경영학박사학위논문

Essays on
Audit Contracts and Regulations

감사계약과 규제에 관한 연구

2022년 2월

서울대학교 대학원

경영학과 경영학 전공

이 유 진

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이 논문을 경영학박사 학위논문으로 제출함

2021년 10월

서울대학교 대학원

경영학과 경영학전공

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2021년 12월

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Abstract

Essays on Audit Contracts and Regulations

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This dissertation is comprised of two essays on audit contracts and regulations. The first essay, entitled "*The Flip Side of Legal Enforcement: International Evidence on Opinion Shopping*," demonstrates the effect of strict legal enforcement on audit clients' tendency to engage in opinion shopping, i.e., opportunistically switching the auditor for a more favorable audit opinion. Using data from 48 countries, I find that an increase in the strictness of country-level legal enforcement is associated with increased opinion shopping by audit clients. Increases in the signaling effect of audit opinions and the quality gap between large and small auditors act as channels through which strict legal enforcement induces opinion shopping by clients. Firms engage in opinion shopping under strict legal enforcement for opportunistic motives: they are more likely to switch to non-Big 4 auditors than to Big 4 auditors and exhibit deteriorated audit quality after the switch. The finding that strict enforcement increases rather than restricts opportunistic opinion shopping provides a new perspective on the effect of legal enforcement on capital markets.

The second essay, entitled "*Preoccupied Auditors: The Spillover Effect of Public Firm Audits on Private Firm Audit Quality*," examines the effect of regulations on auditors' strategic resource allocation. Stricter regulations targeting certain types of firms may benefit and harm unregulated firms that share the auditor with regulated firms, as these regulations may not only create knowledge spillover but also preoccupy much of the auditor's resources. In this study, I document that the drawback of preoccupied resources dominates the benefit of knowledge spillover. Specifically, I find that auditors with a portfolio tilted more toward public clients provide lower-quality audits to their private clients. Furthermore, this finding is more pronounced for auditors with larger resource adjustment costs, consistent with the auditors' strategic resource allocation driving the results. I also find evidence that knowledge spillover mitigates the negative impact of resource allocation decisions. The findings of this study suggest that imposing stricter regulations on certain sectors may have unintended adverse effects on other sectors in the market via intermediaries. Thus, this study highlights the importance of a comprehensive cost–benefit assessment of disclosure and audit regulations in the capital market.

Keywords: auditor; audit contracts; audit opinion; audit quality; legal enforcement; opinion shopping; private firm; regulations.

Student Number: 2016 – 30167

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Essay 1.

The Flip Side of Legal Enforcement: International Evidence on Opinion Shopping

1. Introduction

Opinion shopping refers to an audit client's decision to switch auditors aiming to obtaining a more favorable audit opinion. Despite long-standing regulatory concerns about this opportunistic behavior due to its potential harm to auditors' independence (DeFond and Zhang 2014),[1] the impact of a country's institutional characteristics on opinion shopping behavior is not yet understood. Taking advantage of substantial variations in institutions across countries, I examine how the strictness of legal enforcement affects an audit client's opinion shopping behavior in an international setting.

A country's laws and level of law enforcement shape the behavior of players in the financial market (Djankov, La Porta, Lopez-de-Silanes, and Shleifer 2008; La Porta, Lopez-de-Silanes, Shleifer, and Vishny 1997, 2000). These country-level factors are at least as important as firm-level factors in determining a firm's behavior (Francis, Khurana, Martin, and Pereira 2011; Li, Richardson, and Tuna 2014). However, most studies on opinion shopping use data from a single country (Carey, Geiger, and O'Connell 2008; Chan, Lin, and Mo 2006; Chen, Francis, and Hou 2017; Chow and Rice 1982; Chung, Sonu, Zang, and Choi 2019; Lennox 2000; Newton, Persellin, Wang, and Wilkins 2016), which does not allow examining how institutional characteristics affect opinion shopping behavior. This essay fills this gap in the literature in an attempt to infer regulatory implications regarding opinion shopping from international data.

Strict legal enforcement may affect a client's opinion shopping behavior in two directions. On the one hand, strict legal enforcement may restrict opinion shopping. Under stricter legal enforcement, clients become disciplined and improve their corporate governance (DeFond and Hung 2004;

[1] Examples of regulators that have expressed concerns about opinion shopping include the Cadbury Committee (1992) and Institute of Chartered Accountants in England and Wales (2002) of the United Kingdom, the European Commission (2010), the MacDonald Commission (1987) of Canada, and the Public Company Accounting Oversight Board (2011) and the Securities Exchange Commission (1988) of the United States.

La Porta et al. 2000). Since strict corporate governance restricts a firm's opportunistic behavior (Core, Holthausen, and Larcker 1999; Gompers, Ishii, and Metrick 2003; Jensen and Meckling 1976), it is expected to restrict a firm's tendency to engage in opinion shopping. Moreover, as legal enforcement becomes stricter, auditors become more independent (Choi, Kim, Liu, and Simunic 2008; Ghosh and Pawlewicz 2009) and sensitive to audit risks in their clients' portfolios (Choi, Doogar, and Ganguly 2004; Shu 2000). In such cases, auditors are less likely to accept opinion shoppers as new clients, in turn deterring audit clients' opinion shopping behavior.

On the other hand, stricter legal enforcement may trigger opinion shopping. In countries with stricter legal enforcement, stakeholders increasingly rely on accounting and audits due to the greater value relevance of accounting information (DeFond, Hung, and Trezevant 2007; El Ghoul, Guedhami, and Pittman 2016; El Ghoul, Guedhami, Pittman, and Rizeanu 2016; Gul, Zhou, and Zhu 2013; Kausar, Taffler, and Tan 2017). An increase in capital market penalties on modified audit opinions increases clients' incentives to avoid receiving such opinions and thus encourages opinion shopping. In addition, Big 4 auditors are more strongly affected by legal institutions than non-Big 4 auditors (Francis and Wang 2008; Fung, Zhou, and Zhu 2016). The increased audit-quality gap between the two types of auditors under stricter legal enforcement creates greater incentives for clients to engage in opinion shopping by switching from high- to low-quality auditors. Due to these opposing predictions, the effect of legal enforcement on audit clients' opinion shopping behavior remains an interesting empirical question.

In the empirical analyses, I use a sample of 93,793 nonfinancial firms audited by Big 4 auditors in 48 non-United States (U.S.) countries from 2004

to 2018. I extract firm-level financial data from Compustat Global.[2] I focus on firms audited by Big 4 auditors in year *t* to control for differences in the current auditors' quality and the clients' preference for certain types of auditors. I measure the strictness of country-level legal enforcement using the index proposed by Brown, Preiato, and Tarca (2014), which reflects the quality of auditors' working environment and the level of accounting enforcement activities in a country. I use two methods to identify opinion shopping. First, I examine whether the association between current-year modified opinions and subsequent-year auditor switches is positive. Second, I follow Lennox (2000) and examine whether subsequent-year auditor switches are positively associated with the difference in the expected probabilities of receiving a modified opinion from the incumbent auditor and a new auditor.

Using empirical analyses, I confirm the existence of opinion shopping in an international setting. Specifically, I find that subsequent-year auditor switches are positively associated with current-year modified opinions and with an expected reduction in the probability of receiving a modified audit opinion. More importantly, I find that firms in countries with stricter legal enforcement are more likely to shop for favorable audit opinions than their peers in countries with more lenient legal enforcement through pooled, subsample, and country-level analyses. The evidence indicates that both the increased signaling effect of audits and quality gap between large and small auditors act as channels through which strict legal enforcement increases clients' likelihood of opinion shopping. In addition, I conclude that the increase in opinion shopping under strict enforcement is opportunistic since it is more pronounced for downward auditor switches, i.e., from a Big

[2] Auditor switches are measured reliably only for Big 4 auditors because most non-Big 4 auditors outside the U.S. are coded as "other auditors (9)" in Compustat Global. Switches within the group of "other auditors" cannot be identified. Note that Worldscope, an alternative international database, does not provide historical auditor identities and thus cannot be used to identify auditor switches.

4 to a non-Big 4 auditor, than lateral switches and is associated with a subsequent reduction in audit quality. The main findings are robust to a propensity-score matching analysis, the inclusion of firm-fixed effects, and the exclusion of firms in countries having a mandatory auditor rotation policy.

This study makes the following contributions to the literature. First, it adds to the literature on international business and country-level governance by demonstrating an unintended consequence of strict legal enforcement. In contrast to most prior studies that focus on the benefits of strict legal enforcement in terms of capital market development and transparency (e.g., Brown et al. 2014; Burgstahler, Hail, and Leuz 2006; Francis and Wang 2008; Fung et al. 2016; Lang, Raedy, and Wilson 2006; Leuz, Nanda, and Wysocki 2003), I document evidence that strict legal enforcement increases rather than decreases opportunistic behavior by audit clients. The findings imply that regulatory attempts to improve audit quality by increasing the strictness of legal enforcement may be futile if clients are incentivized to switch opportunistically from high- to low-quality auditors. Given the increase in switches to lower-quality auditors and the deterioration in audit quality observed among opinion shoppers under strict legal enforcement, it is unclear whether an increase in legal enforcement level impairs or enhances audit quality at the aggregate country level. In this regard, my work complements studies by Cohen, Krishnamoorthy, and Wright (2010) and Beck and Mauldin (2014), who find no substantial changes in audit-related firm governance in the U.S. despite the imposition of stricter regulations mandated by the Sarbanes-Oxley Act (SOX) of 2002. Thus, regulators aiming to promote financial transparency in capital markets should carefully consider the comprehensive impact of legal enforcement on audit contracts.[3]

[3] Specifically, strengthening legal enforcement may have unintended consequences, such as an increased likelihood of opinion shopping as documented in this study. Similarly, mandatory auditor rotation policies intended to enhance auditor independence are often criticized because of their potential effect on opinion shopping, i.e., firms may disguise opinion shopping as a mandatory auditor switch required by law (DeFond and Zhang 2014).

Second, the study's findings provide implications regarding auditors' contracting environment. Strict legal enforcement has a greater disciplinary effect on large auditors than on small auditors (e.g., Francis and Wang 2008; Fung et al. 2016). I show that the resulting gap in audit quality between the two types of auditors increases audit clients' incentives to switch from high- to low-quality auditors, which extends DeFond, Wong, and Li's (2000) finding that stricter auditing standards in China reduced the market shares of high-quality auditors. Taken together, these findings suggest that high-quality auditors are penalized under strict legal enforcement both directly, through increases in audit requirements and legal liabilities, and indirectly, through an increased likelihood of audit contract termination.

Finally, this study contributes to the auditing literature by extending research on opinion shopping to the international setting. Although opinion shopping has been a traditional concern in the auditing literature and practice (Chow and Rice 1982; Krishnan and Stephens 1995; Lennox 2000; Teoh 1992), most studies are conducted in single-country settings. Accordingly, the prevalence and determinants of opinion shopping across different jurisdictions remain unknown. I show that opinion shopping is a global phenomenon and affected by institutional characteristics.

The rest of the paper proceeds as follows. The second section discusses related studies and develops the hypothesis. The third section describes the empirical strategy and data. The fourth section provides empirical results of the main tests, and the fifth section provides the results of additional analyses. The final section concludes the paper.

2. Literature Review and Hypothesis Development

2.1 Opinion shopping

External auditors play an important role in promoting firm transparency. After auditing a firm's financial statements, auditors issue an audit report that includes an auditors' opinion on the audited financial statements. The auditor

issues a clean opinion when the financial statements are stated fairly in compliance with the accounting standards, and a modified opinion otherwise. Modified opinions include unqualified opinions with explanatory language, qualified opinions, no opinions, adverse opinions, and in some countries, going-concern opinions. Investors appreciate auditors' work and thus respond negatively to the issuance of modified opinions. U.S. studies document evidence of capital market penalties for firms that receive going-concern audit opinions, in the form of negative stock returns (Blay and Geiger 2001; Menon and Williams 2010), higher implied costs of equity capital (Amin, Krishnan, and Yang 2014), higher interest spreads (Chen, He, Ma, and Stice 2016), and increased selling of shares by institutional investors (Geiger and Kumas 2018). Similarly, the stock market responds negatively to going-concern opinions in the United Kingdom (U.K.) (Kausar et al. 2017) and to modified opinions in China (Chen, Su, and Zhao 2000).

An audit client is incentivized to engage in opinion shopping due to these negative consequences of modified opinions. Numerous studies using data from various countries, including Australia (Carey et al. 2008; Craswell 1988), China (Chan et al. 2006), the U.K. (Citron and Taffler 1992), and the U.S. (Chow and Rice 1982; Mutchler 1984), find evidence of clients switching auditors after receiving a modified audit opinion. Lennox (2000) points out that a client is successfully engaging in opinion shopping when the decision to switch or retain the incumbent auditor is associated with a reduction in the *expected* probability of receiving a modified opinion and, using this approach, finds evidence of successful opinion shopping in the U.K. Using Lennox's approach, Chung et al. (2019) and Newton et al. (2016) report that U.S. firms engage in opinion shopping to avoid receiving going-concern audit opinions and internal control weakness opinions, respectively. Other studies find that opinion shopping occurs not only through audit-firm

switches but also through audit-office switches (Chen, Francis, and Hou 2017) and audit-partner switches (Chen, Peng, et al. 2016).

Opinion shopping has been a longstanding regulatory concern due to its potential impact on auditors' independence (DeFond and Zhang 2014). Specifically, opinion shopping may reduce auditors' independence when used by clients to pressure incumbent auditors or search for auditors who are willing to yield to the clients' demands (Lennox 2000). Empirically, Chung et al. (2019) document that for firms that switched their auditors for opinion shopping motives, the successor auditors exhibit lower audit quality in various dimensions than auditors of clients that do not engage in opinion shopping. Accordingly, regulators worldwide endeavor to curb clients' opinion shopping through measures such as requiring firms to disclose the reasons for auditor changes and imposing a mandatory auditor rotation or retention policy (DeFond and Zhang 2014; Institute of Chartered Accountants in England and Wales 2002; Securities and Exchange Commission 1988).

2.2 Legal enforcement and opinion shopping

Regulators attempt to enhance firm transparency through strict enforcement of laws and regulations. Firms operating in countries with greater investor protection and higher enforcement levels engage less in earnings management than their peers in other countries (Beuselinck, Cascino, Deloof, and Vanstraelen 2019; Brown et al. 2014; Burgstahler et al. 2006; Lang et al. 2006; Leuz et al. 2003).[4] Auditors, especially large auditors, exhibit higher audit

[4] Using data from 31 countries, Leuz et al. (2003) find that firms in countries with greater investor protection are less likely to engage in earnings management because the country-level investor protection restricts corporate insiders from acquiring private control benefits. Similarly, Brown et al. (2014) show that the quality of the auditing environment and strength of accounting standards enforcement in a country largely explain financial market transparency and earnings management. Burgstahler et al. (2006) report that public and private firms in the European Union exhibit lower earnings management when country-level legal enforcement is stricter, and that legal enforcement reduces earnings management of public firms to a greater extent than that of private firms. Lang et al. (2006) show that cross-listing in the U.S. induces foreign firms to exhibit lower earnings management than non-cross-listed firms, suggesting that the disciplinary effect of strict legal enforcement in the U.S. extends to foreign firms. Moreover, Lang et al. (2006) find that cross-listed foreign firms exhibit lower earnings quality than U.S. firms, especially when the firm's home country has weak legal enforcement.

quality in countries with a higher level of legal enforcement (Choi, Choi, and Sohn 2018; Francis and Wang 2008; Fung et al. 2016; Michas 2011).

Increasing the strictness of legal enforcement may affect a client's opinion shopping behavior in two directions through its effects on firms and auditors. On the one hand, increased legal enforcement may restrict opinion shopping by disciplining audit clients and altering auditors' client acceptance policies. First, strict legal enforcement disciplines firms and incentivizes them to improve their corporate governance, which in turn reduces opportunistic behavior. La Porta et al. (2000) illustrate that strong investor protection is associated with effective corporate governance. Empirically, DeFond and Hung (2004) show that strong investor protection increases the sensitivity of CEO turnover to performance. Enhanced corporate governance reduces the agency costs between shareholders and managers (Jensen and Meckling 1976) and restricts managers' opportunistic behavior such as excessive CEO compensation, opinion backdating, and earnings management (e.g., Collins, Gong, and Li 2009; Core et al. 1999; Gompers et al. 2003; Gul, Chen, and Tsui 2003; Warfield, Wild, and Wild 1995). With respect to opinion shopping, Carcello and Neal (2003) find that an audit committee that includes more independent directors shields auditors from dismissals following the issuance of going-concern audit reports. Tan, Ramalingegowda, and Yu (2021) find that an increase in fiduciary duties toward creditors reduces the pressure placed by creditors on auditors, thus reducing the likelihood of auditor dismissals subsequent to the issuance of going-concern opinions.[5]

Leuz (2006) interprets the findings of Lang et al. (2006) as home-country institutions having continuing effects on cross-listed firms' behavior. Beuselinck et al. (2019) report that multinational corporations engage in earnings management through their subsidiaries located in countries with weak accounting enforcement levels.

[5] In additional tests, Newton et al. (2016) document that firms in the post-SOX era engage in opinion shopping for internal control weakness opinions but not for going-concern opinions. Similarly, Chung et al. (2019) report that non-accelerated filers are more likely than accelerated filers to engage in opinion shopping for going-concern opinions. The authors of both studies interpret the evidence as indicating a decrease in the relative importance of going-concern opinions over internal control opinions after the adoption of SOX, especially among accelerated filers, with a consequent reduction in opinion shopping

Second, strict legal enforcement induces auditors to become more conservative in their client acceptance policies, which would reduce the probability that auditors would accept an opinion-shopping client. Bockus and Gigler (1998) analytically demonstrate that auditors are more likely to discard risky clients when the risk of litigation increases. Choi et al. (2004) show that auditors' client portfolios become less risky under regimes with stricter liability than under their less strict counterparts. Kaplan and Williams (2012) confirm that increasing trends in regulatory scrutiny cause Big 4 auditors to shed riskier clients, such that financially distressed firms are more frequently audited by regional audit firms. Catanach, Irving, Williams, and Walker (2011) find that large auditors exhibit greater sensitivity to risks than small auditors when accepting clients whose preceding auditors had resigned. If auditors exhibit a reduced likelihood of accepting risky audit engagements under strict legal enforcement, opinion shoppers will find it difficult to identify successor auditors when switching away from their incumbent auditors for opportunistic motives, restricting opinion shopping behavior.

On the other hand, strict legal enforcement may trigger opinion shopping by strengthening the negative signals conveyed in modified audit opinions and increasing the gap between large and small auditors' quality. First, strict legal enforcement enhances the signaling effects of accounting and audits, creating greater incentives for firms to avoid receiving modified opinions. In countries with stronger institutions in terms of accounting standard-setting processes or legal origin, investors rely more heavily on disclosed accounting information (Ali and Hwang 2000; DeFond et al. 2007; Haw, Hu, Lee, and Wu 2012; Hung 2001; Rossi and Volpin 2004) and high-quality audits (El Ghoul, Guedhami, and Pittman 2016; El Ghoul, Guedhami,

for going-concern opinions. However, the evidence may also be interpreted as the suppression of opportunistic opinion shopping by strict corporate governance.

Pittman, and Rizeanu 2016; Gul et al. 2013).[6] As audit reports provide an assessment of financial information, the cost of qualification is higher for firms located in countries with strict enforcement than for firms in countries with more lenient enforcement. Kausar et al. (2017) directly compare the reactions to audit opinions in the U.S. and U.K. and find that the negative reaction to going-concern audit opinions is more severe in the latter country, suggesting that equity investors are more concerned with the auditors' opinion in when the bankruptcy code is more creditor-friendly. The increase in the signaling effect of audits and the corresponding increase in capital market penalties associated with modified opinions is expected to increase clients' incentives to engage in opinion shopping to avoid these penalties.

Second, strict legal enforcement increases the gap in quality between large and small auditors, creating greater opportunities for clients to engage in opinion shopping successfully. On average, auditors report more conservatively under strict legal enforcement than under weak enforcement (DeFond et al. 2000; Fargher and Jiang 2008; He, Pan, and Tian 2017).[7] More importantly, this increased audit quality is more pronounced for Big 4 than for non-Big 4 auditors. Francis and Wang (2008) show that the quality of Big 4 auditors, but not that of non-Big 4 auditors, is enhanced under strict investor protection regimes. Similarly, Fung et al. (2016) show that the tendency to issue modified opinions to important clients increases more among Big 4 than

[6] El Ghoul, Guedhami, and Pittman (2016) find that the clients of Big 4 auditors enjoy cheaper equity financing, i.e., a lower ex-ante cost of capital, than clients of non-Big 4 auditors, especially in countries with strong investor protection and high disclosure standards. Similarly, Khurana and Raman (2004) find that the high level of litigation exposure in Anglo-American countries induces Big 4 auditors to reduce the ex-ante cost of equity capital. El Ghoul, Guedhami, Pittman, and Rizeanu (2016) and Gul et al. (2013) find that Big 4 auditors successfully reduce debtors' agency costs, observed by increases in maturities and decreases in debt costs, respectively, in countries with strong institutions. These increases in the benefits of high-quality audits in such countries strengthen the demand for high-quality audits.

[7] Fargher and Jiang (2008) find an increase in auditors' likelihood of issuing going-concern opinions immediately after the enactment of SOX in 2002. DeFond et al. (2000) report that auditors in China exhibit increases in independence and the issuance of modified opinions since the strengthening of auditing standards. He et al. (2017) find that auditors in China are more likely to issue modified and going-concern audit opinions since the requirement for audit firms to transform from limited liability companies to limited liability partnerships, which increases individual auditors' exposure to litigation.

among non-Big 4 auditors as a country increases its auditing enforcement level. Choi et al. (2018) find that Big 4 auditors successfully suppress their client's real earnings management under strict accounting regimes to a greater extent than non-Big 4 auditors. Michas (2011) finds that greater development of the audit profession in emerging market countries enables Big 4 auditors, but not non-Big 4 auditors, to limit the use of total and abnormal accruals and promote accounting conservatism. As clients engage in opinion shopping when they believe that the current auditor will be too conservative than a new auditor (Dye 1991; Teoh 1992), the increased gap in audit quality between Big 4 and non-Big 4 auditors may enhance the expected effectiveness of opinion shopping and trigger clients to engage in this opportunistic behavior.

Collectively, stricter legal enforcement may restrict and/or trigger opinion shopping behavior. Drawing on these opposing possibilities, I present my hypothesis in the null form.

Hypothesis. The strictness of legal enforcement does not affect a firm's tendency to engage in opinion shopping.

3. Research Design

3.1 Measuring the strictness of legal enforcement

I measure the strictness of legal enforcement in a country using the accounting and audit enforcement index proposed by Brown et al. (2014), which is considered the most relevant proxy for legal enforcement governing auditors. Brown et al. construct a composite index for 51 countries that comprises two measures: the quality of auditors' working environment (auditors' environment index) and the level of accounting standards enforcement (accounting enforcement index). They collect information from various sources, including the International Federation of Accountants, the Federation of European Accountants (Fédération des Experts-comptables Européens), the World Bank, and national securities regulators. The auditors' environment index incorporates information on the existence and extensiveness of auditor

licensing requirements, the existence of peer-reviewed quality assurance programs within the profession, the existence and authority of an audit oversight body, policies for mandatory auditor rotation, the average level of audit fees, and the level of litigation risk. The accounting enforcement index identifies whether a country's financial regulators monitor financial reporting, have the power to set accounting auditing and auditing standards, review financial statements, take enforcement actions, and have sufficient resources. Brown et al. measure both indices for the years 2002, 2005, and 2008 to capture the effects of various reforms that occurred during this period and the adoption of the International Financial Reporting Standards (IFRS) in many countries.[8]

I calculate the country-level legal enforcement strength, *Enforce*, as the sum of the two indices of the closest year: 2006 and earlier years are matched with the score for 2005, and 2007 and subsequent years are matched with the score for 2008. I scale this variable using the maximum score of 56 to yield values in a range between 0 and 1.

For subsample analyses, I bisect the sample according to the value of *Enforce* at the country-year level. Specifically, weak legal enforcement refers to country-years with a below-median level of *Enforce*, and strong legal enforcement to those with an above-median level of *Enforce*. The weak enforcement subsample includes 360 country-years from 28 countries, and the strong enforcement subsample includes 336 country-years from 23 countries. Three countries, Malaysia, the Netherlands, and Portugal, are classified in different legal enforcement subsamples depending on the year.

[8] Brown et al. (2014) show that this measure explains economic growth and capital market development better than other previously-examined proxies for legal enforcement such as general enforcement levels, the rule of law, or the legal origin. Subsequent studies use the measure by Brown et al. to examine the impact of auditing enforcement on various financial market characteristics (Beuselinck et al. 2019; Cascino and Gassen 2015; Glaum, Landsman, and Wyrwa 2018; Preiato, Brown, and Tarca 2015).

Note that the number of firm-year observations in each subsample differs because the median of *Enforce* is calculated at the country-year level.

3.2 Identifying opinion shopping

I identify opinion shopping using two different approaches. The first model is shown in equation (1) and is estimated using a linear probability model.[9]

$$Switch_{t+1} = \beta_1 Modified_t + \beta_2 Enforce_t + \beta Controls_t + \varepsilon_{t+1}. \quad (1)$$

Switch is an indicator variable that equals 1 if the auditor in year $t + 1$ differs from the auditor in year t and 0 otherwise. *Modified* equals 1 if the audit opinion is modified and 0 if the audit opinion is clean. Modified opinions include unqualified opinions with additional language, qualified opinions, no opinions, and adverse opinions.[10] A positive value of β_1 is consistent with opinion shopping, as it indicates that firms tend to switch auditors following the issuance of modified opinions.

The second model follows the approach of Lennox (2000), who notes that effective opinion shopping should be identified by examining whether an auditor switch is associated with expected (unobserved) audit opinions rather than actual (observed) audit opinions. Specifically, opinion shopping is identified as an auditor switch (retention) that occurs when the expected probability of modified opinions decreases (increases) with the switch than with an auditor retention (switch). Following Lennox (2000), I estimate the

[9] I use a linear probability model because of the ease of interpreting the coefficients on interaction terms and the sum of coefficients on different variables. The magnitudes and signs of interaction terms in logit and probit models do not equal the marginal effects and must be interpreted cautiously (Ai and Norton 2003). Results estimated using a linear probability model differ from those estimated using other maximum likelihood estimators, such as logit or probit estimations, and these differences are most significant when the data include extreme observations. The variables of interest in this study are less subject to outliers because they are either dummy variables, i.e., *Switch* and *Modified*, or scaled variables that lie between 0 and 1, i.e., *Prob Shop* and *Enforce*. Therefore, I believe that the results of this study are less likely to be affected by the choice of estimation model. I find that the main test results are robust to the use of logit or probit estimations.

[10] Note that Compustat Global does not distinguish going-concern audit opinions from other modified opinions.

probability of receiving a modified opinion in year $t + 1$ as shown in equation (2), using a probit estimation.

$$\begin{aligned}\Pr(Modified_{t+1} = 1) \\ = \beta_1 Modified_t + \beta_2 Enforce_t + \beta Controls_t \\ + \beta_3 Switch_{t+1} + Switch_{t+1} \times (\gamma_1 Modified_t \\ + \gamma_2 Enforce_t + \gamma Controls_t) + \varepsilon_{t+1}.\end{aligned} \quad (2)$$

Equation (2) estimates the probability of an opinion modification in year $t + 1$ depending on whether the client did or did not switch auditors after fiscal year *t*. Using the estimated coefficients of equation (2), I calculate the probability of an opinion modification for each firm-year twice: the first calculation assumes that the auditor was switched, $Prob\ Modified^1 \equiv \Pr(Modified_{t+1} = 1 | Switch_{t+1} = 1)$, and the second assumes that the auditor was retained, $Prob\ Modified^0 \equiv \Pr(Modified_{t+1} = 1 | Switch_{t+1} = 0)$. The probability that opinion shopping would cause a firm to switch auditors, *Prob Shop*, is calculated as the difference between the two estimated probabilities, $Prob\ Modified^0 - Prob\ Modified^1$. A higher value of *Prob Shop* indicates that a firm is more likely to receive a clean (modified) opinion if it switches to a new auditor (retains the incumbent auditor). Note that I intentionally subtract $Prob\ Modified^1$ from $Prob\ Modified^0$ rather than the other way around, as done in other studies (Chung et al. 2019; Lennox 2000). This modification ensures that the interpretations of the regression results using the two models are consistent with each other.[11]

Equation (3) is the second model for identifying opinion shopping, which uses *Prob Shop* as the test variable.

[11] I make two main adjustments to Lennox's (2000) model. First, I reverse the sign of the test variable such that $Prob\ Shop = (-1) \times \left[\Pr(\hat{Q}_{it}^{q1} = 1) - \Pr(\hat{Q}_{it}^{q0} = 1)\right]$. I reverse this sign to easily compare the test results of equation (3) with those of equation (1). Second, in predicting the probability of modified opinions, I control for one-year-lagged control variables rather than contemporaneous variables. This is because firms' decisions to switch auditors for the year $t + 1$ are made based on information available before the switch, i.e., financial information of year *t*. In untabulated tests, I confirm that my results are robust when *Prob Shop* is estimated using contemporaneous control variables as in prior studies.

$$Switch_{t+1} = \beta_1 Prob\ Shop_t + \beta_2 Enforce_t + \beta Controls_t + \varepsilon_{t+1}. \quad (3)$$

A positive value of β_1 is consistent with opinion shopping, as it indicates that firms are more likely to switch auditors when the switch is expected to reduce the probability of receiving a modified opinion than in other cases.

The following control variables are included in equations (1) to (3) in accordance with the literature on auditor turnover (Carcello and Neal 2003; Chung et al. 2019; Tan et al. 2021). *Enforce* is the ranked index of audit enforcement, as described in the previous section. *Log Assets* is the natural logarithm of total assets in millions of U.S. dollars. *Leverage* is total debt (long-term and current) divided by total assets. *Cash Flows* is cash flows from operations reported in the cash flow statement divided by total assets. *Loss* is an indicator that equals 1 for firms reporting negative income before extraordinary items and 0 otherwise. *Market-to-Book* is the market value of equity divided by the book value of equity, where the market value of equity is calculated by multiplying the stock price at the fiscal year-end by the number of common shares outstanding. *Future Financing* is an indicator that equals 1 if the subsequent-year increase in common stock, preferred stock, and debt exceeds one percent of beginning-of-year total assets and 0 otherwise. *Returns* is cumulative abnormal stock returns for fiscal year *t*, which is the sum of abnormal returns calculated as a firm's daily stock return less the daily value-weighted market return. *Return Volatility* is the standard deviation of daily abnormal returns in year *t*. *Auditor Tenure* is the number of consecutive years in which the incumbent auditor has audited the firm and is transformed in logarithmic form, *Log Auditor Tenure*, for the use in the regressions. *Auditor's Shares* is the percentage of clients' sales within a country-industry-year that is audited by the incumbent auditor; an industry is defined at the two-digit Standard Industry Classification (SIC) level. All continuous control variables are winsorized at the extreme one percent level

to reduce the effect of outliers in the empirical analyses. I include country, industry, and year fixed effects to control for average differences within each group. I cluster standard errors by firm to control for potential interdependence among observations within a firm.

3.3 Empirical model

To test the hypothesis, I interact *Enforce* with the test variables in equations (1) and (3) as follows.

$$Switch_{t+1} = \beta_1 Modified_t + \beta_2 Modified_t \times Enforce_t + \beta_3 Enforce_t + \beta Controls_t + \varepsilon_{t+1}. \quad (4)$$

$$Switch_{t+1} = \beta_1 Prob\ Shop_t + \beta_2 Prob\ Shop_t \times Enforce_t + \beta_3 Enforce_t + \beta Controls_t + \varepsilon_{t+1}. \quad (5)$$

The variables of interest are the interaction terms in the two equations, i.e., *Modified* × *Enforce* and *Prob Shop* × *Enforce*. $\beta_2 > 0$ indicates that firms in countries with stricter legal enforcement are more likely than firms in other countries to engage in opinion shopping, and $\beta_2 < 0$ indicates that stricter legal enforcement reduces opinion shopping.

3.4 Sample and data

I obtain data on non-U.S. firms for years 2004 to 2018 from Compustat Global.[12] I start my sample period in 2004 to eliminate the effect of large disruptions on audit contracts, observed after the collapse of Arthur Andersen and the implementation of SOX (DeFond and Lennox 2011). The sample ends in 2018 because auditor information is fully available only up to 2019, and

[12] Compustat Global is suspected to provide inaccurate auditor information. Specifically, Francis and Wang (2008) note that the database includes a low percentage of firms audited by Big 4 auditors in Japan, South Korea, India, and Pakistan and suspect potential miscoding of auditor information. As the sample in this study only includes the clients of Big 4 auditors, the dataset is less affected by the potential miscoding of information. For example, my sample does not include Japan and South Korea due to the small number of observations reported to be audited by Big 4 auditors in those countries. In addition, I find that the average ratios of Big 4 auditors in Pakistan were close to 0 before 2004 but increase to above 10% in 2005 and 2006 and to above 20% beginning in 2007. Similarly, the ratios in India increase to above 10% beginning 2006. Thus, the miscoding issue seems to be mitigated in recent periods for these two countries. Nevertheless, the results of this study are robust to excluding observations from Pakistan and India.

my analysis requires available auditor information for the subsequent year to identify firms that switch auditors in that year.

Following Newton et al. (2016), I limit my sample to firms audited by Big 4 auditors in year *t* to ensure that differences in clientele (Chaney, Jeter, and Shivakumar 2004; Lawrence, Minutti-meza, and Zhang 2011) and audit service quality (Becker, DeFond, Jiambalvo, and Subramanyam 1998; DeAngelo 1981; DeFond, Erkens, and Zhang 2016; Francis, Maydew, and Sparks 1999; Krishnan 2003; Teoh and Wong 1993) do not drive the results.[13] I also exclude financial firms (i.e., those with two-digit SIC codes from 60 to 69) from the sample to ensure the comparability of financial statements across firms. I limit my sample to observations with positive sales and assets to ensure the reliability of accounting information. I require firms to have non-missing information on audit reports for three consecutive years ($t - 1$, t, and $t + 1$) to identify current and subsequent auditor switches and audit report modifications.[14] Only firm-year observations with non-missing control variables are retained.[15] Lastly, I require at least 20 observations per country to ensure that comparisons across countries are not driven by a few observations in a certain country.

The final sample includes 93,793 firm-year observations from 48 countries. Panel A of Table 1 presents the frequency and average of the main test variables in the sample by country, where the test variables exhibit

[13] Moreover, as Big 4 auditors apply consistent audit methodologies across jurisdictions (Ege, Kim, and Wang 2020), their reporting practices are more comparable across countries.

[14] Auditor switches are measured reliably for only Big 4 auditors because Compustat Global does not contain sufficient information on the identities of non-Big 4 auditors. Specifically, the auditor codes (AU) range from 1 to 27, and codes 1 to 8 are assigned to Big N auditors, i.e., Arthur Andersen and the current Big 4 auditors before the mergers. In addition, many non-Big 4 auditors identified using separate codes operate only in the United States. In my sample period, only eight non-Big 4 auditors are identified as having more than 10 clients outside the United States, and 59.7 percent of all observations in the sample are classified as being audited by "other auditors" (code 9). Therefore, it is not plausible to identify auditor switches for a large portion of the firms audited by *other auditors*.
Worldscope, an alternative international database, also does not solve this issue because it only contains the identity of the most recent auditor and not historical auditor information.

[15] All financial statement variables are translated from the local currency to U.S. dollars. Balance sheet items are translated using the exchange rate as of the fiscal year-end, and income statement items are translated using the 12-month average exchange rate of the fiscal year.

significant variation across countries. Taiwan has the largest number of observations (n = 12,170), while the Philippines and Ukraine are tied for the smallest number of observations (n = 39 each). Across countries, the percentage of firms that switch auditors in the subsequent year ranges from 3.5 (Canada) to 66.7 (Philippines), with an average of 10.6 percent. The percentage of firms that receive a modified opinion from their auditors ranges from 4.9 (China) to 83.8 (France), with an average of 20.6 percent. The enforcement scores range from 3.9 (Ukraine) to 96.2 percent (Great Britain and Canada), with an average of 62.6 percent. The percentage of Big 4 auditor shares calculated within the industry ranges from 29.2 (India) to 98.2 (Hungary), with an average of 82.1 percent.

Panel B of Table 1 presents the frequency and average values of the test variables by year. The sample is evenly distributed across the studied period. The percentage of auditor switches is highest in 2004, 18.7 percent, and lowest in 2014, 6.8 percent. The percentage of firms receiving modified opinions fluctuates from 11.0 to 26.8 percent over the sample period. Other variables remain stable across years.

[Insert Table 1 about here]

4. Empirical Results

4.1 Descriptive statistics

Table 2 presents descriptive statistics of the variables used in the main analyses. On average, 10.6 percent of firms switch auditors in the subsequent year (*Switch*), and around 20.6 percent of firms receive modified opinions from their auditors (*Modified*). The average enforcement score is 62.6 percent (*Enforce*). The average firm is 21.1 percent leveraged (*Leverage*) and realizes operating cash flows that are 5.9 percent of total assets (*Cash Flows*), and has a market-to-book ratio of 2.231 (*Market-to-Book*). Additionally, 24.2 percent of firms recognize accounting losses (*Loss*), and 47.3 percent turn to external financing in the subsequent year (*Future Financing*).

[Insert Table 2 about here]

Table 3 reports the Pearson correlations between variables used in the study. Auditor switches (*Switch*) are more likely to occur for firms that receive modified audit opinions (*Modified*) and in countries with more lenient legal enforcement (*Enforce*). Firms that are smaller (*Log Assets*), more highly leveraged (*Leverage*), less profitable (*Cash Flows*, *Loss*, *Returns*), and riskier (*Return Volatility*) are more likely to switch auditors in the subsequent year. Regarding auditor characteristics, a shorter tenure (*Log Auditor Tenure*), and smaller market shares (*Auditor's Shares*) are associated with an increased likelihood of auditor switches in the subsequent year.

The probability of receiving modified opinions is larger for firms subject to lenient legal enforcement (*Enforce*) and firms that are smaller (*Log Assets*), more highly leveraged (*Leverage*), less profitable (*Cash Flows*, *Loss*, *Returns*), and riskier (*Return Volatility*), and have lower growth opportunities (*Market-to-Book*). Auditors with a longer tenure (*Auditor Tenure*) and a larger market share (*Auditor's Shares*) are less likely than their peers to issue modified opinions.

[Insert Table 3 about here]

4.2 Predicting the probability of opinion shopping

Following Lennox (2000), I first estimate the probability of receiving modified opinions in the subsequent year, conditional on the decision to switch auditors. Table 4 presents the results of estimating equation (2) with a probit estimation. The coefficient on *Switch* is significantly positive (coefficient = 0.842, χ^2-statistic = 123.92), suggesting that a firm is more likely to receive a modified opinion after an auditor switch. The coefficient on *Modified* is significantly positive (coefficient = 1.648, χ^2-statistic = 133,344.90) confirming the persistence of audit opinions (Lennox 1999). The coefficient on *Switch* × *Modified* is significantly negative (coefficient = –

0.864, χ^2-statistic = 607.91), suggesting that auditor switches help reduce the persistence of audit opinions.

The coefficient on *Enforce* is significantly negative (coefficient = –0.245, χ^2-statistic = 7.86), and that on *Switch* × *Enforce* is significantly positive (coefficient = 0.476, χ^2-statistic = 62.07). Although a negative coefficient on *Enforce* may be inconsistent with the higher conservativeness of auditors in countries with stricter legal enforcement, it is consistent with audit clients having better corporate fundamentals and accounting infrastructure in these countries (Brown et al. 2014); accordingly, these clients are less likely than those in other countries to receive a modified audit opinion. Nevertheless, a positive coefficient on *Switch* × *Enforce* indicates that auditors in such countries view an auditor switch as a signal of low audit quality, and thus are more likely to modify their opinions.

The coefficients on the control variables are consistent with those reported in prior studies. For example, firms are more likely to receive modified opinions when they are larger (*Log Assets*),[16] are more highly leveraged (*Leverage*), are less profitable (*Cash Flows*, *Loss*, *Returns*), are more volatile (*Return Volatility*), and expect to finance in the future (*Future Financing*). Interestingly, the coefficients on the interactions between *Switch* and the control variables tend to have signs that are opposite to those on the individual control variables, suggesting that the audit opinions of new auditors are less sensitive to the clients' financial information than the opinions of incumbent auditors are.

[16] Theoretically, firm size may have opposing effects on audit opinion modifications. Since large firms have a wider stakeholder base, auditors may be more concerned with such firms and modify their opinions more frequently. In contrast, to the extent that large firms are less risky and exhibit higher reporting quality, they may receive modified opinions less. In an international study examining modified opinions, Chen, Zhang, and Zhou (2017) report a positive coefficient on total assets, whereas Fung et al. (2016) report a negative coefficient. The difference between the two lies in the sample selection, where the former focuses on Big 4 audited clients only (as in this study), and the latter includes both Big 4 and non-Big 4 audited clients. Other single-country studies mostly report a lower probability of opinion modifications negative coefficient on firm size (Chen, Sun, and Wu 2010; Chung et al. 2019; Geiger and Rama 2003; Krishnan and Krishnan 1996; Lim and Tan 2008; Reichelt and Wang 2010).

Using the results in Table 4, I construct *Prob Shop* as described in Section 3.2. The average and median values of *Prob Shop* are negative (– 0.158 and – 0.172, respectively, untabulated), suggesting that the average firm does not expect to receive a more favorable audit opinion after an auditor switch. 10.9 percent of the firms have a positive value of *Prob Shop* (untabulated), suggesting that these firms have a higher likelihood of receiving a clean opinion if they switch, rather than retain, their auditors.

[Insert Table 4 about here]

4.3 Univariate analysis

I start with a univariate analysis. Panel A of Table 5 presents the mean percentage of auditor switches in year $t + 1$ depending on the audit opinion received in year t. In the full sample, as reported in columns (1) and (2), 13.9 percent of the 19,280 firms that receive a modified audit opinion switch their auditors in the subsequent year, a value 4.1 percent points greater than the mean percentage of auditor switches among firms that receive a clean audit opinion of 9.8 percent. This difference is statistically significant at the one-percent level (z-statistic = 15.19).[17] Thus, firms switch auditors more often when the incumbent auditor issues a modified opinion than when the auditor issues a clean opinion, providing preliminary evidence of opinion shopping in the international sample.

The next columns of Panel A, Table 5 compare between countries with weak and strong legal enforcement in terms of the association of audit opinions and auditor switching.[18] In the subsample of countries with weak legal enforcement reported in columns (3) and (4), 13.6 percent of firms with a modified opinion and 13.1 percent of firms with a clean opinion switch their

[17] The statistical significance of the difference in mean values between subsamples is calculated based on z-statistics following Clogg, Petkova, and Haritou (1995).

[18] The percentage of firms receiving modified opinions is smaller in a strong legal enforcement country, 16.9 percent (= 10,847 / 64,032) than those in a weak legal enforcement country, 39.5 percent (= 8,433 / 29,761). While this may seem contrary to the findings that auditors are more conservative in countries with stronger audit enforcement, it is more likely caused by the higher quality of firm-generated information in such countries.

auditors, and the difference between these values is statistically insignificant at the ten-percent level (difference = 0.005, *z*-statistic = 1.14). In contrast, in the subsample of countries with strong legal enforcement reported in columns (5) and (6), 14.1 percent of firms with modified opinions and 8.4 percent of firms with clean opinions switch their auditors, and this difference is statistically significant at the one-percent level (difference = 0.057, *z*-statistic = 15.93). This result shows that an increase in auditor switching by firms receiving modified audit opinions is only observed in countries with strong legal enforcement. Columns (7) and (8) compare the weak and strong legal enforcement subsamples and show that the difference between countries with strong and weak legal enforcement in terms of the average auditor turnover among firms receiving modified opinions is significant at the one-percent level (difference-in-differences = 0.052, *z*-statistic = 9.14).

In Panel B of Table 5, I compare the mean percentage of auditor switches in year $t + 1$ depending on estimated probability that an auditor switch will result in an improvement in audit opinion (i.e., firms with negative and positive values of *Prob Shop*). Firms with negative values of *Prob Shop* are more likely to obtain a clean opinion if they retain the incumbent auditor, and those with positive values are more likely to obtain a clean opinion with a new auditor. In the full sample reported in columns (1) and (2), I find that 14.7 percent of firms with positive values of *Prob Shop* switch auditors compared with 10.1 percent of firms with negative values of *Prob Shop*, and this difference is statistically significant at the one-percent level (difference = 0.046, *z*-statistic = 12.61). Again, the evidence is consistent with firms' engagement in opinion shopping, such that firms are more likely to switch auditors when the auditor switch is expected to reduce the probability of receiving a modified opinion.

Next, I compare the frequency of auditor switches according to the strictness of legal enforcement in the country. I find significantly more auditor

switches in firms with positive than negative values of *Prob Shop* in countries with strong legal enforcement in columns (5) and (6) (difference = 0.080, *z*-statistic = 10.77), but not in countries with weak legal enforcement in columns (3) and (4) (difference = 0.007, *z*-statistic = 1.54). Columns (7) and (8) compare between subsamples of countries with strong and weak legal enforcement. The difference in the percentage of clients that switch auditors when *Prob Shop* is negative is significantly smaller under stronger legal enforcement than in weaker ones (difference = – 0.040, *z*-statistic = – 15.84) and the percentage when *Prob Shop* is positive is significantly higher (difference = 0.033, *z*-statistic = 3.95). Again, the difference-in-differences is statistically significant (difference-in-differences = 0.073, *z*-statistic = 8.39). The evidence suggests that auditor switches are more sensitive to the expected probability of receiving a clean opinion in countries with stricter legal enforcement than in others.

[Insert Table 5 about here]

4.4 Multivariate analysis

I next perform a multivariate analysis. I test the existence of opinion shopping in the international sample using equations (1) and (3) and report the results in Table 6. Columns (1) and (2) include industry and year fixed effects, and columns (3) and (4) additionally include country fixed effects to control for average differences in auditor switching tendencies across different countries. In column (1), the coefficient on *Modified* is significantly positive (coefficient = 0.029, *t*-statistic = 10.46), showing that the issuance of a modified opinion increases the likelihood of an auditor switch by 2.9 percent. Similarly, in column (2), the coefficient on *Prob Shop* is positive and significant (coefficient = 0.093, *t*-statistic = 7.86), suggesting that the higher the probability of an improvement in audit opinion after the switch, the more likely a firm will switch its auditor. The magnitudes and significance levels of the coefficients are retained when further including country-fixed effects

in columns (3) and (4), providing further evidence of opinion shopping in the international sample.

[Insert Table 6 about here]

Panel A of Table 7 presents the results of testing the hypothesis using equations (4) and (5). Again, columns (1) and (2) and columns (3) and (4) present the results without and with country fixed effects, respectively. In column (1), the coefficient on *Modified* × *Enforce* is significantly positive (coefficient = 0.064, *t*-statistic = 6.18), whereas the coefficient on *Modified* alone is negative and insignificant (coefficient = – 0.008, *t*-statistic = – 1.26), suggesting that stricter legal enforcement increases the probability of opinion shopping. Similarly, in column (2), the coefficient on *Prob Shop* × *Enforce* is significantly positive (coefficient = 0.114, *t*-statistic = 2.81), whereas the coefficient on *Prob Shop* is positive and insignificant (coefficient = 0.028, *t*-statistic = 1.10). When I further control for country fixed effects, the coefficients on *Modified* × *Enforce* (coefficient = 0.063, *t*-statistic = 5.64) in column (3) and on *Prob Shop* × *Enforce* (coefficient = 0.159, *t*-statistic = 3.57) remain significantly positive. Again, the magnitudes and significance levels of the test variables are similar in the presence and absence of country fixed effects. In terms of economic significance, using the results in column (3), an increase in *Enforce* from the first (0.365) to the third quartile (0.923) increases the probability of opinion shopping by 3.5 percent (= 0.558 × 0.063). Collectively, the results suggest that firms' tendency to engage in opinion shopping increases as the legal enforcement within the country becomes stricter.

The significant coefficients on the control variables have signs consistent with expectations based on the literature. For example, firms that are smaller (*Log Assets*), more highly leveraged (*Leverage*), less profitable (*Cash Flows*, *Loss*, *Returns*), and riskier (*Return Volatility*) have a higher probability of auditor switching than their counterparts, whereas firms that

expect to finance in the subsequent year (*Future Financing*) are less likely to switch auditors, potentially due to the negative signal of an auditor switch (Boone, Khurana, and Raman 2008; Francis et al. 2017). In my sample, auditor-related variables, such as tenure or market shares, do not affect the likelihood of auditor switches when country fixed effects are included.

Panel B of Table 7 presents the results of a subsample analysis, where I regress equations (1) and (3) separately on the weak and strong legal enforcement subsamples. In the weak legal enforcement subsample, the coefficients on *Modified* (coefficient = 0.002, *t*-statistic = 0.36) and *Prob Shop* (coefficient = 0.024, *t*-statistic = 1.29) are positive but insignificant as reported in columns (1) and (3), respectively. In contrast, in the strong legal enforcement subsample, the coefficients on both *Modified* (coefficient = 0.039, *t*-statistic = 8.68) and *Prob Shop* (coefficient = 0.118, *t*-statistic = 6.72) are significantly positive as reported in columns (2) and (4), respectively. Thus, the subsample results confirm those of the previous pooled analysis with interactions reported in Panel A of Table 7, suggesting that firms in countries with stricter legal enforcement are more likely than their counterparts to engage in opinion shopping.[19]

[Insert Table 7 about here]

4.5 Country-level analysis

I further corroborate the firm-level analysis with a country-level analysis. Specifically, I estimate equations (1) and (3) by country to obtain estimates

[19] The coefficients on the control variables are consistent with those in the full sample, as reported in Panel A of Table 7, although many of the coefficients become insignificant in the weak legal enforcement subsample. Interestingly, auditor tenure has an opposite effect in the two subsamples. Specifically, auditors in countries with weak legal enforcement are less likely to be switched as their tenure becomes longer, whereas auditors in countries with strict legal enforcement are more likely to be switched over time. Auditors with a larger market share in the industry are less likely to be switched only under weak legal enforcement, consistent with studies that demonstrate the increased effectiveness of auditor industry specialization as the legal environment becomes lenient (Kwon, Lim, and Tan 2007). Notably, although the coefficients in the weak legal enforcement subsample have weaker statistical significance than those in the strong legal enforcement subsample, the explanatory power of the model is greater in the former subsample (adjusted *R*-squared = 0.080) than in the latter (adjusted *R*-squared = 0.039).

of the coefficients on *Modified* and *Prob Shop*, i.e., β_1, for each country. I examine whether the magnitude and significance of the country-level coefficients can be explained by the legal enforcement level of the country.

The results of identifying opinion shopping by country are presented in Panel A of Table 8. Columns (1) and (2) present the estimated coefficients and *t*-statistics of the coefficient on *Modified*, and columns (3) and (4) present those of the coefficient on *Prob Shop*. Columns (5) and (6) present the average of *Enforce* and the classification of each country into either the weak or strong legal enforcement subsample, which replicates the figures reported in columns (5) and (6) in Panel A of Table 1. The countries are sorted by the magnitude of the *t*-statistics in column (2). I find that, on average, *Enforce* decreases in value across the rows as the *t*-statistics values decrease.

Panel B of Table 8 presents the results of a univariate analysis. On average, the coefficients on both *Modified* and *Prob Shop* are negative and insignificant in the weak legal enforcement subsample but positive and significant in the strong legal enforcement subsample. Thus, the country-level analysis provides further evidence that opinion shopping is triggered under stricter legal enforcement.

[Insert Table 8 about here]

4.6 The channels

4.6.1 Legal enforcement and the signaling effect of audit opinions

I hypothesize that stricter legal enforcement increases opinion shopping through two channels: the signaling effect of audit opinions and the gap in audit quality between large and small auditors. I perform additional analyses to examine which of these two channels drive the results documented above.

I start with an examination of the first potential channel, i.e., whether the audit signals become stronger under stricter legal enforcement.

Specifically, I examine how annual stock returns differ between firms with clean and modified audit opinions using the following equation.[20]

$$\begin{aligned} CumulativeAbnormalReturns_t = {} & \beta_1 \Delta Earnings_t \\ & + \beta_2 Modified_t + \beta_6 LogMarketCapitalization \\ & + \beta_7 Leverage + \beta_8 MarketToBook \\ & + Country, Industry, Year\ Dummies + \varepsilon_t. \end{aligned} \quad (6)$$

The dependent variable, *CumulativeAbnormalReturns*, is cumulative abnormal returns for the 12 months starting three months after the beginning of the year; abnormal returns are calculated as a firm's raw returns less the value-weighted market return of the stock exchange. *ΔEarnings* is the change in earnings divided by beginning-of-year market capitalization. *Log Market Capitalization* is the natural logarithm of beginning-of-year market capitalization. *Leverage* and *Market-to-Book* are as defined in Section 3.2.

Panel A of Table 9 reports the results of this analysis. In column (1), the coefficient on *ΔEarnings* is significantly positive (coefficient = 0.416, *t*-statistic = 34.92), consistent with investors responding to firms' earnings information. The coefficient on *Modified* is significantly negative (coefficient = – 0.034, *t*-statistic = – 7.64), consistent with modified opinions conveying negative information to investors. Columns (2) and (3) provide the estimation results for the weak and strong legal enforcement subsamples, respectively. Although the signs of the coefficients on *Modified* in both columns remain negative, the coefficient is statistically significant only in the strict legal enforcement subsample (coefficient = – 0.066, *t*-statistic = – 9.73) and not in the weak legal enforcement subsample (coefficient = – 0.005, *t*-statistic = – 0.87). The difference in coefficient magnitudes between the two subsamples is also statistically significant (difference = – 0.061, *t*-statistic = – 6.72) as reported in column (4). The result suggests that stricter legal enforcement

[20] Alternatively, I estimate a similar regression using the two- or three-day stock returns around earnings announcement dates and find that the negative returns to firms with modified audit opinions increase with the strictness of legal enforcement (untabulated).

strengthens the negative signal of a modified opinion conveyed to investors.[21] In subsequent tests, I calculate audit signals at the country-year level, *Signaling*, as the coefficient on *Modified* in equation (6) estimated within each country-year multiplied by (–1), such that a higher value indicates a stronger negative signal conveyed by modified opinions.

4.6.2 Legal enforcement and the quality gap among auditors

Next, I examine the second potential channel, i.e., whether the difference in audit quality between Big 4 and non-Big 4 auditors becomes larger under stricter legal enforcement. Note that the sample used in this analysis is expanded to include non-Big 4 auditors. Specifically, I examine how the size of accruals differ for firms audited by Big 4 and non-Big 4 auditors with the following equation.

$$Abs.Disc.Accruals_t = \beta_1 Big\ 4_t + \beta\ Controls + Country,\ Industry,\ Year\ Dummies + \varepsilon_t. \quad (7)$$

The dependent variable, *Abs. Disc. Accruals*, is the absolute value of performance-matched discretionary accruals. Discretionary accruals are measured by the residuals from the modified Jones' (1991) model. I adjust for firm performance by subtracting the median value of discretionary accruals within the return-on-asset decile in each industry-year from the firm's discretionary accruals (Kothari, Leone, and Wasley 2005). *Big 4* is an indicator variable that equals 1 if the firm is audited by one of the Big 4 auditors and 0 otherwise. Control variables are identical to those included in equation (1) except for the exclusion of *Log Auditor Tenure* and *Auditor's Shares* that cannot be calculated for non-Big 4 auditors. I expect the coefficient on Big 4, β_1, to be more negative in the strong legal enforcement

[21] It is difficult to infer causality from annual return regressions. On the one hand, a negative association between annual returns and audit opinions may indicate that investors respond more to the issuance of audit opinions. On the other hand, a negative association may indicate that auditors incorporate the information in stock returns to a greater degree when expressing an opinion. I do not differentiate between the two interpretations since a stronger negative association indicates that audit reports are more informative about the firms' financial status in either case.

subsample if Big 4 auditors are more sensitive to the strictness legal enforcement than non-Big 4 auditors (Francis and Wang 2008).

Panel B of Table 9 reports the results of the ordinary least square estimation of equation (7). In column (1), the coefficient on *Big 4* is significantly negative (coefficient = – 0.003, *t*-statistic = – 4.56), consistent with Big 4 auditors providing higher-quality audits on average in the international sample. Columns (2) and (3) provide the estimation results for the weak and strong legal enforcement subsamples, respectively. The coefficient on *Big 4* is significantly negative only in the strong legal enforcement subsample (coefficient = – 0.005, *t*-statistic = – 6.14) but insignificant in the weak legal enforcement subsample (coefficient = 0.001, *t*-statistic = 0.060). The difference in coefficient magnitudes between the two subsamples is statistically significant (difference = – 0.006, *t*-statistic = – 4.34) as reported in column (4). The result suggests that Big 4 auditors provide higher audit quality only under strong legal enforcement levels, again confirming the findings of Francis and Wang (2008). For subsequent tests, I measure the gap in audit quality among auditors at the country-year level, *Quality Gap*, as the coefficient on *Big 4* in equation (7) estimated within each country-year multiplied by (–1), such that a higher value indicates a larger gap between Big 4 and non-Big 4 auditors' quality.

4.6.3 The moderating effect of audit signals and the quality gap

I now examine whether the stronger signaling effect of audit opinions and the larger gap in audit quality between Big 4 and non-Big 4 auditors moderate the effect of the strictness of legal enforcement on opinion shopping. I regress the following equation on subsamples of weak and strong legal enforcement.

$$\begin{aligned} Switch_{t+1} = {} & \beta_1 Prob\ Shop_t \\ & + \beta_2 Prob\ Shop_t \times Strong\ Signaling_t \\ & + \beta_3 Prob\ Shop_t \times Large\ Quality\ Gap_t \\ & + \beta_4 Strong\ Signaling_t + \beta_5 Large\ Quality\ Gap_t \\ & + \beta Controls_t + \varepsilon_{t+1}, \end{aligned} \quad (8)$$

where *Strong Signaling* indicates above-median levels of *Signaling*, and *Large Quality Gap* is an indicator for above-median levels of *Quality Gap*. Panel C of Table 9 presents the results of the cross-sectional analysis. Most importantly, I focus on the results of testing the strong legal enforcement sample, reported in columns (5) and (6). The coefficient on *Modified* (*Prob Shop*) × *Strong Signaling* is significantly positive, suggesting that the stronger signaling effect of modified opinions cause firms to engage in opinion shopping under strong legal enforcement levels. The coefficient on *Modified* (*Prob Shop*) × *Large Quality Gap* is also significantly positive, suggesting that the difference in audit quality between Big 4 and non-Big 4 auditors acts as a trigger for clients' opinion shopping behavior under strict legal enforcement. Moreover, the magnitudes of the coefficients on the two interaction terms are not significantly different from each other in the strong legal enforcement subsample. The results show that the observed increase in opinion shopping under strict legal enforcement is channeled through both the signaling effects of modified opinions and the quality gap among auditors within the country.

[Insert Table 9 about here]

5. Additional Analyses

5.1 Subsequent audit quality

To understand the consequences of increased opinion shopping under strict legal enforcement, I perform two additional tests of audit quality. First, I examine the direction of auditor switches. If firms engage in opinion shopping for opportunistic motives, then the related auditor switch is more likely to occur from high- to low-quality auditors, i.e., from Big 4 to non-Big 4 auditors, than within the same auditor type, i.e., from Big 4 to Big 4 auditors (Chung et al. 2019; Newton et al. 2016).[22] I define *Switch to Big 4* as a dummy

[22] As the sample only includes firms audited by Big 4 auditors in year *t*, only lateral and downward switches are observed in the data.

variable that equals 1 if the firm switches its auditor and the subsequent auditor is another Big 4 auditor and 0 otherwise. Similarly, *Switch to Non-Big 4* is a dummy variable that equals 1 if the firm switches its auditor and the subsequent auditor is a non-Big 4 auditor and 0 otherwise. In the sample, 4.2 percent of firms make lateral switches to other Big 4 auditors, and 6.5 percent make downward switches to non-Big 4 auditors (untabulated).

I use the two different types of auditor switches as alternative dependent variables in equation (4) and present the results in Panel B of Table 10. Columns (1) to (4) and columns (5) to (8) present the results of testing lateral auditor switches and downward switches, respectively. I find significantly positive coefficients on the test variables only for auditor switches to non-Big 4 auditors in columns (5) to (8), confirming that opinion shopping more likely involves downward than lateral auditor switches. Moreover, the differences between the strong and weak legal enforcement subsample in terms of the coefficients on *Modified* (difference = 0.029, *t*-statistic = 5.66) and *Prob Shop* (difference = 0.081, *t*-statistic = 3.83) are statistically significant. Thus, strict legal enforcement incentivizes firms to switch to non-Big 4 auditors for opinion shopping motives.

[Insert Table 10 about here]

Second, I examine the changes in audit quality after opinion shopping. Specifically, I regress *Abs. Disc. Accruals* on an indicator for opinion shoppers, *Opinion Shopping*. I define *Opinion Shopping* as an indicator variable that equals 1 for firms that have positive values of *Prob Shop* (*Prob Shop* > 0) and switched the auditor in the subsequent period (*Switch* = 1), and 0 otherwise. I control for *Switch* to distinguish the effect of opinion shopping from auditor switches in general. I measure the control variables in year $t + 1$.

Columns (1) and (2) of Table 11 presents the empirical results on the weak and strong legal enforcement subsamples, respectively. The coefficient on *Opinion Shopping* is insignificant in column (1) (coefficient = – 0.01; *t*-

statistic = – 0.32) and significantly positive in column (2) (coefficient = 0.26; *t*-statistic = 2.61), suggesting that opinion shopping impairs audit quality only under strong legal enforcement. The difference between the two coefficients is statistically significant (difference = 0.027; *t*-statistic = 2.57). Columns (3) and (4) further decompose opinion shoppers according to the type of the subsequent auditor. Under strong legal enforcement, the coefficient on *Opinion Shopping* is significantly positive only for switches to non-Big 4 auditors (coefficient = 0.027, *t*-statistic = 2.30), confirming that downward auditor switches cause a greater impairment audit quality.

Collectively, these additional analyses suggest that the increased opinion shopping observed under strict legal enforcement occurs for opportunistic purposes (i.e., switches are made from high- to low-quality auditors) and harms the quality of subsequent audits.

[Insert Table 11 about here]

5.2 Dismissals or resignations

Although I assume that the observed auditor switches are initiated by clients, it is equally possible that auditor switches occur due to auditor resignation. For example, auditors in countries with stricter legal enforcement may be more sensitive to client risks and more likely to discard risky clients than their peers in more lenient jurisdictions (Bockus and Gigler 1998; Shu 2000).[23]

[23] In the model proposed by Bockus and Gigler (1998), firms choose an auditor to minimize the sum of the audit fees, the expected penalties (if the auditor detects risk), and the opportunity cost of hiring wealth-constrained auditors (i.e., the reputational benefits lost by not choosing an unconstrained auditor). Firms may or may not possess hidden risks, and auditors learn about these risks only after assuming the client. Hidden risks create an adverse selection problem for auditors. After the initial-year audit, an auditor becomes aware of the client's risks, albeit with error. An incumbent auditor then prices these risks into the subsequent year's audit fees. However, as firms without actual hidden risks would choose to switch to an auditor offering a lower price, only "lemons" (i.e., high-risk firms) would accept the incumbent auditor's offer. Expecting the clients' choice, the incumbent auditor offers a price assuming that the client is a high-risk firm. In effect, an auditor resigns from a firm with hidden risks by offering a high price that no client would accept. In equilibrium, only a wealth-constrained auditor assumes the client since s/he does not expect to pay liability costs to the full extent (i.e., $W \leq L$). In this setting, increasing auditors' liability costs increases auditor resignation; increases risk detection for incumbent auditors; but reduces risk detection for new auditors that assume the clients after resignation of prior auditor.

First, to examine whether the findings are driven by increased client selectivity among large auditors, I compare the characteristics of the retained and switched audit clients for each auditor. If auditors choose to resign from clients with modified opinions to reduce risk in their own portfolios, then clients who remain in an auditor's portfolio in the subsequent year should be less risky than those that switched to a different auditor. In addition, if this tendency to reduce risk increases as legal enforcement becomes stricter, then the difference in riskiness between retained and dismissed clients should be greater in countries with strong legal enforcement than in countries with weak legal enforcement.

Focusing on firms that received a modified opinion in year *t*, I divide the clients into two groups: those that are retained and those that are switched in year $t + 1$. Table 12 summarizes the client characteristics of each group within each auditor-country-year. To ensure that I compare the characteristics of the retained and lost clients of the same auditor and not those of different auditors, I require a given auditor-country-year to have at least two clients that obtained a modified opinion, and that one of these clients is retained while the other is switched in the subsequent year.

Column (1) presents the sample characteristics of the retained clients, and column (2) presents those of the switched clients. Column (3) presents the differences in the average characteristics and the statistical significance of these differences. The weak legal enforcement subsample, reported in Section A of Table 12, includes 282 auditor-country-year observations. On average, switched clients are smaller (*Log Assets*), less profitable (*Cash Flows*, *Loss*), and riskier (*Return Volatility*) than retained clients. Similar results are obtained in the strong legal enforcement subsample, which is reported in Section B of Table 12, and includes 484 auditor-country-years. The evidence suggests that switched clients are riskier than retained clients, consistent with

the finding that auditors resign more often from riskier clients than from less risky clients under both weak and strong legal enforcement.

In Section C of Table 12, I conduct a difference-in-differences analysis to examine the difference between countries with strong and weak legal enforcement with respect to the difference in riskiness between switched and retained clients that have received modified opinions.[24] As reported in Section C of Table 12, the gap between switched and retained clients that have received modified opinions does not significantly differ between the strong and weak legal enforcement samples, except for the gap in firm size. This finding suggests that auditors are similar in the tendency to discard riskier clients and retain less risky clients, regardless of the legal enforcement level. Thus, the evidence that auditors are more sensitive to risk under strict legal enforcement is limited.

[Insert Table 12 about here]

I next examine the cross-sectional variation in the main results to determine whether the observed auditor switches are client- or auditor-initiated. First, I examine whether clients with higher auditor-switching costs are less likely to engage in opinion shopping in the strong legal enforcement subsample than in the weak subsample. I use large accruals and R&D expenditure to proxy for high switching costs because auditors would need to spend more time to understand clients with such characteristics during the initial year of the audit contract (Blouin and Grein 2007).[25] Panel A of Table 13 presents the results of testing total accruals, and Panel B presents the results of testing R&D expenditure.[26] The coefficients on the interactions

[24] Overall, clients with modified opinions are smaller, less profitable, have greater growth opportunities, and are more volatile under strong legal enforcement than under weak enforcement.

[25] However, if the results are driven by auditor-initiated dismissals, then I would expect the results to be magnified in firms with high accruals and R&D expenditure, as auditors are more likely to resign from riskier firms (Bockus and Gigler 1998).

[26] Total accruals are calculated as the income before extraordinary items less the cash flows from operations divided by total accruals. *High Total Accruals* is an indicator variable that identifies firms with above-median levels of total accruals. R&D expenditure is the research and development

between the two test variables and the cross-sectional test variables are significantly negative in the strong legal enforcement subsample. The results confirm that firms' tendency to engage in opinion shopping under strong legal enforcement decreases with increasing auditor-switching costs, consistent with auditor switches being initiated by the client rather than the auditor.[27]

Second, I examine how the audit market structure affects the main findings. Clients are less likely to switch auditors in concentrated audit market since the clients' options for replacement auditors are reduced. Moreover, if a concentrated market represents increased demand for high-quality audits (Francis, Khurana, and Pereira 2003), then the likelihood of an auditor switch in search of a more favorable opinion should decrease. However, auditors, especially Big 4 auditors are more likely to resign from risky audit contracts in a concentrated audit market since they have increased bargaining power and can more easily replace risky clients with less risky clients.

Panel C of Table 13 presents the results of an analysis in which the cross-sectional variable is the aggregate Big 4 shares, the sum of the market share of Big 4 auditors within the country-industry-year.[28] *High Big 4 Shares* is an indicator variable that identifies firms with above-median levels of aggregate Big 4 shares. Focusing on the strong legal enforcement subsample reported in columns (5) and (6), the coefficient on *Modified* (*Prob Shop*) is significantly positive, and that on *Modified* (*Prob Shop*) × *High Total Accruals* is significantly negative. The results suggest that opinion shopping under strict legal enforcement decreases as the audit market concentration

expenditures divided by total assets. *High R&D* is an indicator variable for firms with above-median levels of R&D expenditure among firms with positive R&D expenditures.

[27] I also proxy for higher switching costs with longer auditor tenure (Blouin and Grein 2007), as auditors with more knowledge about the client are more efficient than auditors with less knowledge; thus, switching a longer-tenured auditor would cause greater losses of efficiency in the audit process and increase the initial-year audit costs.

[28] I use the aggregate Big 4 shares in an industry as a proxy for audit market concentration rather than the Herfindahl–Hirschman index widely used in market structure studies because of the lack of sufficient non-Big 4 auditor information in the international database provided by Compustat Global.

increases, consistent with the findings of Newton et al. (2016). Thus, I conclude that the findings of this study are more likely to be driven by client-initiated than auditor-initiated auditor switches.[29]

Collectively, the cross-sectional test results support the argument that the observed increase in auditor switches under strict legal enforcement is more likely to be driven by client-initiated auditor dismissals than by auditor-initiated resignations.

[Insert Table 13 about here]

5.3 Mandatory auditor rotation regime

This paper assumes that clients are able to freely retain or dismiss their auditors, which would not be possible if the country regulates the length of audit contracts by imposing mandatory auditor rotation or retention requirements. I divide the sample into two based on the existence of a mandatory auditor rotation requirement, where the requirement is identified in item 7, a sub-component of the audit environment proxy of Brown et al. (2014).

Table 14 presents the results of examining the effect of a mandatory auditor rotation. I find that the main results hold in the sample with a voluntary auditor rotation regime but not in the mandatory auditor rotation regime. Specifically, the strength of legal enforcement increases opinion shopping only when the country has a voluntary auditor rotation policy. Interestingly, in countries with a mandatory auditor rotation regime, opinion shopping is observed regardless of the strength of legal enforcement. Thus, mandatory auditor rotation seems to facilitate opinion shopping rather than

[29] In untabulated tests, I directly test the effect of auditors' bargaining power. If auditor resignation is the main driver of the findings, then I would expect the results to be more pronounced when the auditor has stronger bargaining power than the client. I proxy for auditors' bargaining power using the auditor's market share within a country-industry-year and an indicator for industry specialists. However, I do not find evidence that auditor bargaining power magnifies main findings. Thus, I conclude that the observed auditor turnover is unlikely to be initiated by auditors.

suppressing it, which I believe would be an interesting avenue for future research.

[Insert Table 14 about here]

5.4 Types of modified audit opinions

I next explore whether the types of modified opinions issued by incumbent auditors differentially affect auditor turnover. In the dataset, there are four types of audit opinions: unqualified opinions with explanatory language, qualified opinions, no opinions, and adverse opinions. When the financial statements are fairly stated in compliance with the accounting standards, but there exists material information that information users should be aware of, then the auditor adds an explanatory explanation to their audit reports without qualifying the audit opinion. When the auditor is not able to perform adequate audit procedures to assess the financial statements of the clients, the auditor does not issue an audit opinion, i.e., issues a disclaimer of no opinion. When the auditor concludes that the financial statements do not comply with the accounting standards, then the auditor issues an adverse opinion. If the auditor determines that the inability to perform audit procedures or the incompliance of the financial statements is not material, then the auditor issues a qualified opinion. Different types of audit opinions impose different levels of negative information to outsiders, and thus the more severe the content of audit opinions, the more negative the consequences are (Hudaib and Cooke 2005). Considering the severity of the modifications, the effects of no opinion or an adverse opinion are expected to be greater than qualified opinions. In the sample, 18.77 percent of firms receive unqualified opinions with explanatory language, 1.78 percent receive qualified opinions, 0.34 percent receive no opinions, and 0.02 percent receive adverse opinions (untabulated).

The results of testing the hypothesis for different types of audit opinions are reported in Table 15. I first divide modified opinions into two: unqualified opinions with explanatory language and other modified opinions.

In column (1) reporting the results of the full sample, I find that auditor turnover increases after issuing both unqualified opinions with explanatory language and other modified opinions, as observed by the significantly positive coefficients on each variable's interaction with *Enforce*. The magnitude of the coefficient is greater for *Other Modified Opinions* (coefficient = 0.113, *t*-statistic = 10.07) than for *Unqualified Opinions with Explanatory Language* (coefficient = 0.014, *t*-statistic = 4.49), consistent with the severity of the opinion affecting auditor turnover to a greater extent under stronger legal enforcement. In column (2), I further decompose other modified opinions into qualified opinions, no opinions, and adverse opinions and find that the coefficient magnitudes become larger as the opinions convey more severe information.

The next columns examine the effect of different types of audit opinions for the weak and strong legal enforcement subsample. Columns (3) and (4) of Table 15 provide empirical results for the weak legal enforcement subsample. In column (3), the coefficient on *Other Modified Opinions* is significantly positive (coefficient = 0.060, *t*-statistic = 4.50) while that on *Unqualified Opinions with Explanatory Language* is insignificant. In column (4), the three types of other modified opinions are all significantly positive, and the magnitude of the coefficients increases with the severity of audit opinion. This suggests that, even in a weak legal enforcement country, clients engage in opinion shopping as the audit opinion becomes more severe. The last two columns provide the results for the strong legal enforcement subsample. In both columns (5) and (6), the coefficients on each type of opinion are significantly positive, with the magnitude of coefficients monotonically increasing with the severity of audit opinion. More importantly, the coefficient magnitudes are larger for most types of opinions in the strong legal enforcement subsample than in the weak legal enforcement subsample,

with the differences being statistically significant except for that on *No Opinion* (untabulated).

[Insert Table 15 about here]

In untabulated tests, I find that the association between legal enforcement and opinion shopping is more pronounced for initial audit modifications than for recurring ones, and for unexpected modifications than for expected ones. The results suggest that opinion shopping is more likely observed when the firm was surprised with the incumbent auditor's to issuance of a modified audit opinion.

5.5 Balancing the sample

To control for the difference in firm characteristics with modified and clean audit opinions, I attempt to balance the sample as follows. First, I match firms with modified opinions to those with clean audit opinions using a propensity-score matching procedure (Shipman, Swanquist, and Whited 2017). Specifically, I estimate the propensity of obtaining a modified opinion with a logit estimation including all control variables in the main test as suggested by Shipman et al. (2017). Then, firms with modified opinions are matched one-to-one, without replacement, to firms with clean opinions that have the closest propensity score within the country-industry-year, with a caliper distance set at 0.2 of the logit of propensity scores.[30] I successfully match 8,213 firms with modified opinions to the same number of firms with clean opinions. The difference in the logit propensity scores of the two groups reduces from 1.765 to 0.039 after the match. Columns (1) and (2) of Table 16 present the results of examining the hypothesis with the propensity-score-matched sample. The results are consistent with the main findings in Table 7.

To control for the difference in firm characteristics in weak and strong legal enforcement subsamples, I propensity-match firms in each

[30] Austin (2011) shows that setting the caliper distance at 0.2 of the logit of propensity scores is optimal in many settings. The results are robust to imposing a stricter limit of 0.1.

country using a similar procedure as above. This time, I estimate the propensity of firms belonging to a strong legal enforcement country (*Strong Enforce* = 1) and match firms in strong and weak legal enforcement countries with the closest propensity score, setting the caliper distance at 0.2 of the logit of propensity scores.[31] I successfully match, without replacement, 25,702 firms in strong legal enforcement countries to the same number of firms in weak enforcement countries. The difference in logit propensity scores of the two groups reduces from 1.285 to 0.199 after the matching procedure. Columns (3) and (4) of Table 16 present the results of examining the hypothesis with the propensity-score-matched sample. The results are consistent with the main findings in Table 7.

Alternatively, I also perform an entropy-balancing procedure to balance the sample of observations (Hainmueller 2012). I find that the untabulated results balancing either clean and modified opinions or weak and strong legal enforcement remain robust.

[Insert Table 16 about here]

5.6 Alternative specifications

Lastly, I examine the robustness of the findings to different specifications. First, I use alternative specifications for *Prob Shop* and report the results in Panel A of Table 17. Specifically, instead of estimating the difference in probabilities of expected modified opinions, I calculate the difference in the raw values from the prediction model, *Raw Shop*. The results using *Raw Shop* in column (1) are consistent with the main results. Alternatively, I define a dummy variable, *Dummy Shop*, defined as 1 for positive values of *Prob Shop* and 0 otherwise, to identify cases where the probability of receiving a clean audit opinion is higher when switching, rather than retaining, the incumbent auditor. *Dummy Shop* enables an easier comparison of the results with *Modified*, a dummy variable, as the test variable. The results using *Dummy*

[31] Again, imposing a 0.1 limit does not alter the results.

Shop are reported in column (2) and are consistent with the main results. Untabulated results using a threshold of one or five percent instead of zero also remain robust. To make sure that the values of *Prob Shop* around zero do not affect the results, I define an alternative variable *Alt. Prob Shop* by replacing *Prob Shop* values within ±1 percent with zero. The results using this alternative specification reported in column (3) are consistent with the main results. Deleting observations with *Prob Shop* values within ±1 percent from the sample does not alter the results (untabulated).

In the main analyses, I use the enforcement index provided by Brown et al. (2014) since I believe it is the most relevant proxy for enforcement in the auditing industry. Nevertheless, considering that country-level institutional characteristics cannot be clearly isolated from another (Holthausen 2009; Isidro, Nanda, and Wysocki 2020), I additionally examine the effect of alternative proxies for legal enforcement commonly used in prior studies, such as *Investor Protection* (Schwab 2017), *Anti-Self-Dealing Index* (Djankov et al. 2008), and *Common Law* (La Porta et al. 1997, 1998).[32] The results reported in Panel B of Table 17 are consistent with the main results.[33] In addition, to make sure that the repeated use of Brown et al.'s (2014) measure provided only for years 2005 and 2008 in the sample period does not inflate the findings, I restrict the sample observations to years 2005 and 2008

[32] *Investor Protection* is an annual measure that measures how the country establishes mechanisms to protect its investors. It is provided in the Global Competitiveness Report, published by the World Economic Forum, as a component of the strength of private institutions (item 1.21). It incorporates information on the disclosure, director liabilities, and shareholder liabilities (Schwab 2017). *Anti-Self-Dealing Index* measures the legal protection of minority shareholders against expropriation by the controlling shareholder (Djankov et al. 2008), and is an updated version of the anti-director index (La Porta et al. 1997). *Common Law* is an indicator variable for countries with a common-law legal origin, opposed to code-law countries (La Porta et al. 1997, 1998). The coefficients on *Enforce* are omitted in columns (3) to (6) of Panel B, Table 19 because the time-invariant proxies are subsumed by country-fixed effects.

[33] The Rule of Law index is another commonly used proxy for country-level institutions (Kaufmann, Kraay, and Mastruzzi 2009). I find that the interaction between the Rule of Law index and my test variables are significantly positive when I do not include country-fixed effects, but become insignificant when I include country-fixed effects.

and re-estimate the model. The untabulated results remain robust to this restricted sample period.

Panel C of Table 17 presents the results with alternative estimation methods. I include firm-fixed effects to control for average differences in auditor switching and audit opinions across firms. The results presented in the first two columns of Panel C are consistent with the main test results. I also estimate a logit estimation instead of the linear probability model. The results reported in the last two columns of Panel C are consistent with the main results.

Lastly, I restrict the sample to firms with financial difficulties, i.e., firms with either negative earnings or negative operating cash flows, which represents firms who are more willing to engage in opinion shopping to avoid capital market penalties of modified audit opinions. Untabulated results with the restricted sample of distressed firms do not alter the results.

[Insert Table 17 about here]

6. Conclusion

This study examines how a country's level of accounting and auditing enforcement affects the contracting behavior of audit clients. In contrast to beliefs that stricter legal enforcement benefits the capital market by imposing more restrictions on firms and auditors, I report that opinion shopping, an opportunistic behavior by audit clients, increases under strict legal enforcement. The findings are robust to various alternative model specifications. Combined with prior studies demonstrating that country-level institutions widen the gap between large and small auditors (Francis and Wang 2008; Fung et al. 2016; Kaplan and Williams 2012), this study shows that such a gap may incentivize firms to opportunistically switch auditors in search of more favorable audit opinions. In addition, the results imply that clients penalize auditors for providing higher-quality audits by replacing the auditor, which places an additional contractual burden on auditors operating

under strict legal institutions. Thus, this study calls for attention to the negative externalities of strict legal enforcement. The effects of a country's institutions are multi-dimensional and deserve thorough investigation. I hope to contribute to discussions on the costs and benefits of legal enforcement across countries.

This study has some limitations that deserve further examination. For example, it focuses on the behavior of Big 4 auditors' clients. Although this approach enables me to control for the effect of audit clientele, the limited sample restricts the generalizability of the results. In the future, available data on historical non-Big 4 auditors' identities in international datasets will help to expand the findings of the study. Moreover, as in any international study, the results may be contaminated by unobserved country-level differences that may not be successfully eliminated by controlling for country-fixed effects. Future studies could identify sufficient variation in the enforcement levels within a country and thus help to confirm this study's findings in a more nuanced manner.

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Appendix. Variable Definitions

Variables	Definitions
Test variables	
Switch	An indicator variable that equals 1 if the firm switches its auditor in year $t + 1$ and 0 otherwise;
Modified	An indicator variable that equals 1 if the audit opinion is modified and 0 otherwise;
Prob Shop	The probability of opinion-shopping purpose auditor switches, calculated as the probability of receiving a modified opinion without an auditor switch less the probability of receiving a modified opinion after an auditor switch, where the probability is estimated using equation (2);
Enforce	An index for the quality of auditing environment and accounting enforcement of a country by Brown et al. (2014), scaled to range between 0 and 1;
Strong Enforce	An indicator variable that equals 1 if *Enforce* is above the median value at the country-year level;
Control variables	
Log Assets	The natural logarithm of total assets in millions of U.S. Dollars;
Leverage	Long-term debt and debt in current liabilities divided by total assets;
Cash Flows	Cash flows from operations divided by total assets;
Loss	An indicator variable that equals 1 if income before extraordinary items is negative and 0 otherwise;
Market-to-Book	Market value of equity divided by the book value equity, where market value of equity is calculated as stock price multiplied by the number of common shares outstanding at the fiscal-year-end;
Future Financing	An indicator variable that equals 1 if the change in capital in the current year is greater than one percent of beginning-of-year total assets, where the change in capital is the sum of common stock, preferred stock, long-term debt, and debt in current liabilities;
Returns	Cumulative abnormal return for the fiscal year, where abnormal return is the daily stock return less the value-weighted return of the stock exchange;
Return Volatility	The standard deviation of daily abnormal returns within the fiscal year;
Log Auditor Tenure	The natural logarithm of the number of consecutive years audited by the current auditor;
Auditor's Shares	The auditor's market share within the country-industry-year, where market share is calculated based on clients' sales

(continued on the next page)

Additional variables (in order of appearance)	
Big 4 Shares	The percentage of clients audited by Big 4 auditors in a country-industry-year;
Next Modified	An indicator variable that equals 1 if the audit opinion is modified in year $t + 1$ and 0 otherwise;
Cumulative Abnormal Returns	The cumulative abnormal return for the 12 months starting three months after the beginning of the fiscal year, where abnormal returns are the daily stock returns less the value-weighted returns of the stock exchange;
ΔEarnings	The change in income before extraordinary items from year $t - 1$ to t divided by beginning-of-year market capitalization;
Log Market Capitalization	The natural logarithm of the beginning-of-year market capitalization;
Abs. Disc. Accruals	The absolute value of performance-matched discretionary accruals, where discretionary accruals are estimated as the residuals from the modified Jones' (1991) model, and performance-matching is performed by subtracting the median discretionary accruals within the return-on-assets-decile in the industry-year from the firm's discretionary accruals;
Strong Signaling	An indicator of above-median levels of *Signaling*, where *Signaling* is the coefficient on *Modified* in equation (6), estimated within each country-year, multiplied by (–1);
Large Quality Gap	An indicator of above-median levels of *Quality Gap*, where *Quality Gap* is the coefficient on *Big 4* in equation (7), estimated within each country-year multiplied by (–1);
Switch to Big 4 (non-Big 4)	An indicator variable that equals 1 if the firm switches its auditor to a Big 4 (non-Big 4) auditor in year $t + 1$ and 0 otherwise;
Opinion Shopping	An indicator variable that equals 1 for observations with *Prob Shop* > 0 and *Switch* = 1, and 0 otherwise;
Opinion Shopping to Big 4 (non-Big 4)	An indicator variable that equals 1 for observations with *Prob Shop* > 0 and *Switch to Big 4 (non-Big 4)* = 1, and 0 otherwise;
High Total Accruals	An indicator variable that equals 1 if total accruals are above median and 0 otherwise, where total accruals are income before extraordinary items less cash flows from operations divided by total assets;
High R&D	An indicator variable that equals 1 if a firm's R&D intensity is above median and 0 otherwise, where R&D intensity is calculated as research and development expenditure divided by total assets;
High Big 4 Shares	An indicator variable that equals 1 if *Big 4 Shares* is above the median at the country-year and 0 otherwise;
Unqualified Opinion with Explanatory Language	An indicator variable that equals 1 if the firm received an unqualified opinion with explanatory language and 0 otherwise;
Other Modified Opinions	An indicator variable that equals 1 if the firm received either a qualified, no, or adverse opinion and 0 otherwise;

(continued on the next page)

Qualified Opinion	An indicator variable that equals 1 if the firm received a qualified opinion and 0 otherwise;
No Opinion	An indicator variable that equals 1 if the firm received a no opinion and 0 otherwise;
Adverse Opinion	An indicator variable that equals 1 if the firm received an adverse opinion and 0 otherwise;
Raw Shop	The predicted value of equation (2) assuming an auditor retention less the predicted value of equation (2) assuming an auditor switch; and
Dummy Shop	An indicator variable that equals 1 if *Prob Shop* is positive and 0 otherwise.
Alt. Prob Shop	A variable that equals 0 if *Prob Shop* is within the range of (– 0.01, 0.01), and equals *Prob Shop* otherwise;
Investor Protection	An annual ranked index from the Global Competitiveness Report published by the World Economic Forum that measures how the country establishes mechanisms to protect its investors incorporating information on the disclosure, director liabilities, and shareholder liabilities (Schwab 2017);
Anti-Self-Dealing Index	A score that measures the legal protection of minority shareholders against expropriation by the controlling shareholder (Djankov et al. 2008);
Common Law	An indicator variable that equals 1 for countries with a common-law legal origin and 0 for countries with a code-law legal origin.

Table 1. Sample Characteristics

This table presents the frequency and average of the main variables by country and year in Panels A and B, respectively. In panel A, countries are classified as having weak (strong) legal enforcement if the average value of *Enforce* is below (above) the median. In both panels, figures in bold indicate the minimum or maximum values. Detailed variable definitions are provided in the Appendix.

Panel A. Sample characteristics by country

	(1)	(2)	(3)	(4)	(5)	(6)
Country	Freq.	*Switch*	*Modified*	*Enforce*	Sub-sample	*Big 4 Shares*
(1) Argentina	444	0.083	0.651	0.120	Weak	0.866
(2) Australia	6,595	0.073	0.163	0.923	Strong	0.885
(3) Austria	546	0.103	0.099	0.439	Weak	0.896
(4) Belgium	751	0.088	0.285	0.756	Strong	0.911
(5) Brazil	1,865	0.224	0.388	0.371	Weak	0.873
(6) Canada	3,499	**0.035**	0.108	**0.962**	Strong	0.977
(7) Chile	1,344	0.149	0.362	0.096	Weak	0.932
(8) China	4,362	0.115	**0.049**	0.635	Strong	0.408
(9) Croatia	325	0.172	0.345	0.328	Weak	0.777
(10)Czech Republic	72	0.111	0.097	0.275	Weak	0.981
(11)Denmark	1,169	0.094	0.069	0.865	Strong	0.969
(12)Egypt	202	0.124	0.337	0.180	Weak	0.659
(13)Finland	1,483	0.091	0.055	0.538	Strong	0.976
(14)France	2,808	0.154	**0.838**	0.798	Strong	0.666
(15)Germany	3,628	0.101	0.072	0.762	Strong	0.848
(16)Greece	705	0.130	0.275	0.423	Weak	0.685
(17)Hong Kong	7,529	0.085	0.058	0.850	Strong	0.864
(18)Hungary	161	0.093	0.155	0.265	Weak	**0.982**
(19)India	2,533	0.129	0.640	0.319	Weak	**0.292**
(20)Indonesia	171	0.556	0.363	0.192	Weak	0.423
(21)Ireland	667	0.047	0.304	0.666	Strong	0.978
(22)Israel	218	0.404	0.413	0.823	Strong	0.318
(23)Italy	1,405	0.265	0.201	0.797	Strong	0.701
(24)Jordan	272	0.096	0.125	0.134	Weak	0.780
(25)Malaysia	5,463	0.086	0.512	0.587	Strong	0.712
(26)Mexico	461	0.152	0.247	0.415	Weak	0.619
(27)Morocco	177	0.288	0.249	0.129	Weak	0.738
(28)Netherlands	1,283	0.101	0.125	0.649	Strong	0.956
(29)New Zealand	1,116	0.061	0.117	0.737	Strong	0.910
(30)Norway	1,801	0.096	0.094	0.827	Strong	0.947
(31)Pakistan	855	0.119	0.451	0.269	Weak	0.450
(32)Peru	47	0.617	0.170	0.191	Weak	0.438
(33)Philippines	**39**	**0.667**	0.333	0.422	Weak	0.311
(34)Poland	1,202	0.228	0.265	0.445	Weak	0.640
(35)Portugal	433	0.118	0.321	0.471	Strong	0.915
(36)Romania	210	0.238	0.405	0.209	Weak	0.733
(37)Russia	398	0.176	0.244	0.462	Weak	0.482
(38)Singapore	4,429	0.082	0.107	0.538	Strong	0.840
(39)Slovenia	201	0.169	0.164	0.288	Weak	0.969
(40)South Africa	1,831	0.069	0.055	0.481	Strong	0.871
(41)Spain	1,215	0.108	0.320	0.713	Strong	0.928
(42)Sweden	3,374	0.118	0.068	0.564	Strong	0.888
(43)Switzerland	2,252	0.064	0.069	0.855	Strong	0.973
(44)Taiwan	**12,170**	0.112	0.166	0.269	Weak	0.926
(45)Thailand	3,313	0.073	0.359	0.375	Weak	0.773
(46)Turkey	330	0.342	0.279	0.308	Weak	0.509
(47)Ukraine	**39**	0.205	0.615	**0.039**	Weak	0.685
(48)United Kingdom	8,400	0.092	0.091	**0.962**	Strong	0.906
Total	93,793	0.106	0.206	0.626		0.821

Panel B. Sample characteristics by year

Year	(1) Frequency	(2) *Switch*	(3) *Modified*	(4) *Enforce*	(5) *Big 4 Shares*
2004	5,504	**0.187**	**0.110**	0.590	**0.842**
2005	5,575	0.162	0.159	0.576	0.825
2006	5,752	0.154	0.201	**0.575**	0.819
2007	5,807	0.107	0.247	0.655	0.789
2008	5,898	0.129	**0.268**	0.650	**0.787**
2009	**5,272**	0.099	0.253	**0.688**	0.791
2010	5,325	0.079	0.213	0.685	0.806
2011	5,534	0.101	0.225	0.678	0.809
2012	5,791	0.077	0.199	0.680	0.814
2013	6,161	0.076	0.239	0.658	0.822
2014	7,189	**0.068**	0.214	0.609	0.836
2015	7,392	0.073	0.202	0.603	0.836
2016	7,552	0.099	0.225	0.598	0.837
2017	**7,627**	0.097	0.224	0.596	0.838
2018	7,414	0.114	0.113	0.597	0.834
	93,793	0.106	0.206	0.626	0.821

Table 2. Descriptive Statistics

This table reports the summary statistics of the variables used in the main tests of the study (n = 93,793). Detailed variable definitions are provided in the Appendix.

Variables	(1) Mean	(2) Std. Dev.	(3) P1	(4) P25	(5) Median	(6) P75	(7) P99
Switch	0.106	0.308	0	0	0	0	1
Modified	0.206	0.404	0	0	0	0	1
Enforce	0.626	0.264	0.096	0.365	0.635	0.923	0.962
Log Assets	5.784	1.981	1.630	4.347	5.607	7.109	10.870
Leverage	0.211	0.180	0.000	0.048	0.189	0.326	0.761
Cash Flows	0.059	0.125	-0.535	0.017	0.069	0.122	0.352
Loss	0.242	0.428	0	0	0	0	1
Market-to-Book	2.231	2.716	-1.382	0.805	1.430	2.597	18.200
Future Financing	0.473	0.499	0	0	0	1	1
Returns	0.052	0.443	-1.084	-0.197	0.023	0.263	1.645
Return Volatility	0.027	0.016	0.008	0.016	0.022	0.032	0.098
Auditor Tenure (Raw)	6.870	5.256	1	3	6	9	24
Log Auditor Tenure	1.614	0.839	0	1.099	1.792	2.197	3.178
Auditor's Shares	0.340	0.286	0.002	0.108	0.249	0.510	1.000

Table 3. Correlations

This table reports Pearson correlations between variables. Detailed variable definitions are provided in the Appendix. Figures in bold represent statistical significance at the two-tailed, five percent level.

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
(1) *Switch*	1.000											
(2) *Modified*	**0.053**	1.000										
(3) *Enforce*	**-0.050**	**-0.110**	1.000									
(4) *Log Assets*	**-0.040**	**-0.010**	**0.128**	1.000								
(5) *Leverage*	**0.028**	**0.129**	**-0.040**	**0.276**	1.000							
(6) *Cash Flows*	**-0.040**	**-0.100**	**-0.070**	**0.271**	**-0.060**	1.000						
(7) *Loss*	**0.061**	**0.104**	**0.092**	**-0.260**	**0.098**	**-0.450**	1.000					
(8) *Market-to-Book*	**0.000**	**-0.010**	**0.065**	**-0.050**	**-0.060**	**0.033**	**-0.010**	1.000				
(9) *Future Financing*	0.004	**0.000**	**-0.020**	**-0.020**	**0.014**	**-0.060**	**-0.020**	**0.049**	1.000			
(10) *Returns*	**-0.010**	**-0.010**	**0.009**	**-0.060**	**-0.070**	**0.088**	**-0.140**	**0.192**	**0.050**	1.000		
(11) *Return Volatility*	**0.059**	**0.102**	**0.125**	**-0.430**	**0.024**	**-0.320**	**0.398**	**-0.030**	**0.019**	**0.262**	1.000	
(12) *Log Auditor Tenure*	**-0.080**	**-0.060**	**0.153**	**0.146**	**-0.020**	**0.057**	**-0.050**	**-0.020**	**-0.030**	0.000	**-0.110**	1.000
(13) *Auditor's Shares*	**-0.010**	**-0.020**	**-0.050**	**0.270**	**0.080**	**0.094**	**-0.070**	**0.006**	**-0.010**	**-0.010**	**-0.150**	**0.061**

Table 4. Predicting the Issuance of Modified Opinions

This table presents the results of a probit estimation of equation (2), estimating the probability of audit opinion modification in year $t + 1$. Detailed variable definitions are provided in the Appendix. Figures in parentheses represent χ^2 statistics, and the pseudo R-squared is calculated via McFadden's approach. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent level, respectively.

Dependent variable =	*Next Modified*
Modified	1.648***
	(13,344.90)
Enforce	-0.245***
	(7.86)
Log Assets	0.034***
	(61.08)
Leverage	0.399***
	(114.84)
Cash Flows	-0.763***
	(180.46)
Loss	0.156***
	(79.86)
Market-to-Book	0.000
	(0.00)
Future Financing	0.108***
	(69.11)
Returns	-0.073***
	(22.95)
Return Volatility	6.830***
	(166.90)
Switch	0.842***
	(123.92)
Switch × *Modified*	-0.864***
	(607.91)
Switch × *Enforce*	0.476***
	(62.07)
Switch × *Log Assets*	-0.024***
	(6.32)
Switch × *Leverage*	-0.131
	(2.29)
Switch × *Cash Flows*	0.353***
	(7.47)
Switch × *Loss*	-0.044
	(1.18)
Switch × *Market-to-Book*	0.003
	(0.24)
Switch × *Future Financing*	-0.020***
	(0.41)

(continued on the next page)

Switch × *Returns*	-0.042
	(1.45)
Switch × *Return Volatility*	-2.858***
	(6.37)
Country fixed effects	Included
Industry fixed effects	Included
Year fixed effects	Included
Observations	93,793
Pseudo *R*-squared	0.383

Table 5. Univariate Analysis

This table presents the results of the univariate analyses. Panel A [B] presents the frequency of observations and mean of subsequent-year auditor switches, *Switch*, depending on the type of audit opinions [probability of opinion shopping] in rows and strength of legal enforcement in columns. In both panels, country-years are classified as having weak (strong) legal enforcement if *Enforce* is below (above) the median, and the shaded column presents the difference in *Switch* between firms under weak and strong legal enforcement. The shaded rows in Panel A [B] present the difference in *Switch* between firms with a clean and a modified audit opinion [negative and positive values of *Prob Shop*]. Detailed variable definitions are provided in the Appendix. Figures in parentheses represent *z*-statistics of the differences. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent level, respectively.

Panel A. Realized audit opinions and auditor switches

	Full sample		Weak legal enforcement		Strong legal enforcement		Strong–Weak	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Audit Opinion	Freq.	*Switch*	Freq.	*Switch*	Freq.	*Switch*	Diff.	(*z*-stat)
Clean Opinion	74,513	0.098	21,328	0.131	53,185	0.084	-0.047***	(-17.90)
Modified Opinion	19,280	0.139	8,433	0.136	10,847	0.141	0.005	(1.00)
Total	93,793	0.106	29,761	0.133	64,032	0.094	-0.039***	(-17.16)
Diff		0.041***		0.005		0.057***	0.052***	
(z-statistics)		(15.19)		(1.14)		(15.93)	(9.14)	

Panel B. Expected audit opinions and auditor switches

	Full sample		Weak legal enforcement		Strong legal enforcement		Strong–Weak	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Opinion Shopping	Freq.	*Switch*	Freq.	*Switch*	Freq.	*Switch*	Diff.	(*z*-stat)
Prob Shop ≤ 0	83,537	0.101	22,125	0.131	61,412	0.091	-0.040***	(-15.84)
Prob Shop > 0	10,256	0.147	7,636	0.138	2,620	0.171	0.033***	(3.95)
Total	93,793	0.106	29,761	0.133	64,032	0.094	-0.039***	(-17.16)
Diff		0.046***		0.007		0.080***	0.073***	
(z-statistics)		(12.61)		(1.54)		(10.77)	(8.39)	

Table 6. Evidence of Opinion Shopping in the International Sample

This table presents the results of examining the existence of audit clients' opinion shopping behavior in the international setting. Detailed variable definitions are provided in the Appendix. Figures in parentheses represent *t*-statistics calculated based on standard errors clustered by firm. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent level, respectively.

Dependent variable =	*Switch*			
	(1)	(2)	(3)	(4)
Modified	0.029***		0.021***	
	(10.46)		(6.90)	
Prob Shop		0.093***		0.077***
		(7.86)		(6.30)
Enforce	-0.056***	-0.052***	0.156***	0.165***
	(-12.79)	(-11.19)	(9.29)	(9.84)
Log Assets	-0.000	-0.000	-0.005***	-0.005***
	(-0.20)	(-0.73)	(-6.94)	(-7.06)
Leverage	0.021***	0.025***	0.022***	0.023***
	(3.30)	(3.86)	(3.39)	(3.54)
Cash Flows	-0.030***	-0.031***	-0.032***	-0.033***
	(-2.90)	(-2.97)	(-3.05)	(-3.14)
Loss	0.030***	0.031***	0.028***	0.029***
	(9.69)	(10.14)	(9.24)	(9.54)
Market-to-Book	-0.000	-0.000	-0.000	-0.000
	(-0.38)	(-0.08)	(-1.08)	(-0.88)
Future Financing	-0.008***	-0.007***	-0.005**	-0.004**
	(-3.77)	(-3.38)	(-2.45)	(-2.07)
Returns	-0.007***	-0.009***	-0.009***	-0.011***
	(-2.61)	(-3.41)	(-3.35)	(-4.05)
Return Volatility	0.804***	0.799***	0.820***	0.829***
	(8.45)	(8.38)	(8.28)	(8.38)
Log Auditor Tenure	-0.018***	-0.018***	-0.001	-0.001
	(-13.24)	(-13.44)	(-0.84)	(-0.90)
Auditor's Shares	-0.012***	-0.012***	-0.004	-0.004
	(-3.05)	(-3.07)	(-1.03)	(-1.03)
Country fixed effects	Not included	Not included	Included	Included
Industry fixed effects	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included
Observations	93,793	93,793	93,793	93,793
Adjusted *R*-squared	0.027	0.027	0.051	0.051

Table 7. Legal Enforcement and Opinion Shopping

This table presents the results of examining the effect of legal enforcement on clients' opinion shopping behavior. Panel A provides the results of a pooled regression. Panel B provides the results of a subsample regression, where country-years are classified as having weak (strong) legal enforcement if *Enforce* is below (above) the median. Detailed variable definitions are provided in the Appendix. Figures in parentheses represent *t*-statistics calculated based on standard errors clustered by firm. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent level, respectively.

Panel A. Pooled analysis

Dependent variable =	*Switch*			
	(1)	(2)	(3)	(4)
Modified	-0.008		-0.014**	
	(-1.26)		(-2.06)	
Modified × Enforce	0.064***		0.063***	
	(6.18)		(5.64)	
Prob Shop		0.028		-0.011
		(1.10)		(-0.43)
Prob Shop × Enforce		0.114***		0.159***
		(2.81)		(3.57)
Enforce	-0.069***	-0.033***	0.144***	0.194***
	(-14.67)	(-4.06)	(8.51)	(10.28)
Log Assets	-0.000	-0.001	-0.005***	-0.005***
	(-0.03)	(-0.74)	(-6.83)	(-6.99)
Leverage	0.021***	0.025***	0.022***	0.023***
	(3.28)	(3.94)	(3.35)	(3.60)
Cash Flows	-0.027**	-0.030***	-0.029***	-0.032***
	(-2.57)	(-2.89)	(-2.78)	(-3.04)
Loss	0.029***	0.031***	0.028***	0.029***
	(9.67)	(10.25)	(9.24)	(9.66)
Market-to-Book	-0.000	-0.000	-0.001	-0.000
	(-0.39)	(-0.10)	(-1.29)	(-0.98)
Future Financing	-0.008***	-0.007***	-0.005**	-0.004*
	(-3.82)	(-3.29)	(-2.44)	(-1.95)
Returns	-0.007**	-0.009***	-0.008***	-0.011***
	(-2.45)	(-3.39)	(-3.13)	(-4.06)
Return Volatility	0.786***	0.783***	0.783***	0.812***
	(8.23)	(8.20)	(7.89)	(8.21)
Log Auditor Tenure	-0.018***	-0.018***	-0.001	-0.001
	(-13.19)	(-13.42)	(-0.82)	(-0.89)
Auditor's Shares	-0.012***	-0.012***	-0.005	-0.004
	(-3.12)	(-3.07)	(-1.14)	(-1.11)
Country fixed effects	Not included	Not included	Included	Included
Industry fixed effects	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included
Observations	93,793	93,793	93,793	93,793
Adjusted *R*-squared	0.028	0.027	0.051	0.051

Panel B. Subsample analysis

Dependent variable =	*Switch*			
	(1)	(2)	(3)	(4)
Legal enforcement =	Weak	Strong	Weak	Strong
Modified	0.002	0.039***		
	(0.36)	(8.68)		
Prob Shop			0.024	0.118***
			(1.29)	(6.72)
Log Assets	-0.001	-0.008***	-0.001	-0.008***
	(-0.84)	(-8.83)	(-0.98)	(-8.87)
Leverage	-0.015	0.035***	-0.015	0.039***
	(-1.22)	(4.57)	(-1.28)	(5.07)
Cash Flows	-0.026	-0.036***	-0.025	-0.039***
	(-1.18)	(-2.99)	(-1.12)	(-3.27)
Loss	0.041***	0.021***	0.041***	0.023***
	(7.21)	(5.91)	(7.20)	(6.52)
Market-to-Book	-0.001	-0.000	-0.001	-0.000
	(-1.51)	(-0.26)	(-1.44)	(-0.01)
Future Financing	-0.018***	0.001	-0.018***	0.003
	(-4.37)	(0.49)	(-4.36)	(1.12)
Returns	0.001	-0.013***	0.001	-0.016***
	(0.20)	(-4.24)	(0.12)	(-5.25)
Return Volatility	0.801***	0.760***	0.781***	0.801***
	(3.55)	(6.78)	(3.46)	(7.16)
Log Auditor Tenure	-0.007**	0.004***	-0.007***	0.004**
	(-2.57)	(2.59)	(-2.58)	(2.51)
Auditor's Shares	-0.022***	0.002	-0.022***	0.002
	(-2.68)	(0.37)	(-2.66)	(0.40)
Test statistics of [Strong – Weak]				
Modified (Prob Shop)	0.037***		0.094***	
(*t*-statistics)	(5.90)		(3.73)	
Country fixed effects	Included	Included	Included	Included
Industry fixed effects	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included
Observations	29,760	64,032	29,760	64,032
Adjusted *R*-squared	0.080	0.039	0.080	0.039

Table 8. Country-Level Analysis

This table presents the results of a country-level analysis examining the effect of legal enforcement on clients' opinion shopping behavior. Panel A presents the country-level estimates of the coefficient on *Modified* and *Prob Shop* in equations (1) and (3) within each country. The data is sorted in descending order of the *t*-statistics of the coefficients on *Modified* reported in column (2). Panel B provides the results of a univariate analysis on the average coefficients and *t*-statistics. Country-years are classified as having a weak (strong) legal enforcement if *Enforce* is below (above) the median. The shaded rows present the difference in the coefficient estimates between firms in weak and strong legal enforcement countries. Detailed variable definitions are provided in the Appendix. Figures in parentheses represent *t*-statistics and *z*-statistics in Panels A and B, respectively. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent level, respectively.

Panel A. Descriptive statistics

	Coefficient on *Modified*		Coefficient on *Prob Shop*		Legal Enforcement	
	(1)	(2)	(3)	(4)	(5)	(6)
Country	Estimate	*t*-stat.	Estimate	*t*-stat.	*Enforce*	Subsample
(1) China	0.138	(4.55)	0.897	(4.25)	0.635	Strong
(2) Singapore	0.081	(3.99)	0.341	(3.97)	0.538	Strong
(3) Australia	0.044	(3.57)	0.182	(3.60)	0.923	Strong
(4) Malaysia	0.031	(3.41)	0.104	(3.52)	0.587	Strong
(5) Hong Kong	0.065	(3.36)	0.448	(3.65)	0.850	Strong
(6) United Kingdom	0.046	(3.30)	0.197	(2.79)	0.962	Strong
(7) Italy	0.095	(2.75)	0.360	(2.74)	0.797	Strong
(8) Netherlands	0.083	(2.74)	0.340	(2.49)	0.649	Strong
(9) Croatia	0.172	(2.67)	0.714	(2.91)	0.328	Weak
(10) Germany	0.064	(2.52)	0.486	(3.16)	0.762	Strong
(11) South Africa	0.102	(2.36)	0.499	(2.32)	0.481	Strong
(12) Brazil	0.044	(2.15)	0.176	(2.19)	0.371	Weak
(13) Israel	0.163	(2.08)	0.528	(1.86)	0.823	Strong
(14) Poland	0.074	(2.05)	0.329	(2.04)	0.445	Weak
(15) Sweden	0.054	(1.99)	0.225	(1.53)	0.564	Strong
(16) Jordan	0.167	(1.91)	0.694	(1.78)	0.134	Weak
(17) Norway	0.065	(1.86)	0.226	(1.49)	0.827	Strong
(18) Canada	0.022	(1.73)	0.126	(2.05)	0.962	Strong
(19) Chile	0.037	(1.59)	0.149	(1.65)	0.096	Weak
(20) Ireland	0.035	(1.51)	0.150	(1.68)	0.666	Strong
(21) Romania	0.127	(1.34)	0.528	(1.47)	0.209	Weak
(22) Mexico	0.048	(1.15)	0.196	(1.18)	0.415	Weak
(23) Taiwan	0.006	(1.00)	0.045	(1.32)	0.269	Weak
(24) Pakistan	0.023	(0.94)	0.073	(0.83)	0.269	Weak
(25) Philippines	0.387	(0.79)	0.502	(0.35)	0.422	Weak
(26) Switzerland	0.021	(0.76)	0.084	(0.45)	0.855	Strong
(27) Austria	0.046	(0.74)	0.187	(0.54)	0.439	Weak
(28) Turkey	0.055	(0.72)	0.152	(0.52)	0.308	Weak
(29) Finland	0.015	(0.48)	0.120	(0.73)	0.538	Strong
(30) Egypt	0.013	(0.24)	0.029	(0.14)	0.180	Weak
(31) Thailand	0.002	(0.18)	0.007	(0.16)	0.375	Weak
(32) New Zealand	0.000	(0.00)	-0.059	(-0.42)	0.737	Strong
(33) Czech Republic	-0.040	(-0.24)	0.002	(0.00)	0.275	Weak
(34) France	-0.005	(-0.24)	-0.023	(-0.38)	0.798	Strong
(35) Hungary	-0.027	(-0.29)	-0.122	(-0.31)	0.265	Weak

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(36) Slovenia	-0.039	(-0.45)	-0.068	(-0.14)	0.288	Weak
(37) Greece	-0.026	(-0.61)	-0.096	(-0.57)	0.423	Weak
(38) India	-0.011	(-0.73)	-0.033	(-0.67)	0.319	Weak
(39) Portugal	-0.034	(-0.76)	-0.107	(-0.60)	0.471	Strong
(40) Belgium	-0.023	(-0.76)	-0.088	(-0.75)	0.756	Strong
(41) Spain	-0.020	(-0.78)	-0.072	(-0.71)	0.713	Strong
(42) Morocco	-0.118	(-0.88)	-0.398	(-0.82)	0.129	Weak
(43) Russia	-0.043	(-0.91)	-0.172	(-0.87)	0.462	Weak
(44) Indonesia	-0.120	(-1.21)	-0.496	(-1.37)	0.192	Weak
(45) Argentina	-0.041	(-1.43)	-0.131	(-1.35)	0.120	Weak
(46) Denmark	-0.048	(-1.44)	-0.205	(-1.10)	0.865	Strong
(47) Ukraine	-0.649	(-2.59)	-2.241	(-2.56)	0.038	Weak
(48) Peru	-0.668	(-4.99)	-3.081	(-5.29)	0.191	Weak
Average	**0.009**	**(0.88)**	**0.035**	**(0.86)**	**0.494**	

Panel B. Legal enforcement and opinion shopping

		Coefficient on *Modified*		Coefficient on *Prob Shop*	
Legal enforcement	Freq.	Estimate	*t*-statistic	Estimate	*t*-statistic
Weak	25	-0.023	0.13	-0.122	0.13
Strong	23	0.043*	1.69	0.207*	1.67
Total	48	0.009	0.88	0.036	0.86
Difference		0.067	1.57***	0.329*	1.54***
(*z*-statistics)		(1.48)	(3.20)	(1.89)	(3.12)

Table 9. The Channels

This table examines the channels through which legal enforcement affects opinion shopping. Panel A presents the results of testing how legal enforcement affects the signaling effect of modified opinions. The dependent variable is *Cumulative Abnormal Returns*. Panel B presents the results of testing how legal enforcement affects the gap between the quality of Big 4 and non-Big 4 auditors. The dependent variable is *Abs. Disc. Accruals*. Panel C presents the results of testing whether the signaling effect of audit opinions and the quality gap between Big 4 and non-Big 4 auditors moderate the relationship between legal enforcement and opinion shopping. *Strong Signaling* is an indicator for above-median values of *Signaling*, where *Signaling* is the coefficient on *Modified* in equation (6) estimated within each country-year multiplied by (–1). *Large Quality Gap* is an indicator for above-median values of *Quality Gap*, where *Quality Gap* is the coefficient on *Big 4* in equation (7) estimated within each country-year multiplied by (–1). Country-years are classified as having weak (strong) legal enforcement if *Enforce* is below (above) the median. Detailed variable definitions are provided in the Appendix. Figures in parentheses represent *t*-statistics calculated based on standard errors clustered by firm. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent level, respectively.

Panel A. Stock returns and modified opinions

Dependent variable =	*Cumulative Abnormal Returns*			
	(1)	(2)	(3)	(4)
Legal enforcement =	All	Weak	Strong	Diff [= (3) – (2)]
ΔEarnings	0.416***	0.455***	0.393***	-0.061**
	(34.92)	(19.91)	(28.49)	(-2.30)
Modified	-0.034***	-0.005	-0.066***	-0.061***
	(-7.64)	(-0.87)	(-9.73)	(-6.72)
Log Market Capitalization	-0.022***	-0.035***	-0.019***	0.016***
	(-22.16)	(-18.20)	(-15.90)	(6.99)
Leverage	-0.151***	-0.182***	-0.137***	0.045**
	(-15.04)	(-11.88)	(-10.22)	(2.24)
Market-to-Book	0.025***	0.028***	0.024***	-0.004**
	(28.48)	(16.52)	(23.57)	(-2.04)
Country fixed effects	Included	Included	Included	
Industry fixed effects	Included	Included	Included	
Year fixed effects	Included	Included	Included	
Observations	75,638	26,637	49,000	
Adjusted *R*-squared	0.108	0.123	0.107	

Panel B. Quality gap between Big 4 and non-Big 4 auditors

Dependent variable =	*Abs. Disc. Accruals*			
	(1)	(2)	(3)	(4)
Legal enforcement =	All	Weak	Strong	Diff [= (3) – (2)]
Big 4	-0.003***	0.001	-0.005***	-0.006***
	(-4.56)	(0.60)	(-6.14)	(-4.34)
Log Assets	-0.008***	-0.010***	-0.006***	0.003***
	(-39.72)	(-28.79)	(-28.07)	(8.33)
Leverage	0.024***	0.031***	0.021***	-0.010***
	(13.69)	(11.02)	(9.39)	(-2.80)
Cash Flows	-0.180***	-0.184***	-0.179***	0.006
	(-53.25)	(-29.94)	(-43.62)	(0.80)
Loss	0.005***	0.002*	0.007***	0.005***
	(7.75)	(1.90)	(8.49)	(3.93)
Market-to-Book	0.004***	0.004***	0.004***	-0.000*
	(33.74)	(19.72)	(27.48)	(-1.65)
Future Financing	0.001***	0.002***	0.001	-0.002**
	(3.50)	(3.89)	(1.59)	(-2.21)
Returns	-0.002***	-0.002*	-0.002**	-0.000
	(-3.08)	(-1.80)	(-2.54)	(-0.29)
Return Volatility	0.853***	0.704***	0.924***	0.220***
	(35.60)	(16.38)	(32.16)	(4.27)
Country fixed effects	Included	Included	Included	
Industry fixed effects	Included	Included	Included	
Year fixed effects	Included	Included	Included	
Observations	287,900	99,937	187,963	
Adjusted *R*-squared	0.259	0.174	0.300	

Panel C. The moderating effect of audit signals and gap in auditors' quality on opinion shopping

Dependent variable =	*Switch*					
Legal enforcement =	All		Weak		Strong	
	(1)	(2)	(3)	(4)	(5)	(6)
Modified	0.014***		0.014*		0.016**	
	(2.69)		(1.76)		(2.16)	
Modified × Strong Signaling	0.026***		0.003		0.029***	
	(4.52)		(0.30)		(3.84)	
Modified × Large Quality Gap	-0.002		-0.021**		0.012	
	(-0.43)		(-2.33)		(1.58)	
Prob Shop		0.009		0.011		0.014
		(0.47)		(0.37)		(0.51)
Prob Shop × Strong Signaling		0.145***		0.153***		0.128***
		(6.33)		(3.74)		(4.31)
Prob Shop × Large Quality Gap		0.020		-0.049		0.076**
		(0.93)		(-1.40)		(2.55)
Strong Signaling	-0.003	0.025***	0.001	0.021***	-0.006**	0.021***
	(-1.15)	(5.41)	(0.10)	(2.62)	(-2.14)	(3.53)
Large Quality Gap	-0.015***	-0.011**	0.014**	0.001	-0.017***	0.000
	(-5.87)	(-2.44)	(2.17)	(0.13)	(-5.88)	(0.05)

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Test statistics						
Modified (Prob Shop)						
× *[Strong Signaling – Large Quality Gap]*	0.016*	0.002	0.045***	0.080	-0.010	-0.045
(*F*-statistic)	(3.50)	(0.01)	(12.50)	(2.54)	(0.58)	(0.81)
Control variables	Included	Included	Included	Included	Included	Included
Country fixed effects	Included	Included	Included	Included	Included	Included
Industry fixed effects	Included	Included	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included	Included	Included
Observations	93,479	93,479	29,450	29,450	64,028	64,028
Adjusted *R*-squared	0.048	0.048	0.075	0.075	0.040	0.040

Table 10. Direction of Auditor Switches

This table presents the results of examining the effect of legal enforcement on clients' opinion shopping-induced auditor switches to different types of auditors. In columns (1) to (4) [(5) to (8)], the dependent variable is *Switch to Big 4* [*Switch to Non-Big 4*]. Country-years are classified as having weak (strong) legal enforcement if *Enforce* is below (above) the median. Detailed variable definitions are provided in the Appendix. Figures in parentheses represent *t*-statistics calculated based on standard errors clustered by firm. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent level, respectively.

Dep. variable =	*Switch to Big 4*				*Switch to non-Big 4*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Legal Enforcement =	Weak	Strong	Weak	Strong	Weak	Strong	Weak	Strong
Modified	-0.007**	0.001			0.008**	0.038***		
	(-2.30)	(0.35)			(2.39)	(9.95)		
Prob Shop			-0.021*	-0.008			0.044***	0.126***
			(-1.73)	(-0.80)			(2.98)	(8.22)
Log Assets	0.003***	0.002***	0.003***	0.003***	-0.005***	-0.010***	-0.005***	-0.010***
	(3.43)	(4.80)	(3.41)	(4.85)	(-4.10)	(-14.15)	(-4.26)	(-14.25)
Leverage	-0.006	-0.000	-0.006	0.000	-0.009	0.036***	-0.009	0.039***
	(-0.74)	(-0.06)	(-0.76)	(0.02)	(-0.90)	(5.35)	(-0.97)	(5.89)
Cash Flows	0.023*	-0.003	0.023	-0.004	-0.049***	-0.033***	-0.047***	-0.035***
	(1.65)	(-0.48)	(1.64)	(-0.59)	(-2.68)	(-3.18)	(-2.60)	(-3.44)
Loss	0.017***	0.008***	0.017***	0.008***	0.024***	0.013***	0.024***	0.016***
	(4.32)	(3.56)	(4.29)	(3.57)	(5.20)	(4.52)	(5.21)	(5.25)
Market-to-Book	-0.001	-0.000	-0.001	-0.000	-0.001	-0.000	-0.001	0.000
	(-1.20)	(-0.04)	(-1.23)	(-0.03)	(-0.86)	(-0.28)	(-0.75)	(0.01)
Future Financing	-0.010***	0.001	-0.010***	0.001	-0.008**	-0.000	-0.008**	0.001
	(-3.57)	(1.01)	(-3.61)	(0.94)	(-2.43)	(-0.17)	(-2.39)	(0.65)

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Returns	0.000	-0.002	0.001	-0.001	0.001	-0.011***	-0.000	-0.015***
	(0.08)	(-0.86)	(0.20)	(-0.81)	(0.18)	(-4.39)	(-0.03)	(-5.58)
Return Volatility	0.230	0.158***	0.234	0.167***	0.571***	0.602***	0.547***	0.634***
	(1.54)	(2.59)	(1.56)	(2.75)	(3.07)	(6.14)	(2.95)	(6.48)
Log Auditor Tenure	-0.000	0.009***	-0.000	0.009***	-0.007***	-0.005***	-0.007***	-0.005***
	(-0.20)	(10.14)	(-0.19)	(10.14)	(-3.17)	(-4.29)	(-3.19)	(-4.38)
Auditor's Shares	-0.006	-0.004	-0.005	-0.004	-0.017**	0.006	-0.017**	0.006*
	(-0.95)	(-1.51)	(-0.93)	(-1.51)	(-2.53)	(1.64)	(-2.52)	(1.68)
Test Statistic of [Strong – Weak]								
Modified (*Prob Shop*)	0.008**		0.013		0.029***		0.081***	
(*t*-statistic)	(1.97)		(0.84)		(5.66)		(3.83)	
Country fixed effects	Included	Included	Included	Included	Included	Included	Included	Included
Industry fixed effects	Included	Included	Included	Included	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included	Included	Included	Included	Included
Observations	29,760	64,032	29,760	64,032	29,760	64,032	29,760	64,032
Adjusted R^2	0.057	0.009	0.057	0.009	0.077	0.054	0.078	0.054

Table 11. Audit Quality Subsequent to Opinion Shopping

This table presents the results of examining the subsequent audit quality of opinion shoppers. *Opinion Shopping* is an indicator for firms whose expected probability of receiving a modified opinion reduces with switching (*Prob Shop* > 0) and switch their auditors in the subsequent period (*Switch* = 1). Country-years are classified as having weak (strong) legal enforcement if *Enforce* is below (above) the median. Detailed variable definitions are provided in the Appendix. Figures in parentheses represent *t*-statistics calculated based on standard errors clustered by firm. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent level, respectively.

Dependent variable =	*Abs. Disc. Accruals* (t + 1)			
	(1)	(2)	(3)	(4)
Legal Enforcement =	Weak	Strong	Weak	Strong
Opinion Shopping	-0.001	0.026***		
	(-0.32)	(2.61)		
Opinion Shopping to Big 4			-0.003	0.023
			(-0.73)	(1.26)
Opinion Shopping to non-Big 4			0.000	0.027**
			(0.07)	(2.30)
Switch	0.004*	0.001	0.005*	0.002
	(1.79)	(0.67)	(1.87)	(0.75)
Big 4 (t + 1)	0.002	-0.002	0.003	-0.002
	(0.66)	(-0.67)	(0.83)	(-0.61)
Log Assets (t + 1)	-0.005***	-0.006***	-0.005***	-0.006***
	(-8.71)	(-15.12)	(-8.71)	(-15.11)
Leverage (t + 1)	0.017***	-0.001	0.017***	-0.001
	(3.34)	(-0.17)	(3.33)	(-0.17)
Cash Flows (t + 1)	-0.074***	-0.094***	-0.074***	-0.094***
	(-5.25)	(-10.44)	(-5.25)	(-10.44)
Loss (t + 1)	-0.000	0.004**	-0.000	0.004**
	(-0.10)	(2.49)	(-0.10)	(2.50)
Market-to-Book (t + 1)	0.003***	0.004***	0.003***	0.004***
	(7.69)	(15.04)	(7.70)	(15.05)
Future Financing (t + 1)	0.006***	0.008***	0.006***	0.008***
	(5.43)	(9.49)	(5.43)	(9.50)
Returns (t + 1)	0.002	0.004***	0.002	0.004***
	(0.83)	(2.93)	(0.83)	(2.93)
Return Volatility (t + 1)	0.741***	0.755***	0.741***	0.754***
	(7.45)	(13.96)	(7.45)	(13.95)
Test Statistics of [Strong – Weak]				
Opinion Shopping		0.027**		
(*t*-statistic)		(2.57)		
Opinion Shopping to Big 4				0.026
(*t*-statistic)				(1.39)
Opinion Shopping to non-Big 4				0.027**
(*t*-statistic)				(2.09)
Country fixed effects	Included	Included	Included	Included
Industry fixed effects	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included
Observations	28,732	61,719	28,732	61,719
Adjusted *R*-squared	0.082	0.153	0.082	0.153

Table 12. Auditor's Portfolio Adjustment

This table presents a comparison of the characteristics of clients retained and switched in year $t + 1$ within the sample of firms that received a modified opinion in year t. Country-years are classified as having weak (strong) legal enforcement if *Enforce* is below (above) the median. Detailed variable definitions are provided in the Appendix. Figures in parentheses represent z-statistics. ***, **, and * indicate statistical significance at the 1, 5, and 10% levels, respectively, in two-tailed tests.

	(1) Retained	(2) Discarded	(3) Discarded – Retained	(4) (z-stat)
Section A. Weak Legal Enforcement (N=282)				
Log Assets	5.981	5.706	-0.276**	(-2.31)
Leverage	0.285	0.296	0.011	(0.87)
Cash Flows	0.061	0.048	-0.013**	(-2.07)
Loss	0.266	0.357	0.091***	(3.27)
Market-to-Book	1.939	1.952	0.013	(0.07)
Future Financing	0.479	0.441	-0.038	(-1.23)
Returns	0.037	0.039	0.002	(0.07)
Return Volatility	0.027	0.030	0.003***	(2.63)
Section B. Strong Legal Enforcement (N=484)				
Log Assets	5.670	5.018	-0.652***	(-5.60)
Leverage	0.281	0.290	0.009	(0.82)
Cash Flows	-0.018	-0.045	-0.026***	(-2.84)
Loss	0.510	0.615	0.105***	(4.36)
Market-to-Book	2.360	2.080	-0.280*	(-1.95)
Future Financing	0.479	0.460	-0.019	(-0.82)
Returns	0.006	0.000	-0.006	(-0.23)
Return Volatility	0.037	0.042	0.006***	(4.32)

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Section C. [Strong – Weak] Legal Enforcement						
Log Assets	-0.311***	(-2.96)	-0.688***	(-5.32)	-0.376**	(-2.26)
Leverage	-0.004	(-0.40)	-0.006	(-0.42)	-0.002	(-0.12)
Cash Flows	-0.080***	(-12.00)	-0.093***	(-10.34)	-0.013	(-1.20)
Loss	0.244***	(11.23)	0.258***	(8.67)	0.014	(0.37)
Market-to-Book	0.421***	(3.13)	0.127	(0.67)	-0.293	(-1.26)
Future Financing	0.000	(-0.02)	0.019	(0.61)	0.019	(0.49)
Returns	-0.031	(-1.24)	-0.039	(-1.08)	-0.009	(-0.20)
Return Volatility	0.010***	(9.01)	0.012***	(8.75)	0.002	(1.42)

Table 13. Cross-Sectional Analysis

This table presents the results of examining the cross-sectional variation of the effect of legal enforcement on clients' opinion shopping behavior. In Panel A, *High Total Accruals* is an indicator variable that equals 1 if income before extraordinary items less cash flows from operations scaled by lagged total assets, is above the median value, and 0 otherwise. In Panel B, *High R&D* is an indicator variable that equals 1 if research and development expenditure divided by total assets is above the median value within firms with positive R&D expenditures and 0 otherwise. In Panel C, *High Big 4 Shares* is an indicator variable that equals 1 for above-median values of aggregate Big 4 shares, and 0 otherwise, where aggregate Big 4 shares are the sum of the industry market share of Big 4 auditors within the country-industry-year, with market shares calculated based on clients' sales. Country-years are classified as having weak (strong) legal enforcement if *Enforce* is below (above) the median. Detailed variable definitions are provided in the Appendix. Figures in parentheses represent *t*-statistics calculated based on standard errors clustered by firm. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent level, respectively.

Panel A. Total accruals

Dependent variable =	*Switch*					
Legal enforcement =	All		Weak		Strong	
	(1)	(2)	(3)	(4)	(5)	(6)
Modified	0.032***		0.017***		0.044***	
	(7.84)		(2.58)		(7.99)	
Modified × High Total Accruals	-0.020***		-0.029***		-0.012*	
	(-3.78)		(-3.45)		(-1.73)	
Prob Shop		0.115***		0.090***		0.139***
		(6.99)		(3.38)		(6.28)
Prob Shop × High Total Accruals		-0.086***		-0.128***		-0.050*
		(-4.10)		(-3.75)		(-1.76)
High Total Accruals	-0.003	-0.021***	-0.005	-0.029***	-0.002	-0.012**
	(-1.21)	(-4.93)	(-0.95)	(-4.52)	(-0.64)	(-2.16)
Control variables	Included	Included	Included	Included	Included	Included
Country fixed effects	Included	Included	Included	Included	Included	Included
Industry fixed effects	Included	Included	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included	Included	Included
Observations	93,793	93,793	29,760	29,760	64,032	64,032
Adjusted *R*-squared	0.050	0.050	0.080	0.081	0.040	0.039

Panel B. Research and development expenditure

Dependent variable =	*Switch*					
Legal enforcement =	All		Weak		Strong	
	(1)	(2)	(3)	(4)	(5)	(6)
Modified	0.024***		0.003		0.041***	
	(7.59)		(0.60)		(8.89)	
Modified × High R&D	-0.017*		-0.022		-0.022*	
	(-1.79)		(-1.63)		(-1.83)	
Prob Shop		0.080***		0.025		0.130***
		(6.31)		(1.34)		(7.05)
Prob Shop × High R&D		-0.048		-0.038		-0.083*
		(-1.29)		(-0.56)		(-1.83)
High R&D	-0.017***	-0.028***	-0.011	-0.019*	-0.018***	-0.036***
	(-4.26)	(-3.90)	(-1.52)	(-1.89)	(-3.80)	(-3.84)
Control variables	Included	Included	Included	Included	Included	Included
Country fixed effects	Included	Included	Included	Included	Included	Included
Industry fixed effects	Included	Included	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included	Included	Included
Observations	93,793	93,793	29,760	29,760	64,032	64,032
Adjusted *R*-squared	0.050	0.050	0.080	0.080	0.040	0.039

Panel C. Aggregate Big 4 shares

Dependent variable =	*Switch*					
Legal enforcement =	All		Weak		Strong	
	(1)	(2)	(3)	(4)	(5)	(6)
Modified	0.029***		-0.000		0.046***	
	(6.83)		(-0.02)		(7.51)	
Modified × High Big 4 Shares	-0.013**		0.004		-0.015**	
	(-2.39)		(0.53)		(-2.02)	
Prob Shop		0.096***		0.034		0.119***
		(6.18)		(1.46)		(5.24)
Prob Shop × High Big 4 Shares		-0.052**		-0.024		-0.001
		(-2.42)		(-0.72)		(-0.05)
High Big 4 Shares	0.000	-0.011**	-0.006	-0.009	0.007**	0.004
	(0.11)	(-2.35)	(-1.06)	(-1.20)	(2.32)	(0.70)
Control variables	Included	Included	Included	Included	Included	Included
Country fixed effects	Included	Included	Included	Included	Included	Included
Industry fixed effects	Included	Included	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included	Included	Included
Observations	93,793	93,793	29,760	29,760	64,032	64,032
Adjusted *R*-squared	0.050	0.050	0.080	0.080	0.040	0.039

Table 14. Voluntary versus Mandatory Rotation Regime

This table presents the results of testing whether the existence of a mandatory auditor rotation requirement affects the association between legal enforcement and audit clients' opinion shopping behavior. The existence of mandatory auditor rotation requirements is identified by Brown et al. (2014). Columns (1) and (2) [(3) and (4)] represent observations without [with] a mandatory auditor rotation policy. Detailed variable definitions are provided in the Appendix. Figures in parentheses represent *t*-statistics calculated based on standard errors clustered by firm. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent level, respectively.

Dependent variable =	*Switch*			
	Voluntary Auditor Rotation		Mandatory Auditor Rotation	
	(1)	(2)	(3)	(4)
Modified	-0.005		0.037***	
	(-1.17)		(3.44)	
Modified × Enforce	0.063***		-0.003	
	(5.55)		(-0.26)	
Prob Shop		0.009		0.125***
		(0.49)		(3.15)
Prob Shop × Enforce		0.194***		-0.030
		(4.17)		(-0.68)
Enforce	0.104***	0.152***	0.085***	0.085***
	(2.59)	(3.72)	(5.14)	(4.70)
Log Assets	-0.003***	-0.003***	-0.007***	-0.007***
	(-2.94)	(-3.14)	(-6.81)	(-6.88)
Leverage	0.017*	0.018*	0.024***	0.027***
	(1.68)	(1.84)	(2.79)	(3.10)
Cash Flows	-0.023	-0.026	-0.040***	-0.043***
	(-1.37)	(-1.59)	(-2.94)	(-3.17)
Loss	0.032***	0.033***	0.024***	0.025***
	(6.87)	(7.12)	(5.93)	(6.32)

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Market-to-Book	0.000	0.000	-0.001	-0.001
	(0.29)	(0.43)	(-1.52)	(-1.25)
Future Financing	-0.011***	-0.010***	-0.000	0.001
	(-3.33)	(-2.95)	(-0.05)	(0.39)
Returns	-0.005	-0.008*	-0.011***	-0.014***
	(-1.24)	(-1.87)	(-3.26)	(-4.04)
Return Volatility	0.747***	0.781***	0.749***	0.781***
	(4.65)	(4.88)	(5.91)	(6.17)
Log Auditor Tenure	-0.011***	-0.011***	0.007***	0.007***
	(-5.00)	(-5.02)	(4.03)	(3.96)
Auditor's Shares	-0.001	-0.001	-0.004	-0.004
	(-0.24)	(-0.21)	(-0.80)	(-0.79)
Country fixed effects	Included	Included	Included	Included
Industry fixed effects	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included
Observations	41,368	41,368	52,424	52,424
Adjusted *R*-squared	0.056	0.056	0.051	0.051

Table 15. Types of Audit Opinions

This table presents the results of examining the effect of legal enforcement on clients' opinion shopping behavior depending on the type of audit opinions. Odd columns decompose *Modified* into *Unqualified Opinions with Explanatory Language* and *Other Modified Opinions*. Even columns further decompose *Other Modified Opinions* into *Qualified Opinions*, *No Opinions*, and *Adverse Opinions*. Country-years are classified as having weak (strong) legal enforcement if *Enforce* is below (above) the median. Detailed variable definitions are provided in the Appendix. Figures in parentheses represent *t*-statistics calculated based on standard errors clustered by firm. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent level, respectively.

Dependent variable =	*Switch*					
Legal enforcement =	All		Weak		Strong	
	(1)	(2)	(3)	(4)	(5)	(6)
Unqualified Opinion with Explanatory Language	0.014*** (4.49)	0.014*** (4.47)	-0.005 (-1.20)	-0.005 (-1.20)	0.027*** (6.09)	0.027*** (6.10)
Other Modified Opinions	0.113*** (10.07)		0.060*** (4.50)		0.173*** (9.20)	
Qualified Opinion		0.081*** (6.89)		0.039*** (2.91)		0.148*** (6.59)
No Opinion		0.223*** (7.95)		0.280*** (5.10)		0.204*** (6.25)
Adverse Opinion		0.424*** (4.09)		0.326*** (3.25)		0.577*** (3.51)
Control variables	Included	Included	Included	Included	Included	Included
Country fixed effects	Included	Included	Included	Included	Included	Included
Industry fixed effects	Included	Included	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included	Included	Included
Observations	93,793	93,793	29,760	29,760	64,032	64,032
Adjusted *R*-squared	0.051	0.052	0.081	0.082	0.041	0.042

Table 16. Propensity-Score Matching

This table presents the empirical results for the propensity-score-matched sample. Firms are matched one-to-one with another firm that has the closest propensity score without replacement within the country-industry-year, with a caliper distance set at 0.2 of the logit of propensity scores. Propensity scores are calculated with a logit estimation, including all control variables in the main test, as suggested by Shipman et al. (2017). In columns (1) and (2), firms are matched based on the propensity to receive modified opinions (*Modified* = 1), and *Enforce* is the continuous value of Brown et al.'s (2014) score. In columns (3) and (4), firms are matched based on the propensity to be included in a high legal enforcement country (*Strong Enforce* = 1), where *Strong Enforce*, a dummy variable for high legal enforcement, is used as the test variable. Country-years are classified as having weak (strong) legal enforcement if *Enforce* is below (above) the median. Detailed variable definitions are provided in the Appendix. Figures in parentheses represent *t*-statistics calculated based on standard errors clustered by firm. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent level, respectively.

Dependent variable =	*Switch*			
Matching criterion =	Modified vs. Clean Audit Opinions		Weak vs. Strong Legal Enforcement	
	(1)	(2)	(3)	(4)
Modified	0.002		0.006	
	(0.24)		(1.27)	
Modified × *(Strong) Enforce*	0.056***		0.032***	
	(3.41)		(4.16)	
Prob Shop		0.015		0.042**
		(0.35)		(2.24)
Prob Shop × *(Strong) Enforce*		0.213***		0.054*
		(2.91)		(1.79)
(Strong) Enforce	0.211***	0.289***	0.088***	0.115***
	(4.94)	(6.63)	(8.30)	(10.23)
Log Assets	-0.009***	-0.010***	-0.006***	-0.006***
	(-4.95)	(-5.30)	(-5.14)	(-5.11)
Leverage	0.025*	0.023	0.023**	0.024***
	(1.71)	(1.58)	(2.52)	(2.62)
Cash Flows	0.010	0.020	-0.023	-0.027*
	(0.45)	(0.84)	(-1.50)	(-1.74)

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Loss	0.023***	0.023***	0.036***	0.037***
	(3.43)	(3.44)	(8.17)	(8.40)
Market-to-Book	-0.001	-0.001	-0.001**	-0.001*
	(-1.13)	(-0.96)	(-2.16)	(-1.93)
Future Financing	-0.014***	-0.014***	-0.009***	-0.008***
	(-2.86)	(-2.81)	(-3.00)	(-2.80)
Returns	-0.011*	-0.013**	-0.004	-0.006
	(-1.67)	(-2.11)	(-0.96)	(-1.45)
Return Volatility	0.937***	0.868***	0.622***	0.662***
	(3.86)	(3.57)	(3.98)	(4.23)
Log Auditor Tenure	0.005	0.005	-0.001	-0.001
	(1.52)	(1.49)	(-0.62)	(-0.58)
Auditor's Shares	-0.016	-0.015	-0.009	-0.009
	(-1.44)	(-1.41)	(-1.56)	(-1.58)
Country fixed effects	Included	Included	Included	Included
Industry fixed effects	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included
Observations	16,426	16,426	51,404	51,404
Adjusted *R*-squared	0.067	0.066	0.058	0.058

Table 17. Alternative Specifications

This table presents the results of examining the effect of legal enforcement on clients' opinion shopping behavior with alternative specifications. Panel A examines different definitions of *Prob Shop*. In column (1), the test variable is *Raw Shop*, the difference in the raw predicted value of receiving a modified opinion depending on the auditor switch, estimated with equation (2). In column (2), the test variable is *Dummy Shop*, a dummy variable for positive values of *Prob Shop*. In column (3), the test variable is *Prob Shop* (*alternative*), in which *Prob Shop* with absolute values within one percent are replaced with zero. Panel B examines different definitions of enforcement: *Investor Protection*, *Anti-Self-Dealing Index*, and *Common Law.* Panel C uses alternative model specifications. Columns (1) and (2) present the results with firm-fixed effects, and columns (3) and (4) those with logit regressions. Detailed variable definitions are provided in the Appendix. Figures in parentheses represent *t*-statistics calculated based on standard errors clustered by firm. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent level, respectively.

Panel A. Alternative definitions of the *Prob Shop*

	Switch		
	Alternative *Opinion*		
	(1)	(2)	(3)
Opinion =	*Raw Shop*	*Dummy Shop*	*Alt. Prob Shop*
Opinion	-0.008	-0.017**	-0.011
	(-1.15)	(-2.04)	(-0.42)
Opinion × *Enforce*	0.059***	0.064***	0.159***
	(4.95)	(3.40)	(3.57)
Control variables	Included	Included	Included
Country fixed effects	Included	Included	Included
Industry fixed effects	Included	Included	Included
Year fixed effects	Included	Included	Included
Observations	93,793	93,793	93,793
Adjusted *R*-squared	0.051	0.050	0.051

Panel B. Alternative definitions of legal enforcement

Dependent variable =	*Switch*					
Enforce =	*Investor Protection*		*Anti-Self-Dealing*		*Common Law*	
	(1)	(2)	(3)	(4)	(5)	(6)
Modified	-0.008		-0.011		0.007	
	(-0.64)		(-1.20)		(1.48)	
Modified × *Enforce*	0.040***		0.053***		0.033***	
	(2.73)		(4.19)		(5.37)	
Prob Shop		-0.031		-0.033		0.017
		(-0.63)		(-0.96)		(0.87)
Prob Shop × *Enforce*		0.138**		0.170***		0.105***
		(2.35)		(3.60)		(4.46)
Enforce	-0.111***	-0.083***				
	(-8.46)	(-5.34)				
Control variables	Included	Included	Included	Included	Included	Included
Country fixed effects	Included	Included	Included	Included	Included	Included
Industry fixed effects	Included	Included	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included	Included	Included
Observations	93,793	93,793	93,592	93,592	93,592	93,592
Adjusted *R*-squared	0.051	0.050	0.050	0.050	0.050	0.050

Panel C. Alternative estimation methods

Dependent variable =	*Switch*			
	Firm-fixed effects		Logit estimations	
	(1)	(2)	(3)	(4)
Modified	-0.051***		-0.204***	
	(-6.58)		(-3.23)	
Prob Shop		-0.121***		-0.312
		(-4.03)		(-1.27)
Modified × Enforce	0.103***		0.735***	
	(8.40)		(6.97)	
Prob Shop × Enforce		0.274***		1.777***
		(5.76)		(4.21)
Enforce	0.138***	0.211***	1.219***	1.770***
	(7.96)	(11.04)	(7.12)	(9.51)
Log Assets	-0.014***	-0.014***	-0.066***	-0.067***
	(-5.42)	(-5.44)	(-7.96)	(-8.11)
Leverage	0.038***	0.040***	0.204***	0.218***
	(3.34)	(3.51)	(3.12)	(3.34)
Cash Flows	-0.013	-0.015	-0.285***	-0.313***
	(-1.05)	(-1.17)	(-2.83)	(-3.10)
Loss	0.020***	0.021***	0.288***	0.299***
	(6.13)	(6.47)	(9.63)	(9.99)
Market-to-Book	-0.000	-0.000	-0.008*	-0.007*
	(-0.53)	(-0.36)	(-1.94)	(-1.67)

(continued on the next page)

Future Financing	-0.005**	-0.004**	-0.054**	-0.047**
	(-2.31)	(-2.03)	(-2.35)	(-2.02)
Returns	-0.004	-0.005**	-0.081***	-0.103***
	(-1.35)	(-1.98)	(-3.07)	(-3.93)
Return Volatility	0.670***	0.690***	8.063***	8.376***
	(5.75)	(5.92)	(8.77)	(9.11)
Log Auditor Tenure	0.108***	0.108***	-0.021	-0.022
	(58.90)	(59.00)	(-1.40)	(-1.47)
Auditor's Shares	0.001	0.002	-0.055	-0.054
	(0.18)	(0.23)	(-1.24)	(-1.20)
Firm fixed effects	Included	Included	-	-
Country fixed effects	-	-	Included	Included
Industry fixed effects	-	-	Included	Included
Year fixed effects	Included	Included	Included	Included
Observations	91,999	91,999	93,762	93,762
Adjusted *R*-squared	0.107	0.106		
Pseudo *R*-squared			0.069	0.068

Essay 2.

Preoccupied Auditors: The Spillover Effect of Public Firm Audits on Private Firm Audit Quality

1. Introduction

An auditor provides audit services to multiple clients. Although each audit contract is made independently, the auditor's output for one client is affected by their other clients. For example, an auditor's audit quality can be affected by accumulating industry-specific knowledge from numerous other clients in the same industry (Gaver and Utke 2019; Hogan and Jeter 1999; Reichelt and Wang 2010) or by observing other clients' involvement in litigious events or financial misreporting (Francis and Michas 2013; Hall, Judd, and Sunder 2021; Lennox and Li 2014). To fully understand the quality of an auditor's output, one must consider not only the characteristics of the focal client but also those of the auditor's other clients. In this study, I examine whether the quality of audits provided to a focal audit client depends on the regulatory exposure of the auditor's other clients.

When certain clients (hereafter, "regulated clients") face stricter regulations than do others (hereafter, "unregulated clients"), the auditor must make additional effort to audit the regulated client, which may either benefit or harm the auditor's other unregulated clients. Auditors have limited resources and capacity constraints (Bills, Swanquist, and Whited 2016; López and Peters 2012; Persellin, Schmidt, Vandervelde, and Wilkins 2019). Accordingly, audits of regulated clients may adversely affect the auditor's other clients because the former clients preoccupy much of the auditor's resources. To meet the stricter demands placed on regulated clients, the auditor may choose to secure the required resources by shifting resources from other audit engagements. In this case, the regulated clients in an auditor's portfolio would harm unregulated clients as the latter may suffer from lower audit quality.

However, the auditors' experience with the stricter requirements of regulated clients may enhance the auditor's skill and efficiency, creating a knowledge spillover and benefiting other audit engagements. Auditors apply

their knowledge and practices across their engagements (Duh, Knechel, and Lin 2020), enabling them to develop specialized knowledge and provide higher quality audits to their clients (Hogan and Jeter 1999; Reichelt and Wang 2010). Auditors also learn from their experiences with regulators' inspections and apply their knowledge to other audit engagements (Fung, Raman, and Zhu 2017). Similarly, when certain clients require increased audit effort due to stricter regulations, auditors may become better trained and more competent and thus provide higher quality audit services to their other clients. Collectively, it is unclear ex ante how the existence of regulated audit clients affects the quality of the auditor's output provided to unregulated clients, and thus the question remains an empirical question.

To examine this issue, I take advantage of the disclosure environment in Korea, where financial information on both public and private firms is publicly available.[1] In most countries, public firms are subject to stricter regulations than private firms (Minnis and Shroff 2017). In Korea, although both public and private firms are required to have their annual financial statements be audited by an external auditor, public firms additionally are required to have their interim financial statements reviewed and the internal controls attested by the auditor. Thus, clients of the same auditor face systematically different levels of exposure to regulations depending on their listing status, such that public firms are regulated more strictly than private firms.

I test my hypothesis using a sample of 109,010 firm-year observations of Korean private firms during the 2001 to 2018 period. The sample includes 1,816 distinct auditor-year observations wherein public clients constitute 14.6 percent of the average auditor's size-weighted portfolio. Using the absolute value of discretionary accruals, accruals error, and opinion

[1] In this essay, a "public firm" refers to a corporation whose equity is traded in the public market, and a "private firm" refers to that whose equity is privately held.

qualifications as proxies for audit quality, I find that auditors with a portfolio tilted toward public clients provide lower audit quality to their private clients than other auditors. I confirm the results with a difference-in-differences analysis taking advantage of an exogenous shock to the client portfolio of one of the Big N auditors, where the auditor was penalized in 2017 for being involved in a massive accounting fraud of a client. The results suggest that the negative impact of an auditor's strategic allocation of resources from private to public clients outweighs the benefits of knowledge spillover from public to private clients. Notably, I find that public clients do not receive lower audit quality from the corresponding auditor, suggesting that the lower audit quality provided to private firms is not driven by the auditor's incompetence.

I further explore two alternative channels through which an auditor's portfolio composition affects audit quality: resource allocation and knowledge spillover. Consistent with an auditor's allocation of resources away from private firms causes the quality deterioration in audits provided to private firms, I find that the observed effects are strengthened for auditors with high resource adjustment costs and in the presence of exogenous increases in audit requirements toward public firms. In addition, consistent with knowledge spillover from public to private clients creating positive quality impacts on private firms, I find that the negative effect of public firms on private firms is muted when private and public firms share an industry membership or apply the same accounting standards. Lastly, I find that the main findings are observed for only non-Big N auditors but not Big N auditors, consistent with the Big N auditors having smaller resource adjustment costs and realizing greater knowledge spillover than non-Big N auditors.

This study makes several contributions to the literature. First, it provides implications for regulators by documenting negative spillover effects of strict regulations on unregulated sectors, directly answering the call

by DeFond and Zhang (2014) for further evidence on the cost of regulations. I show that the unregulated sector may incur non-trivial costs due to these regulations and suggest that regulators should consider the economic impact of regulations on the overall audit market, even when the regulations target only a subset of firms. In this sense, the study extends the work of Duguay, Minnis, and Sutherland (2020), who illustrate the externalities of the Sarbanes–Oxley Act (SOX) on the audit market for private and nonprofit firms. Taken together, the conclusion reached by DeFond and Lennox (2011), namely that the average audit quality in the public firm market improved after the enactment of SOX, should be revisited from the perspective of private firms.

Second, the study elucidates the economic decisions made under resource constraints. Whereas most auditing studies examine the effects of auditor characteristics on the average quality of audits provided by the auditor (Bills et al. 2016; Choi, Kim, Kim, and Zang 2010; Francis, Michas, and Yu 2013; Francis and Yu 2009; Gaeremynck, Van Der Meulen, and Willekens 2008), this study uniquely shows that an auditor may behave differently toward different clients within their portfolio. Taking advantage of variations in the financial reporting regulations applied to different clients, I provide novel evidence that a spillover effect within an auditor's portfolio is due to not only learning or contagion (Francis and Michas 2013; Reichelt and Wang 2010) but also the shifting of resources from one client to another. Thus, this study complements studies from other disciplines that show how agents exert unequal effort across counterparties (Ben-Rephael, Da, and Israelsen 2017; Kempf, Manconi, and Spalt 2017; Liu, Low, Masulis, and Zhang 2020; Masulis and Mobbs 2014).

Finally, this study clarifies the information environment surrounding private firms, which are important players in the modern economy with significant global influence in terms of employment and output (e.g., Allee

and Yohn 2009; Chaney, Jeter, and Shivakumar 2004; Haw, Lee, and Lee 2014; Hope, Thomas, and Vyas 2011). Despite the economic importance of private firms, our understanding of the information environment surrounding private firms is limited. Increasingly, accounting standard setters are paying attention to the financial reporting requirements of private companies (Botosan et al. 2006; Hope et al. 2011). In the absence of public disclosures and market prices, audits are often the sole source of credible information provided to the stakeholders of private firms (Lennox 2005). The differences between public and private firms limit the generalizability of accounting and auditing research focusing exclusively on public firms, leading to calls for more direct research on private firms (Langli and Svanström 2014). I show that when audit requirements differ across clients, a simple requirement on the external audits of private firms may not achieve the intended goal because auditors do not exert the same level of effort toward all of their clients. The mere fact that a private firm is audited may falsely imply that the firm has higher quality financial information.

The essay proceeds as follows. The second section reviews the literature and develops the hypotheses. The third section describes the research design. The fourth section presents the main empirical results, and the fifth section provides the results of additional analyses. The last section concludes the paper.

2. Literature Review and Hypothesis Development

2.1 Strategic resource allocation

All economic agents face the fundamental constraint of limited resources. Certainly, auditors are not exempt from this constraint. For example, auditors with larger numbers of audit engagements exhibit lower audit quality than their counterparts with fewer engagements (Goodwin and Wu 2016; Lai et al. 2018; Sundgren and Svanström 2014). Similarly, a sudden increase in a local auditor's client base increases the burden imposed on the auditor and

temporarily decreases the quality of its audits (Bills et al. 2016). Auditors also experience a decrease in audit quality when their clients' audit deadlines are concentrated during the "busy season," defined as the period around the first calendar quarter when most audits, tax reports, and professional services are performed (Czerney, Jang, and Omer 2019; López and Peters 2012; Persellin et al. 2019).

An auditor must decide how to allocate its limited resources and capacity. Strategic allocations of constrained resources have been examined in related areas. Most relevant to this study is the study of Masulis and Mobbs (2014), who show that busy directors allocate their resources unequally to their directorships, such that independent directors devote greater effort to more prestigious than less prestigious directorships. Specifically, independent directors with multiple directorships are less likely to miss board meetings or depart from the directorship of firms with larger market capitalization than of smaller firms. Consequently, firms with a greater proportion of directors who prioritize the focal firm exhibit superior operating performance, higher market valuation (Tobin's *q*), and greater sensitivity of CEO turnover to negative performance than their counterparts. In contrast, firms that are of less importance to the directors suffer from inferior performance. Specifically, Masulis and Zhang (2019) find that when directors face challenges (e.g., declining performance, financial misconduct or distress, mergers and acquisitions, divestitures, CEO turnovers) in one directorship, they deprioritize their directorships of other firms by attending fewer meetings, trading less frequently in the firms' stock, and resigning more frequently from the directorships. As a consequence, firms with more distracted independent directors tend to experience lower firm valuation, poorer operating performance, and weaker merger and acquisition profitability. Similarly, studies on other economic agents, such as institutional investors (Ben-Rephael et al. 2017; Kempf et al. 2017; Liu et al. 2020) and analysts (Driskill,

Kirk, and Tucker 2020), provide evidence on strategic resource allocations of the agents.

Like other economic agents, auditors must allocate their efforts strategically to accommodate their resource constraints. Specifically, an auditor may choose to prioritize clients that have stricter audit requirements and are subject to greater regulatory scrutiny over other clients. Accordingly, the stricter audit requirements of some clients may create negative spillover effects on the auditor's other clients by preoccupying the auditor's resources and causing the auditor to sacrifice the quality of audits for their less regulated clients.

2.2 Knowledge spillover

Auditors learn from their audit engagements and provide higher quality auditing services as they gain experience. As auditors obtain a detailed understanding of their clients while conducting audits, they are able to provide higher-quality services to their clients as their auditing tenure lengthens (Chen, Lin, and Lin 2008; Ghosh and Moon 2005; Johnson, Khurana, and Reynolds 2002; Mansi, Maxwell, and Miller 2004; Myers, Myers, and Omer 2003). Additionally, non-audit services comprise a source of client-specific knowledge and help auditors improve the quality of audits (Huang, Mishra, and Raghunandan 2007; Krishnan and Visvanathan 2011; Lim and Tan 2008).

Auditors transfer their experience with audit engagements to other clients through knowledge sharing (Duh et al. 2020).[2] Auditors with larger offices have more in-house experience and expertise with the audits of SEC registrants and thus are able to provide higher quality audits than their smaller peers (Choi et al. 2010; Francis et al. 2013; Francis and Yu 2009; Ittonen, Johnstone, and Myllymäki 2015). Auditors develop industry-specific

[2] Such knowledge sharing may also create a negative contagion effect involving low-quality audits (Francis and Michas 2013).

expertise by auditing numerous clients in the same industry and thus can provide higher quality audits to firms in their specialized industry (Gaver and Utke 2019; Hogan and Jeter 1999; Reichelt and Wang 2010). Such expertise is shared even across countries through the auditors' global network (Carson 2009).

Auditors also learn from regulatory experience. For example, Fung et al. (2017) show that foreign auditors that experience international inspections by the Public Company Accounting Oversight Board of the United States (U.S.) in turn provide higher quality audits to their clients that are not listed in the U.S. Lennox and Li (2014) find that auditors who are sued regarding past audits are induced to provide higher-quality audits, and such lawsuits reduce the probability of subsequent restatements for their clients. Hall et al. (2021) show that when an auditor's client experiences bank failure, the auditor becomes more conservative when auditing other surviving firms. Similarly, applying strict audit procedures when auditing regulated clients may induce auditors to gain valuable experience and learn to provide more efficient audits. If the auditor takes advantage of the knowledge spillover across clients in their portfolio, then an auditor's regulated clients would benefit their unregulated clients by improving the auditor's audit quality.

2.3 Hypothesis development

The above discussions demonstrate the multifaceted nature of the spillover effect of a regulated client on an unregulated client within an auditor's portfolio. An auditor's strategic allocation of resources away from unregulated clients to regulated ones may adversely affect the quality of audits provided to unregulated clients. However, auditors who respond to the strict audit demands of their regulated clients may become more efficient and create a knowledge spillover, in turn providing benefits to the auditors' unregulated clients. Drawing on these opposing expectations of the effect of

regulated clients on the quality of audits provided to unregulated clients of the same auditor, I present the following null hypothesis.

> *Hypothesis.* The importance of an auditor's regulated clients does not affect the quality of audits provided to the auditor's unregulated clients.

3. Research Design

3.1 Variation in regulations

To test the hypothesis, I take advantage of the setting in Korea, where the financial statements of both public and private firms are mandatorily audited and publicly disclosed. I take advantage of an audit client's listing status as a source of systematic variation in the client's regulatory requirements. Public firms face stricter audit requirements and regulatory scrutiny than private firms.[3] Additionally, whereas most countries impose public disclosure and audit requirements on public firms, the regulations imposed on private firms vary across countries: some countries impose no requirements, while others require only private firms above a size threshold to be audited (Minnis and Shroff 2017).[4] Currently, the gap in audit requirements imposed on public and private firms is wider than in previous eras due to recent regulatory reforms that impose stricter requirements on public firms' disclosures and audits. For example, DeFond and Lennox (2011) document that after the adoption of SOX in the U.S., many auditors departed from the public firm audit market due to the stricter burdens imposed on these firms. Duguay et al.

[3] In addition, auditors expend greater effort on public firms than on private firms because public firms have higher demands for financial reporting quality (Ball and Shivakumar 2005; Burgstahler, Hail, and Leuz 2006; Hope, Thomas, and Vyas 2013), and the auditor faces higher litigation risks in the case of audit failure (St. Pierre and Anderson 1984). Accordingly, auditors charge higher fees and expend greater audit effort towards public firms than private firms (Abbott, Gunny, and Pollard 2017; Clatworthy and Peel 2007; O'Keefe, Simunic, and Stein 1994) and are less likely to accept public firms as their clients (Johnstone and Bedard 2003, 2004).

[4] Various countries, including Korea and countries in the European Union, require private firms above certain size thresholds to disclose their financial statements publicly (Ball and Shivakumar 2005; Bernard, Burgstahler, and Kaya 2018). Other countries, such as the U.S. and Canada, do not impose any disclosure requirements on private firms. For example, in the U.S., private firms are not required to disclose their financial statements or be audited unless they issue public debt (Givoly, Hayn, and Katz 2010).

(2020) report a spillover of this change in the audit market to the private and nonprofit firm market, resulting in increased audit costs and a decrease in the market share of high-quality auditors in the non-public sector.

In Korea, the audit requirements imposed on public firms are more intensive than those on private firms in terms of the accounting standards and audit scope. First, public firms in Korea are required to adopt the International Financial Reporting Standards (IFRS), whereas private firms are expected to comply with the Korean Generally Accepted Accounting Principles (K-GAAP), a rules-based standard, and can voluntarily adopt the IFRS. Compliance with the IFRS, a principles-based standard, requires greater professional judgment and auditing effort than compliance with rules-based standards (Kim, Liu, and Zheng 2012). Second, the audit scope differs between public and private firms. Although both types of firms are subject to mandatory audits of annual financial statements, only public firms are required to have their interim financial statements reviewed by an external auditor.[5] Moreover, all public firms have been required to set up an internal control system and submit this system for review by an auditor since 2004, whereas only large private firms are subject to this requirement.[6] Thus, auditors must expend more effort on the audits of public firms than those of private firms.

I measure the auditors' exposure to regulated clients using the weight on public clients within an auditor's portfolio (hereafter "portfolio weight"). To ensure that the size difference between public and private firms does not

[5] All public firms and certain private firms that exceed size thresholds are mandated to undergo external audits of the firm's annual financial statements (Act on External Audit of Stock Companies Article 2–1). The semi-annual financial statements of public firms, as well as the quarterly financial statements of firms with total assets greater than KRW 500 million, must be reviewed by an external auditor (Financial Investment Services and Capital Markets Act Article 160, and Decree Article 170). These requirements have become stricter in recent years. For example, beginning in 2022, public firms will need to have their internal controls audited rather than reviewed.

[6] Private firms with total assets less than KRW 100 billion (50 billion before 2005) are exempted from the internal control attestation requirement (Act on External Audit of Stock Companies Article 2–2).

drive the results, I weight each client by size. Specifically, *%Public Clients* is the size-weighted percentage of public clients in an auditor's portfolio, where size is proxied by the square-root of total assets as in prior auditing studies (Gul, Fung, and Jaggi 2009; Hogan and Jeter 1999; Kwon 1996; Mayhew and Wilkins 2003).[7] Formally, for each client *i* of auditor *a*,

$$\%Public\ Clients_a = \frac{\sum_{public}\sqrt{Assets_i}}{\sum_{public}\sqrt{Assets_i}+\sum_{private}\sqrt{Assets_i}}. \quad (1)$$

To reduce the impact of time trends in the sample, I also use *Ranked %Public Clients*, defined as the decile rank of *%Public Clients* within each year. Specifically, I assign annual decile ranks to observations with non-zero values of *%Public Clients*, and 0 to those with no public clients. I scale the ranked variable by 10 such that the value of *Ranked %Public Clients* yields in a range between 0 and 1.

3.2 Audit quality

Audit quality is measured in three ways: the absolute value of discretionary accruals, the accruals estimation error, and the issuance of qualified opinions. To calculate the absolute value of performance-adjusted discretionary accruals, *Abs. Disc. Accruals*, discretionary accruals are measured using the residuals from the following modified Jones' (1991) model.

$$TotalAccruals_t = \beta_0 + \beta_1\, 1/TotalAssets_{t-1} + \beta_2(Sales_t - \Delta Receivables_t) + \beta_3 PPE_t + \varepsilon_t. \quad (2)$$

For each firm in year *t*, *Total Accruals* is net income less cash flows from operations; *Sales* is total revenues; *ΔReceivables* is the change in account receivables from year *t* – 1 to *t*; and *PPE* is property, plants, and equipment. All variables in equation (2) are scaled by beginning-of-year total assets. I adjust the performance effect by subtracting the median value of discretionary accruals within the same return-on-asset quintile and fiscal year (Kothari,

[7] Audit fees cannot be used as a proxy for size in this study because audit fee data are not available for private firms.

Leone, and Wasley 2005). *Abs. Disc. Accruals* is calculated as the absolute value of performance-adjusted discretionary accruals multiplied by 100. A higher value of *Abs. Disc. Accruals* indicates lower audit quality.

The second is the accruals estimation error, *Accruals Error*, which is measured using the standard deviation of the residuals from the following model by Dechow and Dichev (2002) and McNichols (2002).

$$\begin{aligned} WorkingCapitalAccruals_t = {} & \beta_0 + \beta_1 Cashflows_{t-1} \\ & + \beta_2 Cashflows_t + \beta_3 Cashflows_{t+1} \\ & + \beta_4 (Sales_t - \Delta Receivables_t) + \beta_5 PPE_t + \varepsilon_t. \end{aligned} \quad (3)$$

Working Capital Accruals is the change in current assets, excluding cash and investment securities, less the change in current liabilities, excluding financial liabilities and tax payables. *Cash Flows* is cash flows from operations. All variables in equation (3) are scaled by the average value of beginning and year-end total assets. *Accruals Error* is calculated as the standard deviation of the residuals from equation (3) from year $t - 4$ to t, multiplied by 100, and is calculated for observations with at least three years of non-missing data. A higher value of *Accruals Error* represents lower audit quality.

Lastly, I use the issuance of qualified opinions, *Qualified Opinion*, which is represented by a dummy variable that equals 1 if the auditor issues a qualified opinion, adverse opinion, or disclaimer of opinion and 0 if the auditor issues a clean opinion. A higher probability of issuing a qualified opinion represents a higher audit quality (Craswell, Stokes, and Laughton 2002; DeFond, Raghunandan, and Subramanyam 2002; Reynolds and Francis 2001).

3.3 Empirical model

I estimate the following equation using ordinary least-squares estimation.

$$\begin{aligned} AuditQuality_{i,t} = {} & \beta_1 \%PublicClients_{a,t} + \beta Controls_{i,t} \\ & + \gamma Year_t + \delta Industry_j + \varepsilon_{i,t}. \end{aligned} \quad (4)$$

For each firm *i* operating in industry *j* in year *t* and audited by auditor *a*, *Audit Quality* is either *Abs. Disc. Accruals*, *Accruals Error*, or *Qualified Opinion*. The variable of interest is *%Public Clients*. Positive values of β_1 in the tests of *Abs. Disc. Accruals* and *Accruals Error* and a negative β_1 in the test of *Qualified Opinion* are consistent with auditors' strategic resource allocation from private to public clients; the opposite signs are consistent with auditors' knowledge spillover from public to private clients.

I include firm and auditor characteristics identified in the literature as factors affecting audit quality (e.g., Barth, Landsman, and Lang 2008; Bills et al. 2016; Chung, Sonu, Zang, and Choi 2019; Gaver and Utke 2019; Lim and Tan 2010). For the tests of *Abs. Disc. Accruals* and *Accruals Error*, I include the following firm-level control variables: *Log Assets*, the natural logarithm of total assets; *Leverage*, total liabilities divided by total assets; *Cash Flows*, operating cash flows divided by beginning-of-year total assets; *Loss*, an indicator of firms with negative net income; *Sales Growth*, sales in year *t* divided by sales in year *t* – 1 minus 1; *Cashflow Volatility*, the standard deviation of *Cash Flows* from years *t* – 4 to *t*, with a minimum of three observations; *Financing*, an indicator of firms with an increase in common stock, preferred stock, and debt greater than one percent of beginning-of-year total assets; *Prior Absolute Accruals*, the absolute value of total accruals divided by beginning-of-year total assets in year *t* – 1; and *IFRS*, an indicator of financial statements prepared under the IFRS. The following audit-related control variables are included: *Qualified Opinion*, an indicator of firms that receive a modified audit opinion; *Big N*, an indicator of Big N auditors;[8] *Initial Audit*, an indicator of firms that switched auditors in the current year; and *Log Auditor Tenure*, the natural logarithm of the number of consecutive years audited by the current auditor.

[8] The number of large auditors was six until 2004 and decreased to four after two large auditor mergers in 2005. Each of the current Big 4 auditors are members of the global Big 4 auditors: PwC, KPMG, Deloitte, and Ernst & Young.

For tests of *Qualified Opinion*, I restrict the sample to financially distressed firms to focus on firms that are more likely to have reporting issues (Chen, Martin, and Wang 2013; Chung et al. 2019; DeFond et al. 2002). Following other studies, I identify financially distressed firms as those with either negative earnings or operating cash flows. I include the same control variables as in the previous specification, except that I replace *Cash Flows* with *Return on Assets*, *Prior Absolute Accruals* with *Absolute Accruals*, and *Qualified Opinion* with *Prior Qualified Opinion*. *Return on Assets* is net income divided by beginning-of-year total assets; *Absolute Accruals* is the absolute value of total accruals divided by beginning-of-year total assets in year t; and *Prior Qualified Opinion* is an indicator of firms that received a modified audit opinion in year $t - 1$.

All continuous variables are winsorized at the top and bottom one percent to reduce the effect of outliers. I include year and industry fixed effects to control for the average characteristics of year and industry, respectively. I cluster standard errors by auditor-year because the variable of interest is calculated at that level (Petersen 2009).

3.4 Sample and data

I obtain data on Korean firms and their auditors from DataGuide and KIS-Value. Specifically, I obtain financial information and stock return data from DataGuide. I obtain audit information of public firms from DataGuide and augment the data with audit information of private firms from KIS-Value.

The sample includes the 2001 to 2018 period. The sample period starts in 2001 because the databases contain a sufficient number of private firm observations with non-missing auditor data since this year. The sample ends in 2018 because the calculation of *Accruals Error* requires data for year $t + 1$. To ensure the reliability of the results, I require each auditor to operate in at least two fiscal years and each auditor-year to have at least five clients. I retain firm-years with positive assets and sales, operating in a non-financial

industry, and having fiscal years ending in December. Only observations with non-missing control variables are retained.

The final sample comprises 109,010 private firm-year observations with 1,816 distinct auditor-year observations for the test of *Abs. Disc. Accruals*. Due to the additional data requirements, the number of observations included in the *Accruals Error* test decreases to 104,702. Further restriction of the sample to financially distressed firms in the *Qualified Opinion* tests reduces the sample to 38,306 observations.[9]

Table 1 provides the annual frequencies of observations and reveals two notable trends. First, the number of firms, especially private firms, is shown to increase significantly over time, contributing to the decreasing percentage of public firms in the economy over time. Specifically, the size-weighted percentage of public firms decreases from 32.4 percent in 2001 to 21.0 percent in 2018.

Second, the number of auditors is shown to increase significantly over time from 31 in 2001 to 154 in 2018. The increases in the numbers of private firms and auditors collectively contribute to the decrease in the size-weighted percentage of public clients in the average auditor's portfolio from 35.6 to 7.4 percent over time. The average percentage of public clients is lower for non-Big N auditors than for Big N auditors (13.3 vs. 44.6 percent). The decreasing trend in the portfolio weight on public clients is more pronounced for non-Big N than for Big N auditors, suggesting that private clients are an increasingly important source of revenue for non-Big N auditors. The supplementary use of *Ranked %Public Clients* ensures that the observed results are not driven by these time trends.

[Insert Table 1 about here]

[9] The corresponding sample size for public firms is 25,752, 25,307, and 10,786 for tests of *Abs. Disc. Accruals*, *Accruals Error*, and *Qualified Opinion*, respectively.

4. Empirical Results

4.1 Descriptive statistics

Panel A of Table 2 provides the descriptive statistics of the test and control variable statistics for the full sample used in the *Abs. Disc. Accruals* and *Accruals Error* tests. The average of the absolute value of discretionary accruals is 7.376 in the private firm sample, showing that the average private firm recognizes absolute discretionary accruals amounting to about 7.4 percent of total assets. The average *Accruals Error* is 5.913, i.e., the accruals error amounts to about 5.9 percent of the total assets. An average private firm has total assets (*Assets*) of KRW 91.5 million (approximately USD 83,000), a leverage ratio (*Leverage*) of 0.553, and operating cash flows (*Cash Flows*) that amount to 6.1 percent of total assets. Additionally, 19.4 percent of private firms recognize losses (*Loss*). Private firms, on average, experience a sales growth rate (*Sales Growth*) of 8.8 percent and cash flow volatility (*Cash Flow Volatility*) of 0.100, and 35.7 percent of them undergo external financing (*Financing*). The average private firm recognizes total accruals in the prior year with an absolute value amounting to 8.4 percent of the total assets (*Prior Absolute Accruals*). In the sample, 8.5 percent of private firms voluntarily adopt IFRS (*IFRS*), 1.8 percent receive a qualified opinion from their auditors (*Qualified Opinion*), 28.3 percent are audited by a Big N auditor (*Big N*), and 10.8 percent switch their auditor (*Initial Audit*). The average auditor tenure (*Auditor Tenure*) for private clients is 5.3 years.

Panel B of Table 2 provides the descriptive statistics of the variables used in the *Qualified Opinion* tests with a restricted sample of financially distressed firms. In this subsample, 2.8 percent of distressed firms receive a qualified opinion from their auditor (*Qualified Opinion*). Compared with the full sample, distressed firms are more highly leveraged (*Leverage*), report greater losses (*Loss*), have lower sales growth (*Sales Growth*) and more

volatile cash flows (*Cash Flow Volatility*), and are more likely to increase external financing (*Financing*).

[Insert Table 2 about here]

Table 3 presents correlations between the variables. *%Public Clients* is associated positively with *Abs. Disc. Accruals* and *Accruals Error* and negatively with *Qualified Opinion*, suggesting that a greater focus on public clients is likely to reduce the quality of audits provided to the same auditor's private firms. Higher values of *%Public Clients* are also associated with clients with larger assets (*Log Assets*), lower leverage (*Leverage*), higher profitability (*Cash Flows*, *Loss*), a higher growth rate (*Sales Growth*), and higher volatility (*Cash Flow Volatility*). *%Public Clients* also is positively associated with Big N auditors (*Big N*), initial-year audit contracts (*Initial Audits*), and auditor tenure (*Log Auditor Tenure*).

Among the three audit quality measures, *Abs. Disc. Accruals* is positively associated with *Accruals Error*, consistent with the observation that firms that recognize a high level of discretionary accruals are more likely to have accruals that less accurately predict their cash flows. Interestingly, *Qualified Opinion* is positively associated with both *Abs. Disc. Accruals* and *Accruals Error*, suggesting that clients with high discretionary accruals and larger accruals estimation error are more likely to receive a qualified opinion. As the correlations between other control variables are self-explanatory, I omit detailed explanations.

[Insert Table 3 about here]

4.2 Univariate analysis

Table 4 reports the results of a univariate analysis examining how auditors' portfolio weight on public clients affects the audit quality provided to both private and public clients. Auditor-years are first classified as those without and with public clients. Column (1) of Table 4 provides the average *%Public Clients* for the two groups, showing that the average value *%Public Clients* is

0.182 for auditor-years with at least one public client. Columns (2) to (4) compare between the two groups of auditors in terms of the audit quality provided to *private* clients. In column (2), the average *Abs. Disc. Accruals* of clients of auditors without and with at least one public client are 6.458 and 7.444, respectively, and this difference is statistically significant (difference = 0.985, *z*-statistic = 10.28), consistent with the provision of lower audit quality to private clients audited by an auditor having a public client. Similarly, in column (3), the average *Accruals Error* of clients of auditors without and with public clients are 4.932 and 5.985, respectively, and this difference is statistically significant (difference = 1.053, *z*-statistic = 19.02). However, in column (4), the probability of issuing qualified opinions is not statistically different between auditors with and without public clients (difference = 0.002, *z*-statistic = 0.65).

I further divide auditor-years with at least one public client into quartiles within each year. As reported in column (1) of Table 4, the average value of *%Public Clients* is 0.111 for the first quartile and 0.493 for the last quartile. In column (2), the average *Abs. Disc. Accruals* increases monotonically with the quartile rank, and the difference between the lowest and highest quartiles is statistically significant (difference = 0.193, *z*-statistic = 2.60). Similar findings are observed for *Accruals Error* in column (3). In column (4), *Qualified Opinion* decreases monotonically as the quartile rank increases, with a statistically significant difference between the lowest and highest quartiles (difference = – 0.013, *z*-statistic = – 5.11), suggesting that auditors are less likely to issue a qualified opinion to their private clients when their portfolio includes more public clients. Collectively, the results of univariate analysis suggest that as auditors increasingly accumulate *public* clients, the quality of audits provided to their *private* clients deteriorates.

For comparison, I provide the results of univariate analysis of public clients in columns (5) to (7) of Table 4. In column (5), I find that the average

Abs. Disc. Accruals decreases monotonically with the quartile rank, and the difference between the lowest and highest quartile is statistically significant (difference = – 3.016, *z*-statistic = – 14.37). Similar findings are observed for *Accruals Error* in columns (6). These results suggest that auditors with more public clients provide higher quality audits to their public clients, potentially due to the greater scrutiny of public client audits or because public clients are more likely to select higher-quality auditors. However, in column (7), *Qualified Opinion* decreases monotonically with increasing quartile rank, with a statistically significant difference between the lowest and highest quartiles (difference = – 0.019, *z*-statistic = – 3.33), suggesting that auditors with more public clients in their client portfolio are less likely to issue a qualified opinion to both public and private clients. Collectively, the univariate analysis of the effect of client portfolio weight on the audit quality provided to *public* clients yields mixed results.

[Insert Table 4 about here]

4.3 Multivariate analysis

Table 5 presents the results of multivariate analysis of the effect of public client portfolio weights on the audit quality provided to private clients. Columns (1) and (2) present the results of testing *Abs. Disc. Accruals*; columns (3) and (4), *Accruals Error*; and columns (5) and (6), *Qualified Opinion*. Odd and even columns use the raw value of *%Public Clients* and the decile-rank, *Ranked %Public Clients*, as the test variable, respectively.

Columns (1) and (2) of Table 5 presents the test results with *Abs. Disc. Accruals* as the dependent variable. In column (1), the coefficient on *%Public Clients* is 1.625, which is significant at the one-percent level (*t*-statistic = 5.25). Economically, a one-standard deviation increase in *%Public Clients* is associated with a 0.327 (= 1.625 × 0.201) increase in *Abs. Disc. Accruals*, corresponding to 4.4 percent of the mean value of *Abs. Disc. Accruals*. In column (2), the coefficient on *Ranked %Public Clients* is also significantly

positive (coefficient = 0.626, *t*-statistic = 5.28), ensuring that the results are not an artifact of time trends.

Columns (3) and (4) present the results of testing *Accruals Quality*. Consistent with the results in the first two columns, the coefficients on both the raw and ranked test variables are significantly positive. For example, in column (3), the coefficient on *%Public Clients* is 1.151 and significant at the one-percent level (*t*-statistic = 5.99). Economically, a one-standard deviation increase in *%Public Clients* is associated with a 0.231 (= 1.151 × 0.201) increase in *Accruals Quality*, corresponding to 3.9 percent of the mean value of *Accruals Quality*.

Columns (5) and (6) provide the results of testing *Qualified Opinion*. In column (5), the coefficient on *%Public Clients* is – 0.021 and significant at the five-percent level (*t*-statistic = – 2.45). This suggests that a one-standard deviation increase in *%Public Clients* is associated with a 0.4 percent (= – 0.021× 0.201) decrease in the probability that the auditor will issue a qualified opinion, which corresponds to 14 percent of the unconditional probability that a financially distressed firm will receive a qualified opinion (2.8 percent). The analysis using *Ranked %Public Clients*, shown in column (6), yields consistent results. Collectively, the results in Table 5 consistently provide evidence that auditors exhibit a decrease in audit quality as their client portfolios become more heavily weighted on public clients. Thus, auditors sacrifice the quality of audits provided to less regulated private clients when they are required to allocate more resources to more regulated public clients.

The results of the control variables are consistent with those reported in the literature. Specifically, lower audit quality is observed for firms that are smaller (*Log Assets*), are more highly leveraged (*Leverage*), report losses (*Loss*), have volatile sales or cash flows (*Sales Growth*, *Cash Flows*), and receive a qualified opinion (*Qualified Opinion*). In columns (3) and (4), the coefficient on *IFRS* is significantly positive, consistent with studies that argue

that the benefits of IFRS on financial reporting quality depend on the country's environment (Cameran, Campa, and Pettinicchio 2014; Daske, Leuz, Hail, and Verdi 2008). The coefficient on *Big N* is insignificant in five out of six columns, consistent with studies that fail to find evidence that large auditors provide benefits to private firms.[10]

[Insert Table 5 about here]

As an alternative explanation for the observed findings, private firms may intentionally select lower quality auditors to meet reporting requirements in a cost-effective manner (Chaney et al. 2004). In addition, the heavy workload imposed by more public client audits may reduce the average quality of audits provided to all clients (Bills et al. 2016; López and Peters 2012; Persellin et al. 2019). To test this possibility, I examine the association between the audit quality provided to public clients and the auditor's portfolio weight on public clients. If private clients intentionally seek lower quality auditors, then the selected auditors should also provide lower quality audits to their public clients.

The results reported in Table 6 are strikingly different from those reported in Table 5. The coefficients on *Abs. Disc. Accruals* in columns (1) and (2) and *Qualified Opinions* in columns (5) and (6) are insignificant. Moreover, the coefficients on *Accruals Error* in columns (3) and (4) are significantly negative, suggesting that auditors with greater weight on public clients provide *higher* quality audits to their *public* clients than do auditors with less weight on public clients. Taken together, an auditor with a greater

[10] As private firms face lower litigation risks than public firms (St. Pierre and Anderson 1984), Big N auditors face less incentives to provide high quality audits to private firms. For example, Kim, Simunic, Stein, and Yi (2011) find that although voluntary audits provide considerable value to private firms, the choice to hire a Big N auditor over a non-Big 4 auditor does not lead to an incrementally greater reduction in the cost of debt among voluntarily audited private firms. Examining U.S. firms, Fortin and Pittman (2007) show that bond spreads do not decrease and credit ratings do not improve for private firms that employ a Big 4 auditor compared to those that employ a non-Big 4 auditor. In addition, Chaney et al. (2004) find no fee premium for Big 4 auditors in the private market sector, suggesting that private firms do not perceive differences in the audit quality provided by Big 4 and non-Big 4 auditors.

portfolio weight on public clients does not provide lower audit quality to these *public* clients. Therefore, the lower audit quality provided by such auditors to *private* clients is unlikely to be attributable to private clients' selection of lower quality auditors.

[Insert Table 6 about here]

4.4 Exogenous shock to the client portfolio

In 2016, a massive accounting fraud was revealed by Daewoo Shipbuilding & Marine Co., Ltd. (DSME), a major player in the Korean shipbuilding industry. DSME had managed their cost figures from 2012 to 2014 to inflate their reported earnings. After a criminal investigation, DSME's auditor, Deloitte Anjin LLC (hereafter, "Deloitte"), was penalized with a ban restricting the auditor from making new audit contracts with public firms in the year 2017. This ban created a significant shock to Deloitte's portfolio. As Deloitte was not able to attract new public clients, the percentage of public clients in its portfolio significantly decreased from 45.1 percent in 2016 to 39.4 percent in 2017. In contrast, the corresponding figures for the other three Big 4 auditors increased from 40.6 to 43.2 percent, and those of non-Big 4 auditors decreased from 7.1 to 6.7 percent in the same period. The decrease in public clients would have reduced the amount of resources consumed by Deloitte's public clients and thus increased its capacity for the audits of its private clients. Therefore, I expect the the quality of Deloitte's audits provided to private clients in 2017 to have increased.

To examine the impact of this exogenous shock, I use a difference-in-differences research design. Focusing on the years 2016 and 2017, I compare the pre- and post-shock changes in the audit quality provided to Deloitte's clients to those provided to clients of other auditors. Specifically, I use the following model:

$$AuditQuality_{i,t} = \beta_1 Deloitte + \beta_2 Post + \beta_3 Deloitte \times Post + \beta Controls_{i,t} + \delta Industry_j + \varepsilon_{i,t}. \quad (5)$$

where *Deloitte* is an indicator variable for the clients audited by Deloitte, and *Post* is an indicator variable of the post-penalty period, i.e., year 2017. The control variables are identical to those included in equation (4). If Deloitte provided higher audit quality to its private clients after the ban, I would expect the coefficient on *Deloitte* × *Post* (i.e., β_3) to be negative for *Abs. Disc. Accruals* and *Accruals Error* and positive for *Qualified Opinion*.

Table 7 presents the results of estimating the difference-in-differences analysis. Columns (1) to (3) present the results of testing the audit quality for private clients. In column (1) [(3)] using *Abs. Disc. Accruals* [*Qualified Opinion*] as the dependent variable, the coefficient on *Deloitte* × *Post* is significantly negative [positive], suggesting that Deloitte provided higher audit quality to its private clients in the post-penalty period than in the pre-penalty period, and this increase in audit quality is larger than that of other auditors in the corresponding period. The results in column (2), using *Accruals Error* as the dependent variable, have the predicted signs but are insignificant. Collectively, the results are consistent with Deloitte putting greater effort into the audits of private firms in its portfolio after the shock that decreased its portfolio weight on public clients.[11]

To examine whether the increase in audit quality is due to the disciplinary effect of the criminal investigation on the auditor's overall quality, I examine how the audit quality provided to *public* clients changed during the same period. In columns (4) to (6), the coefficients on *Deloitte* × *Post* are insignificant in columns (4) and (5). The results suggest that the increase in audit quality is not necessarily due to the average increase in audit quality for

[11] The significantly positive coefficient on Deloitte, indicating lower audit quality of Deloitte compared to other auditors, is potentially due to the larger percentage of public clients in its portfolio than that of other auditors in 2016.

all clients but rather to a selective increase in audit quality for a subset of clients, namely private firms. Overall, the findings further support the negative spillover effect of regulated clients on unregulated clients within an auditor's portfolio.

[Insert Table 7 about here]

5. Additional Analysis

5.1 Strategic resource allocation

5.1.1 Resource adjustment costs

I hypothesize that regulated clients affect the quality of audits provided to the auditors' unregulated clients either through resource allocation or knowledge spillover effects. In the following section, I attempt to distinguish these effects.

To examine whether auditors' strategic allocation of resources drives the findings, I examine the moderating effect of resource adjustment costs. If resource adjustment costs are low, then the auditor can simply acquire more resources, i.e., hire more personnel, to meet increased audit requirements, rather than shifting resources between clients. Therefore, I expect the main findings to be more pronounced when the auditor faces higher resource adjustment costs. I use three proxies for resource adjustment costs: the volatility of the auditor's client portfolio, the number of auditors per partner, and audit market competition.[12] Frequent changes in the client portfolio increase the auditor's resource adjustment costs. An insufficient number of staff-level auditors increases the likelihood that resources will be shifted from one client to another. Moreover, in a climate of fierce competition in the audit market, auditors find it difficult to hire competent personnel and thus incur high recruiting costs. Using these proxies, I bisect the sample and compare the impact of portfolio weights on the audit quality provided to private clients.

[12] Information on the number of auditors per partner is obtained from the auditors' annual reports manually collected up to 2015.

Panels A, B, and C of Table 8 provide the results of analyses respectively using the portfolio volatility, the number of auditors per partner, and audit market competition as proxies for resource adjustment costs. In all panels, columns (1) to (3) represent auditors with higher resource adjustment costs, and columns (4) to (6) represent those with lower resource adjustment costs. In Panel A of Table 8, although the signs of the coefficients on *%Public Clients* are consistent with the main findings, the coefficient magnitudes are larger for auditors with a volatile client portfolio, as reported in columns (1) to (3). The differences in coefficient magnitudes between the high and low resource adjustment cost subsamples are statistically significant for *Accruals Error* and *Qualified Opinion*. In Panel B of Table 8, the coefficient on *%Public Clients* is significantly positive only when auditors have less sufficient human capital to employ, reported in columns (1) and (2), but not when auditors have greater flexibility in human resources as reported in columns (4) to (6). Panel C of Table 8 report similar findings. Collectively, the results show that the observed findings are more pronounced when the auditor faces higher resource adjustment costs.

[Insert Table 8 about here]

5.1.2 Increase in requirements on public firm audits

I next examine whether regulatory shocks to certain clients aggravate the reduction in audit quality provided to the auditors' other clients. I first evaluate whether public firms' adoption of the IFRS magnified the negative spillover effect of auditors' public firm clients on their private clients. The IFRS, a principle-based standard, requires the auditor to exert greater professional judgment and thus increased effort during audits (Kim et al. 2012). The requirement to adopt the IFRS was first imposed on Korean public firms in 2011, and most public firms began preparing for IFRS-financial statements beginning in 2010 to ensure comparative financial reporting. Given the increased burden on auditors who handle public firms since the

adoption of IFRS, I expect that this event is associated with an increase in the negative effect of shifting resources from private to public clients on audit quality.

Panel A of Table 9 examines the main findings for the pre- and post-IFRS periods. In the pre-IFRS period, reported in columns (1) to (3), the coefficient on *%Public Clients* is significant with the expected sign only in column (1) that examines *Abs*. *Disc*. Accruals. In contrast, in the post-IFRS period, reported in columns (4) to (6), the coefficients in all columns have significant coefficients in the expected direction. The coefficient on *Accruals Error* is significantly different between the two periods. The results are weakly consistent with the expectation that an increase in public firm audit requirements increases the negative spillover effects on private firm audits.

Next, I use an indicator for auditors with a client that has recently undergone an initial public offering (IPO). A firm's IPO process is strictly regulated; therefore, the role of auditors is highly valued in the IPO market (Beatty 1989; Menon and Williams 1991). I expect that an auditor with a newly listed client would need to allocate greater resources to this client, resulting in a more severe decrease in the audit quality provided to other clients. The variable *IPO Clients* takes a value of 1 if a client becomes public in year t or $t - 1$ and 0 otherwise. I use this variable as an alternative test variable. In Panel B of Table 9, the coefficients on *IPO Clients* are significantly positive in columns (1) and (2) and significantly negative in column (3), consistent with the increased shifting of resources to meet the increased burden of a newly listed client.

[Insert Table 9 about here]

5.2 Knowledge spillover

5.2.1 Industry membership

Because public firms are subject to stricter audit requirements, a higher portfolio weight on public clients may have synergistic effects on the quality

of audits provided to an auditor's private clients. Such knowledge spillover may be more pronounced within the industry because much of the knowledge held by auditors is industry-specific (e.g., Reichelt and Wang 2010). To examine this issue, I separately examine the effect of portfolio weight on public clients on the quality provided to private clients according to whether both clients share industry membership. Specifically, for each auditor *a* and industry *j*, *%Public Clients in Same Industry* is the size-weighted number of auditor *a*'s public clients within industry *j* divided by the size-weighted number of auditor *a*'s public and private clients within industry *j*. In addition, I calculate the percentage of public clients outside the industry: *%Public* Clients *in Different Industry* is the size-weighted number of auditor *a*'s public clients outside industry *j* divided by the size-weighted number of auditor *a*'s public and private clients outside industry *j*.

$$\%Public\ Clients\ in\ Same\ Industry_{a,j} = \frac{\sum_{k=j,public}\sqrt{Assets_{i,k}}}{\sum_{k=j,public}\sqrt{Assets_{i,k}}+\sum_{k=j,private}\sqrt{Assets_{i,k}}}. \quad (5)$$

$$\%Public\ Clients\ in\ Different\ Industry_{a,k} = \frac{\sum_{k\neq j,public}\sqrt{Assets_{i,k}}}{\sum_{k\neq j,public}\sqrt{Assets_{i,k}}+\sum_{k\neq j,private}\sqrt{Assets_{i,k}}}. \quad (6)$$

The mean values of *%Public Clients in Same Industry* and *%Public Clients in Different Industry* are 0.239 and 0.254 percent, respectively (untabulated).[13]

Panel A of Table 10 provides the results of analyses using the two disaggregated measures. The coefficients on *%Public Clients in Different*

[13] Alternatively, I decompose the numerator of *%Public Clients* into public clients within and outside industry *k*, dividing both variables by all clients of the auditor. Using this approach produces qualitatively similar results. Although this approach may be more straightforward, I do not report this result because doing so creates only limited variation in the former variable. Specifically, using this approach, *%Public Clients in Same Industry* has a mean value of 0.010, with a standard deviation of 0.016, whereas *%Public Clients in Different Industry* has a mean value of 0.243 and a standard deviation of 0.158. Thus, it is not clear whether the insignificant coefficient on *%Public Clients in Same Industry* is due to the insignificant effect of same industry membership or a small variation in the variable that creates statistically insignificant results.

Industry are consistent with previous results: significantly positive coefficients for *Abs. Disc. Accruals* and *Accruals Error* and a significantly negative coefficient for *Qualified Opinion*. The results suggest that an auditor with more public clients outside the focal firm's industry is more likely to sacrifice the audit quality provided to the focal client. In contrast, the coefficients on *%Public Clients in Same Industry* are insignificant in the first two columns and significantly positive in the last column.[14] The results show that the negative impact of resource constraints is muted or even reversed when the public and private clients are in the same industry, consistent with the existence of knowledge spillover from public to private clients that share industry membership.

5.2.2 Voluntary adoption of IFRS

Private firms are allowed to prepare their financial statements in accordance with the K-GAAP in principle. However, they may also choose to voluntarily adopt the IFRS. In the latter case, auditors can benefit from knowledge spillover from public firms to private firms as both types of clients share the same accounting standards. Thus, I examine whether the reduction in private-client audit quality is weaker for IFRS-adopting private firms than for non-adopting firms.

Panel B of Table 9 limits the sample to the post-IFRS period and adds an interaction term between *%Public Clients* and *IFRS* in the regression. In columns (1) and (2), the coefficient on the interaction term is significantly negative, suggesting that the negative impact of public clients on the quality of private clients' audits is reduced. In all three columns, the sum of *%Public Clients* and *%Public Clients* × *IFRS* is statistically insignificant. This result suggests that when private and public firms share the same accounting

[14] When using ranked variables, the coefficients on *Ranked %Public Clients in Same Industry* becomes significantly positive in all columns.

standards, knowledge spillover offsets the negative impact of auditors' redirection of resources away from private firms.

[Insert Table 10 about here]

5.3 Auditor type

I next examine the effect of public client audits on private client audit quality differs depending on auditor type. I expect the quality of audits provided by Big 4 auditors to private clients to be less affected by the importance of public clients in the portfolio than the quality provided by non-Big 4 auditors due to differences in exposures to resource constraints and knowledge spillover. Che, Hope, and Langli (2020) find that Big 4 auditors provide higher audit quality than non-Big 4 auditors because the former are able to recruit more talented partners, offer enhanced training programs, and monitor engagements more strictly. Each of these three aspects is expected to affect the impact of regulated clients on unregulated audit engagements. For example, if Big 4 auditors can easily recruit human resources, then they will incur smaller resource allocation costs and have less incentive to allocate resources away from unregulated clients. In addition, their extensive training programs can facilitate the transfer of knowledge from regulated to unregulated clients' engagements. Lastly, stricter monitoring can help remediate any impairments in audit quality caused by resource constraints.

Table 11 presents the results of a subsample analysis for private clients of non-Big N and Big N auditors. Columns (1) to (3) represent clients of non-Big N auditors, and columns (4) to (6) represent those of Big N auditors. The signs and significance levels of the coefficients on *%Public Clients* of Table 5 are retained only in the subsample of non-Big N auditors, reported in columns (1) to (3) of Table 11. For the clients of Big N auditors, the coefficients on *%Public Clients* in all columns are insignificant and smaller in magnitude than those in the non-Big N subsample. Specifically, for *Abs. Disc. Accruals*, the coefficient on *%Public Clients* is significantly

positive in column (1) (coefficient = 1.458, *t*-statistic = 4.39) but insignificant in column (4) (coefficient = 0.761, *t*-statistic = 0.64), suggesting that only non-Big N auditors sacrifice the quality of their private firm audits when their portfolio weight on public clients increases. In contrast, the quality of audits provided by Big N auditors to private firms does not depend on their client portfolio weights, consistent with Big N auditors having effective quality control systems that limit the negative portfolio-level spillover effect (Che et al. 2020).[15]

[Insert Table 11 about here]

5.4 Robustness tests

I perform additional tests to examine the robustness of the results. First, I use alternative identification strategies for auditors' distractions. I use a dummy variable, *Zero Public Clients*, that identifies auditors without any public clients. If a greater focus on public clients impairs the audit quality of the auditors for private clients, then auditors with no public clients would exhibit higher audit quality. In the first three columns of Panel A, Table 12, the coefficients on *Zero Public Clients* are significantly negative for *Abs. Disc. Accruals* and *Accruals Error*, and insignificant for *Qualified Opinion*. The results provide some evidence that auditors exhibit higher audit quality for private clients when they are able to focus solely on the clients without the distraction of more regulated public firms.

Second, I alternatively measure *%Public Clients* with different weights. Specifically, I weigh clients in the portfolio with the square root of clients' sales rather than assets. In addition, I assign equal weights to all clients. Panel B of Table 12 reports the test results, which are all consistent with the main test results.

[15] Although the magnitudes and significances of the coefficients on *%Public Clients* are smaller in the Big N subsample than in the non-Big N subsample for all three dependent variables, the differences are statistically insignificant. This lack of significance is potentially attributable to the smaller variation in *%Public Clients* for Big N auditors, which increases the standard error of the coefficient estimates.

To examine whether within-auditor changes in portfolio weights affect the audit quality of private clients, I additionally control for auditor-fixed effects in the first three columns of Panel C, Table 12. The results show that the coefficients on *%Public Clients* remain significant for *Abs. Disc. Accruals* and *Qualified Opinion*, but becomes insignificant for *Accruals Error*. Thus, I find some support that not only across-auditor but also within-auditor changes in the client portfolio affects the auditors' quality.

Lastly, to test whether unobserved firm policies drive the results, I control for firm-fixed effects and report the results in the first three columns of Panel C, Table 12. I find that the results on *Abs. Disc. Accruals* and *Accruals Error* remain robust, while the statistical significance for *Qualified Opinions* disappear, suggesting that the findings are not driven by firm-effects.

[Insert Table 12 about here]

6. Conclusion

In this study, I find that auditors whose client portfolios are tilted toward public clients provide lower quality audits to their *private* clients but not to their *public* clients. The study thus provides evidence of auditors' strategic allocation of their limited resources. Specifically, an auditor may strategically choose to provide lower quality audits to unregulated clients, when they have to meet the stricter audit requirements of regulated clients. These results can inform discussions of the benefits and costs of audit regulations and contribute to the literature on the unequal effort made by economic agents.

Disclosure regulations typically target a certain segment of the economy, e.g., public firms, with the aim of enhancing the transparency of the targeted segment. Although firms in other segments, e.g., private firms, may not be directly affected by these regulations, they may be indirectly affected via links with the targeted firms through common information intermediaries, e.g., auditors. Therefore, regulators must consider this unintended externality of regulations.

Despite the novelty and contributions of the study, I mention a couple of caveats. First, although the unique setting of Korea allows examination of auditors' resource allocation decisions, the use of Korean data may limit the generalizability of the results to other countries. Second, I draw inferences about auditors' resource allocation based on auditors' output, i.e., audit quality, rather than their input, i.e., audit effort. An examination of how auditors expend different levels of effort on audits of private and public firms would provide more direct evidence of auditors' resource allocation. I believe this is an important avenue for future research.

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Appendix. Variable Definitions

Variables	Definitions
Test variables	
%Public Clients	The auditor's portfolio weight on public clients, defined as the aggregate size of public clients divided by the aggregate size of all clients, where size is the square-root of total assets;
Ranked %Public Clients	The decile rank of *%Public Clients* within each fiscal year;
Dependent variables	
Abs. Disc. Accruals	The absolute value of residuals of the modified Jones' (1991) model, multiplied by 100;
Accruals Error	The standard deviation of residuals of the modified Dechow and Dichev's (2002) model from year $t - 4$ to t, requiring at least three non-missing observations, multiplied by 100;
Qualified Opinion	An indicator variable that equals 1 if the audit opinion is modified and 0 otherwise;
Control variables	
Log Assets	The natural logarithm of total assets in KRW;
Leverage	Total debt divided by total assets;
Cash Flows	Cash flows from operations divided by total assets;
Return on Assets	Net income divided by beginning-of-year total assets;
Loss	An indicator variable that equals 1 if net income is negative and 0 otherwise;
Sales Growth	The percentage growth in sales from year $t - 1$ to t;
Cash Flow Volatility	The standard deviation of cash flows divided by beginning-of-year total assets from year $t - 4$ to t, requiring at least three non-missing observations;
Financing	An indicator variable that equals 1 if the change in capital in the current year is greater than one percent of beginning-of-year total assets, where the change in capital is the sum of common stock, preferred stock, and total debt, and 0 otherwise;
Prior Absolute Accruals	The absolute value of total accruals in year $t - 1$, where total accruals are net income less cash flows from operations;
Absolute Accruals	The absolute value of total accruals in year t, where total accruals are net income less cash flows from operations;
IFRS	An indicator variable that equals 1 if the financial statements are prepared in accordance with the international financial reporting standards and 0 otherwise;
Prior Qualified Opinion	An indicator variable that equals 1 if the audit opinion in year $t - 1$ is modified and 0 otherwise;
Big N	An indicator variable that equals 1 if the firm is audited by a Big N auditor and 0 otherwise;
Initial Audits	An indicator variable that equals 1 if the firm switches its auditor in year $t + 1$ and 0 otherwise;
Log Auditor Tenure	The natural logarithm of the number of consecutive years audited by the current auditor;

(continued on the next page)

Additional variables (in order of appearance)	
Deloitte	An indicator variable that equals 1 if the firm is audited by Deloitte and 0 otherwise;
Post	An indicator variable that equals 1 if the fiscal year is in the post-penalty period, i.e., 2017, and 0 otherwise;
Volatile (Stable) client portfolio	Auditors whose size-weighted portfolio from years $t - 4$ to t is larger (smaller) than the first quartile value;
Low (High) CPAs-to-partner	Auditors whose number of certified public accountants divided by the number of audit partners is below (above) the median value;
High (Low) competition	Industry-years with concentration ratio, i.e., the Herfindahl-Hirshman index measured by the asset-weighted market share of auditors, below (above) the median value;
Pre- (Post-) IFRS	Fiscal years before (since) the IFRS transition period of 2010;
IPO Clients	An indicator variable that equals 1 if the auditor has at least one client that has undergone an initial public offering in years $t - 1$ or t and 0 otherwise;
%Public Clients in Same Industry (Different Industries)	For a firm in industry j, the auditor's within-industry (out-of-industry) portfolio weight on public clients, defined as the aggregate size of public clients (not) in industry j divided by the aggregate size of all clients (not) in industry j, where size is the square-root of total assets;
Zero Public Clients	An indicator variable that equals 1 if the auditor has no public clients in its portfolio and 0 otherwise.

Table 1. Sample Frequency

This table provides the annual frequency of observations used in the study. Columns (1), (2), and (3) present the number of all firms, private firms, and public firms, respectively, each year. Column (4) presents the percentage of public firms, value weighted by the square root of total assets. Column (5) presents the number of auditors each year. Columns (6), (7), and (8) present the size-weighted percentage of public firms in an average auditor's client portfolio for all auditors, non-Big N auditors, and Big N auditors, respectively.

	Firm-year observations				Auditor-year observations			
						%Public Clients		
Year	All firms (1)	Private firms (2)	Public firms (3)	%Public (4)	Observations (5)	All (6)	Non-Big N (7)	Big N (8)
2001	2,489	1,684	805	32.4%	31	35.6%	32.9%	49.7%
2002	3,179	2,176	1,003	28.1%	38	33.8%	31.7%	47.1%
2003	3,487	2,253	1,234	28.6%	50	29.0%	27.0%	46.6%
2004	3,800	2,550	1,250	26.5%	55	26.5%	24.7%	43.6%
2005	4,439	3,175	1,264	25.5%	68	24.8%	23.7%	43.3%
2006	4,604	3,249	1,355	24.7%	80	22.5%	21.4%	42.4%
2007	5,113	3,675	1,438	21.8%	86	19.6%	18.5%	42.2%
2008	5,534	4,128	1,406	21.6%	93	19.0%	17.9%	42.7%
2009	6,076	4,612	1,464	22.3%	101	18.9%	17.9%	44.6%
2010	7,191	5,717	1,474	22.3%	108	15.5%	14.4%	45.0%
2011	7,728	6,210	1,518	24.3%	118	13.6%	12.4%	47.6%
2012	8,508	6,974	1,534	23.9%	124	12.5%	11.4%	46.1%
2013	9,092	7,538	1,554	22.7%	126	11.9%	10.9%	44.8%
2014	9,824	8,267	1,557	22.4%	129	10.4%	9.3%	45.0%
2015	10,812	9,159	1,653	21.8%	141	8.3%	7.3%	42.5%
2016	13,684	11,981	1,703	21.2%	151	8.1%	7.1%	41.7%
2017	14,386	12,623	1,763	21.0%	163	7.6%	6.7%	42.2%
2018	14,816	13,039	1,777	21.0%	154	7.4%	6.5%	43.5%
	134,762	109,010	25,752	22.8%	1,816	14.6%	13.3%	44.6%

Table 2. Descriptive Statistics

This table presents the summary statistics of the test variables. Panel A presents the summary statistics for the full sample used for testing *Abs. Disc. Accruals* and *Accruals Error*. Panel B presents those for the restricted sample of distressed firms, i.e., firms with negative earnings or cash flows, used for testing *Qualified Opinions*. Detailed variable definitions are provided in the Appendix.

Panel A. Discretionary accruals and accruals error sample (n = 109,010)

	(1) Mean	(2) Std. Dev.	(3) P1	(4) Q1	(5) Median	(6) Q3	(7) P99
%Public Clients	0.231	0.161	0.000	0.088	0.201	0.406	0.523
Ranked %Public Clients	0.485	0.306	0.000	0.200	0.500	0.700	1.000
Abs. Disc. Accruals	7.376	8.372	0.074	2.034	4.675	9.410	47.130
Accruals Error (n =104,702)	5.913	4.843	0.606	2.618	4.492	7.547	25.720
Assets (in KRW mil.)	91.500	292.000	8.108	18.600	30.900	63.900	1220.000
Log Assets	17.470	1.027	15.910	16.740	17.250	17.970	20.920
Leverage	0.553	0.243	0.040	0.371	0.580	0.742	0.977
Cash Flows	0.061	0.120	-0.319	0.001	0.050	0.117	0.465
Loss	0.194	0.396	0.000	0.000	0.000	0.000	1.000
Sales Growth	0.088	0.387	-0.765	-0.069	0.037	0.167	2.332
Cash Flow Volatility	0.100	0.097	0.006	0.041	0.072	0.122	0.604
Financing	0.357	0.479	0.000	0.000	0.000	1.000	1.000
Prior Absolute Accruals	0.084	0.095	0.001	0.024	0.054	0.108	0.538
IFRS	0.085	0.279	0.000	0.000	0.000	0.000	1.000
Qualified Opinion	0.018	0.133	0.000	0.000	0.000	0.000	1.000
Big N	0.283	0.451	0.000	0.000	0.000	1.000	1.000
Initial Audits	0.108	0.310	0.000	0.000	0.000	0.000	1.000
Auditor Tenure (Raw)	5.302	3.719	1.000	3.000	4.000	7.000	17.000
Log Auditor Tenure	1.415	0.743	0.000	1.099	1.386	1.946	2.833

Panel B. Qualified opinion sample (n = 38,306)

	(1) Mean	(2) Std. Dev.	(3) P1	(4) Q1	(5) Median	(6) Q3	(7) P99
%Public Clients	0.214	0.158	0.000	0.074	0.186	0.378	0.515
Ranked %Public Clients	0.464	0.304	0.000	0.200	0.400	0.700	1.000
Qualified Opinion	0.028	0.164	0.000	0.000	0.000	0.000	1.000
Assets (in KRW mil.)	82.300	257.000	8.108	17.800	30.000	61.100	935.000
Log Assets	17.420	1.009	15.910	16.690	17.220	17.930	20.660
Leverage	0.623	0.244	0.040	0.464	0.664	0.814	0.977
Return on Assets	-0.012	0.092	-0.356	-0.043	-0.004	0.025	0.277
Loss	0.553	0.497	0.000	0.000	1.000	1.000	1.000
Sales Growth	0.042	0.455	-0.765	-0.150	-0.010	0.137	2.332
Cash Flow Volatility	0.112	0.108	0.006	0.044	0.080	0.139	0.604
Financing	0.501	0.500	0.000	0.000	1.000	1.000	1.000
Absolute Accruals	0.109	0.112	0.001	0.036	0.073	0.140	0.554
IFRS	0.079	0.270	0.000	0.000	0.000	0.000	1.000
Prior Qualified Opinion	0.026	0.160	0.000	0.000	0.000	0.000	1.000
Big N	0.242	0.428	0.000	0.000	0.000	0.000	1.000
Initial Audits	0.118	0.323	0.000	0.000	0.000	0.000	1.000
Auditor Tenure (Raw)	5.096	3.632	1.000	2.000	4.000	7.000	17.000
Log Auditor Tenure	1.372	0.746	0.000	0.693	1.386	1.946	2.833

Table 3. Correlations

This table reports Pearson correlations between variables. Detailed variable definitions are provided in the Appendix. Figures in bold represent statistical significance at the two-tailed, five percent level.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
(1) *%Public Clients*	1.000														
(2) *Abs. Disc. Accruals*	**0.036**	1.000													
(3) *Accruals Error*	**0.113**	**0.267**	1.000												
(4) *Qualified Opinion*	**-0.010**	**0.032**	**0.052**	1.000											
(5) *Log Assets*	**0.280**	**-0.010**	**-0.020**	**-0.030**	1.000										
(6) *Leverage*	**-0.130**	**0.033**	0.005	**0.053**	**-0.040**	1.000									
(7) *Cash Flows*	**0.100**	0.000	**0.010**	**-0.020**	**0.021**	**-0.160**	1.000								
(8) *Loss*	**-0.060**	0.004	**0.041**	**0.075**	**-0.020**	**0.217**	**-0.250**	1.000							
(9) *Sales Growth*	**0.051**	**0.116**	**0.074**	**-0.020**	**0.080**	**0.073**	**0.111**	**-0.110**	1.000						
(10)*Cash Flow Volatility*	**0.042**	**0.467**	**0.413**	0.000	0.000	**0.022**	**0.039**	**0.000**	**0.119**	1.000					
(11)*Financing*	**-0.010**	**0.096**	**0.029**	0.000	**0.052**	**0.204**	**-0.250**	**0.063**	**0.057**	**0.058**	1.000				
(12)*Prior Absolute Accruals*	**0.042**	**0.324**	**0.285**	**0.027**	0.000	**0.047**	**0.014**	**0.010**	**0.082**	**0.486**	**0.047**	1.000			
(13)*IFRS*	**0.226**	**0.000**	0.001	**-0.010**	**0.262**	**-0.050**	**0.030**	**0.016**	**0.027**	0.000	**0.000**	0.003	1.000		
(14)*Big N*	**0.819**	**0.023**	**0.073**	**-0.010**	**0.317**	**-0.120**	**0.102**	**-0.040**	**0.043**	**0.033**	**-0.040**	**0.029**	**0.272**	1.000	
(15)*Initial Audits*	**0.033**	**0.039**	**0.053**	**0.045**	**0.019**	**0.012**	**0.000**	**0.027**	**0.019**	**0.034**	**0.017**	**0.049**	**0.075**	**0.038**	1.000
(16)*Log Auditor Tenure*	**0.045**	**-0.060**	**-0.100**	**-0.050**	**0.114**	**-0.060**	**0.013**	**-0.040**	**-0.030**	**-0.080**	**-0.020**	**-0.070**	**-0.060**	**0.034**	**-0.660**

Table 4. Univariate Analysis

This table presents the results of a univariate analysis. Auditors are classified based on the portfolio weights on public clients, *%Public Clients*. Zero [0] represents auditors with no public clients in the fiscal year. Auditors with non-zero public clients are quartile-ranked within the fiscal year based on the size-weighted percentage of public clients in the portfolio. Column (1) presents the average *%Public Clients* at the auditor-year level. Columns (2) to (4) present the average values of *Abs. Disc. Accruals*, *Accruals Error*, and *Qualified Opinion* of private firms, and columns (5) to (7) present those of public firms. Detailed variable definitions are provided in the Appendix. Shaded rows present the difference in *Abs. Disc. Accruals*, *Accruals Error*, and *Qualified Opinion* of different groups of auditors, and figures in parentheses represent *z*-statistics of the differences. *** denotes statistical significance at the two-tailed 1 percent level.

	Auditors	Private firms			Public firms		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
%Public Clients	*%Public Clients*	*Abs. Disc. Accruals*	*Accruals Error*	*Qualified Opinion*	*Abs. Disc. Accruals*	*Accruals Error*	*Qualified Opinion*
Observations	1,816	109,010	104,702	38,306	25,752	25,307	10,786
Zero [0]	0.000	6.458	4.932	0.026			
Non-Zero	0.182	7.444	5.985	0.028			
Quartile 1	0.111	7.374	5.855	0.033	9.642	7.829	0.041
Quartile 2	0.206	7.417	5.940	0.029	8.531	7.265	0.038
Quartile 3	0.296	7.442	5.900	0.025	7.651	6.560	0.029
Quartile 4	0.493	7.567	6.292	0.021	6.627	5.840	0.022
Total	0.146	7.376	5.913	0.028	7.668	6.573	0.031
Non-Zero – Zero		0.985***	1.053***	0.002			
(*z*-statistics)		(10.28)	(19.02)	(0.65)			
Q4 – Q1		0.193***	0.437***	-0.013***	-3.016***	-1.989***	-0.019***
(*z*-statistics)		(2.60)	(9.91)	(-5.11)	(-14.37)	(-17.25)	(-3.33)
Q4 – Zero		1.108***	1.360***	-0.005			
(*z*-statistics)		(10.32)	(21.73)	(-1.55)			

Table 5. Audit Quality Provided to Private Clients

This table provides the ordinary least squares estimation results using the private firm sample. Detailed variable definitions are provided in the Appendix. Figures in parentheses represent *t*-statistics calculated based on standard errors clustered by auditor-year. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent level, respectively.

Client status =	Private firm					
Dependent variable =	*Abs. Disc. Accruals*		*Accruals Error*		*Qualified Opinion*	
	(1)	(2)	(3)	(4)	(5)	(6)
%Public Clients	1.625***		1.151***		-0.021**	
	(5.25)		(5.99)		(-2.45)	
Ranked %Public Clients		0.626***		0.448***		-0.008**
		(5.28)		(5.84)		(-2.15)
Log Assets	-0.262***	-0.261***	-0.135***	-0.135***	-0.000	-0.000
	(-9.21)	(-9.18)	(-8.88)	(-8.79)	(-0.44)	(-0.48)
Leverage	0.177	0.174	-0.268***	-0.270***	0.024***	0.024***
	(1.45)	(1.43)	(-3.49)	(-3.52)	(6.66)	(6.67)
Cash Flows	-0.396	-0.395	0.052	0.054		
	(-0.95)	(-0.95)	(0.34)	(0.35)		
Return on Assets					-0.192***	-0.192***
					(-11.90)	(-11.88)
Loss	0.084	0.084	0.728***	0.728***	-0.005***	-0.005***
	(1.15)	(1.14)	(16.72)	(16.71)	(-2.67)	(-2.67)
Sales Growth	1.213***	1.212***	0.293***	0.292***	-0.007***	-0.007***
	(12.24)	(12.23)	(6.13)	(6.11)	(-3.96)	(-3.95)
Cash Flow Volatility	32.537***	32.537***	17.182***	17.182***	-0.026***	-0.026***
	(52.77)	(52.78)	(58.88)	(58.87)	(-3.09)	(-3.09)

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Financing	1.166***	1.165***	0.022	0.021	-0.005***	-0.005***
	(22.62)	(22.61)	(0.64)	(0.61)	(-2.97)	(-2.97)
Prior Absolute Accruals	10.068***	10.071***	5.248***	5.251***		
	(21.87)	(21.87)	(22.05)	(22.06)		
Absolute Accruals					0.073***	0.073***
					(6.66)	(6.65)
IFRS	0.033	0.065	0.279***	0.302***	-0.001	-0.001
	(0.35)	(0.68)	(4.03)	(4.33)	(-0.40)	(-0.54)
Qualified Opinion	1.823***	1.819***	1.537***	1.534***		
	(7.44)	(7.43)	(9.96)	(9.94)		
Prior Qualified Opinion					0.525***	0.525***
					(31.53)	(31.53)
Big N Auditor	-0.013	0.087	0.061	0.129**	0.004	0.002
	(-0.13)	(1.06)	(0.95)	(2.20)	(1.48)	(1.04)
Initial Year Audits	0.329***	0.334***	-0.040	-0.037	0.011***	0.011***
	(3.21)	(3.25)	(-0.63)	(-0.59)	(2.67)	(2.65)
Log Auditor Tenure	-0.045	-0.041	-0.321***	-0.319***	-0.000	-0.000
	(-1.13)	(-1.03)	(-11.30)	(-11.20)	(-0.10)	(-0.14)
Industry fixed effects	Included	Included	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included	Included	Included
Observations	109,010	109,010	104,702	104,702	38,306	38,306
Adjusted R-squared	0.249	0.249	0.229	0.229	0.296	0.296

Table 6. Audit Quality Provided to Public Clients

This table provides the ordinary least squares estimation results using the public firm sample. Detailed variable definitions are provided in the Appendix. Figures in parentheses represent *t*-statistics calculated based on standard errors clustered by auditor-year. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent level, respectively.

Client status =	Public firm					
Dependent variable =	*Abs. Disc. Accruals*		*Accruals Error*		*Qualified Opinion*	
	(1)	(2)	(3)	(4)	(5)	(6)
%Public Clients	-0.514		-1.204***		0.000	
	(-0.59)		(-2.69)		(0.00)	
Ranked %Public Clients		-0.450		-0.580***		0.010
		(-1.35)		(-3.21)		(0.99)
Log Assets	-0.751***	-0.749***	-0.488***	-0.489***	-0.006***	-0.006***
	(-15.67)	(-15.62)	(-16.82)	(-16.76)	(-3.73)	(-3.81)
Leverage	4.060***	4.059***	1.891***	1.889***	0.078***	0.078***
	(12.74)	(12.74)	(9.98)	(9.99)	(7.30)	(7.31)
Cash Flows	-7.836***	-7.827***	-2.423***	-2.410***		
	(-9.02)	(-9.00)	(-6.67)	(-6.63)		
Return on Assets					-0.193***	-0.194***
					(-7.37)	(-7.38)
Loss	1.057***	1.059***	1.020***	1.022***	-0.019***	-0.019***
	(7.07)	(7.08)	(12.14)	(12.18)	(-5.08)	(-5.10)
Sales Growth	1.632***	1.632***	0.311***	0.313***	-0.012***	-0.012***
	(8.75)	(8.75)	(3.16)	(3.17)	(-2.78)	(-2.78)
Cash Flow Volatility	21.963***	21.964***	13.680***	13.680***	0.033	0.033
	(22.83)	(22.82)	(25.16)	(25.20)	(1.39)	(1.39)

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Financing	0.448***	0.447***	-0.142**	-0.144**	-0.009***	-0.009***
	(4.14)	(4.13)	(-2.31)	(-2.34)	(-2.73)	(-2.72)
Prior Absolute Accruals	10.910***	10.903***	7.420***	7.409***		
	(12.63)	(12.62)	(18.97)	(18.98)		
Absolute Accruals					0.129***	0.129***
					(5.62)	(5.62)
IFRS	-0.191	-0.188	0.065	0.066	-0.176***	-0.176***
	(-0.18)	(-0.18)	(0.14)	(0.14)	(-3.62)	(-3.63)
Qualified Opinion	6.754***	6.756***	1.203***	1.206***		
	(8.60)	(8.60)	(3.37)	(3.38)		
Prior Qualified Opinion					0.310***	0.310***
					(8.16)	(8.16)
Big N Auditor	0.055	0.132	0.049	0.041	-0.001	-0.005
	(0.25)	(0.76)	(0.45)	(0.44)	(-0.24)	(-1.10)
Initial Year Audits	0.538**	0.536**	-0.177	-0.180	-0.003	-0.003
	(2.56)	(2.55)	(-1.50)	(-1.52)	(-0.44)	(-0.44)
Log Auditor Tenure	-0.037	-0.037	-0.385***	-0.389***	0.001	0.001
	(-0.39)	(-0.39)	(-6.71)	(-6.76)	(0.31)	(0.26)
Industry fixed effects	Included	Included	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included	Included	Included
Observations	25,752	25,752	25,307	25,307	10,785	10,785
Adjusted R-squared	0.264	0.264	0.294	0.294	0.135	0.135

Table 7. Difference-in-Differences Analysis using an Exogenous Shock to Deloitte's Portfolio

This table provides the results of a difference-in-differences analysis of the effect of an exogenous shock to Deloitte's portfolio in 2017 on the audit quality provided by Deloitte. Columns (1) to (3) represent private clients, and columns (4) to (6) represent public clients. Detailed variable definitions are provided in the Appendix. Figures in parentheses represent *t*-statistics calculated based on standard errors clustered by auditor-year. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent level, respectively.

Client status =	Private firm			Public firm		
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable =	*Abs. Disc. Accruals*	*Accruals Error*	*Qualified Opinion*	*Abs. Disc. Accruals*	*Accruals Error*	*Qualified Opinion*
Deloitte	0.775***	-0.002	-0.014***	-0.835***	0.038	0.005
	(9.85)	(-0.01)	(-2.78)	(-3.74)	(0.26)	(0.75)
Post	-0.094	0.034	0.009**	-0.368	-0.100	0.005
	(-1.02)	(0.45)	(2.53)	(-1.39)	(-0.73)	(0.63)
Deloitte × Post	-0.551***	-0.044	0.010***	0.109	-0.052	0.050***
	(-5.92)	(-0.57)	(2.71)	(0.38)	(-0.30)	(4.28)
Log Assets	-0.401***	-0.146***	-0.000	-0.772***	-0.569***	0.002
	(-6.68)	(-4.29)	(-0.17)	(-5.02)	(-8.89)	(0.45)
Leverage	-0.087	0.016	0.036***	3.379***	2.643***	0.039
	(-0.38)	(0.11)	(4.88)	(3.67)	(5.90)	(1.58)
Cash Flows	1.373	0.150		-9.636***	-3.452***	
	(1.45)	(0.49)		(-3.14)	(-3.24)	
Return on Assets			-0.204***			-0.281***
			(-6.39)			(-3.53)
Loss	0.319**	0.648***	-0.003	0.094	0.614**	-0.021**
	(2.23)	(7.32)	(-0.70)	(0.28)	(2.56)	(-2.34)

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Sales Growth	0.750***	0.263***	-0.010***	1.949***	0.902***	-0.014
	(3.64)	(2.90)	(-2.87)	(3.25)	(4.05)	(-0.98)
Cash Flow Volatility	36.725***	18.175***	-0.030*	23.593***	13.035***	-0.043
	(30.34)	(31.64)	(-1.72)	(7.27)	(8.51)	(-1.06)
Financing	1.242***	0.133*	-0.011***	0.792***	0.052	-0.011
	(11.48)	(1.89)	(-2.69)	(2.80)	(0.38)	(-1.45)
Prior Absolute Accruals	9.234***	5.387***		8.625***	9.161***	
	(8.83)	(9.88)		(4.09)	(7.80)	
Absolute Accruals			0.094***			0.153**
			(3.77)			(2.28)
IFRS	0.359*	0.220	-0.001	-6.261***	-2.472***	
	(1.80)	(1.33)	(-0.23)	(-4.80)	(-4.16)	
Qualified Opinion	2.586***	2.139***		7.840***	0.359	
	(4.52)	(5.69)		(3.83)	(0.40)	
Prior Qualified Opinion			0.550***			0.308**
			(19.55)			(2.29)
Big N Auditor	0.440***	0.611***	-0.001	-0.029	-0.217	-0.006
	(4.16)	(3.39)	(-0.12)	(-0.11)	(-1.30)	(-0.77)
Initial Year Audits	0.383*	0.208*	0.014	1.230***	-0.459*	0.008
	(1.81)	(1.73)	(1.63)	(2.93)	(-1.78)	(0.50)
Log Auditor Tenure	-0.005	-0.324***	-0.002	0.434**	-0.562***	0.007
	(-0.07)	(-6.62)	(-1.12)	(2.40)	(-5.02)	(1.04)
Industry fixed effects	Included	Included	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included	Included	Included
Observations	24,604	23,156	9,222	3,466	3,407	1,379
Adjusted R-squared	0.287	0.233	0.278	0.225	0.304	0.096

Table 8. Resource Adjustment Costs

This table provides the ordinary least squares estimation results using the private firm sample. In Panel A, volatile (stable) client portfolio refers to auditors with the five-year standard deviation of the sum of clients' assets from years $t-4$ to t above (below) the first quartile value each year. In Panel B, low (high) CPAs-to-partner refers to auditors whose ratio of the number of certified public accountants over the number of partners is below (above) the median value each year. In Panel C, high (low) competition refers to industry-years with the auditors' market concentration is below (above) the median value. Detailed variable definitions are provided in the Appendix. In all panels, figures in parentheses represent *t*-statistics calculated based on standard errors clustered by auditor-year. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent level, respectively.

Panel A. Portfolio volatility

Client status =	Private firm					
	Volatile client portfolio			Stable client portfolio		
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable =	*Abs. Disc. Accruals*	*Accruals Error*	*Qualified Opinion*	*Abs. Disc. Accruals*	*Accruals Error*	*Qualified Opinion*
%Public Clients	2.065***	1.398***	-0.040***	1.152**	0.533*	-0.001
	(4.32)	(5.08)	(-3.27)	(2.34)	(1.81)	(-0.09)
Test statistic				[(4) – (1)]	[(5) – (2)]	[(6) – (3)]
Coefficient difference				-0.912	-0.865**	0.039*
				(-1.33)	(-2.15)	(1.88)
Control variables	Included	Included	Included	Included	Included	Included
Industry fixed effects	Included	Included	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included	Included	Included
Observations	77,721	74,726	26,882	29,710	28,476	10,877
Adjusted R-squared	0.248	0.230	0.297	0.252	0.227	0.287

Panel B. Ratio of individual auditors to partners

Client status =	Private firm					
	Low CPAs-to-partner			High CPAs-to-partner		
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable =	*Abs. Disc. Accruals*	*Accruals Error*	*Qualified Opinion*	*Abs. Disc. Accruals*	*Accruals Error*	*Qualified Opinion*
%Public Clients	1.249***	1.079***	0.004	0.441	0.175	-0.016
	(2.74)	(3.57)	(0.28)	(0.48)	(0.39)	(-0.66)
Test statistic				[(4) – (1)]	[(5) – (2)]	[(6) – (3)]
Coefficient difference				-0.808	-0.904*	-0.020
				(-0.79)	(-1.68)	(-0.71)
Control variables	Included	Included	Included	Included	Included	Included
Industry fixed effects	Included	Included	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included	Included	Included
Observations	38,037	36,832	13,212	29,139	28,418	9,228
Adjusted R-squared	0.243	0.207	0.297	0.213	0.236	0.281

Panel C. Audit market competition

Client status =	Private firm					
	High competition			Low competition		
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable =	*Abs. Disc. Accruals*	*Accruals Error*	*Qualified Opinion*	*Abs. Disc. Accruals*	*Accruals Error*	*Qualified Opinion*
%Public Clients	1.530***	1.756***	-0.041***	1.664***	0.070	0.006
	(3.67)	(6.63)	(-3.56)	(3.83)	(0.27)	(0.41)
Test statistic				[(4) – (1)]	[(5) – (2)]	[(6) – (3)]
Coefficient difference				0.134	-1.686***	0.046***
				(0.23)	(-4.56)	(2.60)
Control variables	Included	Included	Included	Included	Included	Included
Industry fixed effects	Included	Included	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included	Included	Included
Observations	60,031	57,224	22,676	48,979	47,478	15,630
Adjusted R-squared	0.282	0.220	0.293	0.210	0.237	0.302

Table 9. Shocks to Audit Requirements of Public Clients

This table provides the ordinary least squares estimation results using the private firm sample. In Panel A, the first three columns represent the sample period before the mandatory IFRS adoption and the last three columns, after the mandatory IFRS adoption. In Panel B, *IPO Clients* is an indicator for auditor-years with at least one client that has gone public within the recent two years. Detailed variable definitions are provided in the Appendix. In both panels, figures in parentheses represent *t*-statistics calculated based on standard errors clustered by auditor-year. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent level, respectively.

Panel A. Mandatory IFRS adoption of public firms

Client status =	Private firm					
	Pre-IFRS			Post-IFRS		
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable =	*Abs. Disc. Accruals*	*Accruals Error*	*Qualified Opinion*	*Abs. Disc. Accruals*	*Accruals Error*	*Qualified Opinion*
%Public Clients	1.605***	0.199	-0.014	1.358***	1.327***	-0.019*
	(2.65)	(0.50)	(-0.80)	(3.63)	(5.77)	(-1.83)
Test statistic				[(4) – (1)]	[(5) – (2)]	[(6) – (3)]
Coefficient difference				-0.247	1.128**	-0.005
				(-0.35)	(2.47)	(-0.25)
Control variables	Included	Included	Included	Included	Included	Included
Industry fixed effects	Included	Included	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included	Included	Included
Observations	27,502	26,692	8,469	81,508	78,010	29,836
Adjusted R-squared	0.233	0.214	0.287	0.255	0.218	0.299

Panel B. Initial public offering of client

Client status =	Private firm		
	(1)	(2)	(3)
Dependent variable =	*Abs. Disc. Accruals*	*Accruals Error*	*Qualified Opinion*
IPO Clients	0.183***	0.120***	-0.003*
	(3.26)	(3.33)	(-1.66)
Control variables	Included	Included	Included
Industry fixed effects	Included	Included	Included
Year fixed effects	Included	Included	Included
Observations	109,010	104,702	38,306
Adjusted R-squared	0.249	0.229	0.296

Table 10. Knowledge Spillover

This table provides the ordinary least squares estimation results using the private firm sample. In Panel B, the sample size is restricted to the post-IFRS period. Detailed variable definitions are provided in the Appendix. In all panels, figures in parentheses represent *t*-statistics calculated based on standard errors clustered by auditor-year. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent level, respectively.

Panel A. Industry membership

Client status =	Private firm		
	(1)	(2)	(3)
Dependent variable =	*Abs. Disc. Accruals*	*Accruals Error*	*Qualified Opinion*
%Public Clients in Same Industry	0.160	-0.004	0.014**
	(1.14)	(-0.04)	(2.50)
%Public Clients in Different Industries	1.514***	1.114***	-0.028***
	(4.76)	(5.65)	(-3.27)
Test statistic			
%Public Clients in Same Industry – %Public Clients in Different Industries	-0.086***	1.118***	-0.042***
(*F*-statistics)	(12.62)	(23.14)	(13.73)
Control variables	Included	Included	Included
Industry fixed effects	Included	Included	Included
Year fixed effects	Included	Included	Included
Observations	109,009	104,701	38,305
Adjusted R-squared	0.249	0.229	0.296

Panel B. Voluntary IFRS adoption

Client status =	Private firm		
	(1)	(2)	(3)
Dependent variable =	*Abs. Disc. Accruals*	*Accruals Error*	*Qualified Opinion*
%Public Clients	1.393***	1.364***	-0.019*
	(3.73)	(5.93)	(-1.83)
IFRS	0.624***	0.893***	-0.002
	(2.95)	(6.28)	(-0.28)
%Public Clients × IFRS	-1.962***	-2.078***	0.002
	(-3.44)	(-5.04)	(0.11)
Test statistic			
%Public Clients + %Public Clients × IFRS	-0.569	-0.714	-0.017
(*F*-statistics)	(0.70)	(2.33)	(0.80)
Control variables	Included	Included	Included
Industry fixed effects	Included	Included	Included
Year fixed effects	Included	Included	Included
Observations	81,508	78,010	29,836
Adjusted R-squared	0.255	0.219	0.299

Table 11. Auditor Type

This table provides the ordinary least squares estimation results using the private firm sample. Columns (1) to (3) represent clients of non-Big N auditors, and columns (4) to (6) represent clients of Big N auditors. Detailed variable definitions are provided in the Appendix. Figures in parentheses represent *t*-statistics calculated based on standard errors clustered by auditor-year. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent level, respectively.

Client status =	Private firm					
Auditor type =	Non-Big N			Big N		
	(1)	(2)	(3)	(4)	(5)	(6)
Dep. variable =	*Abs. Disc. Accruals*	*Accruals Error*	*Qualified Opinion*	*Abs. Disc. Accruals*	*Accruals Error*	*Qualified Opinion*
%Public Clients	1.458***	1.133***	-0.020**	0.761	0.413	0.016
	(4.39)	(5.41)	(-2.10)	(0.64)	(0.58)	(0.37)
Log Assets	-0.324***	-0.168***	-0.001	-0.256***	-0.087***	0.001
	(-9.06)	(-7.53)	(-1.02)	(-5.88)	(-3.98)	(0.61)
Leverage	-0.152	-0.253***	0.027***	1.239***	-0.281*	0.011
	(-1.15)	(-2.89)	(6.46)	(5.36)	(-1.76)	(1.63)
Cash Flows	-1.263**	-0.178		1.459**	0.453*	
	(-2.45)	(-0.94)		(2.27)	(1.75)	
Return on Assets			-0.225***			-0.110***
			(-11.34)			(-4.01)
Loss	-0.066	0.710***	-0.007***	0.550***	0.805***	0.001
	(-0.80)	(13.98)	(-3.10)	(3.66)	(9.89)	(0.20)
Sales Growth	1.087***	0.283***	-0.007***	1.607***	0.336***	-0.007**
	(9.49)	(4.94)	(-3.18)	(7.98)	(3.91)	(-2.31)
Cash Flow Volatility	34.613***	17.446***	-0.030***	27.284***	16.516***	-0.010
	(50.40)	(57.19)	(-2.89)	(25.70)	(26.25)	(-0.82)

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Financing	1.206***	0.063*	-0.007***	0.977***	-0.082	0.001
	(20.52)	(1.81)	(-3.45)	(9.33)	(-1.01)	(0.41)
Prior Absolute Accruals	9.428***	5.409***		10.724***	4.573***	
	(16.49)	(18.38)		(15.02)	(12.44)	
Absolute Accruals			0.086***			0.044**
			(6.45)			(2.26)
IFRS	0.323**	0.716***	0.001	-0.154	-0.017	-0.000
	(2.23)	(7.76)	(0.17)	(-1.23)	(-0.23)	(-0.03)
Qualified Opinion	2.057***	1.846***		0.929*	0.613**	
	(7.54)	(10.43)		(1.81)	(2.17)	
Prior Qualified Opinion			0.518***			0.549***
			(29.00)			(12.75)
Big N Auditor	0.225*	-0.033	0.008	0.671***	-0.064	0.018***
	(1.79)	(-0.43)	(1.57)	(3.85)	(-0.55)	(3.07)
Initial Year Audits	-0.150***	-0.356***	-0.001	0.206***	-0.257***	0.002
	(-3.24)	(-10.81)	(-0.61)	(3.21)	(-5.10)	(1.28)
Log Auditor Tenure	1.458***	1.133***	-0.020**	0.761	0.413	0.016
	(4.39)	(5.41)	(-2.10)	(0.64)	(0.58)	(0.37)
Test statistic				[(4) – (1)]	[(5) – (2)]	[(6) – (3)]
Coefficient difference				-0.697	-0.720	0.036
				(-0.57)	(-0.98)	(0.83)
Industry fixed effects	Included	Included	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included	Included	Included
Observations	78,106	74,676	29,038	30,903	30,026	9,267
Adjusted R-squared	0.264	0.225	0.297	0.217	0.234	0.294

Table 12. Robustness Tests

This table provides the robustness test results using the private firm sample. Panel A presents the results of using alternative test variables. *Zero Public Clients* is an indicator for auditor-years with only private clients. Panel B presents the results measuring *%Public Clients* that are sales-weighted and equal-weighted. Panel C presents the results additionally controlling for auditor-fixed effects in columns (1) to (3) and firm-fixed effects in columns (4) to (6). Detailed variable definitions are provided in the Appendix. In all panels, figures in parentheses represent *t*-statistics calculated based on standard errors clustered by auditor-year. ***, **, and * denote statistical significance at the two-tailed 1, 5, and 10 percent level, respectively.

Panel A. Alternative test variables

Firm status =	Private firm		
	(1)	(2)	(3)
Dependent variable =	*Abs. Disc. Accruals*	*Accruals Error*	*Qualified Opinion*
Zero Public Clients	-0.427***	-0.404***	-0.002
	(-4.16)	(-5.28)	(-0.56)
Control variables	Included	Included	Included
Industry fixed effects	Included	Included	Included
Year fixed effects	Included	Included	Included
Observations	109,010	104,702	38,306
Adjusted R-squared	0.249	0.229	0.295

Panel B. Alternative portfolio weights

Firm status =	Private firm					
Portfolio weights =	Square-root of sales			Equal weights		
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable =	*Abs. Disc. Accruals*	*Accruals Error*	*Qualified Opinion*	*Abs. Disc. Accruals*	*Accruals Error*	*Qualified Opinion*
%Public Clients	1.480***	1.003***	-0.017**	2.237***	1.442***	-0.028*
	(4.90)	(5.17)	(-2.02)	(4.48)	(4.37)	(-1.90)
Control variables	Included	Included	Included	Included	Included	Included
Industry fixed effects	Included	Included	Included	Included	Included	Included
Year fixed effects	Included	Included	Included	Included	Included	Included
Observations	109,010	104,702	38,306	109,010	104,702	38,306
Adjusted R-squared	0.249	0.229	0.296	0.249	0.229	0.296

Panel C. Alternative fixed effects

Firm status =	Private firm					
	Auditor-fixed effects			Firm-fixed effects		
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable =	*Abs. Disc. Accruals*	*Accruals Error*	*Qualified Opinion*	*Abs. Disc. Accruals*	*Accruals Error*	*Qualified Opinion*
%Public Clients	2.004***	0.494	-0.042***	0.886**	0.743***	-0.017
	(3.86)	(1.51)	(-2.66)	(1.98)	(3.03)	(-1.07)
Log Assets	-0.259***	-0.139***	-0.000	-0.898***	-0.734***	-0.006
	(-9.05)	(-9.13)	(-0.46)	(-8.09)	(-12.56)	(-1.60)
Leverage	0.269**	-0.154**	0.025***	2.648***	0.880***	0.061***
	(2.23)	(-2.09)	(6.87)	(8.53)	(6.10)	(6.58)
Cash Flows	-0.348	0.105		-1.020**	0.177	
	(-0.84)	(0.69)		(-2.10)	(1.42)	
Return on Assets			-0.188***			-0.154***
			(-11.53)			(-7.39)
Loss	0.120	0.775***	-0.004**	-0.057	0.407***	-0.003
	(1.64)	(18.13)	(-2.22)	(-0.67)	(11.20)	(-0.99)
Sales Growth	1.205***	0.282***	-0.007***	1.183***	0.061	-0.004**
	(12.15)	(5.94)	(-3.93)	(12.02)	(1.63)	(-2.09)
Cash Flow Volatility	32.402***	16.962***	-0.029***	28.802***	13.463***	-0.032**
	(52.75)	(58.29)	(-3.45)	(33.48)	(39.22)	(-2.00)
Financing	1.136***	-0.006	-0.006***	1.108***	0.010	-0.005***
	(22.26)	(-0.18)	(-3.32)	(19.71)	(0.39)	(-2.60)

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Prior Absolute Accruals	9.881***	5.156***		-4.309***	2.297***	
	(21.49)	(21.83)		(-7.64)	(12.22)	
Absolute Accruals			0.071***			0.067***
			(6.43)			(5.71)
IFRS	0.025	0.275***	-0.001	0.041	0.137	-0.001
	(0.26)	(3.84)	(-0.25)	(0.26)	(1.58)	(-0.27)
Qualified Opinion	1.798***	1.483***		1.767***	0.801***	
	(7.32)	(9.57)		(6.00)	(4.92)	
Prior Qualified Opinion			0.524***			0.263***
			(31.50)			(12.01)
Big N Auditor				-0.089	-0.147*	0.019***
				(-0.62)	(-1.92)	(3.67)
Initial Year Audits	0.304***	-0.052	0.011***	0.479***	-0.116**	0.009**
	(2.93)	(-0.82)	(2.87)	(4.25)	(-2.16)	(2.06)
Log Auditor Tenure	-0.057	-0.314***	0.000	0.076	-0.275***	-0.000
	(-1.35)	(-10.90)	(0.36)	(1.38)	(-9.12)	(-0.14)
Control variables	Included	Included	Included	Included	Included	Included
Auditor fixed effects	Included	Included	Included	-	-	-
Firm fixed effects	-	-	-	Included	Included	Included
Industry fixed effects	Included	Included	Included	-	-	-
Year fixed effects	Included	Included	Included	Included	Included	Included
Observations	109,010	104,702	38,306	105,909	102,086	33,824
Adjusted R-squared	0.250	0.233	0.297	0.307	0.573	0.411

국문 초록

감사계약과 규제에 관한 연구

본 학위논문은 감사계약과 규제에 관한 두 개의 독립적인 논문으로 구성되어 있다. 첫 번째 논문에서는 더 유리한 감사의견을 얻기 위해 감사인을 교체하는 감사의견 구매 행동이 국가별 법적 강제력에 따라 어떻게 달라지는지 분석한다. 48개 국가의 자료를 이용하여 실증분석을 수행한 결과 국가별 법적 강제력이 강해질수록 감사고객의 감사의견 구매 행동이 더욱 빈번해짐을 발견하였다. 추가 분석을 통해 감사의견 구매 행동이 증가하는데 기여하는 두 가지 요인을 식별하였다. 먼저 법적 강제력이 강해질수록 감사의견이 변형될 때 주식시장에서 부정적으로 반응하는 정도가 커져 피감기업이 감사의견의 변형을 회피하기 위해 감사의견 구매 행동을 할 유인이 커진다. 또한 법적 강제력이 강해질수록 대형감사인과 중소형감사인 사이의 품질 차이가 커져 피감기업이 중소형감사인으로 감사인을 교체하여 감사의견 구매 행동의 효과성을 높일 수 있다. 강한 법적 강제력 하에서 감사의견 구매 행동이 증가하는 현상은 대형감사인에서 중소형감사인으로의 감사인 교체에서 더욱 두드러지며, 차기 연도에 더 낮은 감사품질로 이어져 기업이 기회주의적인 동기를 가지고 감사의견 구매 행동을 하는 것으로 해석된다. 강한 법적 강제력이 기회주의적인 감사의견 구매 행동을 오히려 증가시킨다는 본 연구의 발견은 강한 법적 강제력의 긍정적인 효과에 집중된 기존의 선행연구의 결론과는 상반되며, 법적 강제력이 자본시장의 정보환경에 미치는 효과에 대해 새로운 시각을 제시한다.

두 번째 논문에서는 규제로 인한 감사인의 전략적 자원배분에 대해 다룬다. 동일한 감사인이 강한 규제의 대상이 되는 기업('규제기업')과 그렇지 않은 기업('비규제기업')을 동시에 감사고객으로 보유하는 경우, 감사인이 제한된 자원을 규제기업의 감사에 집중적으로 투입함으로 인해 비규제기업의 감사품질을 저해시킬 수 있다. 그러나 동시에 감사인이 규제기업의 감사에서 습득한 지식을 비규제기업의 감사 업무에 활용한다면 비규제기업의 감사품질이 향상될 수 있다. 본 연구에서는 감사고객의 상장여부를 규제 강도의 지표로 활용하여 감사인의 고객 포트폴리오 내에서 발생하는 규제의 외부효과를 분석한다. 국내 기업 자료를 이용한 실증분석 결과, 상장기업 고객의 비중이 큰 감사인은 비상장기업에 대해서만 더 낮은 품질의 감사를 제공한다는 사실을 발견하였다. 이러한 현상은 자원 조정 비용이 큰 감사인에게서 더 두드러져, 해당 결과가 감사인의 전략적 자원 배분으로 인한 효과임을 보여준다. 한편 감사인이 규제기업의 감사로부터 얻은 지식이 비규제기업의 감사에 활용될 수 있는 경우 자원 배분으로 인한 부정적인 효과가 상쇄된다는 증거도 발견하였다. 본 연구의 결과는 특정 집단에 강한 규제를 부과하는 것이 다른 집단에 부정적인 외부효과를 가져올 수 있음을 보여준다. 따라서 자본시장에서 정보의 공시와 외부감사 관련 규제를 설정할 때에는 수반되는 비용과 효익을 포괄적으로 분석하는 것이 중요하다는 것을 강조한다.

주제어: 감사계약; 감사보고서; 감사의견구매; 감사인; 감사품질; 규제; 법적 강제력; 비상장기업.

학 번: 2016 – 30167