

The impact of revealing auditor partner quality: evidence from a long panel

C. S. Agnes Cheng

The Hong Kong Polytechnic University, Hong Kong
afagnes@polyu.edu.hk

Kun Wang

Tsinghua University, China
wangk@sem.tsinghua.edu.cn

Yanping Xu

Jinan University, China
xuyanping@jnu.edu.cn

Ning Zhang

Queen's University, Canada
nz8@queensu.ca

Abstract

We examine whether the revelation of individual audit partner reputation affects client firms' external financing choice. Specifically, we investigate whether a firm switches its financing choices once its auditor partner is perceived to be a low-quality partner, captured by whether one of the audit partner's other clients is sanctioned for financial misreporting. We identify firms audited by a low-quality partner as the treatment firms and designate firms audited by other audit partners from the same audit office as the control firms. Using a long panel of data with audit partner identity, we find that, on average, the treatment firm switches from equity financing to credit financing after the discovery of individual audit partner quality. In addition, reduced equity financing is primarily concentrated among firms that choose to keep low-quality partners. By building an implicit link between the non-sanctioned firm and the sanctioned firm through a common audit partner, we show that investors can infer the quality of external audits using the auditor-level information, thus empirically supporting to the new PCAOB rule that requires disclosure of the partner-level information.

Keywords Audit · Individual audit partner · External financing · Equity investors

Acknowledgments We thank the editor and an anonymous reviewer for very helpful comments. We thank Michael Welker, Bohui Zhang, and seminar participants at Chinese University of Hong Kong (Shenzhen) for suggestions. Zhang and Cheng acknowledge financial supports from the Commerce '83 Fellowship at Queen's University and Hong Kong Polytechnic University, respectively. Cheng and Wang acknowledge financial support from the Smith School of Business for their visits to Queen's University. Kun Wang acknowledges financial support from the National Natural Science Foundation of China (Project 71372048). Yanping Xu acknowledges financial support from the National Natural Science Foundation of China (Project 71802092), the Enterprise Transformation Research Team Project of the Institute for Enterprise Development and School of Management, Jinan University, Guangdong Province and Jinan University Management School Funding Program, No. GY18002.

1 Introduction

We examine whether the revelation of individual audit partner reputation affects client firms' external financing choices. Specifically, we investigate whether a firm switches its financing choice once its auditor partner is perceived to be of poor quality, captured by whether one of the auditor partner's other clients is sanctioned for financial misreporting. To the extent that external financing is essential for a firm's growth and external financing is costly, when compared to using internally generated funds, our study highlights the impact of external audit partner quality on firms' financing decisions.

Our research question is motivated by the recent academic interest in understanding the capital market consequences of individual audit partner profiles. For example, Gul et al. (2013) document that, due to differences in individual-level characteristics, such as education, work experience, and so forth, individual audit partners exhibit considerable variation in audit quality. Li et al. (2017) show that the "contagion effect" of audit quality at the office level (e.g., at the city level) is due to the "individual effect" in that individual auditors carry over "individual fixed effects" between audit engagements. Studies generally focus on the stock market reactions at the partner level (Abodia et al. 2015; Li et al. 2017). Our study examines the effect of individual audit partner quality on firms' financing choices—a *real* corporate decision. We focus on firms that do not misreport themselves but are audited by auditors who are later identified as low quality. This investigation is important for several reasons. Managers and stakeholders generally have imperfect signals, at best, to assess the quality of their audit partners. The sanction can serve as a direct means through which stakeholders infer the quality of individual auditors as well as the quality of the financial audits performed by these auditors. To the extent that information asymmetry between insiders and outsiders hinges on the quality of audited financial reports, we investigate whether firms alter their financing choices when the audit partner is perceived to be of poor quality due to financial misreporting at the auditor partner's *other* client firms.

Our study is also motivated by the recent change in the Public Company Accounting Oversight Board's (PCAOB's) rules requiring the disclosure of the engagement partner and other audit firms participating in an audit. After many years of extensive discussions and debates, on May 10, 2016, PCAOB announced a new regulation. Under the new regime, audit firms are required to file with the PCAOB on a new form (i.e., Form AP) the names of the engagement partner for all public company audits issued on or after January 31, 2017.¹ The purpose of this rule is to increase transparency and enhance the perceived reliability of financial reports. With the individual auditor partner names disclosed, investors will have greater ability to track the quality of audit engagements performed by individual partners. We illuminate the economic implications of this policy by providing evidence as to whether different types of capital providers can recognize the quality of individual audit partners using a long panel data.

To capture the quality of individual auditor partners, we employ a list of audit partners whose client firms are involved in regulatory sanctions due to financial

¹ Information about other audit firms participating in the audit must also be filed for all public company audits issued on or after June 30, 2017. See the full text at <https://pcaobus.org/News/Releases/Pages/SEC-approves-transparency-Form-AP-051016.aspx>.

misreporting. When misreporting of a firm is discovered and sanctioned by the regulator, the sanction event conveys a negative signal about the quality of the responsible auditor partner, raising concerns with his or her *other* clients.² Thus we identify an audit partner to be a low-quality partner (LQP) if (at least) one of his or her client firms is sanctioned. We investigate whether the event causes the low-quality audit partner's non-sanctioned clients (i.e., "innocent" firms that are not sanctioned) to alter their financing choices. To the extent that equity investors rely primarily on publicly disclosed financial reports to understand operations and coordinate investments (Leuz and Verrecchia 2004; Kedia et al. 2018), equity investors of the non-sanctioned clients perceive heightened information asymmetry due to financial reports audited by the LQP and thus assess a higher cost of equity capital.

In contrast, debt holders have a different payoff structure and normally have better expertise in screening the information risks. Unlike equity investors, debt holders could acquire information about their borrowers via private communication channels to supplement information from publicly released audited financial statements. Theory shows that information asymmetry between lenders and the manager is lower than that between equity investors and the manager (Diamond 1991). Yet another type of debt, particularly important and popular in emerging markets, is loans from listed companies' related parties (e.g., controlling shareholders or firms under control). Related-party transactions between listed firms and their controlling shareholders are frequent and substantial (Jian and Wong 2010; Cheung et al. 2009). One of their findings is that the listed companies provide loans to controlling shareholders when they have raised capital from stock market (often referred to it as tunneling) and controlling shareholders offer loans to listed companies when listed companies have trouble raising capital from other sources (referred to as propping). Debt financing through related parties is less contingent on audited public information. Instead, private information may well substitute public information in shaping related party transactions. Taken together, we hypothesize that treatment firms, as a response, revise their external financing by using more debt financing than equity financing after their audit partner is discovered to be of low quality.

Notwithstanding of our main prediction, it is also likely the sanction has no impact on the firms audited by LQP for two reasons. First, the sanction per se does not send a *direct* signal about the treatment firm's financial reporting. Investors may attribute misreporting solely to the sanctioned firm but not draw inferences about the quality of the auditor partner. Second, capital providers may be more concerned about office-level quality even when partner-level information is available. For example, Doxey et al. (2018) document that the partner-level disclosure does not result in significant abnormal trading volumes and numbers of trades, suggesting that partner information is less informative to investors. If this is the case, we expect that client firms audited by low-quality partners will not respond to the sanction events. Ultimately, it is an open empirical question to ascertain the (spillover) impact of auditor partner quality.

² From time to time, Chinese regulators examine the outcomes of audit engagements and audit failures are publicly announced to the public by government sanctions. Similarly, in the United States, the Government Accountability Office (GAO, formerly named the General Accounting Office) has conducted two waves of investigations and identified a list of financial restatements that occurred from 1997 to 2001 and from 2002 to 2006, respectively.

We test our hypothesis using the Chinese setting for several reasons. First, China is one of very few countries that requires the disclosure of individual audit partners' names on audited financial reports for a reasonably long span of time. Unlike the United States, where only office-level auditor identity could be identified before PCAOB's new rule, in China, the names of signatory auditors are publicly available on annual reports.³ Second, many studies using individual audit partner-level data employ the Chinese setting (e.g., Chen et al. 2010; Gul et al. 2013; Wang et al. 2015; Li et al. 2017). As such, the sample in our study shares very similar country-specific institutions and regulations with those of the previous related studies, and it is easy to compare and extend our research framework to their studies. Third, firms that are involved in financial misreporting can be clearly identified, as the China Securities Regulatory Commission (CSRC) publicly publishes regulatory sanctions. These cases are similar to those in the Accounting and Auditing Enforcement Releases (AAERs) in the United States. Hence the sanction setting provides a powerful identification for an LQP that failed to detect financial irregularities. As such, we take advantage of the laboratory in China with a long panel of auditor partner data to examine the effect of individual auditor partner reputation.

We define an audit partner as an LQP if one of the audit partner's clients is sanctioned for financial misreporting by the regulator.⁴ We designate the sanction as the triggering event to provide a signal about individual auditor partners' quality and compare whether non-sanctioned firms audited by the LQP (i.e., innocent firms that are also clients of the LQP) switch their external financing choices. To differentiate the impact of audit firm- or office-level characteristics from the impact of partner-level characteristics, we identify firms audited by a low-quality partner as the treatment firms and designate firms audited by non-low-quality audit partners from the same audit office as control firms. Using 6874 firm-years listed in the Shanghai and Shenzhen stock exchanges covering 1998 to 2015, we find that equity financing is reduced after the firm's audit partner is sanctioned.⁵ This effect is both economically meaningful and statistically significant after controlling for firm characteristics as well as year and firm fixed effects. This is consistent with the idea that equity investors perceive increased information asymmetry when they find that the firm is audited by an LQP. Meanwhile, we find that the bank loan financing or bond financing do not change. However, loans from related parties increase significantly after the sanction. We perform a battery of sensitivity tests using alternative econometric specifications and our results remain robust.

³ It is also available on the website of the China Institute of Certified Public Accountants (CICPA) at www.cicpa.org.cn.

⁴ The Chinese auditors are required to sign their audit reports in accordance with China's Independent Auditing Standard (CIAS) No.7, Audit Report, issued in 1995 (Chen et al. 2010). The CIAS requires that at least two auditors sign an audit report. Typically, two engagement auditors sign each audit report, with the review partner mainly performing review work and the engagement partner mainly conducting fieldwork. Both signing auditors have the same legal liability and are equally responsible for the reports signed (Lennox et al. 2014). Therefore, in the main results, we define the treatment firms if any of review and engagement partners is an LQP.

⁵ We need the data for external financing three years pre-sanction announcement and post-sanction announcement. In addition, the data for statements of cash flows begins in 1998 in China. As a result, our final sample covers from 1998 to 2015, while the sanction sample covers from 2001 to 2012.

To the extent that a firm can either retain the LQP or replace the incumbent with a new partner after their auditor is found to be poor quality, we next probe the differential impact of the firm's decision to keep or replace the LQP following the sanction announcement. We find that the reduced equity financing is primarily concentrated in firms that have chosen to keep the LQP or firms that have replaced the incumbent LQP with another LQP. We interpret this result as consistent with the idea that a "correctional" decision to improve information quality is vital to external financing. Firms that do not fix the information risks caused by the LQP are penalized in the market, and they have to substitute equity financing with alternative debt financing.

Our study contributes to the literature in corporate finance and accounting in several ways. First, we document the impact of individual auditor quality on financing decisions. Unlike studies that use audit firm-level indicators to proxy for audit quality (i.e., Big N versus non-Big N auditors in Chang et al. 2009), we employ more granular data to show that the impact of individual auditor matters for corporate policies. Possibly due to lack of information on individual auditor partners that actually perform each audit engagement, researchers usually treat the quality of partners within each city-level office as homogenous, though they could very well be quite distinct. To this end, our study echoes the call for research on individual-level audit quality (Francis 2011). Specifically, Francis argues that "it might be more important that [audit] input measures be reported for the individual engagement or engagement office rather than aggregated to the firm level." Exploiting the unique Chinese setting, which provides individual auditor names, our study is, to the best of our knowledge, the first to document the impact of individual auditor partners on firms' financing decisions.

Second, our research design highlights information asymmetry between stakeholders and (individual) auditors. Although studies and anecdotes imply that there is information asymmetry between auditors and shareholders, and that auditors, in some cases, can collude with managers to mislead shareholders (e.g., the Enron scandal; Moore and Scott 1989), there is little empirical evidence thus far to show how shareholders and managers react to this type of information asymmetry. Taking advantage of the individual auditor partner setting, we exogenously shock the information asymmetry between auditors and stakeholders and ascertain how firm behavior changes in response to the shock.

Third, we extend findings of Chen et al. (2013). While their study focuses on the restating firm's external financing choices after the restatement, we investigate the non-restating (i.e., in our setting, non-sanctioned) firms' external financing choices after their auditor partners are revealed to be of low quality due to audit failure at another client firm. Using the audit failure as a device to infer partner quality, we show that the audit partner's sanction has a spillover effect on the individual auditor's *other* clients. We also complement the work of Abodia et al. (2015). Using individual partner-level data in Taiwan, they document that investors have typically reacted to the perceived audit quality during earnings announcements. While their study focuses on the pricing effect of individual audit-partner quality in earnings announcements and assumes that each audit partner has a *constant* quality across the sample period, we emphasize the dynamic dimension of individual audit-

partner quality. Our approach models audit quality discovery by employing an arguably exogenous event to trigger a change in perceived audit partner quality.

Finally, our study joins the arising literature that examines managerial credibility, reputation, and reputation spillovers. For example, Benabou and Laroque (1992) analytically show that insiders (those who have information advantages) have incentives to manipulate prices through distorted announcements because the public cannot fully distinguish whether the insiders are credible. Focusing on the credibility of credit agencies, deHaan (2017) documents that, after the financial crisis, rating agencies suffer spillover reputation damage from their failed ratings on financial instruments. Focusing on the credibility of management forecasts, Ng et al. (2013) show that less credible forecasts result in more underreaction. Focusing on credibility of financial reporting, Costello and Wittenberg-Moerman (2011) shows that after a material internal control weakness, lenders reduce their use of financial numbers, due to a decrease in lenders' trust in reporting. Our study emphasizes the credibility of individual audit partners and documents how the perception of that credibility by capital providers shapes the firm's external financing choices.

The rest of the paper proceeds as follows. Section 2 discusses prior studies and develops the hypothesis. Section 3 outlines the sample selection process and the empirical strategies. Section 4 discusses the main results. Section 5 provides additional analyses, and Section 6 concludes.

2 The institutions and hypothesis development

2.1 Institutional background of the auditing and capital markets in China

The audit market in China has been expanding rapidly with the fast-growing Chinese economy over the last three decades. Audit firms were initially founded as state owned and were affiliated with local or central governments, until they disaffiliated from the government around 1998 and 1999. Since then, audit firms have been independent entities that bear full legal responsibilities for their conducts. With almost 40 years of development, audit practices in China have been converging with those of the developed markets.

A distinct feature of the audit in China is that the Chinese government requires auditors to sign their audit reports. In general, there are two signing auditors in one audit engagement. The senior auditor is often the review auditor, who reviews the job at the end. The junior auditor is the engagement auditor, who leads the audit team through the fieldwork. Both signatory auditors share the same legal liability and are accountable for the reports signed in their names (Lennox et al. 2014). The China Securities Regulatory Commission (CSRC) conducts both regular reviews and random inspections of public firms. The findings of the CSRC investigations and sanction decisions are announced publicly on its official website with detailed information regarding wrongdoing. By far, the investigation by CSRC is the most official investigation in public firms' financial reporting practices. The sanctions are also broadcast by major business news media. These cases typically involve a combination of a misstatement of revenue, income, or assets that materially changes

the financial position of the accused firm.⁶ Prior U.S.-based studies employ AAERs as a proxy for fraudulent financial reporting, indicating audit failure (e.g., Bonner et al. 1998; DeFond and Zhang 2014; Lennox and Pittman 2010). To a large extent, the Chinese regulatory sanctions resemble AAERs in the United States.

This unique signatory auditor setting provides a powerful way to study the economic consequences of individual auditor quality. Specifically, regulator-identified audit deficiencies serve as strong signals of individual partner quality. To the extent that auditors who have performed failed audits may carry over their characteristics (i.e., cognitive styles, expertise, and risk preference) to other audit engagements (Li et al. 2017), investors can infer the individual audit partner's quality from the public announcements of sanctions. In other words, the sanctions trigger the discovery of the audit-engagement quality performed by the same low-quality auditors at the non-sanctioned firms (i.e., the innocent firms).

To the extent that equity investors primarily rely on publicly disclosed financial reports to understand operations and coordinate investments (Leuz and Verrecchia 2004), equity investors perceive heightened information asymmetry due to low-quality audit performed by the LQP. In contrast, debt holders have a different payoff structure and normally have better expertise and information acquisition strategies than equity investors. For example, banks privately communicate with borrowers to acquire information about default risks and information risks (Chen 2016; Vashishtha 2014). In addition, debt holders also protect themselves via other means, such as shortening maturities, requiring more collateral, and tightening covenants (Graham et al. 2008). Compared with equity investors that are either unwilling to supply capital or demand a higher rate of return stemming from the heightened information asymmetry, the discovery of an LQP has less impact on the perceived information risks for debt holders.

In addition to traditional financing through banks and bonds, in China, related parties of a public firm may also supply capital. Controlling shareholders or other related firms offer inter-company loans to a public firm when the public firm has exhausted its ability to draw on financing from other sources (commonly referred to as propping). These corporate insiders have their own way of knowing what is going on with the firm and may simply bypass published financial reports. Propping substitutes equity financing when public equity investors start to lose confidence in management when they know the external auditor partner is of low quality.

Notwithstanding of our main prediction, it is also likely the sanction event at another firm has no impact on the focal firm's financing. This is because the sanction event per se does not send a direct signal about the treatment firm's financial reporting. Investors may attribute the sanction event solely due to the sanctioned firm, but not to the auditor partner. As such, it is an open empirical question to ascertain the spillover impact of audit partner quality. We state our main hypothesis (in the conceptual-level) as follows.

H1: Individual audit partner quality is a driver in determining external financing.

⁶ For example, on December 21, 2004, Hefei Fengle Seed Co., Ltd., was sanctioned by CSRC for financial reporting fraud between 1997 and 2002. Three major issues are involved in this case: (1) failure to disclose significant security investment outflows and inflows between 1997 and 2001, (2) inflated revenues between 1997 and 2001 and inflated assets on the balance sheet between 1992 and 2002, and (3) misleading information about the use of raised funds. This information is publicly disclosed at http://www.csrc.gov.cn/pub/zjpublic/G00306212/200804/t20080418_14421.htm

Table 1 Sample Construction Procedure

Year	# of firms	# of total firms in CSMAR	Percentage
Panel A: The year breakdown of sanction events			
2001	16	1143	1.40%
2002	28	1205	2.32%
2003	21	1267	1.66%
2004	28	1356	2.06%
2005	23	1352	1.70%
2006	17	1435	1.18%
2007	22	1549	1.42%
2008	22	1603	1.37%
2009	31	1752	1.77%
2010	31	2107	1.47%
2011	38	2341	1.62%
2012	84	2470	3.40%
Total	361	19,580	
Panel B: The industry breakdown of sanction events			
Agriculture (A)	14	421	3.33%
Mining (B)	7	402	1.74%
Manufacturing (C)	233	12,008	1.94%
Energy and Water (D)	3	725	0.41%
Construction (E)	7	408	1.72%
Wholesale and Retail Trade (F)	20	1273	1.57%
Transportation (G)	7	760	0.92%
Hotels and Catering Services (H)	4	105	3.81%
IT and Computing (I)	14	848	1.65%
Financial (J)	3	287	1.05%
Real Estate (K)	19	1017	1.87%
Rental and Business Services (L)	4	212	1.89%
Scientific Research and Technology (M)	1	46	2.17%
Public Utilities (N)	4	143	2.80%
Other Service (O)	1	74	1.35%
Education (P)	0	1	0.00%
Health and Social (Q)	0	12	0.00%
Entertainment (R)	0	96	0.00%
Conglomerates (S)	20	742	2.70%
Total	361	19,580	1.84%

Table 1 (continued)

Panel C: Sample selection for treatment firms	
Filters	Number of observations
All non-sanctioned firms audited by an LQP in the year before the sanction announcements.	1623
Less:	
Firms subsequently identified as treatment the second time	(1105)
Firms without at least one year of data before and after the event	(75)
Firms without external financing activities	(28)
Firms in the financial industry	(5)
Firms without sufficient data to calculate regression variables	(3)
Total number of treatment firms	407
Panel D: Sample selection for control firms	
Filters	Number of observations
All non-sanctioned firms audited by the low-quality audit offices but not by an LQP in the year before the sanction announcements.	4038
Less:	
Firms subsequently identified as control the second time	(2577)
Firms without at least one year of data before and after the event	(628)
Firms without external financing	(45)
Firms in the financial industry	(21)
Firms without sufficient data to calculate regression variables	(36)
Total number of control firms	731

Panel A presents the year breakdown of sanction events. Panel B presents the industry breakdown of sanction events. Panel C presents the sample selection procedures for the treatment firms. Panel D presents the sample selection procedures for the control firms.

Empirically, we predict that treatment firms switch from equity financing to debt financing after the event when the sanction provides a signal that their audit partners are of low quality.

3 Sample selection and research design

3.1 Sample selection

We select our sample firms from the main boards in the Shanghai and Shenzhen stock exchanges from 1998 to 2015. We start our sample selection process by

identifying the CSRC-sanctioned firms from 2001 to 2012. We first collect a list of sanctioned firms from the CSRC's website as well as the websites of the two stock exchanges. In total, we have 361 sanction announcements related to financial reporting frauds during the sample period. We provide the year and industry breakdown of these sanction events in Panel A and Panel B of Table 1, respectively. The sanctions are more or less evenly distributed across years and industries without any particular clusters.⁷ For each sanctioned firm, we designate the sanction date as the triggering event that provides a strong signal about the auditor partner quality and designate the audit partner in the fraud-committing years as an LQP. Next, we remove all sanctioned firms from the universe to form the basis of our treatment and control firms. For each sanction event, we designate a firm as a treatment firm if at least one of the two audit partners in the year before the sanction announcement is an LQP (i.e., identify an affected firm through the linkage to the LQP).⁸

A key design issue is to differentiate the *individual-level* auditor partner's quality from the *office-level* quality. To do so, we employ the clients of a non-LQP in the same audit office as the control firms. Specifically, for each treatment firm, we designate those firms audited by non-LQP from the same audit office as control firms. To illustrate, suppose firm *A* is sanctioned by CSRC in 2008 for misreporting conducted in 2006. We then discover that company *A* in 2006 is audited by individual auditor partners *X* and *Y* in audit office *M*. Then all firms that are audited by *X* or *Y* in 2007 are identified as the treatment firms. Firms that are audited by auditor partners from office *M*, but not *X* or *Y*, in 2007 are identified as the control firms. As such, we capture how firms audited by an LQP behave differently from firms audited by a non-LQP but from the same audit office. If the difference is economically large and statistically significant, we can then attribute the difference to the impact of individual auditor partners, rather than to the impact of the office-level characteristics.

We align the event dates so that the fiscal year-end in year *t* is the first year-end after the sanction. Similar as the practice in the United States, the engagement and review partners are subject to mandatory rotation every five years. To the extent that we identify treatment firms using the auditor partner information in year *t*-1, the last year that the treatment firm can appoint the same partner is year *t*+3. In other words, the LQP will rotate out of the treatment firm by the end of year *t*+3, at the latest. As such, we investigate treatment firms' financing

⁷ The uptick in 2012 is partially due to China's steps in recent years to improve financial reporting for the public firms and align with global accounting standards. The increased number of sanctions is the result of the CSRC's initiatives to improve stock market transparency and strengthen the regulations of capital market professionals. See the full text of the CSRC 2012 annual report (English version) at http://www.csrc.gov.cn/pub/csrc_en/about/annual/201307/P020130716403852654782.pdf. In untabulated tests, results and inferences are similar if we remove sanction events in 2012.

⁸ For each LQP-revelation event, we identify treatment firms as those that have been audited by the low-quality partners in the year before the revelation event. This design is predicated on the assumption that, if a firm has recently been audited by an LQP before the revelation, investors perceive high information risks on the firm's financial statements. In untabulated tests, we alternatively identify firms as the treatment firms when they have been audited by LQP in the recent three years. Results and inferences are qualitatively similar. In particular, when we estimate external equity financing in the baseline test, the coefficient on *Treat*Post* in equation (1) is statistically significant at the 5% level.

up to year $t+3$. To form a balanced panel, we include three years before the triggering event (years $t-3$, $t-2$, and $t-1$), where the triggering event is in year t . This design leaves us with seven years for each treatment firm.

We obtain firm-level financial data and auditor information from CSMAR (China Stock Market and Accounting Research). We only keep the first sanction event if a treatment firm is identified in more than one year during the sample period.⁹ We require that a firm have at least one year of data before and after the sanction announcement event for the difference-in-differences specification. Since we are interested in external financing activities, we remove firms that do not secure external financing around the triggering event. We remove firms in the financial industry, as their financial reporting differs from that of industrial firms.¹⁰ After requiring data availability for all variables in the tests, our final sample has 6874 firm-year observations, covering years from 1998 to 2015. Specifically, we have 407 treatment firms and 731 control firms. Panels C and D of Table 1 outline the sample-selection process.

3.2 Empirical design

Our main prediction is that firms switch to debt financing from equity financing after an event that signals the poor auditor partner quality. To measure the mix of external financing, following Chen et al. (2013), we employ the percentage of external financing obtained from equity and debt as the dependent variables, respectively. Specifically, we define *Equity* as the gross amount of equity financing scaled by the total amount of financing.¹¹ Similarly, we define *Bank* and *Bond* as the gross amount of financing from bank loans and bonds, scaled by the total amount of financing, respectively. We also define *Other* as financing received from other types of sources (e.g., a firm's related parties) scaled by the total amount of financing. All financing numbers represent the gross amount of new financing. We calculate these variables using data from the statement of cash flows.

As previously discussed, we designate firms that are audited by the LQP in the year before the sanction announcement as the treatment firms. For each treatment firm, we employ the firms audited by non-LQP audit partners from the same office as control firms. We estimate the following model.

$$\begin{aligned} \text{External financing} = & \beta_0 + \beta_1 \text{Treat*Post} + \beta_2 \text{Post} + \text{Firm characteristics} \\ & + \text{Year fixed effects} + \text{Firm fixed effects} \end{aligned} \quad (1)$$

⁹ For example, if a firm has two triggering events in 2006 and 2009, respectively (i.e., the firm is audited by two distinct LQPs, the first in 2006 and the other in 2009), the years after 2006 but before 2009 are the post-event years with respect to the first event, but they constitute the pre-event years with respect to the second event. As such, the years in between are confounded by the two triggering events. If this is the case, we only keep the triggering event in 2006.

¹⁰ In addition to the difference in financial reporting, the financial industry is a highly regulated industry in China. As such, external financing for financial firms are likely to be subject to additional requirements by the regulatory bodies. Nevertheless, our results are similar if we include financial firms in the sample.

¹¹ It is likely that a firm may issue equity and repurchase shares in the same fiscal year. To the extent that we capture the mix of new financing, we use the gross amount of equity issuance, instead of the net amount.

External financing is the placeholder for *Equity*, *Bank*, *Bond*, and *Other*, respectively. *Treat* is an indicator variable that takes a value of one for treatment firms and zero for control firms. We designate the sanction as the triggering event that signals individual auditor partner quality. *Post* is an indicator variable that takes a value of one for the post period and zero otherwise. The key variable of interest is the interaction term *Treat*Post*. Essentially, eq. (1) is a standard difference-in-differences design. The standalone variable *Treat* is dropped, as we include firm fixed effects. Since we expect that firms audited by an LQP have less equity financing in the post period, we predict a negative β_1 . We also have a collapsed version of eq. (1) where we decompose the *Post* indicator into several event year indicators. Year t is the year in which the sanction event occurred, thus including part of the year after the sanction.

We include a set of firm characteristics in the regression. Specifically, following Chen et al. (2013), we include *Size* (defined as the natural logarithm of total assets), *Lev* (defined as the total liability scaled by total assets), *ROA* (defined by net income scaled by total assets), *MB* (defined as the end of fiscal year stock price times total shares outstanding scaled by total shareholders' equity), and *Loss* (defined as the indicator variable that takes a value of one if a firm-year has a negative net income and zero otherwise).¹² To control for the possibility that a firm raises capital in response to the stock price valuation (Baker et al. 2003), we include *Return*, defined as the prior 12-month cumulative stock return. To the extent that equity investors are concerned about the degree to which potential costs associated with the increased use of credit financing would make firms forego positive net present value projects due to debt-overhang problems (Myers 1977), we capture risk-taking using stock price volatility. Specifically, we define *Return Volatility* as the standard deviation of the returns during the prior year. To capture the trade-off between the tax benefits of debt and the costs of financial distress (i.e., bankruptcy costs), we include the tax rate in the regression.¹³ Finally, in all regressions, we include firm fixed effects to remove the impact of stable firm characteristics. We also include year fixed effects to ensure that our key variable does not simply capture a time trend in financing. We cluster the White heteroscedasticity-robust standard errors by firm. We winsorize all continuous variables at the first and 99th percentiles to avoid the influence of outliers.

4 Empirical results

4.1 Descriptive statistics

Panel A of Table 2 presents the descriptive statistics of the main variables. We first examine our key external financing variables: *Equity*, *Bank*, *Bond*, and

¹² We use net income rather than income before extraordinary items because, in China, firms do not report extraordinary items as a line item.

¹³ In theory, we would need the marginal tax rate, following Graham (1996). U.S.-based studies (e.g., Chang et al. 2009; Chen et al. 2013) employ data estimated by Professor John Graham at <https://faculty.fuqua.duke.edu/~jgraham/taxform.html>. To the extent that this data is not available for international firms, we approximate the marginal tax rate with the average tax rate.

Other. On average, bank loans represent more than 87.5% of the total external financing. This is consistent with notion that China is a bank-based economy. Bank financing is followed by equity financing, which represents approximately 8.4% of the total external financing. In contrast, financing from bonds and other sources is less economically significant, representing only 0.8% and 3.2%, respectively, consistent with the fact that China's corporate bond market is still developing (Fan et al. 2011). The mean *ROA* is 3.4%, and approximately 9.5% of the firm-year observations are loss years during our sample period. Overall, the distribution of key variables in our setting is comparable to that of other studies (e.g., Lennox et al. 2014).

Table 2 Descriptive statistics and univariate tests

Panel A: Summary statistics						
Variable	N	Mean	Q1	Median	Q3	Std Dev.
<i>Equity</i>	6874	0.084	0.000	0.000	0.024	0.213
<i>Bank</i>	6874	0.875	0.903	0.995	1.000	0.251
<i>Bond</i>	6874	0.008	0.000	0.000	0.000	0.051
<i>Other</i>	6874	0.032	0.000	0.000	0.000	0.131
<i>Size</i>	6874	21.320	20.590	21.200	21.890	1.041
<i>Lev</i>	6874	0.468	0.334	0.470	0.600	0.187
<i>ROA</i>	6874	0.034	0.014	0.037	0.062	0.059
<i>PPE</i>	6874	0.345	0.194	0.322	0.479	0.196
<i>MB</i>	6874	1.931	0.901	1.512	2.486	1.459
<i>Loss</i>	6874	0.095	0.000	0.000	0.000	0.293
<i>Dividend</i>	6874	0.013	0.000	0.006	0.019	0.017
<i>Return</i>	6874	0.126	-0.235	0.009	0.418	0.543
<i>Return Volatility</i>	6874	0.118	0.082	0.105	0.142	0.052
<i>Tax Rate</i>	6874	0.182	0.090	0.155	0.253	0.148

Panel B: Descriptive statistics on external financing for treatment firms							
	<i>t-3</i>	<i>t-2</i>	<i>t-1</i>	<i>t</i>	<i>t+1</i>	<i>t+2</i>	<i>t+3</i>
Number of firms with external financing	243	280	329	324	316	319	312
Relative percentage of funds raised (%)							
Equity	11.29	10.91	8.79	5.80	5.60	5.78	5.93
Bank Loan	84.87	85.49	88.49	88.55	88.85	88.40	90.30
Bond	0.69	0.32	0.50	0.94	0.54	0.79	0.97
Other Financing	2.90	3.01	2.10	4.38	5.00	4.98	2.74

Panel A presents the summary statistics for the main variables. Panel B presents the descriptive statistics on relative use of external financing for the treatment firms audited by LQPs in our sample. The panel reports the average use of each type of financing (equity, bank, bonds, and other) along the timeline. Year *t* refers to the fiscal year in which the LQPs' associated sanction announcement falls.

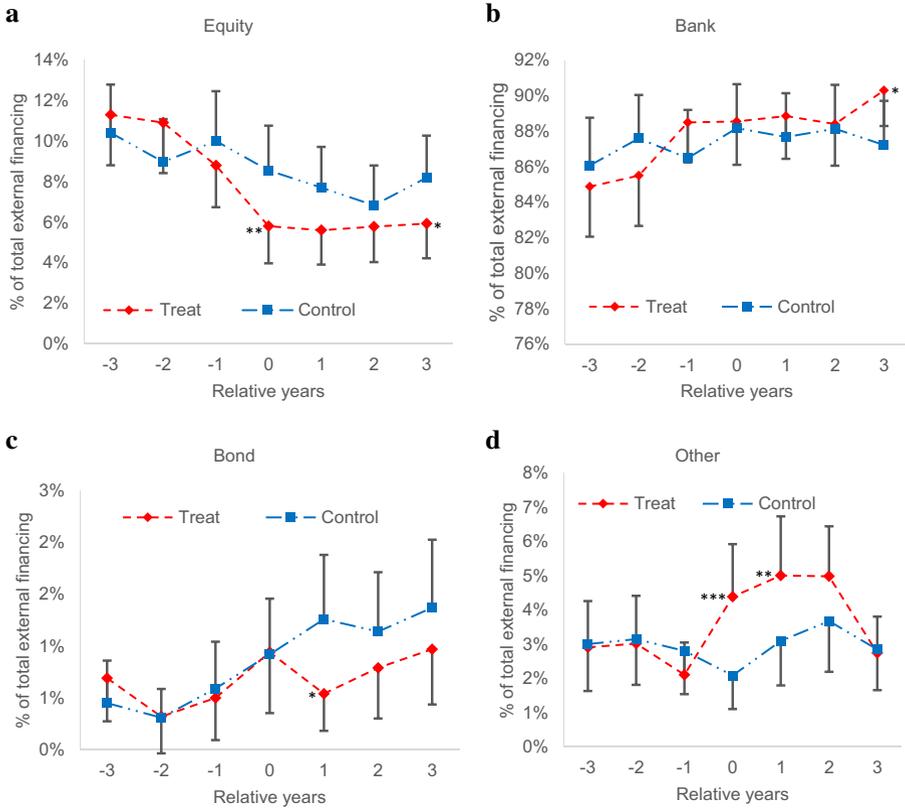


Fig. 1 The pattern of external financing variables along the timeline. Each panel in this figure plots the average amount of each type of financing (equity, bank, bonds, and other) along the timeline. The treatment and control firms are represented using the red (dashed) and blue (dot-dashed) lines, respectively. The vertical lines from each node reflect standard errors of the mean values multiplied by 10. In each plot, we also indicate whether the difference between the treatment and control firm is statistically significant. Specifically, we use *, **, and *** to indicate significance at the 10%, 5%, and 1% levels, respectively

We describe the financing patterns for the treatment firm along seven years around the event date in Panel B of Table 2. Specifically, we compare the three-year period before the event (i.e., year $t-3$, $t-2$, and $t-1$) with the four year-period afterward.¹⁴ Among the treatment firms, the proportion of equity financing drops substantially, from 8.79% in year $t-1$ to 5.60% in year $t+1$, suggesting that the treatment firms experience a reduction in equity financing after their auditor partner revealed to be low quality. The drop in equity financing is primarily substituted by other financing, which increases by approximately 2.90%.

We produce several plots in Fig. 1 to describe the dynamics of each type of financing along the timeline. In particular, for each type of financing (equity,

¹⁴ The relative use of each type is defined as the amount of this type scaled by the total amount of external financing. We only keep observations with nonzero total external financing.

Table 3 The baseline result: The impact of LQP on equity financing

	Predicted Signs	<i>Equity_Financing</i>	
		(1)	(2)
<i>Treat*Post</i>	–	–0.050** (–2.255)	
<i>Treat*Before</i> ($t = -1$)	0		–0.039 (–1.138)
<i>Treat*Post</i> ($t = 0$)	–		–0.083** (–2.513)
<i>Treat*Post</i> ($t = 1$)	–		–0.063* (–1.860)
<i>Treat*Post</i> ($t \geq 2$)			–0.058* (–1.852)
<i>Post</i>		0.012 (0.602)	
<i>Before</i> ($t = -1$)			0.025 (0.953)
<i>Post</i> ($t = 0$)			0.025 (0.656)
<i>Post</i> ($t = 1$)			0.030 (0.620)
<i>Post</i> ($t \geq 2$)			0.009 (0.140)
<i>Size</i>		–0.142*** (–6.030)	–0.142*** (–6.070)
<i>Lev</i>		0.645*** (8.538)	0.648*** (8.554)
<i>ROA</i>		1.599*** (6.728)	1.601*** (6.754)
<i>PPE</i>		0.024 (0.359)	0.024 (0.350)
<i>MB</i>		0.031*** (3.531)	0.031*** (3.529)
<i>Loss</i>		0.057* (1.785)	0.057* (1.771)
<i>Dividend</i>		–0.074 (–0.142)	–0.078 (–0.150)
<i>Return</i>		0.012 (0.578)	0.012 (0.588)
<i>Return Volatility</i>		–0.098 (–0.622)	–0.103 (–0.652)
<i>Tax Rate</i>		–0.078* (–1.858)	–0.078* (–1.851)
Year Fixed Effects		Yes	Yes
Firm Fixed Effects		Yes	Yes
Observations		6874	6874
Pseudo R ²		0.429	0.429

This table presents the baseline results of estimating equity financing on *Treat*, *Post*, and their interaction. For each sanction event, we designate firms that have been audited by the LQPs in the year before the sanction announcement as the treatment firms. *Post* is an indicator variable that takes a value of one for the period after the LQP's sanction announcement and zero otherwise. Variable definitions are presented in the appendix. We include firm and year fixed effects. Standard errors are heteroskedasticity-robust and clustered by firm. *T*-statistics are presented beneath the estimates. Significance at the 10%, 5%, and 1% levels are indicated by *, **, and ***, respectively.

bank loan, bond, and other financing), we plot the average relative use. The treatment and control firms are represented using the red (dashed) and blue (dot-dashed) lines, respectively. The vertical lines from each node reflect standard errors of the mean values multiplied by 10. In particular, Panel A of Fig. 1 describes the dynamics of equity financing. As shown in the plot, the two lines representing the percentage of equity financing for the treatment group and the control group trend more or less in parallel in the three years leading up to year t . Equity financing in the treatment firms is not higher or lower systematically than the one in the control firms until year t . Starting in year t , the two lines diverge, indicating a remarkable drop for the treatment firms. Sample wide, in the plot, the difference between the treatment and control firms in equity financing is most statistically significant in year t , the year when the sanction event conveys a negative connotation about the audit partner's quality at the treatment firm. The difference between treatment and control firms remains economically large in other years in the post-period. The difference between treatment firms and control firms is marginally statistically significant in year $t+1$ (p value = 0.11) and significant (p value = 0.09) in year $t+3$, respectively. Panel D describes the dynamics of other financing. Similar to Panel A, the two lines representing the use of other financing for the treatment group and the control group trend in parallel in the three years leading up to year t before diverging in the event year. Taken together, we interpret the results as suggesting that the treatment firm switches from equity financing to credit financing after the audit partner is perceived to be low quality.

4.2 Main results

4.2.1 The use of equity financing

Table 3 presents the regression results of eq. (1). The dependent variable is *Equity*, defined as the amount of equity financing scaled by the total amount of financing. In the years when firms do not have equity issuance, *Equity* takes a value of zero. To the extent that equity financing turns to a positive value only when the firm has an external financing need, we employ the Tobit regression model to estimate eq. (1). The independent variable of interest is *Treat*Post*. In column (1), the coefficient on *Treat*Post* ($\beta = -0.050$) is negative and statistically significant at the 5% level, consistent with the prediction that firms audited by an LQP issue less equity when their auditor partners are found to be low quality. The magnitude is also economically significant given that the sample's unconditional mean of *Equity* is 8.4%. In other words, among those firms with an average equity financing intensity of 8.4%, moving from a non-LQP's client firm to an otherwise identical LQP's client results in a 58% reduction in equity financing.¹⁵

¹⁵ We also assess the statistical difference between the coefficient on *Treat*Before* ($t = -1$) and the coefficient on *Treat*Post* ($t = 0$). We expect that the coefficient on *Treat*Post* ($t = 0$) is more negative than the coefficient on *Treat*Before* ($t = -1$), as treat firms that have an information problem due to quality of audit partner have reduced equity financing. The difference between the coefficient on *Treat*Before* ($t = -1$) and the coefficient on *Treat*Post* ($t = 0$) is 0.044 and is marginally statistically significant at the 10% level (p value = 0.10).

The coefficient on $Treat*Post$ in column (1) captures the average change in equity financing in the post-period. To show the dynamics of the effect of LQP on external financing choice, following Chen et al. (2013), we introduce several year indicator variables ($Post(t=0)$, $Post(t=1)$, etc.). $Post(t=i)$ takes a value of one if it is the i -th year after the identification of the LQP and zero otherwise. $Before(t=-1)$ takes a value of one if it is the year before the identification of the LQP and zero otherwise. If the auditor partner's sanction induces a lower level of equity financing for firms audited by the LQP, we should observe negative coefficients for the interaction terms $Treat*Post(t=i)$ but an insignificant coefficient for the interaction term $Treat*Before(t=-1)$. Specifically, we expect that the most salient change in equity financing occurs in the first couple of years when the auditor partner is found to be an LQP. Column (2) shows the results from this dynamic specification. Overall, the results support the interpretation that the discovery of audit partners' quality results in a decline in equity financing and the effect persists for approximately two years.

The coefficients on the control variables in Table 3 are largely consistent with those identified in prior studies. For example, we find that firms with better performance (ROA) and higher growth opportunities (MB) rely more on equity financing. Larger firms ($Size$) are less likely to use equity financing, as they can use less costly debt. In addition, firms that have greater access to debt financing, as measured by the higher leverage ratio (Lev), are more likely to issue equity. Other control variables are not statistically significant at the conventional levels.

4.2.2 The use of debt financing

We next explore the relative use of three types of debt financing: bank loans, bonds, and other. We use the same model specifications as in eq. (1), except that the dependent variables are the relative use of bank financing ($Bank$), bonds ($Bond$), and other financing ($Other$) over total external financing, respectively. In particular, $Bank$ ($Bond$) is defined as external financing through bank loans (public bonds) scaled by the total amount of external financing. $Other$ is defined as the loans provided from related parties, including controlling shareholders, block holders, or companies invested by the firm, scaled by the total amount of external financing. Inter-firm loans in this type are more likely to be relationship-based financing, as the capital providers are typically insiders to the firm. We collect this data field by manually inspecting the annual reports. Finally, if a firm does not have a certain type of financing, we set the value to zero.

Table 4 reports the regression results. The coefficients on $Treat*Post$ in columns (1) and (2) are insignificant at conventional levels when we use $Bank$ and $Bond$ as dependent variables, suggesting that bank loans and corporate bonds do not change significantly in the post-period. In contrast, the coefficient on $Treat*Post$ in Column (3) ($\beta=0.088$) is positive and significant at the 1% level, suggesting that firms rely significantly more on other debt financing provided by related parties. This capital injection is to meet the need for financing and to compensate for the decline in equity financing. We interpret this finding as providing evidence on the substitution effect between public trust and private trust. The sanction damages the public trust between equity investors and the management in treatment firms. To compensate for the loss of capital, these firms

Table 4 The impact of LQP on other financing

	<i>External Financing through Debt</i>		
	(1) <i>Bank</i>	(2) <i>Bond</i>	(3) <i>Other</i>
<i>Treat*Post</i>	0.004 (0.195)	-0.069 (-0.896)	0.088*** (3.937)
<i>Post</i>	-0.012 (-0.646)	0.085 (1.374)	0.007 (0.383)
<i>Size</i>	0.102*** (4.970)	0.081 (1.402)	-0.024 (-1.010)
<i>Lev</i>	-0.555*** (-7.826)	-0.055 (-0.251)	0.219*** (2.808)
<i>ROA</i>	-0.944*** (-5.378)	-0.128 (-0.201)	-0.197 (-1.146)
<i>PPE</i>	-0.060 (-0.982)	0.074 (0.376)	0.031 (0.418)
<i>MB</i>	-0.025*** (-3.150)	-0.014 (-0.570)	-0.004 (-0.530)
<i>Loss</i>	-0.079*** (-3.022)	-0.026 (-0.235)	0.024 (0.850)
<i>Dividend</i>	-0.524 (-1.174)	-1.606 (-0.781)	0.614 (1.438)
<i>Return</i>	-0.001 (-0.039)	0.053 (1.088)	0.011 (0.567)
<i>Return Volatility</i>	0.218* (1.656)	-0.538 (-1.224)	-0.398*** (-2.809)
<i>Tax Rate</i>	-0.013 (-0.368)	0.099 (0.720)	0.051 (1.256)
Year Fixed Effects	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Observations	6874	6874	6874
Pseudo R ²	0.449	0.662	0.563

This table presents the baseline results of estimating external financing through debt on *Treat*, *Post*, and the interaction between *Treat* and *Post*. *Bank* represents the financing obtained from banks scaled by total external financing. *Bond* represents the financing obtained from the issuance of bonds scaled by total external financing. *Other* represents the financing obtained from other sources scaled by total external financing. *Treat* is an indicator variable that takes a value of one for treatment firms and zero for the control firms. *Post* is an indicator variable that takes a value of one for period after the LQP's associated sanction announcement and zero otherwise. The key variable of interest is the interaction term *Treat*Post*. Variable definitions are presented in the appendix. We include firm and year fixed effects. Standard errors are heteroskedasticity-robust and clustered by firm. *T*-statistics are presented beneath the estimates. Significance at the 10%, 5%, and 1% levels are indicated by *, **, and ***, respectively.

rely more on relationship-based financing, in which private trust plays a more important role.

With respect to the control variables, we find that larger firms (*Size*) have more bank loans but not bond financing or other financing. This may be due to the possibility that larger firms can better access the loan market. On average, firms with a higher leverage ratio (*Lev*) have less debt financing; firms with higher growth (*MB*) and negative net income (*Loss*) have fewer bank loans.

Table 5 Sensitivity tests

	(1)	(2)	(3)	(4)
	<i>Equity</i>	<i>Bank</i>	<i>Bond</i>	<i>Other</i>
Panel A: Using Logit regressions.				
	<i>Indicator for Each Type of External Financing</i>			
<i>Treat*Post</i>	-0.429*** (-2.607)	0.342 (0.660)	-0.664 (-1.208)	0.985*** (5.182)
Firm characteristics	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Observations	6874	6874	6874	6874
Observations Used	5643	823	1127	4840
Pseudo R ²	0.181	0.204	0.209	0.176
Panel B: Using OLS regressions				
<i>Treat*Post</i>	-0.014* (-1.669)	0.007 (0.500)	-0.002 (-0.871)	0.012* (1.883)
Firm characteristics	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Observations	6874	6874	6874	6874
R ²	0.384	0.405	0.279	0.386
Panel C: Using total assets to scale external financing variables				
	<i>Relative Percentage of External Financing</i>			
<i>Treat*Post</i>	-0.024** (-2.436)	-0.012 (-0.946)	-0.026 (-0.906)	0.019*** (4.329)
Firm characteristics	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Observations	7408	7408	7408	7408
Pseudo R ²	0.414	0.494	0.690	0.553
Panel D: Using the amount of external financing as the dependent variable				
	<i>Amounts of External Financing</i>			
<i>Treat*Post</i>	-1.186** (-2.496)	0.115 (0.573)	-0.281 (-1.362)	2.060*** (4.649)
Firm characteristics	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Observations	6874	6874	6874	6874
R ²	0.370	0.562	0.307	0.372

Panel A presents the results of estimating external financing variables using Logit regressions. Dependent variables are indicator variables that takes a value of one if the specific type of financing (equity, bank, bonds, and other) is positive and zero otherwise.

Panel B presents the results of estimating external financing variables using OLS regressions. Dependent variables are the amount of financing through each type (equity, bank, bonds, and other) scaled by total external financing.

Panel C presents results of estimating external financing variables using total assets as an alternative scale variable. Dependent variables are the amount of financing through each type (equity, bank, bonds, and other) scaled by total assets.

Panel D presents the results using the absolute amount of financing as external financing variables. We use the natural logarithm of one plus the amount of each type of financing as the dependent variable. For the sake of brevity, we only present the coefficient of variable on the interest (*Treat*Post*). Standard errors are heteroskedasticity-robust and clustered by firm. *T*-statistics are presented beneath the estimates. Significance at the 10%, 5%, and 1% levels are indicated by *, **, and ***, respectively.

4.2.3 Alternative specifications

In the baseline test, we use the Tobit model because our dependent variables are truncated (zero-bounded). We next assess the robustness of our results using alternative econometric specifications. First, we employ a Logit model instead and use an indicator variable to capture whether the firm has any specific type of external financing as the dependent variable. We present the results in Panel A of Table 5. Similar to the results presented in the baseline test in the main text, when the dependent variable is *Equity*, the coefficient on *Treat*Post* in column (1) is negative and statistically significant at the 1% level. When the dependent variable is *Other*, the coefficient on *Treat*Post* in column (4) is positive and statistically significant at the 1% level. Second, we employ the OLS regression and present the results in Panel B of Table 5. Results and inferences are similar. Third, we assess the robustness of our results to the inclusion of observations with zero financing. In the baseline tests, we have removed observations with zero financing, because we use total external financing to scale each individual type of external financing. To recover these observations, we use total assets as an alternative scale variable. Total assets help mitigate the impact of firm size without generating sample attrition. We present the results in Panel C of Table 5. The sample size is larger than the sample in the baseline because of the less restrictive data requirement on total financing. Most importantly, the results and inferences are similar. In untabulated tests, we have replicated our results using different combinations of (1) Logit and OLS and (2) an enlarged sample using total assets as the scale variable. Our inferences remain unchanged if we use these alternative designs. Panel D of Table 5 presents the results when we use the absolute amount of each type of financing as the dependent variable as a robustness check. Specifically, the dependent variable is the natural logarithm of one plus the amount of each type of financing. The results are qualitatively similar to the design where we use the relative percentage of financing as the dependent variable. To the extent that we investigate the mix of financing, in subsequent tests, we use the relative use of financing as our dependent variables.

4.3 The cost of external financing

4.3.1 Investors' response to the sanction

An implicit assumption underlying our prior analyses is that the audit partner's sanction event negatively shocks the equity investors' perception of audit partner quality and the

Table 6 The impact of LQP on the cost of financing**Panel A: Investors' reaction**

	(1) Keep all the announcements CAR[-1,+1]	(2) Keep the first announcement only CAR[-1,+1]
<i>Treat</i>	-0.004** (-2.339)	-0.007*** (-3.441)
<i>Size</i>	0.000 (0.209)	-0.001 (-0.521)
<i>Lev</i>	-0.005 (-0.831)	0.001 (0.191)
<i>ROA</i>	-0.027 (-1.015)	-0.033 (-1.097)
<i>MB</i>	-0.000 (-0.252)	0.000 (0.006)
Year Fixed Effects	Yes	Yes
Industry Fixed Effects	Yes	Yes
Observations	1328	980
R ²	0.032	0.060

Panel B: Cost of capital

	<i>Cost of Capital</i>	
	(1) <i>CoE</i>	(2) <i>CoD</i>
<i>Treat*Post</i>	0.016* (1.713)	-0.002 (-0.253)
<i>Post</i>	-0.009 (-1.126)	0.010 (1.564)
<i>Size</i>	0.028** (2.537)	-0.016 (-1.609)
<i>Lev</i>	-0.077** (-2.545)	0.069*** (2.602)
<i>ROA</i>	-0.035 (-0.517)	0.031 (0.903)
<i>MB</i>	-0.008*** (-2.782)	0.001 (0.304)
<i>Return Volatility</i>	-0.095* (-1.763)	-0.004 (-0.092)
<i>Beta</i>	0.008 (0.643)	0.011 (1.278)
Year Fixed Effects	Yes	Yes
Firm Fixed Effects	Yes	Yes
Observations	3334	6435
R ²	0.546	0.508

Panel A presents the results of estimating three-day cumulative abnormal returns around the associated sanction events of the audit partners. For each treatment firm, we calculate the three-day abnormal returns around the sanction announcement date. We use the same algorithm outlined in Section 3.1 to identify control firms and use the treatment firm's event date as the pseudo-event date for the control firms. The dependent variables are $CAR[-1, +1]$, where day 0 is the sanction announcement date. We use the market-return-adjusted daily return (the daily return minus the market portfolio's return) as the abnormal return. We include firm size, leverage, ROA, and the market-to-book ratio as additional firm-characteristics controls in this short-window return test. We include industry and year fixed effects. Panel B presents the baseline results of estimating the cost of capital (i.e., cost of equity and cost debt, respectively) on *Treat*, *Post*, and the interaction between *Treat* and *Post* with firm characteristics. *Treat* is an indicator variable that takes a value of one for treatment firms and zero for the control firms. *Post* is an indicator variable that takes a value of one for the period after the LQP's associated sanction announcement and zero otherwise. The key variable of interest is the interaction term *Treat*Post*. Variable definitions are presented in the appendix. We include firm and year fixed effects. Standard errors are heteroskedasticity-robust and clustered by firm. *T*-statistics are presented beneath the estimates. Significance at the 10%, 5%, and 1% levels are indicated by *, **, and ***, respectively.

audit performed at the treatment firms. An implication is that we should expect our treatment firms to have more negative stock returns around the sanction, compared with control firms. We test this prediction using an event study. Specifically, for each treatment firm, we calculate the three-day abnormal returns around the sanction announcement date of the LQP. We use the same algorithm outlined in section 3.1 to identify control firms and use the treatment firm's event date as the pseudo-event date for the control firms. The dependent variable is $CAR[-1, +1]$, where day 0 is the sanction announcement date. We use the market-return-adjusted daily return (the daily return minus the market portfolio's return) as the abnormal return.

We expect a negative coefficient on *Treat* as the sanction conveys negative news about the audit partner's quality at the treatment firm. To avoid the potential confounding effect due to other corporate events, we exclude observations if other events, such as earnings releases, earnings forecasts, de-listings or suspensions of listing, or special treatment, occur in the window.¹⁶ We include firm size, leverage, ROA, and the market-to-book ratio as additional firm-characteristics controls in this short-window return test. Because this is an event study and our treatment firms only appear once or twice (if two sanction dates occur in the same year) in this test, we do not include firm fixed effects. Instead, we use industry fixed effects. As usual, we include year fixed effects in the regressions.

We tabulate the results in Panel A of Table 6. As we explained earlier, one treatment firm may have more than one sanction announcement during the same year. For example, the auditor partner *X* for a treatment firm may have been involved in two sanctions in year *t* (due to financial misreporting at two distinct client firms) and thus have two distinct sanction announcement dates during this year. If this is case, we include both sanction announcements and calculate two distinct cumulative abnormal returns for this treatment firm. We present the result in column (1). In addition, we consider an alternative research design. It is conceptually possible that the first sanction date in the same year provides a more informative signal about the quality of the audit partner and the audit at the treatment firm performed by that partner. So we keep only the first announcement for each treatment firm and present the result in column (2). In both columns, the coefficient on *Treat* is negative and statistically significant at the 5%

¹⁶ In China, a listed firm is designated as a special treatment (ST) firm if it reports a net loss for two consecutive years.

level or better. Interestingly, the coefficient on *Treat* in column (2) is larger than the magnitude of the coefficient in column (1), consistent with the idea that when a treatment firm's auditor partner is involved in two sanction announcements in a given year, the first contains more information about the quality of the audit.

4.3.2 Cost of capital

One implication of our previous analysis is that equity and credit investors respond differentially after the sanction event of auditor partners. This distinction stems from differences in how the two groups acquire information. While equity investors rely on public disclosures and delegate information verification to the auditors, creditors often have private communications with management to assess firm-specific risks. An assumption underlying our prediction is that the cost of external financing changes after the audit partner revealed to be low quality. To validate this assumption directly, we estimate the cost of capital using the following equation.

$$\begin{aligned} \text{Cost of capital} = & \beta_0 + \beta_1 \text{ Treat*Post} + \beta_2 \text{ Post} + \text{Firm characteristics} \\ & + \text{Year fixed effects} + \text{Firm fixed effects} \end{aligned} \quad (2)$$

Specifically, *Cost of capital* is a placeholder for cost of equity (*CoE*) and cost of debt (*CoD*), respectively. *CoE* is cost of equity capital, calculated as the industry-adjusted square root of the inverse of the price-earnings-growth ratio, whereas earnings growth is calculated as the two-year-ahead realized earnings-per-share minus the one-year-ahead realized earnings-per-share, and price is the stock price 90 days after the fiscal year-end (Easton 2004; Fang et al. 2017).¹⁷ This measure has intuitive appeal, insofar as a firm with a higher price-earnings-growth ratio has a lower cost of capital—investors are willing to pay more for a given dollar of earnings growth. We follow Francis et al. (2005) and define *CoD* as the (weighted-average) cost of debt, calculated as the ratio of a firm's interest expense to interest-bearing debt outstanding.¹⁸ Following Hail and Leuz (2006), we include the CAPM *Beta* (defined as the beta from a capital asset pricing model that regresses monthly returns from the prior three years on a value-weighted market return) as the control variable. We also keep firm size, leverage, market-to-book, and return volatility as firm-characteristic controls, as do Francis et al. (2005).

We present the results in Panel B of Table 6. Most importantly, in column (1), when the dependent variable is *CoE*, the coefficient on *Treat*Post* is positive and statistically significant at the 10% level. In contrast, in column (2), when the dependent variable is *CoD*, the coefficient on *Treat*Post* is indistinguishable from zero at conventional levels. Taken together, we conclude that equity investors start to charge a higher cost

¹⁷ To the extent that we need the assumption that the two-year-ahead realized EPS is greater than the one-year-ahead realized EPS (i.e., there is positive earnings growth), our sample size in this test is smaller.

¹⁸ Ideally, we would have used the more granular transaction-level data (e.g., the Dealscan-like databases provided by LPC) to calculate the cost of debt, as this data would incorporate more deal-level information, such as loan type, loan term, loan purposes, etc. However, the database that contains such detailed deal-level information is not available in China to researchers. We instead approximate the cost of debt using the approach adopted by U.S.-based studies before the availability of Dealscan, for example, Francis et al. (2005).

Table 7 A validation test: the effect of correctional actions

	<i>Equity_Financing</i>	
	(1)	(2)
<i>Treat^{Still LQP}*Post</i>	-0.061*** (-3.344)	
<i>Treat^{Retain LQP}*Post</i>		-0.057*** (-2.795)
<i>Treat^{Another LQP}*Post</i>		-0.075*** (-2.508)
<i>Treat^{Dismiss LQP}*Post</i>	-0.022 (-0.877)	-0.022 (-0.877)
<i>Post</i>	0.012 (0.656)	0.012 (0.657)
<i>Size</i>	-0.141*** (-7.358)	-0.141*** (-7.342)
<i>Lev</i>	0.644*** (9.829)	0.643*** (9.803)
<i>ROA</i>	1.603*** (7.733)	1.602*** (7.730)
<i>PPE</i>	0.025 (0.427)	0.025 (0.428)
<i>MB</i>	0.031*** (4.067)	0.031*** (4.059)
<i>Loss</i>	0.057* (1.944)	0.057* (1.945)
<i>Dividend</i>	-0.085 (-0.176)	-0.085 (-0.176)
<i>Return</i>	0.012 (0.673)	0.012 (0.680)
<i>Return Volatility</i>	-0.102 (-0.715)	-0.102 (-0.718)
<i>Tax Rate</i>	-0.079** (-2.066)	-0.079** (-2.060)
Year Fixed Effects	Yes	Yes
Firm Fixed Effects	Yes	Yes
Observations	6874	6874
Pseudo R ²	0.429	0.429

This table examines the effect of LQP turnovers on equity financing. We classify treatment firms into two types: firms that still hire an LQP (identified by *Treat^{Still LQP}*) and firms that dismiss the LQP and hire a good auditor (identified by *Treat^{Dismiss LQP}*). In column (2), we further classify treatment firms that still hire an LQP into firms that retain the current LQP (identified by *Treat^{Retain LQP}*) and firms that replace the current LQP with another LQP (identified by *Treat^{Another LQP}*). We include firm and year fixed effects. Standard errors are heteroskedasticity-robust and clustered by firm. *T*-statistics are presented beneath the estimates. Significance at the 10%, 5%, and 1% levels are indicated by *, **, and ***, respectively.

of equity capital when the quality of the individual auditor partner is perceived to be poor, whereas the cost of debt is less sensitive to such a discovery of audit quality.

4.4 A validation test: The effect correctional actions

We next probe whether firms can rebuild investors' trust by pursuing correctional strategies. This investigation hinges on how firms respond to significant losses in reputation and stakeholder trust that occur when their auditors are revealed as low quality (Chakravarthy et al. 2014). Specifically, we focus on whether the firm *replaces* the LQP when the managers themselves learn that the audit engagements performed by these partners may carry information risks to outsiders. If dismissing the LQP restores trust, we should expect that firms that were audited by LQPs but dismissed them upon sanctions experience a damped negative impact; that is, they experience a smaller decrease in equity financing.

To test this prediction, we first examine the proportion of LQPs that were dismissed after their quality is revealed to be poor. We focus on the correctional strategy in year t , that is, immediately after the sanction event. In our sample, 670 out of 2123 firms (i.e., 31.56%) in the treatment sample dismiss LQPs and switch to a high-quality audit partner right afterwards. However, 296 of these firms replace the original LQP with another LQP and 1157 of them retain the LQP.¹⁹

To probe the impact of LQP dismissal on firms' financial choices, we revise eq. (1) and separate the treatment group into two subgroups. We capture whether treatment firms' audit partners in the post-period are still of low quality or not. Specifically, $Treat^{Still\ LQP}$ takes a value of one if the treatment firm still hires an LQP (either the incumbent LQP or another new LQP) in the post period and zero otherwise. $Treat^{Dismiss\ LQP}$ takes a value of one if the treatment firm replaces the LQP with a quality audit partner and zero otherwise. We replace $Treat*Post$ in eq. (1) with the two interaction terms, $Treat^{Still\ LQP}*Post$ and $Treat^{Dismiss\ LQP}*Post$, and present the results in column (1) of Table 7. The coefficient on $Treat^{Still\ LQP}*Post$ is -0.061 and statistically significant at the 1% level, suggesting that firms audited by an LQP experience a significant decrease in equity financing. In contrast, the coefficient on $Treat^{Dismiss\ LQP}*Post$ is -0.022 and statistically insignificantly different from zero, suggesting that the effect of LQP on equity financing is immaterial once the firm replaces the LQP with a quality partner. The difference between the coefficient on $Treat^{Still\ LQP}*Post$ and coefficient on $Treat^{Dismiss\ LQP}*Post$ is 0.039 and is statistically significantly (p value = 0.08, untabulated).

To the extent that firms indicated by $Treat^{Still\ LQP}$ include both firms that keep their original LQP and those that replace their original LQP with another LQP, we further separate these two types of firms to see whether the market views them differentially. To do so, we create two indicator variables, $Treat^{Retain\ LQP}$ and $Treat^{Another\ LQP}$. $Treat^{Retain\ LQP}$ takes a value of one if the treatment firm retains its incumbent LQP in the post period and zero otherwise. $Treat^{Another\ LQP}$ takes a value of one if the treatment firm replaces the

¹⁹ We take a dynamic approach when we identify LQPs. Specifically, we identify whether an audit partner is of low quality based on all public information as of time t . For example, if partner X has not been the auditor for any of the firms receiving regulatory sanctions as of 2008, we identify X as a high-quality auditor in years up to 2008, although X may be later identified as low quality in years after 2008. Under this approach, we, as researchers, work with the same information set as equity investors without introducing "look-ahead bias."

incumbent LQP with another LQP and zero otherwise. We then replace $Treat^{Still\ LQP}$ in column (1) with $Treat^{Retain\ LQP}$ and $Treat^{Another\ LQP}$ and present the results in column (2). The coefficients on both $Treat^{Retain\ LQP*Post}$ and $Treat^{Another\ LQP*Post}$ are negative and statistically significant at the 5% level or better, suggesting that firms use less equity financing, regardless of whether they keep the original LQP or hire another new external LQP. The coefficient on $Treat^{Dismiss\ LQP}$ is indistinguishable from zero at conventional levels. In addition, the difference between the coefficient on $Treat^{Another\ LQP*Post}$ and coefficient on $Treat^{Dismiss\ LQP*Post}$ is 0.053 and is statistically significantly (p value = 0.07, untabulated). In contrast, the difference between the coefficient on $Treat^{Retain*Post}$ and the coefficient on $Treat^{Another\ LQP*Post}$ is statistically indifferent from zero (p value = 0.58). This is to suggest that firms that retain the old LQP or hire another LQP externally both have reduced equity financing in the post-period and the reduction does not differ from each other. Taken together, we conclude that firms benefit from the correctional actions that replace the incumbent LQP with a high-quality partner. The treatment firms that replace an LQP with a high-quality partner experience a smaller decrease in equity financing. To the extent that the quality of audit partners is revealed to the equity investors, replacing an LPQ with another LQP still results in penalties to the firm. Instead, only the prompt replacement of an LQP with a high-quality partner significantly impacts external financing choices through rebuilding investors' trust.

5 Additional analyses

In our main tests, we employ firms audited by the audit partners from the same audit offices as the LQP as the control firms. In other words, we compare the affected firms against firms that are audited by the LQP's home audit office. The rationale for holding office-level characteristics constant is that we want to attribute the difference in external financing to the individual auditor quality, rather than to office characteristics. To assess the robustness of our results to this choice of control firms, we next use a set of alternative control firms. We first employ all firms audited by the audit partners from the *same audit firms* as the LQP as the alternative control firms. That is, for each LQP, we use all client firms audited by the same audit firm at which the LQP works as control firms, regardless of whether the control firm and the treatment firm are audited by the same office. Under this choice, we compare the affected firms against firms that are audited by the LQP's home audit firm. In this alternative design, since we relax the requirement that the treatment and control firms need to come from the same audit office, the final sample size is larger than the main sample in our baseline test.

Second, we employ all client firms audited by audit firms without an LQP as the control firms. That is, for each LQP, we use all client firms audited by audit firms without any LQPs (i.e., high-quality audit firms). Under this choice, we compare the affected firms against those firms that are audited by high-quality auditors from high-quality audit firms. We repeat the analysis based on these two alternative designs and report the results in two panels of Table 8, respectively. When *Equity* is the dependent variable, the coefficient on $Treat*Post$ remains positive and statistically significant at the 10% level or better in both columns, consistent with the findings in the baseline test.

Table 8 Robustness checks using alternative control firms

	(1)	(2)	(3)	(4)
	<i>Equity</i>	<i>Bank</i>	<i>Bond</i>	<i>Other</i>
Panel A: Firms audited by the LQP's home audit firm as control firms				
<i>Treat*Post</i>	-0.043*	-0.005	-0.043	0.085***
	(-1.913)	(-0.235)	(-0.589)	(3.806)
Firm Characteristics	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes
Firm Fixed Effect	Yes	Yes	Yes	Yes
Control Firms Constructed from	Firms audited by the LQP's home audit firm			
Observations	9459	9459	9459	9459
Pseudo R ²	0.465	0.490	0.690	0.500
Panel B: Firms audited by non-LQAF as control firms				
<i>Treat*Post</i>	-0.057*	0.021	-0.237*	0.051**
	(-1.671)	(0.781)	(-1.708)	(2.011)
Firm Characteristics	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes
Firm Fixed Effect	Yes	Yes	Yes	Yes
Control Firms Constructed from	Firms audited by the LQP's home audit firm			
Observations	9040	9040	9040	9040
Pseudo R ²	0.489	0.490	0.685	0.490

In Panel A, we employ all firms audited by the audit partners from the *same audit firms* as the LQP as control firms. That is, for each LQP, we use all client firms audited by the same audit firm at which the LQP works as the control firms, regardless of whether the control firm and the treatment firm are audited by the same *office*. In Panel B, we use all client firms audited by those audit firms without any LQP (i.e., high-quality audit firms) as the control firms. Specifically, for each LQP, we use all client firms audited by audit firms without any LQPs. For the sake of brevity, we only present the coefficient on the variable of interest (*Treat*Post*). Control variables are included but not tabulated. Variable definitions are presented in the appendix. We include firm and year fixed effects. Standard errors are heteroskedasticity-robust and clustered by firm. *T*-statistics are presented beneath the estimates. Significance at the 10%, 5%, and 1% levels are indicated by *, **, and ***, respectively.

Another possibility to explain our results is that managers have raised sufficient cash from equity financing with the help of low-quality partners *before* the discovery of individual audit partner quality.²⁰ If this is true, after investors learn the quality of audit partners, firms naturally do not need equity financing because they already have already raised sufficient funds. To ensure that our prior results are not driven by this alternative explanation, following Chen et al. (2013), we exclude the years right before the associated sanction announcement year (years $t-1$ and $t-2$, respectively) from the benchmark years. We do so because these years should be the ones that are most likely to be affected by

²⁰ We reason that this alternative explanation is unlikely to be the reason for our results. To the extent that we define LQP as the auditor partner who is involved in an audit failure at a sanctioned firm other than the treatment firm, it is almost impossible for the treatment firm's manager to time the equity financing based on when the sanction is publicly announced. In fact, it is more likely that the manager of the treatment firm would have no information regarding the quality of the audit partner's performance at another client firm. Nevertheless, we conduct the empirical test to rule out this possibility.

Table 9 Robustness checks after removing pre-event years

	(1)	(2)	(3)	(4)
	<i>Equity</i>	<i>Bank</i>	<i>Bond</i>	<i>Other</i>
Panel A: Removing year $t-1$				
<i>Treat*Post</i>	-0.063** (-2.334)	0.022 (0.896)	-0.007 (-0.068)	0.073*** (2.798)
Firm Characteristics	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes
Firm Fixed Effect	Yes	Yes	Yes	Yes
Alternative Design	Removing year $t-1$			
Observations	5799	5799	5799	5799
Pseudo R ²	0.469	0.496	0.697	0.609
Panel B: Removing year $t-1$ and year $t-2$				
<i>Treat*Post</i>	-0.060* (-1.952)	0.020 (0.636)	0.012 (0.090)	0.070** (2.101)
Firm Characteristics	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes
Firm Fixed Effect	Yes	Yes	Yes	Yes
Alternative Design	Removing years $t-1$ and $t-2$			
Observations	4921	4921	4921	4921
Pseudo R ²	0.492	0.572	0.711	0.685

This Table assesses the sensitivity of our benchmark years. We remove year $t-1$ in Panel A and both years $t-1$ and $t-2$ in Panel B, respectively. For the sake of brevity, we only present the coefficient on the variable of interest (*Treat*Post*). Control variables are included but not tabulated. Variable definitions are presented in the appendix. We include firm and year fixed effects. Standard errors are heteroskedasticity-robust and clustered by firm. T-statistics are presented beneath the estimates. Significance at the 10%, 5%, and 1% levels are indicated by *, **, and ***, respectively.

earnings management-induced equity financing, if any. We remove year $t-1$ and both years $t-1$ and $t-2$ in columns (1) and (2), respectively, and present the results two panels of Table 9. We find that, when *Equity* is the dependent variable, the coefficients on *Treat*Post* are negative and statistically significant at the 10% level or better, suggesting that our results are not driven by timing equity financing.

6 Conclusion

We investigate whether the revelation of individual audit partner quality affects the client firms' external financing choices. We identify the audit partner as low quality if one of the partner's clients receives a regulatory sanction for financial misreporting. We designate firms that do not receive the sanction but that are audited by low-quality partners as affected firms. Using a difference-in-differences design with 6874 firm-year observations spanning from 1998 to 2015, we find that the affected firm experiences a lower level of equity financing after the audit partner is revealed to be of low quality. In response, the

affected firm substitutes the reduction in equity financing with debt financing, primarily from related parties. We also find that the cost of equity significantly increases after the discovery of an LQP while the cost of debt does not. Further, our additional analyses show that reduced equity financing is concentrated among firms that have chosen to keep the LQP or firms that have replaced an LQP with another external LQP.

Our findings speak to the importance of individual audit partner quality. We argue that the regulatory sanctions reveal an audit partners' quality. To the extent that investors can infer the quality of external audits, the discovery then spills over: firms audited by low-quality audit partners will respond by changing the ways in which they obtain external financing.

Acknowledgments We thank the editor and an anonymous reviewer for very helpful comments. We thank Michael Welker, Bohui Zhang, and seminar participants at Chinese University of Hong Kong (Shenzhen) for suggestions. Zhang and Cheng acknowledge financial supports from the Commerce '83 Fellowship at Queen's University and Hong Kong Polytechnic University, respectively. Cheng and Wang acknowledge financial support from the Smith School of Business for their visits to Queen's University. Kun Wang acknowledges financial support from the National Natural Science Foundation of China (Project 71372048). Yanping Xu acknowledges financial support from the National Natural Science Foundation of China (Project 71802092), the Enterprise Transformation Research Team Project of the Institute for Enterprise Development and School of Management, Jinan University, Guangdong Province and Jinan University Management School Funding Program, No. GY18002.

Appendix: Variable Definitions

External financing variables

<i>Equity</i>	Relative use of equity financing, defined as equity financing scaled by the total amount of external financing in the year. We set the value to zero for firm-years without equity financing.
<i>Bank</i>	Relative use of bank loan financing, defined as bank loans scaled by the total amount of external financing in the year. We set the value to zero for firm-years without bank loan financing.
<i>Bond</i>	Relative use of bond financing, defined as public bonds scaled by the total amount of external financing in the year. We set the value to zero for firm-years without bond financing.
<i>Other</i>	Relative use of other types of financing (e.g., loans from related parties), defined as the ratio of other types of financing scaled by the total amount of external financing. We set the value to zero for firm-years without other types of financing.
<i>Cost of Equity</i>	The industry-median adjusted square root of the inverse of the price-earnings-growth ratio. We calculate earnings growth as the two-year-ahead realized earnings per share (EPS) minus the one-year-ahead realized EPS, and price is the share price 90 days after the fiscal year-end.
<i>Cost of Debt</i>	Interest expense scaled by the total interest-bearing debt outstanding.

Key independent variables

<i>Treat</i>	An indicator that takes a value of one if the firm is audited by an LQP over the past three years and zero otherwise. We identify a partner as an LQP if the partner is involved in a regulatory sanction due to financial misreporting in <i>another</i> client firm.
--------------	------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------

<i>Post</i>	An indicator variable that takes a value of one for the post period and zero otherwise.
<i>Before(t = - 1)</i>	An indicator variable that takes a value of one for the year before the sanction event and zero otherwise.
<i>Post(t = i)</i>	An indicator variable that takes a value of one for the <i>i</i> th year after the sanction event and zero otherwise.
<i>Treat^{Still LQP}</i>	An indicator that takes a value of one if the treatment firm is still audited by the LQP in the post-period and zero otherwise.
<i>Treat^{Retain LQP}</i>	An indicator that takes a value of one if the treatment firm retains the incumbent LQP in the post-period and zero otherwise.
<i>Treat^{Another LQP}</i>	An indicator that takes a value of one if the treatment firm replaces the LQP with another LQP in the post-period and zero otherwise.
<i>Treat^{Dismiss LQP}</i>	An indicator that takes a value of one if the treatment firm replaces the LQP with a high-quality partner in the post-period and zero otherwise.

Control variables

<i>Size</i>	The natural log of a firm's total assets.
<i>Lev</i>	Total liability scaled by total assets.
<i>ROA</i>	Return on assets, net income scaled by total assets.
<i>PPE</i>	Net property, plant, and equipment scaled by total assets.
<i>MB</i>	End-of-year stock price times total shares outstanding scaled by total shareholders' equity
<i>Loss</i>	An indicator that takes a value of one if a firm reports negative net income and zero otherwise.
<i>Dividend</i>	Dividend scaled by total assets.
<i>Return</i>	The cumulative 12-month stock returns in the prior year
<i>Return Volatility</i>	The standard deviation of monthly stock returns in the prior year
<i>Tax Rate</i>	Income tax expense scaled by pre-tax income.
<i>Beta</i>	The CAPM-beta estimated by regressing 36 monthly returns on a value-weighted market return.

References

- Abodia, D., Lin, C., & Petacchi, R. (2015). Capital market consequences of audit partner quality. *The Accounting Review*, 90(6), 2143–2176.
- Baker, M., Stein, J. C., & Wurgler, J. (2003). When does the market matter? Stock prices and the investment of equity-dependent firms. *The Quarterly Journal of Economics*, 118(3), 969–1005.
- Benabou, R., & Laroque, G. (1992). Using privileged information to manipulate markets: Insiders, gurus, and credibility. *The Quarterly Journal of Economics*, 107(3), 921–958.
- Bonner, S. E., Palmrose, Z. V., & Young, S. M. (1998). Fraud type and auditor litigation: An analysis of SEC accounting and auditing enforcement releases. *The Accounting Review*, 73(4), 503–532.
- Chakravarthy, J., deHaan, E., & Rajgopal, S. (2014). Reputation repair after a serious restatement. *The Accounting Review*, 89(4), 1329–1363.
- Chang, X., Dasgupta, S., & Hilary, G. (2009). The effect of auditor quality on financing decisions. *The Accounting Review*, 84(4), 1085–1117.
- Chen, P. C. (2016). Banks' acquisition of private information about financial misreporting. *The Accounting Review*, 91(3), 835–857.

- Chen, X., Cheng, Q., & Lo, A. K. (2013). Accounting restatements and external financing choices. *Contemporary Accounting Research*, 30(2), 750–779.
- Chen, S., Sun, S. Y. J., & Wu, D. (2010). Client importance, institutional improvements, and audit quality in China: An office and individual auditor level analysis. *The Accounting Review*, 85(1), 127–158.
- Cheung, Y., L., Jing, T. Lu, P. R. Rau, A. Stouraitis. 2009. Tunneling and propping up An analysis of related party transactions by Chinese listed companies: *Pacific-Basin Finance Journal* 17(3): 372–393.
- Costello, A. M., & Wittenberg-Moerman, R. (2011). The impact of financial reporting quality on debt contracting: Evidence from internal control weakness reports. *Journal of Accounting Research*, 49(1), 97–136.
- DeFond, M., & Zhang, J. (2014). A review of archival auditing research. *Journal of Accounting & Economics*, 58(2–3), 275–326.
- deHaan, E. (2017). The financial crisis and corporate credit ratings. *The Accounting Review*, 92(4), 161–189.
- Diamond, D. W. (1991). Monitoring and reputation choice between bank loans and directly placed debt. *Journal of Political Economy*, 99, 689–721.
- Doxey, M. M., J. G. Lawson, T. J. Lopez, and Q. Swanquist. 2018. Do investors care who did the audit? Early evidence of the informativeness of form AP. **Working paper**.
- Easton, P. D. (2004). PE ratios, PEG ratios and estimating the implied expected rate of return on equity capital. *The Accounting Review*, 79(1), 73–95.
- Fan, G., X. Wang, and H. Zhu, 2011, NERI Index of Marketization for China's Provinces: 2010 Report, Beijing, China, Economic Science Press **(in Chinese)**.
- Fang, J., Pittman, J., Zhang, Y., & Zhao, Y. (2017). Auditor choice and its implication for group-affiliated firms. *Contemporary Accounting Research*, 34(1), 39–82.
- Francis, J. R. (2011). A framework for understanding and researching audit quality. *Auditing: A Journal of Practice & Theory*, 30(2), 125–152.
- Francis, J., LaFond, R., Olsson, P., & Schipper, K. (2005). The market pricing of accruals quality. *Journal of Accounting and Economics*, 39(2), 295–327.
- Graham, J. R. (1996). Debt and the marginal tax rate. *Journal of Financial Economics*, 41(1), 41–73.
- Graham, J. R., Li, S., & Qiu, J. (2008). Corporate misreporting and bank loan contracting. *Journal of Financial Economics*, 89(1), 44–61.
- Gul, F. A., Wu, D., & Yang, Z. (2013). Do individual auditors affect audit quality? Evidence from archival data. *The Accounting Review*, 88(6), 1993–2023.
- Hail, L., & Leuz, C. (2006). International differences in the cost of equity capital: Do legal institutions and securities regulation matter? *Journal of Accounting Research*, 44(3), 485–531.
- Jian, M., & Wong, T. J. (2010). Propping through related party transactions. *Review of Accounting Studies*, 15(1), 70–105.
- Kedia, S., Khan, U., & Rajgopal, S. (2018). The SEC's enforcement record against auditors. *Journal of Law, Finance and Accounting*, 3(2), 243–289.
- Lennox, C., & Pittman, J. A. (2010). Big five audits and accounting fraud. *Contemporary Accounting Research*, 27(1), 209–247.
- Lennox, C. S., Wu, X., & Zhang, T. (2014). Does mandatory rotation of audit partners improve audit quality? *The Accounting Review*, 89(5), 1775–1803.
- Leuz, C., and Verrecchia, R., 2004. Firms' capital allocation choices, information quality, and the cost of capital. University of Pennsylvania working paper.
- Li, L., Qi, B., Tian, G., & Zhang, G. (2017). The contagion effect of low-quality audits at the level of individual auditors. *The Accounting Review*, 92(1), 137–163.
- Moore, G., & Scott, W. R. (1989). Auditors' legal liability, collusion with management, and investors' loss. *Contemporary Accounting Research*, 5(2), 754–774.
- Myers, S. C. (1977). Determinants of corporate borrowing. *Journal of Financial Economics*, 5(2), 147–175.
- Ng, J., Tuna, I., & Verdi, R. (2013). Management forecast credibility and underreaction to news. *Review of Accounting Studies*, 18(4), 956–986.
- Vashishtha, R. (2014). The role of bank monitoring in borrowers' discretionary disclosure: Evidence from covenant violation. *Journal of Accounting and Economics*, 57(2–3), 176–195.
- Wang, Y., Yu, L., & Zhao, Y. (2015). The association between audit-partner quality and engagement quality: Evidence from financial report misstatements. *Auditing: A Journal of Practice & Theory*, 34(3), 81–111.