases of a lawsuit jurors perceive greater auditor negligence when a CAM related to the misstated topic is removed from the auditor’s report over time to when it is disclosed consistently over multiple years. Ma et al. (2020) conduct another experiment in which 50 auditors from Australia are asked about their perceived accountability to users of financial statements. The results show that under the new reporting regime which allows auditors to inform investors about the most risky areas of financial reports via KAMs, they feel less responsibility for the accuracy of financial statements. Based on the above-mentioned literature, I hypothesize that auditors will be reluctant to comply with clients’ requests to report minimal numbers of KAMs, as such under-reporting may compromise the perceived quality of their work and, consequently, increase their litigation and reputational risks.

1.3.3 Expanded Reporting and Auditor–Client Tensions

If companies and auditors differ in their incentives regarding KAM disclosures as hypothesized above, the new regulation may have heightened tensions between the two parties and, consequently, increased the likelihood of voluntary auditor switches, with

such terminations potentially initiated by either party.

The notion that firms may voluntarily switch auditors in search of a more “accommodating” one — essentially engaging in opinion shopping — has been recognized and examined for decades, with early studies dating back to the 1980s and 1990s (e.g., Chow and Rice, 1982; Smith, 1986; Krishnan, 1994; Krishnan & Stephens, 1995). Lennox (2000) was among the first to provide evidence that such opinion shopping can indeed be successful. Using a model that predicts the likelihood of receiving a modified opinion had the firm made the opposite switching decision, and comparing this counterfactual to the actual outcome under the chosen auditor, he shows that companies would have received unfavorable reports more frequently under alternative choices. A separate strand of research building on Lennox’s (2000) model extends the evidence of successful opinion shopping. More recent studies show that companies can also successfully engage in partner-level opinion shopping (Chen et al., 2015) and increase their likelihood of obtaining not only unqualified opinions, but also more favorable internal control (Newton et al., 2016; Amin et al., 2021) and going-concern opinions (Chung et al., 2019).

To the best of my knowledge, there is no direct evidence that difficulties in client–auditor negotiations lead auditors to initiate termination of the engagement (likely due to the previously discussed lack of publicly available information on the audit process and the parties’ negotiations). However, prior studies document that auditors may choose to resign when the litigation risk associated with an engagement becomes excessively high (e.g., Bockus & Gigler, 1998; Shu, 2000; Ghosh & Tang, 2015; DeFond et al., 2016). To the extent that client-preferred under-reporting of KAMs increases the auditor’s future exposure to litigation risk and the parties cannot reach agreement on the appropriate level of disclosure, the new regulation might also push increase in the cases of auditor-initiated resignations.

Taken together, the prior discussion suggests that the expanded audit report may have intensified disagreements within auditor–client negotiations by creating the gap between companies’ and auditors’ incentives regarding KAM disclosure. If companies increasingly

seek to limit the number of KAMs while auditors simultaneously become more inclined to report them, the new regime may generate additional strain in the relationship, making it more difficult for the parties to reach mutually acceptable reporting outcomes. Should these frictions be sufficiently strong, they may ultimately translate into a higher incidence of voluntary auditor switches following the introduction of the expanded disclosure rules. This leads to the following hypothesis:

H1. There is an increase in the number of audit firm switches following the adoption of expanded audit report rules.

However, the hypothesis is not without tension. If the incentives outlined above do not hold in real life, the introduction of the new reporting rules may not have altered the dynamics of auditor–client relations. Indeed, some studies provide evidence that challenges the assumption of conflicting incentives between the two parties.

First, as discussed in the literature review, prior auditing research has reached no consensus regarding the informational relevance of expanded audit reports. For example, Gutierrez et al. (2018) and Lennox et al. (2023) question the informative value of KAM disclosures, suggesting that they merely duplicate information already available from other sources. Bédard et al. (2019) examine the French disclosure requirement known as “justifications of assessments” — a disclosure similar to KAMs that has been mandatory since 2002 — and find no evidence of investor reactions to such information. Likewise, Boolaky and Quick (2016) study bank directors’ perceptions of audit reporting and find no support for the idea that KAM reporting provides incremental informational value and affects credit approval decisions. Taken together, these studies cast doubt on the relevance of KAM reporting and, consequently, challenge the notion that companies have strong incentives to influence KAM disclosures. If most stakeholders disregard auditors’ signals about risky reporting areas, client companies are unlikely to view such disclosures as consequential or to seek to limit the number of KAMs reported.

Second, the assumption that auditors themselves have strong preferences for extensive KAM reporting can also be questioned. Although some studies suggest that timely dis-

closure of KAMs (or CAMs) may protect auditors from negligence claims and thereby reduce litigation risk, other research documents contradictory findings. Gimbar et al. (2016) conjecture that when auditors disclose a CAM related to a subsequently detected misstatement, jurors perceive that the auditors could have anticipated the adverse outcome yet did not take adequate action to prevent it, thereby attributing partial responsibility for its occurrence. Consistent with this reasoning, their experimental results show that both related and unrelated CAM disclosures increase perceived auditor liability. Likewise, Backof et al. (2022), in an experiment involving 163 undergraduate participants acting as jurors, find that auditors are judged more harshly — being viewed as more negligent — when the audit report includes a CAM related to the misstatement than when it does not. The findings of these studies therefore imply that auditors may actually have an incentive to limit, rather than expand, the number of KAM disclosures. If this is the case in practice, the introduction of the new reporting regime is unlikely to generate significant tension in auditor–client relations, as the incentives of both parties would be either aligned or largely indifferent.

The next hypothesis is developed in the same vein but shifts the focus to the period after the new reporting rules had been implemented and auditors began making concrete decisions about what to disclose in the KAM sections of their reports. Specifically, I examine the amount and content of reported KAMs and argue that both the number and the “atypicality” of these disclosures may reflect the underlying tension between companies and their auditors. The UK ISA 700 (Forming an Opinion and Reporting on Financial Statements) and ISA 701 (Communicating Key Audit Matters in the Independent Auditor’s Report) do not prescribe a specific number of KAMs to be reported, stating that their determination is a matter of the auditor’s professional judgment. If companies prefer to disclose the minimal number of KAMs while auditors, by contrast, have strong incentives to report as many KAMs as possible, such discretion may heighten the potential for disagreement between the two parties. An alternative “one-size-fits-all” approach — prescribing that auditors discuss a fixed number of KAMs for comparable companies — would likely have created less scope for disagreement, as variation across

audit reports for the same firm would then primarily concern the topics and descriptions of the risky areas. The fact that the number of disclosed KAMs ultimately depends on the individual auditor further increases the potential for tension and may foster opinion shopping. The existing data confirms that there is no clear benchmark for the number of disclosed KAMs: Lennox et al. (2023) document that, during the first two years following the introduction of the regulation in the U.K., the 25th percentile of RMMs per company was three, whereas the 75th percentile was five. If KAM disclosure has become another stumbling block in the auditor–client relationship, a higher number of reported KAMs may signal greater disagreement between the parties and, consequently, increase the likelihood of a subsequent auditor switch. This leads to the following hypothesis:

H2. The probability of audit firm switch is positively associated with the number of reported KAMs.

In this setting, the termination of the auditor–client relationship is more likely to be initiated by the client, who - under the assumption that KAMs signal additional information to the stakeholders - would have preferred fewer KAMs to be reported. By contrast, the auditor, having disclosed a relatively high number of KAMs, not only fulfills the regulatory requirement to report all identified risk areas but may also reduce future litigation risk, assuming that prior evidence suggesting that extensive KAM reporting mitigates such risk holds true. Hence, one would not expect the auditor to initiate the termination of the engagement. However, there is one scenario in which the auditor might choose to resign. If the alternative hypothesis holds — that reporting a larger number of risky areas increases the auditor’s perceived liability — and the client simultaneously refuses to modify its reporting or internal controls, the engagement may become excessively risky relative to the auditor’s overall portfolio. In such a case, the auditor may prefer to resign rather than continue the relationship under elevated litigation exposure. Unfortunately, these channels are not empirically testable, as publicly available data on auditor resignations are limited, and in the U.K. context, information on auditor-requested reporting adjustments is unavailable. Consequently, while these mechanisms offer a plausible expla-

nation for auditor-initiated terminations, they cannot be directly examined in the context of this study.

Conversely, if at least one of the parties pays little attention to the new reporting requirements and lacks strong incentives to report a higher or lower number of KAMs, no systematic association between the number of reported KAMs and the probability of an auditor switch should be expected.

1.3.4 Additional Analyses: Unexpected KAMs, Topics, and Opinion Shopping

The next set of hypotheses builds on Hypothesis 2 by focusing on situations in which excessive KAM reporting is most likely to trigger disagreement between auditors and clients. The goal is to separate KAMs that address substantive issues in companies' reports from those that are mainly routine or compliance-driven. Concentrating on the first type of KAMs and leaving out the second helps maintain the focus on the idea that conflicts about reporting are the main factor behind auditor–client terminations.

In this part of the analysis, I examine the unexpectedness of the reported KAMs, both in terms of their number and their topics. Although U.K. regulations do not prohibit auditors from reporting zero KAMs, ISA (UK) 700 (Forming an Opinion and Reporting on Financial Statements) explicitly states that it is highly unlikely that “...the auditor of a complete set of general purpose financial statements of a listed entity would not determine at least one key audit matter.” Thus, reporting a minimal amount of KAMs appears inevitable and, as a result, is unlikely to generate tension between companies and their auditors. If the expanded audit reporting rules have created additional tension, the latter expected to arise when the reported KAMs disclose genuinely company-specific matters or issues that are ex-ante unexpected by stakeholders.

In assessing the unexpectedness of reported KAMs, I employ several complementary approaches based on both industry-level benchmarks and company-specific expectations.

First, KAMs can be considered unexpected in terms of their number. External users of financial statements may hold expectations about how many KAMs a company should receive and interpret any number above this expectation as a negative signal—one that the company would prefer to avoid. Hence, KAMs that exceed expected levels can be viewed as those most likely to generate disagreement between the auditor and the client. One way to identify such unexpected KAMs is to rely on industry-level benchmarks (Lennox et al., 2023). Accordingly, any number of KAMs exceeding the industry average can be regarded as unexpected. Second, KAMs may also be viewed as expected or unexpected based on the topics they address. Reporting a KAM that is common within the company’s industry is unlikely to raise concern, whereas the inclusion of a KAM covering a topic rarely mentioned in the audit reports of peer companies may be perceived as an unfavorable signal that the client would prefer to avoid. Additionally, I extend the analysis of expectations about the number of reported KAMs to the company level. Following Burke et al.’s (2022) approach, I model the expected number of KAMs for each firm based on its specific characteristics and then test whether deviations from this predicted value are linked to a higher likelihood of an auditor switch. If clients seek to limit the disclosure of unexpected KAMs, whereas auditors regard such disclosures as necessary to preserve audit quality and reduce litigation risk, I expect a positive association between the number of unexpected KAMs — whether unexpected in quantity or in topic — and the probability of an audit firm switch.

Additionally, I examine whether certain KAM topics create particularly high tension between companies and their auditors, and whether their presence in the audit report is associated with a higher likelihood of relationship termination. To conduct a meaningful analysis, I collapse the topics into broader categories as different KAM topics may cover similar risk areas. Following Burke et al. (2023), I group all the topics into consistent subject areas and analyze whether each of them is associated with higher probability of consequent auditor change. As this part of the paper is mainly exploratory, I do not make any particular predictions regarding the link between auditor firm switched and different subject areas.

The final part of the analysis takes a first step toward examining one of the mechanisms underlying the main hypotheses. As discussed above, there are limited opportunities to directly test the incentives that drive the main predictions or to identify which party initiates the termination of the auditor–client relationship. Nevertheless, I attempt to shed light on whether, consistent with my conjecture, the expanded disclosure rules have created a new form of client opinion shopping. One key assumption is that companies become dissatisfied when the number of reported KAMs is high and, in attempt to obtain more favorable audit reports, begin searching for a more lenient auditor. Accordingly, to examine whether KAM reporting gives rise to this new form of opinion shopping, I test whether companies (1) switch to auditors perceived as more client-friendly and (2) obtain subsequent reduction in the number of KAMs disclosed.

To address the first point, I focus on between-tier switches. The audit literature commonly distinguishes between Big 4 and non-Big 4 auditors and investigates how they differ. While some studies cast doubt on meaningful quality differences between these groups (e.g., Lawrence et al., 2011), others suggest that Big 4 auditors deliver higher-quality audits (e.g., Berglund et al., 2018; Che et al., 2020). In addition, non-Big 4 firms generally face lower litigation risk because, unlike Big 4 auditors, they do not have “deep pockets” (Lennox, 1999; Khurana & Raman, 2004). This combination of potentially lower audit quality and reduced exposure to litigation may make non-Big 4 firms particularly attractive to clients engaging in opinion shopping. Supporting this view, Amin et al. (2021) find that firms involved in internal control opinion shopping are more likely to switch “downward” in auditor tiers. Similarly, if extensive KAM reporting induces companies to change auditors in search of more lenient ones, former Big 4 clients with a high number of KAMs should be especially inclined to move to non-Big 4 auditors

To conclude the analysis I examine whether and under which circumstances companies that switch auditors succeed in reducing the number of KAMs reported. Such a reduction would be the desired outcome for clients if the decision to terminate the relationship with the previous auditor is triggered by aggressive KAM reporting of the latter. In line with Hypothesis 4 which states that opinion shopping would more likely result in downward

audit shift, the reduction of the number of KAMs is expected to be especially pronounced for the companies which switch from a Big 4 auditor to a non-Big 4 one.

1.4 Research Design and Sample

1.4.1 Difference-in-Differences model

In order to test Hypothesis 1, I employ difference-in-differences research design which allows to account for the temporal trends of auditors changes and rule out the effect of contaminating events. The treatment group consists of the U.K.-incorporated companies shares of which are traded on the LSE Main Market. International Standard on Auditing (ISA) 700 obliges audit reports which accompany financial statements of these companies with fiscal year-ends on or after September 2013 to contain KAM discussion section. The control group consists of the U.K.-incorporated companies shares of which are traded on LSE AIM . This market segment was designed to meet the needs of small and medium size growth companies² for which the enactment of expanded report rules was postponed until July 2017.

I use the four years during which the extended audit report rules were mandatory only for the treatment group and not for the control one (i.e., 2013-2016) as the post- period in my analysis. Symmetrically, the pre- period covers four years before the auditors of LSE Main Market companies were obliged to discuss KAMs in their reports (i.e., 2009-2012). Due to the data availability, in this study I manage to cover more years than the papers of Gutierrez et al. (2018) who also use difference-in-differences research design to study the consequences of the new reporting regime and of Lennox et al. (2023) who look at the auditor changes during the first two years of the new regime adoption. This is especially important given the research question: decisions to change the auditors could be taken

²AIM-listed companies are, by definition, different from Main Market firms in terms of size, growth stage, and regulatory environment. This constitutes a limitation of the current research design. In future iterations of the study, I plan to reinforce the identification strategy by introducing an alternative control group consisting of comparable U.S. firms, for which the expanded audit reporting requirements became mandatory only in 2020.

some years after the new rule was introduced. Such delay could occur due to the fact that both, auditors and clients, used the very first years to understand the essence of new regulation, consequences of the KAMs disclosure and variability of the disclosure among peer companies. To examine the effect the new audit reporting rules on auditor switch, I estimate the following difference-in-differences regression:

$$AUD_CH_{i,t} = \beta_0 + \beta_1 Post * Adopt_{i,t} + \sum \beta_j Controls_{i,t-1} + FFE + YFE + \varepsilon_{i,t} \quad (1.1)$$

where *AUD_CH* is an indicator variable that equals 1 if a company changes its auditor, and zero otherwise; *Post* is a time variable equal to one for fiscal year-ends on or after September 2013 and to zero otherwise; *Adopt* is a binary variable equal to one if a company belongs to a treatment group and zero if it belong to control group, and interaction term *Post * Adopt* is the main variable of interest which captures the difference-in-differences effect. Positive and statistically significant β_1 coefficient would mean that companies affected by extended audit report rules were more likely to change their auditors after the new reporting regime was enacted. In the majority of specifications, I include firm and year fixed effects. The former absorb all time-invariant differences between Main Market and AIM firms, while the latter capture macroeconomic conditions and other shocks common to all firms in a given year³

The set of controls used for this analysis consists of the variables that were established to be audit change determinants in the prior literature. This set includes *SIZE*, the natural logarithm of total assets; *GROWTH*, percentage change of total assets; *INVREC*, the ratio of the sum of inventories and receivables to total assets; *LOSS*, an indicator variable that equals 1 if the company reports a net loss, and 0 otherwise; *ROA*, the ratio of net income before extraordinary items to total assets; *MTB*, the ratio of market value of equity to book value of equity; *CASH*, the ratio of cash and cash equivalents to total assets;

³Although this specification makes it impossible to estimate the coefficients on *Post_t* and *Adopt_i* separately, it ensures that the estimated β_1 reflects only the within-firm change in the likelihood of auditor switches for treated companies relative to control firms after the reform, net of any firm-specific or time-specific confounding factors.

LEV, the ratio of total debt to total assets, *DACC*, the absolute value of discretionary accruals, which is estimated using the modified Jones model; *FEES*, the natural logarithm of total fees paid to the auditor; *TENURE*, the number of years the company is audited the current audit firm; *BIG4*, an indicator variable that equals 1 if the company is audited by a Big4 audit firm, and 0 otherwise, *SPEC*, an indicator variable that equals 1 if the company is audited by a firm which audits at least 25% of the client's industry's revenues, 0 otherwise; and *OPINION*, an indicator that equals 1 if the company receives anything but a unqualified audit opinion, and 0 otherwise. In line with the prior literature all the control variables are lagged by one year.

1.4.2 Post-adoption analysis

For the rest of the analysis I concentrate only on the post-adoption period and expand the sample until 2021. In order to test *H2* I estimate the following logistic regressions:

$$AUD_CH_{i,t} = \beta_0 + \beta_1 KAM_{i,t-1} + \sum \beta_j Controls_{i,t-1} + \varepsilon_{i,t} \quad (1.2)$$

where *AUD_CH* is a binary variable equal to one in the cases of audit firm change and to zero otherwise. The main variable of interest, *KAM*, is either the total number of risks disclosed in the audit report, or the number of atypical/unpredicted risks depending on the model specification.

The majority of control variables are the same as in Equation (1). Due to the different data availability for the post-implementation analysis which starts from 2013, I drop *FEES* and *OPINION* variables and use *AUD_FEES* and *GC* instead, where *AUD_FEES* is the natural logarithm of audit fees and *GC* is an indicator variable that equals 1 if the company receives a going concern opinion, and 0 otherwise. I also add to the set of controls *REST* which is an indicator variable that equals 1 if the company announces a restatement in the fiscal year, and 0 otherwise. All the regressors are lagged by one year. To investigate the link between the number of KAMs and within- and between-tier

switches, I run the multinomial logit regression (Landsman et al., 2009) where the outcome variable, *AUD_CH_TIER* equals to 0 if the company does not switch current auditors, to 1 if the switch of the auditor is lateral (Big 4 to Big4 or non-Big4 to non-Big 4), and to 2 if the switch is upward (from non-Big 4 to Big 4) or downward (from Big 4 to non-Big 4)⁴. As the related hypothesis concerns primarily downward audit switches, I then run the regression on the sample of Big4 clients only. In this case *AUD_CH_TIER* equals to 0 if the company does not switch its current Big 4 auditors, to 1 if the switch is to another Big 4 auditor, and to 2 if the switch is downward. Most of the variables of interest are same as in Equation (2). Following literature which discusses between-tier auditor switches (e.g., Landsman et al., 2009; Chang et al., 2010) *MISMATCH* variable is added to the set of controls. The variable, adapted from Shu (2000), indicates whether a client company is misaligned with its current Big 4 or non Big 4 auditor—in other words, whether the client, based on characteristics such as size, ROA, market-to-book ratio, and external financing, would be better suited to an auditor from a different tier.

Finally, to test whether audit switches are associated with following reduction in the number of reported KAMs, I estimate the following OLS regression:

$$KAM_REDUCT_{i,t} = \beta_0 + \beta_1 AUD_CH_{i,t} / AUD_CH_DET_{i,t} + \sum \beta_j Controls_{i,t} + \epsilon_{i,t} \quad (1.3)$$

where *KAM_REDUCT* is the reduction of the number of disclosed KAMs within a year (the variable can take negative values if the number of disclosed KAMs increases). Following previous analysis, the dependent variable is either reduction in total number of KAMs or reduction in the number of atypical KAMs⁵. The set of control variables is constructed using the model by Burke et al. (2023).

⁴The multinomial logit model allows the dependent variable - *AUD_CH* - to take more than two unordered categorical outcomes and estimates the relative likelihood of each outcome occurring compared to a baseline category—in this case, the “no switch” scenario.

⁵The number of unpredicted risks is excluded from the analysis as a very similar regression is used to predict the number of KAMs and using it as main variable of interest can introduce endogeneity issues.

1.4.3 Atypical and unpredicted KAMs

The first measure which I use to analyse the relations between unexpected KAMs and likelihood of auditor switch is *atyp_KAM_num* which is the difference between the number of reported risks and the industry-year average number. The second measure is *atyp_KAM_top* which is the sum of the KAMs covering topics which are atypical for a given industry-year group. Following Lennox et al. (2023) in the main analysis I consider topic to be typical if it appears in more than 50% of the audit reports in a given industry and year. In the sensitivity analysis I change the 50% threshold and explore the association between number of KAMs covering atypical topics and consequent auditor switches when more (and less) rigid rules of identifying topic as atypical are applied.

Finally, following Burke et al. (2022), I use a negative binomial regression model to predict the number of KAMs a given company should receive. The regressors which are expected to influence the number of reported KAMs are *SIZE*, *AGE*, *LOSS*, *ROA*, *MTB*, *LEV*, *SALES_GR*, *REST*, *AUD_CH*, *BIG4*, *TENURE*, *RISK_IND* and *RESTRUCT*. *AGE* is the natural logarithm of the number of years for which total assets are reported in Compustat; *SALES_GR* is percentage change of lagged sales; *RISK_IND* is an indicator variable that equals 1 if the issuer belongs to an industry with high litigation risk⁶, 0 otherwise; and *RESTRUCT* is an indicator variable that equals 1 if a company reports non-zero restructuring costs, and 0 otherwise. All the other variables are the same as in Equation (1). After estimating predicted number of KAMs, I construct the *unpred_KAM* variable which is calculated as a difference between the actual and predicted number of reported KAMs. This measure allows to assess to what extent the amount of disclosed KAMs was unexpected after other relevant company-specific information is taken into consideration.

⁶High litigation risk industries as defined by Francis et al. (1994) are: biotech (2833-36, 8731-34), computer (3570-77, 7370-74), electronics (3670-74), or retail (5200-5961).

1.4.4 KAM subject areas

In order to analyse whether some KAMs topics create especially strong controversy between companies and their auditors, I group reported KAMs in different subject areas. Collapsing some topics identified by Audit Analytics into broader subject areas makes the analysis more informative as many of the topics cover very similar issues. When grouping the topics into subject areas, I use the classification provided by Burke et al. (2022). I also follow the above mentioned study in focusing only on those subject areas which represent at least five percent of unique KAMs in my sample. The resulting subject areas are INTANGIBLES, REVENUE, M&A, GOING CONCERN, PPE, INVENTORY, and TAXES.

1.4.5 Sample Selection

I download the initial lists of all the securities traded on the LSE Main Market and AIM during 2013–2021 from the LSE website⁷. Then, I merge the identified International Securities Identification Numbers (ISINs) — which are the identifiers provided in the LSE files — with the Refinitiv Worldscope data to get a sample of unique companies for my analysis (i.e., to drop duplicates which arise because one company may have multiple ISINs). I also obtain all the financial and market information needed to construct control variables from Worldscope. For the audit-related variables, I use different data sources depending on the analysis year coverage. Audit Analytics Europe, which has better coverage of audit-related data, provides this information only starting from 2010, which is sufficient for the post-adoption analysis. For it, I obtain audit firm, going concern opinion, and audit fees data from Audit Analytics Europe, which also includes information about reported KAMs. However, for my difference-in-differences sample, which starts in 2009, I need data from 2008 in order to construct the variable capturing auditor changes between 2008 and 2009 and obtain lagged controls. For this part of the analysis, I obtain

⁷<https://www.londonstockexchange.com/reports?tab=equities>

I do not download the information for the prior years, as it serves only for constructing the pre-treatment part of the sample, where the firm's classification into Main Markt or AIM is irrelevant.

information about the company's audit firm and audit opinion from Compustat Global, and total fees paid to the auditor from Refinitiv.

Combining the LSE and Refinitiv datasets and removing ISIN duplicates, I arrive at a sample of 2 715 unique companies, which are used either in both analyses (DiD and post-adoption) or in only one of them, depending on the years during which a company is publicly traded. Table 1.1 provides further details of the sample selection process for both subsamples. For the DiD analysis, I require firms to have at least one observation in both the pre- and post-adoption periods and start with 8 285 firm-year observations for which data on the dependent variable (AUD_CH) is available. Following Gutierrez et al. (2018), I eliminate non-U.K. incorporated companies and companies operating in the financial sector (SIC codes 6000–6999). After dropping all observations with missing financial or audit-related data, I arrive at a sample of 4 545 firm-year observations. For the post-adoption analysis, I start with 10 901 firm-year observations for which data on both the change of the auditor and the number of received KAMs is available. I apply the same sample selection procedure as in the case of DiD subsample and arrive at 3 833 firm-year observations. Additionally, I report the number of observations used in the logit specification, which is the main model throughout the analysis given the binary nature of the dependent variable. In this case, the inclusion of firm fixed effects, which account for unobservable, time-invariant firm characteristics, restricts the sample to companies that have changed their auditors at least once during the sample period.

Table 1.1: Sample Selection

DiD analysis: 2009-2016	Treatment Group	Control Group
All firm-year observations	3 002	5 283
- non U.K. incorporated companies	(307)	(1 340)
- U.K. incorporated financial companies	(87)	(189)
- observations with missing control variables	(585)	(1 232)
Firm-year observations in the final sample	2 023	2 522
- cases when a company has never changed its auditor throughout the sample	(1 358)	(1 515)
Firm-year observations in the reduced final sample	665	1 007
Post-adoption analysis: 2013-2021		
All firm-year observations	10 901	
- non-U.K. incorporated companies	(2 045)	

Table 1.1: Sample Selection (continued)

- U.K. incorporated financial companies	(3 327)
- companies with missing control variables	(1 696)
Firm-year observations in the final sample	3 833
- cases when a company has never changed its auditor throughout the sample	(1 814)
Firm-year observations in the reduced final sample	2 019

1.5 Results

1.5.1 Difference-in-differences analysis

Table 1.2 presents descriptive statistics for all variables used in estimating Equation (1). Reflecting the fact that AIM is an LSE sub-market designed for small- and medium-sized growth companies, my control group (ADT=0) consists of smaller, faster-growing firms compared to the treatment group (ADT=1). Regarding audit-related variables, consistent with existing literature suggesting that larger companies are more likely to engage Big 4 auditors and pay higher audit fees, the table shows higher FEES and BIG4 values for companies listed on the Main Market across the periods. Furthermore, companies traded on the AIM sub-market are more likely to receive going concern opinions, tend to be less profitable, and are more likely to report losses. Despite these differences, the descriptive statistics reveal some similar trends across both markets over time. For instance, average firm size and market-to-book ratios increase in both the Main Market and AIM segments of the LSE. Additionally, in the post-period, companies from both groups became more likely to hire Big 4 auditors and pay higher total audit fees. For the main variable of interest, AUD_CH, the table indicates that prior to the enactment of expanded audit reporting rules, companies on the AIM sub-market were more likely to switch auditors (8.7% for the AIM sample versus 5.1% for the Main Market). However, in the post-period, the percentage of companies changing auditors aligned closely between the groups (7.8% for AIM compared to 7.4% for the Main Market). These statistics provide initial support to the idea that KAM reporting added tension to client-auditor

relationships, forcing some companies to switch their auditors.

Table 1.2: Descriptive statistics. Difference-in-differences

	ADT=0 POST=0			ADT=0 POST=1			Diff
	MEAN	MED	SD	MEAN	MED	SD	
AUD_CH	0.087	0.000	0.282	0.078	0.000	0.268	-0.009
SIZE	9.847	9.933	1.418	10.153	10.248	1.388	0.305***
GROWTH	0.196	0.036	0.736	0.207	0.039	0.789	0.011
INVREC	0.246	0.191	0.204	0.233	0.182	0.192	-0.013
LOSS	0.501	1.000	0.500	0.504	1.000	0.500	0.003
ROA	-0.134	-0.000	0.427	-0.159	-0.002	0.486	-0.025
MTB	2.172	1.309	4.379	2.676	1.697	4.584	0.504**
CASH	0.203	0.131	0.221	0.215	0.128	0.226	0.012
LEV	0.070	0.002	0.128	0.070	0.002	0.138	0.000
DACC	0.084	0.043	0.114	0.083	0.045	0.105	-0.001
FEES	4.275	4.220	0.961	4.395	4.382	0.908	0.120**
TEN	5.656	5.000	3.856	6.786	6.000	4.486	1.129***
BIG4	0.323	0.000	0.468	0.383	0.000	0.486	0.061**
OPINION	0.185	0.000	0.388	0.117	0.000	0.322	-0.067***
Observations	1 202			1 320			2 522
	ADT=1 POST=0			ADT=1 POST=1			Diff
	MEAN	MED	SD	MEAN	MED	SD	
AUD_CH	0.051	0.000	0.220	0.074	0.000	0.261	0.023*
SIZE	13.303	13.095	1.814	13.485	13.364	1.795	0.182*
GROWTH	0.061	0.025	0.315	0.055	0.027	0.306	-0.006
INVREC	0.288	0.260	0.197	0.281	0.258	0.196	-0.007
LOSS	0.143	0.000	0.350	0.169	0.000	0.375	0.026
ROA	0.050	0.052	0.096	0.040	0.052	0.151	-0.010
MTB	2.673	1.782	4.766	3.334	2.304	4.851	0.661**
CASH	0.116	0.078	0.116	0.116	0.081	0.111	0.001
LEV	0.158	0.125	0.164	0.166	0.138	0.163	0.008
DACC	0.052	0.028	0.078	0.052	0.030	0.073	0.000
FEES	6.541	6.479	1.318	6.561	6.466	1.362	0.019
TEN	10.033	9.000	6.295	11.415	11.000	7.115	1.381***
BIG4	0.899	1.000	0.301	0.939	1.000	0.240	0.039**
OPINION	0.068	0.000	0.253	0.039	0.000	0.193	-0.030**
Observations	964			1 059			2 023

Note: The Table presents descriptive statistics for all variables used in the DiD analysis. All the variables are as described in Appendix 1, and the continuous variables are winsorized at the 1st and 99th percentiles.

Table 1.3 presents the results of the difference-in-differences analyses examining the effect of the implementation of the extended audit reporting rules on auditor switches. The first three columns report estimated coefficients from logit regressions. Column (1) shows the baseline specification without fixed effects, which allows the coefficients on $Post_t$ and $Adopt_i$ to be estimated separately.⁸ Column (2) presents the results from the preferred

⁸In the reported specifications, industry fixed effects are included to capture unobserved factors shared by firms within the same industry. The results are robust to excluding these fixed effects (untabulated).

specification, which includes the full set of control variables as well as firm and year fixed effects, thereby accounting for unobserved heterogeneity across firms and common shocks over time. Column (3) adds an additional control variable, *MA*, an indicator equal to one if the company underwent a merger or acquisition in the two most recent years and zero otherwise. The results with and without the *MA* variable are reported separately because including this control substantially reduces the sample size.

The logit regression is chosen as the main one throughout the analysis as the outcome variable *AUD_CH* is binary. However, combining the logit specification with fixed effects leads to a substantial reduction in sample size, as all firms with no variation in the outcome variable are automatically dropped from the estimation⁹. To address this issue, I also present estimates from linear regressions, which allow me the full sample of 4 545 observations while including fixed effects. Column (4) reports the results with no fixed effect included and Column (5) presents the coefficients from a linear regression that includes all control variables except *MA*. Including *MA* as a control variable (untabulated) does not change the statistical significance of the coefficient of interest but reduces the sample size. Finally, Column (6) reports the results when the observations are attributed entropy balancing weight which allows to partially address the problem of substantial difference between companies in treated and control groups. The entropy balancing focuses on a subset of control variables, as balancing on all variables is infeasible due to collinearity. Specifically, observations are balanced based on *SIZE*, which reflects the primary distinction between companies traded on the Main Market and AIM segments of the LSE. Additional variables, such as *ROA*, *MTB*, and *INVREC* ratios, are also used to calculate the balancing weights.

⁹This issue is discussed by Chamberlain (1980), who recommends using the conditional fixed-effects logit model, which conditions out the fixed effects and relies solely on within-unit variation. Consequently, units exhibiting no within-unit variation in the dependent variable are excluded from the estimation, as they do not contribute to the identification of the structural parameters. The unconditional (joint) likelihood approach instead attempts to estimate the fixed effects directly, which leads to fixed-effect estimates of $\pm\infty$ for units with constant outcomes and therefore fails to yield meaningful estimates of the coefficients of interest. See also Chamberlain (1984) and Wooldridge (2010).

Table 1.3: Difference-in-differences analysis

<i>dependent variable</i>	<i>Logit</i>			<i>OLS</i>		
	(1)	(2)	(3)	(4)	(5)	(6)
POST	-0.074 (-0.49)			-0.004 (-0.45)		
ADOPT	-0.514*** (-2.16)			-0.029** (-1.98)		
POST*ADOPT	0.501** (2.09)	1.047*** (2.69)	1.251*** (3.09)	0.029* (1.90)	0.029* (1.85)	0.044** (2.15)
SIZE	0.112* (1.75)	0.047 (0.21)	-0.214 (-0.84)	0.007 (1.45)	0.004 (0.30)	-0.002 (-0.08)
GROWTH	0.095 (1.18)	-0.050 (-0.37)	-0.016 (-0.10)	0.008 (1.24)	0.003 (0.33)	0.017 (0.88)
INVREC	0.609* (1.95)	0.045 (0.04)	0.012 (0.01)	0.050** (2.25)	0.031 (0.42)	-0.078 (-0.62)
LOSS	0.155 (1.05)	0.447 (1.64)	0.455 (1.58)	0.008 (0.73)	0.012 (0.85)	0.033* (1.68)
ROA	-0.235 (-1.39)	-0.311 (-0.95)	-0.263 (-0.67)	-0.020 (-1.42)	-0.048* (-1.83)	-0.025 (-0.50)
MTB	0.023 (1.97)	0.011 (0.51)	0.016 (0.64)	0.002** (2.03)	0.001 (1.05)	0.009 (1.46)
CASH	0.205 (0.58)	1.230 (1.57)	1.776** (1.98)	0.014 (0.56)	0.081* (1.69)	-0.147 (-1.03)
FEES	0.115 (1.39)	0.228 (1.13)	0.302 (1.48)	0.007 (1.34)	0.014 (1.55)	-0.015 (-0.82)
LEV	0.071 (0.17)	0.812 (0.84)	0.227 (0.20)	0.004 (0.14)	0.068 (1.20)	0.098 (1.05)
DACC	0.042 (0.07)	0.546 (0.57)	0.962 (0.94)	0.005 (0.11)	-0.023 (-0.39)	0.010 (0.11)
TEN	0.001 (0.11)	0.831*** (11.47)	0.775*** (10.63)	0.000 (0.09)	0.031*** (13.05)	0.025*** (9.48)
BIG4	-0.874*** (-5.34)	-1.923*** (-5.25)	-1.943*** (-4.84)	-0.057*** (-5.37)	-0.186*** (-4.82)	-0.311*** (-5.22)
OPINION	0.837 (5.28)	0.965*** (3.24)	1.210*** (3.84)	0.077*** (5.72)	0.066*** (3.00)	0.036 (1.21)
MA			0.850* (1.84)			
<i>Const.</i>	-2.346*** (-22.97)				-0.187 (-1.12)	0.273 (1.01)
Firm FE	No	Yes	Yes	No	Yes	Yes
Year FE	No	Yes	Yes	No	Yes	Yes
Ind-ry FE	Yes	No	No	Yes	No	No
Obs	4 545	1 672	1 417	4 545	4 545	4 545
Pseudo R-sq. & R-sq.	0.038	0.405	0.409	0.022	0.127	0.165

Note: The Table reports estimates from DiD regressions examining the effect of the extended audit reporting rules on auditor changes. The dependent variable is *AUD.CH*, an indicator equal to one if a firm switches its auditor. Columns (1)–(3) present logit specifications, and Columns (4)–(6) report OLS estimates. All variables are defined as detailed in Appendix A. Continuous variables are winsorized at the 1st and 99th percentiles to mitigate the influence of extreme observations. Standard errors are clustered at the firm level. *t*-statistics are reported in parentheses, and *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

The interaction term $POST * ADOPT$, which captures the difference-in-differences effect, is positive and statistically significant across all models indicating that companies included in the treatments group experienced an increase in the likelihood of voluntary audit change after the adoption of the expanded audit reporting rules. Thus, for companies traded on the LSE, which constitute the treatment group, the likelihood of changing auditors increased from 37% in the pre-period to 45% in the post-period following the introduction of KAM reporting rules. The results of OLS specification suggest that the probability of changing an auditor increased by 3% for the companies traded on LSE in the post adoption period.

The negative coefficient on $ADOPT$ indicates that, prior to the introduction of the expanded audit reporting rules, LSE Main Market firms had a significantly lower likelihood of auditor switching compared to AIM firms. One possible explanation for this finding lies in the structural characteristics of the UK audit market. The audit market for larger, Main Market companies has historically been dominated by the Big 4 firms, resulting in limited switching opportunities and long-standing audit relationships (Oxera, 2006; Kittsteiner & Selvaggi, 2008). In contrast, AIM companies — being smaller and subject to a less concentrated audit market — can choose from a broader range of non-Big 4 auditors which increases the range of viable auditor selections and potentially leads to more frequent auditor switching (Xue & O’Sullivan, 2023).

Most of the control variables are not significant at any reasonable level which can be explained by the fact that both year and firm fixed effects are included the models, controlling for time-invariant firm characteristics and temporal trends. Audit related variables instead are also consistently significant throughout the Table. Auditor tenure and receiving an opinion other than an unqualified opinion are positively associated with the likelihood of auditor changes, consistent with the literature on companies’ opinion shopping behavior (e.g., Lennox, 2000; Chung et al., 2019) which suggests that companies receiving an unfavorable opinion from their auditor are more likely to switch the latter in search of a more ‘tolerant’ one. The *Big 4* coefficient is also statistically significant: companies in the sample are less likely to switch their auditor if audited by one of Big 4

companies. These results are in line with the some other studies of audit switches (e.g., Aobdia, 2018; Chung et al., 2019; Hunt et. al., 2022).

To facilitate interpretation, I also estimate a simplified specification that replaces year fixed effects with a *POST* dummy. While this model is less rigorous than the main DiD specification, it provides a direct estimate of the average change in auditor-switch probability across all firms after the reform period. The results presented in Appendix B (Table 1.12) are consistent with the main findings. The coefficient on *POST* is negative and statistically significant, indicating that the overall frequency of auditor switches declined across all firms after 2013. Importantly, the interaction term $POST * ADOPT$ remains positive, suggesting that this general downward trend was less pronounced for Main Market firms affected by the new reporting rules.

One potential flaw in analyzing auditor switch rotation during this period is the simultaneous introduction of expanded reporting rules and mandatory audit firm rotation. The latter, implemented in the UK in 2016, required companies listed on regulated markets (including the Main Market of the LSE) to rotate auditors every 10 years. However, transitional provisions allowed for exceptions: audit engagements of 20 or more consecutive years as of 16 June 2014 could continue until 17 June 2020, while those of 11 to 20 consecutive years could continue until 17 June 2023, after which the auditor could not be reappointed. Consequently, the initial cases of mandatory rotation fall outside the difference-in-differences sample period and only partially overlap with the post-KAM implementation analysis discussed further.

1.5.2 Post-adoption analysis

In the following section I discuss the results of the cross-sectional analysis. Table [1.4](#) Panel A contains descriptive statistics for the main variables of interest: on average, an audit report contains 3 KAMs, the minimum amount of reported KAMs is 1, while the maximum is 11. Throughout the observation period, firms in the full sample received 12 440 KAMs in total, of which 6 934 were assigned to firms included in the logit estimation

sample. The distribution of the number of KAMs which cover atypical topics (where a topic is considered to be atypical if it appears in less than 50% of reports for a given year and industry) is quite similar to the one of *KAM_N*. The maximum amount of atypical number of the KAMs which a company receives in my sample is 7. Also, a company may receive up to 3 KAMs less as compared to the year-industry average. As for the company-specific predictions for the amounts of reported KAMs, at most a company receives 4 KAMs less and 7 KAMs more as compared to the predicted numbers. Panel B of Table 1.4 contains descriptive statistics for the rest of the variables.

Table 1.4: Descriptive statistics

Panel A: KAM-related Variables

Variable	N obs	Mean	Min	Q1	Med	Q3	Max	Std.Dev
KAM_N	3 833	3.25	1	2	3	4	11	1.52
atyp_KAM_num	3 833	0.06	-3	-1	0	1	7	1.46
atyp_KAM_top	3 833	2.94	0	2	3	4	10	1.48
unpr_KAM_num	3 774	0.01	-4	-1	0	1	7	1.34

Panel B: Control Variables

Variable	Mean	Q1	Med	Q3	Std. Dev.
AUD_CH	0.11	0.00	0.00	0.00	0.31
SIZE	12.36	10.66	12.32	13.99	2.34
GROWTH	0.15	-0.04	0.05	0.17	0.59
INVREC	0.25	0.10	0.21	0.36	0.19
LOSS	0.36	0.00	0.00	1.00	0.48
ROA	-0.04	-0.05	0.03	0.07	0.30
MTB	3.19	1.09	2.10	4.01	4.52
CASH	0.16	0.05	0.10	0.21	0.17
LEV	0.16	0.01	0.11	0.25	0.18
DACC	0.09	0.02	0.05	0.11	0.12
AUD_FEES	12.57	11.45	12.42	13.54	1.50
TEN	5.39	3.00	5.00	8.00	3.13
BIG4	0.62	0.00	1.00	1.00	0.48
SPEC	0.29	0.00	0.00	1.00	0.45
GC	0.11	0.00	0.00	0.00	0.31
REST	0.06	0.00	0.00	0.00	0.24
Obs	3 833 ¹⁰				

¹⁰The sample differs from that employed in the DiD analysis for two reasons: (1) the period is extended to 2021, and (2) it begins in 2014, as the lagged number of KAMs is required for the analysis.

Note: The Table reports descriptive statistics for the KAM-related variables (Panel A) and for the control variables used in the cross-sectional analysis (Panel B). KAM-related variables are defined as follows: *KAM_N* is the total number of reported KAMs; *atyp_KAM_num* is the difference between the number of reported KAMs and the industry-year average; *atyp_KAM_top* is the number of reported KAMs covering topics that are atypical within the corresponding industry-year (a topic is considered atypical if it appears in fewer than 50% of reports unless otherwise specified); and *unpred_KAM_num* is the difference between the number of reported KAMs and the firm-specific number predicted using the Burke et al. (2022) model. All other variables are defined in Appendix A. Continuous variables are winsorized at the 1st and 99th percentiles.

Table 1.5 reports the estimated coefficients from the regressions examining the relationship between the probability of an auditor switch and the number of reported KAMs. Panel A presents the estimates from the logistic specification, which is more appropriate given the binary nature of the dependent variable. Panel B reports the OLS estimates, which, although less suitable for a binary outcome, allow the use of the full sample while controlling for both company and year fixed effects.¹¹ In the logit specification the sample is reduced to 2 019 as the estimation method requires that each group (i.e., individual firm) has changed its auditor at least once during the sample period. KAM coefficients presented in Panel A Table 1.5 are mainly significant throughout the table with the only exception being specification with the number of KAMs covering atypical for the industry topics as the variable of interest. The probability of having auditor switch increases with the total number of reported KAMs (Column 1 - *KAM_N*); with the number of KAMs above industry-year average amount (Column 2 - *atyp_KAM_num*); and with the number of KAMs which exceeds the amount predicted using company-specific variables (Column 5 - *unpr_KAM_num*)¹². These results provide some support of the idea that new audit disclosure rules created additional tension in client-auditor relations. In terms of economic significance, having additional KAM increase the odds by 22.2% (*KAM_num*) - 25% (*unpr_KAM_num*). If taking as a baseline the probability of changing auditor equal to 10% (sample average), receiving of an additional KAM increases this probability to 12,2% (*KAM_num*) - 12,5% (*unpr_KAM_num*).

¹¹For brevity, Panel B reports only the coefficients of interest, omitting the control variables. The complete table is provided in the Appendix.

¹²The results of the negative binomial regression model used for predicting the company-specific number of KAMs are reported in the Appendix.

Table 1.5: Number of reported KAMs and Audit Switch Probability

Panel A: Logistic Regression						
	KAM_num	atyp_KAM_num	atyp_KAM_num	atyp_KAM_top	unpr_KAM_num	unpr_KAM_num
<i>KAM</i>	0.201** (2.39)	0.207** (2.53)		0.128 (1.61)	0.223*** (2.75)	
<i>KAM_extra</i>			0.143 (1.50)			0.118 (1.04)
<i>KAM_miss</i>			-0.317* (-1.89)			-0.390*** (-2.64)
SIZE	-0.983** (-2.14)	-0.987** (-2.16)	-1.008** (-2.22)	-0.971** (-2.10)	-0.934** (-2.01)	-0.935** (-2.01)
GROWTH	0.293* (1.85)	0.293* (1.87)	0.304* (1.93)	0.298* (1.89)	0.281* (1.73)	0.284* (1.75)
INVREC	4.520** (2.51)	4.559** (2.57)	4.586*** (2.61)	4.388** (2.44)	4.296** (2.40)	4.296** (2.42)
LOSS	0.00568 (0.02)	0.0109 (0.05)	0.0264 (0.11)	0.0382 (0.16)	0.123 (0.52)	0.125 (0.53)
ROA	0.178 (0.33)	0.183 (0.34)	0.209 (0.38)	0.154 (0.28)	0.289 (0.52)	0.313 (0.56)
MTB	-0.0116 (-0.55)	-0.0114 (-0.54)	-0.0115 (-0.55)	-0.0104 (-0.49)	-0.0134 (-0.64)	-0.0132 (-0.62)
CASH	1.356 (1.03)	1.303 (0.99)	1.279 (0.97)	1.309 (1.00)	1.350 (1.00)	1.318 (0.98)
LEV	2.427*** (2.64)	2.466*** (2.66)	2.488*** (2.69)	2.418*** (2.63)	2.611*** (2.71)	2.752*** (2.88)
DACC	-0.935 (-1.51)	-0.927 (-1.50)	-0.942 (-1.51)	-0.988 (-1.57)	-0.822 (-1.31)	-0.862 (-1.37)
AUD_FEES	0.434 (0.90)	0.411 (0.86)	0.428 (0.90)	0.515 (1.07)	0.382 (0.79)	0.389 (0.81)
TENURE	0.657*** (6.69)	0.656*** (6.69)	0.656*** (6.73)	0.658*** (6.72)	0.651*** (6.69)	0.653*** (6.74)
SPEC	-0.166 (-0.36)	-0.172 (-0.37)	-0.164 (-0.35)	-0.172 (-0.37)	-0.155 (-0.34)	-0.153 (-0.33)
GC	0.309 (1.00)	0.308 (0.99)	0.341 (1.08)	0.352 (1.14)	0.288 (0.93)	0.304 (0.99)
REST	-0.310 (-1.10)	-0.308 (-1.08)	-0.312 (-1.09)	-0.265 (-0.97)	-0.181 (-0.64)	-0.164 (-0.59)
BIG4	1.066 (1.60)	1.110* (1.67)	1.128* (1.73)	1.056 (1.59)	1.089 (1.63)	1.129* (1.72)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	2 019 ¹³	2 019	2 019	2 019	1 987	1 987
Pseudo R-sq.	0.529	0.529	0.530	0.526	0.529	0.530
Panel B: OLS Regression						
	KAM_num	atyp_KAM_num	atyp_KAM_num	atyp_KAM_top	unpr_KAM_num	unpr_KAM_num
KAM	0.027*** (3.94)	0.027*** (4.24)		0.022*** (3.35)	0.028*** (4.46)	
KAM_extra			0.027*** (3.10)			0.026*** (2.78)

¹³Replicating the analysis on the shorter sample employed for the DiD analysis is feasible only for OLS. Because the model includes the lagged number of KAMs, the usable period begins in 2014, reducing the OLS sample to 760 firm-year observations. The estimated coefficients (untabulated) preserve their signs but lose statistical significance. The logit model fails to converge due to insufficient within-firm variation.

Table 1.5: Number of reported KAMs and Audit Switch Probability (continued)

KAM_miss			-0.027** (-2.44)			-0.032*** (-2.98)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs	3 833 ¹³	3 833	3 833	3 833	3 787	3 787
R-sq.	0.180	0.180	0.180	0.178	0.181	0.181

Note: The Table reports estimates from regressions examining the association between the number of reported KAMs and the probability of an auditor switch. The dependent variable is *AUD_CH*, an indicator equal to one if the firm changes its auditor. Panel A presents logistic regressions and Panel B reports OLS estimates. KAM-related variables are defined as follows: *KAM_N* is the total number of reported KAMs; *atyp_KAM_num* is the deviation from the industry-year average number of KAMs; *atyp_KAM_top* counts KAMs on atypical industry-year topics (a topic is atypical if it appears in fewer than 50% of reports); *unpr_KAM_num* is the deviation from the firm-specific predicted number of KAMs. All control variables are defined in Appendix A. Continuous variables are winsorized at the 1st and 99th percentiles. Standard errors are clustered at the firm level. *t*-statistics are reported in parentheses; *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

For the atypical for the industry -*atyp_KAM_num* - and unpredicted company-specific -*unpr_KAM_num* - KAMs I also construct *KAM_extra* and *KAM_miss* variables separately where *extra* equals to *atyp_KAM_num* (*unpr_KAM_num*) when the latter is positive and 0 otherwise; and *miss* equals to *atyp_KAM_num* (*unpr_KAM_num*) when the latter is negative and 0 otherwise. The results of the regression where these variables are the main variables of interest are reported in Columns (3) and (6) of Table 1.5. This subdivision of the atypical and unpredicted KAM numbers suggests that companies receiving less KAMs than expected are less likely to change their auditors.

Results presented in Panel B of Table 1.5 generally confirm the findings discussed above. KAM-related variables appear to be statistically significant at 1% confidence level and of predicted signs throughout the specification. Receiving additional KAM is associated with a 3% increase in the probability of auditor switch. Moreover, results presented in Columns (3) and (6) also suggest that under-reporting of KAMs is associated with decrease in such probability.

The results reported in Panel A Table 1.5, Column 4, are not statistically significant at the 10% confidence level, questioning the association between the number of KAMs covering

topics received by fewer than a half of companies in the same industry and the probability of an audit switch. In order to deepen this part of topic-related analysis, I construct alternative *atyp_KAM_top* measures using different percentage thresholds to classify topics as atypical topics. Column (1) of Table 1.6 shows the coefficient of *atyp_KAM_top* when topics which are received by at least 15% of the companies operating in the same receive in a particular year considered to be typical, whereas in Column (14) a topic become typical only if 85% of the industry-year audit reports contain it. The coefficients are positive and statistically significant starting from the 40% threshold. As the threshold used to classify topic as typical for the industry goes up (i.e., as more and more topics start being considered atypical), the statistical significance of the association between number of atypical KAMs received by a company and probability of auditor switch increases. These results provide limited support for the idea that KAM topic coverage influences client-auditor relationships. The number of atypical KAMs approaches the total number of reported KAMs in the final columns of the Table, where the magnitude and statistical significance of the KAM coefficient are at their highest. Such results suggest that it is the overall number of reported KAMs rather than their specific topic coverage that matters.

Table 1.6: Sensitivity analysis: different thresholds for identifying typical KAM topics

[illegible]

Note: The Table presents a sensitivity analysis in which the definition of an atypical KAM topic is varied by applying alternative industry–year frequency thresholds. The dependent variable is AUD CH, an indicator equal to one if the firm changes its auditor. For each column, *atyp_KAM_top* counts the number of reported KAMs whose topics fall below the corresponding threshold (15%–85%). All regressions include the full set of controls, as well as firm and year fixed effects. All variables are defined in Appendix A. Continuous variables are winsorized at the 1st and 99th percentiles. Standard errors are clustered at the firm level. *t*-statistics are reported in parentheses; *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Finally, Tables 1.7 and 1.8 reports the results of subject areas analysis. Table 1.7 shows the mapping of unique KAM topics to the broader subject areas. The most commonly encountered subject area covers intangibles-related issues and the second popular area comprises revenues-related topics. Differently from the U.S. companies analysed by Burke et al. (2022), the U.K. companies quite often receive going concern¹⁴ (8.51% of the unique KAMs) and inventory-related (5.49%) KAMs. All the subject areas which represent less than five percent of unique KAM topics in the sample are excluded from this analyses.

Table 1.7: KAM Subject Areas

KAM Topic	N	% of unique KAMs	Subject Area	% of unique KAMs
Goodwill And Intangible Assets	762	6.13	INTANG	18.40
Goodwill	583	4.69	INTANG	18.40
Deferred And Capitalized Costs	497	4.00	INTANG	18.40
Other Intangible Assets	445	3.58	INTANG	18.40
Revenue And Other Income	1447	11.63	REV	17.48
Revenue From Customer Contracts	666	5.35	REV	17.48
Sales Return And Allowances	44	0.35	REV	17.48
Revenue Recognition-Gift Card And Loyalty Programs	19	0.15	REV	17.48
Subsidiary/Affiliate	664	5.34	MA	10.38
Business Combinations	541	4.35	MA	10.38
Equity Investments And Joint Ventures	67	0.54	MA	10.38

¹⁴Even though the regulation states that "... material uncertainty related to events or conditions that may cast significant doubt on the entity's ability to continue as a going concern" should be discussed separately and not in the KAM section of audit report, the subject area is still quite frequent among the KAMs received by the U.K. companies. To the best of my knowledge, the reports do not explicitly mention going concern among other significant risks in the KAM section of their reports but rather discuss the related issues when describing other KAMs. For instance, with regard to the 2020 Annual Report of Fidelity China Special Solutions auditors point out going concern issues while assessing the impact of COVID-19. Another example is independent auditor's report to Lonmin Plc of 2017 where the auditors explicitly highlight only impairment-, inventory- and valuation-related KAMs, but start the KAMs section with the following words: "Going concern is the most significant key audit matter and is described in section 2 above. We summarize below the other key audit matters..."

https://s3-eu-west-1.amazonaws.com/eumultisitev4prod-live-eb461540d2184169bb77db2b062d9318-f268f99/filer_public/68/d8/68d87fb9-ba6c-4f63-8db2-d509bbc413cc/china-annual-report-2020.pdf

https://minedocs.com/17/Lonmin_AR2017.pdf

Table 1.7: KAM Subject Areas (continued)

Consolidation	19	0.15	MA	10.38
Going Concern	1059	8.51	GC	8.51
Property, Plant, And Equipment	411	3.30	PPE	7.13
Long-Lived Assets	282	2.27	PPE	7.13
Leases	108	0.87	PPE	7.13
Land And Stores Under Construction	52	0.42	PPE	7.13
Depreciation And Amortization	34	0.27	PPE	7.13
Inventory	683	5.49	INVENT	5.49
Other Income Taxes	244	1.96	TAX	5.01
Uncertain Tax Positions	229	1.84	TAX	5.01
Deferred Income Taxes	150	1.21	TAX	5.01
Others	4166	-	-	33.35
Total	12 440			

Note: The Table shows the distribution of unique KAM topics and their mapping into broader subject areas used in the subject-area analysis. Only subject areas representing at least 5% of unique KAM topics in the sample are retained.

Table 1.8 reports the results for the analysis of relations between KAMs covering particular subject areas and probabilities of following audit firm changes. In Column (1) all the KAMs received by companies are considered when constructing subject areas variables, whereas in the other columns only subject areas of atypical at the industry-year level KAMs are considered. None of the individual subject areas result to be statistically significant suggesting. There seem to be no particular KAM topics that reflect (or create) especially high disagreement between companies and their auditors. The results of the OLS specification presented in the Appendix confirm these findings: after inclusion of firm fixed effects the subject areas coefficient lose their statistical significance at any conventional level.

Table 1.8: Auditor Changes and KAM Subject Areas

	AUD_CH					
	All KAMs	KAMs covering atypical topics				
<i>typical topics threshold</i>		45%	50%	55%	60%	65%
REV_sa	-0.034 (-0.14)	-0.176 (-0.67)	-0.194 (-0.75)	-0.045 (-0.18)	-0.186 (-0.74)	-0.276 (-1.05)
GC_sa	0.363 (1.60)	0.289 (1.24)	0.074 (0.33)	0.339 (1.54)	0.337 (1.53)	0.333 (1.51)
INTANGIBLES_sa	0.067 (0.28)	0.161 (0.70)	0.144 (0.62)	0.073 (0.31)	0.068 (0.28)	0.073 (0.30)
M&A_sa	-0.000 (-0.00)	0.015 (0.08)	0.005 (0.02)	-0.001 (-0.01)	-0.009 (-0.05)	-0.008 (-0.04)
PPE_sa	0.114	0.151	0.135	0.121	0.106	0.103

Table 1.8: Auditor Changes and KAM Subject Areas (continued)

	(0.35)	(0.46)	(0.41)	(0.36)	(0.32)	(0.31)
INVENTORY_sa	-0.241	-0.241	-0.378	-0.404	-0.349	-0.224
	(-0.44)	(-0.49)	(-0.87)	(-0.92)	(-0.80)	(-0.42)
TAX_sa	0.691	0.696	0.687	0.676	0.697	0.67
	(1.21)	(1.23)	(1.23)	(1.19)	(1.23)	(1.19)
SIZE	-0.893*	-0.926*	-0.912*	-0.894*	-0.867*	-0.867*
	(-1.87)	(-1.92)	(-1.87)	(-1.85)	(-1.80)	(-1.80)
GROWTH	0.299*	0.289*	0.281*	0.301*	0.288*	0.287*
	(1.88)	(1.84)	(1.76)	(1.89)	(1.83)	(1.82)
INVREC	4.703***	4.515**	4.594**	4.686***	4.698***	4.689***
	(2.58)	(2.51)	(2.54)	(2.58)	(2.59)	(2.58)
LOSS	0.049	0.051	0.062	0.044	0.056	0.059
	(0.21)	(0.22)	(0.27)	(0.19)	(0.24)	(0.25)
ROA	0.155	0.198	0.172	0.158	0.147	0.144
	(0.28)	(0.36)	(0.31)	(0.29)	(0.27)	(0.26)
MTB	-0.009	-0.008	-0.010	-0.011	-0.010	-0.009
	(-0.46)	(-0.40)	(-0.48)	(-0.51)	(-0.46)	(-0.44)
CASH	1.619	1.496	1.513	1.618	1.651	1.645
	(1.14)	(1.09)	(1.11)	(1.13)	(1.16)	(1.15)
LEV	2.564***	2.636***	2.724***	2.623***	2.631***	2.624***
	(2.82)	(2.86)	(2.98)	(2.86)	(2.85)	(2.82)
DACC	-1.022	-1.036	-1.045	-1.007	-1.014	-1.004
	(-1.58)	(-1.63)	(-1.62)	(-1.57)	(-1.57)	(-1.55)
AUD_FEES	0.569	0.583	0.584	0.580	0.575	0.588
	(1.19)	(1.22)	(1.22)	(1.23)	(1.21)	(1.24)
TENURE	0.659***	0.661***	0.663***	0.660***	0.663***	0.662***
	(6.76)	(6.78)	(6.73)	(6.72)	(6.70)	(6.66)
BIG4	1.053	1.046	1.052	1.041	1.039	1.038
	(1.58)	(1.56)	(1.54)	(1.54)	(1.54)	(1.53)
SPEC	-0.228	-0.209	-0.204	-0.207	-0.229	-0.242
	(-0.49)	(-0.45)	(-0.44)	(-0.45)	(-0.49)	(-0.52)
GC	0.332	0.416	0.410	0.359	0.346	0.340
	(1.04)	(1.31)	(1.27)	(1.13)	(1.08)	(1.07)
REST	-0.173	-0.197	-0.198	-0.153	-0.164	-0.180
	(-0.64)	(-0.72)	(-0.73)	(-0.56)	(-0.60)	(-0.66)
Const	-3.519***	-3.415***	-3.459***	-3.517***	-3.522***	-3.529***
	(-4.58)	(-4.46)	(-4.50)	(-4.56)	(-4.58)	(-4.59)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Ind FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs	2 019	2 019	2 019	2 019	2 019	2 019
Pseudo R-sq.	0.528	0.528	0.528	0.528	0.529	0.529

Note: The Table reports logistic regression estimates examining the association between KAMs covering specific subject areas and the probability of an auditor change. The dependent variable is AUD CH, an indicator equal to one if the firm changes its auditor. Column (1) uses all reported KAMs to construct subject-area variables, while Columns (2)–(6) restrict the analysis to the KAMs that are atypical within the corresponding industry-year. All models include the full set of control variables and both firm and year fixed effects. All variables are defined in Appendix A. Standard errors are clustered at the firm level. *t*-statistics are reported in parentheses; *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Next, to test the hypotheses related to possible opinion shopping, I turn to a more detailed analysis of the switching behavior associated with extensive KAM disclosure. Panel A

of Table 1.9 provides an initial descriptive step. Estimating a multinomial logit model for the full sample allows identifying which types of auditor switches — lateral versus between-tier — are related to extensive KAM reporting. In each specification, Columns (1) and (2) correspond to different contrasts within the multinomial model: Column (1) reports coefficients for the comparison between lateral switches and no switch, whereas Column (2) reports coefficients for the comparison between between-tier switches and no switch. The results indicate that the effect of KAMs is concentrated primarily in between-tier switches: although the coefficients in both columns are of the predicted sign, those in Columns (2) are generally larger and statistically significant.

Panel B then focuses on Big 4 clients—the only firms for whom a downward switch is feasible. Consistent with the hypothesis, if extensive KAM reporting generates client dissatisfaction and prompts a new form of opinion shopping, this effect should be most pronounced among Big 4 clients, as they are the ones who have an option to respond by switching “downward” to a potentially more accommodating non-Big 4 auditor. By restricting the analysis to current Big 4 clients, Panel B directly tests this mechanism. The results confirm that the increased likelihood of between-tier switching observed in Panel A is driven by Big 4 firms moving to non-Big 4 auditors, consistent with the conjectured opinion-shopping behavior.

In the Appendix I also present results when concentrating exclusively on the non-Big 4 sample. In contrast to the findings discussed above, receiving extensive amounts of KAMs does not seem to affect the probability of lateral or upward auditor switching for non-Big 4 clients. Table 1.10 indicates that the results are driven by companies receiving more KAMs than both the industry average and the number predicted by Burke et al. (2023) model as these companies are more likely to change auditors switching between auditor tiers. In contrast, there is no evidence that under-reporting of KAMs is associated with a reduced likelihood of switching auditors laterally or between tiers.

Table 1.9: Multinomial Logit Regression

Panel A. Whole Sample

Table 1.9: Multinomial Logit Regression (continued)

	KAM_num		atyp_KAM_num		atyp_KAM_top		unpr_KAM_num	
AUD_CH_TIER	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
KAM	0.073*	0.220***	0.079	0.213***	0.046	0.239***	0.098**	0.233***
	(1.82)	(2.99)	(1.61)	(2.68)	(0.75)	(3.17)	(2.24)	(3.33)
SIZE	-0.142*	-0.339***	-0.141**	-0.337***	-0.140**	-0.340***	-0.117*	-0.262*
	(-1.94)	(-2.84)	(-2.07)	(-2.66)	(-1.97)	(-2.84)	(-1.74)	(-1.87)
GROWTH	0.031	0.083	0.031	0.083	0.0312	0.082	0.040	0.009
	(0.34)	(0.45)	(0.30)	(0.43)	(0.33)	(0.47)	(0.43)	(0.03)
INVREC	0.618*	0.637	0.633*	0.651	0.609**	0.616	0.638*	0.724
	(1.87)	(1.20)	(1.76)	(1.20)	(2.02)	(1.24)	(1.86)	(1.26)
LOSS	0.080	0.238	0.081	0.252	0.092	0.235	0.129	0.295
	(0.60)	(0.83)	(0.52)	(1.00)	(0.52)	(0.82)	(0.86)	(1.18)
ROA	0.047	0.273	0.046	0.278	0.050	0.301	0.103	0.271
	(0.28)	(1.06)	(0.30)	(1.02)	(0.25)	(0.81)	(0.46)	(0.77)
MTB	0.012	-0.007	0.012	-0.007	0.012	-0.005	0.009	-0.012
	(0.88)	(-0.31)	(1.07)	(-0.28)	(0.82)	(-0.20)	(0.74)	(-0.52)
CASH	-0.381	0.294	-0.373	0.301	-0.392	0.332	-0.236	0.495
	(-0.85)	(0.45)	(-0.87)	(0.60)	(-0.97)	(0.49)	(-0.61)	(0.62)
LEV	0.668*	0.430	0.667	0.442	0.672	0.388	0.700	0.551
	(1.70)	(0.68)	(1.53)	(0.81)	(1.54)	(0.54)	(1.59)	(0.82)
DACC	0.033	-1.065*	0.035	-1.075	0.030	-1.077	0.093	-0.874
	(0.07)	(-1.82)	(0.07)	(-1.57)	(0.08)	(-1.62)	(0.19)	(-1.15)
AUD_FEES	0.164	-0.007	0.162*	-0.005	0.176*	-0.013	0.144	0.001
	(1.48)	(-0.05)	(1.77)	(-0.03)	(1.86)	(-0.08)	(1.63)	(0.00)
TENURE	0.096***	0.066***	0.096***	0.066***	0.096***	0.066***	0.096***	0.067***
	(10.89)	(4.94)	(10.33)	(5.43)	(12.46)	(5.12)	(13.27)	(4.97)
SPEC	0.201	-0.688***	0.198	-0.690***	0.215	-0.690***	0.198	-0.640**
	(1.11)	(-2.58)	(1.18)	(-3.03)	(1.28)	(-2.73)	(1.17)	(-2.52)
GC	-0.147	0.822**	-0.147	0.833**	-0.133	0.853**	-0.187	0.819**
	(-0.65)	(2.53)	(-0.66)	(2.48)	(-0.65)	(2.55)	(-0.72)	(2.24)
REST	0.317	-0.0434	0.316	-0.0369	0.329	-0.0416	0.359	-0.102
	(1.02)	(-0.09)	(1.09)	(-0.08)	(1.16)	(-0.09)	(1.27)	(-0.17)
BIG4	-0.725***	2.606***	-0.725***	2.611***	-0.737***	2.610***	-0.670***	2.493***
	(-2.97)	(5.60)	(-2.59)	(4.61)	(-3.32)	(5.51)	(-2.77)	(5.81)
MISMATCH	-0.508**	1.321***	-0.509**	1.317***	-0.507**	1.310***	-0.497**	1.354***
	(-2.02)	(4.03)	(-2.23)	(3.46)	(-2.22)	(3.53)	(-2.22)	(4.06)
Const	-4.641***	-18.46***	-4.356***	-17.76***	-4.709***	-18.39***	-4.535***	-18.92***
	(-5.19)	(-12.28)	(-4.69)	(-11.07)	(-5.96)	(-12.18)	(-5.17)	(-13.19)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs	3 833	3 833	3 833	3 833	3 833	3 833	3 787	3 787
Pseudo R-sq.	0.141	0.141	0.141	0.141	0.141	0.141	0.140	0.140

Panel B. Big 4 Sample

	KAM_num		atyp_KAM_num		atyp_KAM_top		unpr_KAM_num	
AUD_CH_TIER	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
KAM	0.086	0.211**	0.086	0.197**	0.085	0.238**	0.122*	0.225***
	(1.14)	(2.36)	(1.48)	(2.14)	(1.25)	(2.49)	(1.90)	(2.71)
SIZE	-0.014	-0.966***	-0.013	-0.963***	-0.011	-0.972***	0.016	-0.879***
	(-0.13)	(-4.72)	(-0.13)	(-4.98)	(-0.10)	(-5.22)	(0.15)	(-4.57)
GROWTH	0.115	0.136	0.117	0.138	0.116	0.139	0.128	-0.008
	(0.57)	(0.40)	(0.66)	(0.46)	(0.55)	(0.64)	(0.60)	(-0.02)

Table 1.9: Multinomial Logit Regression (continued)

INVREC	0.631 (1.47)	0.593 (0.76)	0.641 (1.42)	0.611 (0.85)	0.649 (1.28)	0.554 (0.77)	0.728 (1.40)	0.691 (1.17)
LOSS	0.341 (1.36)	0.385 (1.05)	0.346 (1.36)	0.400 (1.11)	0.346 (1.35)	0.377 (1.29)	0.411* (1.69)	0.439 (1.28)
ROA	0.213 (0.33)	0.642 (1.18)	0.212 (0.38)	0.639 (1.20)	0.219 (0.31)	0.668 (1.46)	0.263 (0.43)	0.674 (1.41)
MTB	0.031* (1.74)	-0.028 (-1.00)	0.031* (1.87)	-0.029 (-1.15)	0.032** (2.04)	-0.027 (-1.12)	0.025 (1.21)	-0.033 (-1.12)
CASH	-0.468 (-0.49)	-0.058 (-0.08)	-0.455 (-0.68)	-0.047 (-0.06)	-0.455 (-0.51)	-0.032 (-0.05)	-0.214 (-0.26)	0.212 (0.24)
LEV	-0.064 (-0.09)	0.290 (0.47)	-0.060 (-0.10)	0.322 (0.41)	-0.070 (-0.10)	0.242 (0.25)	0.109 (0.12)	0.390 (0.59)
DACC	-0.285 (-0.37)	-1.111 (-1.54)	-0.284 (-0.36)	-1.135 (-1.59)	-0.297 (-0.40)	-1.147* (-1.87)	-0.311 (-0.37)	-0.897 (-1.49)
AUD_FEES	0.108 (0.72)	0.120 (0.44)	0.108 (0.85)	0.126 (0.49)	0.109 (0.70)	0.114 (0.41)	0.081 (0.60)	0.118 (0.48)
TENURE	0.112*** (11.08)	0.077*** (3.49)	0.112*** (11.30)	0.077*** (4.11)	0.112*** (10.72)	0.077*** (4.55)	0.113*** (11.11)	0.077*** (4.03)
SPEC	0.177 (1.06)	-0.823*** (-3.13)	0.175 (1.07)	-0.826*** (-2.83)	0.179 (0.92)	-0.819** (-2.20)	0.173 (1.01)	-0.781*** (-2.66)
GC	0.275 (0.54)	1.181*** (2.97)	0.279 (0.72)	1.195*** (2.74)	0.293 (0.52)	1.218*** (4.11)	0.337 (0.84)	1.153*** (2.81)
REST	0.620 (1.51)	-0.146 (-0.26)	0.623 (1.25)	-0.139 (-0.32)	0.621 (1.15)	-0.146 (-0.28)	0.648 (1.45)	-0.227 (-0.38)
MISMATCH	0.064 (0.16)	-0.177 (-0.39)	0.062 (0.16)	-0.182 (-0.46)	0.067 (0.15)	-0.192 (-0.47)	0.012 (0.03)	-0.151 (-0.40)
Const	-6.867*** (-5.76)	-10.210*** (-4.02)	-6.559*** (-4.85)	-9.601*** (-3.98)	-6.898*** (-6.25)	-10.880*** (-4.41)	-6.775*** (-6.34)	-9.369*** (-4.47)
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs	2 480	2 480	2 480	2 480	2 480	2 480	2 452	2 452
Pseudo R-sq.	0.210	0.210	0.210	0.210	0.211	0.211	0.209	0.209

Note: The Table reports multinomial logit estimates examining the association between the number of reported KAMs and the type of subsequent auditor switch. Panel A reports results for the full sample, while Panel B restricts the analysis to current Big 4 clients. The dependent variable, *AUD_CH_TIER*, equals 0 when a company does not change its auditor, 1 when the switch is lateral, and 2 when the switch occurs between tiers. Each pair of columns shows coefficients from the multinomial model comparing (1) lateral switches vs. no switch and (2) between-tier switches vs. no switch. All specifications include the full set of control variables as well as industry and year fixed effects. All variables are defined in Appendix A; *t*-statistics are reported in parentheses, and *, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 1.10: Multinomial Logit Regression Results. Under- and Over-Reporting of KAMs

Whole Sample

Big 4 Sample

Table 1.10: Multinomial Logit Regression Results. Under- and Over-Reporting of KAMs (continued)

	atyp_KAM_num		unpr_KAM_num		atyp_KAM_num		unpr_KAM_num	
AUD_CH_TIER	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
KAM_extra	0.0494 (0.78)	0.327*** (3.62)	0.045 (0.79)	0.305*** (3.26)	0.094 (0.99)	0.332*** (2.72)	0.082 (0.93)	0.294*** (2.69)
KAM_miss	-0.133 (-1.29)	0.041 (0.20)	-0.195* (-1.77)	-0.069 (-0.38)	-0.071 (-0.50)	0.079 (0.36)	-0.192 (-1.41)	-0.085 (-0.33)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs	3 833	3 833	3 787	3 787	2 480	2 480	2 452	2 452
Pseudo R-sq.	0.142	0.142	0.141	0.141	0.211	0.211	0.210	0.210

Note: The table reports multinomial logit estimates examining whether under- or over-reporting of KAMs is associated with different types of auditor switches. The dependent variable, *AUD_CH_TIER*, equals 0 for no auditor change, 1 for a lateral switch within the same tier, and 2 for a between-tier switch, either upward or downward. *KAM_extra* measures the number of KAMs above the relevant benchmark (industry-year average or predicted number), while *KAM_miss* captures the shortfall relative to the same benchmark. Each pair of columns shows coefficients from the multinomial model comparing (1) lateral switches vs. no switch and (2) between-tier switches vs. no switch. All models include the full set of controls and industry and year fixed effects.

This part of analysis which aims to investigate particular patterns of switching behavior shows results that are to some extent aligned with opinion shopping behavior. Specifically, higher numbers of reported KAMs is associated with a greater likelihood of between-tier audit switches rather than lateral ones. A breakdown of the sample suggests that this behavior is primarily driven by former Big 4 clients opting for non-Big 4 auditors. This pattern aligns with findings of Amin et al. (2021), who demonstrate that opinion shopping-driven auditor switches are more likely to involve downward transitions, as clients seek more favorable opinions from less stringent auditors.

Lastly, I examine whether there is any evidence of a reduction in reported KAMs following auditor switches. This final step also provides an initial insight into the opinion-shopping hypothesis: if companies that switch auditors subsequently receive fewer KAMs, this would be consistent with at least some instances of successful shopping for a more accommodating auditor. The results presented in Panel A of Table 1.11 suggest that, for Big 4 clients, changing an auditor is indeed associated with a reduction in the number of disclosed KAMs. Specifications with all three dependent variables — the total number of

KAMs, the number of KAMs exceeding the industry average, and the number of KAM topics uncommon for the industry — show a positive association between auditor changes and a reduction in reported KAMs. Panel B of the Table reports the coefficients when auditor changes are categorized into within- and between-tier switches (*AUD_CH_TIER* equals 0 for no switch, 1 for lateral switches, and 2 for between-tier switches). The findings suggest that, for the Big 4 sample, downward switches primarily drive the results: switching from a Big 4 to a non-Big 4 auditor is, on average, associated with a reduction of approximately one (0.6) reported KAM. Among the control variables, *ASSETS_GR* and *LOSS* are statistically significant, indicating that rapid client growth and reporting losses are associated with an increase in the number of disclosed KAMs.

Table 1.11: Incoming Auditors - Reduction in Number of KAMs

PANEL A. SWITCHES VS NON SWITCHES

KAM_REDUCT	Whole Sample			Big 4 Sample			Non-Big 4 Sample		
	num	atyp_num	atyp_top	num	atyp_num	atyp_top	num	atyp_num	atyp_top
AUD_CH	0.155* (1.87)	0.164* (1.91)	0.095 (1.17)	0.282*** (2.79)	0.270** (2.51)	0.230** (2.32)	-0.128 (-0.86)	-0.078 (-0.53)	-0.227 (-1.56)
ASSETS_GR	-0.214*** (-4.01)	-0.184*** (-3.56)	-0.182*** (-3.84)	-0.418*** (-3.86)	-0.418*** (-3.90)	-0.318*** (-3.62)	-0.112* (-1.93)	-0.074 (-1.32)	-0.110** (-2.02)
LOSS	-0.239*** (-3.26)	-0.233*** (-3.03)	-0.242*** (-3.20)	-0.299*** (-3.11)	-0.300*** (-2.93)	-0.315*** (-3.17)	-0.089 (-0.70)	-0.094 (-0.74)	-0.083 (-0.68)
ROA	0.045 (0.39)	0.057 (0.44)	-0.075 (-0.67)	0.416 (1.63)	0.360 (1.31)	0.063 (0.26)	-0.094 (-0.76)	-0.090 (-0.61)	-0.103 (-0.76)
MTB	-0.005 (-0.70)	-0.006 (-0.79)	-0.003 (-0.50)	0.003 (0.28)	0.000 (0.04)	0.004 (0.45)	-0.013 (-1.13)	-0.014 (-1.18)	-0.011 (-1.18)
LEV	0.551** (2.45)	0.291 (1.25)	0.521** (2.30)	0.928*** (3.07)	0.656** (2.08)	0.919*** (3.01)	0.053 (0.15)	-0.143 (-0.38)	-0.109 (-0.31)
SALES_GR	0.046 (0.38)	0.056 (0.44)	0.091 (0.78)	0.123 (0.74)	0.249 (1.41)	0.208 (1.30)	0.042 (0.25)	-0.014 (-0.08)	0.026 (0.16)
REST	0.061 (0.71)	0.059 (0.66)	0.114 (1.34)	0.009 (0.08)	0.017 (0.16)	0.028 (0.16)	0.000 (-0.00)	0.028 (0.27)	0.130 (0.82)
RESTRUCT	-0.072 (-1.04)	-0.079 (-1.09)	-0.081 (-1.17)	-0.062 (-0.79)	-0.072 (-0.86)	-0.063 (-0.81)	0.017 (0.11)	0.027 (0.18)	-0.023 (-0.15)
Const	-0.038 (-0.29)	-0.350*** (-2.69)	-0.099 (-0.77)	-0.154 (-1.17)	-0.451*** (-3.33)	-0.223 (-1.64)	-0.036 (-0.04)	-0.499 (-0.52)	0.090 (0.10)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs	3 781	3 781	3 781	2 457	2 457	2 457	1 324	1 324	1 324
R-sq.	0.088	0.044	0.059	0.116	0.067	0.063	0.067	0.015	0.102

PANEL B. WITHIN AND BETWEEN TIER SWITCHES

KAM_REDUCT	Whole Sample			Big 4 Sample			Non-Big 4 Sample		
	num	atyp_num	atyp_top	num	atyp_num	atyp_top	num	atyp_num	atyp_top
AUD_CH==1	0.031	0.038	-0.037	0.136	0.103	0.094	-0.137	-0.078	-0.264*

Table 1.11: Incoming Auditors - Reduction in Number of KAMs (continued)

	(0.34)	(0.41)	(-0.41)	(1.18)	(0.85)	(0.83)	(-0.90)	(-0.50)	(-1.75)
AUD_CH=2	0.450***	0.464***	0.408**	0.652***	0.694***	0.574***	-0.041	-0.076	0.117
	(2.68)	(2.62)	(2.52)	(3.32)	(3.23)	(2.94)	(-0.07)	(-0.16)	(0.24)
SIZE	-0.214***	-0.184***	-0.181***	-0.419***	-0.420***	-0.319***	-0.112*	-0.074	-0.110**
	(-4.00)	(-3.55)	(-3.84)	(-3.88)	(-3.92)	(-3.65)	(-1.93)	(-1.32)	(-2.02)
LOSS	-0.243***	-0.238***	-0.246***	-0.308***	-0.310***	-0.323***	-0.088	-0.094	-0.077
	(-3.32)	(-3.08)	(-3.27)	(-3.19)	(-3.02)	(-3.25)	(-0.69)	(-0.74)	(-0.63)
ROA	0.042	0.055	-0.078	0.395	0.335	0.043	-0.094	-0.090	-0.102
	(0.37)	(0.43)	(-0.71)	(1.55)	(1.23)	(0.17)	(-0.75)	(-0.61)	(-0.75)
MTB	-0.006	-0.007	-0.004	0.001	-0.001	0.003	-0.013	-0.014	-0.011
	(-0.81)	(-0.90)	(-0.63)	(0.14)	(-0.10)	(0.32)	(-1.14)	(-1.18)	(-1.24)
LEV	0.561**	0.302	0.532**	0.968***	0.701**	0.955***	0.049	-0.143	-0.126
	(2.50)	(1.29)	(2.36)	(3.19)	(2.22)	(3.13)	(0.13)	(-0.38)	(-0.36)
GR_ASSETS	0.049	0.060	0.095	0.135	0.262	0.219	0.042	-0.014	0.028
	(0.41)	(0.47)	(0.81)	(0.81)	(1.49)	(1.37)	(0.25)	(-0.08)	(0.17)
REST	0.053	0.051	0.105	0.006	0.015	0.026	-0.003	0.026	0.117
	(0.61)	(0.57)	(1.23)	(0.06)	(0.14)	(0.25)	(-0.02)	(0.25)	(0.75)
RESTRUCT	-0.075	-0.082	-0.085	-0.066	-0.076	-0.067	0.014	0.027	-0.033
	(-1.08)	(-1.14)	(-1.22)	(-0.83)	(-0.91)	(-0.85)	(0.09)	(0.18)	(-0.21)
Const	-0.032	-0.344***	-0.093	-0.154	-0.451***	-0.224	-0.030	-0.499	0.113
	(-0.25)	(-2.64)	(-0.71)	(-1.16)	(-3.31)	(-1.63)	(-0.03)	(-0.52)	(0.13)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs	3 781	3 781	3 781	2 457	2 457	2 457	1 324	1 324	1 324
R-sq.	0.090	0.046	0.061	0.119	0.071	0.066	0.067	0.015	0.102

Note: The Table reports regression estimates examining the association between auditor changes and the subsequent reduction in the number of KAMs disclosed by incoming auditors. The dependent variable, *KAM_REDUCT*, measures the yearly change in the number of reported KAMs; it takes negative values when the number of disclosed KAMs increases. Panel A compares firms that switch auditors with those that do not, while Panel B distinguishes between lateral switches (*AUD_CH* = 1) and between-tier switches (*AUD_CH* = 2). All specifications include the full set of control variables as well as firm and year fixed effects. Standard errors are clustered at the firm level; *t*-statistics are reported in parentheses, and *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

The results of this section provide some initial evidence of successful KAM opinion shopping. Thus, companies which change their auditors and opt to switch from a Big 4 auditor to a non-Big one tend to receive fewer KAMs in the first year of engagement. To some extent these results are consistent with classical opinion shopping behavior when clients not satisfied with their auditor verdicts choose a new auditor who, for various reasons, is ready to issue a more favorable report. However, these results are rather preliminary and more work is to be done in terms of understanding what are the attributes of high quality KAM reporting and why some audit firms are more likely to report fewer amount of risky areas than the others.

1.6 Conclusion

In this paper I investigate the impact of expanded audit reporting rules implementation in the U.K. on the relations between companies and their auditors. Contrary to the viewpoint that KAM reporting is barely relevant and therefore neither auditors nor their clients are concerned with what is disclosed as KAMs, my findings suggest some evidence of enhanced tension between the two parties raised by the new risk disclosure requirements. In particular, the analysis shows that those U.K. companies which, after September, 2013 were obliged to have KAMs section in their audit reports experienced an increase in the probability of auditor switch as compared to the companies for which this requirement was postponed until July, 2017. Moreover, consistent with the idea that excessive KAM reporting indicates elevates tension between auditors and their clients I find that, in the post-implementation period, the number of disclosed KAMs is positively associated with the probability of auditor switch. The results hold when I regress this probability on the total number of reported risky areas as well as when I concentrate on atypical or unexpected KAMs only. In general, the results support the idea that companies aim to minimize the number of disclosed risky areas in the reporting and opt to change their auditor when the receiving excessive number of KAMS. In particular, ex clients of Big 4 companies become more likely to switch to non-Big 4 auditors which are known to be more client-friendly. Lastly, the paper demonstrates some initial evidence of successful KAM opinion shopping: companies switching to non-Big 4 auditors do receive fewer KAMs in the first year of audit engagement.

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1.8 Appendix A

Variable	Definition
AUD_CH	indicator variable that equals 1 if the company switches its auditor, and 0 otherwise
AUD_CH_TIER	categorical variable equal to 0 if the company does not switch its auditor, 1 if the switch is lateral (within-tier), and 2 if the switch is between-tier switch (upward or downward)
KAM_N	sum of the reported KAMs
atyp_KAM_num	difference between the number of reported KAMs and industry-year average number
atyp_KAM_top	sum of the reported KAMs covering 'atypical' for the industry-year topics <i>*a topic is considered to be atypical if it is found in less than 50% of the reports unless other is specified</i>
unpred_KAM_num	difference between the number of reported KAMs and company-specific number predicted based on Burke et al. (2022) model
KAM_REDUCT	reduction in the number of disclosed KAMs within a year (the variable can take negative values if the number of disclosed KAMs increases)
REV_sa	sum of reported KAMs covering revenue-related topics
INTANG_sa	sum of reported KAMs intangibles-related topics
GC_sa	sum of reported KAMs covering going concern-related topics
M&A_sa	sum of reported KAMs covering M&A-related topics
PPE_sa	sum of reported KAMs covering property, plant and equipment-related topics
INVENTORY_sa	sum of reported KAMs covering inventory-related topics
TAX_sa	sum of reported KAMs covering taxes-related topics
SIZE	natural logarithm of total assets
GROWTH	percentage change of total assets
INVREC	ratio of the sum of inventories and receivables to total assets
LOSS	an indicator variable that equals 1 if the company reports a net loss, and 0 otherwise
ROA	ratio of net income before extraordinary items to total assets
MTB	ratio of market value of equity to book value of equity
CASH	ratio of cash and cash equivalents to total assets
LEV	ratio of total debt to total assets
DACC	absolute value of discretionary accruals estimated using the modified Jones model
TEN	number of years the company is audited the current audit firm
FEES	natural logarithm of total fees paid to the auditor
AUD_FEES	natural logarithm of audit fees

SPEC	indicator variable that equals 1 if the company is audited by a firm which audits at least 25% of the client's industry's revenues
BIG 4	indicator variable that equals 1 if the company is audited by a Big4 audit firm, and
OPINION	indicator that equals 1 if the company receives anything but an unqualified audit opinion, and 0 otherwise
GC	indicator variable that equals 1 if the company receives a going concern opinion, and 0 otherwise
REST	indicator variable that equals 1 if the company announces a restatement in the fiscal year, and 0 otherwise
MA	indicator variable equal to 1 if the company is engaged in an acquisition reporting an increase in goodwill during the two most recent years
AGE	natural logarithm of the number of years for which total assets are reported in Compustat
SALES_GR	percentage change of lagged sales
RISK_IND	indicator variable that equals 1 if the issuer belongs to an industry with high litigation risk, and 0 otherwise <i>High litigation risk industries as defined by Francis et al. (1994) are: biotech (2833-36, 8731-34), computer (3570-77, 7370-74), electronics (3670-74), or retail (5200-5961)</i>
RESTRUCT	indicator variable that equals 1 if a company reports non-zero restructuring costs, and 0 otherwise

1.9 Appendix B

Table 1.12: Difference-in-Differences Robustness: Including POST Indicator

<i>dependent variable</i>	<i>Logit</i>		<i>OLS</i>	
	(1)	(2)	(3)	(4)
POST	-0.600*** (-2.66)	-0.530** (-2.19)	-0.058*** (-4.47)	-0.059*** (-4.46)
POST*ADOPT	1.139*** (2.91)	1.363*** (3.30)	0.033* (1.94)	0.040** (2.29)
SIZE	0.027 (0.12)	-0.186 (-0.73)	0.000 (0.01)	-0.008 (-0.50)
GROWTH	-0.059 (-0.44)	-0.047 (-0.30)	0.002 (0.27)	-0.001 (-0.08)
INVREC	0.133 (0.11)	0.131 (0.10)	0.025 (0.33)	0.027 (0.33)
LOSS	0.391 (1.45)	0.398 (1.40)	0.015 (1.01)	0.019 (1.21)
ROA	-0.314 (-0.96)	-0.308 (-0.77)	-0.046* (-1.75)	-0.040 (-1.35)
MTB	0.009 (0.39)	0.014 (0.57)	0.001 (0.90)	0.001 (0.91)
CASH	1.288 (1.64)	1.756* (1.96)	0.084* (1.78)	0.137** (2.47)
LEV	1.007 (1.05)	0.362 (0.33)	0.0791 (1.41)	0.0398 (0.67)
DACC	0.301 (0.32)	0.651 (0.64)	-0.025 (-0.41)	-0.010 (-0.16)
AUD_FEES	0.240 (1.19)	0.299 (1.45)	0.0126 (1.40)	0.013 (1.44)
TEN	0.808*** (12.08)	0.773*** (11.08)	0.030*** (13.74)	0.029*** (12.54)
OPINION	1.044*** (3.51)	1.260*** (4.01)	0.077*** (3.54)	0.087*** (3.80)
BIG4	-2.007*** (-5.49)	-2.006*** (-4.95)	-0.192*** (-4.90)	-0.195*** (-4.55)
MA		0.849* (1.82)		0.0454 (1.45)
Const.			-0.149 (-0.91)	-0.0992 (-0.55)
Obs	1 672	1 417	4 545	4 171
Pseudo R-sq. & R-sq.	0.398	0.404	0.123	0.126

Note: The Table reports robustness checks for the DiD analysis by estimating both logit and OLS specifications that additionally include the main-effect POST indicator. The dependent variable is *AUD_CH*, an indicator equal to one if a firm switches its auditor. Columns (1)–(3) present logit specifications, and Columns (4)–(6) report OLS estimates. All specifications include the full set of control variables. Columns (2) and (4) additionally include the M&A indicator. Firm fixed effects are included in all regressions. Standard errors are clustered at the firm level; *t*-statistics are reported in parentheses, and *, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 1.13: KAMs prediction model

	KAM_num	
SIZE	0.076***	(14.78)
AGE	0.034***	(3.34)
LOSS	0.143***	(6.98)
ROA	-0.045	(-1.42)
MTB	-0.005***	(-2.62)
LEV	0.158***	(3.22)
SALES_GR	0.007	(0.20)
REST	0.074***	(3.00)
AUD_CH	0.076***	(2.71)
BIG4	0.046**	(2.03)
TENURE	0.002	(1.51)
RISK_IND	0.045**	(1.98)
RESTRUCT	0.096***	(5.30)
Const	-0.148**	(-2.24)
Ind FE	Yes	
Obs	5029	
Log lik-d	-8697.7	

Note: The Table reports estimates from the prediction model of Burke et al. (2023), used to obtain the company-specific expected number of KAMs. The dependent variable is *KAM_num*. All variables are defined in Appendix A. All the variables with the exception of *SALES_GR* and *ROA* result statistically significant. The number of the reported KAMs is positively associated with a company's size, age and leverage and negatively associated with its market-to-book value. Also, companies that have negative net income, file a restatement, operate in a risky industry and are audited by Big 4 firms tend to receive more KAMs. Industry fixed effects are included. Standard errors are clustered at the firm level; *t*-statistics are reported in parentheses, and *, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 1.14: Number of Reported KAMs and Audit Switches. OLS

	KAM_num	atyp_KAM_num	atyp_KAM_num	atyp_KAM_top	unpr_KAM_num	unpr_KAM_num
KAM	0.027*** (3.94)	0.027*** (4.24)		0.022*** (3.35)	0.028*** (4.46)	
KAM_extra			0.027*** (3.10)			0.026*** (2.78)
KAM_miss			-0.027** (-2.44)			-0.032*** (-2.98)
SIZE	-0.047* (-1.85)	-0.046* (-1.81)	-0.046* (-1.82)	-0.046* (-1.81)	-0.036 (-1.42)	-0.036 (-1.41)
GROWTH	0.015 (1.52)	0.015 (1.55)	0.015 (1.55)	0.016 (1.55)	0.013 (1.20)	0.013 (1.20)
INVREC	0.242** (2.00)	0.252** (2.08)	0.251** (2.08)	0.237* (1.95)	0.231* (1.90)	0.231* (1.90)
LOSS	0.005 (0.27)	0.006 (0.34)	0.006 (0.33)	0.007 (0.37)	0.021 (1.11)	0.022 (1.12)
ROA	0.001 (0.05)	0.001 (0.02)	0.001 (0.02)	0.001 (0.04)	0.008 (0.27)	0.008 (0.25)
MTB	0.000 (0.04)	0.000 (0.00)	0.000 (0.00)	0.000 (0.08)	-0.000 (-0.04)	-0.000 (-0.05)
CASH	0.130* (1.74)	0.136* (1.82)	0.135* (1.82)	0.125* (1.68)	0.139* (1.79)	0.140* (1.80)
LEV	0.175** (2.46)	0.178** (2.49)	0.178** (2.50)	0.176** (2.46)	0.189** (2.53)	0.190** (2.54)
DACC	-0.070 (-1.46)	-0.068 (-1.42)	-0.068 (-1.42)	-0.070 (-1.46)	-0.061 (-1.28)	-0.061 (-1.27)
AUD_FEES	0.013 (0.43)	0.014 (0.47)	0.013 (0.46)	0.016 (0.56)	0.008 (0.26)	0.008 (0.27)
TENURE	0.028*** (20.73)	0.028*** (20.89)	0.028*** (20.89)	0.028*** (20.80)	0.028*** (20.73)	0.028*** (20.77)
SPEC	-0.031 (-1.36)	-0.032 (-1.39)	-0.032 (-1.39)	-0.031 (-1.33)	-0.031 (-1.37)	-0.032 (-1.38)
GC	0.048* (1.71)	0.050* (1.76)	0.050* (1.76)	0.054* (1.90)	0.048* (1.67)	0.048* (1.68)
REST	-0.023 (-0.78)	-0.022 (-0.75)	-0.022 (-0.75)	-0.021 (-0.72)	-0.014 (-0.48)	-0.014 (-0.47)
BIG4	0.164*** (3.49)	0.165*** (3.49)	0.165*** (3.49)	0.162*** (3.43)	0.169*** (3.58)	0.169*** (3.59)
Const	-0.125 (-0.37)	-0.045 (-0.13)	-0.045 (-0.13)	-0.155 (-0.46)	-0.120 (-0.35)	-0.124 (-0.36)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs	3833	3833	3833	3833	3787	3787
R-sq.	0.180	0.180	0.180	0.178	0.181	0.181

Note: The Table presents the complete OLS estimates for the model relating the number of reported KAMs to the probability of an auditor switch (Table 1.5, Panel B in the main text reports only the coefficients of primary interest). The dependent variable is *AUD_CH*, an indicator equal to one if the firm changes its auditor. All control variables are defined in Appendix A. Continuous variables are winsorized at the 1st and 99th percentiles. Standard errors are clustered at the firm level. *t*-statistics are reported in parentheses; *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 1.15: Multinomial Logit Regression Results. Non-Big 4 Sample

AUD.CH.TIER	KAM_num		atyp_KAM_num		atyp_KAM_top		unpr_KAM_num	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
KAM	0.046 (0.58)	0.270 (1.27)	0.059 (0.74)	0.293 (1.25)	-0.023 (-0.34)	0.312 (1.00)	0.061 (1.06)	0.274 (1.15)
SIZE	-0.167 (-1.25)	0.213 (0.69)	-0.168 (-1.36)	0.215 (0.64)	-0.162 (-1.29)	0.193 (0.43)	-0.143 (-1.44)	0.271 (0.93)
GROWTH	-0.046 (-0.29)	0.125 (0.32)	-0.046 (-0.35)	0.117 (0.15)	-0.042 (-0.19)	0.125 (0.15)	-0.036 (-0.28)	0.120 (0.23)
INVREC	0.438 (0.84)	-0.947 (-0.41)	0.447 (0.84)	-0.987 (-0.45)	0.413 (0.64)	-0.970 (-0.30)	0.444 (0.85)	-0.974 (-0.52)
LOSS	-0.285 (-1.14)	-0.301 (-0.35)	-0.287 (-1.25)	-0.318 (-0.41)	-0.275 (-0.99)	-0.323 (-0.32)	-0.301 (-1.22)	-0.163 (-0.24)
ROA	0.093 (0.43)	0.811 (0.28)	0.094 (0.37)	0.786 (0.30)	0.082 (0.39)	0.836 (0.18)	0.119 (0.55)	0.808 (0.36)
MTB	-0.002 (-0.07)	0.024 (0.31)	-0.002 (-0.07)	0.024 (0.29)	-0.001 (-0.06)	0.024 (0.28)	-0.002 (-0.11)	0.019 (0.24)
CASH	-0.432 (-0.65)	2.949 (1.03)	-0.422 (-0.75)	2.955* (1.65)	-0.481 (-0.72)	2.965 (1.05)	-0.283 (-0.49)	2.927** (1.97)
LEV	1.240** (2.56)	1.891 (0.56)	1.232** (1.96)	1.847 (0.66)	1.267** (2.14)	1.809 (0.52)	1.212* (1.85)	1.977 (0.65)
DACC	0.133 (0.26)	-0.698 (-0.23)	0.137 (0.23)	-0.680 (-0.37)	0.129 (0.26)	-0.664 (-0.28)	0.208 (0.36)	-0.712 (-0.36)
AUD.FEES	-0.010 (-0.05)	0.183 (0.51)	-0.014 (-0.07)	0.170 (0.38)	0.009 (0.04)	0.174 (0.37)	-0.049 (-0.25)	0.188 (0.50)
TENURE	0.053*** (3.62)	0.086 (1.45)	0.052*** (3.13)	0.085** (2.31)	0.054*** (3.09)	0.082* (1.66)	0.049*** (3.28)	0.085* (1.72)
SPEC	-14.57*** (-18.29)	-15.02*** (-11.87)	-14.56*** (-16.15)	-15.01*** (-13.99)	-14.56*** (-18.18)	-15.02*** (-8.95)	-14.58*** (-15.79)	-14.98*** (-11.63)
GC	-0.252 (-0.82)	-0.059 (-0.01)	-0.251 (-0.85)	-0.089 (-0.03)	-0.244 (-0.78)	-0.076 (-0.02)	-0.341 (-1.64)	-0.032 (-0.01)
REST	0.030 (0.08)	0.493 (0.16)	0.025 (0.07)	0.495 (0.10)	0.051 (0.16)	0.520 (0.10)	0.092 (0.24)	0.557 (0.10)
MISMATCH	-0.653* (-1.83)	1.103 (1.22)	-0.654 (-1.53)	1.105 (1.36)	-0.662* (-1.68)	1.140 (1.05)	-0.639** (-2.12)	1.124 (1.25)
Const	-15.48*** (-7.18)	-28.02*** (-3.41)	-15.26*** (-7.66)	-26.86*** (-3.79)	-15.52*** (-9.08)	-27.87*** (-3.57)	-15.15*** (-6.35)	-28.22*** (-3.23)
Ind FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs	1353	1353	1353	1353	1353	1353	1335	1335
R-sq.	0.094	0.094	0.095	0.095	0.094	0.094	0.096	0.096

Note: The Table reports multinomial logit estimates examining the association between the number of reported KAMs and the type of subsequent auditor switch for the sample restricted to the current non-Big 4 clients. The dependent variable, *AUD.CH.TIER*, equals 0 when a company does not change its auditor, 1 when the switch is lateral, and 2 when the switch is upward. Each pair of columns shows coefficients from the multinomial model comparing (1) lateral switches vs. no switch and (2) upward switches vs. no switch. All specifications include the full set of control variables as well as industry and year fixed effects. All variables are defined in Appendix A. *t*-statistics are reported in parentheses, and *, **, *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 1.16: Auditor Changes and KAM Subject Areas

<i>typical topics threshold</i>	AUD_CH					
	ALL KAMS		KAMs covering atypical topics			
		45%	50%	55%	60%	65%
REV_sa	0.009 (0.51)	0.010 (0.57)	0.008 (0.44)	-0.000 (-0.02)	-0.009 (-0.52)	-0.004 (-0.21)
GOING_CONC_sa	0.043** (2.21)	0.034 (1.52)	0.031 (1.50)	0.046** (2.40)	0.047** (2.40)	0.047** (2.40)
INTANGIBLES_sa	0.022 (1.34)	0.026 (1.63)	0.024 (1.49)	0.021 (1.29)	0.021 (1.28)	0.021 (1.30)
MA_sa	0.003 (0.23)	0.004 (0.25)	0.003 (0.22)	0.003 (0.20)	0.003 (0.19)	0.003 (0.21)
PPE_sa	0.000 (0.00)	0.001 (0.03)	-0.000 (-0.00)	-0.000 (-0.02)	-0.001 (-0.04)	-0.000 (-0.01)
TAX_sa	0.030 (1.18)	0.031 (1.21)	0.030 (1.18)	0.030 (1.15)	0.030 (1.16)	0.030 (1.16)
INVENTORY_sa	0.004 (0.13)	-0.013 (-0.44)	-0.032 (-1.26)	-0.032 (-1.25)	-0.005 (-0.22)	0.003 (0.11)
SIZE	-0.040 (-1.56)	-0.043* (-1.68)	-0.042* (-1.67)	-0.039 (-1.55)	-0.039 (-1.52)	-0.039 (-1.54)
GROWTH	0.017* (1.69)	0.017* (1.67)	0.017* (1.68)	0.018* (1.72)	0.017* (1.71)	0.018* (1.72)
INVREC	0.249** (2.03)	0.244** (1.99)	0.250** (2.04)	0.250** (2.04)	0.244** (1.99)	0.243** (1.98)
LOSS	0.009 (0.47)	0.010 (0.52)	0.011 (0.57)	0.010 (0.52)	0.010 (0.53)	0.010 (0.51)
ROA	-0.002 (-0.05)	-0.001 (-0.02)	0.000 (0.02)	-0.000 (-0.01)	-0.000 (-0.01)	-0.000 (-0.02)
MTB	0.000 (0.09)	0.000 (0.07)	0.000 (0.02)	0.000 (0.07)	0.000 (0.12)	0.000 (0.11)
CASH	0.131* (1.73)	0.128* (1.72)	0.132* (1.76)	0.135* (1.79)	0.132* (1.74)	0.130* (1.72)
LEV	0.168** (2.33)	0.177** (2.47)	0.181** (2.52)	0.178** (2.50)	0.175** (2.45)	0.175** (2.44)
DACC	-0.065 (-1.35)	-0.068 (-1.41)	-0.068 (-1.41)	-0.065 (-1.34)	-0.064 (-1.32)	-0.064 (-1.31)
AUD_FEES	0.023 (0.80)	0.026 (0.87)	0.027 (0.92)	0.026 (0.89)	0.025 (0.86)	0.025 (0.85)
TENURE	0.028*** (20.51)	0.028*** (20.64)	0.028*** (20.64)	0.028*** (20.61)	0.028*** (20.50)	0.028*** (20.45)
SPEC	-0.027 (-1.19)	-0.029 (-1.25)	-0.029 (-1.26)	-0.029 (-1.28)	-0.029 (-1.25)	-0.028 (-1.22)
GC	0.047 (1.61)	0.057** (1.99)	0.055* (1.90)	0.049* (1.68)	0.049* (1.67)	0.049* (1.67)
REST	-0.017 (-0.56)	-0.018 (-0.60)	-0.017 (-0.57)	-0.015 (-0.51)	-0.016 (-0.52)	-0.016 (-0.53)
BIG4	0.162*** (3.39)	0.160*** (3.36)	0.160*** (3.37)	0.159*** (3.34)	0.160*** (3.34)	0.160*** (3.34)
Const	-0.286 (-0.84)	-0.269 (-0.79)	-0.292 (-0.86)	-0.310 (-0.91)	-0.304 (-0.89)	-0.298 (-0.87)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs	3 833	3 833	3 833	3 833	3 833	3 833

Note: The Table reports OLS regression estimates examining the association between KAMs covering specific subject areas and the probability of an auditor change. The dependent variable is AUD CH, an indicator equal to one if the firm changes its auditor. Column (1) uses all reported KAMs to construct subject-area variables, while Columns (2)–(6) restrict the analysis to the KAMs that are atypical within the corresponding industry–year. All models include the full set of control variables and both firm and year fixed effects. All variables are defined in Appendix A. Standard errors are clustered at the firm level. *t*-statistics are reported in parentheses; *, **, and *** denote significance at the 10%, 5%, and 1% levels.

Chapter 2

Litigation Risk: Audit Reporting, Pricing, and Quality Consequences

2.1 Introduction

The legal liability of auditors and its influence on the auditing process remains a topic of ongoing debate. The United States is frequently cited as imposing some of the highest litigation risks on auditors (Talley, 2006; Donelson, 2013). Nevertheless, this exposure has not prevented auditors from playing a central role in facilitating—or at least failing to prevent—corporate fraud. Recent cases underscore the continued relevance of this concern: BF Borgers¹ was charged by the SEC for “deliberate and systemic failures” across more than 1,500 filings and paid a \$12 million penalty, and Prager Metis² settled charges related to its role in the FTX scandal, paying \$1.95 million. Similarly, a widely known Big 4 auditor, PwC, paid a \$335 million settlement in 2019, stemming from alleged gross negligence in auditing Colonial Bank—whose collapse was tied to fraud at Taylor, Bean & Whitaker—which further illustrates the substantial financial and reputational risks auditors face.³ These and similar cases lend support to longstanding critiques of the

¹<https://www.sec.gov/newsroom/press-releases/2024-51>

²<https://www.sec.gov/newsroom/press-releases/2024-133>

³<https://www.fdic.gov/news/press-releases/2019/pr19019.html>

legal framework, which argue that existing laws still afford auditors excessive protection, thereby undermining the quality and integrity of audit practices (Hamdani, 2003; Coffee, 2004).

Despite this anecdotal evidence, it remains largely unclear from an empirical perspective whether the legal system—particularly that of the United States—has real leverage to influence auditors’ behavior and compel them to fulfill their role as gatekeepers in financial reporting. In this paper, we seek to unveil the answer by exploiting differences in auditors’ exposure to litigation risk across U.S. states. Specifically, we examine whether auditors adjust their pricing and reporting decisions in response to the level of litigation risk associated with client engagements, and whether these adjustments translate into changes in audit quality.

Focusing on the U.S. setting offers not only a valuable empirical context — where time-varying changes in litigation risk enable a difference-in-differences design — but also allows us to concentrate on the market that hosts the largest share of publicly traded companies. Importantly, these companies—and their auditors—operate in one of the most litigation-intensive environments globally, a feature that is central to our research question. Studying this relationship in countries with weak shareholder protection mechanisms would offer limited practical relevance, as legal enforcement in such contexts is unlikely to exert comparable influence on auditor behavior.

In this paper, we hypothesize that if the current legal system—at least to some extent—succeeds in providing auditors with the right incentives, rather than shielding them entirely from tangible risk, then auditors will be responsive to changes in circuit-level litigation risk and will adopt strategies to adjust their practices accordingly. Our analysis focuses on three possible adjustment strategies. First, auditors may charge a risk premium to compensate for the increased effort and potential losses that could arise if a misstatement in the audited financial statements is later identified by third parties. Second, auditors might adjust the level of conservatism in their reporting — particularly in issuing modified and going concern opinions — to reflect the heightened litigation

risk. Third, auditors may increase the overall quality of the audit process to enhance the likelihood of detecting and addressing material errors in clients' financial statements.

To answer our research questions, we exploit the heterogeneity in litigation risk faced by U.S. companies arising from the structure of the U.S. federal judicial system, which is organized into 12 regional circuits and overseen by the Supreme Court. Although all circuits are bound by the same federal laws, their interpretation of those laws often varies. These inconsistencies—known as circuit splits—persist until the Supreme Court hears a relevant case and issues a ruling that resolves the disagreement and establishes a uniform interpretation to be followed across all circuits. This study leverages two such historical circuit splits, each of which created differential litigation risk for auditors by affecting the probability of being named as a defendant in lawsuits filed against their clients.

The first circuit split arose from differences in pleading requirements across various circuits. Under the U.S. federal law, the plaintiff must satisfy the "scienter" requirement in order to name a certain party as a defendant. This requirement entails alleging that the defendant knew, or should have known, about the unlawfulness of their actions. However, the law remained silent on the level of evidence that the plaintiff had to present to effectively convince the court of the defendant's proper scienter. As a result, it was left to the discretion of the individual circuits to determine this criteria themselves. In the "pro-plaintiff" courts, it was relatively easy for shareholders to sue not only the company which issued misleading financial statements, but also the other parties involved in fraud preparation. Thus, in order to name the auditors among defendants it was enough to demonstrate that they had motive and opportunity to deceive. Conversely, in "pro-defendants" courts, shareholders were required to provide evidence of auditors' intentional recklessness and conscious misconduct to pursue legal action against them. The split was resolved in June 2007 when the Supreme Court ruled on *Tellabs, Inc. v. Makor Issues & Rights, Ltd* (Tellabs) case and legitimized middle ground approach stating that the pleading criteria is satisfied when a plaintiff manages to provide facts demonstrating that auditor had the required state of mind to commit a fraud. After this decision circuits had to revise their approaches accordingly: the previously pro-plaintiff ones had

to restrict their pleading requirements which lead to decrease of auditor's litigation risk, whereas the pro-defendant ones had to loose the rules thereby increasing the likelihood of auditors facing lawsuits.

The other circuit split analysed in this paper stemmed from the differences in defining the circle of primary violators. According to the federal law, defendants can be held liable for committing a fraud only if they are recognised as primary violators, meaning that they have made a false or misleading statement. However, the regulator have not specified what is meant by making a statement. The majority of the circuits used a so-called bright-light test approach which requires a violator to personally make a false or deceptive statement in order to be recognized as primary actor and be held accountable. Few pro-plaintiff courts, instead, applied substantial-participation test to determine whether a violator could be considered as a primary actor. In this case, the significant involvement of auditors in preparation of the clients' fraudulent reports could be considered a form of making a statement. This made it considerably easier for shareholders to name an auditor among other defendants when initiating legal action against the client company. The resolution of the circuit split regarding liability standards came with the Supreme Court's decision in *Janus Capital Group, Inc. v. First Derivative Traders* (Janus), which occurred in June 2011. According to the ruling, an individual or entity can be considered a primary violator only if they hold the "ultimate authority" over a fraudulent statement. As a result, courts that had previously applied the substantial-participation test were compelled to adopt the narrower Janus standard, recognizing auditor liability only in cases where the auditor knowingly signed a false audit report. This shift significantly reduced auditors' exposure to litigation risk in those circuits.

Both splits were resolved by the Supreme Court's decision which caused changes in auditors' litigation risk in some of the circuits which had to adopt their approaches accordingly. This setting makes it possible to address endogeneity concern by using DiD research design. Firstly, we explore whether there are any significant changes in audit pricing or GC reporting conservatism evoked by the circuit-level litigation risk shifts and then switch to the analysis of possible audit quality adjustments. For the latter we concentrate on two

alternative measures. The first one is restatements which have several strong advantages given the research question of this paper. Thus, material misstatements fall under direct influence of auditors (if compared to some other audit quality such as earnings-per-share coefficients or cost of capital, for instance) and can signal relatively severe audit failures (DeFond and Zhang, 2014). If auditors do increase their efforts when facing heightened litigation risk, we would expect these additional efforts to be especially aimed at preventing of future restatements. While other minor evidence of diminished audit quality may fail to garner shareholders' attention or be deemed sufficient proof of auditors' misconduct in court, restatements are known to substantially increase the probability of formal allegations against auditors (Rapojal et al., 2021). The other measure of audit quality which we use in this paper is accuracy of going concern opinions which helps to overcome the major drawback of the first proxy. Being relatively straightforward measure of financial reporting quality in general, material restatements do not allow to separate the quality of conducted audit from that of the unaudited financial statements initially prepared by clients. GC opinions instead are entirely under auditors' control allowing to track changes in audit quality that result solely from auditors' choices.

Our study aims to enrich the literature on the strategies used by auditors to accommodate for different levels of litigation risk they face. Existing papers exploit very different settings to investigate different aspects of this question. For instance, Seetharaman et al. (2002) examine audit fees paid by the cross-listed U.K. firms with and without access to U.S. capital markets and they find that the former (i.e., the ones exposed to higher litigation risk) are more likely to pay higher audit fees. Venkataraman et al. (2008) claim that companies undergoing the IPO process are inherently more susceptible to litigation risk and show that they pay higher audit fees and report lower accruals as compared to the very same companies in subsequent post-IPO years. Badertscher et al. (2014) analyze public and private companies that have publicly traded debt and demonstrate that the former, on average, pay higher audit fees. The authors attribute their findings to the fact that public companies are exposed to higher litigation risk. Some other papers analyse GC reporting strategies. Carcello and Palmrose (1994), for example, focus on a sample

of companies filing for bankruptcy and find that auditors of such companies do use GC opinion as a preemptive strategy against heightened litigation risk. Kaplan and Williams (2013) concentrate on a sample of financially distressed U.S. companies that are involved in securities class actions and document positive association between ex ante litigation risk of these companies and auditors' propensity to issue a modified GC report.

The majority of these studies, however, concentrate on restricted samples and compare arguably heterogeneous groups of companies. In this paper we try to overcome these issues by analyzing a wide sample of U.S. public companies not limiting our analysis to those companies which undergo specific phase of their life-cycle such as IPO, bankruptcy, involvement in a lawsuit, etc. To the best of our knowledge, our study is the first one to explore whether auditors apply the risk mitigation strategies discussed above when it comes to an "average" client. Another advantage of our study is that we are able to concentrate on the U.S. companies only. Our findings enrich the understanding of whether and how the legal system of a particular country can affect auditors' work - limiting the analysis to the U.S. headquartered companies allows to rule out many alternative explanations of the found results.

At the same time, this study is subject to several limitations. While we base our analysis on what are presumably exogenous shocks to litigation risk resulting from Supreme Court rulings—thereby strengthening our identification strategy and supporting causal interpretation—we cannot entirely rule out the influence of other circuit-level factors. For such factors to plausibly explain our results, however, they would need to coincide precisely with the timing of the rulings and affect the exact same circuits, which makes this possibility less likely. More importantly, due to data limitations, we are unable to directly test some alternative strategies auditors may use to respond to changes in litigation risk. For instance, it remains unclear whether U.S. auditors increased (or decreased) their effort in response to upward (or downward) shifts in circuit-level litigation risk. However, given that audit quality is a complex concept, it is entirely possible that some auditor responses to changes in litigation risk may remain undetected in our analysis.

The results of the analysis provide nuanced evidence of auditors' strategies to adjust to changes in litigation risk induced by Supreme Court rulings. The strongest results support the price-adjusting strategies: following the announcement of the Tellabs decision, auditors appear to have increased the fees charged to clients headquartered in circuits that experienced an increase in litigation risk. For the Janus case, there is strong evidence suggesting that auditors reduced audit fees for clients headquartered in the Fourth and Ninth Circuits, where the ruling lowered litigation risk. However, when examining the effects on auditors' reporting conservatism and audit quality, the findings are less definitive, with no strong or consistent patterns emerging. There is some evidence that auditors became more conservative in issuing modified opinions (MAO) to clients headquartered in circuits where the Tellabs ruling increased litigation risk. At the same time, auditors do not appear to have become less conservative toward clients in circuits where litigation risk declined—neither in response to Tellabs nor Janus rulings. Additionally, the analysis of going concern (GC) opinion issuance provides no further support for the idea that auditors become more (less) conservative when facing higher (lower) litigation risk. The results of the restatement rate analysis—the main proxy for audit quality in our setting—are also mixed. At first glance, there is some (moderate, both statistically and economically) evidence of a decrease in restatement rates among clients headquartered in circuits where the Tellabs decision increased auditors' litigation risk. However, somewhat surprisingly, this decrease appears to be driven by clients of non-Big 4 auditors, who, according to the literature, are generally expected to respond less strongly to shifts in litigation risk. Moreover, contrary to the idea that auditors are willing to sacrifice audit quality when their litigation risk decreases, a decline in restatement rates is also observed in some of the circuits where the Tellabs ruling reduced auditors' exposure to litigation risk. Similar results are found for Big 4 engagements in circuits where the Janus decision lowered litigation risk. Additional audit quality indicators — specifically, GC error rates and MAO aggressiveness — show occasional shifts that align with changes in litigation risk. However, these findings are not consistent across specifications.

The remainder of the paper is organized as follows. Section 2 provides an overview of

the legal framework in the United States as it relates to our research question. Drawing on existing literature, we develop hypotheses concerning how changes in litigation risk may influence audit outcomes. Section 3 outlines the research design, including data, variables, and methodology. Section 4 presents the empirical results. Section 5 offers sensitivity analyses to assess the robustness of our findings, and Section 6 concludes the paper.

2.2 Literature Review and Hypotheses Development

This paper investigates whether U.S. auditors—who are known to operate in one of the most rigorous legal environments—react to changes in litigation risk by adjusting their audit processes and outcomes. DeFond and Zhang (2014) define litigation risk as “the risk that an auditor will be sued by a client or a third party for damages resulting from an audit failure.” Anecdotal evidence suggests that auditors regularly confront such risks and, in some cases, incur substantial payments to resolve securities class-action claims. Although exceptionally large penalties are relatively rare, they nonetheless occur. For example, in 2019 PwC paid \$335 million to resolve allegations of gross negligence in its audit of Colonial Bank, and BF Borgers — a non-Big 4 firm with considerably smaller audit revenues — was fined \$12 million for “deliberate and systemic failures” across more than 1 500 filings. Perhaps the most dramatic and consequential illustration of realized litigation and reputational exposure is the collapse of Arthur Andersen following its involvement in the Enron scandal. The firm not only paid a \$72.5 million settlement to Enron shareholders, but also ultimately ceased to exist after suffering irreparable reputational harm. Smaller penalties, while less severe, are far more common. Among the many examples are Deloitte & Touche, which paid \$1 million in 2017 for failing to properly audit the financial statements of Jack Henry & Associates, and Grant Thornton, which agreed to a \$1.5 million penalty and censure in 2022 for deficiencies in its audit of a public company’s goodwill impairment analysis.⁴ While individually modest, such

⁴<https://news.bloombergtax.com/financial-accounting/auditors-paid-record-penalties-for-us-violations-in-2022>

penalties remain an important source of exposure: auditors cannot anticipate whether any particular action will escalate into a major case. Moreover, smaller sanctions—when recurring—can impose meaningful cumulative financial and reputational costs. Consequently, auditors have strong incentives to employ strategies that reduce litigation risk. Francis and Krishnan (1999) list five of such strategies: screening out high-risk clients; increasing audit effort to decrease the chances of error detection; charging additional premium to high-risk clients; negotiating adjustments to the financial statements; and issuing modified reports. Researchers have been studying different environments and employing various methodological approaches in attempts to understand whether and under which circumstances auditors employ these approaches in practice.

In this paper we leverage on plausibly exogenous shocks to auditors' litigation risk in some of the U.S. circuits to enrich this stand of research and test whether auditors respond to this circuit-level shifts by exploiting some of the above-mentioned strategies. The structure of the U.S. federal law system which is discussed in details below allows for variation in litigation risk across the different states and changes in this risk over the time. We explore whether two of such litigation risk shocks, shown to alter auditors' litigation risk in some of the states, also affected the auditors' behavior in line with the predictions based on the findings of existing literature. In particular, we investigate whether changes in pleading requirements and tightening of the circle of parties that can be pledged guilty for financial misreporting pushed auditors to adjust their risk-mitigation strategies. While data limitations prevent us from studying whether auditors screen out higher-risk clients, we examine the other strategies — risk-premium pricing, changes in reporting conservatism, shifts in effort, and negotiation over adjustments — though acknowledging that these channels are not always empirically distinguishable.

First, we test whether auditors in our sample reacted to the litigation risks changes by adjusting pricing of their services as it can be predicted based on the existing literature discussed later in this section. Finding evidence of audit fees shifts will speak in favor of effort and/or risk premium adjustment strategies. Then, we develop and test hypotheses on how litigation risk shifts affect auditors' reporting conservatism — an-

other well-documented strategy — and audit quality, which would be expected to change if auditors adopt the effort-adjusting strategy (and, for certain proxies, the strategy of imposing different adjustment requirements) in response to changes in their litigation exposure.

Data availability and our empirical design do not always permit us to disentangle individual strategies. Reporting conservatism—proxied by the issuance of modified or qualified opinions—is the only strategy we can examine directly. The other outcomes we study may reflect the joint influence of several strategies. In particular, fee responses do not allow us to distinguish between higher risk-premium charges and changes in audit effort. Because no publicly available data exist on engagement hours or audit-team composition, effort is unobservable, making it impossible to test the effort-adjusting strategy directly. Given this limitation, the best we can do is examine whether the same litigation-risk changes are accompanied by shifts in audit quality.⁵ Prior work suggests that if auditors respond to heightened litigation exposure by increasing effort, the additional work is conducted to improve audit quality and reduce the likelihood of future error detection (Simunic, 1990; Francis and Krishnan, 1999; Choi et al., 2008). We therefore hypothesize that litigation-risk shocks in some U.S. circuits led to audit-quality changes of the same sign. Evidence consistent with this prediction would support the use of the effort-adjusting strategy, whereas fee changes without corresponding quality movements would indicate that risk-premium pricing is the more likely channel.

In the following subsections we discuss in details the structure of the U.S. federal law system and circuit-level shocks which affected auditors' litigation risks; review existing literature on the mechanisms which auditors tend to use when accommodating to litigation risk and, based on that, develop the hypotheses regarding the links between regional-level litigation risk changes in the U.S. and the various audit outcomes mentioned above.

⁵Some of the audit-quality proxies we use may also capture auditors' negotiation over proposed adjustments, providing an additional channel through which litigation risk could influence outcomes.

2.2.1 Litigation Risk Changes in the U.S.

In the U.S. auditors of public companies are liable under both federal security laws and common laws. Federal level is an important source of litigation risk as it is one of the main channels through which shareholders of a company can sue auditors in the case of detected fraud. The two most common claims used by shareholders against auditors are Rule 10b-5 and 11. The first one has a broader reach as it can be applicable whenever a company releases fraudulent periodic reports, whereas the second extends only to the misstatements found in the documents filed before securities are sold to the public. In this paper, following Honigsberg et al. (2020), we focus on the auditors' liability under Rule 10b-5 and explore the shifts in litigation risk which took place in some of the U.S. states.

The U.S. federal judicial system is led by the Supreme Court, with courts of appeals and district-level trial courts beneath it. Geographically, the federal districts are grouped into 12 regional circuits. Eleven of these circuits are numbered First through Eleventh and cover distinct regions of the United States, handling appeals from the district courts within their boundaries. The District of Columbia Circuit has jurisdiction exclusively over Washington, D.C. All the courts are expected to follow the same federal laws which, however, can vary in their level of detail and specificity. In the cases when these laws are general, the circuit courts are free to apply their own approaches and interpretations. Such legal system structure results in the divergence of federal law application among different circuits, a situation known as a circuit split. These splits are typically resolved by a ruling from the Supreme Court on a particular randomly chosen case. Once the Supreme Court addresses a case related to a specific split, all circuits that previously followed a different approach are obliged to adjust and align with the Court's decision.

Honigsberg et al. (2020) discuss circuit splits which were directly related to the auditors' liabilities to shareholders. These splits were due to different interpretations of pleading procedures and secondary actors' liability and were resolved by Supreme Court in 2007 and 2011 respectively. In the case of the first split circuits used different approaches to

define pleading scienter which is one of the six necessary elements to be proved by the plaintiff in order to initiate a lawsuit under Rule 10b-5. Before Supreme Court's rulings in *Tellabs, Inc. v. Makor Issues & Rights, Ltd.* (*Tellabs*), the Second and Third circuits were the most favorable to shareholders for whom, in order to meet pleading scienter criteria, it was enough to demonstrate auditor's motive and opportunity to deceive, manipulate, or defraud. In the First, Fourth, Fifth, Sixth, Seventh, Eighth, and Tenth circuits shareholders had to provide facts proving the corresponding state of mind of the auditors. Therefore, while these courts may have taken the auditor's motive and opportunity to deceive into account when assessing a lawsuit, in many instances, these facts alone was deemed insufficient to initiate legal actions against auditors. Lastly, Ninth and Eleventh circuits were the most favorable to the auditors as they required shareholders to prove deliberate recklessness and misconduct of the former in order to name them among other defendants. In its decision regarding *Tellabs* case Supreme Court stated that a party can be pleaded only when the inference of guilt is at least as strong as the opposing inference. Thus, after 2007, some of the circuits (Second and Third) had to become stricter to the plaintiffs which lead to decrease of litigation risk for auditors, whereas others (Ninth and Eleventh) had to implement more shareholders-favorable approach and increase auditors' liability exposure.

Supreme Court ruling in *Janus Capital Group, Inc. v. First Derivative Traders* (*Janus*) in 2011 resolved circuits split regarding liability of secondary actors. Since *Central Bank v. First Interstate* Supreme Court's decision of 1994, under the U.S. federal law, a secondary actor - party which assisted with respect to a fraud - cannot be held guilty. Therefore, the only way for shareholders to make auditors liable for any kind of mistake found in a company's financial statement is to show the direct involvement of the latter in the fraud. Till *Janus* case resolution in 2011, however, the circuits followed different approaches in defining primary participation. Thus, the Second, Fifth, Eighth, Tenth, and Eleventh Circuits agreed to recognize actors' guilt only in the cases when these actors themselves made some false statements (i.e., released misleading audit reports). The Fourth and Ninth circuits were more favorable to shareholders regarding the primary violation recog-

nition rules. In these courts, parties could be held liable as primary actors if there was substantial evidence of their significant participation in preparation of fraudulent financial statements. The Supreme Court's verdict regarding Janus legitimized a more stringent approach in recognizing secondary actors' liability as it stated that a party can be liable under Rule 10b-5 as primary violator only if it has ultimate authority over a fraudulent statement. Therefore, after Supreme Court's ruling of 2011, in the Fourth and Ninth circuits it became more challenging for investors to sue auditors as a direct actors of a fraud which brought the auditors' litigation risk down.

Some recent papers investigate whether the Supreme Court's decisions discussed above did really entail some changes in terms of auditors' litigation risk or just stayed on paper without being followed by any of the circuits. In particular, researchers examine changes in the likelihood of case dismissals and settlement amounts paid by the auditors for the cases that were not dismissed. Choi & Pritchard (2012) track all the securities class actions filed in 2003-2007 and find that previously pro-defendant circuits became less likely to dismiss shareholders' claims after the Tellabs decision (i.e., it became easier to sue a company and its auditor in these circuits). The authors also observe an increase in the absolute amount of the low value settlements in these courts which they explain by the defendants' readiness to settle even 'weak' cases in advance in order to avoid further litigation costs. Honigsberg et al. (2020) instead exclusively concentrate on the lawsuits which were initiated following severe restatements and had auditors named among defendants. When analyzing the changes brought in by Tellabs decision the authors find limited evidence of its effects on the litigation outcomes. Specifically, they observe a decrease in settlement amounts paid by auditors in circuits that were previously considered pro-plaintiff where the litigation risk for auditors was expected to decrease following the Supreme Court's decision. However, the authors do not find the anticipated increase in auditors' settlement values in circuits that were previously considered pro-defendant. The analyses of securities class actions' outcomes in the Fourth and Ninth circuits where the auditors' liability was expected to go down following Janus decision show more consistent results. In line with their hypothesis, Honigsberg et al. (2020) provide evidence of an

increased likelihood of lawsuit dismissal and higher settlement amounts paid by auditors in cases where the plaintiffs' complaints were not dismissed.

It is important to note that there is no clear consensus on the classification of circuits' positions regarding strictness of the scienter requirements before the Tellabs case ruling. The approach used by Honigsberg et al.'s (2020) is grounded on the differences in evidence base which was required by the circuits to demonstrate auditors guilt and discussed in details above. Choi & Pritchard (2012), however, stick to another classification which takes as a starting point differences in the pre-Tellabs circuits approaches to assessment of competing inferences when recognizing potential defendants' scienter. Thus, in the most pro-auditor circuits (the First, Fourth, Sixth, and Ninth, in order to sue auditors alongside the company's management, shareholders were required to establish that the presumption of auditors' scienter outweighed presumption of its absence. The middle-ground approach employed by the Second, Eighth, Tenth, and Eleventh Circuit and ultimately chosen by the Supreme Court demanded that the two presumptions were at least equally possible. Finally, the pro-shareholders circuits (the Third and Seventh) required no assessment of the competing inferences at all. According to this classification, it were the First, Fourth, Sixth, and Ninth circuits which, in order to align their practices with the Supreme Court ruling, had to become less demanding in what concerned defendants' scienter which entailed increase in auditors' litigation risk. The circuits which instead became less risky from the auditors' litigation standpoint were the Third and Seventh. In our analysis we look at both the classifications separately and then also focus our attention on the interaction of the classifications. In the latter case, we assume that in the circuits which are recognized by both classification as affected by the Supreme Court's ruling, auditors could have experienced more pronounced upwards and downwards litigation risk shocks. Table 2.1 summarizes the effects of the Supreme Court's ruling on the litigation risk of the auditors under different classifications

Table 2.1: Auditors' Litigation Risk Changes by Ruling, Classification, and Circuit

Ruling	Classification (Basis)	Affected Circuits	Litigation Risk Effect
Tellabs	Honigsberg et al. (evidence rule)	9 & 11	Increased

Ruling	Classification (Basis)	Affected Circuits	Litigation Risk Effect
		2 & 3	Decreased
Tellabs	Choi & Pritchard (inference test)	1, 4, 6, 9	Increased
		3 & 7	Decreased
Tellabs	Combined (both matched)	9	Increased
		3	Decreased
Janus	Honigsberg et al.	4 & 9	Decreased

As the literature shows some empirical evidence of auditors' litigation risk shifts followed by the Supreme Courts' rulings of 2007 and 2011, in this paper we suggest to take a step further and to analyse whether auditors employed certain strategies to accommodate to the new levels of litigation risk. The hypotheses regarding possible auditors' reactions to the circuit-level litigation risk changes and the literature that backs them up are presented below.

2.2.2 Litigation Risk and Audit Fees

In his work of 1980 Simunic was the first to show that audit effort and, consequently, audit fees may depend on the rigidity of the legal system. The author claimed that under different circumstances both auditors and their clients may want to proactively secure themselves against potential legal disputes by ensuring the high quality of the audited financial statements. His model shows that increase in overall legal liability exposure (i.e., expected losses) results in clients' voluntary desire to increase the audit quality and readiness to pay additional fees for the extra effort. On the other hand, increase in the auditors' legal liability exposure (i.e., the expected share of losses which is to be covered by the auditors) leads to situations when the auditors themselves insist on higher fees from their clients to compensate for the additional audit work and refuse to perform the audit otherwise.

Later studies explore the possibility of charging higher audit fees due to increased litigation risk without consequent enhancement of audit effort. Simunic & Stein (1996) were one of the first to discuss this phenomena known as risk premium. The authors analyze

audit engagements of one of the BIG6 firms and find that, in their sample, growth in audit fees under high litigation risk corresponds to the actual increase in audit hours for such engagements. In other words, auditors seem to respond to higher litigation risk by increasing their efforts rather than purely increasing risk premium. Some other studies, however, do find evidence of risk premium existence. For instance, Johnstone and Jean (2003) study distinct components of auditors' engagement risk and demonstrate that auditors use different strategies to address each of these components. Thus, auditors choose to allocate more experienced experts (i.e., aim at enhancing their work quality) only when dealing with the risks related to potential errors and fraud detection. However, when the risks stem from a clients' ability to continue as going concern or their public status, auditors tend to charge higher risk premium without subsequent increase in audit effort and quality. Bedard and Johnstone (2004) conduct a survey in which they ask audit partners to assess the risk of their clients and show that higher estimated risk is positively associated not only with the planned audit hours, but also with the planned hourly billing rates which supports the idea that auditors do charge additional risk premium to compensate for potential litigation costs in the future. Finally, Bell et al. (2008) explore consequences of business risk auditing adaptation (i.e., approach which implies comprehensive understanding of client's environment, business processes and strategy) and also find some evidence of risk premium charged by auditors. The authors use proprietary data on engagement hours and audit generated risk assessment and show that clients with higher assessed risk pay higher audit fees even after controlling for the actual labor usage.

Some of the empirical studies step back from trying to disentangle the drivers of the positive link between litigation risk. Instead, using larger samples of the companies worldwide, they demonstrate the existence of this link by comparing fees paid by companies which operate in different from the litigation risk standpoint environments. Thus, Seetharaman et al. (2002) compare U.K.-incorporated entities cross-listed in the U.S. with the ones cross-listed elsewhere and argue that auditors of the former are exposed to higher litigation risk. The authors find evidence that this enhanced risk related to being

cross-listed in the U.S. is positively associated with the fees charged by auditors. Choi et al. (2009) analyze cross-listed and non-cross listed companies from 14 non-U.S. countries and find evidence of the increase audit fees only for the cases when firms cross-list in countries with stricter legal regime than the one in the domestic country. Based on these results the authors conclude that it is not audit complexity which lead auditors to raise their fees for cross-listed firm but the enhanced litigation risk they become exposed to. Venkataraman et al. (2008) analyze audit pricing for companies at different stages of their life cycle when they and their auditors are exposed to different regulations. Particularly, the paper compared audit fees paid by the companies filing for IPOs with the fees paid by the very same companies afterwards. The results show that entities tend to pay higher fees when undergoing the IPO process which the authors relate to especially strict regulation applied in those cases. Badertscher et al. (2014) concentrate on a sample of the U.S. companies with publicly traded debt and find that public companies, which are exposed to higher litigation risks than private ones on average pay higher audit fees.

The papers discussed above analyze very different contexts and environments and all find support for the idea that auditors raise pricing of their services when exposed to higher litigation risks which can be explained by additional efforts put in auditing and (or) higher risk premium charged as a insurance against future litigation costs. Based on these studies we hypothesize that circuit-level shocks to auditors' litigation risk also caused corresponding changes in the audit fees charged to the clients headquartered in the affected U.S. states:

H1a: U.S. companies headquartered in the previously pro-auditors circuits experienced audit fees increase after Supreme Court's Tellabs decision in 2007.

H1b: U.S. companies headquartered in the previously pro-shareholders circuits experienced audit fees reduction after Supreme Court's Tellabs decision in 2007.

H1c: U.S. companies headquartered in the previously pro-shareholders circuits experienced audit fees reduction after Supreme Court's Janus decision in 2011.

In this paper we identify a company and its auditor as affected by the regulation if the former is headquartered in a state which belongs to a circuit that had to change their approaches to the pleading procedures and secondary actors' responsibility after the Supreme Courts rulings were announced. However, the state of headquarters is main but not exclusive jurisdiction for shareholders to bring a lawsuit against companies. Alternatively, they may file a lawsuit in the state where the company primarily conducts its business or where the alleged misconduct occurred. One of the limitations of this study is the inability to include these additional sources of litigation risk in the analysis. Yet, previous studies (e.g., Park, 2017) find that around 85% of the cases are filed in the headquarter state which still makes it reasonable to believe that auditors do take these states into consideration when assessing their potential litigation costs. Furthermore, it's important to note that while actual lawsuits may be initiated in states other than the companies' headquarters, auditors can hardly foresee the location of the legal proceedings in advance. As a result, when assessing the litigation risk associated with a particular engagement, their primary focus will likely be on the legal consequences in the state where the lawsuit is *ex ante* most likely to be initiated (i.e., the client's headquarter state).

2.2.3 Litigation Risk and Audit Opinions

Another strategy that auditors can employ when dealing with litigation risk is the issuance of a modified audit opinion (MAO) or a going concern (GC) opinion⁶. According to Auditing Standard (AS) 3101, auditors must issue a modified opinion — qualified, adverse, or disclaimer — if they identify a material misstatement or are unable to obtain sufficient appropriate audit evidence. These opinions must clearly state the nature of the modification, its basis, and its impact on the financial statements. Such opinions may serve as a form of legal protection, allowing auditors to document professional skepticism and limit their exposure to liability in the event of future financial failure or restatement.

⁶In this section, we focus on reporting conservatism *per se*, which reflects auditors' disclosure choices, rather than on the quality of the audit work itself — better captured by the accuracy of such opinions — which we discuss separately in the next subsection.

A related mechanism is the issuance of a going concern opinion, which auditors of U.S. public companies have been required to report on since 1918. Financial markets and regulatory bodies have consistently paid close attention to this part of the audit report, expecting auditors to alert stakeholders to potential risks concerning a company's survival. Regulatory standards have progressively expanded the auditor's responsibility in this area. While earlier standards such as SAS No. 34 required disclosure only when "a question arises about an entity's continued existence," the current version — SAS No. 132 — requires auditors to evaluate a broader set of conditions that may "raise substantial doubt about an entity's ability to continue as a going concern for a reasonable period of time."

Auditing literature provides broad evidence that absence of both, modified and a timely issued going concern opinion for a financially distressed client, increases auditors' litigation risk since investors see this as auditor's reporting error which creates grounds for legal actions. For instance, Carcello and Palmrose (1994) focus on a sample of firms that filed for bankruptcy and show that auditors are named as defendants in absolute majority of the cases when investors decide to initiate a lawsuit against a bankrupt company. However, the results also reveal that auditors that do manage to issue timely modified opinions, on average, have lower chances to be pledged guilty (i.e., dismissal rates are higher) and pay lower amount settlements in if they are found guilty. These findings suggest that auditors could choose issuing MAO opinions as a preemptive strategy against heightened litigation risk. Further, Kaplan and Williams (2013) employ simultaneous equation approach and, concentrating on a sample of financially distressed U.S. companies involved in securities class action litigation, find that *ex ante* litigation risk is positively associated with auditors' propensity to issue a modified GC report. Moreover, the results reveal that timely issued GC reports do indeed discourage investors from naming auditors among other defendants.

Some studies investigate whether auditors themselves consider issuing MAO and GC opinions as a mean of protecting themselves against litigation risk. Thus, Firth et al. (2012) explore the Chinese setting where audit companies may have different organiza-

tional structures with varying forms of liability and show that auditors of partnerships — who face unlimited personal liability — are more likely to issue modified audit opinions compared to their counterparts in limited liability firms. Geiger et al. (2006) study whether decrease in auditors' litigation risk related to the adoption of the Private Securities Litigation Reform Act of 1995 reduced auditors' precaution and affected their decisions to issue GC opinions to their financially distressed clients. In line with the hypothesis, the authors find a significant decrease in this likelihood of issuing such opinion after the reform took place. Blay (2005) runs an experiment where Big4 auditors are asked to form an opinion about a client's going concern ability based on a set of provided financial statements. The author manipulated the litigation risk faces by auditors and shows that under elevated litigation risk conditions auditors tend to assess the client as less likely to survive as compared to the evaluation made for the very same client under lower litigation risk conditions.

Taken together, the studies discussed in this section suggest that auditors use conservative reporting to protect themselves from litigation risk. Building on this, we hypothesize that auditors affected by the circuit-level litigation risk changes examined in this paper likely reconsidered their approach to issuing modified and going concern opinions, aligning their decisions with the changes in their legal exposure.

H2a: U.S. companies headquartered in previously pro-auditor circuits experienced an increase in MAO and GC opinion rates following the Supreme Court's Tellabs decision in 2007.

H2b: U.S. companies headquartered in previously pro-shareholder circuits experienced a decrease in MAO and GC opinion rates following the Supreme Court's Tellabs decision in 2007.

H2c: U.S. companies headquartered in previously pro-shareholder circuits experienced a decrease in MAO and GC opinion rates following the Supreme Court's Janus decision in 2011.

2.2.4 Audit Quality

Finally, we turn to the hypotheses concerning audit quality shifts induced by the Supreme Court decisions. In our setting, audit quality provides the most direct empirical test of the effort-adjustment strategy as a means of mitigating litigation risk. Francis and Krishnan (1999) argue that auditors seeking to protect themselves from heightened litigation exposure may increase audit effort to verify accruals more thoroughly. Dye (1993) develops a theoretical model showing that when auditors have sufficient wealth at stake, moderately stringent legal regimes induce higher audit quality. Consistent with these predictions, several empirical studies document a positive association between litigation risk and audit quality. Francis and Wang (2008), using data from 42 countries, find that investor protection is positively associated with earnings quality for Big 4 clients. Venkataraman et al. (2008) show that firms report lower accruals in the pre-IPO period, a time of heightened litigation risk. Boone et al. (2011), using a simultaneous-equations approach, document a negative relation between client-level litigation risk and abnormal accruals. Collectively, this evidence indicates that higher litigation risk tends to be accompanied by higher audit quality.

Although assessing whether litigation-risk shocks affect audit quality is an important question on its own, doing so also clarifies the mechanism behind the fee responses discussed earlier. Because audit fees may increase either due to higher risk-premium charges or because auditors expend more effort, the behavior of audit quality helps distinguish between these two channels. Fee changes accompanied by similar movements in audit quality would be consistent with an effort-adjustment response, whereas fee changes without corresponding quality shifts would point instead to risk-premium pricing as the dominant mechanism.

Taken together, the theoretical arguments and empirical evidence imply that circuit-level litigation risk shocks could have induced audit quality adjustments — operating through increased effort — in the same direction as the change in auditors' exposure. Accordingly, we formulate the following hypotheses:

H3a: U.S. companies headquartered in the previously pro-auditors circuits experienced an increase in audit quality after Supreme Court's Tellabs decision in 2007.

H3b: U.S. companies headquartered in the previously pro-shareholders circuits experienced a decrease in audit quality after Supreme Court's Tellabs decision in 2007.

H3c: U.S. companies headquartered in the previously pro-shareholders circuits experienced a decrease in audit quality after Supreme Court's Janus decision in 2011.

The remaining challenge of the audit quality analysis part is to find an appropriate way to measure possible quality-related changes. Audit quality is a broad notion defined by DeFond & Zhang (2014) as "assurance that the financial statements faithfully reflect the firm's underlying economics, conditioned on its financial reporting system and innate characteristics". Due to complexity and relative subjectivity of the concept there is no unique and straightforward measure of audit quality. Instead, audit literature proposes various audit quality proxies that are intended to capture its different aspects. DeFond & Zhang (2014) suggest researchers to choose among these proxies the ones which better suit the posed research question and particular dimension of audit quality they want to study. Given that this paper examines how litigation risk affects audit outcomes, the audit quality measures should ideally reflect changes in auditor effort — and the consequent reporting choices — aimed at improving those aspects of quality that help reduce legal risk.

Thus, the main measure of audit quality used in this paper is material misstatements, a straightforward indicator of relatively severe financial reporting failures "overlooked" by the auditor, which may entail significant litigation costs. In his seminal paper, DeAngelo (1981) defines audit quality as the joint probability that an auditor both detects and reports a material misstatement. More recent empirical work by Rajgopal et al. (2021) supports the relevance of this measure, particularly from the perspective of third parties, showing that restatements are among the strongest predictors of legal actions against auditors, including SEC enforcement and securities class action lawsuits. In other words, the presence of a restatement is shown to significantly increase the likelihood of auditors

being accused of negligence. Given our focus on litigation risk, restatements serve as a suitable proxy for testing whether auditors respond by improving audit quality. Since restatements are highly visible indicators of audit failure and are strongly associated with legal consequences, auditors facing greater litigation exposure have a clear incentive to prevent them. A decline (increase) in restatement incidence under higher (lower) litigation risk would provide direct evidence that auditors adjust their effort and reporting to reduce liability.⁷ The proxy is not without limitations though - reflecting both the client's initial reporting choices and subsequent auditor-client negotiations over adjustments, material misstatements are not under the auditor's direct control and, therefore, do not depend exclusively on audit quality. To address this, we employ several alternative measures of audit quality which allows us to test the robustness of our findings.

First, we test whether there are any shifts in the accuracy of issued GC opinions. This is one of the few reporting choices directly under the auditor's control, allowing us to track changes in audit quality driven solely by the auditor's work and reporting decisions. While the probability of issuing a GC opinion discussed earlier may reflect an auditor's conservatism (rather than pure audit quality) and could be a strategy to mitigate litigation risk (Carson et al., 2013; Francis & Yu, 2009), the accuracy of these opinions better reflects the quality of the auditor's work. Specifically, we examine two types of audit errors related to issuing GC opinions: Type I error, where auditors issue a GC opinion without the subsequent bankruptcy of their client, and Type II error, where auditors fail to issue a GC opinion but the client files for bankruptcy within a year after the fiscal year-end. Additionally, following Gul et al. (2013), we test whether auditors become more or less aggressive in their MAO reporting choices. Higher aggressiveness reflect more favorable reporting for the client, which may indicate lower audit quality.

⁷The underling assumption is that the litigation risk shocks trigger immediate adjustments in audit quality, and that these adjustments are the primary determinant of whether a restatement becomes necessary in subsequent periods. However, the shock may instead influence the probability that a restatement is later communicated, rather than the underlying need for one. In future work, we plan to address this by incorporating proxies for the ex ante likelihood of restatement.

2.2.5 Big4 vs non Big4

Finally, we hypothesize that the effects of the litigation risk changes discussed above are more pronounced for Big 4 auditors. Big 4 auditors are known to have deep pockets which exposes them to greater wealth at risk in the cases of legal disputes (Dye, 1993; Choi et al., 2004). These potentially high litigation costs should make Big 4 auditors especially sensitive to legal environment in which they operate. Indeed, some papers document that the well established in the literature high audit quality provided by Big4 companies is, to a great extent, driven by the greater exposure of these firms to potential litigation costs (Palmrose, 1988; Lennox, 1999; Khurana and Raman, 2004). In other words, these papers claim that Big4 auditors recognize themselves to be especially "appealing" targets for plaintiffs in the cases of detected financial frauds and choose to provide ex-ante high-quality audits as a preemption of future lawsuits. Several studies show that while the enhanced audit quality enables Big 4 companies to diminish the frequency of lawsuits filed against them, the payouts they incur if found guilty are greater than those borne by non-Big 4 auditors (Andersen et al., 1992; Lennox and Li, 2019). The fact that Big 4 firms ex ante deliver superior audit quality in order to minimize the chances of legal actions against them and still face substantial fines if found culpable, leads us to hypothesize that Big 4 auditors are especially sensitive to the circuit-level litigation risk changes explored in this paper. Hence, we additionally test the formulated above hypotheses exclusively within the context of the Big 4 engagements. The following provides more details on the research methodology and sample composition.

2.3 Research Design and Results

2.3.1 Research Design

Existence of circuit splits and their arguably unanticipated resolutions typical for the U.S. federal court system, provides us with an opportunity to employ a difference-in-difference

research design. This empirical approach allows better identification of the effect of litigation risk shocks on audit outcomes as it helps to rule out the impact of potential contaminating events and to control for temporal trends in audit reporting, pricing and quality. Regressions used to examine the effects of litigation risk changes induced by Supreme Court's decisions regarding Tellabs case have the following specification:

$$\begin{aligned} AUD_FEE_{i,t} \text{ or } MAO_OPINION_{i,t} \text{ or } RESTATE_{i,t} = \\ \beta_0 + \beta_1 Post * RISK_UP_{i,t} + \beta_2 Post * RISK_DOWN_{i,t} + \\ \sum \beta_j Controls_{i,t} + YearFE + FirmFE + \varepsilon_{i,t} \end{aligned}$$

where $AUD_FEE_{i,t}$ is a natural logarithm of audit fees, $MAO_OPINION_{i,t}$ is a binary variable equal to one if an auditor issues an opinion different from unmodified and to zero otherwise; and $RESTATE_{i,t}$ is a binary variable equal to one if a company filed material restatement and to zero otherwise.

We also examine additional outcome variables. The first is $GC_OPINION_{i,t}$, a binary variable equal to one if the auditor issues a GC opinion and zero otherwise. The second is $GC_ERROR_{i,t}$, a binary variable equal to one if the auditor's GC opinion involves a Type I or Type II error, and zero otherwise. As discussed earlier, the issuance of a GC opinion is a significant reporting decision that may influence auditors' litigation risk. To test the link between litigation risk and such opinions, we follow prior studies (e.g., Reynolds and Francis, 2000; DeFond et al., 2002; Carson et al., 2013) and restrict the analysis to a sample of financially distressed firms, for which GC opinions are particularly relevant. Since this restriction substantially reduces the sample size, the findings should be interpreted with appropriate caution. Finally, we include in the analysis a variable which reflects auditors' aggressiveness in issuing modified opinions. $AUD_AGGR_{i,t}$, is borrowed from Gul et al. (2013) and defined as the difference between the predicted probability of receiving a modified audit opinion (MAO) and the actual MAO outcome.⁸

⁸Following Gul et al. (2013), we estimate the probability of receiving an MAO using annual logistic regressions; the actual MAO equals one if the client receives a modified opinion, and zero otherwise.

$RISK_DOWN_{i,t}$ and $RISK_UP_{i,t}$ are the variables indicating whether a client company's headquarter is located in a circuit where auditors experienced litigation risk changes after the decision on Tellabs case was announced. Specifically, $RISK_DOWN_{i,t}$ is a binary variable which equals to one if a client is headquartered in a circuit where the auditors' litigation risk decreased and $RISK_UP_{i,t}$ equals one if the headquarter is in the circuits where the auditors' litigation risk increased. As discussed earlier, academic literature struggles to develop a unique classification of the circuits' positions regarding the pleading scienter and, as a result, to clearly identify which of the circuit courts were affected by this decision. For the sake of completeness of the analysis, we test the hypotheses using both classifications and their interaction (i.e., including in the treatment group companies headquartered in the circuits which are recognized as affected ones by both classifications).

Following the classification suggested by Honigsberg et al. (2020), the first treatment group consists of the companies headquartered in the Ninth and Eleventh Circuits where the auditors' litigation risk increased ($RISK_UP = 1$) and in the Second and Third Circuits where it decreased ($RISK_DOWN = 1$). Companies headquartered in the circuits that experience no changes in terms of approaches used to assess the proof of defendants' scienter compose the control group. Under the second classification of the circuits borrowed from Choi and Pitchard (2012) the treatment groups consist of the client companies headquartered in the First, Fourth, Sixth, and Ninth circuits ($RISK_UP = 1$) or in the Third or Seventh ($RISK_DOWN = 1$). Audit engagements in which clients are headquartered in the Second, Fifth, Seventh, Eighth, Tenth, Eleventh circuits and District of Columbia comprise the control group. Finally, both classifications recognize the Second circuit as a circuit where auditors benefited from the revision of scienter recognition rules, and the Ninth circuit as the one where auditors encountered an increase in litigation risk. Therefore, under the third approach to identification of the treatment and controls groups, the former consists of the client companies headquartered in the Ninth ($RISK_UP = 1$) or in the Second circuits ($RISK_DOWN = 1$) while all the rest companies comprise control group.

$Post_{i,t}$ is a time variable which indicates whether the fiscal year end of the audited financial report is after the announcement of the Supreme Court's decision. The verdict regarding Tellabs case was announced in June 2007 and the circuit courts were supposed to adapt their practices to the Supreme Courts' ruling immediately. In the main specification we exclude year 2007 from the event window as there could have been some uncertainty regarding the circuit-level legal changes around the ruling announcements and it could have taken auditors some time to realize the real effects of the Supreme Court's decision on their litigation risks. The time period covered in the analysis is from January 2003 to December 2010 with four-year pre-period (2003-2006) and three-year post-period (2008-2010). As 2011 was the year of Janus ruling announcement, in order to isolate the effect of the Supreme Court's decision on Tellabs case, we choose to disrupt the event window in 2010. To ensure the robustness of the results, we also run the regressions on the event window which includes the year of announcement. In this case, $Post_{i,t}$ is set equal to 1 for the annual reports with financial year end in July 2007 or later. Finally, the main variables of interest are the interaction terms $Post * RISK_UP_{i,t}$ and $Post * RISK_DOWN_{i,t}$, which capture the difference-in-differences effects.

In line with the hypotheses, when analyzing the effects of Supreme Court's decisions on audit pricing, we expect the sign of $Post * RISK_UP_{i,t}$ to be positive and the sign of $Post * RISK_DOWN_{i,t}$ - to be negative. The set of control variables included in this regression specification is typical for the audit fee literature and includes such company-specific variables as size, inventories & receivables, ROA, leverage, market-to-book ratio, and dummies for reported losses, negative equity, December year end, acquisitions, restatements, and foreign operations. The audit-related controls are Big 4 and industry specialist dummies, a dummy for a qualified audit opinion and a dummy for an auditor change. In the additional analysis we also consider two additional variables to control for the complexity of audit engagement, number of operational and geographical segments. These variables are excluded from the main analysis as this information is available for fewer number of companies and their inclusion leads to a significant reduction of the sample.

Further, we expect the sign of $Post * RISK_UP_{i,t}$ coefficient to be positive (and $Post * RISK_DOWN_{i,t}$ to be negative) when analyzing the effect of the Supreme Court's decision on the probability of receiving a MAO and GC opinions which is in line with the hypothesis that auditors switch to more (less) conservative reporting when facing increased (decreased) litigation risk. In the specifications where the dependent variable is *RESTATE* instead the expected sign of $Post * RISK_UP_{i,t}$ ($Post * RISK_DOWN_{i,t}$) coefficient is negative (positive) which would suggest upward (downward) shifts in audit quality induced by the litigation risk changes. The set of controls used in the MAO and GC opinion reporting analysis is similar to the one used in the audit fees regressions with the exception of few additional variables. In particular, we include Z-Score and operating cash flow as measures of the client's probability of going bankrupt, and early returns and their monthly volatility which contain additional information regarding client's financial health.

For the analysis of the effects of Janus ruling on the audit process and outcomes, the research design is very similar to the one of Tellabs case with the only difference in treatment and control groups composition and the *Post* time indicator.

$$AUD_FEE_{i,t} \text{ or } MAO_OPINION_{i,t} \text{ or } RESTATE_{i,t} = \\ \beta_0 + \beta_1 Post * RISK_DOWN_{i,t} + \sum \beta_j Controls_{i,t} + YearFE + FirmFE + \varepsilon_{i,t}$$

To the best of our knowledge, existing literature suggests only one classification of the pre-ruling circuit courts' positions regarding primary violation identification. Thus, the only two circuits that are recognized as being affected by the Janus ruling are the Fourth and Ninth. The Supreme Court's decision stated that the only party which can be accused of making a fraudulent statement is the one with the ultimate authority over such statement. The verdict virtually prohibited the substantial participation test previously used by the Fourth and Ninth circuits making it impossible to initiate lawsuits against parties that assisted management in preparing fraudulent statements. Hence, the treatment

group in this case consists of the companies headquartered in the Fourth and Ninth circuits ($RISK_DOWN = 1$), where auditors' litigation risk decreased following the announcement of the Supreme Court decision, while all other companies form the control group.

In the main analysis the event window in case of Janus spans from 2007 to 2015 with the year of ruling announcement (2011) excluded from the sample and $Post$ equal to 1 for the annual reports with financial year end in 2012 or later. The alternative sample period tested in the robustness includes the Supreme Court's decision announcement year. In this case $Post$ is set to 1 for the annual reports with financial year end in July 2011 or later. The main variable of interest is the interaction term $POST * RISK_DOWN$ which captures the effect of the litigation risk change for the auditors engagements potentially affected by Janus ruling. The main variables of interest and sets of controls are identical to the one used in the Tellabs case analysis.

2.3.2 Sample Selection and Descriptive Statistics

To obtain the samples needed to analyse the effects of the Tellabs and Janus cases, we first download information regarding all the public companies headquartered in the U.S. throughout the examined periods from CRSP/Compustat merged database. The financial companies are excluded from the analysis due to their specific nature. The current headquarters data which is needed to construct treatment and control groups is obtained from Compustat and then updated for the historical changes of the headquarters location using the data from mingze-gao.com. This website provides a database that contains the historical headquarters of the companies retrieved from the 8-K SEC filings. Finally, the initial samples are merged with audit-related information taken from Audit Analytics.

Table 2.2 provides details of the sampling procedure. In the case of Tellabs, we start with 20 797 company-year observations (4 618 unique companies) operating in non-financial industries, for which we are able to identify the headquarters state and obtain audit fee

information. Dropping the companies with no information needed to construct the control variables brings the sample down to 19 982 observations (4 567 unique companies) for the audit pricing analysis. For the modified opinions analysis, we lose a few observations at the starting point due to the unavailability of opinion data, and start with 20 386 firm-year observations. Then, given that in this part of the analysis we include some additional controls in line with the literature studying MAOs, we lose 1 085 observations due to unavailability of the data needed to construct these controls, and the resulting sample is slightly smaller than the one used for the audit fee analysis — 19 301 firm-year observations. For the restatements analysis, we start with 20 397 firm-year observations and lose 1 705 observations due to unavailability of the data needed to construct control variables (in particular, adding the SALES GROWTH variable, which is typical for restatement research, causes us to lose more than a thousand observations). Samples used to test the effect of the Janus ruling on audit-related outcomes are constructed using the same approach. As shown in Panel B of Table 2.2, the Janus-related samples comprise 20 567 observations for the audit fee analysis, 19 645 for the MAO analysis, and 19 180 for the restatement analysis.

For the additional analysis focusing on GC-related outcomes potentially influenced by shifts in litigation risk, we follow prior literature (e.g., Reynolds and Francis, 2000; DeFond et al., 2002; Carson et al., 2013) and restrict the sample to financially distressed firms. Following Reynolds and Francis (2000) and DeFond et al. (2002), we classify a firm as financially distressed if it reports either negative earnings or negative operating cash flows. The final sample used to examine the GC-related effects of the Supreme Court rulings comprises 5 277 observations for the Tellabs ruling and 8 149 observations for the Janus ruling.

Table 2.2: Sample Selection

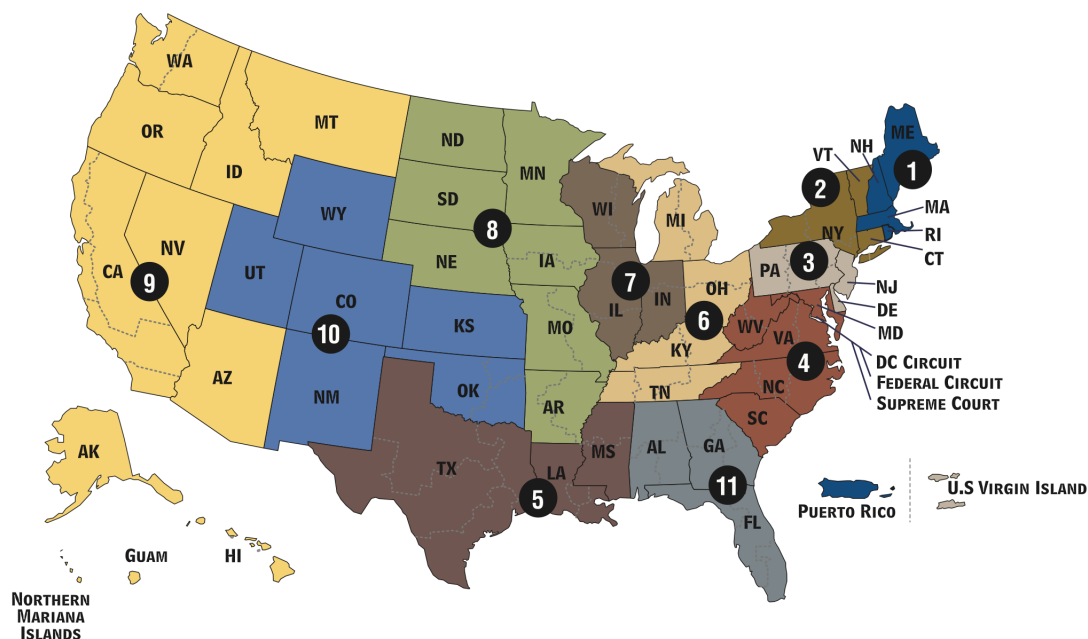
Panel A: TELLABS			
	Audit Fees	Modified Opinions	Restatements
Observations with data needed to construct the outcome variable	20 397	20 386	20 397
<i>Less: observations with no data needed to construct control variables</i>	(415)	(1 085)	(1 705)

Table 2.2: Sample Selection (continued)

Final Sample:			
Firm-year observations with all information available	19 982	19 301	18 692
Panel B: JANUS			
	Audit Fees	Modified Opinions	Restatements
Observations with data needed to construct outcome variable	21 050	21 044	21 150
<i>Less: observations with no data needed to construct control variables</i>	(483)	(1 399)	(1 970)
Final Sample:			
Firm-year observations with all information available	20 567	19 645	19 180

Figure 2.1 shows geographic boundaries of the U.S. Federal Court's circuits⁹ and Table 2.3 Panel A reports geographical distribution of headquarter locations of the companies included in the samples as for the most recent year of observation. For all the samples the most populated circuit is the Ninth which contains California state where the majority of the U.S companies are headquartered. For the rest, the distribution of the companies among circuits does not reveal any particular patterns. Industry distribution is outlined in Panel B of the Table shows that companies operating in all the main industries are well presented in the samples. Finally, Panel C of the Table reports distribution of the observations by years.

Figure 2.1: U.S.Federal Court Circuits



⁹https://www.uscourts.gov/sites/default/files/u.s._federal_courts_circuit_map.1.pdf

Table 2.3: Sample distribution by circuits, years, and industries

Panel A: Distribution by Circuits						
Circuit	Tellabs			Janus		
	Audit Fees	GC	Restatements	Audit Fees	GC	Restatements
DC	77	76	72	76	76	75
1	1 300	1 272	1 201	1 374	1 349	1 253
2	2 125	2 052	1 973	2 149	2 070	1 965
3	1 802	1 760	1 700	1 749	1 695	1 646
4	1 231	1 213	1 127	1 339	1 316	1 241
5	2 251	2 113	2 157	2 549	2 262	2 403
6	1 274	1 241	1 214	1 367	1 315	1 316
7	1 301	1 275	1 230	1 440	1 409	1 371
8	1 164	1 129	1 115	1 109	1 081	1 083
9	4 940	4 785	4 576	4 830	4 651	4 421
10	1 084	1 016	988	1 264	1 144	1 153
11	1 433	1 396	1 339	1 321	1 277	1 253
Total	19 982	19 301	18 692	20 567	19 645	19 180

Panel B: Distribution by Industries						
Industry	Tellabs			Janus		
	Audit Fees	GC	Restatements	Audit Fees	GC	Restatements
Agriculture	78	66	72	71	62	65
Construction	264	162	281	330	207	332
Manufacturing	9 853	9 721	9 103	9 990	9 836	9 158
Mining	1 142	1 093	1 054	1 469	1 338	1 351
Retail Trade	816	791	670	870	832	792
Services	4 629	4 475	4 463	4 555	4 397	4 337
Transportation & Utilities	2 209	2 104	2 150	2 307	2 147	2 275
Wholesale Trade	742	674	694	767	659	729
Other	249	215	205	208	167	141
Total	19 982	19 301	18 692	20 567	19 645	19 180

Panel C: Distribution by Years						
Year	Tellabs			Janus		
	Audit Fees	GC	Restatements	Audit Fees	GC	Restatements
2003	3 038	2 936	2 893			
2004	3 073	2 972	2 789			
2005	3 016	2 916	2 722			
2006	3 044	2 934	2 785			
2008	2 762	2 671	2 637			
2009	2 608	2 521	2 533			
2010	2 441	2 351	2 333			
2007				2 981	2 859	2 733
2008				2 762	2 671	2 637
2009				2 608	2 521	2 533
2010				2 441	2 351	2 333
2012				2 335	2 232	2 199
2013				2 387	2 250	2 190
2014				2 507	2 357	2 236
2015				2 547	2 407	2 319
Total	19 982	19 301	18 692	20 576	19 645	19 180

Table 2.4 presents the summary statistics for all variables included in the analyses. For the Tellabs ruling, where the sample period spans from 2003 to 2010 (excluding 2007), client firms have average total assets of approximately \$326 million in the audit fees sample (corresponding to a mean log of assets of 19.60). Average firm size is comparable across the other two samples, at approximately \$ 314 million in the MAO sample and \$

348 million in the restatement sample. On average, a client in the audit fees sample pays \$663 thousand in audit fees (mean log of 13.399). Audit fees are slightly lower in the MAO sample, averaging around \$648 thousand and slightly higher in the restatement sample. In the MAO sample, in 44.3% of the cases companies receive a modified audit opinion, while in 12.2% of the cases companies in the restatement sample report a restatement of their financial statements.

The Janus ruling sample, which covers the 2007–2015 period (excluding 2011), reflects a slight upward trend in client size and audit fees, consistent with macroeconomic growth and regulatory evolution. The average log of assets is 20.013 (approximately \$491 million) in the audit fee sample, 19.955 (approximately \$479 million) in the MAO sample, and 20.121 (approximately \$548 million) in the restatement sample. On average, clients pay \$931 thousand in audit fees in the audit fee sample; in the MAO sample, 32.6% of firm-year observations receive a modified audit opinion, while 7.5% of observations in the restatement sample involve a financial statement restatements.

Notably, across all samples, firms exhibit negative average return on assets, and loss rates are markedly high — approaching 40% — which is consistent with the overlap of the sample periods and the 2008 global financial crisis. The incidence of modified audit opinions is also elevated under both legal regimes, aligning with prior research documenting heightened auditor caution during periods of financial distress. Overall, the remaining firm characteristics are consistent with those observed in typical archival auditing studies.

Table 2.4: Descriptive Statistics

Panel A: Audit Fees Sample

Variable	TELLABS					JANUS				
	Min	25%	Mean	75%	Max	Min	25%	Mean	75%	Max
FEE_AUD	9.838	12.471	13.399	14.286	16.643	10.586	12.898	13.731	14.539	16.788
SIZE	15.416	18.086	19.599	21.011	24.548	15.663	18.479	20.013	21.477	24.900
INVREC	0	0.090	0.245	0.356	0.800	0	0.075	0.227	0.333	0.766
IMPAIR	0	0	0.094	0	1	0	0	0.108	0	1
ROA	-1.650	-0.068	-0.065	0.070	0.289	-1.653	-0.075	-0.071	0.066	0.292
LOSS	0	0	0.376	1	1	0	0	0.388	1	1
LEV	0	0	0.169	0.276	0.948	0	0	0.188	0.307	0.939
NEG_EQ	0	0	0.046	0	1	0	0	0.050	0	1

Table 2.4: Description Statistics (continued)

MTB	-15.793	1.247	2.890	3.529	29.365	-18.645	1.176	3.071	3.652	34.917
ACQUIS	0	0	0.105	0	1	0	0	0.104	0	1
FOREIGN	0	0	0.300	1	1	0	0	0.359	1	1
WEAK	0	0	0.115	0	1	0	0	0.112	0	1
BUSY	0	0	0.711	1	1	0	0	0.742	1	1
BIG4	0	1	0.751	1	1	0	0	0.710	1	1
OPINION	0	0	0.442	1	1	0	0	0.324	1	1
IND_SPEC	0	0	0.275	1	1	0	0	0.268	1	1
AUD_NEW	0	0	0.081	0	1	0	0	0.069	0	1
number of firm-year observations					19 982	20 567				

Panel B: Modified Audit Opinions Sample

	TELLABS					JANUS				
<i>Variable</i>	<i>Min</i>	<i>25%</i>	<i>Mean</i>	<i>75%</i>	<i>Max</i>	<i>Min</i>	<i>25%</i>	<i>Mean</i>	<i>75%</i>	<i>Max</i>
OPINION	0	0	0.443	1	1	0	0	0.326	1	1
SIZE	15.371	18.038	19.548	20.941	24.497	15.630	18.412	19.955	21.426	24.851
LEV	0	0	0.163	0.266	0.948	0	0	0.179	0.291	0.935
LOSS	0	0	0.381	1	1	0	0	0.394	1	1
ROA	-1.701	-0.074	-0.069	0.070	0.283	-1.719	-0.081	-0.076	0.067	0.288
MTB	-15.793	1.253	2.901	3.553	29.347	-19.436	1.183	3.070	3.667	35.085
Z.Score	-24.347	1.408	3.956	5.439	39.068	-25.192	1.244	3.647	5.072	42.922
OCF	-1.228	-0.002	0.015	0.127	0.333	-1.229	0.005	0.014	0.125	0.331
RETURN	-0.881	-0.240	0.299	0.432	7.243	-0.890	-0.287	0.188	0.332	6.753
VOL	0.108	0.309	0.567	0.660	3.602	0.107	0.276	0.517	0.610	3.405
FEE_AUD	9.822	12.457	13.382	14.270	16.601	10.571	12.881	13.718	14.531	16.727
WEAK	0	0	0.116	0	1	0	0	0.112	0	1
BIG4	0	0	0.748	1	1	0	0	0.707	1	1
IND_SPEC	0	0	0.274	1	1	0	0	0.264	1	1
AUD_NEW	0	0	0.080	0	1	0	0	0.068	0	1
TENURE	1	3	4.915	7	11	1	4	7.121	10	16
<i>number of firm-year observations</i>					19 301	19 654				

Panel C: Restatements Sample

Variable	TELLABS					JANUS				
	Min	25%	Mean	75%	Max	Min	25%	Mean	75%	Max
REST	0	0	0.122	0	1	0	0	0.075	0	1
SIZE	15.481	18.141	19.665	21.093	24.591	15.779	18.607	20.121	21.574	24.943
LEV	0	0	0.172	0.281	0.947	0	0	0.194	0.313	0.939
ROA	-1.448	-0.059	-0.053	0.071	0.285	-1.474	-0.055	-0.052	0.069	0.294
MTB	-14.963	1.231	2.810	3.435	28.027	-18.293	1.152	2.940	3.513	33.484
SALE_GROWTH	-0.773	-0.034	0.167	0.230	3.710	-0.817	-0.058	0.134	0.184	3.831
ACQUIS	0	0	0.105	0	1	0	0	0.105	0	1
WEAK	0	0	0.114	0	1	0	0	0.106	0	1
FEE_AUD	9.909	12.520	13.438	14.314	16.666	10.736	12.972	13.788	14.582	16.826
BIG4	0	1	0.755	1	1	0	0	0.718	1	1
IND_SPEC	0	0	0.279	1	1	0	0	0.273	1	1
number of firm-year observations					18 692	19 180				

Note: The Table presents descriptive statistics for all variables included in the audit fee, modified audit opinion, and restatement analyses for the Tellabs and Janus samples. All variables are defined in Appendix A, and continuous variables are winsorized at the 1st and 99th percentiles.

Table 2.5 separately reports summary statistics for the main variables of interest across

the treatment and control groups in the pre- and post-periods. To improve readability, for the Tellabs case sample we focus on the classification that combines the circuit assignments used by Honigsberg et al. (2020) and Choi and Pritchard (2012). Under this combined specification, the treatment group consists of firms headquartered in the Ninth (the Third) Circuit, where the Supreme Court decision increased (decreased) expected litigation exposure, while the control group includes all other firms, excluding those located in the circuit experiencing the opposite directional effect. This construction avoids overlap between the upward- and downward-affected circuits and provides a cleaner comparison. The Table reports mean log audit fees and the corresponding t-tests and median tests comparing differences across treatment status and across time. Panel A presents the audit-fee results, which show a marked increase in fees over time for both treatment and control firms, with POST–PRE differences consistently positive and statistically significant. For the Tellabs setting, firms located in circuits where the ruling increased auditors’ liability appear to converge toward the control group in fee levels, whereas in the pre-period the control firms paid somewhat higher fees. In the Janus setting, firms located in circuits where expected liability declined exhibit a noticeably smaller increase in fees. By contrast, Panels B and C indicate that while certain differences in audit opinions and restatements are statistically significant—particularly across the pre–post comparisons—these patterns do not display a consistent directional change across treatment and control groups. In other words, unlike audit fees, the responses in opinions and restatements do not follow a clear litigation-risk pattern at the univariate level. These preliminary results suggest that the primary adjustment to litigation-risk changes is reflected in audit pricing rather than in auditor reporting outcomes or restatement incidence. The regression analysis that follows examines these relationships formally, incorporates additional controls and fixed effects, and evaluates whether the fee-specific patterns persist once firm characteristics and time-varying factors are taken into account.

Table 2.5: Univariate Tests for Audit Fees, Opinions, and Restatements

Panel A: Audit Fees							
Comparison	Group 1		Group 2		Mean Diff (2)-(1)	t-test p	Median p
	Mean	Median	Mean	Median			
Tellabs: Increase vs No Effect							
PRE: Treatment vs. Control	13.196	13.227	13.261	13.252	0.065	0.024	0.153
POST: Treatment vs. Control	13.652	13.703	13.646	13.680	-0.005	0.875	0.933
Treatment: PRE vs. POST	13.196	13.227	13.652	13.703	0.456	0.000	0.000
Control: PRE vs. POST	13.261	13.252	13.646	13.680	0.385	0.000	0.000
Tellabs: Decrease vs No Effect							
PRE: Treatment vs. Control	13.222	13.194	13.261	13.252	0.039	0.386	0.400
POST: Treatment vs. Control	13.622	13.576	13.646	13.680	0.025	0.294	0.746
Treatment: PRE vs. POST	13.222	13.194	13.622	13.576	0.399	0.000	0.000
Control: PRE vs. POST	13.261	13.252	13.646	13.680	0.385	0.000	0.000
Janus							
PRE: Treatment vs. Control	13.656	13.710	13.641	13.658	-0.015	0.571	0.381
POST: Treatment vs. Control	13.753	13.844	13.855	13.895	0.102	0.000	0.001
Treatment: PRE vs. POST	13.656	13.710	13.753	13.844	0.097	0.001	0.000
Control: PRE vs. POST	13.641	13.658	13.855	13.895	0.214	0.000	0.000

Panel B: Opinions (MAO)							
Comparison	Group 1		Group 2		Mean Diff (2)-(1)	t-test	10
	Mean	Median	Mean	Median			
Tellabs: Increase vs No Effect							
PRE: Treatment vs. Control	0.433	0	0.462	0	0.029	0.007	
POST: Treatment vs. Control	0.440	0	0.422	0	-0.019	0.174	
Treatment: PRE vs. POST	0.433	0	0.440	0	0.007	0.629	
Control: PRE vs. POST	0.462	0	0.422	0	-0.040	0.000	
Tellabs: Decrease vs No Effect							
PRE: Treatment vs. Control	0.448	0	0.462	0	0.015	0.365	
POST: Treatment vs. Control	0.438	0	0.422	0	-0.016	0.471	
Treatment: PRE vs. POST	0.448	0	0.438	0	-0.010	0.698	
Control: PRE vs. POST	0.462	0	0.422	0	-0.040	0.000	

¹⁰Panels B and C report only t-test p-values. For binary outcomes, the median (Wilcoxon rank-sum) test and the t-test yield numerically identical p-values because both tests reduce to comparing proportions (means of 0/1 variables). Median tests are therefore redundant and omitted.

Table 2.5: Univariate Tests for Audit Fees, Opinions, and Restatements

Janus

PRE: Treatment vs. Control	0.533	1	0.509	1	-0.024	0.022
POST: Treatment vs. Control	0.118	0	0.110	0	-0.007	0.321
Treatment: PRE vs. POST	0.533	1	0.118	0	-0.415	0.000
Control: PRE vs. POST	0.509	1	0.110	0	-0.398	0.000

Panel C: Restatements

Comparison	Group 1		Group 2		Mean Diff (2)-(1)	t-test p
	Mean	Median	Mean	Median		

Tellabs: Increase vs No Effect

PRE: Treatment vs. Control	0.172	0	0.133	0	0.039	0.000
POST: Treatment vs. Control	0.099	0	0.091	0	0.009	0.277
Treatment: PRE vs. POST	0.172	0	0.099	0	-0.073	0.000
Control: PRE vs. POST	0.133	0	0.091	0	-0.043	0.000

Tellabs: Decrease vs No Effect

PRE: Treatment vs. Control	0.127	0	0.133	0	-0.007	0.548
POST: Treatment vs. Control	0.066	0	0.091	0	-0.024	0.021
Treatment: PRE vs. POST	0.127	0	0.066	0	-0.061	0.000
Control: PRE vs. POST	0.133	0	0.091	0	-0.043	0.000

Janus

PRE: Treatment vs. Control	0.100	0	0.082	0	0.018	0.004
POST: Treatment vs. Control	0.061	0	0.061	0	-0.001	0.920
Treatment: PRE vs. POST	0.100	0	0.061	0	-0.039	0.000
Control: PRE vs. POST	0.082	0	0.061	0	-0.021	0.000

Note: The Table reports summary statistics and univariate tests for audit fees, modified audit opinions, and restatements across the treatment and control groups in the pre- and post-periods for both the Tellabs and Janus samples. All variables are defined in Appendix A, and continuous variables are winsorized at the 1st and 99th percentiles.

2.4 Empirical Results

The following is dedicated to the results of the analyses. We start with discussing the results regarding audit pricing changes around the Supreme Court's rulings announcements. Figure [2.2](#) Panel A plots average audit fees paid by the companies in circuits which

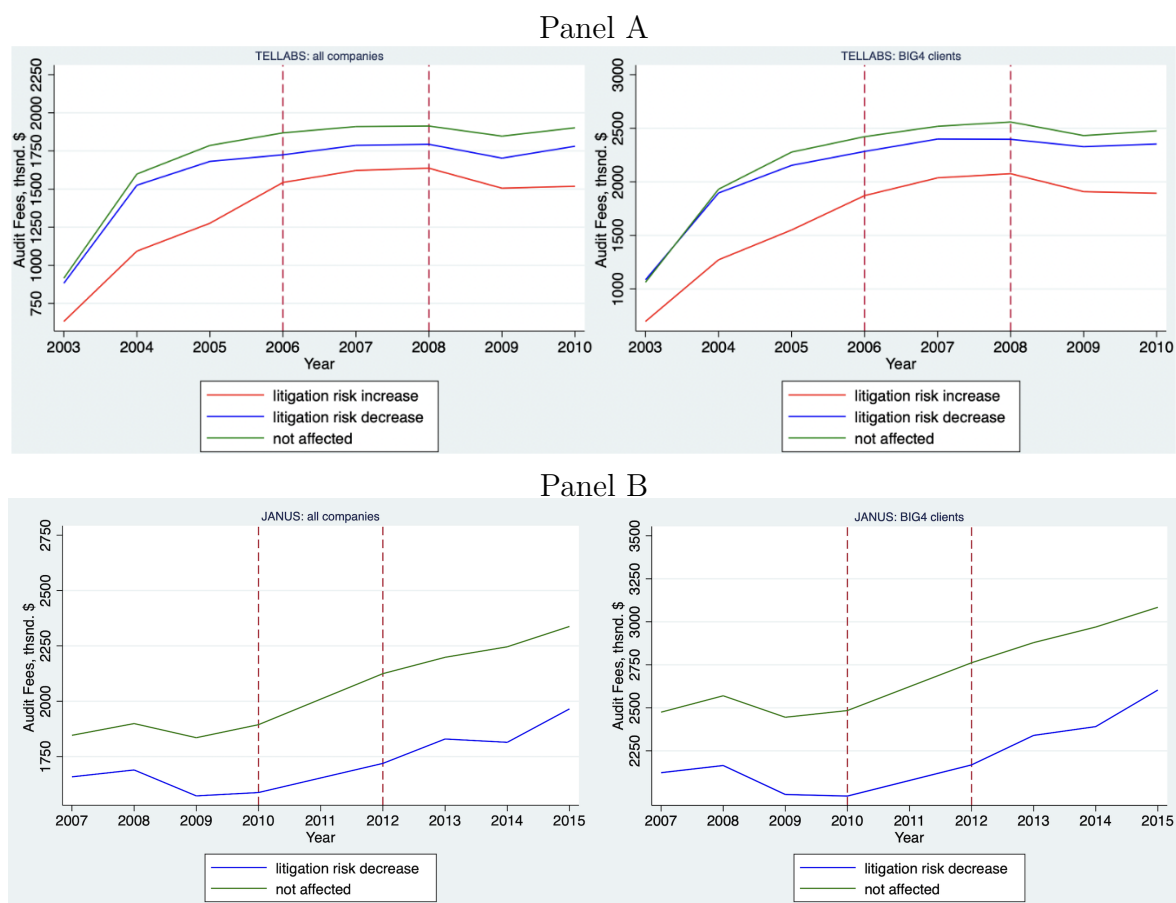
were differently affected by the Court's decision regarding Tellabs case ¹¹ (i.e., pleading scienter requirement). The red lines depict audits fees paid by companies headquartered in the Ninth circuit, where the auditors' litigation risk went up after the ruling was announced in June 2007; the blue lines depict fees charged to the companies headquartered in the Third circuit where the ruling led to decrease in the legal liability and the green lines represent audit fees paid by all the companies headquartered elsewhere. The left-hand side of the Figure summarises fees paid by all the companies in the sample, whereas the right-hand side plots audit fees paid by BIG 4 clients only. The plots show that even though companies headquartered in different circuits tend to pay different amounts of audit fees (which could be explained by the systematic differences between those companies addressed further in the analysis), there are still some common, usually upward, trends followed by all the companies. Also, one can notice that, in line with the hypotheses, the rise in audit fees during 2007 and 2008 was slightly greater for clients situated in the circuit where auditor liability increased. This trend is particularly notable for companies audited by the BIG 4 firms. Moreover, the graphs indicate a more moderated growth of audit fees for the clients headquartered in the Third circuit when compared to the other two groups.

Panel B of Figure 2.2 plots average audit fees paid by the companies in circuits which were differently affected by the Supreme Court's ruling on the Janus case (i.e., liability of secondary actors). In particular, the red lines show the average yearly fees paid by the companies headquartered in the as in the Fourth and the Ninth circuits where the auditors' litigation risk went down after the announcement of the Court's decision and green lines plot the fees paid by rest of the companies. As in the case of Tellabs, there are notable common trends shared by both groups of companies. At the same time the plots capture steeper growth in audit fees experienced by the companies that constitute the treatment group (i.e., those affected by the Janus decision).

Table 2.6 reports the results of a regression analysis aimed at studying the impacts of the

¹¹The most strict classification which combines the approached of Honigsberg et al.(2020) and Choi & Pitchard (2012) is applied

Figure 2.2: Audit fees paid in different Federal Circuits



Tellabs case ruling on audit fees. In particular, we run fixed effects regressions without and with a set of control variables included in the model. In the analysis we apply different classifications of the affected circuits that can be found in the existing literature. Columns 1 & 2 report the results for the classification borrowed from Honigsberg et al. (2020) that is based on the pre-ruling differences regarding the evidence required by the circuit level courts in order to blame auditors guilty. Columns 3 & 4, instead, show the results when applying the classification borrowed from Choi & Pitchard (2012). This classification relies on the pre-ruling circuit courts' positions regarding the competing inferences when acknowledging auditors' scienter. Finally, in Columns 5 & 6, we combine these two approaches and treat the circuits as effected ones only if both classifications recognize them as the ones where auditors' litigation changed after the Tellabs ruling was announced.

Table 2.6: Tellabs case: Audit Fees

<i>Variable</i>	Honigsberg et al.		Choi et al.		Combination	
POST*RISK_UP	0.016	0.048**	-0.007	0.013	0.048*	0.063***
<i>pred. sign: +</i>	(0.67)	(2.31)	(-0.32)	(0.67)	(1.95)	(2.92)
POST*RISK_DOWN	-0.027	0.012	-0.039	-0.018	-0.025	-0.004
<i>pred. sign: -</i>	(-1.04)	(0.51)	(-1.39)	(-0.74)	(-0.68)	(-0.12)
SIZE		0.397***		0.395***		0.396***
		(27.40)		(27.51)		(27.57)
INVREC		0.506***		0.511***		0.504***
		(6.81)		(6.84)		(6.75)
IMPAIR		0.022		0.022		0.022
		(1.47)		(1.48)		(1.46)
ROA		-0.229***		-0.229***		-0.229***
		(-7.66)		(-7.62)		(-7.65)
LOSS		0.035***		0.035***		0.036***
		(2.79)		(2.79)		(2.82)
LEV		0.073*		0.076*		0.072*
		(1.83)		(1.89)		(1.80)
NEG_EQ		0.090**		0.087**		0.091**
		(2.53)		(2.47)		(2.56)
MTB		0.003**		0.003**		0.003**
		(2.50)		(2.42)		(2.51)
ACQUIS		0.040***		0.040***		0.040***
		(3.18)		(3.20)		(3.18)
FOREIGN		0.022*		0.023*		0.022*
		(1.83)		(1.87)		(1.80)
WEAK		0.192***		0.192***		0.192***
		(10.87)		(10.84)		(10.87)
BUSY		0.223***		0.224***		0.227***
		(2.91)		(2.93)		(2.99)
BIG4		0.092***		0.090***		0.091***
		(2.65)		(2.59)		(2.62)
OPINION		0.038***		0.038***		0.038***
		(4.41)		(4.40)		(4.37)
IND_SPEC		0.040**		0.041**		0.041**
		(2.35)		(2.40)		(2.38)
AUD_NEW		-0.072***		-0.072***		-0.072***
		(-4.50)		(-4.51)		(-4.49)
<i>Const.</i>	13.400***	5.160***	13.400***	5.188***	13.400***	5.170***
	(3376.86)	(17.65)	(2839.48)	(17.92)	(4872.62)	(17.85)
<i>Firm FE</i>	Yes	Yes	Yes	Yes	Yes	Yes
<i>Year FE</i>	Yes	Yes	Yes	Yes	Yes	Yes
Obs	19 982	19 982	19 982	19 982	19 982	19 982
R-sq.	0.880	0.895	0.880	0.895	0.880	0.895

Note: The Table reports the results of fixed effects regressions estimating the effects of the Tellabs ruling on audit fees. The dependent variable is *AUD_FEE* the natural logarithm of audit fees. Columns 1–2 use the circuit classification of Honigsberg et al. (2020), Columns 3–4 use the classification of Choi and Pritchard (2012), and Columns 5–6 apply the combined classification in which a circuit is treated as affected only if both approaches identify it as such. Odd-numbered columns report specifications without control variables, while even-numbered columns include the full set of controls listed in the table. All regressions include firm and year fixed effects, and standard errors are clustered at the firm level. *t*-statistics are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

In most specifications, the main variables of interest — $POST * RISK_UP$ and $POST * RISK_DOWN$ — exhibit the predicted signs, although the results are not consistently statistically significant. The coefficient on $POST_RISK_UP$ is significant only when the treatment and control groups are defined according to the Honigsberg et al. (2020) classification (Column 2) or the combination of both classification schemes (Columns 5 and 6). These findings suggest that, consistent with the hypothesis, firms headquartered in the Ninth Circuit experienced an increase in audit fees following the rise in auditors' litigation risk in this jurisdiction. The Circuit is the one where the auditors were negatively affected by the Supreme Court ruling the most - it is recognized by both classifications as having been auditor-friendly prior to the Tellabs ruling and as having to become less favorable to auditors after June 2007. In terms of economic significance, firms in this circuit faced an estimated 6.5% increase in audit fees (Column 6), holding other factors constant. The results also hold when entropy balance is applied in order to match the companies from treatment and control groups (5).

Conversely, there is no evidence of a decrease in audit fees in the circuits where litigation risk declined following the Supreme Court decision, as the $POST * RISK_DOWN$ coefficient is never statistically significant across any specification. Most of the control variables are statistically significant and in line with the previous literature. Thus, the amount of audit fees is positively associated with the client company's size and intensity of receivables and inventory and is negatively associated with its profitability. Clients that engage in acquisitions or impair their assets pay more fees. As for audit-related

control variables, companies with Big 4 auditors and those receiving qualified auditors opinion tend to have higher fee payments, while a fee discount is observed in the first year of the audit engagement.

Table 2.7 reports the results when the sample is divided into Big 4 and non-Big 4 client sub-samples. In line with the hypotheses, the results show that it was the Big 4 auditors who responded to circuit-level litigation risk changes by adjusting the pricing of their services. Having deep pockets, these audit firms often become “cherished targets” for shareholder lawsuits and, as a consequence, are more attentive to litigation risk levels. Consistent with expectations, for the Big 4 client sub-sample (Columns 1, 3, and 5), the interaction coefficient $POST * RISK_UP$ is of the predicted sign throughout the table. As in the case of the full sample, the coefficients are statistically significant when the Honigsberg et al. (2020) or the combined classifications are applied. By contrast, there is no evidence that non-Big 4 audit firms adjusted their pricing in response to increased litigation risk. Finally, the results show that neither Big 4 nor non-Big 4 auditors changed their fees in response to the reduction in litigation risk observed in some circuits following the Tellabs decision - $POST * RISK_DOWN$ is never statistically significant and, in most specifications, has a sign opposite to the one predicted.

Table 2.7: Tellabs case: BIG4 vs non-BIG4 Audit Fees

Variable	Honigsberg et al.		Choi et al.		Combination	
	BIG4	non-BIG4	BIG4	non-BIG4	BIG4	non-BIG4
POST*RISK_UP	0.051**	-0.046	0.031	-0.003	0.078***	-0.022
<i>pred. sign: +</i>	(2.14)	(-1.05)	(1.41)	(-0.08)	(3.17)	(-0.49)
POST*RISK_DOWN	0.009	0.016	-0.001	0.060	0.012	0.047
<i>pred. sign: -</i>	(0.36)	(0.35)	(-0.02)	(1.20)	(0.32)	(0.78)
SIZE	0.386***	0.364***	0.385***	0.367***	0.385***	0.366***
	(21.57)	(14.13)	(21.64)	(14.48)	(21.72)	(14.41)
INVREC	0.621***	0.223**	0.627***	0.224**	0.614***	0.226**
	(6.12)	(2.17)	(6.15)	(2.18)	(6.04)	(2.20)
IMPAIR	0.031*	-0.015	0.031*	-0.014	0.031*	-0.014
	(1.78)	(-0.47)	(1.78)	(-0.46)	(1.78)	(-0.46)
ROA	-0.202***	-0.251***	-0.202***	-0.253***	-0.202***	-0.250***
	(-4.65)	(-6.40)	(-4.65)	(-6.48)	(-4.65)	(-6.40)
LOSS	0.037**	0.008	0.037**	0.008	0.037**	0.008
	(2.38)	(0.37)	(2.40)	(0.37)	(2.41)	(0.37)
LEV	0.093*	0.019	0.097**	0.016	0.092*	0.016
	(1.93)	(0.27)	(2.01)	(0.23)	(1.91)	(0.23)
NEG_EQ	0.112***	0.034	0.108***	0.035	0.114***	0.035
	(2.73)	(0.54)	(2.65)	(0.56)	(2.77)	(0.56)
MTB	0.004***	-0.001	0.004***	-0.000	0.004***	-0.001

Table 2.7: Tellabs case: BIG4 vs non-BIG4 Audit Fees (continued)

<i>Variable</i>	Honigsberg et al.		Choi et al.		Combination	
	BIG4	non-BIG4	BIG4	non-BIG4	BIG4	non-BIG4
	(3.10)	(-0.31)	(3.02)	(-0.25)	(3.12)	(-0.28)
ACQUIS	0.040***	0.021	0.040***	0.022	0.040***	0.021
	(2.90)	(0.74)	(2.91)	(0.77)	(2.88)	(0.76)
FOREIGN	0.019	0.033	0.019	0.032	0.019	0.033
	(1.43)	(1.07)	(1.45)	(1.06)	(1.40)	(1.08)
WEAK	0.197***	0.197***	0.196***	0.198***	0.197***	0.197***
	(9.11)	(7.37)	(9.11)	(7.39)	(9.13)	(7.37)
BUSY	0.354***	-0.006	0.355***	-0.005	0.357***	-0.007
	(3.95)	(-0.04)	(3.95)	(-0.03)	(4.01)	(-0.05)
OPINION	0.037***	0.063***	0.036***	0.064***	0.036***	0.063***
	(3.75)	(3.52)	(3.70)	(3.57)	(3.70)	(3.56)
IND_SPEC	0.035**	-0.119	0.036**	-0.123	0.036**	-0.123
	(2.09)	(-0.29)	(2.15)	(-0.30)	(2.12)	(-0.30)
AUD_NEW	-0.041	-0.095***	-0.040	-0.094***	-0.042	-0.095***
	(-1.50)	(-4.94)	(-1.48)	(-4.91)	(-1.53)	(-4.94)
<i>Const.</i>	5.452***	5.642***	5.467***	5.595***	5.463***	5.615***
	(14.73)	(11.59)	(14.86)	(11.73)	(14.89)	(11.74)
<i>Firm FE</i>	Yes	Yes	Yes	Yes	Yes	Yes
<i>Year FE</i>	Yes	Yes	Yes	Yes	Yes	Yes
Obs	15 005	49 77	15 005	4 977	15 005	4 977
R-sq.	0.885	0.847	0.884	0.847	0.885	0.847

Note: The Table reports the results of fixed effects regressions estimating the effects of the Tellabs ruling on audit fees separately for Big 4 and non-Big 4 client sub-samples. The dependent variable is *AUD_FEE*, the natural logarithm of audit fees. Columns 1 and 2 use the circuit classification of Honigsberg et al. (2020), Columns 3 and 4 use the classification of Choi and Pritchard (2012), and Columns 5 and 6 apply the combined classification in which a circuit is treated as affected only if both approaches identify it as such. All regressions include firm and year fixed effects, and standard errors are clustered at the firm level. *t*-statistics are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 2.8 reports the results for the analysis of the impact of Janus case ruling on the audit fees. Columns 1 & 2 display the results for the full sample, while the Columns 3 & 4 report results for the subsamples of Big 4 and non Big 4 engagements separately. In this case there is only one treatment group which consists of the companies headquartered in the Fourth and Ninth circuits where the auditors litigation risk went down due to the Janus ruling. Essentially, the ruling prohibited circuit level courts to claim auditors guilty for the misreporting found in the clients' reports and limited the responsibility of the former to the fairness and truthfulness of the signed audit report. The *POST*RISK_DOWN* coefficient is negative and statistically significant throughout the Table which suggests

that auditors seem to have reacted to the legislative easing by lowering their fees. In terms of economic significance, the results suggests that audit fees decreased by approximately 6.9% for the overall sample following the ruling. Splitting the sample into Big 4 and non-Big 4 clients shows that the effect was more pronounced for non-Big 4 clients: while Big 4 clients experienced an estimated 6.0% reduction in fees, non-Big 4 clients saw a decline of approximately 11.6%.

Table 2.8: Janus case: Audit Fees

<i>Variable</i>	Full Sample	Full Sample	BIG4	non-BIG4
POST*RISK.DOWN	-0.074***	-0.072***	-0.062***	-0.123***
<i>pred. sign: -</i>	(-3.58)	(-4.07)	(-3.11)	(-3.48)
SIZE		0.336***	0.326***	0.309***
		(21.70)	(17.40)	(13.04)
INVREC		0.394***	0.461***	0.164*
		(5.66)	(5.26)	(1.78)
IMPAIR		0.032***	0.030***	0.030
		(3.44)	(3.12)	(1.43)
ROA		-0.186***	-0.161***	-0.192***
		(-7.37)	(-5.42)	(-5.73)
LOSS		0.028***	0.027**	0.028*
		(2.81)	(2.52)	(1.75)
LEV		0.027	0.062	-0.064
		(0.71)	(1.44)	(-0.93)
NEG_EQ		-0.023	-0.023	0.023
		(-0.72)	(-0.72)	(0.42)
MTB		-0.000	-0.000	-0.002
		(-0.13)	(-0.13)	(-1.38)
ACQUIS		0.052***	0.066***	0.028
		(5.18)	(6.08)	(1.33)
FOREIGN		0.014	0.011	0.023
		(1.31)	(0.99)	(0.92)
WEAK		0.144***	0.167***	0.121***
		(9.80)	(9.68)	(4.96)
BUSY		0.116	0.084	0.251*
		(1.34)	(0.75)	(1.69)
BIG4		0.260***		
		(5.66)		
OPINION		0.047***	0.045***	0.047***
		(5.73)	(5.21)	(2.61)
IND_SPEC		0.003	0.003	
		(0.20)	(0.20)	
AUD_NEW		-0.095***	-0.159***	-0.076***
		(-6.84)	(-6.87)	(-4.42)
<i>Const.</i>	13.740***	6.589***	7.219***	6.685***
	(4696.26)	(21.47)	(19.54)	(15.04)
<i>Firm FE</i>	Yes	Yes	Yes	Yes
<i>Year FE</i>	Yes	Yes	Yes	Yes
Obs	20 567	20 567	14 600	5 967
R-sq.	0.920	0.933	0.922	0.879

Note: The Table reports the results of fixed effects regressions estimating the effects of the Janus ruling on audit fees. The dependent variable is *AUD_FEE*, the natural logarithm of audit fees. Columns 1–2 present results for the full sample, while Columns 3–4 report separate regressions for Big 4 and non-Big 4 client sub-samples. All regressions include firm and year fixed effects, and standard errors are clustered at the firm level. *t*-statistics are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

To rule out the possibility that the 2007 Tellabs ruling — which increased auditors’ litigation risk in the 4th and 9th Circuits — biases our identification of the Janus effect, we re-estimate our models after excluding the year 2007. As shown in Table 2.9, the results remain consistent in both magnitude and significance. This suggests that our findings are not driven by the Tellabs year and that the Janus effects we document are robust to this exclusion.

Table 2.9: Janus case: Audit Fees, year 2007 excluded

<i>Variable</i>	Full Sample	Full Sample	BIG4	non-BIG4
POST*RISK_DOWN	-0.074***	-0.074***	-0.059***	-0.117***
<i>pred. sign: -</i>	(-3.65)	(-4.16)	(-2.93)	(-3.56)
SIZE		0.329***	0.321***	0.292***
		(19.41)	(15.59)	(11.43)
INVREC		0.337***	0.461***	0.091
		(4.49)	(4.80)	(0.96)
IMPAIR		0.037***	0.031***	0.041*
		(3.78)	(3.01)	(1.92)
ROA		-0.172***	-0.144***	-0.169***
		(-6.28)	(-4.46)	(-4.73)
LOSS		0.031***	0.034***	0.021
		(3.05)	(3.05)	(1.31)
LEV		0.006	0.063	-0.101
		(0.14)	(1.32)	(-1.45)
NEG_EQ		0.011	-0.037	0.111**
		(0.35)	(-1.05)	(2.08)
MTB		0.000	-0.000	0.001
		(0.02)	(-0.46)	(0.41)
ACQUIS		0.057***	0.076***	0.016
		(5.45)	(6.89)	(0.71)
FOREIGN		0.024**	0.022**	0.030
		(2.24)	(2.06)	(1.13)
WEAK		0.135***	0.155***	0.114***
		(8.36)	(8.31)	(4.09)
BUSY		0.131	0.071	0.342**
		(1.43)	(0.62)	(2.14)
BIG4		0.287***		
		(5.77)		
OPINION		0.040***	0.043***	0.045**
		(4.55)	(4.71)	(2.19)
IND_SPEC		0.012	0.020	
		(0.64)	(1.16)	

Table 2.9: Janus case: Audit Fees, year 2007 excluded (continued)

<i>Variable</i>	Full Sample	Full Sample	BIG4	non-BIG4
AUD_NEW		-0.082*** (-5.45)	-0.137*** (-5.41)	-0.068*** (-3.78)
<i>Const.</i>	13.760*** (4109.97)	6.715*** (20.15)	7.321*** (17.95)	6.946*** (14.32)
<i>Firm FE</i>	Yes	Yes	Yes	Yes
<i>Year FE</i>	Yes	Yes	Yes	Yes
Obs	17 586	17 586	12 519	5 067
R-sq.	0.925	0.938	0.925	0.888

Note: The Table reports the results of fixed effects regressions estimating the effects of the Janus ruling on audit fees over a sample period that excludes the year 2007. All other elements follow Table 2.8, including variable definitions, fixed effects, and inference conventions.

The results of the analysis of audit pricing around Tellabs and Janus ruling provide some support for the Hypothesis 1. There empirical findings suggest that the U.S. auditors do adapt the fees they charge to the levels of litigation risk level associated with each particular engagement. However, usage of such price adjustment strategy seems to vary depending on the context. The results indicate that post Tellabs litigation risk changes provoked an increase of audit fees in the previously "pro-auditors" circuits where the litigation risk of the latter went up as the regional level courts had to adjust to the Supreme Court's decision. At the same time, there is no evidence of a corresponding decrease in audit pricing observed in the previously "pro-shareholders" circuits which had to become more loyal to the auditors after the ruling was announced. In the case of Janus ruling instead, there is some clear evidence that the auditor did revise their fees downward in the circuits where they experienced legislation risk reduction. One possible explanation of such difference in the results could be the extent to which these two ruling have changed the levels of the litigation risk faced by auditors. Whereas in the Tellabs case the Supreme Court's ruling led to reassessment of the evidence considered sufficient to establish auditors' scienter, in the Janus case the ruling automatically prohibited claiming auditors guilty for the misreporting found in the clients' reports.

Further, we examine whether there is evidence that auditors adopt alternative strategies in response to shifts in the litigation risk they face. Specifically, we investigate whether such shifts prompt changes in auditors' reporting conservatism and audit quality. We

start the analysis with assessing whether litigation risk influences auditors' decisions to issue modified audit and going concern opinions. Table 2.10 reports the percentage of companies receiving MAO opinions each year by circuit in the sample. The table reveals several common trends in MAO rates across circuits. Most notably, there is a general increase in MAO frequencies between 2003 and 2007, which likely reflects a growing incidence of reporting issues in the years preceding the financial crisis. This trend is followed by a plateau or spike around 2007–2008 — coinciding with the peak of market uncertainty — and then a gradual decline in most circuits through to 2010. For the Tellabs sample (Panel A), the bolded Circuits 3 and 9 represent those where auditors faced the most radical (i.e., recognized by both classifications) changes in litigation risk after the 2007 ruling. The Ninth Circuit experienced one of the most pronounced increases in the rate of modified audit opinions in 2007, immediately following the Tellabs ruling — rising by 6.5 percentage points. At first glance, this provides some initial support for the hypothesis that auditors responded to increased litigation risk by becoming more conservative in their reporting. However, in the following years (2008–2010), the MAO rate also declined significantly, with cumulative drops totaling nearly 40 percentage points, thereby questioning the hypothesis. Circuit 3, where litigation risk decreased, shows no distinct pattern that sets it apart from other circuits. For the Janus sample (Panel B), Circuits 4 and 9 — where auditors' litigation risk declined following the Supreme Court's 2011 ruling — do not exhibit MAO trends notably different from those in other circuits, which also provides no initial support for Hypothesis 2.

Table 2.10: Modified Audit Opinion Rates

Panel A: Tellabs sample											
Year/Circuit	DC	1	2	3	4	5	6	7	9	10	11
12											
2003	0.636	0.471	0.482	0.588	0.537	0.639	0.690	0.615	0.503	0.530	0.583
2004	0.417	0.225	0.280	0.308	0.356	0.442	0.427	0.414	0.294	0.336	0.347
2005	0.154	0.186	0.183	0.208	0.276	0.331	0.257	0.235	0.201	0.299	0.159
2006	0.615	0.770	0.679	0.703	0.634	0.731	0.700	0.760	0.736	0.591	0.636
2007	0.833	0.850	0.726	0.745	0.723	0.739	0.777	0.780	0.801	0.507	0.645
2008	0.727	0.617	0.563	0.608	0.580	0.539	0.714	0.615	0.618	0.376	0.537
2009	0.444	0.415	0.434	0.430	0.424	0.465	0.577	0.390	0.434	0.357	0.360
2010	0.286	0.227	0.255	0.252	0.207	0.302	0.277	0.202	0.236	0.237	0.205

Table 2.10: Modified Audit Opinion Rates (continued)

Panel B: Janus sample											
Year/Circuit 12	DC	1	2	3	4	5	6	7	9	10	11
2007	0.833	0.850	0.726	0.745	0.723	0.739	0.777	0.780	0.801	0.507	0.645
2008	0.727	0.617	0.563	0.608	0.580	0.539	0.714	0.615	0.618	0.376	0.537
2009	0.444	0.415	0.434	0.430	0.424	0.465	0.577	0.390	0.434	0.357	0.360
2010	0.286	0.227	0.255	0.252	0.207	0.302	0.277	0.202	0.236	0.237	0.205
2011	0.000	0.142	0.104	0.103	0.154	0.201	0.140	0.160	0.164	0.169	0.112
2012	0.000	0.111	0.106	0.096	0.140	0.124	0.072	0.078	0.138	0.104	0.081
2013	0.000	0.088	0.064	0.059	0.104	0.080	0.050	0.055	0.096	0.083	0.066
2014	0.000	0.074	0.082	0.065	0.097	0.081	0.068	0.076	0.110	0.095	0.070
2015	0.000	0.060	0.067	0.055	0.090	0.086	0.072	0.079	0.118	0.102	0.091

Next we move to the results of multivariate analysis. Table [2.11](#) reports results of the analysis aimed to investigate whether auditors reacted to circuit level litigation risk shifts by changing their MAO reporting decisions in line with the stated hypotheses. Panel A presents coefficients of the logit specification which is more suited for this analysis as the modeled outcome variable, MAO, is binary. As an additional analysis, Panel B presents estimates from a fixed effects OLS model, which enables testing the hypotheses using the full sample of companies (in the logit model with firm fixed effects, only companies experiencing a change in audit opinion during the sample period are included in the regression).

Overall, there is some evidence of increased auditor reporting conservatism in circuits where litigation risk went up: the coefficient on the $POST * RISK_UP$ interaction term is consistently positive and statistically significant in most logit specifications. The OLS estimates show slightly weaker significance, but the direction and magnitude of the effect are still consistent across models, providing additional support to the hypothesis. However, the economic significance is less straightforward. The fixed-effects logit models yield marginal effects that are small in absolute terms (less than 0.05 percentage points), likely reflecting the model's nonlinear structure and reliance on within-firm variation. In contrast, the OLS estimates suggest a more economically meaningful effect, with the likelihood of a modified opinion increasing by approximately 4 to 7 percentage points.

By contrast, and consistent with the audit fees analysis, there is no evidence that auditors responded to reductions in litigation risk in certain circuits: the *POST*RISK_DOWN* coefficient remains positive throughout, contrary to the hypothesized negative sign. This effect even reaches statistical significance under the circuit classification proposed by Honigsberg et al. (2020), suggesting that in some circuits where the Tellabs ruling reduced auditors' litigation exposure, auditors still became more conservative in their reporting choices.

Table 2.11: Tellabs case: Modified Audit Opinions

Panel A: Logit Specifications

<i>Variable</i>	Honigsberg et al.		Choi et al.		Combination	
POST*RISK_UP	0.173*	0.124	0.390***	0.354***	0.274***	0.195**
<i>pred. sign: +</i>	(1.90)	(1.33)	(4.49)	(3.99)	(2.90)	(2.01)
POST*RISK_DOWN	0.221**	0.226**	0.048	0.077	0.134	0.137
<i>pred. sign: -</i>	(2.08)	(2.09)	(0.40)	(0.63)	(0.94)	(0.95)
SIZE		0.247***		0.244***		0.243***
		(4.07)		(4.02)		(4.01)
LEV		-0.352*		-0.344*		-0.352*
		(-1.71)		(-1.67)		(-1.71)
ROA		-0.623***		-0.618***		-0.622***
		(-3.89)		(-3.87)		(-3.89)
MTB		0.002		0.002		0.002
		(0.48)		(0.40)		(0.44)
Z_SCORE		-0.045***		-0.045***		-0.045***
		(-8.59)		(-8.47)		(-8.51)
OCF		-0.299		-0.297		-0.298
		(-1.40)		(-1.39)		(-1.40)
LOSS		0.031		0.040		0.033
		(0.49)		(0.62)		(0.52)
RETURN		0.034		0.035		0.034
		(1.50)		(1.52)		(1.51)
VOL		-0.014		-0.012		-0.013
		(-0.23)		(-0.20)		(-0.21)
WEAK		0.356***		0.359***		0.357***
		(4.68)		(4.72)		(4.69)
AUD_FEE		0.261***		0.260***		0.259***
		(5.36)		(5.34)		(5.31)
BIG4		0.500***		0.490***		0.501***
		(4.37)		(4.28)		(4.38)
IND_SPEC		-0.080		-0.074		-0.082
		(-1.07)		(-0.98)		(-1.09)
AUD_NEW		-0.237**		-0.243**		-0.237**
		(-2.50)		(-2.57)		(-2.51)
TENURE		-0.018		-0.020		-0.019
		(-1.01)		(-1.15)		(-1.08)
<i>Firm FE</i>	Yes	Yes	Yes	Yes	Yes	Yes
<i>Year FE</i>	Yes	Yes	Yes	Yes	Yes	Yes
Obs	15 351	15 351	15 351	15 351	15 351	15 351
Pseudo R-sq.	0.206	0.230	0.207	0.231	0.206	0.230

Table 2.11: Tellabs case: Modified Audit Opinions (continued)

Panel B: OLS Specifications

<i>Variable</i>	Honigsberg et al.		Choi et al.		Combination	
POST*RISK_UP	0.026	0.019	0.067***	0.061***	0.047**	0.034
<i>pred. sign: +</i>	(1.31)	(0.95)	(3.52)	(3.25)	(2.20)	(1.61)
POST*RISK_DOWN	0.037	0.039*	0.006	0.012	0.019	0.022
<i>pred. sign: -</i>	(1.61)	(1.73)	(0.23)	(0.49)	(0.63)	(0.74)
<i>Controls</i>	No	Yes	No	Yes	No	Yes
<i>Firm FE</i>	Yes	Yes	Yes	Yes	Yes	Yes
<i>Year FE</i>	Yes	Yes	Yes	Yes	Yes	Yes
Obs	19 301	19 301	19 301	19 301	19 301	19 301
R-sq.	0.445	0.456	0.446	0.456	0.445	0.456

Note: The Table reports the results of the analysis estimating the effects of the Tellabs ruling on the likelihood of issuing a modified audit opinion. The dependent variable is *MAO_OPINION*, an indicator variable equal to 1 if the audit opinion is modified and 0 otherwise. Panel A presents fixed effects logit specifications, while Panel B reports the corresponding OLS estimates. All models include firm and year fixed effects, and standard errors are clustered at the firm level. Test statistics are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A of Table [2.12](#) presents coefficients from logistic regressions estimated separately for the Big 4 and non-Big 4 auditor subsamples. Consistent with the idea that Big 4 firms are more sensitive to litigation risk, the *POST * RISK_UP* coefficients remain significant only for the Big 4 group. In contradiction to any hypothesis, also the coefficients of *POST*RISK_DOWN* are positive and statistically significant for the Big 4 subsample (the results are especially strong for companies headquartered in the Second and Third Circuits) — Big 4 auditors seem to have become more conservative in their reporting choices even with respect to clients located in circuits where their litigation risk decreased after the Tellabs decision. Interestingly, for the non-Big 4 auditors, the *POST*RISK_DOWN* coefficient is statistically significant at the 5% level and has the predicted negative sign when the Choi et al. (2012) circuit classification is applied, indicating that smaller audit firms may have adjusted their reporting in line with reduced litigation exposure. Panel B of the Table reports coefficients from an OLS regression which generally confirm the main results, although the statistical significance becomes weaker.

Table 2.12: Tellabs case: Big 4 vs non-Big 4 Modified Audit Opinions

Panel A: Logit Specification

<i>Variable</i>	Honigsberg et al.		Choi et al.		Combination	
	BIG4	non-BIG4	BIG4	non-BIG4	BIG4	non-BIG4
POST*RISK_UP	0.167	0.096	0.453***	0.021	0.222*	0.114
<i>pred. sign: +</i>	(1.50)	(0.45)	(4.26)	(0.10)	(1.91)	(0.51)
POST*RISK_DOWN	0.362***	0.105	0.261*	-0.574**	0.341*	-0.490
<i>pred. sign: -</i>	(2.75)	(0.45)	(1.82)	(-2.04)	(1.95)	(-1.57)
SIZE	0.317***	0.005	0.311***	-0.003	0.310***	0.002
	(4.16)	(0.04)	(4.08)	(-0.03)	(4.07)	(0.01)
LEV	-0.372	-0.744*	-0.358	-0.751*	-0.369	-0.730*
	(-1.47)	(-1.84)	(-1.41)	(-1.84)	(-1.46)	(-1.80)
ROA	-0.629***	-0.408*	-0.627***	-0.386	-0.627***	-0.404
	(-2.79)	(-1.65)	(-2.79)	(-1.56)	(-2.79)	(-1.63)
MTB	0.004	-0.001	0.004	-0.002	0.004	-0.001
	(0.66)	(-0.06)	(0.57)	(-0.16)	(0.58)	(-0.09)
Z_SCORE	-0.050***	-0.040***	-0.048***	-0.039***	-0.049***	-0.039***
	(-6.84)	(-4.61)	(-6.71)	(-4.52)	(-6.75)	(-4.50)
OCF	-0.455	-0.165	-0.444	-0.169	-0.448	-0.177
	(-1.58)	(-0.47)	(-1.55)	(-0.48)	(-1.56)	(-0.51)
LOSS	-0.015	0.206	-0.007	0.206	-0.012	0.206
	(-0.19)	(1.55)	(-0.08)	(1.55)	(-0.16)	(1.55)
RETURN	0.031	0.066	0.030	0.067	0.031	0.066
	(1.10)	(1.50)	(1.08)	(1.53)	(1.08)	(1.50)
VOL	-0.031	-0.033	-0.025	-0.037	-0.027	-0.035
	(-0.39)	(-0.31)	(-0.31)	(-0.35)	(-0.34)	(-0.33)
WEAK	0.299***	0.280*	0.301***	0.273*	0.300***	0.278*
	(3.08)	(1.95)	(3.09)	(1.90)	(3.09)	(1.94)
AUD_FEE	0.271***	0.502***	0.271***	0.506***	0.268***	0.506***
	(4.54)	(4.18)	(4.55)	(4.19)	(4.50)	(4.19)
IND_SPEC	-0.094	0.536	-0.091	0.478	-0.098	0.494
	(-1.14)	(0.24)	(-1.10)	(0.21)	(-1.19)	(0.22)
AUD_NEW	-0.490***	-0.292**	-0.491***	-0.298**	-0.496***	-0.288**
	(-3.27)	(-2.03)	(-3.27)	(-2.07)	(-3.31)	(-2.00)
TENURE	0.019	-0.063*	0.018	-0.072*	0.018	-0.070*
	(0.74)	(-1.69)	(0.70)	(-1.91)	(0.71)	(-1.85)
<i>Firm FE</i>	Yes	Yes	Yes	Yes	Yes	Yes
<i>Year FE</i>	Yes	Yes	Yes	Yes	Yes	Yes
Obs	11 449	3 066	11 449	3 066	11 449	3 066
Pseudo R-sq.	0.261	0.189	0.262	0.191	0.261	0.190

Panel B: OLS Specification

<i>Variable</i>	Honigsberg et al.		Choi et al.		Combination	
	BIG4	non-BIG4	BIG4	non-BIG4	BIG4	non-BIG4
POST*RISK_UP	0.028	0.016	0.078**	0.003	0.039	0.021
<i>pred. sign: +</i>	(1.18)	(0.39)	(3.52)	(0.07)	(1.57)	(0.45)
POST*RISK_DOWN	0.063**	0.022	0.041	-0.089	0.059	-0.079
<i>pred. sign: -</i>	(2.34)	(0.49)	(1.36)	(-1.76)	(1.60)	(-1.36)
<i>Controls</i>	Yes	Yes	Yes	Yes	Yes	Yes
<i>Firm FE</i>	Yes	Yes	Yes	Yes	Yes	Yes
<i>Year FE</i>	Yes	Yes	Yes	Yes	Yes	Yes
Obs	19 301	19 301	19 301	19 301	19 301	19 301
R-sq.	0.476	0.486	0.476	0.486	0.475	0.486

Note: The Table reports the results of the analysis estimating the effects of the Tellabs ruling on the likelihood of issuing a modified audit opinion, separately for Big 4 and non-Big 4 auditor subsamples. The dependent variable is *MAO.OPINION*, an indicator variable equal to 1 if the audit opinion is modified and 0 otherwise. Panel A presents fixed effects logit specifications, while Panel B reports the corresponding OLS estimates. All models include firm and year fixed effects, and standard errors are clustered at the firm level. Test statistics are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 2.13 presents the results of the analysis of MAO reporting following the announcement of the Janus decision. Contrary to the stated hypothesis, the *POST*RISK_DOWN* coefficient is positive and marginally statistically significant at the 10% level. If anything, auditors appear to have become more conservative toward clients headquartered in the Fourth and Ninth Circuits, despite the Supreme Court ruling making it more difficult to hold auditors liable as defendants.

Table 2.13: Janus case: Modified Audit Opinion

Panel A: Logit Specification

<i>Variable</i>	Full Sample	Full Sample	BIG4	non-BIG4
POST*RISK_DOWN	0.232*	0.245*	0.292*	0.218
<i>pred. sign: -</i>	(1.89)	(1.94)	(1.93)	(0.84)
SIZE		-0.223***	-0.279**	-0.260*
		(-2.66)	(-2.48)	(-1.85)
LEV		-0.505**	-0.730**	-0.449
		(-2.00)	(-2.20)	(-1.04)
ROA		-0.720***	-0.742***	-0.872***
		(-4.20)	(-2.87)	(-3.54)
MTB		0.005	0.009	-0.006
		(0.90)	(1.34)	(-0.54)
Z_SCORE		-0.052***	-0.044***	-0.040***
		(-7.71)	(-4.46)	(-4.28)
OCF		-0.114	-0.299	0.184
		(-0.46)	(-0.83)	(0.50)
LOSS		-0.054	-0.118	-0.067
		(-0.69)	(-1.20)	(-0.48)
RETURN		-0.078***	-0.050	-0.101*
		(-2.61)	(-1.28)	(-1.92)
VOL		0.318***	0.219**	0.345***
		(4.26)	(2.07)	(3.05)
WEAK		0.462***	0.376***	0.555***
		(4.81)	(2.86)	(3.63)
AUD_FEE		0.361***	0.587***	0.330**
		(4.63)	(4.97)	(2.45)
BIG4		0.115		
		(0.64)		

Table 2.13: Janus case: Modified Audit Opinion (continued)

IND_SPEC		0.276***	0.313***	
		(2.91)	(3.00)	
AUD_NEW		-0.479***	-0.445**	-0.179
		(-4.06)	(-2.14)	(-1.11)
TENURE		-0.059***	0.012	-0.039
		(-3.95)	(0.48)	(-1.25)
<i>Firm FE</i>	Yes	Yes	Yes	Yes
<i>Year FE</i>	Yes	Yes	Yes	Yes
Obs.	14 579	14 579	10 808	3 268
Pseudo R-sq.	0.414	0.440	0.497	0.283

Panel B: OLS Specifications

<i>Variable</i>	Full Sample	Full Sample	BIG4	non-BIG4
POST*RISK_DOWN	0.016	0.020	0.041**	-0.013
<i>pred. sign: -</i>	(0.90)	(1.14)	(2.08)	(-0.41)
<i>Controls</i>	Yes	Yes	Yes	Yes
<i>Firm FE</i>	Yes	Yes	Yes	Yes
<i>Year FE</i>	Yes	Yes	Yes	Yes
Obs.	19 645	19 645	13 888	5 757
R-sq.	0.533	0.544	0.577	0.525

Note: The Table reports the results of the analysis estimating the effects of the Janus ruling on the likelihood of issuing a modified audit opinion. The dependent variable is *MAO_OPINION*, an indicator equal to 1 if the audit opinion is modified and 0 otherwise. Panel A presents fixed effects logit specifications, while Panel B reports the corresponding OLS estimates. All models include firm and year fixed effects, and standard errors are clustered at the firm level. Test statistics are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

We conclude the discussion of changes in auditors' reporting conservatism following shifts in litigation risk prompted by the Supreme Court's decision by presenting results related to GC opinions. Consistent with prior literature, the hypotheses are tested on a subsample of financially distressed firms, for which a GC opinion is of particular importance. For the Tellabs case, in this case, the sample size decreases to 7 918 observations for the OLS specifications and 1 339 for the panel logit model with firm and year fixed effects. The regression results are presented in Table 2.14. The coefficient on the interaction term *POST * RISK_UP* does not exhibit a stable sign across specifications and is never statistically significant. In contrast, the *POST * RISK_DOWN* coefficient is consistently positive — contrary to the hypothesis — and in some specifications approaches marginal statistical significance at the 10% level. Splitting the sample into Big 4 and non-Big 4 auditors also does not provide consistent support for the hypothesis that auditors became

more (less) conservative in issuing GC opinions to companies headquartered in circuits where litigation risk increased (decreased).

Table 2.14: Tellabs case: GC Opinion

Panel A: Full Sample						
Variable	Logit			OLS		
	Hon. et al.	Choi et al.	Comb.	Hon. et al.	Choi et al.	Comb.
POST*RISK_UP	0.311	-0.065	0.220	-0.003	-0.002	-0.005
<i>pred. sign: +</i>	(0.78)	(-0.17)	(0.56)	(-0.22)	(-0.14)	(-0.36)
POST*RISK_DOWN	0.538	0.536	0.749	0.015	0.048*	0.040
<i>pred. sign: -</i>	(1.18)	(0.96)	(1.22)	(0.77)	(1.82)	(1.40)
SIZE	-0.668***	-0.665***	-0.666***	-0.026***	-0.025***	-0.026***
	(-3.37)	(-3.38)	(-3.37)	(-2.74)	(-2.62)	(-2.70)
LEV	-1.338***	-1.348***	-1.351***	-0.126***	-0.126***	-0.126***
	(-3.20)	(-3.22)	(-3.23)	(-3.40)	(-3.40)	(-3.40)
ROA	-0.178	-0.173	-0.185	-0.058***	-0.059***	-0.059***
	(-0.72)	(-0.70)	(-0.75)	(-2.63)	(-2.67)	(-2.65)
MTB	-0.019**	-0.019**	-0.019**	-0.001**	-0.001**	-0.001**
	(-2.19)	(-2.12)	(-2.16)	(-2.47)	(-2.44)	(-2.47)
Z_SCORE	-0.020*	-0.019*	-0.019*	-0.002***	-0.002***	-0.002***
	(-1.91)	(-1.90)	(-1.85)	(-4.06)	(-4.14)	(-4.10)
OCF	-0.356	-0.382	-0.369	-0.054*	-0.054*	-0.054*
	(-0.95)	(-1.01)	(-0.98)	(-1.82)	(-1.83)	(-1.80)
LOSS	0.868*	0.862*	0.873*	0.006	0.006	0.006
	(1.90)	(1.88)	(1.91)	(0.61)	(0.57)	(0.59)
RETURN	-0.515***	-0.519***	-0.517***	-0.015***	-0.015***	-0.015***
	(-5.08)	(-5.14)	(-5.12)	(-5.34)	(-5.34)	(-5.35)
VOL	0.175**	0.184**	0.181**	0.007	0.007	0.007
	(2.01)	(2.10)	(2.06)	(1.26)	(1.29)	(1.29)
WEAK	0.219	0.219	0.224	0.026**	0.026**	0.026**
	(0.90)	(0.91)	(0.93)	(2.21)	(2.22)	(2.23)
AUD_FEE	0.842***	0.839***	0.841***	0.031***	0.031***	0.031***
	(3.92)	(3.90)	(3.91)	(3.05)	(3.02)	(3.03)
BIG4	0.939**	0.871**	0.921**	0.042***	0.043***	0.042***
	(2.35)	(2.16)	(2.29)	(2.72)	(2.81)	(2.74)
IND_SPEC	0.065	0.061	0.070	0.009	0.009	0.009
	(0.20)	(0.18)	(0.21)	(0.69)	(0.67)	(0.69)
AUD_NEW	-0.326	-0.332	-0.330	-0.009	-0.008	-0.009
	(-1.17)	(-1.19)	(-1.19)	(-0.64)	(-0.61)	(-0.65)
TENURE	0.062	0.054	0.054	0.000	0.000	0.000
	(0.96)	(0.84)	(0.85)	(0.14)	(0.12)	(0.10)
<i>Firm FE</i>	Yes	Yes	Yes	Yes	Yes	Yes
<i>Year FE</i>	Yes	Yes	Yes	Yes	Yes	Yes
Obs	1 339	1 339	1 339	7 918	7 918	7 918
Pseudo R-sq. & R-sq.	0.287	0.287	0.288	0.448	0.448	0.448

Panel B: BIG4 Sample						
Variable	Logit			OLS		
	Hon. et al.	Choi et al.	Comb.	Hon. et al.	Choi et al.	Comb.
POST*RISK_UP	1.062	0.614	1.384*	-0.011	0.006	-0.010
<i>pred. sign: +</i>	(1.62)	(0.99)	(1.93)	(-0.60)	(0.29)	(-0.58)
POST*RISK_DOWN	1.263	1.127	1.605	0.030	0.088**	0.071
<i>pred. sign: -</i>	(1.46)	(1.22)	(1.41)	(0.97)	(2.15)	(1.64)
<i>Controls</i>	Yes	Yes	Yes	Yes	Yes	Yes
<i>Firm FE</i>	Yes	Yes	Yes	Yes	Yes	Yes
<i>Year FE</i>	Yes	Yes	Yes	Yes	Yes	Yes

Table 2.14: Tellabs case: GC Opinion (continued)

Obs	650	650	650	5 227	5 227	5 227
Pseudo R-sq. & R-sq.	0.410	0.405	0.414	0.496	0.498	0.497
Panel C: non-BIG4 Sample						
	Logit			OLS		
Variable	Hon. et al.	Choi et al.	Comb.	Hon. et al.	Choi et al.	Comb.
POST*RISK_UP	-1.165*	-0.884	-1.397**	-0.017	-0.016	-0.018
<i>pred. sign: +</i>	(-1.72)	(-1.44)	(-2.23)	(-0.48)	(-0.51)	(-0.50)
POST*RISK_DOWN	-0.249	-0.725	-0.793	-0.009	-0.040	-0.036
<i>pred. sign: -</i>	(-0.31)	(-0.86)	(-0.88)	(-0.29)	(-1.00)	(-0.82)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs	567	567	567	2 691	2 691	2 691
Pseudo R-sq. & R-sq.	0.254	0.251	0.258	0.475	0.475	0.475

Note: The Table reports the results of the analysis estimating the effects of the Tellabs ruling on the likelihood of issuing a GC opinion. The dependent variable is *GC_OPINION*, an indicator variable equal to 1 if the auditor issues a GC opinion and 0 otherwise. Panel A presents fixed effects logit and OLS specifications for the full sample of financially distressed firms, while Panels B and C report corresponding estimates for Big 4 and non-Big 4 auditor subsamples. All models include firm and year fixed effects, and standard errors are clustered at the firm level. Test statistics are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 2.15 reports results for the Janus case analysis, where the decline in litigation risk faced by auditors in some circuits was hypothesized to make them less conservative in issuing GC opinions. The hypothesis is tested on a subsample of financially distressed companies, comprising 8 149 observations for the OLS specification and 1 643 for the logit model. In most of the specifications (excluding OLS run on the Big 4 sample) the *POST*RISK_DOWN* coefficient is of predicted sign. However, the coefficient does not reach conventional levels of statistical significance in any specification.

Table 2.15: Janus case: GC Opinion

Variable	Logit			OLS		
	Full Sample	BIG4	non-BIG4	Full Sample	BIG4	non-BIG4
POST*RISK_DOWN	-0.247	-0.042	-0.255	-0.013	0.001	-0.025
<i>pred. sign: -</i>	(0.72)	(0.07)	(0.50)	(1.02)	(0.08)	(0.95)
SIZE	-0.536***	-0.481	-0.659***	-0.025**	-0.011	-0.047**
	(2.97)	(1.53)	(2.63)	(2.57)	(0.98)	(2.55)
LEV	-1.054***	-1.844***	-0.419	-0.174***	-0.213***	-0.119
	(2.99)	(3.18)	(0.84)	(4.56)	(4.54)	(1.59)
ROA	-0.818***	-0.860**	-0.787***	-0.142***	-0.132***	-0.149***

Table 2.15: Janus case: GC Opinion (continued)

	(3.96)	(2.28)	(2.95)	(6.50)	(4.45)	(4.52)
MTB	-0.005	-0.019*	0.009	-0.000	-0.001**	0.001
	(0.66)	(1.66)	(0.95)	(0.62)	(2.22)	(1.09)
Z_SCORE	-0.007	-0.009	-0.008	-0.001***	-0.002***	-0.001
	(0.82)	(0.66)	(0.74)	(3.05)	(3.60)	(1.55)
OCF	0.389	0.278	0.292	0.027	-0.020	0.058
	(1.20)	(0.48)	(0.67)	(0.88)	(0.46)	(1.27)
LOSS	1.021*	15.820	0.484	-0.002	0.004	-0.012
	(1.86)	(0.01)	(0.83)	(0.12)	(0.30)	(0.54)
RETURN	-0.241***	-0.310**	-0.162*	-0.008***	-0.007**	-0.007
	(3.07)	(2.06)	(1.73)	(2.97)	(2.37)	(1.47)
VOL	0.019	-0.100	0.060	-0.001	-0.006	0.006
	(0.29)	(0.83)	(0.70)	(0.16)	(0.72)	(0.62)
WEAK	0.279	0.282	0.041	0.017	0.026*	0.005
	(1.39)	(0.83)	(0.14)	(1.47)	(1.72)	(0.23)
AUD_FEE	0.563**	0.503	0.992***	0.032**	0.018	0.065***
	(2.57)	(1.30)	(3.06)	(2.56)	(1.20)	(2.81)
BIG4	0.213			0.018		
	(0.56)			(0.83)		
IND_SPEC	0.342	0.475		0.014	0.021*	
	(1.07)	(1.24)		(1.22)	(1.80)	
AUD_NEW	0.404*	1.474**	0.489*	0.026*	0.061**	0.031*
	(1.78)	(2.44)	(1.72)	(1.73)	(2.10)	(1.65)
TENURE	0.073*	0.114	0.114*	0.003*	0.004	0.003
	(1.82)	(1.42)	(1.67)	(1.78)	(1.62)	(0.95)
Const.				0.073	-0.020	0.080
				(0.41)	(0.08)	(0.25)
<i>Firm FE</i>	Yes	Yes	Yes	Yes	Yes	Yes
<i>Year FE</i>	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	1 643	711	817	8 149	4 924	3 225
Pseudo R-sq. & R-sq.	0.198	0.280	0.199	0.443	0.469	0.465

Note: The Table reports the results of the analysis estimating the effects of the Janus ruling on the likelihood of issuing a GC opinion. The dependent variable is *GC_OPINION*, an indicator variable equal to 1 if the auditor issues a GC opinion and 0 otherwise. Columns 1–3 present fixed effects logit specifications for the full sample of financially distressed firms, Big 4 clients, and non-Big 4 clients, respectively, while Columns 4–6 report the corresponding OLS estimates. All models include firm and year fixed effects, and standard errors are clustered at the firm level. Test statistics are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

In general, the analysis offers only modest evidence that auditors adjust their reporting conservatism in response to client-related litigation risks. The only robust and consistently statistically significant result is the increase in MAO rates following the Tellabs decision in the circuits where the decision led to an increase in auditors' litigation risk exposure. The results do not hold when the sample is restricted to financially distressed

companies and the GC opinions issued to them. In circuits where litigation risk decreased — whether following the Tellabs or Janus decisions — there is no evidence of less stringent reporting afterward. Furthermore, contrary to predicted, the results often show a reduction in restatement rates among clients headquartered in circuits where auditors’ litigation risk increased.

Lastly, we present the findings from the analysis investigating the impact of changes in audit litigation risk on audit quality. As discussed in the hypothesis development section, the key variable examined is the incidence of material restatements, which reflect significant financial reporting failures and therefore serve as a straightforward indicator of poor audit quality. However, because erroneous financial statements result from decisions and negotiations between both clients and auditors, they are not entirely under the auditors’ control. Hence, the (absence of) changes in restatements does not solely reflect the (absence of) changes in audit reporting quality. To offer a more comprehensive perspective and isolate the audit quality dimension, we also examine results using alternative audit quality measures.

Table 2.16 reports the percentage of companies restating their financial reports each year, by judicial circuit. At first glance, the patterns offer limited support for the hypothesis. Panel A shows a slight increase in restatement rates in the Third Circuit — where auditor litigation risk declined following the Tellabs ruling (under both classifications) — and a slight decrease in the Ninth Circuit, where litigation risk increased. However, these shifts appear modest and do not notably distinguish the affected circuits from those where auditor litigation risk remained unchanged. The two circuits in which the Janus ruling reduced auditors’ litigation risk — the Fourth and Ninth — experienced a decline in restatement rates, contrary to what the hypothesis would predict.

Table 2.16: Restatement rates

Panel A: Tellabs sample											
Year/Circuit	DC	1	2	3	4	5	6	7	9	10	11
2003	0,273	0,157	0,179	0,183	0,224	0,184	0,131	0,189	0,199	0,190	0,211
2004	0,182	0,140	0,158	0,157	0,168	0,160	0,143	0,156	0,203	0,167	0,157
2005	0,000	0,135	0,127	0,099	0,131	0,087	0,065	0,130	0,164	0,118	0,131
2006	0,000	0,068	0,092	0,068	0,149	0,075	0,102	0,081	0,119	0,128	0,083

Table 2.16: Restatement rates (continued)

2007	0,000	0,066	0,065	0,051	0,110	0,080	0,065	0,122	0,092	0,114	0,062
2008	0,200	0,071	0,055	0,055	0,098	0,086	0,086	0,140	0,039	0,089	0,128
2009	0,111	0,088	0,064	0,060	0,110	0,065	0,091	0,155	0,039	0,083	0,114
2010	0,143	0,107	0,065	0,086	0,119	0,092	0,068	0,132	0,083	0,129	0,110

Panel B: Janus sample

Year/Circuit	DC	1	2	3	4	5	7	8	9	10	11
2007	0,000	0,066	0,065	0,051	0,110	0,080	0,122	0,051	0,092	0,114	0,062
2008	0,200	0,071	0,055	0,055	0,098	0,086	0,136	0,039	0,089	0,128	0,059
2009	0,111	0,088	0,064	0,060	0,110	0,065	0,155	0,039	0,083	0,114	0,109
2010	0,143	0,107	0,065	0,086	0,119	0,092	0,132	0,083	0,129	0,165	0,110
2011	0,125	0,149	0,094	0,112	0,173	0,095	0,138	0,108	0,129	0,169	0,124
2012	0,333	0,130	0,082	0,143	0,107	0,095	0,109	0,127	0,114	0,148	0,149
2013	0,222	0,052	0,059	0,135	0,073	0,083	0,066	0,116	0,094	0,119	0,124
2014	0,000	0,022	0,028	0,049	0,059	0,040	0,045	0,059	0,036	0,013	0,067
2015	0,000	0,000	0,000	0,000	0,013	0,000	0,006	0,008	0,000	0,006	0,000

Table 2.17 presents the results of the regression analysis examining the effect of the Tellabs case on restatement rates. The coefficient on *POST*RISK_UP* is negative across all specifications, consistent with the stated hypothesis: audit quality improved in circuits where auditors' litigation risk increased following the Supreme Court's decision announcement. However, the statistical significance of the effect is marginal, reaching at most the 10% level in some logit specifications. Moreover, the economic significance is negligible, with the estimated reduction in restatement probability amounting to no more than 0.002 percentage points (for the logit specifications). The estimated coefficients on *POST*RISK_DOWN* yield mixed support for the hypothesized positive effect. Under the circuit classification from Choi et al. (2012), the coefficient is positive and marginally significant at the 10% level, consistent with expectations. However, when using the classification from Honigsberg et al. (2020), the coefficient becomes negative and is statistically significant — at the 1% level in the logit models and 5% in the OLS specifications — contradicting the prediction.

Table 2.17: Tellabs case: Restatements

Panel A: Logit Specifications

Variable	Honigsberg et al.	Choi et al.	Combination
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Table 2.17: Tellabs case: Restatements (continued)

POST*RISK_UP	-0.257*	-0.238	-0.094	-0.087	-0.258*	-0.260*
<i>pred. sign: -</i>	(-1.76)	(-1.61)	(-0.66)	(-0.60)	(-1.72)	(-1.70)
POST*RISK_DOWN	-0.573***	-0.499***	0.268	0.326*	-0.248	-0.189
<i>pred. sign: +</i>	(-3.12)	(-2.66)	(1.41)	(1.67)	(-1.03)	(-0.76)
SIZE		0.596***		0.623***		0.611***
		(6.41)		(6.69)		(6.57)
LEV		1.246***		1.243***		1.249***
		(3.70)		(3.68)		(3.70)
ROA		0.077		0.038		0.059
		(0.36)		(0.18)		(0.28)
MTB		-0.003		-0.003		-0.003
		(-0.35)		(-0.39)		(-0.39)
SALE_GROWTH		0.021		0.025		0.022
		(0.30)		(0.35)		(0.32)
ACQUISITION		0.054		0.047		0.050
		(0.49)		(0.43)		(0.45)
WEAK		-0.269***		-0.265***		-0.262***
		(-2.84)		(-2.80)		(-2.77)
AUD_FEE		-0.110*		-0.114*		-0.107
		(-1.65)		(-1.71)		(-1.60)
BIG4		0.401**		0.399**		0.394**
		(2.47)		(2.46)		(2.43)
AUD_NEW		-0.499***		-0.504***		-0.506***
		(-3.78)		(-3.82)		(-3.84)
IND_SPEC		0.120		0.105		0.118
		(1.03)		(0.90)		(1.02)
<i>Firm FE</i>	Yes	Yes	Yes	Yes	Yes	Yes
<i>Year FE</i>	Yes	Yes	Yes	Yes	Yes	Yes
Obs	5 715	5 715	5 715	5 715	5 715	5 715
Pseudo R-sq.	0.074	0.099	0.072	0.098	0.072	0.098

Panel B: OLS Specifications

<i>Variable</i>	Honigsberg et al.		Choi et al.		Combination	
POST*RISK_UP	-0.029	-0.026	-0.013	-0.011	-0.032	-0.032
<i>pred. sign: -</i>	(-1.62)	(-1.43)	(-0.78)	(-0.67)	(-1.63)	(-1.60)
POST*RISK_DOWN	-0.038**	-0.030*	0.019	0.022	-0.015	-0.012
<i>pred. sign: +</i>	(-2.08)	(-1.66)	(0.87)	(1.03)	(-0.62)	(-0.49)
<i>Controls</i>	No	Yes	No	Yes	No	Yes
<i>Firm FE</i>	Yes	Yes	Yes	Yes	Yes	Yes
<i>Year FE</i>	Yes	Yes	Yes	Yes	Yes	Yes
Obs	18 692	18 692	18 692	18 692	18 692	18 692
R-sq.	0.454	0.460	0.454	0.460	0.454	0.460

Note: The Table reports the results of the analysis estimating the effects of the Tellabs ruling on the likelihood of a financial statement restatement. The dependent variable is *REST*, an indicator equal to 1 if a restatement is announced for the corresponding fiscal year, and 0 otherwise. Panel A presents fixed effects logit specifications, while Panel B reports the corresponding OLS estimates. All models include firm and year fixed effects, and standard errors are clustered at the firm level. Test statistics are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 2.18 reports the coefficients for both Big 4 and non-Big 4 auditor samples. Interestingly, the observed reduction in restatement rates appears to be primarily driven by firms audited by non-Big 4 auditors. For this subgroup, the coefficients on both $POST * RISK_UP$ (as predicted) and $POST * RISK_DOWN$ (contrary to the predicted) are consistently negative across all classifications and model specifications. For the Big 4 sample, the coefficients are of different signs depending on the circuit classification. The only case where the coefficient is statistically significant and of predicted sign is $POST * RISK_DOWN$ when Choi et al. (2012) classification is applied (in line with the main results presented in Panel A Table 2.17).

Table 2.18: Tellabs case: Big 4 vs non-Big 4 Restatements

Panel A: Logit Specification

Variable	Honigsberg et al.		Choi et al.		Combination	
	BIG4	non-BIG4	BIG4	non-BIG4	BIG4	non-BIG4
POST*RISK_UP	-0.161	-0.413	0.0902	-0.938**	-0.102	-0.823*
<i>pred. sign: -</i>	(-0.94)	(-1.04)	(0.53)	(-2.40)	(-0.58)	(-1.85)
POST*RISK_DOWN	-0.383*	-0.983**	0.560**	-0.196	0.283	-1.399**
<i>pred. sign: +</i>	(-1.73)	(-2.23)	(2.42)	(-0.46)	(0.98)	(-2.38)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs	4 161	1 052	4 161	1 052	4 161	1 052
Pseudo R-sq.	0.095	0.117	0.096	0.118	0.094	0.121

Panel B: OLS Specification

Variable	Honigsberg et al.		Choi et al.		Combination	
	BIG4	non-BIG4	BIG4	non-BIG4	BIG4	non-BIG4
POST*RISK_UP	-0.021	-0.004	0.003	-0.042	-0.020	-0.026
<i>pred. sign: -</i>	(-0.92)	(-0.14)	(0.13)	(-1.64)	(-0.81)	(-0.96)
POST*RISK_DOWN	-0.028	-0.036	0.038	-0.025	0.013	-0.083*
<i>pred. sign: +</i>	(-1.26)	(-1.08)	(1.52)	(-0.50)	(0.46)	(-1.67)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs	14 117	4 575	14 117	4 575	14 117	4 575
R-sq.	0.500	0.493	0.500	0.493	0.500	0.493

Note: The Table reports the results of the analysis estimating the effects of the Tellabs ruling on the likelihood of a financial statement restatement for Big 4 and non-Big 4 auditor subsamples. The dependent variable is *REST*, an indicator equal to 1 if a restatement is announced for the corresponding fiscal year, and 0 otherwise. Panel A presents fixed effects logit specifications, while Panel B reports the corresponding OLS estimates. All models include firm and year fixed effects, and standard errors are clustered at the firm level. Test statistics are shown in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Finally, the results of the analysis assessing changes in restatement rates following the Supreme Court's Janus ruling reported in Table 2.19, do not provide additional support for Hypothesis 3. Contrary to the predicted outcome, the affected circuits — if anything — experienced a more pronounced decline in restatement rates than their unaffected counterparts, despite the reduction in auditors' litigation risk. The coefficient of interest is consistently negative across all specifications, although it reaches statistical significance at the 10% level only in the logit model for the Big 4 subsample.

Table 2.19: Janus case: Restatement rates

Panel A: Logit Specification

<i>Variable</i>	Full Sample	Full Sample	BIG4	non-BIG4
POST*RISK_DOWN	-0.251	-0.251	-0.309*	-0.416
<i>pred. sign: +</i>	(-1.55)	(-1.55)	(-1.73)	(-0.89)
SIZE		0.667***	0.630***	0.535*
		(5.33)	(4.23)	(1.85)
LEV		0.267	-0.127	1.620*
		(0.70)	(-0.28)	(1.88)
ROA		-0.500*	-0.354	-0.392
		(-1.94)	(-1.07)	(-0.84)
MTB		0.0119	0.0215**	-0.0265
		(1.47)	(2.32)	(-1.36)
SALE_GROWTH		-0.00725	-0.119	0.217
		(-0.08)	(-0.98)	(1.38)
ACQUISITION		0.0252	0.135	-0.429
		(0.20)	(0.96)	(-1.43)
WEAK		0.176	0.197	0.160
		(1.57)	(1.45)	(0.70)
AUD_FEE		-0.139	-0.0123	-0.289
		(-1.20)	(-0.08)	(-1.21)
BIG4		0.0389		
		(0.15)		
IND_SPEC		0.355***	0.370***	
		(2.68)	(2.68)	
AUD_NEW		-0.475***	-0.215	-0.592**
		(-2.85)	(-0.83)	(-2.32)

Table 2.19: Janus case: Restatement rates (continued)

<i>Firm FE</i>	No	No	No	No
<i>Year FE</i>	Yes	Yes	Yes	Yes
Obs.	4660	4660	3652	837

Panel B: OLS Specifications

<i>Variable</i>	Full Sample	Full Sample	BIG4	non-BIG4
POST*RISK_DOWN	-0.018	-0.018	-0.026	-0.007
<i>pred. sign: +</i>	(-1.36)	(-1.36)	(-1.55)	(-0.38)
<i>Cont.</i>	Yes	Yes	Yes	Yes
<i>Firm FE</i>	Yes	Yes	Yes	Yes
<i>Year FE</i>	Yes	Yes	Yes	Yes
Obs.	19 180	19 180	13 768	5 412

Note: The Table reports the results of the analysis estimating the effects of the Janus ruling on the likelihood of a financial statement restatement. The dependent variable is *REST*, an indicator equal to 1 if a restatement is announced for the corresponding fiscal year, and 0 otherwise. In Panel A, Columns 1–2 present logit specifications for the full sample, while Columns 3–4 report the corresponding estimates for the Big 4 and non-Big 4 subsamples. Panel B presents the analogous OLS specifications. All models include year fixed effects, and the OLS specifications additionally include firm fixed effects. Standard errors are clustered at the firm level. Test statistics are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Overall, the results offer only limited support for the idea that changes in litigation risk—triggered by the two Supreme Court decisions—affected restatement rates in the relevant circuits through changes in audit quality. There is modest evidence that restatement rates declined among firms headquartered in circuits where auditor liability became easier to establish. Interestingly, this effect does not appear to be driven by the Big 4 auditors. As for the circuits where litigation risk decreased following either decision, there is no evidence of a corresponding increase in restatement rates.

Additionally, we test two other variables that may capture changes in audit quality triggered by the Tellabs and Janus rulings: errors in GC opinions issued and reporting aggressiveness, a continuous variable measured as the difference between the predicted probability of receiving a MAO and actual MAO issuance.

Tables [2.20](#) (Tellabs ruling) and [2.21](#) (Janus ruling) present the results of the GC error rate analysis. Notably, the coefficients for the interaction terms *POST*RISK_UP* and

*POST*RISK_DOWN* generally align with the expected directions in both cases: GC errors tend to decrease in circuits where auditors faced heightened litigation risk, and increase where the risk was reduced. However, statistical significance is observed only in the analysis related to the Tellabs ruling — specifically for clients headquartered in circuits where auditor risk declined. Further division of the sample revealed that the effect is driven by the Big 4 subsample.

These findings suggest that auditors may have adjusted the quality of their work in response to shifts in litigation risk. Nonetheless, the results should be interpreted with caution, as the analysis is limited to a subsample of financially distressed companies. Additionally, the bankruptcy data used to construct the outcome variable was sourced from Compustat, with only about one percent of the firms in both the Tellabs and Janus samples resulting to go bankrupt. A next step could be to supplement this analysis with data from alternative sources that offer broader bankruptcy coverage, such as Audit Analytics or Bankruptcy.com.

Table 2.20: Tellabs case: GC errors

Panel A: Honigsberg et al. (2020) classification						
<i>Variable</i>	<i>Logit</i>			<i>OLS</i>		
	Full Sample	BIG4	non-BIG4	Full Sample	BIG4	non-BIG4
POST*RISK_UP	-0.118	-0.010	-0.169	-0.010	-0.021	0.016
<i>pred. sign: -</i>	(-0.31)	(-0.02)	(-0.27)	(-0.48)	(-0.90)	(0.35)
POST*RISK_DOWN	1.465***	2.563**	1.048	0.040	0.047	0.023
<i>pred. sign: +</i>	(2.60)	(2.14)	(1.22)	(1.50)	(1.39)	(0.45)
SIZE	-0.943***	-0.905**	-1.160***	-0.054***	-0.044***	-0.063***
	(-4.54)	(-2.40)	(-3.64)	(-4.76)	(-3.40)	(-2.60)
LEV	-1.722***	-1.722**	-1.889***	-0.150***	-0.138***	-0.158***
	(-3.82)	(-2.50)	(-2.63)	(-3.70)	(-2.90)	(-1.90)
ROA	-0.562*	-1.091**	-0.661	-0.073***	-0.069***	-0.088**
	(-1.95)	(-2.12)	(-1.50)	(-3.48)	(-2.72)	(-2.55)
MTB	-0.028***	-0.014	-0.037**	-0.002***	-0.001	-0.002**
	(-2.91)	(-1.00)	(-2.50)	(-3.01)	(-1.28)	(-2.46)
Z.SCORE	-0.080***	-0.049*	-0.092***	-0.004***	-0.004***	-0.005***
	(-5.22)	(-1.81)	(-4.06)	(-7.13)	(-4.80)	(-4.45)
OCF	0.124	-0.680	0.736	-0.079***	-0.127***	-0.026
	(0.29)	(-0.98)	(1.03)	(-2.68)	(-3.38)	(-0.55)
LOSS	0.984**	0.758	1.117*	0.004	0.013	-0.005
	(2.25)	(1.10)	(1.75)	(0.38)	(0.95)	(-0.23)
RETURN	-0.229***	-0.437***	-0.063	-0.013***	-0.011***	-0.016**
	(-3.47)	(-3.41)	(-0.65)	(-4.39)	(-3.54)	(-2.27)
VOL	0.216**	0.330**	0.177	0.019***	0.012*	0.030***
	(2.54)	(2.19)	(1.34)	(3.44)	(1.73)	(2.76)
WEAK	0.139	0.658	-0.569	0.033***	0.043***	0.031
	(0.52)	(1.60)	(-1.24)	(2.82)	(2.75)	(1.56)

Table 2.20: Tellabs case: GC errors (continued)

AUD_FEE	0.907*** (3.74)	0.462 (1.20)	1.752*** (4.16)	0.031*** (3.07)	0.015 (1.19)	0.063*** (2.95)
BIG4	-0.218 (-0.50)			0.011 (0.63)		
IND_SPEC	0.335 (0.96)	-0.438 (-1.09)	-11.360 (-0.01)	0.017 (1.26)	0.008 (0.54)	-0.422*** (-11.87)
AUD_NEW	-0.264 (-0.94)	0.731 (1.04)	-1.048*** (-2.64)	-0.013 (-0.87)	0.029 (1.22)	-0.041* (-1.90)
TENURE	0.002 (0.03)	0.275** (2.08)	-0.232** (-2.14)	-0.004 (-1.11)	0.010** (2.29)	-0.014** (-2.00)
<i>Firm FE</i>	Yes	Yes	Yes	Yes	Yes	Yes
<i>Year FE</i>	Yes	Yes	Yes	Yes	Yes	Yes
Obs	1 415	709	596	7 898	5 218	2 680
Pseudo R-sq.& R-sq.	0.423	0.478	0.459	0.648	0.636	0.698

Panel B: Choi et al. (2012) classification

<i>Variable</i>	<i>Logit</i>			<i>OLS</i>		
	Full Sample	BIG4	non-BIG4	Full Sample	BIG4	non-BIG4
POST*RISK_UP	-0.403 (-1.06)	-0.280 (-0.47)	-0.516 (-0.81)	-0.012 (-0.60)	-0.014 (-0.57)	0.006 (0.15)
<i>pred. sign: -</i>						
POST*RISK_DOWN	0.114 (0.20)	2.154* (1.71)	-1.804** (-2.01)	0.025 (0.78)	0.095** (2.08)	-0.088* (-1.69)
<i>pred. sign: +</i>						
<i>Controls</i>	Yes	Yes	Yes	Yes	Yes	Yes
<i>Firm FE</i>	Yes	Yes	Yes	Yes	Yes	Yes
<i>Year FE</i>	Yes	Yes	Yes	Yes	Yes	Yes
Obs	1 415	709	596	7 898	5 218	2 680
Pseudo R-sq.& R-sq.	0.416	0.475	0.463	0.648	0.636	0.699

Panel C: combination of classification

<i>Variable</i>	<i>Logit</i>			<i>OLS</i>		
	Full Sample	BIG4	non-BIG4	Full Sample	BIG4	non-BIG4
POST*RISK_UP	-0.411 (-1.08)	0.099 (0.17)	-1.159* (-1.80)	-0.013 (-0.67)	-0.017 (-0.77)	0.009 (0.21)
<i>pred. sign: -</i>						
POST*RISK_DOWN	0.832 (1.17)	16.270 (0.02)	-1.766* (-1.88)	0.052 (1.45)	0.102** (2.13)	-0.051 (-0.89)
<i>pred. sign: +</i>						
<i>Controls</i>	Yes	Yes	Yes	Yes	Yes	Yes
<i>Firm FE</i>	Yes	Yes	Yes	Yes	Yes	Yes
<i>Year FE</i>	Yes	Yes	Yes	Yes	Yes	Yes
Obs	1 415	709	596	7 898	5 218	2 680
Pseudo R-sq.& R-sq.	0.418	0.485	0.465	0.648	0.636	0.698

Note: The Table reports the results of the analysis estimating the effects of the Tellabs ruling on the likelihood of a GC error. The dependent variable is *GC_ERROR*, an indicator variable that equals 1 if the auditor commits a GC Type I or Type II error, and 0 otherwise. Panel A presents estimates using the circuit classification of Honigsberg et al. (2020), Panel B applies the classification of Choi et al. (2012), and Panel C reports results using a combined classification. Within each panel, logit specifications are shown in Columns 1–3 (full sample, Big 4, and non-Big 4), and OLS estimates in Columns 4–6. All models include firm and year fixed effects, and standard errors are clustered at the firm level. Test statistics are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 2.21: Janus case: GC errors

<i>Variable</i>	<i>Logit</i>			<i>OLS</i>		
	Full Sample	BIG4	non-BIG4	Full Sample	BIG4	non-BIG4
POST*RISK_DOWN	0.212	0.280	-0.227	0.003	0.007	-0.032
<i>pred. sign: -</i>	(0.60)	(0.49)	(-0.41)	(0.16)	(0.32)	(-0.93)
SIZE	-0.862***	-0.527	-1.145***	-0.040***	-0.025**	-0.064***
	(-3.77)	(-1.27)	(-3.68)	(-3.75)	(-2.00)	(-3.18)
LEV	-1.056***	-1.919***	-0.519	-0.135***	-0.187***	-0.101
	(-2.74)	(-2.99)	(-0.93)	(-3.41)	(-4.04)	(-1.33)
ROA	-1.036***	-1.032**	-1.304***	-0.149***	-0.145***	-0.158***
	(-3.71)	(-2.18)	(-3.46)	(-7.22)	(-5.33)	(-5.18)
MTB	-0.007	-0.009	0.002	-0.001**	-0.001**	-0.000
	(-0.80)	(-0.76)	(0.17)	(-1.99)	(-2.06)	(-0.54)
Z_SCORE	-0.055***	-0.116***	-0.015	-0.003***	-0.004***	-0.003***
	(-3.76)	(-3.82)	(-1.00)	(-6.52)	(-6.07)	(-2.85)
OCF	0.176	0.765	-0.039	-0.029	-0.047	-0.001
	(0.42)	(1.12)	(-0.07)	(-1.00)	(-1.20)	(-0.02)
LOSS	1.356**	16.640	0.468	-0.004	0.021	-0.028
	(2.24)	(0.02)	(0.72)	(-0.30)	(1.16)	(-1.36)
RETURN	-0.285***	-0.264**	-0.314***	-0.012***	-0.009**	-0.015***
	(-4.17)	(-2.42)	(-3.15)	(-4.48)	(-2.55)	(-3.32)
VOL	0.313***	0.238*	0.428***	0.022***	0.019**	0.026***
	(4.18)	(1.87)	(3.88)	(3.59)	(2.02)	(2.94)
WEAK	0.210	0.407	0.054	0.010	0.023	-0.002
	(0.94)	(1.14)	(0.16)	(0.86)	(1.39)	(-0.12)
AUD_FEE	0.538**	0.522	0.517	0.024*	0.017	0.036
	(2.11)	(1.20)	(1.35)	(1.80)	(1.06)	(1.50)
BIG4	-0.648			-0.033		
	(-1.54)			(-1.33)		
IND_SPEC	0.321	0.248		0.013	0.013	
	(0.91)	(0.60)		(0.95)	(0.89)	
AUD_NEW	0.598**	0.527	0.568*	0.033**	0.041	0.033*
	(2.28)	(0.82)	(1.75)	(2.21)	(1.54)	(1.76)
TENURE	0.081*	0.098	0.032	0.003	0.006	0.001
	(1.91)	(1.32)	(0.42)	(1.30)	(1.31)	(0.15)
Const.				0.516**	0.247	0.801**
				(2.46)	(0.95)	(2.13)
<i>Firm FE</i>	Yes	Yes	Yes	Yes	Yes	Yes
<i>Year FE</i>	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	1 595	707	775	8 134	4 920	3 214
Pseudo R-sq. & R-sq.	0.392	0.449	0.397	0.653	0.637	0.688

Note: The Table reports the results of the analysis estimating the effects of the Janus ruling on the likelihood of a GC error. The dependent variable is *GC_ERROR*, an indicator variable that equals 1 if the auditor commits a GC Type I or Type II error, and 0 otherwise. Columns 1–3 present fixed effects logit estimates for the full sample, Big 4, and non-Big 4 subsamples, respectively, while Columns 4–6 report the corresponding OLS specifications. All models include firm and year fixed effects, and standard errors are clustered at the firm level. Test statistics are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Tables [2.22](#) and [2.23](#) present the results of the audit aggressiveness (*AUD_AGGR*) anal-

ysis. This variable compares the predicted probability of receiving a MAO opinion — estimated using the model by Gul et al. (2013) — with the actual occurrence of a MAO. Higher values indicate more client-friendly reporting choices, which, to some extent, reflect lower audit quality. The findings support the hypothesis only when the classification of affected circuits from Choi et al. (2012), or the combined classification, is applied: auditors appear to have become more aggressive in their MAO reporting for clients headquartered in circuits where litigation risk increased following the Tellabs ruling. As in the case of the GC errors analysis, the effect is particularly evident among Big 4 auditors. When the alternative Honigsberg et al. (2020) classification is applied the $POST*RISK_UP$ coefficient is also negative, in line with the predictions, however the results are not statistically significant. In contraction with the hypothesized relations the $POST*RISK_UP$ coefficient is also negative and statistically significant. For the Janus ruling no results supporting the hypothesis are found.

Table 2.22: Tellabs case: audit aggressiveness

Panel A: Honigsberg et al. (2020) classification			
Variable	Full Sample	BIG4	non-BIG4
POST*RISK_UP	-0.007	-0.035	-0.026
<i>pred. sign: -</i>	(-0.35)	(-1.45)	(-0.62)
POST*RISK_DOWN	-0.044**	-0.068**	-0.028
<i>pred. sign: +</i>	(-1.96)	(-2.50)	(-0.60)
SIZE	-0.027**	-0.038***	0.009
	(-2.26)	(-2.59)	(0.40)
LEV	0.207***	0.183***	0.283***
	(4.88)	(3.49)	(3.71)
ROA	0.095***	0.105***	0.064
	(3.26)	(2.78)	(1.32)
MTB	-0.000	-0.001	0.001
	(-0.03)	(-0.69)	(0.55)
Z_SCORE	0.005***	0.005***	0.005***
	(6.06)	(4.45)	(3.78)
OCF	0.052	0.088*	0.027
	(1.40)	(1.81)	(0.46)
LOSS	0.028**	0.038***	0.013
	(2.46)	(2.72)	(0.59)
RETURN	-0.003	-0.002	-0.006
	(-0.74)	(-0.47)	(-0.86)
VOL	-0.008	0.001	-0.009
	(-0.79)	(0.08)	(-0.55)
WEAK	-0.053***	-0.045***	-0.043
	(-3.84)	(-2.73)	(-1.62)
AUD_FEE	-0.039***	-0.040***	-0.068***
	(-4.78)	(-4.01)	(-3.63)
BIG4	-0.101***		

Table 2.22: Tellabs case: audit aggressiveness (continued)

	(-4.54)		
IND_SPEC	0.013	0.012	-0.205
	(0.90)	(0.84)	(-1.13)
AUD_NEW	0.030**	0.085***	0.036*
	(2.03)	(3.46)	(1.65)
TENURE	0.003	-0.003	0.008
	(0.98)	(-0.49)	(1.26)
Const.	1.030***	1.222***	0.691*
	(4.51)	(4.04)	(1.78)
<i>Firm FE</i>	Yes	Yes	Yes
<i>Year FE</i>	Yes	Yes	Yes
Obs	19 078	14 259	4 819
R-sq.	0.368	0.381	0.440

Panel B: Choi et al. (2012) classification

POST*RISK_UP	-0.063***	-0.081***	-0.016
<i>pred. sign: -</i>	(-3.33)	(-3.58)	(-0.39)
POST*RISK_DOWN	-0.014	-0.043	0.078
<i>pred. sign: +</i>	(-0.55)	(-1.39)	(1.53)
<i>Controls</i>	Yes	Yes	Yes
<i>Firm FE</i>	Yes	Yes	Yes
<i>Year FE</i>	Yes	Yes	Yes
R-sq.	0.368	0.382	0.441

Panel C: Combination

POST*RISK_UP	-0.038*	-0.046*	-0.036
<i>pred. sign: -</i>	(-1.80)	(-1.85)	(-0.77)
POST*RISK_DOWN	-0.022	-0.057	0.072
<i>pred. sign: +</i>	(-0.73)	(-1.57)	(1.22)
<i>Controls</i>	Yes	Yes	Yes
<i>Firm FE</i>	Yes	Yes	Yes
<i>Year FE</i>	Yes	Yes	Yes
R-sq.	0.368	0.381	0.441

Note: The Table reports the results of the analysis estimating the effects of the Tellabs ruling on audit aggressiveness. The dependent variable is *AUD_AGGR*, defined as the difference between the predicted probability of receiving a modified audit opinion—estimated using the model of Gul et al. (2013)—and the actual issuance of a MAO, with higher values indicating more aggressive (client-favorable) reporting. Panel A presents estimates based on the circuit classification of Honigsberg et al. (2020), Panel B applies the classification of Choi et al. (2012), and Panel C reports results using a combined classification. For each panel, regressions are estimated for the full sample, the Big 4 subsample, and the non-Big 4 subsample. All models include firm and year fixed effects, and standard errors are clustered at the firm level. Test statistics are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 2.23: Janus case: audit aggressiveness

<i>Variable</i>	Full Sample	BIG4	non-BIG4
POST*RISK_DOWN	0.000	-0.016	0.025
<i>pred. sign: +</i>	(0.01)	(-0.80)	(0.78)
SIZE	0.030	0.031	0.033
	(2.67)	(2.14)	(1.61)
LEV	0.167	0.175	0.165
	(4.75)	(4.24)	(2.34)
ROA	0.094	0.096	0.118
	(3.45)	(2.45)	(2.95)
MTB	-0.001	-0.001	0.000
	(-0.88)	(-0.99)	(0.10)
Z_SCORE	0.004	0.002	0.004
	(4.53)	(2.19)	(3.50)
OCF	0.043	0.061	0.009
	(1.28)	(1.26)	(0.19)
LOSS	0.034	0.043	0.040
	(3.37)	(3.42)	(2.29)
RETURN	0.011	0.007	0.014
	(2.88)	(1.46)	(2.35)
VOL	-0.046	-0.033	-0.045
	(-4.64)	(-2.35)	(-3.04)
WEAK	-0.051	-0.041	-0.069
	(-3.79)	(-2.38)	(-3.02)
AUD_FEE	-0.059	-0.068	-0.062
	(-5.78)	(-5.08)	(-3.02)
BIG4	-0.032		
	(-1.21)		
IND_SPEC	-0.030	-0.028	
	(-2.31)	(-2.11)	
AUD_NEW	0.055	0.038	0.009
	(3.86)	(1.43)	(0.51)
TENURE	0.008	-0.005	0.002
	(3.83)	(-1.68)	(0.61)
<i>Firm FE</i>	Yes	Yes	Yes
<i>Year FE</i>	Yes	Yes	Yes
Const.	0.137	0.292	0.214
Obs	19 389	13 672	5 717
R-sq.	0.375	0.370	0.461

Note: The Table reports the results of the analysis estimating the effects of the Janus ruling on audit aggressiveness. The dependent variable is *AUD_AGGR*, defined as the difference between the predicted probability of receiving a modified audit opinion—based on Gul et al. (2013)—and the actual issuance of a MAO. Estimates are presented separately for the full sample, Big 4 auditors, and non-Big 4 auditors. All models include firm and year fixed effects, with standard errors clustered at the firm level. Test statistics are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Taken together, the evidence supporting the hypothesized positive relationship between auditors' litigation risk and audit quality is limited. For the Tellabs ruling, the expected pattern in restatement behavior appears only when applying the circuit classification from

Choi et al. (2012): clients of Big 4 auditors headquartered in circuits where litigation risk declined experienced an increase in restatements after the ruling, while non-Big 4 clients in circuits where litigation risk increased exhibited a decrease. In contrast, the analysis of the Janus ruling yields no results consistent with the hypothesis. With respect to GC error rates, there is some support for the hypothesis — under the Honigsberg et al. (2020) classification (and under Choi et al. (2012) for the Big 4 subsample), a notable rise in GC error rates is observed in circuits where auditors’ litigation risk decreased following the Tellabs decision. The results in the case of the Janus ruling are of predicted sign, but not statistically significant. Finally, under the Choi et al. (2012) classification, auditors appear to have adopted a more aggressive stance in issuing MAO for clients headquartered in circuits where litigation risk increased after the Tellabs ruling.

2.5 Sensitivity Analysis

In this section, we conduct additional robustness tests focusing on the audit fee effects of the Supreme Court rulings, as this part of the analysis yielded the most consistent results in the main findings. First, we perform entropy balancing to mitigate observable heterogeneity between the treatment and control groups. Next, we test the relationship using alternative sample periods, including the years in which the Supreme Court decisions were announced—2007 for Tellabs and 2011 for Janus. Finally, we introduce two additional control variables: the number of business segments and the number of geographical segments. These variables capture firm complexity, which is known to influence audit fees. However, they were excluded from the main analysis due to limited data availability, which would significantly reduce the sample size.

2.5.1 Entropy Balancing

Table [2.24](#) presents the results of the audit fees changes analysis when entropy balancing procedure is applied to match the samples of the treatment and controls groups. The

results are presented only for those specification which are statistically significant in the main part of the analysis. We match firms headquartered in circuits where litigation risk increased (Tellabs) or decreased (Janus) with firms in other circuits, as these firms are the ones that, in the main analysis, ended up paying higher or lower audit fees as a result of the rulings. Entropy balancing is performed in the pre-treatment period to reweight the control group so that it matches the treatment group on the means of a set of controls variables. The Table shows that the coefficients of interest remain of predicted signs and are mostly statistically significant. The only exception is when the analysis is conducted on the Big 4 sub sample and Honingsberg et al. (2020) classification is applied.

Table 2.24: Audit Fees: Entropy Balancing

Variable	Tellabs				Janus	
	Full Sample		Big 4		Full Sample	Big 4
	Hon. et al.	Comb.	Hon. et al.	Comb.		
POST*RISK_UP	0.035*	0.051**	0.034	0.060**		
	(1.69)	(2.23)	(1.42)	(2.29)		
POST*RISK_DOWN					-0.071***	-0.060***
					(-3.80)	(-2.82)
SIZE	0.388***	0.385***	0.377***	0.368***	0.314***	0.300***
	(23.68)	(22.33)	(19.07)	(17.75)	(18.68)	(14.12)
INVREC	0.481***	0.482***	0.636***	0.667***	0.389***	0.476***
	(5.75)	(5.01)	(5.55)	(5.11)	(4.57)	(4.46)
IMPAIR	0.013	0.012	0.024	0.024	0.029**	0.031**
	(0.73)	(0.58)	(1.15)	(1.00)	(2.43)	(2.40)
ROA	-0.241***	-0.246***	-0.222***	-0.236***	-0.178***	-0.150***
	(-7.07)	(-7.28)	(-4.51)	(-5.32)	(-6.31)	(-4.54)
LOSS	0.033**	0.034**	0.027	0.025	0.022*	0.020
	(2.28)	(2.23)	(1.51)	(1.31)	(1.80)	(1.62)
LEV	0.092**	0.098**	0.080	0.099*	0.013	0.053
	(2.08)	(2.07)	(1.56)	(1.82)	(0.31)	(1.12)
NEG_EQ	0.074*	0.075	0.108**	0.105*	-0.001	-0.014
	(1.71)	(1.47)	(2.19)	(1.90)	(-0.03)	(-0.39)
MTB	0.003**	0.003*	0.005***	0.004**	-0.000	0.000
	(2.12)	(1.77)	(2.67)	(2.42)	(-0.19)	(0.23)
ACQUIS	0.037**	0.033*	0.042**	0.036*	0.054***	0.068***
	(2.31)	(1.85)	(2.39)	(1.82)	(4.14)	(4.62)
FOREIGN	0.022	0.018	0.018	0.015	0.009	0.012
	(1.59)	(1.19)	(1.13)	(0.87)	(0.77)	(0.94)
WEAK	0.196***	0.199***	0.189***	0.192***	0.163***	0.176***
	(9.71)	(9.19)	(7.30)	(6.96)	(9.74)	(8.69)
BUSY	0.225***	0.244***	0.318***	0.306***	0.129	0.106
	(3.33)	(4.28)	(4.38)	(4.22)	(1.45)	(0.92)
BIG4	0.092**	0.099**			0.283***	
	(2.34)	(2.29)			(5.37)	
OPINION	0.034***	0.037***	0.038***	0.037***	0.046***	0.050***
	(3.29)	(3.33)	(3.19)	(2.94)	(4.66)	(4.95)
IND_SPEC	0.031	0.035	0.029	0.034	-0.018	-0.004
	(1.51)	(1.54)	(1.46)	(1.50)	(-0.85)	(-0.21)
AUD_NEW	-0.057***	-0.044*	-0.033	-0.025	-0.106***	-0.156***

	(-2.91)	(-1.95)	(-0.87)	(-0.60)	(-6.07)	(-5.15)
Const.	5.329***	5.386***	5.648***	5.834***	7.005***	7.714***
	(16.46)	(15.95)	(14.21)	(14.09)	(20.96)	(18.51)
<i>Firm FE</i>	Yes	Yes	Yes	Yes	Yes	Yes
<i>Year FE</i>	Yes	Yes	Yes	Yes	Yes	Yes
Obs	18 830	18 830	14 187	14 187	20 567	14 600
R-sq.	0.884	0.880	0.877	0.874	0.923	0.911

Note: The Table reports audit fee regressions estimated on entropy-balanced samples. The dependent variable is *AUD_FEE*, natural logarithm of audit fees. Results are shown for the Tellabs (Honigsberg et al. and combined classifications) and Janus settings, for the full sample and the Big 4 subsample. All specifications include firm and year fixed effects with standard errors clustered at the firm level. Test statistics are in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels.

2.5.2 Alternative Sample Periods

In the main analysis the years of the rulings announcements (2007 and 2011) are excluded from the sample. Alternatively, we include these years in the sample periods and set *Post* equal to 1 for annual reports with financial year end in July 2007 (2011) or later for the Tellabs (Janus) analysis. As in the case with entropy balancing, the results remain mainly unchanged: the coefficients of interest are of predicted signs and mostly statistically significant.

Table 2.25: Audit Fees: Alternative Sample Periods

Variable	Tellabs				Janus	
	Full Sample		Big 4		Full Sample	Big 4
	Hon. et al.	Comb.	Hon. et al.	Comb.		
POST*RISK_UP	0.037** (2.08)	0.048** (2.54)	0.024 (1.21)	0.043** (2.02)		
POST*RISK_DOWN	-0.007 (-0.37)	-0.023 (-0.79)	-0.007 (-0.32)	-0.017 (-0.53)	-0.066*** (-4.48)	-0.056*** (-3.47)
SIZE	0.395*** (28.30)	0.395*** (28.42)	0.388*** (22.40)	0.388*** (22.52)	0.326*** (17.95)	0.303*** (13.17)
INVREC	0.522*** (7.34)	0.520*** (7.32)	0.634*** (6.51)	0.631*** (6.47)	0.446*** (5.25)	0.152* (1.78)
IMPAIR	0.017 (1.26)	0.017 (1.25)	0.024 (1.53)	0.024 (1.53)	0.030*** (3.36)	0.024 (1.23)
ROA	-0.238*** (-8.53)	-0.238*** (-8.53)	-0.210*** (-5.20)	-0.210*** (-5.20)	-0.166*** (-5.93)	-0.182*** (-5.88)
LOSS	0.033*** (2.90)	0.034*** (2.93)	0.033** (2.33)	0.033** (2.35)	0.022** (2.28)	0.027* (1.81)
LEV	0.083** (2.23)	0.083** (2.22)	0.095** (2.13)	0.094** (2.11)	0.058 (1.39)	-0.072 (-1.15)

NEG_EQ	0.060*	0.060*	0.110***	0.111***	-0.011	0.029
	(1.81)	(1.81)	(2.86)	(2.87)	(-0.36)	(0.55)
MTB	0.002*	0.002*	0.004***	0.004***	0.000	-0.002
	(1.94)	(1.95)	(3.45)	(3.47)	(0.05)	(-1.47)
ACQUIS	0.034***	0.034***	0.032**	0.032**	0.064***	0.020
	(2.93)	(2.93)	(2.51)	(2.49)	(6.72)	(1.04)
FOREIGN	0.019*	0.019*	0.014	0.014	0.010	0.017
	(1.67)	(1.65)	(1.12)	(1.09)	(0.96)	(0.75)
WEAK	0.184***	0.184***	0.191***	0.191***	0.165***	0.116***
	(11.39)	(11.39)	(9.46)	(9.47)	(10.03)	(5.07)
BUSY	0.220***	0.222***	0.350***	0.351***	0.082	0.328*
	(3.01)	(3.06)	(4.07)	(4.07)	(0.73)	(1.79)
BIG4	0.076**	0.076**			0.274***	
	(2.24)	(2.22)			(6.12)	
OPINION	0.040***	0.040***	0.041***	0.040***	0.039***	0.052***
	(4.92)	(4.88)	(4.33)	(4.29)	(4.93)	(3.04)
IND_SPEC	0.028*	0.028*	0.024	0.024	0.002	
	(1.73)	(1.76)	(1.50)	(1.53)	(0.16)	
AUD_NEW	-0.078***	-0.078***	-0.060**	-0.060**	-0.157***	-0.068***
	(-5.23)	(-5.24)	(-2.40)	(-2.41)	(-7.44)	(-4.21)
Const.	5.230***	5.235***	5.448***	5.450***	7.242***	6.719***
	(18.50)	(18.62)	(15.19)	(15.29)	(20.41)	(15.41)
<i>Firm FE</i>	Yes	Yes	Yes	Yes		
<i>Year FE</i>	Yes	Yes	Yes	Yes	Yes	Yes
Obs	22 963	22 963	17 086	17 086	22 994	16 358
R-sq.	0.896	0.896	0.887	0.887	0.764	0.672

Note: The Table reports audit fee regressions estimated using alternative sample periods in which the announcement years of the Tellabs (2007) and Janus (2011) rulings are retained. The dependent variable is *AUD_FEE*, natural logarithm of audit fees. Specifications are shown for the full and Big 4 samples under each circuit classification. All models include firm and year fixed effects with standard errors clustered at the firm level. Test statistics are reported in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

2.5.3 Additional Control Variables

Table 2.26 presents results of the audit pricing changes when additional control variables, number of business and geographical segments, are included in the model. Inclusion of these control variables lead to a significant reduction of the sample sizes, however it allows to control better for complexity of audit engagements which is known to be an important components of audit pricing. Indeed, the number of business segments of the client company, *BUS_SEG*, is of positive sign and statistically significant throughout the Table; the number of geographical segments of the client company, *GEO_SEG*, is

statistically significant for the Janus sample. These results are in line with the audit literature which states that auditors charge higher fees for complexity of the clients' companies. The signs and statistical significance of main variables of interest, confirms the results presented in the main analysis.

Table 2.26: Audit Fees: Expanded Set of Control Variables

Variable	Tellabs				Janus	
	Full Sample		Big 4		Full Sample	Big 4
POST*RISK_UP	0.052** (2.22)	0.059** (2.43)	0.059** (2.19)	0.080*** (2.84)		
POST*RISK_DOWN	0.017 (0.66)	0.017 (0.51)	0.021 (0.72)	0.042 (1.03)	-0.077*** (-3.79)	-0.067*** (-3.07)
SIZE	0.377*** (22.30)	0.376*** (22.48)	0.371*** (17.68)	0.370*** (17.85)	0.340*** (20.10)	0.325*** (18.22)
INVREC	0.413*** (4.73)	0.409*** (4.68)	0.530*** (4.60)	0.523*** (4.52)	0.298*** (3.83)	0.359*** (3.88)
IMPAIR	0.018 (1.09)	0.018 (1.09)	0.027 (1.47)	0.027 (1.48)	0.031*** (2.88)	0.029** (2.57)
ROA	-0.187*** (-4.61)	-0.187*** (-4.60)	-0.144** (-2.48)	-0.144** (-2.47)	-0.186*** (-5.60)	-0.147*** (-3.89)
LOSS	0.042*** (2.82)	0.042*** (2.84)	0.048*** (2.68)	0.049*** (2.69)	0.022* (1.95)	0.022* (1.73)
LEV	0.097** (2.01)	0.096** (1.99)	0.090 (1.61)	0.088 (1.59)	0.047 (1.04)	0.077 (1.46)
NEG_EQ	0.080* (1.93)	0.082** (1.99)	0.086* (1.82)	0.089* (1.89)	-0.025 (-0.71)	-0.030 (-0.79)
MTB	0.002 (1.16)	0.002 (1.19)	0.003** (2.00)	0.003** (2.04)	-0.001 (-0.92)	-0.000 (-0.08)
ACQUIS	0.030** (2.11)	0.031** (2.14)	0.036** (2.37)	0.036** (2.36)	0.042*** (3.66)	0.058*** (4.76)
FOREIGN	0.006 (0.41)	0.005 (0.39)	0.004 (0.29)	0.004 (0.27)	0.011 (0.92)	0.016 (1.35)
BUS_SEG	0.048** (2.19)	0.047** (2.16)	0.052** (2.04)	0.052** (2.02)	0.035** (2.32)	0.033** (2.29)
GEO_SEG	0.049* (1.68)	0.048 (1.64)	0.013 (0.45)	0.012 (0.40)	0.060*** (3.03)	0.063*** (3.40)
WEAK	0.195*** (9.70)	0.195*** (9.70)	0.207*** (8.61)	0.206*** (8.61)	0.140*** (8.35)	0.165*** (8.36)
BUSY	0.228** (2.55)	0.233*** (2.64)	0.324*** (3.26)	0.328*** (3.35)	0.175* (1.94)	0.125 (1.25)
BIG4	0.084** (2.15)	0.083** (2.12)			0.173*** (3.36)	
OPINION	0.030*** (3.05)	0.029*** (3.02)	0.033*** (2.93)	0.032*** (2.88)	0.046*** (4.97)	0.047*** (4.71)
IND_SPEC	0.055*** (2.80)	0.055*** (2.83)	0.044** (2.27)	0.045** (2.30)	0.016 (0.89)	0.022 (1.30)
AUD_NEW	-0.071*** (-3.92)	-0.071*** (-3.91)	-0.044 (-1.42)	-0.045 (-1.45)	-0.100*** (-6.40)	-0.147*** (-5.51)
Const.	5.464*** (16.08)	5.475*** (16.30)	5.716*** (13.32)	5.737*** (13.54)	6.454*** (18.08)	7.087*** (18.55)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs	15 398	15 398	11 744	11 744	15 464	11 149
R-sq.	0.893	0.893	0.883	0.883	0.932	0.919

Note: The Table reports the results of fixed effects regressions estimating the effects of the Tellabs and Janus rulings on audit fees. The dependent variable is *AUD_FEE*, the natural logarithm of audit fees. All models include firm and year fixed effects, and standard errors are clustered at the firm level. Test statistics are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

2.6 Conclusion

This paper aims to study the strategies used by the U.S. auditors to accommodate for the legal risks they face. Using the circuit level legislative changes we study whether auditors adjust their pricing, reporting conservatism and audit quality to the litigation risk levels associated with the engagements. Following previous literature we concentrate on two Supreme Court's rulings that enforced some of the circuits to revise their approaches to federal law interpretation and, consequently, altered auditors' litigation risk in these circuits. The first ruling (Tellabs) took place in 2007 and harmonized the approach regarding pleading scienter requirements obliging previously auditors-friendly circuits to soften their requirements for the proof of auditors' "fraudulent state of mind" and previously shareholders-friendly circuits, instead, to tighten such requirements. The second ruling (Janus) was announced in 2011 and affected the possibility of bringing to the court secondary actors of a crime. This ruling virtually prohibited to sue auditors for the misstatements found in their clients' reports but not in the signed audit reports which lead to a decrease of auditors' litigation risk in the circuits which previously could accept guiltiness of the auditors in such cases.

We use these two shocks to investigate whether the U.S. auditors resort to certain risk accommodation strategies discussed in the existing literature. In line with hypotheses, the results of the analysis suggest some evidence of the audit pricing adjustments evoked by the litigation shifts. Thus, we consistently find an increase in audit fees paid clients headquartered in the circuits where auditors experienced litigation risk increase after the Tellabs ruling. In the case of Janus ruling analysis we find a significant decrease in the fees paid by clients headquartered in the previously shareholder-friendly circuits.

Results for other auditor responses — reporting conservatism and audit quality — are less definitive. The most promising evidence relates to MAOs: auditors appear to adopt a more conservative approach in MAO reporting for clients in circuits where litigation risk increased after the Tellabs decision. However, we also observe an increase in MAO rates in circuits where both rulings led to reduced litigation risk, which complicates the interpretation. For the remaining variables reflecting auditor reporting choices and resulting audit quality, the findings are also mixed and do not provide a consistent pattern.

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Appendix A. Variable Definitions

Variable	Definition
AUD_FEE	Natural logarithm of audit fees.
MAO_OPINION	Indicator variable that equals 1 if the audit opinion is modified, and 0 otherwise.
GC_OPINION	Indicator variable that equals 1 if the auditor issues a going concern opinion, and 0 otherwise.
RESTATE	Indicator variable that equals 1 if a restatement is announced for the corresponding fiscal year, and 0 otherwise.
GC_ERROR	Indicator variable that equals 1 if the auditor commits a GC Type I or Type II error.
AUD_AGGR	Measure of auditor aggressiveness in issuing modified audit opinions, defined as the difference between the predicted probability of receiving a modified audit opinion—estimated using the model of Gul et al. (2013)—and the actual issuance of a MAO.
POST	Indicator variable that equals 1 if the fiscal year-end occurs after the relevant Supreme Court decision, and 0 otherwise.
RISK_UP	Indicator variable that equals 1 if the auditor's litigation risk increased due to the Supreme Court ruling, and 0 otherwise.
RISK_DOWN	Indicator variable that equals 1 if the auditor's litigation risk decreased due to the Supreme Court ruling, and 0 otherwise.
SIZE	Natural logarithm of total assets.
INVREC	Ratio of inventories and receivables to total assets.
IMPAIR	Indicator variable that equals 1 if an impairment is reported, and 0 otherwise.
ROA	Return on assets, calculated as net income divided by total assets.
LOSS	Indicator variable that equals 1 if the company reports a net loss, and 0 otherwise.
LEV	Leverage, calculates as total debt divided by total assets.
NEG_EQ	Indicator variable that equals 1 if the company reports negative equity, and 0 otherwise.
MTB	Market-to-book ratio.
ACQUIS	Indicator variable that equals 1 if the company reports merger or acquisition activity, and 0 otherwise.
FOREIGN	Indicator variable that equals 1 if the company has foreign operations, and 0 otherwise.
WEAK	Indicator variable that equals 1 if there is an internal control weakness disclosed, and 0 otherwise.
BUSY	Indicator variable that equals 1 if the company has a December fiscal year-end, and 0 otherwise.
BIG4	Indicator variable that equals 1 if the company is audited by a Big 4 audit firm, and 0 otherwise.
OPINION	Indicator variable that equals 1 if the audit opinion is modified (used as control), and 0 otherwise.
IND_SPEC	Indicator variable that equals 1 if the auditor is an industry specialist, and 0 otherwise.
AUD_NEW	Indicator variable that equals 1 if the audit firm is newly engaged, and 0 otherwise.
Z_SCORE	Altman Z-score to proxy bankruptcy risk.
OCF	Operating cash flow scaled by total assets.
RETURN	Stock returns.

VOL	Standard deviation of stock returns.
TENURE	Number of consecutive years the auditor has audited the company.
SALE.GROWTH	Yearly percentage growth in sales.

Chapter 3

Corporate Responses to ESG Regulation Violations

3.1 Introduction

In recent years, environmental, social, and governance (ESG) factors have become increasingly important components of corporate strategy and risk management. A growing body of literature shows that both equity and debt market participants incorporate ESG considerations into their investment and lending decisions, with firms exhibiting stronger ESG performance often enjoying lower cost of capital, improved operational outcomes, and superior long-term performance (e.g., Ng & Rezaee, 2015; Flammer, 2015; Eccles et al., 2014; Degryse et al., 2023; Amiraslani et al., 2023). Conversely, negative ESG-related events are typically perceived as adverse signals by the market, resulting in stock price declines, increases in credit risk, higher audit fees, and even auditor resignations (Krüger, 2015; Capelle-Blancard & Petit, 2019; Kölbel et al., 2017; Burke et al., 2019; Chakrabarty et al., 2024).

While prior research has documented robust market reactions to ESG misconduct, less is known about whether firms themselves respond to these negative events by taking corrective action. Do companies attempt to repair their damaged reputations and signal

renewed ESG commitments following ESG violations? The existing evidence on this question is scarce, with only a few studies examining firm-level responses to ESG crises. One exception is Asante-Appiah and Lambert (2022), who suggest that firms respond to ESG-related crises by seeking external support to manage risks and repair reputational damage, and, in line with this hypothesis, find that firms with tainted ESG reputations increase their nonaudit service purchases. Moreover, they provide evidence that auditors succeed in mitigating the adverse consequences of ESG violations, as higher nonaudit service purchases are associated with attenuated declines in firm value and operating performance.

In this paper, I seek to extend the analysis of corporate responses to ESG-related disruptions by focusing on a broad set of internal ESG-related actions. Specifically, I investigate whether firms penalized for ESG regulation violations subsequently strengthen their ESG infrastructure, launch new initiatives, improve their ESG standing relative to peers, enhance reporting transparency, and achieve measurable improvements in ESG outcomes. These questions are motivated by the premise that regulatory ESG violations signal elevated risk and impose reputational and operational costs. As such, I hypothesize that affected firms have strong incentives to take remedial actions aimed at restoring stakeholder confidence, mitigating potential financial and operational consequences, and reducing the likelihood of future violations.

In order to test the hypotheses, I combine ESG performance data from Refinitiv with enforcement records from Violation Tracker, which allows me to construct a sample of over 3 000 U.S. public companies observed between 2000 and 2023. Within this sample, some firms are identified as having committed ESG-related violations, while others have no recorded violations and thus serve as a sort of control group. In total, the enforcement data capture more than 31 000 regulatory violations, with the majority related to safety and environmental compliance, alongside employment and healthcare offenses. Among the violators, the size of imposed penalties varies substantially, ranging from minor fines to very large sanctions exceeding several billion dollars. This heterogeneity in penalty size allows me to strengthen the analysis by examining whether firms' responses vary with

the severity of the violation. To capture this dimension, I create a subset of material violations, based on penalty amounts scaled by firm sales and thresholds derived from the empirical distribution (e.g., mean, median, and upper percentiles), and test their relation to subsequent ESG-related outcomes.

The results provide a nuanced picture of firm behavior following ESG violations. The strongest and most consistent evidence relates to improvements in firms' CSR Activities Score, a ranking provided by the ASSET4 database that captures each company's relative ESG performance compared to its industry peers. Firms subjected to material violations exhibit statistically significant increases in their CSR Activities Score following the violation, with effects persisting over subsequent years, suggesting that at least some firms actively enhance their ESG profile in response to reputational damage. Companies are also more likely to adopt initiatives aimed at reducing their environmental impact following violations. These effects are particularly pronounced in cases of material or repeated violations, while improvements in other types of initiatives — such as environmental investments and labor-related programs — are generally weak, sporadic, or inconsistent across specifications. The evidence for firms strengthening their ESG infrastructure (e.g., ESG committees, employee training programs, ESG-linked executive compensation) is more limited, with statistically significant effects emerging only in a subset of specifications and primarily in cases involving severe penalties. The analysis of quantitative ESG outcomes—including waste generation, energy consumption, injury rates, and donations—does not yield robust evidence of significant improvements. For energy consumption (and in some cases injury rates), the estimated coefficients consistently have the expected negative sign, but are not statistically significant. This lack of significance may, however, partly be explained by the reduced sample size resulting from limited data availability for these outcomes. Finally, the analysis documents some evidence of increased transparency in ESG reporting following violations, especially when violations occur repeatedly over multiple consecutive years. This pattern is consistent with the view that greater reporting transparency may serve reputational repair and risk management objectives after ESG controversies.

From a practical standpoint, the fragmented pattern of results are to some extent consistent with concerns that the ESG domain continues to lack standardized metrics and consistent enforcement. While revealed wrongdoing appears to incentivize firms to prioritize certain ESG dimensions — particularly those related to reported activities and transparency—other areas remain largely unaffected, raising questions about whether existing penalties and reputational consequences are sufficient to induce broader remediation. For policymakers, this pattern suggests that greater standardization and stronger enforcement may be necessary if regulatory violations are to generate substantive, rather than primarily disclosure-based, improvements in firms' ESG practices.

This study contributes to the literature on corporate responses to ESG-related controversies in several ways. Prior research has extensively documented the negative consequences of such events, showing that ESG controversies trigger adverse stock price reactions (Krüger, 2015; Capelle-Blancard & Petit, 2019), increase credit risk (Kölbel et al., 2017), lead to higher audit fees, and even result in auditor resignations (Chakrabarty et al., 2024; Burke et al., 2019). In contrast, the literature on possible remedial actions undertaken by firms remains scarce. To the best of my knowledge, the only study that directly examines this issue is Asante-Appiah and Lambert (2022), who provide some evidence that firms may turn to auditors for external support in the aftermath of ESG controversies. However, much less is known about whether and how firms themselves actively adjust their ESG practices following such violations. This paper aims to fill this gap by examining a broad set of possible internal corporate responses to ESG-related regulatory violations. In doing so, the paper extends the broader remediation literature—traditionally focused on firms' responses to financial-reporting failures and governance-related misconduct (e.g., Farber, 2005; Desai et al., 2006; Karpoff et al., 2008; Brochet & Srinivasan, 2014; Agrawal & Cooper, 2017; Chakrabarty et al., 2024) — into the domain of non-financial, ESG-related violations.

Secondly, I contribute to the growing literature that examines a specific type of adverse ESG event — regulatory violations — as captured in Violation Tracker. Much of the existing research on the consequences of ESG controversies discussed above — including

stock market reactions, credit risk, and audit outcomes — identifies ESG events primarily through negative media coverage. In contrast, this paper uses regulatory violations as a direct and observable measure of ESG misconduct. Regulatory enforcement data offer an arguably more objective and standardized way to identify ESG violations (McCarthy & Dolfsma, 2014; Cohen et al., 2017; Rees & Twedt, 2022). Moreover, regulatory violations capture a distinct class of adverse ESG events, as they reflect formal regulatory actions and may carry different implications for firms’ future risk exposure compared to controversies reported in the media.

To date, most studies using the Violation Tracker database have focused on the factors that help reduce ESG violations, examining the role of internal monitoring (Heese & Pérez-Cavazos, 2020), enterprise resource planning (ERP) system implementation (Heese & Pacelli, 2025), social media exposure (Heese & Pacelli, 2024), and government subsidies (Raghunandan, 2024). The only exception is Chakrabarty et al. (2024), who shift the focus to the consequences of violations, examining their effects on audit pricing. In this paper, I further extend the analysis of the consequences of regulatory violations by studying whether, and how, firms adjust their internal ESG practices after being penalized.

Naturally, the study is subject to certain limitations. First, the voluntary nature of ESG reporting in the U.S. may limit both the availability and consistency of some ESG performance indicators. As observed in the sample, quantitative ESG outcomes are particularly affected by incomplete or selective reporting, introducing potential measurement error. These issues may affect the ability to fully capture firms’ actual improvements following violations. Second, while the analysis includes a rich set of firm-level controls and uses panel specifications to account for unobserved heterogeneity, causal interpretation should be made with caution given the absence of exogenous variation in violation assignments. Unobserved factors influencing both the likelihood of violations and subsequent ESG adjustments may still bias the estimates. Finally, as the study focuses on U.S. public companies and regulatory enforcement data, the generalizability of the findings to other settings with different legal, institutional, or disclosure frameworks may be limited.

The remainder of the paper proceeds as follows. Section 2 reviews the related literature and develops the main hypotheses. Section 3 describes the data sources, introduces the sample construction, and outlines the empirical approach. Section 4 presents the core results, while Section 5 discusses additional analyses and robustness checks. Section 6 concludes with a summary of the findings.

3.2 Literature Review

A company's ESG performance has long been regarded as an important metric, with studies showing that investors and other stakeholders often factor this information into their decision-making. Firms with strong ESG performance tend to enjoy several advantages, such as a lower cost of equity (Ng & Rezaee, 2015), higher returns, greater labor productivity, and increased sales (Flammer, 2015). Over the long run, these companies also tend to outperform their peers in both stock market and accounting terms (Eccles et al., 2014). Debt providers, in some cases, also appear to appreciate ESG-oriented companies. For example, Degryse et al. (2023) find that, following the ratification of the Paris Agreement in 2015, green banks offer cheaper syndicated loans to ESG-oriented companies. Similarly, Amiraslani et al. (2023) show that during times of crisis, companies with higher social capital enjoy lower bond spreads.

At the same time, markets participants also appear to price in ESG-related risks. For instance, Krueger et al. (2020) find that institutional investors consider climate risks financially material and are increasingly incorporating them into investment strategies. Similarly, Choi et al. (2020) show that retail investors are more inclined to sell shares in carbon-intensive firms during unusually warm weather. Matsumura et al. (2014) further reveal a negative correlation between a firm's carbon emissions and its market value. Chava (2014) find that companies with environmental concerns not only face higher expected equity returns, but also suffer loan interest rates, and smaller loan syndicates. Javadi & Masum (2021) show that firms in located in areas more exposed to climate change pay significantly higher spreads on their bank loans.

Given consistent evidence that equity and debt providers value strong ESG commitments and price ESG risks, it is not surprising that negative ESG-related news is typically perceived as a bad signal. Krüger (2015) examines investor reactions to ESG-related news covered by the media between 2001 and 2007 and finds that negative ESG events — such as environmental violations or social controversies — are associated with sharp declines in stock prices of the public U.S. companies. Capelle-Blancard and Petit (2019) focus on an international sample of large multinational firms and similarly document a decline in market prices following the release of negative ESG news. The analysis in this case considers a broader set of news sources—including business media, company press releases, and NGO reports — though the media still appears to trigger the strongest market reactions. Kölbel et al. (2017) focus on an international sample of 539 firms and show that media coverage of corporate social irresponsibility (i.e., behavior that negatively affects the rights of stakeholder groups) leads to an increase in a firm's credit risk, as measured by CDS spreads.

Interestingly enough, the literature shows that external auditors are also attentive to ESG-related news concerning their clients, and revealed ESG misconduct can have adverse consequences for the audit engagement. Burke et al. (2019) analyze a sample of 1 171 U.S. public companies between 2007 and 2014, some of which were subject to negative ESG media coverage. The findings suggest that auditors respond to such negative attention by either increasing their fees or, in cases involving governance-related concerns, by resigning from the engagement. Chakrabarty et al. (2024) investigate how non-financial violations committed by U.S. companies between 2004 and 2015 affected audit-related outcomes. They find that firms involved in such misconduct tend to pay higher audit fees and face an increased likelihood of receiving a going concern opinion. Both studies suggest that the observed reactions (i.e., increased audit fees, higher resignation rates, and a greater likelihood of issuing a GC opinion) are due to the fact that auditors adjust their perception of client risk in response to negative ESG news.

Although the aforementioned literature consistently shows that various groups incorporate ESG risks into their decision-making and react negatively to news regarding cor-

porate ESG misconduct, it largely remains unclear whether the companies involved take steps to repair their damaged reputations and demonstrate a genuine commitment to ESG values. To the best of my knowledge, the only paper that addresses this issue is by Asante-Appiah and Lambert (2022), who hypothesize that firms experiencing ESG-related crises will seek to restore their reputations in order to reassure market participants of their truthful commitment to ESG principles, and that they will likely do so by seeking external assistance from their auditors. The authors argue that the latter can be particularly effective in managing ESG risks due to their experience with enterprise risk management and deep understanding of clients' business structures. Their empirical findings support this hypothesis: there is a positive association between a firm's tainted reputation — measured by abnormal negative ESG-related media coverage — and nonaudit service fees paid to auditors. Moreover, the study shows that this strategy is effective, as increased purchases of nonaudit services during periods of elevated ESG risk help mitigate subsequent declines in market value and operating performance.

This paper aims to deepen the understanding of corporate behavior in response to adverse ESG-related developments. Building on literature that shows third parties perceive such events as unfavorable and reassess firms' risk profiles, I examine whether companies facing ESG-related issues take remedial actions to signal a sincere commitment to ESG principles. Specifically, I test whether firms found to have committed ESG-related violations subsequently strengthen their ESG infrastructure, improve their ESG-related behavior and positioning relative to peers, and increase the transparency of their ESG reporting. The following section provides more detail on the development of the hypotheses and the constructs used to test them.

3.3 Hypotheses Development

3.3.1 Corporate Responses to Misconduct Revelations

Traditional accounting research shows that once financial misconduct is revealed, firms face a serious credibility problem. The discovery of fraud exposes weaknesses in oversight, signals elevated risk to outside stakeholders, and often triggers negative market reactions, legal exposure, and intensified scrutiny from regulators and investors. Prior studies provide a relatively clear picture of how firms attempt to address these problems. Restatements and enforcement actions are commonly followed by governance reforms—such as increasing board independence, enhancing audit committee monitoring, or hiring more reputable auditors (Farber, 2005). Firms also replace implicated executives, including CEOs and CFOs, in an effort to restore confidence in internal oversight and reduce the likelihood of future violations (Desai et al., 2006; Karpoff et al., 2008; Agrawal & Cooper, 2017). In addition, companies engage in broad reputation-repair efforts aimed at investors, employees, customers, and communities, such as public commitments, operational improvements, and CSR-oriented initiatives (Chakrabarty et al., 2014). At the board level, the removal or diminished influence of directors associated with oversight failures serves as another mechanism to signal strengthened monitoring (Brochet & Srinivasan, 2014). Taken together, this literature shows that when misconduct becomes public, firms have both incentives and opportunities to undertake remedial actions — both substantive and symbolic — to mitigate reputational damage, manage regulatory and legal consequences, and prevent future problems.

Building on these insights, I examine whether similar mechanisms operate in the context of ESG-related misconduct. There are strong reasons to expect that ESG-violating firms should care about repairing their reputation. Prior research shows that ESG controversies can trigger negative stock price reactions (Krüger, 2015; Capelle-Blancard & Petit, 2019), increases in credit risk (Kölbel et al., 2017), and adverse audit-related consequences, including higher audit fees and auditor resignations (Burke et al., 2019;

Chakrabarty et al., 2024). Regulatory ESG violations also impose direct financial penalties and are perceived as particularly risk-enhancing because they may introduce cash flow uncertainty and divert managerial attention from core operations (Chakrabarty et al., 2024). These costs give firms incentives to strengthen their ESG practices to avoid further sanctions and restore stakeholder trust. The potential responses studied in this paper — improvements in ESG infrastructure, adoption of new ESG initiatives, enhancements in relative ESG performance, changes in quantitative ESG outcomes, and increases in ESG reporting transparency — parallel the types of remedial actions documented in the financial-misconduct setting. I hypothesize that the revelation of ESG misconduct creates incentives for managers to modify the firm’s ESG practices, much as firms adjust governance and oversight structures after financial-reporting fraud. In fact, because ESG violations relate directly to governance, oversight, and sustainability practices, the conceptual link between the nature of the misconduct and feasible remedial actions is, if anything, even more immediate.

Despite these parallels, the hypotheses regarding firms’ ESG-related responses are not without tension. Unlike financial misstatements — which directly impair earnings quality, invite regulatory enforcement, and reliably affect firm valuation — ESG violations are often perceived as less financially material or less central to core business activities, resulting in more heterogeneous and sometimes limited stakeholder reactions (Khan, Serafeim, & Yoon, 2016; Krüger, 2015). Empirical evidence suggests that many ESG controversies yield penalties that are modest or short-lived, thereby weakening managerial incentives to undertake costly or structural improvements (Capelle-Blancard & Petit, 2019; Kölbel et al., 2017). Moreover, ESG measurement and disclosure remain largely voluntary and weakly standardized in the U.S., granting managers considerable discretion over whether and how to respond (Christensen et al., 2021). This institutional environment allows for symbolic behavior or greenwashing, where firms emphasize communication and reporting rather than substantive changes in ESG practices (Lyon & Montgomery, 2015; Marquis et al., 2016), a point that I attempt to explore by analyzing a broad set of outcomes that span both potentially more symbolic measures — such as reporting transparency — and

more operational ones, such as waste, energy use, and injury rates.

The remainder of this section is structured as follows: I first clarify what constitutes ESG misconduct in my sample and explain why regulatory violations represent a conceptually and empirically suitable setting for studying firms' responses and then outline the expected links between revealed misconduct and potential remedial actions. The outcome set is intentionally broad, given the early stage of research in this area, the study's exploratory orientation, and the opportunity to analyze a range of ESG dimensions that span symbolic adjustments — such as changes in reporting practices — and more substantive improvements in performance.

3.3.2 Regulatory ESG Violations as Misconduct Events

In the current version of the paper, I focus on a specific type of negative company-level ESG event — regulatory violations — and hypothesize that such incidents may lead to changes in companies' ESG-related practices. Compared to the negative media coverage examined in many existing studies (e.g., Köbel et al., 2017; Capelle-Blancard & Petit, 2019; Burke et al., 2019), regulatory violations represent a distinct form of ESG risk as they present formally recognized instances of ESG misconduct and involve financial penalties. Chakrabarty et al. (2024) emphasize that such violations are perceived by external stakeholders as particularly risk-enhancing, as they can introduce cash flow uncertainty and divert management's attention from core operations. Building on this perspective, I propose that ESG regulatory violations serve as credible signals of heightened ESG risk, prompting companies to improve their ESG practices in an effort to restore stakeholder trust and reduce future risk exposure. Specifically, I hypothesize that firms penalized for ESG-related violations — such as non-compliance with safety, environmental, employment, or healthcare regulations — may seek to mitigate the reputational and operational consequences of the violation by strengthening their commitment to ESG-related practices.

Primarily focusing on officially recognized violations offers an additional methodological

advantage — arguably a more standardized and comprehensive sample of firms facing ESG-related problems. Since regulatory agencies operate within a legal framework, their enforcement actions are expected to be less selective and less prone to bias. In contrast, media coverage is more likely to be influenced by the personal beliefs of content creators (McCarthy & Dolfsma, 2014) and may introduce biases when reporting on corporate news (e.g., Cohen et al., 2017; Rees & Twedt, 2022). Thus, concentrating exclusively on companies with negative media coverage could potentially lead to an overestimation of ESG-related problems for some firms while overlooking issues faced by others. That said, further comparison of the firms’ responses to regulatory violations versus negative media coverage could be a fruitful research avenue.

3.3.3 ESG Remediation Outcomes

Drawing on prior literature and available data on corporate ESG-related outcomes, I develop a set of hypotheses on how firms may respond when seeking to manage elevated ESG risks and reaffirm their ESG commitments. The set of potential ESG outcomes is adapted from Fiechter et al. (2022), who examine the real effects of the EU’s ESG Directive—a recent regulation requiring large listed firms to disclose key environmental, social, and governance information. Their framework provides a comprehensive set of ESG measures that are both empirically accessible and theoretically aligned with the focus of this study.

When confronted with ESG-related challenges, a company may first seek to strengthen its ESG infrastructure, with the aim of improving its overall ESG positioning. Prior studies (e.g., Farber, 2005; Marciukaityte et al., 2006; Wilson, 2008) show that firms respond to restatements and other forms of financial misconduct by enhancing corporate governance — for instance, by increasing board independence, creating new oversight committees, or revising executive compensation schemes — as a way to prevent future violations, restore credibility, and signal commitment to improved oversight. In a similar vein, I argue that firms penalized for ESG regulation violations may respond by investing in their ESG

infrastructure to reduce the likelihood of future ESG breaches and to signal a renewed commitment to ESG principles. This leads to my first hypothesis:

H1a: Firms penalized for violating ESG regulations are more likely to enhance their ESG infrastructure.

where ESG infrastructure may include features such as a dedicated ESG committee, employee ESG training programs, and ESG-linked executive compensation.

Beyond infrastructural changes, companies may also seek to improve their public ESG profile by launching new initiatives with visible environmental or social impacts. Such ESG initiatives — for example, energy efficiency programs, waste reduction efforts, and actions promoting diversity, human rights, and employee well-being — serve to demonstrate proactive ESG engagement and rebuild stakeholder trust. Hence, alongside strengthening ESG infrastructure, firms may increase the number of ESG initiatives following an ESG violation:

H1b: Firms penalized for violating ESG regulations are more likely to launch new ESG initiatives.

Additionally, following Fiechter et al. (2022), I extend the analysis by employing a broader ESG-related outcome and test whether firms penalized for ESG violations exhibit changes in their CSR Activity Score. This score, which combines Environmental and Social Scores from ASSET4, reflects a firm's industry-relative performance based on self-reported policies, activities, and practices. It captures efforts in managing environmental impacts and social responsibilities, including stakeholder engagement, workforce welfare, and community involvement. Assuming penalized firms are motivated to enhance ESG commitments, and given that the CSR Activity Score reflects proactive ESG efforts, I hypothesize the following:

H1c: Firms penalized for violating ESG regulations are more likely to demonstrate improvements in their CSR Activity Score.

Next, I turn to a set of quantitative ESG outcomes that are expected to reflect the real effects of firms' ESG-related responses. These measures — waste reduction, energy consumption, workplace injury rates, and corporate donations — capture concrete, observable changes that result from firms' investments in ESG infrastructure and initiatives. By analyzing changes in these outcomes, I aim to investigate whether firms' potential responses to ESG violations translate into substantive improvements in ESG performance, beyond symbolic or window dressing adjustments. Consistent with the rationale underlying the preceding hypotheses, I suggest that firms found to have breached ESG regulations may seek to mitigate the negative consequences of such events by demonstrating a renewed and genuine commitment to ESG principles. This commitment, if genuine, is expected to manifest in improved performance across key quantitative ESG outcomes:

H2: Firms penalized for violating ESG regulations are more likely to reduce waste generation, energy consumption, and workplace injury rates, and to increase charitable donations.

Finally, I hypothesize that firms penalized for ESG violations and seeking to minimize adverse consequences of that misbehavior will alter the transparency of their ESG reporting. Despite the growing significance of ESG disclosures, reporting standards in the U.S. remain largely voluntary, with no unified framework mandating comprehensive or standardized disclosures. As a result, firms retain considerable discretion over whether to disclose ESG information, as well as the content, depth, and format of such disclosures. This flexibility allows companies to use ESG reporting strategically — even in the absence of real changes in ESG commitment — to project a more favorable public image. Combined with the preceding hypotheses, which suggest also improvements in genuine commitment to ESG principles, I propose that revealed violations can trigger increased reporting transparency — whether as a symbolic effort, a reflection of real actions, or a combination of both:

H3: Firms penalized for violating ESG regulations are more likely to increase transparency of their ESG reporting.

Borrowed from Fiechter et al. (2022), transparency score reflects the availability of an ESG report itself, its coverage of global activities, adherence to widely recognized ESG reporting frameworks (such as GRI or OECD), and the presence of an external assurer. Consistent with the stated hypothesis, prior accounting research has found these elements to play significant role in shaping positive image of a firm. For instance, Dhaliwal et al. (2011) examine a sample of U.S. firms that issued their first standalone ESG reports between 1993 and 2007 and show that such disclosures — especially when supported by strong ESG performance — can serve as a strategic tool to lower the cost of equity capital. Christensen (2016) shows that firms publishing ESG reports are less likely to suffer severe stock price declines following high-profile ESG-related misconducts, especially when they adhere to GRI guidelines. Complementing the picture, some other studies (e.g., Simnett et al., 2009; Pflugrath et al., 2011; and Casey & Grenier, 2015) find that third-party assurance can further enhance the credibility of ESG disclosures.

Taken together, these findings support the idea that increased ESG reporting transparency can be a strategic tool for managing stakeholder perceptions and addressing reputational damage following ESG-related violations. Interestingly, Christensen (2016) finds that firms engaging in ESG reporting are less likely to commit ESG-related misconduct, suggesting that transparency may have a disciplining effect. Therefore, ESG reporting transparency, when adopted in response to a violation, can itself serve as a starting point for the substantive changes in ESG practices outlined in the prior hypotheses.

3.4 Research Design

Having outlined the hypothesized relationships between ESG regulation violations and the actions firms may take to reduce associated risks and demonstrate genuine commitment to ESG principles, I now turn to a detailed description of the econometric model and the key variables used in the analysis. The general model used to examine the relationship between ESG violations and subsequent ESG-related firm outcomes is estimated using a

linear fixed effects OLS model:

$$ESG_Outcome_{i,t} = \beta_0 + \sum_{k=0}^n \beta_k ESG_Violation_{i,t-k} + \mathbf{X}'_{i,t} \gamma + \alpha_i + \delta_t + \varepsilon_{i,t}$$

which firm fixed effects control for unobserved characteristics that do not vary over time, while year fixed effects capture common shocks across firms. Both the dependent variable and the main explanatory variables differ across model specifications and are described in details in the following subsections. In all cases, standard errors are clustered at the firm level to account for within-firm serial correlation. The set of control variables closely follows those used in prior studies examining various determinants of ESG-related outcomes (e.g., Simnett et al., 2009; Casey & Grenier, 2015; Fiechter et al., 2022), and includes Size (the natural logarithm of total assets), Return on Assets (net income divided by total assets), Long-Term Debt (the ratio of long-term debt to total assets), Market-to-Book ratio, and Liquidity (the ratio of current assets to current liabilities).

3.4.1 Main Variable of Interest

The main variable of interest, *ESG_VIOL*, is constructed using data from the Violation Tracker, which documents non-financial corporate violations in the United States. This database compiles enforcement actions and monetary penalties exceeding \$ 5 000 issued by federal, state, and local regulatory agencies. From the wide range of violation types covered by the data provider, I focus on four ESG-related categories: employment, environmental, healthcare, and safety regulation violations. Following prior research that uses this database to examine various causes (e.g., Raghunandan, 2021; Heese & Pacelli, 2025) and consequences (e.g., Chakrabarty et al., 2024) of non-financial misconduct, I aggregate all selected violations at the parent-company level. Consistent with Raghunandan (2021) and Chakrabarty et al. (2024), companies that do not appear in the database are assumed to have never committed ESG-related misconduct or received a corresponding penalty. These firms constitute a sort of control group, although not in the strict causal

inference sense, since the study does not follow a clear difference-in-differences structure and the non-violator status is not randomly assigned.

To conduct a comprehensive analysis, I use several alternative approaches to construct the main variable of interest. In the first case, the *ESG_VIOL* variable is a dummy that takes the value of one if a company commits **any penalized violation** in a given year, and zero otherwise. However, as discussed further below, the distribution of penalty amounts is highly left-skewed, with many firms subject to only minor penalties. Therefore, I apply several methods to identify high-penalty cases — hereafter referred to as material violations — and analyze their relationship with subsequent ESG outcomes. In each case, *ESG_VIOL* is defined as a binary variable equal to one if a firm receives a **material penalty** in a given year, and zero otherwise. To construct the materiality measures, penalty amounts for individual violations are summed if a firm incurs multiple violations during the same year. The rationale for this threshold is twofold: it ensures that the identified violations remain economically meaningful relative to firm’s annual sales, and, at the same time, it yields a balanced distribution between material and non-material violations (around 23% cases are classified as material and 77% as non-material). Higher thresholds substantially reduce the number of material observations, thereby diminishing the meaningfulness of the analysis. Alternatively, I classify penalties as material if they exceed, on a yearly basis, the mean, median, 75th percentile, or 90th percentile of the penalty distribution.

Given that the exact timing of expected changes in ESG-related outcomes is uncertain, different model specifications include violations occurring at different points in time. On the one hand, regulatory penalties are typically the result of prolonged processes involving investigations, evidence gathering, and internal communication, which may give firms time to anticipate negative ESG disclosures and begin corrective actions beforehand. On the other hand, ESG-related initiatives are complex and investment-intensive, with measurable effects likely to be lagged over time. Hence, to capture the dynamics of ESG-related changes, I estimate alternative specifications that include (i) only current-year penalized violations; (ii) current-year penalized violations along with a dummy in-

dicating at least one violation in three preceding years; and (iii) current-year penalized violations together with separate dummies for violations in each of the three prior years. Additionally, I attempt to capture the effect of repeated, sequential violations. To do so, I run separate model specifications that include indicators for whether the current or any of the previous years' violations were sequential (i.e., occurred in two or more consecutive years).

3.4.2 Outcome Variables

The dependent variable, *ESG_Outcome*, is defined according to the specific hypothesis being tested. The variable ***ESG Infrastructure***, which examines the relationship between penalized ESG violations and improvements in a firm's internal ESG governance, captures three key elements: the existence of a dedicated ESG committee, the presence of employee training programs, and the inclusion of ESG-related goals in executive compensation. Firms receive a score ranging from 0 to 3 based on how many of these components they have implemented. To evaluate whether ESG violations are associated with the implementation of ESG-related initiatives, the analysis includes three variables: ***Environmental Impact Reduction Initiatives***, ***Environmental Investment Initiatives***, and ***Labor-related Initiatives***. Each variable measures the number of relevant initiatives adopted by a firm within its respective category. A detailed list of these initiatives is provided in the Appendix. The ***CSR Activities Score***, used to test Hypothesis 1c, is constructed by summing the Environmental and Social Scores from ASSET4. This score reflects a company's performance relative to industry peers in areas such as human rights, community engagement, emissions, and resource use.

To assess whether firms identified as ESG regulation violators improve their measurable ESG outcomes — i.e., whether these responses lead to observable real effects — the analysis also considers the following variables: ***Waste Generation*** (tonnes of waste per year), ***Energy Consumption*** (giga-joules used annually), ***Injury Rates*** (Labor-related accidents and deaths per one million hours worked), and ***Donations*** (total monetary

contributions made by the firm). All variables except Injury Rates are scaled by total assets.

Finally, the *ESG Reporting Transparency Score* assesses the scope and quality of ESG disclosures. It is based on four components: the availability of a ESG report, its coverage of global operations, the use of GRI or OECD frameworks, and the presence of external assurance. Each component adds one point to the total score, resulting in a possible range from 0 to 4.

3.5 Sample Selection and Descriptives

I first download ESG data from Refinitiv to construct the outcome variables. Then, these observations are merged with enforcement records from Violation Tracker, which include information on the type and category of offense, the issuing agency, the year of the verdict, and the penalty imposed. Firms that do not appear in this database are assumed not to have committed ESG-related violations or incurred related penalties. The sample covers the period from 2000 to 2023, in line with the maximum coverage available in Violation Tracker. Table 3.1 summarizes the sample selection process for each outcome variable. For ESG Infrastructure, Initiatives, CSR Activities Score, and Reporting Transparency, the final samples contain slightly fewer than 22 000 firm-year observations. In contrast, data availability for quantitative ESG outcomes is considerably more limited, ranging from 4 324 firm-year observations for waste generation to 6 537 for energy consumption.

Table 3.1: Sample Selection

	Infrastructure	ESG Initiatives	CSR Act. Score	Rep. Transparency
Firm-year obs. with ESG data	28 147	28 034	28 156	28 041
<i>less: observations with no controls</i>	(6 207)	(6 187)	(6 206)	(6 187)
Final sample:	21 940	21 849¹	21 950	21 854
<i>ESG violation = 0</i>	15 122	15 054	15 132	15 058
<i>ESG violation = 1</i>	6 818	6 793	6 818	6 796
	Waste	Energy	Injury Rate	Donations

¹The number refers to the labor-related initiatives; in the case of the environmental impact reduction (environmental investments) the number goes down to 21 839 (21 847) firm-year observations

Table 3.1: Sample Selection (continued)

Firm-year obs. with ESG data	4 959	7 423	5 883	6 791
<i>less: observations with no controls</i>	(635)	(886)	(224)	(1 461)
Final sample:	4 324	6 537	5 659	5 330
<i>ESG violation = 0</i>	2 241	3 577	3 010	2 644
<i>ESG violation = 1</i>	2 083	2 960	2 649	2 686

Table 3.2 presents descriptive statistics for the dependent and control variables used in the analysis. On average, firms in the sample have ESG Infrastructure and Reporting Transparency scores of 1, suggesting that they typically implement just one of the available elements. The median Infrastructure Score is also 1, while the median Reporting Transparency Score is 0, indicating that many firms do not publicly report their CSR Activities at all. Among ESG initiatives, Labor-related actions are the most commonly adopted (3 initiatives on average), followed closely by Environmental Impact Reduction Initiatives (2 on average), whereas Environmental Investments remain rare. Firms in the 75th percentile report the maximum number of Labor-related and Environmental Impact Reduction Initiatives. As for quantitative ESG outcomes, the average firm generates around 5 580 thousand tonnes of waste, consumes approximately 34 500 thousand gigajoules of energy, and donates 426 000 thousand annually. The mean injury rate is 6.76 incidents per one million hours worked. Regarding firm-level control characteristics, the mean firm size (log of total assets) is 14.75, while the market-to-book ratio averages 1.95. The average ROA is slightly negative (-0.024), indicating that a portion of firms in the sample includes loss-making companies. Long-term debt and liquidity levels average 0.24 and 2.91, respectively.

Table 3.2: Descriptive Statistics

Variable	Mean	Min	P25	Median	P75	Max	SD
Dependent Variables							
<i>ESG Infrastructure</i>	1	0	0	1	2	3	1.11
<i>ESG Initiatives</i>							
<i>Env. Impact Reduction</i>	2	0	0	2	5	7	2.33
<i>Environmental Investment</i>	0	0	0	0	0	2	0.62
<i>Labor-related</i>	3	0	1	2	4	5	1.75
<i>CSR Activities Score</i>	69.92	0.38	31.06	57.59	105.15	193.51	46.32

Quantitative Outcomes

<i>Waste</i> (thsd tonnes)	5 580	0	8	41	177	211 000	52 500.00
<i>Energy Use</i> (thsd GJ)	34 500	0	529	2 629	13 600	3 740 000	141 000.00
<i>Injury Rate</i> (accid./mn hrs)	7	0	2	4	9	179	8.43
<i>Donations</i> (thsd \$)	426 000	0	1 500	6 543	25 000	752 000 000	13 700 000

ESG Reporting Transparency	1	0	0	0	2	4	1.46
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Control Variables

<i>SIZE</i>	14.75	7.46	13.49	14.83	16.05	19.32	1.92
<i>ROA</i>	-0.024	-2.44	-0.017	0.038	0.083	0.301	0.27
<i>LTDEBT</i>	0.235	0	0.055	0.211	0.346	1.061	0.21
<i>MTB</i>	1.95	0.040	0.653	1.218	2.343	14.24	2.19
<i>LIQUID</i>	2.91	0.193	1.210	1.816	3.001	24.17	3.53

The following tables provide details on the main variable of interest — ESG-related violations. Table 3.3 shows the distribution of violations by year and category; Table 3.4 presents the distribution of non-zero penalties amounts across the sample; and Table 3.5 reports how firms are distributed according to the number of years in which they were penalized for violating ESG-related violations. The final sample includes 31 748 regulatory violations, with some firms incurring multiple infractions across different categories within a single year (hence, the number of total violations is higher than the number of firm-year observations where at least one violation has occurred). More than half of these violations pertain to noncompliance with safety regulations. The data reveal a marked increase in penalized ESG violations over the past two decades, particularly in the areas of workplace safety and environmental compliance.

Table 3.3: Distribution of ESG violations

Year	Employment	Environmental	Health	Safety	Total
2002	53	189	2	309	553
2003	88	188	0	345	621
2004	148	219	5	433	805
2005	159	318	2	477	956
2006	146	308	2	557	1 013
2007	173	296	4	736	1 209
2008	160	411	8	890	1 469
2009	172	377	6	930	1 485
2010	198	399	10	1 000	1 607
2011	182	408	8	1 033	1 631
2012	177	404	8	996	1 585
2013	123	400	13	875	1 411
2014	141	425	17	703	1 286
2015	216	449	29	977	1 671

Table 3.3: Distribution of ESG violations (continued)

Year	Employment	Environmental	Health	Safety	Total
2016	313	500	63	1 086	1 962
2017	307	429	43	1 164	1 943
2018	215	430	57	1 121	1 823
2019	199	451	61	1 155	1 866
2020	154	452	91	807	1 504
2021	157	382	113	990	1 642
2022	179	386	129	1 224	1 918
2023	230	349	63	1 146	1 788
Total	3 890	8 170	734	18 954	31 748

Table 3.4 summarizes the distribution of non-zero ESG-related penalties paid in a single year across violation categories. Penalty amounts are highly skewed, with a few extreme values driving up the mean. While the median total yearly paid penalty is \$66 000, the average exceeds \$25 million, and the maximum reaches over \$15.5 billion. While safety and environmental violations are the most common (Table 3.3), health-related cases imply the highest penalties, including a median of over \$1.1 million and a maximum of \$8 billion. Overall, these figures reveal the presence of a small number of severe violations amid a broader set of relatively modest penalties, which leads me to focus a substantial part of the analysis on a subsample of material violations.

Table 3.4: Summary Statistics of Non-Zero Penalties

Variable	Mean	Min	P25	Median	P75	Max
<i>Total</i>	25 463 281	5 000	17 669	66 000	488 361	15 552 542 000
<i>Health</i>	401 771 752	6 000	206 212	1 140 260	49 333 073	8 001 000 000
<i>Employment</i>	4 744 438	5 000	25 000	130 222	1 755 353	694 655 089
<i>Environmental</i>	11 525 378	5 000	16 870	63 045	339 000	9 356 975 000
<i>Safety</i>	13 620 625	5 000	9 938	22 150	57 455	15 548 542 000

Finally, Table 3.5 presents the distribution of firms by the number of years in which they were penalized for ESG violations, which allows for an evaluation of the presence of subsequent violators. Column 1 shows that 1 521 firms in the sample did not incur any violations during the entire sample period, while the remaining 902 firms were penalized in at least one year. Among these, a substantial proportion received penalties in

multiple years. The following columns display the distribution of different measures of material ESG violations used in the analysis. When materiality is defined as a penalty exceeding 0.01% of annual sales (Column 2), 1 922 firms had no material violations over the observation period, 212 firms incurred such violations only once, and the remainder were penalized in multiple years. A similar pattern is observed in Columns 3 to 6, where alternative thresholds are used to define materiality. As the criteria become more stringent, the number of firms with no material violations increases slightly, yet a significant share of firms still exhibit repeated violations.

Table 3.5: Distribution of Violations by Years

N Years	All	Material				
		SALES_SCALED	MEAN	MED	P75	P 90
0	1 521	1 922	2 241	1 759	1 956	2 158
1	143	212	87	178	173	118
2	95	94	35	94	82	55
3	82	62	17	82	43	29
4	80	42	15	45	36	13
5	54	21	9	39	24	14
6	57	18	5	26	13	10
7	48	20	3	31	21	4
8	33	10	4	19	16	4
9	38	8	3	24	15	2
10	25	4	0	9	5	5
More than 10	247	10	4	117	39	10

3.6 Empirical Results

With the sample and key variable distributions now described, I proceed to the discussion of the results. Due to the various combinations of ESG-related outcomes, differently constructed violation variables, and multiple model specifications, I present the full results for each hypothesis only for the ESG Violation variable, which equals 1 whenever a company receives any penalty for an ESG-related violation. Results based on alternative violation variables — designed to reflect varying levels of violation materiality — are reported in a summarized form. All the regressions discussed below include year and firm fixed effect, standard errors are clustered at the firm level.

3.6.1 ESG Infrastructure

The first set of regressions tests whether violations of ESG regulations are associated with subsequent improvements in corporate ESG infrastructure. As reported in Table 3.6, none of the VIOL coefficients are statistically significant at the 10% level. There is no evidence that companies receiving penalties for ESG violations improve their ESG infrastructure either in the same year (Model 1) or in the years that follow (Models 2 and 3).

Table 3.6: ESG Infrastructure: All Violations Dummy

	INFR.M1	INFR.M2	INFR.M3	INFR.M4	INFR.M5
<i>VIOL_CURR</i>	0.005 (0.35)	0.005 (0.33)	0.006 (0.41)	-0.003 (-0.21)	-0.003 (-0.23)
<i>VIOL_PR_3_YRS</i>		0.005 (-0.22)			-0.015 (-0.59)
<i>VIOL_PR_1</i>			-0.007 (-0.45)		
<i>VIOL_PR_2</i>			-0.003 (-0.16)		
<i>VIOL_PR_3</i>			0.005 (0.34)		
<i>VIOL_CURR*SEQ</i>				0.017 (0.76)	0.018 (0.80)
<i>VIOL_PR_3_YRS*SEQ</i>					0.017 (0.67)
SIZE	0.141*** (7.34)	0.141*** (7.34)	0.149*** (7.55)	0.141*** (7.32)	0.141*** (7.31)
ROA	-0.150*** (-5.11)	-0.151*** (-5.11)	-0.153*** (-5.06)	-0.150*** (-5.10)	-0.150*** (-5.10)
LTDEBT	0.070 (-1.12)	-0.070 (-1.13)	-0.076 (-1.19)	-0.069 (-1.12)	-0.070 (-1.12)
MTB	0.013*** (2.79)	0.013*** (2.80)	0.015*** (2.87)	0.013*** (2.79)	0.013*** (2.78)
LIQUID	0.001 (0.41)	0.001 (0.43)	-0.001 (-0.51)	0.001 (0.42)	0.001 (0.40)
Const.	-2.397***	-2.395***	-2.466***	-2.394***	-2.395***
R-squared	0.491	0.491	0.484	0.491	0.491
Observations	21 940	21 940	21 211	21 940	21 940

Restricting the analysis to material violations —those involving penalties above a specified threshold —yields slightly stronger results. There is limited support for the idea that firms respond by improving their ESG infrastructure — establishing a dedicated

ESG committee, launching employee training programs, or integrating ESG-related objectives into executive compensation schemes — within the same year the penalty is imposed.

Table 3.7: ESG Infrastructure: Material Violations Dummies

	SALES_CUT	MEAN_CUT	MED_CUT	P75_CUT	P90_CUT
Model 1					
<i>VIOL_CURR</i>	0.039** (2.23)	0.010 (0.34)	0.017 (1.07)	0.036** (2.00)	0.007 (0.27)
Model 2					
<i>VIOL_CURR</i>	0.038** (2.15)	0.010 (0.33)	0.018 (1.08)	0.036* (1.95)	0.007 (0.28)
<i>VIOL_PR_3_YRS</i>	-0.014 (-0.72)	-0.007 (-0.26)	0.014 (0.69)	-0.013 (-0.67)	-0.015 (-0.15)
Model 3					
<i>VIOL_CURR</i>	0.039** (2.18)	0.011 (0.35)	0.019 (1.18)	0.038** (2.10)	0.006 (0.22)
<i>VIOL_PR_1</i>	-0.003 (-0.16)	-0.019 (-0.65)	0.000 (0.02)	0.003 (0.18)	-0.010 (-0.38)
<i>VIOL_PR_2</i>	-0.016 (-0.89)	-0.048 (-1.53)	0.010 (0.66)	-0.017 (-0.91)	-0.043* (-1.71)
<i>VIOL_PR_3</i>	0.001 (0.08)	-0.014 (-0.44)	0.028* (1.68)	-0.001 (-0.05)	-0.014 (-0.48)
Model 4					
<i>VIOL_CURR</i>	0.041** (2.44)	0.037 (1.23)	0.011 (0.76)	0.030* (1.68)	0.017 (0.66)
<i>VIOL_CURR*SEQ</i>	-0.008 (-0.22)	-0.119* (-1.86)	0.016 (0.62)	0.020 (0.55)	-0.040 (-0.72)
Model 5					
<i>VIOL_CURR</i>	0.038** (2.07)	0.037 (1.18)	0.013 (0.80)	0.027 (1.42)	0.016 (0.59)
<i>VIOL_PR_3_YRS</i>	-0.026 (-1.11)	0.021 (0.58)	0.009 (0.39)	-0.024 (-0.98)	-0.005 (-0.15)
<i>VIOL_CURR*SEQ</i>	0.000 (0.01)	-0.125* (-1.82)	0.012 (0.45)	0.027 (0.71)	-0.035 (-0.61)
<i>VIOL_PR_3_YRS*SEQ</i>	0.037 (1.17)	-0.049 (-1.14)	0.008 (0.29)	0.021 (0.67)	-0.020 (-0.47)

Table 3.7 presents results based on various definitions of material violation dummies. In Column (1), a violation is classified as material if the penalty exceeds 0.01% of the firm's sales in the year it is imposed. In Column (2), a violation is considered material if the penalty amount exceeds the mean penalty imposed in that year. Columns (3), (4), and (5) define violations as material if the penalty amount falls above the 50th, 75th, and 90th percentiles, respectively, of the distribution of penalties within a given year. Models (1) to

(5) represent alternative specifications: Model (1) includes only current-year violations; Model (2) adds a dummy variable indicating whether a violation occurred in any of the three preceding years; Model (3) adds separate dummy variables for each of the preceding three years; and Models (4) and (5) further incorporate indicators capturing sequential violations over time.

The results indicate that the *VIOL* coefficients are statistically significant only when material violations are defined as those exceeding 0.01% of the firm's sales or falling above the 75th percentile of penalties imposed in a given year. Moreover, there is no evidence that previously imposed penalties have any meaningful effect on the ESG infrastructure in place: none of the *VIOL_PR* coefficients are statistically significant in any of the specifications. Taken together, the results offer at best very limited support for Hypothesis 1a. If firms respond to ESG-related penalties by improving their general ESG infrastructure, they do so only in rare cases of severe violations and even then, the evidence suggests that such responses are modest and short-term in nature.

3.6.2 ESG Initiatives

The next subsection presents the analysis of more specific ESG-related outcomes that may be adjusted in response to ESG violations — targeted ESG initiatives. Table 3.8 reports the results for model specifications using the ESG Violation variable, which takes the value of 1 whenever any company receives a penalty, regardless of the materiality threshold. There is no evidence that firms initiate new Environmental Investments or Labor-related initiatives following the imposition of a penalty. The estimated coefficients are never statistically significant, and in some cases, have the opposite sign. However, there is some limited evidence suggesting a positive effect on Environmental Impact Reduction Initiatives. In Models 1 through 3, the coefficients for both current and lagged violations are in the expected direction and, in some cases, come close to reaching statistical significance at the 10% level. Notably, the results indicate that the positive association is particularly strong in cases of sequential violations — those that occur repeatedly over

multiple years. Both interaction terms, $VIOL_CURR*SEQ$ (equal to one if the current-year violation is part of a sequence) and $VIOL_PR_3_YRS*SEQ$ (equal to one if any of the previous three years included sequential violations), are positive and statistically significant at the 1% level.

Table 3.8: ESG Initiatives: All Violations Dummy

<i>Panel A: Environmental Impact Reduction Initiatives</i>					
	E_RED_M1	E_RED_M2	E_RED_M3	E_RED_M4	E_RED_M5
<i>VIOL_CURR</i>	0.036 (1.10)	0.036 (1.08)	0.041 (1.21)	-0.013 (-0.44)	-0.011 (-0.34)
<i>VIOL_PR_3_YRS</i>		0.002 (0.05)			-0.073 (-1.29)
<i>VIOL_PR_1</i>			0.033 (0.94)		
<i>VIOL_PR_2</i>			0.051 (1.44)		
<i>VIOL_PR_3</i>			0.063* (1.84)		
<i>VIOL_CURR*SEQ</i>				0.104** (2.08)	0.102** (2.02)
<i>VIOL_PR_3_YRS*SEQ</i>					0.133** (2.53)
Controls	Yes	Yes	Yes	Yes	Yes
R-squared	0.516	0.516	0.504	0.516	0.517
Observations	21 839	21 839	21 111	21 839	21 839
<i>Panel B: Environmental Investments Initiatives</i>					
	E_INV_M1	E_INV_M2	E_INV_M3	E_INV_M4	E_INV_M5
<i>VIOL_CURR</i>	-0.001 (-0.11)	-0.001 (-0.12)	0.001 (0.06)	0.004 (0.38)	0.004 (0.37)
<i>VIOL_PR_3_YRS</i>		-0.004 (-0.23)			-0.013 (-0.76)
<i>VIOL_PR_1</i>			-0.018 (-1.35)		
<i>VIOL_PR_2</i>			-0.005 (-0.35)		
<i>VIOL_PR_3</i>			-0.004 (-0.35)		
<i>VIOL_CURR*SEQ</i>				-0.011 (-0.63)	-0.012 (-0.66)
<i>VIOL_PR_3_YRS*SEQ</i>					0.024 (1.31)
Controls	Yes	Yes	Yes	Yes	Yes
Const.	-0.63*** (-3.14)	-0.628*** (-3.12)	-0.651*** (-3.07)	-0.632*** (-3.15)	-0.639*** (-3.17)
R-squared	0.111	0.111	0.102	0.111	0.112
Observations	21 847	21 847	21 119	21 847	21 847
<i>Panel C: Labor-related Initiatives</i>					
	LAB_M1	LAB_M2	LAB_M3	LAB_M4	LAB_M5
<i>VIOL_CURR</i>	0.006	0.005	0.005	0.006	0.001

	(0.29)	(0.24)	(0.21)	(0.28)	(0.03)
<i>VIOL_PR_3_YRS</i>		-0.032			-0.024
		(-1.10)			(-0.71)
<i>VIOL_PR_1</i>			-0.021		
			(-0.93)		
<i>VIOL_PR_2</i>			-0.023		
			(-1.06)		
<i>VIOL_PR_3</i>			-0.026		
			(-1.15)		
<i>VIOL_CURR*SEQ</i>				0.001	0.009
				(0.03)	(0.29)
<i>VIOL_PR_3_YRS*SEQ</i>					-0.021
					(-0.64)
Controls	Yes	Yes	Yes	Yes	Yes
Const.	-3.411***	-3.394***	-3.415***	-3.411***	-3.384***
	(-8.53)	(-8.48)	(-8.15)	(-8.53)	(-8.44)
R-squared	0.656	0.656	0.651	0.656	0.656
Observations	21 849	21 849	21 121	21 849	21 849

Table 3.9 presents the results examining the relationship between material ESG regulation violations and the implementation of Environmental Impact Reduction Initiatives. Focusing exclusively on material violations yields statistically significant findings—particularly when materiality is defined based on sales-scaled penalties, penalties exceeding the annual mean, or those in the top 10th percentile of the penalty distribution. In contrast to the results for general ESG infrastructure, the coefficients for lagged violations are also statistically significant (Models 2 and 3), suggesting that firms may implement Environmental Impact Reduction Initiatives with a delay in response to past ESG-related penalties. However, the inclusion of sequential violation indicators in Models 4 and 5 does not yield additional insights, as the corresponding coefficients are not statistically significant. In terms of economic significance, depending on the materiality definition, material violations are associated with the adoption of approximately 0.1 to 0.2 additional environmental impact reduction initiatives, corresponding to about a 5–10% increase relative to the average number of initiatives.

Table 3.9: Environmental Impact Reduction Initiatives: Material Violations Dummies

	SALES_CUT	MEAN_CUT	MED_CUT	P75_CUT	P90_CUT
Model 1					
<i>VIOL_CURR</i>	0.087**	0.209***	0.042	0.072	0.109**

Table 3.9: Environmental Impact Reduction Initiatives: Material Violations Dummies (continued)

	SALES_CUT (2.06)	MEAN_CUT (3.08)	MED_CUT (1.17)	P75_CUT (1.60)	P90_CUT (1.98)
Model 2					
<i>VIOL_CURR</i>	0.091** (2.12)	0.212*** (3.10)	0.043 (1.18)	0.073 (1.61)	0.108** (1.97)
<i>VIOL_PR_3_YRS</i>	0.096** (2.16)	0.144*** (2.67)	0.041 (0.97)	0.064 (1.40)	0.085* (1.65)
Model 3					
<i>VIOL_CURR</i>	0.092** (2.17)	0.205*** (2.95)	0.052 (1.45)	0.068 (1.53)	0.098* (1.80)
<i>VIOL_PR_1</i>	0.092** (2.17)	0.208*** (3.15)	0.065* (1.78)	0.080* (1.90)	0.107* (1.92)
<i>VIOL_PR_2</i>	0.060 (1.41)	0.116* (1.79)	0.076** (2.06)	0.051 (1.21)	0.050 (0.93)
<i>VIOL_PR_3</i>	0.056 (1.30)	0.059 (0.96)	0.064* (1.76)	-0.002 (-0.04)	-0.005 (-0.09)
Model 4					
<i>VIOL_CURR</i>	0.084** (2.11)	0.188*** (2.86)	-0.021 (-0.66)	0.054 (1.28)	0.122** (2.26)
<i>VIOL_CURR*SEQ</i>	0.010 (0.13)	0.095 (0.78)	-0.005 (-0.23)	0.058 (0.72)	-0.054 (-0.54)
Model 5					
<i>VIOL_CURR</i>	0.101** (2.29)	0.211*** (3.02)	-0.016 (-0.45)	0.064 (1.38)	0.132** (2.33)
<i>VIOL_PR_3_YRS</i>	0.098* (1.78)	0.244*** (3.23)	-0.033 (-0.69)	0.061 (1.07)	0.157** (2.27)
<i>VIOL_CURR*SEQ</i>	-0.042 (-0.48)	-0.010 (-0.07)	-0.013 (-0.55)	0.030 (0.35)	-0.100 (-0.94)
<i>VIOL_PR_3_YRS*SEQ</i>	0.003 (0.05)	-0.221** (-2.50)	0.040* (1.86)	0.025 (0.95)	-0.169** (-2.03)

The analysis of the relationship between material ESG regulation violations and Environmental Investment Initiatives yields only weak empirical support. Across all specifications (Table 3.10), current-year violations are not statistically significant. Limited evidence of a positive association emerges for lagged violations, but only under stricter definitions of materiality—specifically, when violations are defined as those falling within the top 75th or 90th percentiles of the penalty distribution. Results for Models 4 and 5, which incorporate sequential violation indicators, are not presented, as they do not provide any additional meaningful insights.

Table 3.10: Environmental Investments: Material Violations Dummies

	SALES_CUT	MEAN_CUT	MED_CUT	P75_CUT	P90_CUT
Model 1					
<i>VIOL_CURR</i>	0.007 (0.38)	0.035 (1.08)	-0.018 (-1.37)	0.036 (0.44)	0.054 (0.49)
Model 2					
<i>VIOL_CURR</i>	0.008 (0.42)	0.036 (1.09)	-0.018 (-1.30)	0.033 (0.41)	0.053 (0.48)
<i>VIOL_PR_3_YRS</i>	0.023 (1.43)	0.020 (0.87)	0.018 (1.12)	0.039** (2.00)	0.038* (1.79)
Model 3					
<i>VIOL_CURR</i>	0.011 (0.60)	0.033 (0.99)	-0.018 (-1.34)	0.051 (0.63)	0.059 (0.55)
<i>VIOL_PR_1</i>	0.001 (0.04)	0.002 (0.06)	-0.009 (-0.59)	-0.013 (-0.24)	0.044 (0.41)
<i>VIOL_PR_2</i>	0.013 (0.83)	0.030 (0.98)	0.003 (0.20)	0.012 (0.19)	0.024 (0.21)
<i>VIOL_PR_3</i>	0.020 (1.11)	0.034 (1.00)	0.019 (1.28)	0.164* (1.71)	0.184 (1.22)

The analysis of Labor-related Initiatives, presented in Table 3.11, reveals broadly similar patterns when focusing on material ESG violations. Across all specifications, current-year violations are not statistically significant. However, some evidence of a delayed response emerges in relation to lagged violations. In particular, Models 2 and 3 show statistically significant coefficients for lagged violations when materiality is defined using either the mean or the 90th percentile of the penalty distribution. Notably, in Model 3, violations occurring one to three years prior are consistently significant at the 5% level under these definitions. This suggests that firms may respond to more severe ESG violations with delayed Labor-related ESG initiatives, although the results are not robust across all materiality thresholds.

Table 3.11: Labor-related Initiatives: Material Violations Dummies

	SALES_CUT	MEAN_CUT	MED_CUT	P75_CUT	P90_CUT
Model 1					
<i>VIOL_CURR</i>	0.025 (0.90)	0.060 (1.32)	0.028 (1.20)	0.031 (1.10)	-0.001 (-0.02)
Model 2					
<i>VIOL_CURR</i>	0.025 (0.88)	0.061 (1.34)	0.026 (1.09)	0.031 (1.08)	-0.001 (-0.03)
<i>VIOL_PR_3_YRS</i>	-0.001 (-0.04)	0.082** (2.19)	-0.063** (-2.19)	-0.002 (-0.08)	0.063* (1.82)

Table 3.11: Labor-related Initiatives: Material Violations Dummies (continued)

	SALES_CUT	MEAN_CUT	MED_CUT	P75_CUT	P90_CUT
Model 3					
<i>VIOL_CURR</i>	0.025 (0.92)	0.057 (1.23)	0.028 (1.2)	0.026 (0.94)	-0.009 (-0.23)
<i>VIOL_PR_1</i>	0.012 (0.39)	0.092** (1.98)	0.017 (0.73)	0.027 (1.00)	0.073** (2.00)
<i>VIOL_PR_2</i>	0.031 (1.12)	0.090** (1.97)	-0.011 (-0.47)	0.040 (1.53)	0.072* (1.83)
<i>VIOL_PR_3</i>	0.001 (0.04)	0.095** (2.08)	-0.047** (-1.96)	-0.002 (-0.07)	0.093** (2.19)

The empirical evidence offers only limited support for Hypothesis 1b, which posits that firms penalized for violating ESG regulations are more likely to launch new ESG initiatives. Environmental Impact Reduction Initiatives represent the only category where some consistent evidence of a positive association is observed. When all violations, regardless of severity, are considered, the results suggest that firms are more likely to adopt new initiatives when they are repeatedly identified as violators over consecutive years. For material violations, positive effects are observed for both current and lagged violations, particularly when materiality is defined more stringently—such as penalties in the top 75th or 90th percentiles. These effects, while not uniformly robust across all models, suggest that firms may respond to more serious ESG breaches by adopting targeted environmental ESG measures, either immediately or with some delay. The results for Environmental Investment and Labor-related Initiatives are largely inconclusive. While some lagged effects appear under stricter materiality thresholds, these associations are sporadic and lack not consistent across the models.

3.6.3 CSR Activities Score

Next, I turn to the final part of the analysis under Hypothesis 1, which examines the relationship between penalized ESG violations and firms' CSR Activities Score — an indicator of a company's ESG performance relative to its industry peers. Table 3.12 presents the results for the specification when all violations are considered, regardless of

their materiality, and provides no support for the hypothesis: all coefficients of interest are statistically insignificant, and in many cases, their signs are contrary to predicted.

Table 3.12: CSR Activities Score: All Violations Dummy

	SCR_M1	SCR_M2	SCR_M3	SCR_M4	SCR_M5
<i>VIOL_CURR</i>	-0.173 (-0.35)	-0.191 (-0.37)	-0.240 (-0.46)	-0.543 (-1.16)	-0.600 (-1.17)
<i>VIOL_PR_3_YRS</i>		-0.551 (-0.73)			-1.266 (-1.47)
<i>VIOL_PR_1</i>			0.029 (0.05)		
<i>VIOL_PR_2</i>			0.545 (1.00)		
<i>VIOL_PR_3</i>			0.732 (1.33)		
<i>VIOL_CURR*SEQ</i>				0.788 (1.00)	0.891 (1.13)
<i>VIOL_PR_3_YRS*SEQ</i>					1.301 (1.55)
SIZE	6.702*** (8.13)	6.708*** (8.14)	7.044*** (8.24)	6.686*** (8.11)	6.684*** (8.11)
ROA	-5.055*** (-4.58)	-5.068*** (-4.59)	-5.094*** (-4.64)	-5.047*** (-4.57)	-5.051*** (-4.58)
LTDEBT	-1.984 (-0.84)	-2.019 (-0.85)	-2.521 (-1.03)	-1.977 (-0.83)	-1.992 (-0.84)
MTB	0.695*** (3.65)	0.698*** (3.67)	0.740*** (3.59)	0.695*** (3.65)	0.695*** (3.65)
LIQUID	0.004 (0.04)	0.008 (0.09)	-0.031 (-0.31)	0.004 (0.05)	0.003 (0.03)
Const.	-108.1*** (-9.19)	-107.8*** (-9.14)	-112.0*** (-9.15)	-107.9*** (-9.18)	-107.9*** (-9.15)
R-squared	0.587	0.587	0.573	0.587	0.587
Observations	21950	21950	21221	21950	21950

However, the picture changes when switching to the analysis of material violations (Table 3.13. Several specifications consistently show positive and statistically significant associations between penalized ESG violations and the CSR Activities Score, particularly for current-year violations and recent lagged violations under stricter materiality thresholds (0.01% of annual sales, above the mean, and the top 10th percentile of penalties within a given year). The additional analysis of sequential violations provides little further insight, except that past sequential violations are negatively associated with CSR Activities Scores, potentially indicating that firms with repeated violations are those that do not undertake efforts to improve their overall ESG performance and, as a result, consistently

receive low ESG scores relative to their industry peers. In terms of economic significance, depending on the model specification, material violations are associated with an increase of approximately 1.5 to 3 points (out of a maximum of 200) in the CSR Activities Score.

Table 3.13: CSR Activities Score: Material Violations Dummies

	SALES_CUT	MEAN_CUT	MED_CUT	P75_CUT	P90_CUT
Model 1					
<i>VIOL_CURR</i>	1.542** (2.19)	3.046*** (2.63)	0.385 (0.69)	0.778 (1.00)	1.691* (1.75)
Model 2					
<i>VIOL_CURR</i>	1.581** (2.20)	3.086*** (2.65)	0.375 (0.66)	0.786 (1.00)	1.682* (1.75)
<i>VIOL_PR_3_YRS</i>	1.033 (1.39)	2.316** (2.38)	-0.283 (-0.40)	0.311 (0.40)	1.817* (1.95)
Model 3					
<i>VIOL_CURR</i>	1.434** (2.01)	2.618** (2.23)	0.344 (0.62)	0.489 (0.63)	1.296 (1.37)
<i>VIOL_PR_1</i>	1.621** (2.19)	3.687*** (3.26)	0.644 (1.09)	1.299* (1.74)	2.735*** (2.77)
<i>VIOL_PR_2</i>	1.299* (1.77)	2.800** (2.48)	0.816 (1.35)	1.096 (1.51)	2.350** (2.33)
<i>VIOL_PR_3</i>	0.404 (0.55)	1.886* (1.66)	0.891 (1.48)	-0.0631 (-0.09)	1.359 (1.29)
Model 4					
<i>VIOL_CURR</i>	1.368** (2.10)	3.172*** (2.67)	-0.233 (-0.46)	0.268 (0.38)	1.852** (1.98)
<i>VIOL_CURR*SEQ</i>	0.697 (0.49)	-0.567 (-0.25)	1.623* (1.70)	1.656 (1.11)	-0.661 (-0.35)
Model 5					
<i>VIOL_CURR</i>	1.555** (2.14)	3.580*** (2.87)	-0.290 (-0.50)	0.296 (0.38)	2.059** (2.09)
<i>VIOL_PR_3_YRS</i>	1.372 (1.54)	4.993*** (3.63)	-1.307 (-1.63)	-0.221 (-0.23)	2.996** (2.40)
<i>VIOL_CURR*SEQ</i>	0.124 (0.08)	-2.594 (-1.10)	1.733* (1.78)	1.558 (1.01)	-1.611 (-0.83)
<i>VIOL_PR_3_YRS*SEQ</i>	-1.053 (-0.89)	-5.658*** (-3.84)	2.075** (2.25)	1.031 (0.90)	-2.772* (-1.83)

The analysis presented above provides an overall, yet nuanced, picture of how firms adjust their internal ESG structures and policies in response to penalized ESG violations. There is some evidence that companies sanctioned for ESG violations — potentially seeking to prevent recurrence and repair their reputation — immediately modify their ESG infrastructure (the variable captures presence of ESG committees, employee training programs, and ESG objectives in executive compensation scheme) when the penalty is

material. However, the adjustment is not prolonged; the improvements take place either in the year of the violation or, at the latest, in the following year. When narrowing the analysis to targeted ESG initiatives, the most consistent effects are found for Environmental Impact Reduction Initiatives, particularly in cases of more severe or repeated violations. In contrast, Environmental Investment and Labor-related Initiatives yield weak and inconsistent results. Finally, the analysis of the CSR Activities Score suggests that firms do improve their ESG positioning relative to industry peers following material ESG violations, with both current and lagged violation indicators showing statistically significant effects.

3.6.4 Quantitative Outcomes

In this subsection, I discuss the results of the analysis examining the association between penalized ESG violations and quantitative ESG-related outcomes, specifically waste generation, energy consumption, injury rates, and charitable donations. The analysis provides no support for Hypothesis 2, which posits that such violations are followed by improvements in these categories. However, these results should be interpreted with caution, as the lack of uniform regulations on ESG reporting results in limited data availability, with information on quantitative outcomes being scarce and self-reported by the companies themselves.

Given the absence of statistically significant results, for the sake of brevity, the outcomes are presented in a reduced form. None of the estimated coefficients is statistically significant. The signs of the coefficients for waste generation are inconsistent across specifications, showing no clear pattern. For energy consumption, the coefficients consistently display the expected negative sign, suggesting a potential reduction, but the effects are never statistically significant. In the case of injury rates, no meaningful associations are observed. Finally, for charitable donations, the coefficients are extremely small and lack any meaningful interpretation, which likely reflects the fact that donation amounts are scaled by total assets and thus capture little variation.

Table 3.14: Quantitative ESG Outcomes

<i>Panel A: Waste Generation</i>						
	All	Material				
		SALES_CUT	MEAN_CUT	MED_CUT	P75_CUT	P90_CUT
Model 1						
<i>VIOL_CURR</i>	0.091 (0.81)	-0.025 (-0.88)	-0.055 (-0.87)	0.070 (0.77)	0.025 (0.44)	-0.039 (-0.87)
Model 2						
<i>VIOL_CURR</i>	0.092 (0.81)	-0.023 (-0.85)	-0.052 (-0.85)	0.070 (0.77)	0.026 (0.46)	-0.037 (-0.85)
<i>VIOL_PR_3_YRS</i>	0.033 (0.52)	0.036 (0.86)	0.089 (1.35)	-0.003 (-0.26)	0.008 (0.18)	0.071 (1.28)
Model 3						
<i>VIOL_CURR</i>	0.091 (0.79)	-0.020 (-0.79)	-0.047 (-0.81)	0.074 (0.79)	0.028 (0.48)	-0.033 (0.77)
<i>VIOL_PR_1</i>	0.068 (1.59)	-0.005 (-0.42)	0.007 (0.40)	0.030 (1.10)	0.007 (0.29)	0.007 (0.42)
<i>VIOL_PR_2</i>	0.019 (0.24)	0.047 (1.29)	0.103 (1.51)	0.037 (1.48)	0.035 (1.40)	0.078 (1.48)
<i>VIOL_PR_3</i>	-0.087 (-1.53)	0.030 (0.81)	0.066 (1.04)	-0.013 (-0.51)	0.033 (0.96)	0.055 (1.08)
<i>Panel B: Energy Consumption</i>						
	All	Material				
		SALES_CUT	MEAN_CUT	MED_CUT	P75_CUT	P90_CUT
Model 1						
<i>VIOL_CURR</i>	-1.207 (-0.84)	-0.206 (-0.84)	-0.611 (-0.94)	-0.536 (-0.73)	-0.744 (-0.81)	-0.120 (-0.64)
Model 2						
<i>VIOL_CURR</i>	-1.226 (-0.85)	-0.230 (-0.91)	-0.620 (-0.94)	-0.533 (-0.71)	-0.735 (-0.79)	-0.114 (-0.60)
<i>VIOL_PR_3_YRS</i>	-0.493 (-1.36)	-0.275 (-1.07)	-0.194 (-0.91)	0.045 (-0.17)	0.119 (-0.57)	0.149 (-0.84)
Model 3						
<i>VIOL_CURR</i>	-1.209 (-0.83)	-0.270 (-1.04)	-0.796 (-1.23)	-0.538 (-0.70)	-0.762 (-0.81)	-0.170 (-0.88)
<i>VIOL_PR_1</i>	-1.191 (-0.91)	-0.074 (-0.39)	-0.327 (-1.30)	0.002 (-0.01)	0.157 (-1.05)	-0.033 (-0.18)
<i>VIOL_PR_2</i>	1.392 (-1.45)	-0.109 (-0.73)	-0.185 (-0.88)	0.049 (-0.25)	0.085 (-0.58)	0.085 (-0.51)
<i>VIOL_PR_3</i>	-0.379 (-0.39)	-0.190 (-0.70)	0.185 (-0.89)	0.048 (-0.23)	0.166 (-1.22)	0.265 (-1.59)
<i>Panel C: Injury Rate</i>						
	All	Material				
		SALES_CUT	MEAN_CUT	MED_CUT	P75_CUT	P90_CUT
Model 1						
<i>VIOL_CURR</i>	0.173 (-1.29)	0.237 (0.66)	-0.276 (-1.02)	0.030 (0.10)	-0.203 (-0.98)	-0.061 (-0.27)
Model 2						
<i>VIOL_CURR</i>	0.172 (1.22)	0.263 (0.68)	-0.316 (-1.15)	0.036 (0.12)	-0.216 (-1.02)	-0.071 (-0.30)
<i>VIOL_PR_3_YRS</i>	-0.013 (-0.04)	0.203 (0.68)	-0.472 (-1.40)	0.065 (0.20)	-0.112 (-0.49)	-0.160 (-0.55)
Model 3						
<i>VIOL_CURR</i>	0.137 (0.97)	0.218 (0.57)	-0.320 (-1.19)	0.033 (0.11)	-0.215 (-0.99)	-0.066 (-0.28)
<i>VIOL_PR_1</i>	-0.091	-0.234	-0.473	-0.181	-0.233	-0.282

	(-0.50)	(-0.99)	(-2.03)	(-0.97)	(-1.10)	(-1.05)
<i>VIOL_PR_2</i>	-0.119	-0.021	-0.569	-0.078	-0.117	-0.483
	(-0.68)	(-0.09)	(-1.92)	(-0.46)	(-0.53)	(-1.57)
<i>VIOL_PR_3</i>	-0.073	0.007	-0.148	-0.001	-0.109	-0.380
	(-0.44)	(0.03)	(-0.47)	(-0.01)	(-0.49)	(-1.06)
Panel D: Donations						
	All			Material		
		SALES_CUT	MEAN_CUT	MED_CUT	P75_CUT	P90_CUT
Model 1						
<i>VIOL_CURR</i>	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(-1.04)	(-1.00)	(-0.09)	(-1.02)	(-0.99)	(-0.30)
Model 2						
<i>VIOL_CURR</i>	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(-1.04)	(-1.00)	(-0.23)	(-1.02)	(-0.99)	(-0.33)
<i>VIOL_PR_3_YRS</i>	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(-0.89)	(-1.00)	(-1.03)	(-0.90)	(-1.00)	(-0.91)
Model 3						
<i>VIOL_CURR</i>	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(-1.04)	(-1.00)	(-0.16)	(-1.02)	(-0.99)	(-0.32)
<i>VIOL_PR_1</i>	-0.000	-0.000	0.000	-0.000	-0.000	0.000
	(-1.00)	(-1.00)	(0.71)	(-1.00)	(-1.00)	(0.65)
<i>VIOL_PR_2</i>	0.000	-0.000	-0.000	0.000	-0.000	-0.000
	(1.00)	(-0.87)	(-1.06)	(1.00)	(-0.77)	(-0.78)
<i>VIOL_PR_3</i>	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(-0.98)	(-0.49)	(-0.66)	(-0.99)	(-0.73)	(-0.41)

3.6.5 Reporting Transparency

Finally, I conclude with the analysis of ESG Reporting Transparency and its relationship with previously identified penalized ESG regulation violations. Table 3.15 presents the results for the specification where the main explanatory variable, *ESG_VIOL*, equals one whenever a penalized violation occurs, regardless of its materiality. Although most of the coefficients are statistically insignificant or only marginally significant, their signs remain consistent across the different specifications. Interestingly, the results indicate that companies identified as violators over multiple consecutive years (Columns 4 and 5) are more likely to improve their ESG reporting practices.

Table 3.15: Reporting Transparency: All Violations Dummy

	TRNSP_M1	TRNSP_M2	TRNSP_M3	TRNSP_M4	TRNSP_M5
<i>VIOL_CURR</i>	0.032	0.033	0.033	0.002	0.010
	(1.42)	(1.48)	(1.45)	(0.10)	(0.43)

<i>VIOL_PR_3_YRS</i>		0.050*			0.012
		(1.72)			(0.34)
<i>VIOL_PR_1</i>			0.019		
			(0.82)		
<i>VIOL_PR_2</i>			0.015		
			(0.63)		
<i>VIOL_PR_3</i>			0.029		
			(1.23)		
<i>VIOL_CURR*SEQ</i>				0.062*	0.050
				(1.86)	(1.48)
<i>VIOL_PR_3_YRS*SEQ</i>					0.067*
					(1.85)
SIZE	0.200***	0.199***	0.210***	0.198***	0.198***
	(7.01)	(6.99)	(7.17)	(6.96)	(6.95)
ROA	-0.141***	-0.140***	-0.131***	-0.141***	-0.139***
	(-3.24)	(-3.22)	(-3.03)	(-3.22)	(-3.20)
LTDEBT	-0.055	-0.052	-0.065	-0.054	-0.050
	(-0.58)	(-0.54)	(-0.66)	(-0.57)	(-0.53)
MTB	0.030***	0.030***	0.031***	0.030***	0.029***
	(4.49)	(4.44)	(4.30)	(4.48)	(4.42)
LIQUID	0.006	0.006	0.002	0.006	0.005
	(1.34)	(1.25)	(0.34)	(1.35)	(1.20)
Const.	-4.063***	-4.089***	-4.130***	-4.049***	-4.095***
R-squared	0.454	0.454	0.445	0.454	0.454
Observations	21854	21854	21126	21854	21854

Table 3.16 presents the results of the analysis examining the association between material ESG violations and firms' subsequent ESG reporting practices. Similar to the results based on all violations discussed above, the key coefficients are consistently of the predicted sign. In many instances, these coefficients are statistically significant, especially for current and lagged violations under stricter materiality thresholds, providing some empirical support for the idea that firms respond to more severe ESG violations by improving their ESG reporting transparency, possibly as an attempt to signal their ongoing commitment to ESG principles. When the analysis focuses exclusively on material violations, examining sequential violations does not yield any additional insights: the interaction terms are generally statistically insignificant and do not indicate any systematic pattern.

Table 3.16: ESG Reporting Transparency: Material Violations Dummies

	SALES_CUT	MEAN_CUT	MED_CUT	P75_CUT	P90_CUT
Model 1					
<i>VIOL_CURR</i>	0.053*	0.104**	0.049*	0.048	0.071*
	(1.66)	(1.97)	(1.92)	(1.53)	(1.73)

Model 2					
<i>VIOL_CURR</i>	0.056*	0.106**	0.051*	0.050	0.070*
	(1.71)	(2.00)	(1.96)	(1.57)	(1.72)
<i>VIOL_PR_3_YRS</i>	0.061**	0.142***	0.045	0.064**	0.104***
	(2.07)	(3.67)	(1.63)	(2.15)	(2.86)
Model 3					
<i>VIOL_CURR</i>	0.058*	0.097*	0.054**	0.046	0.069*
	(1.80)	(1.82)	(2.08)	(1.48)	(1.69)
<i>VIOL_PR_1</i>	0.032	0.124**	0.014	0.028	0.090**
	(1.01)	(2.45)	(0.57)	(0.91)	(2.25)
<i>VIOL_PR_2</i>	0.021	0.122**	0.010	0.023	0.067*
	(0.68)	(2.42)	(0.40)	(0.79)	(1.65)
<i>VIOL_PR_3</i>	-0.008	0.068	0.023	-0.011	0.014
	(-0.26)	(1.37)	(0.91)	(-0.37)	(0.31)
Model 4					
<i>VIOL_CURR</i>	0.045	0.112**	0.036	0.041	0.067*
	(1.49)	(2.34)	(1.42)	(1.33)	(1.68)
<i>VIOL_CURR*SEQ</i>	0.033	-0.039	0.034	0.024	0.015
	(0.52)	(-0.37)	(0.85)	(0.40)	(0.19)
Model 5					
<i>VIOL_CURR</i>	0.055*	0.133***	0.044	0.052	0.078*
	(1.69)	(2.64)	(1.61)	(1.56)	(1.87)
<i>VIOL_PR_3_YRS</i>	0.063*	0.165**	0.012	0.057	0.111**
	(1.70)	(2.79)	(0.37)	(1.45)	(2.21)
<i>VIOL_CURR*SEQ</i>	0.001	-0.123	0.018	-0.007	-0.032
	(0.02)	(-1.11)	(0.43)	(-0.11)	(-0.39)
<i>VIOL_PR_3_YRS*SEQ</i>	-0.005	-0.036	0.078**	0.022	-0.014
	(-0.12)	(-0.51)	(1.99)	(0.47)	(-0.23)

In general, the main analysis provides partial support for the hypotheses proposed in this paper. While there is no consistent evidence across all ESG outcomes that firms systematically adjust their infrastructures and improve their ESG performance following violations to mitigate heightened ESG risk and signal commitment to ESG principles, the results indicate some meaningful improvements. The most consistent reactions occur in Environmental Impact Reduction Initiatives, where both current and lagged violations — especially material and repeated ones — lead to significant improvements. CSR Activity scores, which reflect a company's position relative to industry peers, also increase after material violations. Labor-related initiatives respond with a delay, showing significant changes mainly after serious violations in previous years. ESG infrastructure shows limited adjustments that occur only immediately following severe violations, while reporting transparency improves modestly, mainly after repeated or severe violations. By contrast, there is little evidence of meaningful changes in environmental investment or quantitative

outcomes such as waste generation, energy use, or injury rates.

3.7 Sensitivity Analysis

Lastly, since the violations (and corresponding penalties) examined throughout the analysis fall into four categories — employment, environmental, health, and safety — and given that some of the ESG outcome variables differ in focus, with certain measures reflecting environmental aspects and others social aspects, I account for this distinction in the additional analysis. Specifically, I examine the relationship between environmental violations and the following outcomes: (1) environmental reduction and investment initiatives, (2) the environmental component of the CSR Activities score, and (3) waste generation and energy consumption. For the social-oriented outcomes, I group all non-environmental penalties and analyze their association with: (1) Labor-related initiatives, (2) the social component of the ESG score, and (3) injury rates.

Table 3.17 reports the analysis results for the association of environmental violations with environmental reduction and investment initiatives, and of social, employment, and health regulation violations with Labor-related initiatives. Panel A (Environmental Impact Reduction Initiatives) shows some evidence that firms respond to environmental violations by adopting additional environmental initiatives, particularly when the associated penalties are relatively high. Across the models, the coefficients on current-year violations are generally positive, and reach statistical significance in several cases where penalties exceed the yearly median or fall into the upper quartiles. This suggests that firms are more likely to initiate corrective actions when violations result in more substantial penalties. When interaction terms capturing repeated violations are included (not presented for the sake of brevity), the estimated coefficients on these terms are not consistent in sign or significance across models, making it difficult to draw conclusions regarding their role in influencing environmental reduction policies. Panel B (Environmental Investment Initiatives) does not provide additional support for the hypothesis. Across all models, the estimated coefficients remain statistically insignificant, which, as

in the case of the main analysis, may be partly explained by the relatively small number of environmental investment initiatives launched over the sample period. Finally, the analysis of social, employment, and health regulation violations in Panel C suggests that material violations from previous years are positively associated with the adoption of Labor-related initiatives, as several coefficients on lagged violations are positive and statistically significant.

Table 3.17: Sensitivity Analysis: ESG Initiatives

<i>Panel A: Environmental Impact Reduction Initiatives</i>						
	All	Material				
		SALES_CUT	MEAN_CUT	MED_CUT	P75_CUT	P90_CUT
Model 1						
<i>VIOL_CURR</i>	0.045 (1.17)	0.063 (0.92)	0.039 (0.46)	0.086* (1.87)	0.116* (1.89)	0.121 (1.52)
Model 2						
<i>VIOL_CURR</i>	0.047 (1.18)	0.067 (0.94)	0.040 (0.46)	0.089* (1.88)	0.119* (1.92)	0.125 (1.53)
<i>VIOL_PR_3_YRS</i>	0.028 (0.63)	0.044 (0.77)	0.012 (0.19)	0.058 (1.13)	0.082 (1.62)	0.070 (1.15)
Model 3						
<i>VIOL_CURR</i>	0.053 (1.32)	0.079 (1.08)	0.054 (0.59)	0.098** (2.13)	0.125** (2.00)	0.129 (1.56)
<i>VIOL_PR_1</i>	0.030 (0.75)	-0.021 (-0.29)	0.009 (0.09)	-0.011 (-0.60)	0.001 (0.03)	-0.020 (-0.53)
<i>VIOL_PR_2</i>	0.040 (0.99)	0.017 (0.24)	-0.011 (-0.12)	-0.002 (-0.10)	-0.016 (-0.63)	0.013 (0.36)
<i>VIOL_PR_3</i>	0.038 (0.95)	-0.017 (-0.24)	-0.052 (-0.58)	0.014 (0.28)	-0.032 (-1.32)	-0.014 (-0.39)
<i>Panel B: Environmental Investment Initiatives</i>						
	All	Material				
		SALES_CUT	MEAN_CUT	MED_CUT	P75_CUT	P90_CUT
Model 1						
<i>VIOL_CURR</i>	0.001 (0.09)	0.003 (0.13)	0.003 (0.06)	-0.009 (-0.48)	0.003 (0.12)	-0.021 (-0.59)
Model 2						
<i>VIOL_CURR</i>	0.001 (0.04)	0.003 (0.13)	0.005 (0.11)	-0.009 (-0.46)	0.003 (0.13)	-0.020 (-0.55)
<i>VIOL_PR_3_YRS</i>	-0.013 (-0.69)	-0.001 (-0.07)	0.019 (0.80)	0.008 (0.48)	0.004 (0.26)	0.016 (0.68)
Model 3						
<i>VIOL_CURR</i>	-0.002 (-0.12)	0.014 (0.54)	0.009 (0.21)	-0.010 (-0.56)	0.009 (0.36)	-0.008 (-0.23)
<i>VIOL_PR_1</i>	-0.018 (-1.20)	-0.011 (-0.45)	-0.006 (-0.14)	-0.011 (-0.60)	0.001 (0.03)	-0.020 (-0.53)
<i>VIOL_PR_2</i>	-0.002 (-0.11)	-0.007 (-0.28)	-0.013 (-0.29)	-0.002 (-0.10)	-0.016 (-0.63)	0.013 (0.36)
<i>VIOL_PR_3</i>	0.000 (0.02)	-0.028 (-1.13)	-0.041 (-0.96)	0.008 (0.43)	-0.032 (-1.32)	-0.014 (-0.39)
<i>Panel C: Labor-related Initiatives</i>						
	All	Material				
		SALES_CUT	MEAN_CUT	MED_CUT	P75_CUT	P90_CUT

Table 3.17: Sensitivity Analysis: ESG Initiatives (continued)

Model 1						
<i>VIOL_CURR</i>	-0.018 (-0.80)	0.028 (0.78)	0.034 (0.80)	-0.002 (-0.07)	-0.013 (-0.48)	0.027 (0.68)
Model 2						
<i>VIOL_CURR</i>	-0.018 (-0.82)	0.029 (0.82)	0.035 (0.82)	-0.001 (-0.03)	-0.013 (-0.45)	0.028 (0.70)
<i>VIOL_PR_3_YRS</i>	-0.027 (-0.94)	0.039 (1.18)	0.109** (3.07)	0.023 (0.72)	0.025 (0.80)	0.070** (2.00)
Model 3						
<i>VIOL_CURR</i>	-0.022 (-0.98)	0.031 (0.85)	0.035 (0.83)	-0.002 (-0.07)	-0.012 (-0.44)	0.024 (0.59)
<i>VIOL_PR_1</i>	-0.027 (-1.24)	0.053 (1.53)	0.103** (2.47)	-0.000 (-0.01)	0.034 (1.21)	0.054 (1.53)
<i>VIOL_PR_2</i>	-0.019 (-0.88)	0.071** (2.16)	0.101* (2.34)	-0.002 (-0.09)	0.047* (1.66)	0.022 (0.61)
<i>VIOL_PR_3</i>	0.005 (0.21)	0.052 (1.49)	0.101** (2.40)	0.012 (0.42)	-0.009 (-0.30)	0.030 (0.77)

Table 3.18 presents the results for the Environmental and Social sub-score analysis. In the case of the environmental score and environmental violations, reported in Panel A, the coefficients on both current and previous-year violations are mostly positive across specifications, and in some cases, primarily for previous-year violations, are statistically significant. This suggests that firms found to have violated environmental regulations may begin improving their environmental positioning relative to industry peers, potentially as a way to reduce risk exposure and signal a credible commitment to good environmental practices. Furthermore, the coefficients on sequential violations are in some cases statistically significant and of the expected sign, suggesting that firms repeatedly recognized as violators take additional steps to demonstrate efforts to improve their environmental performance.

The results for the Social sub-score following employment, health, and safety regulations are less conclusive. When material violations are defined as those associated with penalties above the yearly mean, the results provide some support for the hypothesis: the vast majority of the relevant coefficients are positive, and many of them are statistically significant. When penalties are scaled by sales to identify material violations, or when only violations in the top 25% percentile of yearly penalties are considered, the coefficients remain of the expected sign but are rarely significant at the 10% level. In the remaining

specifications, even the signs of the coefficients are inconsistent.

Table 3.18: Sensitivity Analysis: CSR Activities Score

<i>Panel A: Environmental ESG Score</i>						
	All	Material				
		SALES_CUT	MEAN_CUT	MED_CUT	P75_CUT	P90_CUT
Model 1						
<i>VIOL_CURR</i>	0.242 (0.59)	0.604 (0.72)	0.604 (0.72)	0.739 (1.46)	1.275 (1.57)	1.570 (1.61)
Model 2						
<i>VIOL_CURR</i>	0.251 (0.60)	0.645 (0.75)	0.645 (0.75)	0.751 (1.46)	1.310 (1.60)	1.654* (1.66)
<i>VIOL_PR_3_YRS</i>	0.173 (0.37)	0.538 (0.92)	0.538 (0.92)	0.312 (0.59)	1.041* (1.88)	1.410** (2.20)
Model 3						
<i>VIOL_CURR</i>	0.206 (0.49)	0.729 (0.81)	0.729 (0.81)	0.761 (1.55)	1.229 (1.50)	1.664 (1.64)
<i>VIOL_PR_1</i>	0.588 (1.43)	0.408 (0.49)	0.408 (0.49)	0.714 (1.25)	1.534* (1.94)	2.446** (2.50)
<i>VIOL_PR_2</i>	0.785* (1.78)	0.445 (0.57)	0.445 (0.57)	1.014* (1.80)	1.372* (1.79)	2.446** (2.48)
<i>VIOL_PR_3</i>	0.906** (2.22)	-0.0477 (-0.07)	-0.0477 (-0.07)	0.289 (0.55)	0.114 (0.17)	1.322 (1.52)
Model 4						
<i>VIOL_CURR</i>	-0.629* (-1.65)	0.819 (1.05)	0.819 (1.05)	-0.0280 (-0.06)	1.070 (1.48)	1.336 (1.42)
<i>VIOL_CURR*SEQ</i>	2.173*** (3.17)	-0.960 (-0.59)	-0.960 (-0.59)	2.245** (2.13)	0.767 (0.43)	1.055 (0.62)
Model 5						
<i>VIOL_CURR</i>	-0.622 (-1.51)	1.023 (1.17)	1.023 (1.17)	0.00127 (0.00)	1.232 (1.59)	1.626 (1.55)
<i>VIOL_PR_3_YRS</i>	-0.360 (-0.65)	1.412 (1.54)	1.412 (1.54)	-0.577 (-0.89)	1.028 (1.27)	1.768* (1.70)
<i>VIOL_CURR*SEQ</i>	2.186*** (3.10)	-1.619 (-0.91)	-1.619 (-0.91)	2.223** (2.09)	0.288 (0.15)	0.180 (0.10)
<i>VIOL_PR_3_YRS*SEQ</i>	0.747 (1.16)	-1.730 (-1.52)	-1.730 (-1.52)	1.613** (1.96)	-0.0115 (-0.01)	-0.685 (-0.54)
<i>Panel B: Social Activities Score</i>						
	All	Material				
		SALES_CUT	MEAN_CUT	MED_CUT	P75_CUT	P90_CUT
Model 1						
<i>VIOL_CURR</i>	-0.254 (-0.91)	0.702 (1.64)	1.677*** (2.96)	-0.105 (-0.31)	0.132 (0.34)	0.275 (0.51)
Model 2						
<i>VIOL_CURR</i>	-0.267 (-0.94)	0.704 (1.61)	1.681*** (2.97)	-0.118 (-0.34)	0.131 (0.33)	0.271 (0.50)
<i>VIOL_PR_3_YRS</i>	-0.503 (-1.31)	0.064 (0.15)	0.538 (1.14)	-0.298 (-0.79)	-0.106 (-0.25)	-0.370 (-0.76)
Model 3						
<i>VIOL_CURR</i>	-0.298 (-1.06)	0.652 (1.53)	1.613*** (2.82)	-0.123 (-0.36)	0.061 (0.16)	0.181 (0.34)
<i>VIOL_PR_1</i>	-0.420 (-1.42)	0.978** (2.24)	1.964*** (3.38)	-0.153 (-0.43)	0.698* (1.78)	0.330 (0.61)
<i>VIOL_PR_2</i>	-0.147 (-0.53)	0.710 (1.64)	1.071* (1.75)	0.078 (0.23)	0.631 (1.56)	0.263 (0.49)

Table 3.18: Sensitivity Analysis: CSR Activities Score (continued)

<i>VIOL_PR_3</i>	0.232 (0.83)	0.234 (0.54)	0.692 (1.35)	0.226 (0.69)	0.222 (0.55)	0.619 (1.16)
Model 4						
<i>VIOL_CURR</i>	0.074 (0.29)	0.465 (1.07)	1.396** (2.42)	0.092 (0.30)	-0.180 (-0.46)	0.298 (0.53)
<i>VIOL_CURR*SEQ</i>	-0.740* (-1.72)	1.155 (1.41)	1.284 (1.20)	-0.561 (-0.99)	1.038 (1.58)	-0.087 (-0.10)
Model 5						
<i>VIOL_CURR</i>	0.015 (0.05)	0.573 (1.01)	1.473** (2.46)	0.049 (0.15)	-0.212 (-0.50)	0.268 (0.44)
<i>VIOL_PR_3_YRS</i>	-0.389 (-0.87)	0.310 (0.59)	1.589** (2.36)	-0.134 (-0.31)	-0.100 (-0.20)	0.288 (0.43)
<i>VIOL_CURR*SEQ</i>	-0.630 (-1.44)	1.081 (1.21)	0.741 (0.65)	-0.467 (-0.81)	1.134* (1.67)	0.029 (0.03)
<i>VIOL_PR_3_YRS*SEQ</i>	-0.048 (-0.11)	-0.929 (-1.43)	-2.525*** (-3.03)	-0.291 (-0.61)	-0.278 (-0.46)	-1.475** (-2.09)

Finally, Table 3.19 presents the results for the relationship between regulation violations and ESG quantitative outcomes. The analysis distinguishes between environmental violations, which are related to waste generation and energy consumption, and employment, health, and safety violations, which are linked to injury rates. In general, the results lack statistical significance across most specifications and are therefore presented in a shortened form. For waste generation (Panel A) the coefficients on material current-year violations are of the expected negative sign and, in some cases, approach statistical significance at the 10 percent level. The coefficients on previous-year violations are positive and statistically insignificant. For energy consumption, presented in Panel B, the current-year coefficients are generally of the predicted negative sign, but none reach statistical significance. For employment, health, and safety regulation violations and their relationship with injury rates (Panel C), the coefficients on material violations are consistently negative for both current and previous-year violations. When materiality is defined using penalties above the yearly mean or median, some of the coefficients on previous-year violations are statistically significant which suggests that firms may respond to material violations by adopting measures that contribute to reductions in injury rates in subsequent periods.

Table 3.19: Sensitivity Analysis: ESG Quantitative Outcomes

<i>Panel A: Waste Generation</i>						
	All	Material				
		SALES_CUT	MEAN_CUT	MED_CUT	P75_CUT	P90_CUT
Model 1						
<i>VIOL_CURR</i>	0.026 (1.16)	-0.071 (-0.98)	-0.104 (-0.92)	-0.057 (-1.24)	-0.098 (-1.54)	-0.096 (-1.04)
Model 2						
<i>VIOL_CURR</i>	0.028 (1.16)	-0.054 (-0.84)	-0.079 (-0.80)	-0.054 (-1.19)	-0.095 (-1.52)	-0.081 (-0.96)
<i>VIOL_PR_3_YRS</i>	0.046 (0.96)	0.110 (1.38)	0.139 (1.37)	0.045 (1.29)	0.039 (0.77)	0.136 (1.50)
Model 3						
<i>VIOL_CURR</i>	0.040 (1.25)	-0.046 (-0.75)	-0.074 (-0.77)	-0.048 (-1.12)	-0.088 (-1.46)	-0.073 (-0.90)
<i>VIOL_PR_1</i>	0.043 (1.46)	0.006 (0.18)	0.001 (0.02)	0.019 (0.84)	0.022 (0.52)	0.007 (0.18)
<i>VIOL_PR_2</i>	0.145 (1.22)	0.125 (1.52)	0.209 (1.62)	0.100 (1.56)	0.119 (1.19)	0.162 (1.65)
<i>VIOL_PR_3</i>	0.030 (0.89)	0.085 (1.02)	0.141 (0.98)	0.033 (1.27)	0.014 (0.45)	0.112 (1.05)
<i>Panel B: Energy Consumption</i>						
	All	Material				
		SALES_CUT	MEAN_CUT	MED_CUT	P75_CUT	P90_CUT
Model 1						
<i>VIOL_CURR</i>	-2.723 (-0.94)	-0.120 (-0.36)	0.118 (0.29)	-1.112 (-0.81)	-1.975 (-0.98)	-0.097 (-0.30)
Model 2						
<i>VIOL_CURR</i>	-2.763 (-0.94)	-0.171 (-0.50)	0.182 (0.43)	-1.125 (-0.80)	-1.981 (-0.97)	-0.063 (-0.19)
<i>VIOL_PR_3_YRS</i>	-0.617 (-1.06)	-0.392 (-0.74)	0.402 (0.87)	-0.126 (-0.32)	-0.089 (-0.31)	0.366 (1.06)
Model 3						
<i>VIOL_CURR</i>	-2.746 (-0.93)	-0.285 (-0.81)	-0.109 (-0.32)	-1.144 (-0.80)	-2.073 (-0.99)	-0.200 (-0.60)
<i>VIOL_PR_1</i>	-0.279 (-0.63)	0.057 (0.17)	0.129 (0.31)	-0.078 (-0.21)	-0.021 (-0.07)	0.124 (0.37)
<i>VIOL_PR_2</i>	0.350 (0.92)	-0.216 (-0.73)	0.315 (0.66)	-0.161 (-0.61)	-0.411 (-1.11)	0.184 (0.74)
<i>VIOL_PR_3</i>	-0.385 (-1.25)	-0.524 (-0.75)	0.486* (1.65)	-0.033 (-0.14)	-0.071 (-0.28)	0.502** (1.97)
<i>Panel C: Injury Rate</i>						
	All	Material				
		SALES_CUT	MEAN_CUT	MED_CUT	P75_CUT	P90_CUT
Model 1						
<i>VIOL_CURR</i>	0.069 (0.53)	0.428 (0.73)	-0.234 (-1.01)	-0.233 (-1.27)	-0.103 (-0.53)	-0.323 (-0.80)
Model 2						
<i>VIOL_CURR</i>	0.075 (0.55)	0.338 (0.63)	-0.250 (-1.05)	-0.279 (-1.37)	-0.113 (-0.56)	-0.326 (-0.80)
<i>VIOL_PR_3_YRS</i>	0.092 (0.34)	-0.646 (-1.28)	-0.197 (-0.68)	-0.436 (-1.23)	-0.145 (-0.57)	-0.026 (-0.11)
Model 3						
<i>VIOL_CURR</i>	0.043 (0.29)	0.289 (0.53)	-0.343 (-1.36)	-0.280 (-1.41)	-0.113 (-0.56)	-0.341 (-0.83)
<i>VIOL_PR_1</i>	-0.162	-0.528	-0.567*	-0.337*	-0.133	-0.149

Table 3.19: Sensitivity Analysis: ESG Quantitative Outcomes (continued)

	(-0.71)	(-1.21)	(-1.72)	(-1.75)	(-0.63)	(-0.39)
<i>VIOL_PR_2</i>	-0.174	-0.650	-0.761**	-0.194	-0.114	-0.082
	(-0.61)	(-1.52)	(-2.18)	(-1.14)	(-0.63)	(-0.37)
<i>VIOL_PR_3</i>	-0.281	-0.661	-0.421	-0.360**	-0.045	0.140
	(-1.46)	(-1.56)	(-1.14)	(-2.19)	(-0.22)	(0.58)

The sensitivity analysis generally supports the main results. The findings suggest that firms respond to environmental violations with clearer improvements in Environmental Impact Reduction Initiatives and environmental ESG scores, mainly when the violations are material. In the case of social, employment, and health violations, companies tend to initiate new Labor-related initiatives and show moderate improvements in social ESG scores. For quantitative outcomes, the evidence remains generally weak, though some limited improvements are observed for waste generation and injury rates following material violations.

3.8 Conclusion

This study examines how firms adjust their ESG practices after being penalized for regulatory violations. Using a large sample of U.S. public companies from 2000 to 2023, I analyze whether violating firms — potentially seeking to mitigate elevated risks, reassure stakeholders of their commitment to ESG principles, and prevent repeated violations — strengthen their ESG performance and reporting. In particular, I investigate whether they improve their internal ESG infrastructure, adopt new initiatives, and achieve improvements in quantitative outcomes. The results show that firms committing material or repeated violations are more likely to adopt certain types of ESG-related initiatives and improve their CSR Activities Score, which reflects their ESG positioning relative to peers. Moreover, reporting transparency tends to increase, particularly following the repeated violations. By contrast, adjustments in ESG infrastructure are generally limited and statistically significant only in a few specifications. For quantitative ESG outcomes, such as

waste generation, energy consumption, and injury rates, no consistent improvements are observed.

3.9 Literature

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Appendix: Variable Descriptions

Table 20: Summary of Variables Used in the Analysis

Variable	Description
Main Independent Variables	
<i>ESG Violation</i>	Indicator variable equal to 1 if a firm is penalized for any ESG violation in a given year (employment, environmental, healthcare, or safety), and 0 otherwise.
<i>Material ESG Violation</i>	Alternative definitions of ESG Violation based on penalty amount. Materiality is determined by five approaches: (i) Penalty amount exceeding 0.01% of firm's annual sales; (ii) Penalty amount exceeding the annual mean of all penalties; (iii) Penalty amount exceeding the annual median; (iv) Penalty amount exceeding the 75th percentile of the annual penalty distribution; (v) Penalty amount exceeding the 90th percentile of the annual penalty distribution.
Violation Timing Variables	
<i>VIOL_CURR</i>	Indicator equal to 1 if a penalized ESG violation occurs in the current year.
<i>VIOL_PR_3YRS</i>	Indicator equal to 1 if a violation occurred in any of the three preceding years.
<i>VIOL_PR1</i>	Indicator equal to 1 if a violation occurred one year prior.
<i>VIOL_PR2</i>	Indicator equal to 1 if a violation occurred two years prior.
<i>VIOL_PR3</i>	Indicator equal to 1 if a violation occurred three years prior.
<i>VIOL_CURR*SEQ</i>	Indicator equal to 1 if the current violation is part of a sequence (violations in consecutive years).
<i>VIOL_PR_3YRS*SEQ</i>	Indicator equal to 1 if any violation in the past three years is part of a sequence.
Dependent Variables	
ESG Infrastructure	Composite score (range 0-3) capturing presence of: (i) ESG committee, (ii) employee ESG training programs, and (iii) ESG-linked executive compensation.
ESG Initiatives	
<i>Environmental Impact Reduction Initiatives</i>	Composite score (range 0-7) based on the number of operational programs aimed at reducing environmental impact: - energy efficiency policies - emissions reduction policies - water efficiency programs - waste reduction initiatives - e-waste reduction initiatives - renewable energy use initiatives - clean energy production initiatives
<i>Environmental Investment Initiatives</i>	Composite score (range 0-2) based on the number of capital investment programs in environmental protection: - environmental protection expenditures (pollution prevention, waste management, remediation) - environmental investment initiatives (renewable energy investment projects, carbon mitigation investments, sustainable infrastructure projects)

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Variable	Description
<i>Labor-related Initiatives</i>	Composite score (range 0–5) based on the number of programs promoting labor standards, diversity, and employee welfare: - diversity policies - child labor prohibition policies - forced labor prohibition policies - human rights policies - employee health and safety programs
CSR Activities Score	Sum of Environmental and Social scores from ASSET4, capturing firm performance relative to industry peers across areas such as human rights, community engagement, emissions, and resource use.
Quantitative ESG Outcomes	
<i>Waste Generation</i>	Total waste generated (thousand tonnes per year), scaled by total assets.
<i>Energy Consumption</i>	Total energy used (thousand gigajoules per year), scaled by total assets.
<i>Injury Rates</i>	Workplace injuries per one million hours worked.
<i>Donations</i>	Total monetary donations, scaled by total assets.
ESG Reporting Transparency Score	Composite score (range 0–4) based on: (i) ESG report availability, (ii) global operational coverage, (iii) adherence to GRI/OECD frameworks, and (iv) external assurance of ESG reporting.
Control Variables	
<i>SIZE</i>	Natural logarithm of total assets.
<i>ROA</i>	Return on assets (net income divided by total assets).
<i>LTDEBT</i>	Long-term debt ratio (long-term debt divided by total assets).
<i>MTB</i>	Market-to-book ratio.
<i>LIQUID</i>	Liquidity ratio (current assets divided by current liabilities).