Audit process, private information, and insider trading

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Abstract

While the shareholder benefits of audits are well documented, evidence on whether audits can facilitate opportunistic behavior by corporate insiders is scarce. In this paper, we examine whether the audit process facilitates one particular form of opportunism: informed trading by corporate insiders. We focus our analysis on insider trading around the audit report date. We find an increase in trading around the audit report date and that the increase is abnormally large for firms that subsequently report modified opinions. The abnormal increase in trading is concentrated among officers and non-audit committee independent directors, and most pronounced in first-time modified opinions and modified opinions in years where financial results are subsequently restated. These trades are highly opportunistic: they predict restatements, and as a consequence, we show they avoid significant losses. Collectively, our findings provide novel evidence that insiders appear to exploit private information about the audit process—a process ostensibly designed to protect shareholders—for opportunistic gain.

Keywords Audit opinions · Audit process · Audit report · Private information · Insider trading · Opportunism

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1 Introduction

Audit reports—and the requirement that public companies file audited financial state-ments—are a cornerstone of modern corporate governance practices. The Securities Exchange Act of 1934 requires that all public companies disclose audited financial statements and the associated audit findings. While it is generally accepted that financial statement audits reduce information asymmetry and mitigate agency conflicts, managers and directors (hereafter "corporate insiders") are typically aware of audit findings well in advance of the general public. Thus, although a key purpose of audits is to protect stakeholders, an unintended consequence of the audit process is that it endows corporate insiders with an information advantage. While the stakeholder benefits of audits are well documented, evidence on whether audits can facilitate opportunistic behavior by corporate insiders is scarce. In this paper, we examine whether aspects of the audit process facilitate one particular form of opportunism: informed trading.

The audit process represents a negotiation between the external auditor, manage-ment, and the board of directors. A typical audit entails planning and interim procedures during the year, year-end fieldwork around the earnings announcement, and culminates with the preparation of the final audit report. Throughout the audit process, the auditor is in frequent contact with management and the board and provides continuous updates regarding preliminary findings, audit adjustments, and potential modifications to a standard unqualified audit report.[1] These internal communications with auditors represent the first time some officers and directors learn of reporting and control deficiencies (e.g., independent directors learn about issues with revenue recognition). The auditor formally briefs the board on the audit findings close to the date the audit is finalized, or "audit report date" (PCAOB AS 1301), and subsequently discloses the audit report to the public as part of the firm's 10-K filing. The audit report provides the auditor's summary opinion but does not include the detailed findings that were presented to management and the board. Thus, prior to the 10-K filing, officers and directors are aware not only of the contents of the audit report (i.e., the auditor's opinion) but also any detailed findings of the audit that are not conveyed in the report. In many cases—especially ones with adverse outcomes—this information is material (e.g., Ghicas et al. 2008; Gutierrez et al. 2018; Gutierrez et al. 2020).

While there are good reasons to suspect that the audit process provides insiders with private information, it is not obvious that insiders would trade based on this

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[1] The audit process itself generates new information along several dimensions that would not exist in the absence of an audit. For example, the audit provides officers and directors with private information about the auditor's opinion of the financial and internal controls. The auditor's opinion has clear informational value to the market; consequently, private information about that opinion is also valuable. Beyond private information about the opinion itself, the audit process generates new private information related to internal control deficiencies that constitute material weaknesses and detects financial misstatements that either need to be adjusted if material or remain unadjusted if immaterial.

information. Opportunistic use of private information carries significant risks. Corporate officers and directors owe a fiduciary duty to shareholders to "disclose or abstain (from trading)" (Jagolinzer et al. 2011). Consequently, if officers or directors have private information about their firm's financial reporting that was uncovered by the audit, their fiduciary duty compels them to abstain from trading until such time as the information becomes public. In this regard, our tests are joint tests that the audit process provides insiders with an information advantage and that insiders trade based on this advantage.

We examine whether corporate insiders trade based on private information gleaned from the audit process using a standard short-window event study around the audit report date. The audit report date signifies the end of the audit and serves as a reasonable proxy for the latest possible date at which corporate insiders are aware of the final audit findings (PCAOB AS 1301; 3110). Our tests focus on a sample of firms for which the audit report date occurs after the earnings announcement and more than ten days prior to the public disclosure of the report. We focus on audit report dates after the earnings announcement in order to cleanly separate insider trading in conjunction with the audit report from insider trading in conjunction with the earnings announcement. We focus on audit report dates more than ten days prior to the public disclosure of the report in the 10-K to ensure that any trading activity associated with the report is distinct from that associated with the 10-K.[2] In this regard, we select our sample to maximize the ability of our tests to detect opportunistic trading on private information about the audit process.

By examining insider trading in a tight window around the audit report date, these tests mitigate concerns that our results are attributable to either (i) the audit findings themselves being influenced by insider trading (Chen et al. 2013), or (ii) omitted firm characteristics that are correlated with the audit findings. Evidence of a change in insider trading activity in a short window around the audit report date—*when audit findings are known to insiders but not to the market*—suggests that insiders are trading based on private information about audit findings.

We find an abnormally large increase in trading around the audit report date for firms that subsequently report modified opinions. These abnormal patterns in trading disappear shortly before the firm publicly discloses the report in the annual 10-K filing and do not appear in the firm's other quarters. In contrast to the trading activity of insiders, we find no evidence of a capital market reaction on the audit report date, suggesting that insiders are reacting to a private information event.

We conduct an extensive battery of sensitivity tests. For example, we repeat our tests focusing exclusively on *within-firm-quarter* variation in insider trading (i.e., including firm-quarter fixed effects). These tests should alleviate concerns that our results are attributable to omitted firm-quarter characteristics or time trends. To the extent that an omitted variable does not vary *within a given firm-quarter* (e.g., within Firm A's 2009-Q4), this analysis controls for the omitted variable. This design choice is important because it controls for many of the determinants of audit opinions and associated firm-level consequences documented in prior research (e.g., financial distress, innate audit

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[2] The audit report date is not observable in real time and can only be inferred after the audit report is subsequently disclosed. We assess the robustness of our results to alternative selection criteria, e.g., relaxing the restriction on the number of days between the audit report date and 10-K. See Sections 2 and 3 for more detail.

risk, corporate governance, etc.).[3] Focusing exclusively on the timing of trades within the firm-quarter, we continue to find evidence of abnormal trading in firms that subsequently report modified opinions.

Next, we conduct three additional sets of tests to sharpen our empirical identification and triangulate our inferences. First, we examine trading separately for officers and independent directors. We find evidence of abnormal trading in firms that subsequently report modified opinions for senior managers and for independent directors *not* on the audit committee, but no evidence of abnormal trading for independent directors on the audit committee. Notably, we find evidence that independent directors not on the audit committee actively postpone their trades until *after* the audit report date. These results are consistent with the notion that non-audit committee directors learn more about audit findings around the audit report date than audit committee members (who may have been previously aware of key developments in the audit process).

Second, we examine whether the abnormal increase in insider trading in firms with modified opinions varies with the type of opinion. We find that the increase in abnormal trading activity for firms with modified opinions is concentrated in years when the opinion is likely to be unexpected and material—in first-time modified opinions, and in years where financial results are subsequently restated. These results suggest that abnormal trading is larger when the audit process provides insiders with a greater information advantage over the market.

Third, we examine whether abnormal trading around the audit report date is opportunistically timed to avoid losses. We find that these trades are a leading indicator of whether the financial statements for that period will be subsequently restated—conditional on insiders selling in a short window around the audit report date, the probability of restatement increases by over 35% (0.034) relative to the unconditional probability of a restatement (0.09). As a consequence, we show that these trades avoid significant losses.[4] Thus, the audit process appears to convey meaningful private information to insiders about the probability of a subsequent restatement, allowing them to exit their positions and avoid the losses associated with a subsequent restatement. Collectively, our findings provide novel evidence that insiders appear to exploit private information about the audit process—a process ostensibly designed to protect shareholders—for opportunistic gain.

Our findings are subject to three important caveats. First, while we cannot definitively rule out the possibility that our collective results are explained by a correlated omitted variable, it seems unlikely. To explain our collective results, an omitted variable would have to (i) vary with the timing of insider trades within a given firm-quarter relative to the audit report date, (ii) vary with the subsequent audit opinion, (iii) vary with insiders' committee membership (audit committee or not), and (iv) predict subsequent restatements and future abnormal returns. Second, our analysis focuses on a specific sample of firms for which we can identify a reasonable proxy for when audit findings are communicated to the board. Similar to Dechow et al. (2016), we intentionally choose a sample that maximizes our ability to detect trades placed in

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[3] See for example Ashbaugh-Skaife et al. (2007), Ashbaugh-Skaife et al. (2009).

[4] Loss avoidance is approximately 7.2% on an average sale of $3.7 million. Loss avoidance of this magnitude is quite common among insider trading cases pursued by the SEC and Department of Justice (Jagolinzer et al. 2020).

conjunction with the audit, and acknowledge that this potentially limits the generalizability of our findings. Third, similar to the options-backdating literature, from statistical evidence alone, it is difficult to speak to the legality of a given trade or series of trades. This limitation is not unique to our study and applies broadly to the academic literature studying corporate misconduct (see, e.g., Ritter 2008). At a minimum, the behavior we document is evidence that audits can facilitate opportunistic behavior by corporate insiders.

Our research question and findings should be of interest to academics, boards, and regulators. With respect to academics, our study extends a long line of research at the intersection of auditing and corporate governance. While this literature has primarily focused on examining the benefit or value of audits by examining the relationship between cross-sectional differences in audit characteristics and various outcomes (e.g., audit quality, cost of capital, and audit fees), there is scant evidence of managerial opportunism in conjunction with the audit process. In this regard, this is the first paper to suggest and provide evidence that insiders at some firms appear to exploit private information gleaned from the audit process—an important governance mechanism ostensibly designed to protect stakeholders—for personal gain.

Our study also contributes to the academic literature on forensic studies that documents suspicious—if not outright illicit—behavior in capital markets. Examples include Lie (2005) and Heron and Lie (2007) studying option backdating; Cohen et al. (2016) studying insider trading between the date of a material event and the date that event is disclosed in an 8-K filing; Kim (2016) studying insider trading around board meeting dates; Dechow et al. (2016) studying insider trading related to correspondence with the SEC related to revenue recognition; Heitzman and Klasa (2020) studying abnormal options trading activity around nonpublic merger negotiations; Mehta et al. (2021) studying "shadow trading" in peer firms prior to public announcements of focal firms; Blackburne et al. (2020) studying trading around nonpublic SEC investigations; Bianchi et al. (2021) studying firms with board connections to organized crime; and Haselmann et al. (2021) studying whether banks trade on private information about their borrowers. While such studies are ultimately descriptive and do not seek to draw causal inferences, they play an important role in drawing attention to concerning patterns in the data that at best are inconsistent with good corporate governance and at worst are evidence of violations of securities laws.[5]

With respect to boards and corporate governance practitioners, our findings underscore the need for meaningful insider trading policies that restrict the trades of key personnel involved in the audit. While most firms have voluntary trading restrictions (i.e., blackout windows), these restrictions typically expire at the earnings announcement and do not continue through the public disclosure of the audit report and audited financial statements in the 10-K (e.g., Kepler et al. 2020).[6] Thus, to the extent that the 10-K is filed sufficiently long after the earnings announcement—a circumstance where research has shown an

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[5] A recent example of academic evidence informing SEC enforcement decisions comes from Malenko et al. (2020), which contributed to the development of the SEC's EPS Initiative. This initiative uses risk-based analytics to uncover potential accounting violations and EPS rounding (https://www.sec.gov/news/press-release/2020-226).

[6] Our conversations with numerous general counsels and securities lawyers suggest that practitioners generally do not view 10-K filings or audited financials as containing information incremental to the earnings announcement.

increased likelihood of a restatement (e.g., Bhaskar et al. 2019; Marshall et al. 2019) —insiders have an opportunity to trade on private information revealed by the audit but not yet disclosed to the market. Our findings highlight that a detectable mass of insiders take advantage of this opportunity and trade based on information gleaned from the audit process. Consequently, boards should consider restricting the trades of all officers and directors involved with the audit until the findings are publicly disclosed.

With respect to regulators, the Securities and Exchange Commission (SEC) and Public Company Audit Oversight Board (PCAOB) are charged with protecting the interests of individual investors. Consequently, empirical evidence on how the audit process affects insider trading represents an important consideration for these agencies' dual role of enforcement and setting auditing standards and auditing procedures. We encourage auditors, boards, and regulators to scrutinize insider trades placed in conjunction with corporate audits.

The remainder of this paper proceeds as follows. Section 2 discusses institutional features of our setting and related literature. Section 3 describes our sample and measurement choices. Section 4 describes our research design and presents results. Section 5 provides concluding remarks.

2 Institutional background and related literature

2.1 The audit process

The audit reports of most large publicly traded companies contain the auditor's opinion on firms' financial reports and the effectiveness of the firm's internal controls. While most publicly traded companies receive an unqualified opinion on their financial reports, auditors occasionally include additional explanatory language to highlight internal control weaknesses, going concern issues, restatements of prior financial statements, or other matters of emphasis (e.g., an unqualified opinion on financial reports paired with an adverse opinion on internal controls). The literature collectively refers to audit opinions that deviate from a standard unqualified opinion as "modified opinions" and examines the relation between such opinions and a variety of firm outcomes (e.g., Hammersley et al. 2008; Menon and Williams 2010; Kravet et al. 2018). The general consensus of this literature is that modified opinions have significant negative capital market consequences and portend future restatements (Czerney et al. 2014).

It takes auditors several months to conduct the audit and issue their opinion. A typical audit begins in the second half of the year with internal control walkthroughs and testing. Interim testing on specific accounts typically occurs in the third quarter, and year-end fieldwork typically starts one month after the fiscal year-end, when management has completed the closing process for year-end financial statements. The process culminates with the preparation of the audit report, which is usually finalized after the earnings announcement.[7]

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[7] Prior to 2004, it was customary for firms to announce fourth quarter earnings after the completion of the audit report (Bamber et al. 1993). However, since the adoption of PCAOB Auditing Standards No. 2 and 3 in 2004, audits now take approximately 15 days longer. Consequently, recent studies document that most firms (70%) now announce fourth quarter earnings prior to the completion of the audit (Cao et al. 2016; Schroeder 2016; Bhaskar et al. 2019; Marshall et al. 2019).

Throughout the audit process, the auditor is in frequent contact with management and the board and provides continuous updates regarding preliminary findings, audit adjustments, and potential modifications to a standard unqualified audit report. The auditor formally briefs the board on the final audit results close to when the audit report is finalized, or "audit report date" (PCAOB AS 1301; 3110). After the board is briefed, the report is disclosed to the public in Item 8 of the firm's 10-K filing. Notably, however, the audit report will not contain detailed results on all of the audit findings presented to the board.

While we do not observe the auditor's briefing to the board, the information contained in this briefing has the potential to be material. First, the briefing reveals the auditor's opinion of the internal policies, controls, and financial statements. The auditor's opinions on these matters have clear informational value to the market, so private information about them is potentially valuable. For example, research shows that companies with modified opinions are at heightened risk for future restatements (e.g., Czerney et al. 2014). In this regard, the briefing provides insiders with information that is useful for updating their beliefs about the probability of a restatement. The auditor might detect misstatements to the financial statements and propose audit adjustments. It is unlikely that management would have known about these adjustments absent the audit process. Thus, officers and directors have the potential to learn material nonpublic information in conjunction with the audit.

Ideally, we would observe the dates at which audit findings were privately communicated to corporate insiders and investigate insider trading around those specific communications. However, this information is not publicly available.[8] Instead, we rely on the audit report date because auditors are required to brief the board close to this date (PCAOB AS 1301, 3110). We acknowledge the presence of measurement error in this date. Measurement error in event dates biases against finding results in a short-window event study (Berkman and Truong 2009). If anything, empirical evidence of a spike in insider trading around the audit report date that varies predictably with the audit opinion validates that the audit report date measures (with noise) a significant internal information event.

2.2 Related literature on insider trading

It is illegal for insiders to trade while in possession of material nonpublic information (Securities and Exchange Acts of 1933 and 1934; Insider Trading Sanctions Act of 1984; Insider Trading and Securities Fraud Enforcement Act of 1988). However, a large body of research finds that corporate insiders appear to place, and profit from, trades based on superior information (e.g., Brochet 2010; Cohen et al. 2012; Jagolinzer et al. 2020). Within this literature, several studies link insider trading to firm characteristics related to poor accounting quality (e.g., Beneish and Vargus 2002) or poor governance (e.g., Jagolinzer et al. 2011). These studies examine whether insiders extract rents in opaque information environments with weak governance, but they do not examine the specific source of insiders' private information.

For example, Huddart et al. (2007) find that insiders trade more when the trading blackout window is open (i.e., between the earnings announcement and the 10-K filing), but they do not examine how trading over this window relates to the audit, modified opinions, directors' committee membership, restatements, or long-window

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[8] Firms generally do not disclose the dates of board meetings, and the alternative data sources and data vendors we investigated did not have information on the dates of board meetings throughout the year.

future returns. Skaife et al. (2013) find that insiders at firms with internal control weaknesses have a greater information advantage, which they attribute to poor governance and weak "tone at the top." However, they do not examine the source of the information advantage or the role of the audit process.[9] Chen et al. (2013) find that insider sales over the fiscal year are negatively associated with a going concern opinion and interpret this as evidence that these trades influence the conduct of the audit and its outcome (see also Hallman et al. (2020)). By focusing on insider trading in a tight window around the audit report date, our analysis mitigates concerns about reverse causality raised by Chen et al. (2013); insider trades in a short window around the audit report date cannot influence the conduct of the audit.[10] None of these papers examine the specific source of insiders' private information.

One recent exception is Dechow et al. (2016), who show that insiders at firms with significant short interest front-run the public disclosure of SEC comment letters related to revenue recognition. Like Dechow et al. (2016), our analysis is exploratory and descriptive, and we select our sample to maximize the ability of our tests to detect opportunistic trading on private information about the audit process. Unlike Dechow et al. (2016), we focus our analysis on the audit process.

We contribute to this literature by examining whether insiders at some firms appear to exploit private information about the audit process—a process ostensibly designed to protect shareholders—for personal gain. Ours is the first study to suggest that managers may opportunistically exploit the audit process. This is conceptually distinct from the notion that modified audit opinions proxy for opaque information environments or weak corporate governance. In contrast to prior work that relates annual audit outcomes to annual measures of insider trading (e.g., Skaife et al. 2013; Chen et al. 2013), our analysis focuses on the timing of insider trades within the fiscal year relative to the audit report date. We explicitly control for opacity of the information environment and weak corporate governance by exploiting the fact that these characteristics do not vary within a firm-quarter: our tests examine variation in the timing of insider trades within the firm-quarter. We provide novel evidence on (i) how the audit is an important source of some insiders' private information, (ii) the timing of when insiders trade on that information, (iii) who trades on that information, and (iv) the extent to which such trades are opportunistic.

3 Sample construction and variable measurement

3.1 Sample

We collect data on trades of senior managers and directors from the Thomson Reuters Insider Filings Form 4 database. We restrict our analyses to open market purchases and sales of common stock and exclude option exercises, option grants, and equity gifts.

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[9] The notion that insiders trade on private information learned from the audit process is conceptually distinct from the notion that modified audit opinions proxy for opaque information environments or weak corporate governance. Our tests explicitly control for opacity of the information environment and weak corporate governance by exploiting the fact that these characteristics do not vary within a firm-quarter around the audit report date.

[10] The sample excludes 15 firm-years with going concern opinions. In untabulated analyses, we include these observations and find that our inferences are unchanged.

For each transaction, we require the trade price, the number of shares traded, and the date of the trade.[11] We merge the Thomson Reuters Insider Filings database with CRSP and Compustat to obtain data on our control variables. We obtain data on audit opinions from Compustat and Audit Analytics.

We collect data on audit report dates from Audit Analytics. Audit report dates are only observable ex post, after the report is publicly disclosed in the 10-K filing. Audit Analytics collects data on audit report dates by scraping the auditor's report from Item 8 of the 10-K and extracting the date line of the report. To be included in the sample, we require that the audit report date fall *after* the annual earnings announcement and at least ten days *before* the public disclosure of the report. We focus on audit report dates after the earnings announcement in order to cleanly separate insider trading in conjunction with the audit from insider trading in conjunction with the earnings announcement. We focus on audit report dates more than ten days prior to the public disclosure of the report in order to disentangle trades occurring in a short window around the audit report from trades occurring in a short window around the 10-K. In Table IA1, we find that our inferences are unchanged if we alternatively require the audit report date to be N days before the 10-K, where $15 \geq N > 5$. An additional advantage of focusing on audit reports during this period is that prior research suggests that firms' internal restricted trade windows generally end one or two days after the earnings announcement and allow trading during this period.[12] See Fig. 1 for a diagram of the timing of events for our sample and Panel A of Table 1 for the number of observations at each stage of our sample selection criteria. The resulting sample consists of 1948 firm-years from 2003 to 2015.

We argue that our sample selection criteria facilitate the identification of trades placed in conjunction with the audit and increase the power of our tests to detect opportunistic trading related to the audit. However, we readily acknowledge that this potentially limits the generalizability of our findings, as our findings do not generalize to the "average firm." In our research design, we embrace the tradeoff between generalizability and specific identification, and have intentionally chosen the latter. Generalizability is less of a concern given our research question. We do not seek to examine whether insiders at the average firm trade opportunistically in conjunction with the audit but rather whether a detectable set of insiders engage in such behavior when given the opportunity; i.e., we are not seeking to recover estimates of an "average effect" that applies to the population of firms on Compustat.

3.2 Descriptive statistics

Table 1 Panel B presents descriptive statistics for firm characteristics in our sample. The unit of observation in Panel B is the firm-year (sample of 1948 firm-years). *EAtoAR* is the number of days between the earnings announcement and the audit report date. *ARto10K* is the number of days between the audit report date and the 10-K filing. *ModifiedAudit* is an indicator variable that equals one if the audit opinion is anything other than a clean unqualified opinion (e.g., unqualified opinion with additional language, SOX 404b material weaknesses, or SOX 302 material weakness) and zero

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[11] Section 16(b) of the Securities and Exchange Act of 1934 requires that senior managers and directors disclose all trades in the firm's securities on Form 4. The Sarbanes-Oxley Act of 2002 requires that these disclosures be filed electronically with the SEC within two business days.

[12] Using internal data on actual restricted trade windows at 260 firms, Jagolinzer et al. (2011) find that the most common restricted trade window starts (ends) 46 days prior to (1 day after) the earnings announcement.
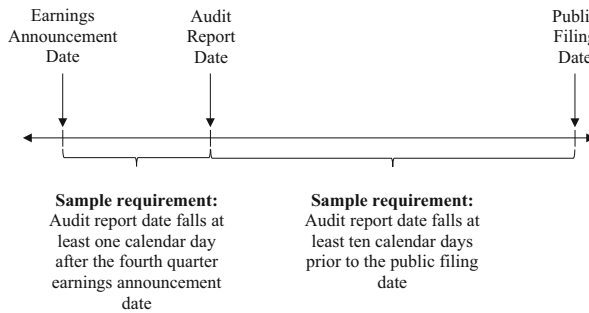
Fig. 1 Event Study Timeline. This figure illustrates the timing of the earnings announcement, audit report date, and the public filing of the audit report in the firm's 10-K for observations in our sample. In our sample, the average audit report date occurs 21 days after the earnings announcement and 21 days before the report is filed with the SEC. See Table 1 for more details

otherwise. *Size* is the natural log of market capitalization. *BM* is book value of equity scaled by market value of equity. *Surprise* is the seasonal random walk earnings surprise scaled by total assets. *AbReturn* is the firm's market-adjusted buy-and-hold return over the fiscal year. *Volatility* is the standard deviation of monthly stock returns over the fiscal year.

Panel B indicates that, on average, firms in our sample provide their annual earnings announcement approximately 21 days before the audit report date (mean *EAtoAR* = 20.64) and that audit reports are filed with the SEC approximately 21 days after the audit report date (mean *ARto10K* = 21.48). Thus, insiders in our sample have (on average) 42 days between the earnings announcement and the 10-K in which to trade—41 accounting for blackout windows that terminate the day after the earnings announcement. These numbers are larger than the universe of firms on Compustat (mean *EAtoAR* = 13.34, mean *ARto10K* = 4.51) because our research design choice requires the audit report date to be at least one calendar day after the EA release date and at least ten calendar days before the 10-K filing.

Panel C presents descriptive statistics for several common measures of insider trading activity for our sample. We calculate daily measures of insider trading activity for all days in a [−30, +30] window around the audit report date for our sample of 1948 firm-years. This results in a sample of 113,854 unique firm-days within 30 days of the audit report date. The unit of observation in Panel B is the firm-day. *InsiderTrade* is an indicator variable equal to one if an insider at the firm traded that day and zero otherwise. *InsiderSeller* is an indicator variable equal to one if insiders at the firm are net sellers on that day and zero otherwise. *InsiderBSI* is the daily insider buy-sell imbalance, calculated as the number of shares bought by insiders minus the number of shares sold by insiders scaled by insider trading volume. Similar to Jagolinzer et al. (2011), *BlackoutPd* measures whether a given day falls within a restricted trade window and is an indicator variable equal to one if the day falls within [−46, +1] days of the firm's earnings announcement and zero otherwise.[13] We also calculate short-window return and volatility measures during the five trading days leading up to each event day (*DailyAbReturn* and *DailyVolatility*, respectively).

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[13] A small fraction of trades in our short-window event study (2.8%) are classified as "routine" according to prevailing indicators in the literature (e.g., Jagolinzer 2009; Cohen et al. 2012). The notion that most of the trades in our event study are non-routine is consistent with such trades being informed. In untabulated analysis, we find that excluding these trades does not affect our inferences.

Table 1 Descriptive Statistics

Panel A. Sample Selection

N-obs

53,009	Compustat/CRSP universe without missing controls
36,014	Audit report date after EA
6242	Audit report date at least 10 days prior to 10-K
1963	Audit report date after EA and at least 10 days prior to 10-K
1948	Audit report date after EA and at least 10 days prior to 10-K; after excluding going concern opinions
113,854	Unique firm-days in [−30,+30] days around investigation opening

Panel B. Firm Characteristics

	Sample (1948 firm-years)		Compustat/Audit Analytics (53,009 firm-years)	
Variable	Mean	Median	Mean	Median
EAtoAR	20.643	19.000	13.342***	9.000***
ARto10K	21.476	18.000	4.507***	0.000***
ModifiedAudit	0.376	0.000	0.416	0.000
Size	6.108	5.918	6.160	6.151***
BM	0.575	0.496	0.628*	0.506
Surprise	0.010	0.000	−0.004**	0.000
AbReturn	0.133	−0.011	0.040	−0.038***
Volatility	0.105	0.082	0.125*	0.103***

Panel C. Daily Market Activity

Variable	Mean	Median	Std	N-obs
InsiderTrade	0.045	0.000	0.208	113,854
InsiderSeller	0.035	0.000	0.184	113,854
InsiderBSI	−0.025	0.000	0.211	113,854
BlackoutPd	0.284	0.000	0.451	113,854
DailyAbReturn	0.002	−0.001	0.054	113,854
DailyVolatility	0.023	0.018	0.017	113,854

Panel D. Univariate Differences in Insider Trading Activity around the Audit Report Date

	Average Pr(InsiderTrade)	Average Pr(InsiderSeller) ModifiedAudit		
Event Period		= 0	= 1	Difference
−30 to +30	0.045	0.032	0.040	0.008***
−30 to −16	0.027	0.019	0.017	−0.002
−15 to −11	0.045	0.029	0.038	0.009**
−10 to −6	0.065	0.047	0.059	0.013***
−5 to −1	0.074	0.051	0.067	0.016***
0 to +5	0.078	0.052	0.082	0.030***
+6 to +10	0.064	0.045	0.065	0.020***
+11 to +15	0.045	0.033	0.040	0.007*
+16 to +30	0.028	0.022	0.023	0.002

This table presents descriptive statistics for the variables used in our analysis. Panel A outlines our event study sample selection procedure. Panel B presents the distribution of firm characteristics for our sample and the intersection of firms in the Compustat/Audit Analytics universe with non-missing control variables. Panel C presents the distribution of common measures of market activity. Panel D presents average insider trading activity around the audit report date and the difference between firms with clean audit opinions and modified audit opinions. The unit of analysis in panels A and B is the firm-year, and the unit of analysis in panels C and D is the firm-day. Sample of 1948 unique firm-years from 2003 to 2015 and 113,854 unique firm-days within 30 days of the audit report date. *, **, and *** indicate statistical significance of the difference (two-sided) at the 0.1, 0.05, and 0.01 levels, respectively

EAtoAR is the number of days between the audit report date and the earnings announcement (we include the natural log of this variable, *LnEAtoAR*, in all of our subsequent empirical tests). *ARto10K* is the number of days between the audit report date and the 10-K filing. *ModifiedAudit* is an indicator variable that equals one if the audit opinion is anything other than a clean unqualified opinion, excluding going concern opinions (e.g., unqualified with additional language, SOX 404b material weaknesses, and SOX 302 material weakness) and zero otherwise. *Size* is the natural log of market capitalization. *BM* is book value of equity scaled by market value of equity. *Surprise* is the seasonal random walk earnings surprise scaled by total assets. *AbReturn* is the firm's market-adjusted buy-and-hold return over the fiscal year. *Volatility* is the standard deviation of monthly stock returns over the fiscal year. *InsiderTrade* is an indicator variable equal to one if an insider at the firm traded that day and zero otherwise. *InsiderSeller* is an indicator variable equal to one if insiders at the firm are net sellers on that day and zero otherwise. *InsiderBSI* is the daily insider buy-sell imbalance, calculated as the number of shares bought by insiders minus the number of shares sold by insiders scaled by insider trading volume. *BlackoutPd* is an indicator variable equal to one if the day falls within $[-46, +1]$ days of the firm's earnings announcement and zero otherwise. *DailyAbReturn* is the firm's market adjusted buy-and-hold return over the $[-5,0]$ days around the firm-day. *DailyVolatility* is the standard deviation of daily stock returns during the $[-5,0]$ days around the firm-day. All continuous variables are winsorized at the 1st and 99th percentiles

Panel C indicates that insiders trade on 4.5% of days in our sample (mean *InsiderTrade* = 0.045) and are net sellers on 3.5% of days in our sample (mean *InsiderSeller* = 0.035). These statistics are consistent with prior research that finds that a natural tendency among insiders is to sell shares to diversify their equity holdings in the firm (e.g., Brochet 2010).

Panel D presents average values of insider trading activity in five-day intervals around the audit report date. Panel D indicates that the base rate of insider trading is 4.5% per day (i.e., on any given day there is a 4.5% probability of an insider trade). In the $[0, +5]$ window around the audit report date, the probability of an insider trade increases to 7.8% per day, suggesting that insiders are nearly twice as likely to trade shares in this window. Panel D also indicates that insiders at firms receiving clean (modified) opinions are net sellers on 3% (4%) of days. However, in the $[0, +5]$ window around the audit report date, insiders at firms receiving clean (modified) opinions are net sellers on 5% (8%) of days. Thus, around the audit report date, the difference in the selling between insiders at firms receiving clean opinions and insiders at firms receiving modified opinions roughly triples, from 0.8% over the full sample $(0.008 = 0.040–0.032)$ to 3.0% $(0.030 = 0.082–0.052)$ in the $[0,+5]$ window.

4 Empirical tests and results

4.1 Insider selling around the audit report date

We examine insider trading activity around the audit report date conditional on whether the firm subsequently reports a modified audit opinion. If insiders trade based on

A. Probability of an Insider Sale

B. Insider Buy-Sell Imbalance
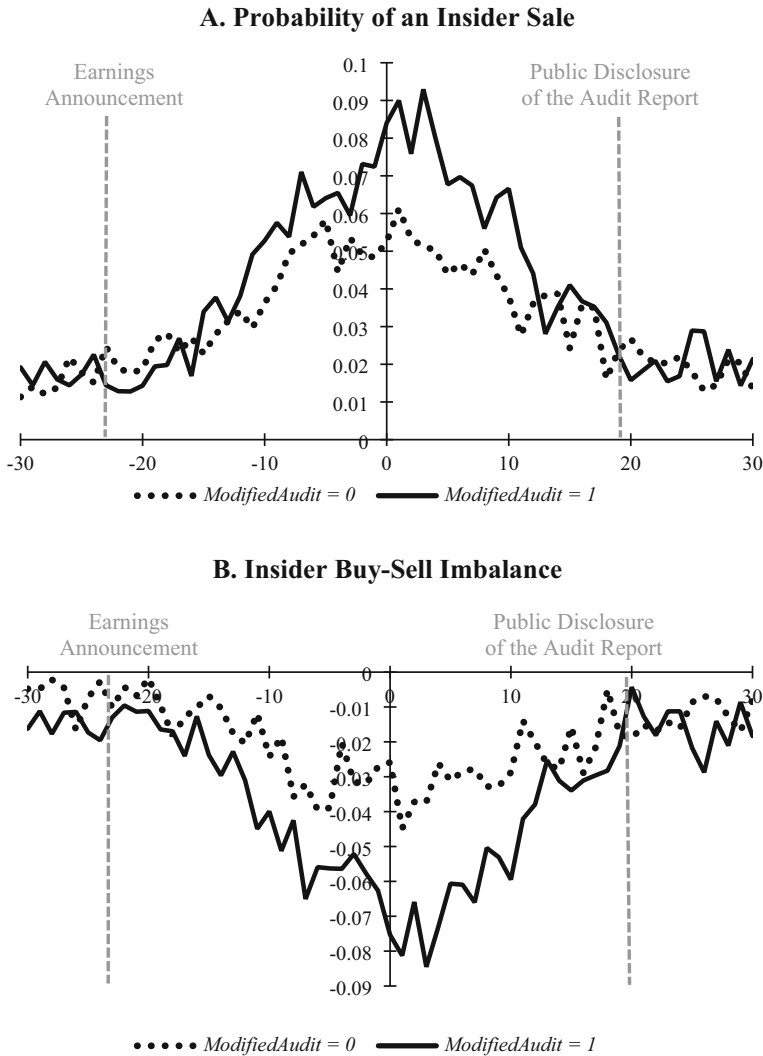
Fig. 2 Insider Trading around the Audit Report Date: Signed Trading. Panel A. Probability of an Insider Sale. Panel B. Insider Buy-Sell Imbalance. This figure plots average signed insider trading activity in the [−30, +30] window around the audit report date separately for firms with clean audit opinions (*ModifiedAudit* = 0) and modified audit opinions (*ModifiedAudit* = 1). Day 0 represents the audit report date, and the vertical dashes represent the average firm's earnings announcement (Day −21) and public disclosure of the report (Day +21). Panel A presents the daily probability that insiders are net sellers (*InsiderSeller*). Panel B presents the daily insider buy-sell imbalance (*InsiderBSI*). All variables are as defined in Table 1. Sample of 113,854 unique firm-days in the [−30, +30] window around the audit report date

private information, then we expect to see a larger increase in selling for firms that subsequently report modified opinions. Panel A of Fig. 2 plots the probability that insiders at the firm are net sellers on the respective day (*InsiderSeller*) separately for the baseline sample of firms with clean opinions and the sample of firms with modified opinions. Panel B of Fig. 2 plots the insider buy-sell imbalance (*InsiderBSI*) separately for these two groups. Both panels indicate an increase in insider selling for all firms.

These figures present the difference in trading each day around the audit report and thus provide a sense of the robustness of our results to alternative event windows.

For firms with clean opinions, the average probability of an insider sale on any given day is 3.2% (see Panel D of Table 1), and this probability increases to 5.3% on the audit report date (a difference on 2.1%). For firms with modified opinions, the average probability of an insider sale on any given day is 4% (see Panel D of Table 1), and this probability increases to 8.4% on the audit report date (a difference of 4.4%). These figures show that the increase in selling for firms that subsequently report a modified opinion is twice that of firms with clean opinions (the difference in differences is 4.4% − 2.1% = 2.3%).

To test whether this difference is statistically significant and robust to controlling for various firm characteristics that are known to be associated with insider trading, we estimate the following regression, pooling across all firm-days in the [−30, +30] window around the audit report date:

$$InsiderSeller \text{ or } InsiderBSI = \alpha + \beta_1 \, Day[-5, +5]^* \, ModifiedAudit$$
$$+ \beta_2 \, ModifiedAudit + \beta_3 \, Day[-5, +5] + \theta \, Controls$$
$$+ \varepsilon. \tag{1}$$

$Day[-5, +5]$ is an indicator variable equal to one if the day falls within five days of the firm's audit report date and zero otherwise. *ModifiedAudit* is an indicator for whether the firm subsequently reports a modified audit opinion. *Controls* is a vector of control variables, including *BlackoutPd*, *dailyAbReturn*, *dailyVolatility*, *LnEAtoAR*, *Size*, *BM*, *Surprise*, *AbReturn*, and *Volatility* (e.g., Blackburne et al. 2020). *BlackoutPd* and *LnEAtoAR* control for differences in corporate insiders' opportunity to trade. The *dailyAbReturn*, *dailyVolatility*, *Surprise*, *AbReturn*, and *Volatility* variables all control for contemporaneous public news that drives trading behavior by corporate insiders. *Size* and *BM* control for the impact of market values and growth opportunities on managers' trading decisions. All variables are defined in Table 1.

The coefficient of interest in Eq. (1) is β_1, which represents the increase in the difference between firms with clean and modified opinions during the event window (i.e., β_1 is analogous to a difference-in-difference estimate). Thus, β_1 compares the increased selling around the audit report for firms with modified opinions vs. firms with unmodified opinions. If insiders trade based on private information about audit findings around the audit report date, we predict that $\beta_1 > 0$ when the dependent variable is the probability of a sale and that $\beta_1 < 0$ when the dependent variable is the buy-sell imbalance.[14]

We estimate two versions of Eq. (1). We estimate the first version using pooled regressions and the second version after including firm-quarter fixed effects. The latter specification focuses exclusively on within-firm-quarter variation in insider trading. The firm-quarter fixed effects subsume any variables that do not vary over time *within the firm-quarter* (e.g., within Firm A's 2009-Q4). These fixed effects subsume all variables that are measured at either an annual or quarterly frequency. Throughout our analyses, we estimate regressions using linear models and calculate standard errors clustered by firm and date,

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[14] When *InsiderBSI* is the dependent variable, a negative coefficient indicates greater net selling, and a positive coefficient represents less net selling.

which allows for arbitrary correlation across time within a given firm as well as arbitrary correlation across firms within a given date.[15] Table 2 presents the results. Across all specifications, we find that the difference in insider selling between firms with clean opinions and firms with modified opinions increases around the audit report date (*Day[−5, +5] * ModifiedAudit*, *t*-stats 3.37, 3.50, −3.86, and − 3.82, respectively).[16]

4.2 Falsification tests

4.2.1 Insider trading around the public disclosure of the audit report

We repeat our earlier short-window event study tests focusing on insider trading around the public disclosure of the report in the 10-K rather than the audit report date. These tests can be viewed as falsification tests. If insiders trade strategically, we do not expect to find evidence of opportunistic trading on audit findings shortly before the report is publicly disclosed, because trades placed immediately prior to public disclosures are subject to considerable legal scrutiny.

Table 3 Panel A presents results from estimating Eq. (1) over the [−30, +30] window around the public disclosure of the report, where day 0 corresponds to the public filing date and *Day[−5, +5]* is an indicator variable equal to one if the day falls within five days of the public filing date (zero otherwise).[17] All other variables are as previously defined. Similar to Huddart et al. (2007), we do not find any detectable evidence of elevated abnormal insider trading around the 10-K (*t*-stats range from −1.54 to 1.20). These results suggest that insiders do not appear to trade immediately prior to the public disclosure of the report, but rather trade in close proximity to when the report is finalized (and not yet publicly available).

4.2.2 Third-quarter placebo test

Next, we examine whether the abnormal trading patterns we document are also present in other quarters. Specifically, we re-estimate Eq. (1) using data from the third quarter (Q3) and assuming that the placebo audit report date is *N* days after the Q3 earnings announcement (where *N* is the number of days between the Q4 earnings announcement and the audit report date). Table 3 Panel B presents the results. Across all specifications, the coefficient on *Day[−5,+5]*ModifiedAudit* is statistically indistinguishable from zero (*t*-stats range from

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[15] We follow Armstrong et al. (2022) and estimate regressions using linear models for two reasons. First, Greene (2004) and Arellano and Hahn (2007) raise concerns about bias and consistency of probit and logit models with high-dimensional fixed effects, and our analysis relies on such effects. Second, Ai and Norton (2003) show that in probit and logit models, interaction terms do not represent marginal effects; and our subsequent analysis relies on interaction terms. Nevertheless, in untabulated analyses, our inferences are robust to estimating results using probit and logit models.

[16] In Table IA2, we consider an alternative research design that estimates a first-stage model of "expected" audit outcomes based on a vector of firm characteristics, uses the residual from this first stage model to measure "unexpected" audit outcomes (which we expect to entail relatively new information to managers), and replaces *ModifiedAudit* in Eq. (2) with the residual from the first-stage model. We implement this design estimating *ModifiedAudit* as a function of lagged *ModifiedAudit* and the control variables in prior literature. Our inferences are unchanged.

[17] To ensure that any trading around the public filing is not confounded by trading around the audit report date, we require that the audit report date fall outside the [−30, +30] window. This results in a sample of 19,233 unique firm-days.

Table 2 Insider Trading around the Audit Report Date

Variable	Dependent Variable: Pr(InsiderSeller$_{i,t}$)		Dependent Variable: InsiderBSI$_{i,t}$	
	Pooled (1)	Within firm-quarter (2)	Pooled (3)	Within firm-quarter(4)
Day[−5, +5]*ModifiedAudit	0.018***	0.019***	−0.022***	−0.022***
	(3.37)	(3.50)	(−3.86)	(−3.82)
Controls				
ModifiedAudit	−0.005	.	0.002	.
	(−1.48)	.	(0.64)	.
Day[−5, +5]	0.016***	0.012***	−0.010***	−0.008**
	(5.35)	(3.83)	(−2.90)	(−2.25)
BlackoutPd	−0.031***	−0.037***	0.023***	0.027***
	(−13.78)	(−13.47)	(9.54)	(8.93)
DailyAbReturn	0.135***	0.083***	−0.152***	−0.086***
	(8.03)	(5.85)	(−7.81)	(−5.02)
DailyVolatility	0.096*	0.226***	0.108	−0.095
	(1.76)	(4.04)	(1.41)	(−1.35)
LnEAtoAR	−0.007***	.	0.008***	.
	(−4.27)	.	(4.70)	.
Size	0.007***	.	−0.009***	.
	(6.77)	.	(−7.54)	.
BM	−0.012***	.	0.017***	.
	(−4.07)	.	(4.36)	.
Surprise	0.003	.	−0.006	.
	(0.12)	.	(−0.27)	.
AbReturn	0.009**	.	−0.010**	.
	(2.43)	.	(−2.54)	.
Volatility	−0.038	.	−0.019	.
	(−1.60)	.	(−0.73)	.
Fixed Effects	none	firm-quarter	none	firm-quarter
F	25.75	50.97	23.84	25.30
N-obs	113,854	113,854	113,854	113,854

This table presents results from estimating Eq. (1) using both pooled and within-firm-quarter designs. Columns (1) and (2) present results when the dependent variable is the probability that insiders are net sellers (*InsiderSeller*). Columns (3) and (4) present results when the dependent variable is the insider buy-sell imbalance (*InsiderBSI*). Columns (2) and (4) present results from including firm-quarter fixed effects. *Day[−5, +5]* is an indicator variable equal to one if the day falls within [−5, +5] of the firm's audit report date and zero otherwise. *ModifiedAudit* is an indicator variable that equals one if the audit opinion is anything other than a clean unqualified opinion and zero otherwise. All other variables are as defined in Table 1. Firm-quarter fixed effects subsume the coefficients on all of our control variables except for *BlackoutPd*, *DailyAbReturn*, *DailyVolatility*, and *Day[−5, +5]*. *t*-statistics appear in parentheses and are clustered by firm and date. *, **, and *** indicate statistical significance (two-sided) at the 0.1, 0.05, and 0.01 levels, respectively. Sample of 113,854 unique firm-days in the [−30, +30] window around the audit report date

0.03 to 0.49), consistent with insiders at firms with modified opinions trading similarly to insiders at firms with clean opinions in Q3 (when no audit report is provided).

4.2.3 Stock market reaction around the audit report date

Our analysis presupposes that *nonpublic* information motivates insiders' trades. Consequently, we do not expect a capital market reaction (i.e., changes in price or public trading volume) on the audit report date. However, an alternative explanation is that *public* information events systematically occur in the short window around the audit report date and that insiders are trading in response to these *public* events. Under this alternative explanation, we would expect to observe changes in stock price and trading volume around the audit report date.

We test for changes in stock price and trading volume by estimating the following short-window event study design:

$$PublicVolume \text{ or } |Ret_{i,t}| = \alpha + \beta_1 \, Day[-5, +5] + \theta \, Controls + \varepsilon, \qquad (2)$$

where *PublicVolume* is daily CRSP trading volume less daily insider trading volume, scaled by shares outstanding, and normalized using the sample average and standard deviation; $|Ret_{i,t}|$ is the absolute value of the daily buy-and-hold returns listed on CRSP (inclusive of dividends); and all other variables are as previously defined.

Table 3 Panel C presents results. Consistent with the *nonpublic* nature of the audit findings, we find no evidence of a capital market reaction. In particular, across all specifications, and regardless of whether we use public trading volume or unsigned price changes to measure information content, the coefficient on *Day[−5, +5]* is not significantly different from zero at conventional levels (*t*-stats range from −0.74 to 0.88). These results suggest that abnormal trading activity around the audit report date is unique to corporate insiders and highlight the nonpublic nature of the audit findings at the time of the audit report date.

4.3 Alternative specifications

4.3.1 Alternative fixed effects

Next, we consider the robustness of our inferences to empirical specifications that include alternative fixed effects when estimating Eq. (1). Table 4 Panel A presents the results. Across all specifications, we find that our inferences are unchanged if we additionally include (i) a vector of fixed effects based on the distance, in days, between the observation and the earnings announcement (e.g., separate indicator variables if the observation is 13 days after the earnings announcement, 12 days after the earnings announcement, etc.), and (ii) a vector of fixed effects for the distance, in days, between the observation and the 10-K filing (e.g., separate indicator variables if the observation is 30 days before the 10-K, 29 days before the 10-K, etc.).

4.3.2 Alternative windows.

Next, we differentiate between trades placed before and after the audit report date. Specifically, we first estimate Eq. (1) after replacing the event window indicator, *Day[−5, +5]*, with the following vector of event window indicators: *Day[−10, −6]*,

Table 3 Placebo Tests

Panel A. Insider Trading around Public Disclosure of the Audit Report

Variable	Dependent Variable: Pr(InsiderSeller$_{i,t}$)		Dependent Variable: InsiderBSI$_{i,t}$	
	Pooled (1)	Within firm-quarter (2)	Pooled (3)	Within firm-quarter (4)
Day[–5, +5]*ModifiedAudit	–0.009	–0.016	0.009	0.014
	(–1.03)	(–1.54)	(0.90)	(1.20)
Controls	yes	yes	yes	yes
Fixed Effects	none	firm-quarter	none	firm-quarter
F	3.743	6.024	4.463	4.425
N-obs	19,233	19,233	19,233	19,233

Panel B. Insider Trading around Third Quarter Placebo Audit Report Date

Variable	Dependent Variable: Pr(InsiderSeller$_{i,t}$)		Dependent Variable: InsiderBSI$_{i,t}$	
	Pooled (1)	Within firm-quarter (2)	Pooled (3)	Within firm-quarter (4)
Day[–5, +5]*ModifiedAudit	0.002	0.002	0.000	0.000
	(0.49)	(0.46)	(0.03)	(0.04)
Controls	yes	yes	yes	yes
Fixed Effects	none	firm-quarter	none	firm-quarter
F	20.37	39.29	21.13	21.39
N-obs	114,056	114,056	114,056	114,056

Panel C. Stock Market Reaction around the Audit Report Date

Variable	Dependent Variable: PublicVolume$_{i,t}$		Dependent Variable: |Ret$_{i,t}$|	
	Pooled (1)	Within firm-quarter (2)	Pooled (3)	Within firm-quarter (4)
Day[–5, +5]	–0.010	0.000	–0.000	0.000
	(–0.74)	(0.06)	(–0.41)	(0.88)
Controls	yes	yes	yes	yes
Fixed Effects	none	firm-quarter	none	firm-quarter
F	57.06	35.86	1007	642.3
N-obs	113,854	113,854	113,854	113,854

This table presents results from our three placebo tests. Panel A presents results from estimating Eq. (1) over the [–30, +30] window around the public disclosure of the audit report. Day 0 corresponds to the public filing date. Columns (1) and (2) of each panel present results when the dependent variable is the probability that insiders are net sellers (*InsiderSeller*). Columns (3) and (4) present results when the dependent variable is the insider buy-sell imbalance (*InsiderBSI*). *Day[–5, +5]* is an indicator variable equal to one if the day falls within [–5, +5] of the public filing date and zero otherwise. *ModifiedAudit* is an indicator variable that equals one if the audit opinion is anything other than a clean unqualified opinion and zero otherwise. Sample of 19,233 unique firm-days in the [–30, +30] window around the public disclosure of the report, after excluding observations where the audit report date falls within this window

Panel B presents results from estimating Eq. (1) over the [–30, +30] window around a placebo date for Q3. The placebo audit report date occurs the same number days after the Q3 earnings announcement that the actual audit report date occurs after the Q4 earnings announcement during the same fiscal year. Day 0 corresponds to the placebo audit report date. Columns (1) and (2) present results when the dependent variable is the probability that insiders are net sellers (*InsiderSeller*). Columns (3) and (4) present results when the dependent

variable is the insider buy-sell imbalance (*InsiderBSI*). *Day[–5, +5]* is an indicator variable equal to one if the day falls within [–5, +5] of the public filing date and zero otherwise. *ModifiedAudit* is an indicator variable that equals one if the audit opinion is anything other than a clean unqualified opinion and zero otherwise. Sample of 114,056 unique firm-days in the [–30, +30] window around the third quarter placebo audit report date

Panel C presents results from estimating Eq. (3) using both pooled and within-firm-quarter designs. Columns (1) and (2) present results when the dependent variable is public trading volume (*PublicVolume*). Columns (3) and (4) present results when the dependent variable is absolute value of daily returns ($|Ret_{i,t}|$). *PublicVolume* is daily CRSP trading volume less daily insider trading volume, scaled by shares outstanding, and normalized using the sample average and standard deviation. $|Ret_{i,t}|$ is the absolute value of the daily buy-and-hold returns listed on CRSP (inclusive of dividends). *Day[–5, +5]* is an indicator variable equal to one if the day falls within [–5, +5] of the firm's audit report date and zero otherwise. All other variables are as defined in Table 1. Sample of 113,854 unique firm-days in the [–30, +30] window around the audit report date

Columns (2) and (4) of each panel present results from including firm-quarter fixed effects. For parsimony, we do not tabulate coefficients on control variables or main effects. *t*-statistics appear in parentheses and are clustered by firm and date. *, **, and *** indicate statistical significance (two-sided) at the 0.1, 0.05, and 0.01 levels, respectively

Day[−5, −1], *Day[0, +5]*, and *Day[+6, +10]*. Each of these variables represents an indicator variable equal to one if the day falls in the respective window relative to the audit report date and zero otherwise. Table 4 Panel B presents the results. Across all specifications, we detect limited evidence of abnormal trading more than five days prior to the audit report date and find that abnormal trading is greatest in the five-days after the audit report date (*Day[0,+5]*ModifiedAudit*, *t*-stats 3.71, 3.85, −4.04, and − 4.02, respectively) and remains elevated over the [+6, +10] window (*Day[+6,+10]*ModifiedAudit*, *t*-stats 2.47, 2.65, −2.51, −2.52, respectively).

Next, we estimate Eq. (1) after replacing the event window indicator, *Day[−5, +5]*, with the following vector of event window indicators: *Day[−2, −1]* and *Day[0, +2]*. Each of these variables represents an indicator variable equal to one if the day falls in the respective window relative to the audit report date and zero otherwise. Table 4 Panel C presents the results. Across all specifications, we continue to find that abnormal trading is most pronounced in the narrow two-day window after the audit report date (*Day[0,+2]*ModifiedAudit*, *t*-stats 2.38, 2.40, −2.69, and − 2.62, respectively).

4.4 Who trades around the audit report date?

We examine insider trading around the audit report date separately for officers, independent directors on the audit committee, and independent directors not on the audit committee. Panel A of Table 5 presents results for trades placed by officers (i.e., *InsiderSeller_Officer* and *InsiderBSI_Officer*). We find that significant abnormal trading by officers begins during the [−5, −1] window and continues through the [+6, +10] window. Panels B and C present the results after distinguishing between independent directors that do and do not sit on the audit committee. Panel B presents the results for trades placed by independent directors on the audit committee (i.e., *InsiderSeller_Audit* and *InsiderBSI_Audit*).[18] We find no evidence of significant abnormal trading by audit committee members around the audit report date. Panel C presents results for independent directors not on the audit committee (i.e., *InsiderSeller_NonAudit* and

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[18] For this analysis, we manually match director names on Thomson Reuters to BoardEx to identify the independent directors that sit on the audit committee during the year.

Table 4 Insider Trading around the Audit Report Date: Alternative Specifications

Panel A. Alternative Fixed Effects

Variable	Dependent Variable: Pr(InsiderSeller$_{i,t}$)			Dependent Variable: InsiderBSI$_{i,t}$		
	(1)	(2)	(3)	(4)	(5)	(6)
Day[−5, +5]*ModifiedAudit	0.016***	0.014***	0.014**	−0.020***	−0.019***	−0.017***
	(3.03)	(2.68)	(2.55)	(−3.49)	(−3.24)	(−3.06)
Controls	yes	Yes	yes	yes	yes	yes
Firm-quarter Fixed Effects	yes	Yes	yes	yes	yes	yes
Days-from-10 K Fixed Effects	yes	No	yes	yes	no	yes
Days-from-EA Fixed Effects	no	Yes	yes	no	yes	yes
F	52.46	12.28	10.69	18.62	10.64	8.683
N-obs	113,854	113,854	113,854	113,854	113,854	113,854

Panel B. Extended Windows

Variable	Dependent Variable: Pr(InsiderSeller$_{i,t}$)		Dependent Variable: InsiderBSI$_{i,t}$	
	Pooled (1)	Within firm-quarter (2)	Pooled (3)	Within firm-quarter (4)
Day[−10, −6]*ModifiedAudit	0.012*	0.013*	−0.013*	−0.013*
	(1.74)	(1.92)	(−1.86)	(−1.86)
Day[−5, −1]*ModifiedAudit	0.015**	0.016**	−0.020***	−0.020***
	(2.28)	(2.43)	(−2.73)	(−2.70)
Day[0, +5]*ModifiedAudit	0.026***	0.028***	−0.030***	−0.031***
	(3.71)	(3.85)	(−4.04)	(−4.02)
Day[+6, +10]*ModifiedAudit	0.017**	0.018***	−0.019**	−0.019**
	(2.47)	(2.65)	(−2.51)	(−2.52)
Controls	yes	yes	yes	yes
Fixed Effects	none	firm-quarter	none	firm-quarter
F	18.47	25.63	16.66	13.04
N-obs	113,854	113,854	113,854	113,854

Panel C. Narrowed Windows

Variable	Dependent Variable: Pr(InsiderSeller$_{i,t}$)		Dependent Variable: InsiderBSI$_{i,t}$	
	Pooled (1)	Within firm-quarter (2)	Pooled (3)	Within firm-quarter (4)
Day[−2, −1]*ModifiedAudit	0.018**	0.018**	−0.017*	−0.017*
	(2.27)	(2.27)	(−1.95)	(−1.88)
Day[0,+2]*ModifiedAudit	0.019**	0.020**	−0.023***	−0.023***
	(2.38)	(2.40)	(−2.69)	(−2.62)
Controls	yes	yes	yes	yes
Fixed Effects	none	firm-quarter	none	firm-quarter
F	21.32	35.04	19.98	16.39
N-obs	113,854	113,854	113,854	113,854

This table presents results from estimating Eq. (1) using alternative specifications. Panel A presents results from re-estimating Table 2 after including fixed effects for the number of days between the observation and the 10-K filing (*Days-from-10-K*) and the number of days between the observation and the earnings announcement (*Days-from-EA*). Panel B presents results using wider windows, and Panel C presents results for narrower windows. *Day[−10, −6]*, *Day[−5, −1]*, *Day[0, +5]*, *Day[+6, +10]*, *Day[−2, −1]*, and *Day[0,+2]* are indicator variables equal to one if the day falls in the respective window relative to the audit report date and zero otherwise. *ModifiedAudit* is an indicator variable that equals one if the audit opinion is anything other than a clean unqualified opinion and zero otherwise. All other variables and specifications follow Table 2. For parsimony, we do not tabulate coefficients on control variables or main effects. *t*-statistics appear in parentheses and are clustered by firm and date. *, **, and *** indicate statistical significance (two-sided) at the 0.1, 0.05, and 0.01 levels, respectively

InsiderBSI_NonAudit). Interestingly, we find some evidence that *non-audit committee directors* postpone their trades until after the auditor's briefing. For example, there is an abnormal decrease in net selling (*InsiderSeller_NonAudit*) over the five days *before* the audit report date (*Day[−5, −1]*ModifiedAudit*, *t*-stats −2.17, −2.29), and an abnormal increase in net selling over the five days *after* the audit report date (*Day[0, +5]*ModifiedAudit*, *t*-stats 2.04, 2.12). These results are consistent with the notion that non-audit committee directors learn more about audit findings around the audit report date than audit committee members (who may have been previously aware of key developments in the audit process).

4.5 Insider trading around the audit report date: types of audit modifications

Next, to investigate the types of information generated by the audit process that insiders can exploit, we examine whether the abnormal patterns in trading activity differ based on the type of modified opinion. Specifically, we focus on audit opinions that prior research suggests are material audit outcomes, and sort the modified audit opinions into groups based on whether they (i) are first-time modifications (*Modified_First* and *Modified_NoFirst*), and (ii) occur in a year where financial results are subsequently restated (*Modified_R* and *Modified_NoR*).[19] Panel A of Table 6 shows the number of firms with each type of modified opinion in our sample.

Panel B presents results from estimating Eq. (1) after decomposing *ModifiedAudit* into *Modified_First* and *Modified_NoFirst*.[20] Relative to firms with clean opinions (the baseline), we find that abnormal trading is generally concentrated in first-time modified opinions. Comparing trading between the two types of modified opinions, we find that abnormal trading in the [+6, +10] window is ten times larger for first-time modified opinions than for non-first-time modified opinions (*p* values test of differences in coefficients ≤0.10 in all but one specification).

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[19] In each group, the binary indicator variables are mutually exclusive and sum to *ModifiedAudit*. For example, *Modified_NoFirst* + *Modified_First* = *ModifiedAudit*.

[20] In this specification, the coefficients on the interaction terms measure the difference-in-differences between firms with clean opinions and firms with a particular type of modification. For example, the coefficient on *Day[0,+5]*Modified_First* measures the difference in the increase in trading around the audit report date for material first-time modified opinions, relative to the increase in trading around the audit report date for clean opinions. As such, each type of modified opinion has its own difference-in-differences estimator. We then test for a difference in these estimators across the types of opinions (e.g., *p* value test *Day[0, +5]*Modified_First* = *Day[0,+5]*Modified_NoFirst*), which effectively represents a triple-differences design.

Table 5 Who Trades around the Audit Report?

Panel A. Officers

Variable	Dependent Variable: Pr(InsiderSeller_Officer$_{i,t}$)		Dependent Variable: InsiderBSI_Officer$_{i,t}$	
	Pooled (1)	Within firm-quarter (2)	Pooled (3)	Within firm-quarter (4)
Day[−10, −6]*ModifiedAudit	0.009	0.009	−0.008	−0.007
	(1.50)	(1.52)	(−1.27)	(−1.23)
Day[−5, −1]*ModifiedAudit	0.018***	0.019***	−0.021***	−0.021***
	(3.05)	(3.10)	(−3.31)	(−3.27)
Day[0, +5]*ModifiedAudit	0.021***	0.021***	−0.020***	−0.020***
	(3.50)	(3.52)	(−3.21)	(−3.16)
Day[+6, +10]*ModifiedAudit	0.014**	0.015**	−0.014**	−0.014**
	(2.36)	(2.48)	(−2.37)	(−2.42)
Controls	Yes	yes	yes	yes
Fixed Effects	None	firm-quarter	none	firm-quarter
F	15.69	22.20	14.48	15.66
N-obs	113,854	113,854	113,854	113,854

Panel B. Independent Directors on the Audit Committee

Variable	Dependent Variable: Pr(InsiderSeller_Audit$_{i,t}$)		Dependent Variable: InsiderBSI_Audit$_{i,t}$	
	Pooled (1)	Within firm-quarter (2)	Pooled (3)	Within firm-quarter (4)
Day[−10, −6]*ModifiedAudit	0.002	0.003	−0.001	−0.001
	(1.10)	(1.33)	(−0.48)	(−0.42)
Day[−5, −1]*ModifiedAudit	0.001	0.001	−0.001	−0.001
	(0.34)	(0.44)	(−0.40)	(−0.28)
Day[0, +5]*ModifiedAudit	0.001	0.001	−0.003	−0.003
	(0.40)	(0.50)	(−1.07)	(−1.05)
Day[+6, +10]*ModifiedAudit	0.003	0.003	−0.005	−0.005
	(1.38)	(1.48)	(−1.59)	(−1.53)
Controls	Yes	yes	yes	yes
Fixed Effects	None	firm-quarter	none	firm-quarter
F	4.794	4.502	4.154	1.786
N-obs	113,854	113,854	113,854	113,854

Panel C. Independent Directors not on the Audit Committee

Variable	Dependent Variable: Pr(InsiderSeller_NonAudit$_{i,t}$)		Dependent Variable: InsiderBSI_NonAudit$_{i,t}$	
	Pooled (1)	Within firm-quarter (2)	Pooled (3)	Within firm-quarter (4)
Day[−10, −6]*ModifiedAudit	−0.000	0.000	0.000	−0.000
	(−0.21)	(0.01)	(0.16)	(−0.02)
Day[−5, −1]*ModifiedAudit	−0.004**	−0.003**	0.002	0.001
	(−2.17)	(−2.29)	(0.77)	(0.78)

Table 5 (continued)

Day[0, +5]*ModifiedAudit	0.003**	0.004**	−0.005***	−0.005***
	(2.04)	(2.12)	(−2.81)	(−2.78)
Day[+6, +10]*ModifiedAudit	−0.001	−0.001	0.002	0.002
	(−0.40)	(−0.34)	(0.88)	(0.89)
Controls	yes	yes	yes	yes
Fixed Effects	none	firm-quarter	none	firm-quarter
F	2.451	2.910	3.133	2.260
N-obs	113,854	113,854	113,854	113,854

This table presents results from estimating Eq. (1) using alternative event windows and differentiating between trades placed by officers, independent directors, audit committee independent directors, and non-audit committee independent directors. Panel A presents results for trades placed by officers. Panel B presents results for trades placed by independent directors on the audit committee. Panel C presents results for trades placed by independent directors not on the audit committee. Columns (1) and (2) of each panel present results when the dependent variable is the probability that officers are net sellers and independent directors are net sellers, respectively. Columns (3) and (4) of each panel present results when the dependent variable is the officer buy-sell imbalance and independent director buy-sell imbalance, respectively. Columns (2) and (4) of each panel present results from including firm-quarter fixed effects. Day[−10, −6], Day[−5, −1], Day[0, +5], and Day[+6, +10] are indicator variables equal to one if the day falls in the respective window relative to the audit report date and zero otherwise. ModifiedAudit is an indicator variable that equals one if the audit opinion is anything other than a clean unqualified opinion and zero otherwise. All other variables are as defined in Table 1. For parsimony, we do not tabulate coefficients on control variables or main effects. t-statistics appear in parentheses and are clustered by firm and date. *, **, and *** indicate statistical significance (two-sided) at the 0.1, 0.05, and 0.01 levels, respectively

Panel C presents results from estimating Eq. (1) after decomposing *ModifiedAudit* into *Modified_R* and *Modified_NoR*. Across all specifications, we find that abnormal trading is concentrated among modified opinions in years where financial results are subsequently restated. Comparing abnormal trading between the two types of modified opinions, we find twice as much abnormal trading over the [0,+5] window and five times as much abnormal trading over the [+6,+10] window for modified opinions in the years where financial results are subsequently restated (p values test of differences in coefficients ≤ 0.05).

Collectively, the results in Table 6 suggest that abnormal trading activity does not appear to vary with the stated reason for the modified opinion, but is most pronounced in the settings where insiders are most likely to have significant private information about the audit (e.g., first-time modified opinions and modified opinions in years where financial results are subsequently restated).

4.6 Tests of opportunism.

Finally, we conduct two sets of tests to examine whether trades around the audit report date are opportunistically timed to avoid losses. In particular, we examine whether these trades are a leading indicator of accounting restatements as well as abnormal stock returns.

4.6.1 Predictive ability of trades for restatements.

To test whether trades around the audit report date are a leading indicator that the period's financials results will be subsequently restated, we adapt the restatement

Table 6 Types of Modified Audits

Panel A. Types of Modified Opinions

Type of Modified Opinion	N-obs
First-Time Modified (*Modified_First*=1)	529
Not First-Time Modified (*Modified_NoFirst*=1)	203
Total Modified	732
Modified with Future Restatement (*Modified_R*=1)	126
Modified without Future Restatement (*Modified_NoR*=1)	606
Total Modified	732

Panel B. Types of Modified Opinions: First Time vs. Not First Time

Variable	Dependent Variable: Pr(*InsiderSeller*$_{i,t}$)		Dependent Variable: *InsiderBSI*$_{i,t}$	
	Pooled (1)	Within firm-quarter (2)	Pooled (3)	Within firm-quarter (4)
First Time Modified				
*Day[−10, −6]*Modified_First*	0.012	0.013*	−0.015*	−0.015*
	(1.49)	(1.69)	(−1.85)	(−1.85)
*Day[−5, −1]*Modified_First*	0.014*	0.016**	−0.019**	−0.019**
	(1.87)	(2.09)	(−2.36)	(−2.36)
*Day[0, +5]*Modified_First*	0.027***	0.030***	−0.031***	−0.032***
	(3.46)	(3.67)	(−3.68)	(−3.69)
*Day[+6, +10]*Modified_First*	0.022***	0.025***	−0.025***	−0.026***
	(2.77)	(3.01)	(−2.82)	(−2.90)
Not First Time Modified				
*Day[−10, −6]*Modified_NoFirst*	0.013	0.014	−0.009	−0.010
	(1.29)	(1.37)	(−0.85)	(−0.87)
*Day[−5, −1]*Modified_NoFirst*	0.017	0.017	−0.022*	−0.021*
	(1.46)	(1.41)	(−1.72)	(−1.66)
*Day[0, +5]*Modified_NoFirst*	0.022*	0.022*	−0.028**	−0.028**
	(1.94)	(1.93)	(−2.32)	(−2.29)
*Day[+6, +10]*Modified_NoFirst*	0.002	0.002	−0.003	−0.002
	(0.20)	(0.16)	(−0.25)	(−0.13)
Controls	yes	yes	yes	yes
Fixed Effects	none	firm-quarter	none	firm-quarter
F	14.71	19.02	13.34	9.845
N-obs	113,854	113,854	113,854	113,854
p value: test *Day[−10, −6]*Modified_First=Day[−10, −6]*Modified_NoFirst*				
	[0.88]	[0.91]	[0.63]	[0.65]
p value: test *Day[−5, −1]*Modified_First=Day[−5, −1]*Modified_NoFirst*				
	[0.81]	[0.95]	[0.83]	[0.89]
p value: test *Day[0, +5]*Modified_First=Day[0, +5]*Modified_NoFirst*				
	[0.69]	[0.55]	[0.86]	[0.78]
p value: test *Day[+6, 10]*Modified_First=Day[+6, 10]*Modified_NoFirst*				
	[0.09]	[0.05]	[0.12]	[0.08]

Panel C. Types of Modified Opinions: Subsequently Restated vs. Not Subsequently Restated

Variable	Dependent Variable: Pr(*InsiderSeller*$_{i,t}$)		Dependent Variable: *InsiderBSI*$_{i,t}$	
	Pooled (1)	Within firm-quarter	Pooled (3)	Within firm-quarter

Table 6 (continued)

	(2)		(4)	
Subsequently Restated				
Day[−10, −6]*Modified_R	0.027**	0.027**	−0.029***	−0.029***
	(2.22)	(2.29)	(−2.66)	(−2.72)
Day[−5, −1]*Modified_R	0.023*	0.024*	−0.027*	−0.028**
	(1.77)	(1.93)	(−1.88)	(−2.04)
Day[0, +5]*Modified_R	0.039***	0.041***	−0.050***	−0.052***
	(2.66)	(2.77)	(−3.28)	(−3.35)
Day[+6, +10]*Modified_R	0.056***	0.057***	−0.060***	−0.060***
	(3.53)	(3.54)	(−3.82)	(−3.80)
Not Subsequently Restated				
Day[−10, −6]*Modified_NoR	0.009	0.010	−0.010	−0.010
	(1.24)	(1.41)	(−1.32)	(−1.31)
Day[−5, −1]*Modified_NoR	0.014*	0.015**	−0.018**	−0.018**
	(1.90)	(2.03)	(−2.35)	(−2.28)
Day[0, +5]*Modified_NoR	0.023***	0.025***	−0.026***	−0.027***
	(3.10)	(3.23)	(−3.25)	(−3.22)
Day[+6, +10]*Modified_NoR	0.009	0.011	−0.010	−0.011
	(1.26)	(1.49)	(−1.32)	(−1.36)
Controls	yes	yes	yes	yes
Fixed Effects	none	firm-quarter	none	firm-quarter
F	15.64	19.64	13.66	10.55
N-obs	113,854	113,854	113,854	113,854
p value: test Day[−10, −6]*Modified_R=Day[−10, −6]*Modified_NoR				
	[0.04]	[0.04]	[0.06]	[0.05]
p value: test Day[−5, −1]*Modified_R=Day[−5, −1]*Modified_NoR				
	[0.29]	[0.26]	[0.40]	[0.31]
p value: test Day[0, +5]*Modified_R=Day[0, +5]*Modified_NoR				
	[0.05]	[0.04]	[0.01]	[<0.01]
p value: test Day[+6, +10]*Modified_R=Day[+6, +10]*Modified_NoR				
	[<0.01]	[<0.01]	[<0.01]	[<0.01]

This table presents results from estimating Eq. (2) using alternative event windows and differentiating between types of modified audit opinions. Panel A presents descriptive statistics for different types of audit opinions for our sample of 732 firm-years with modified opinions. Panel B presents results from estimating Eq. (2) after differentiating between first-time and non-first-time modified opinions (i.e., modified opinion in the current year but not in the prior year; *Modified_First* and *Modified_NoFirst*, respectively). Panel C presents results from estimating Eq. (2) after differentiating between modified opinions that involve subsequent restatements vs. modified opinions that do not involve subsequent restatements (*Modified_R* and *Modified_NoR*, respectively). Columns (1) and (2) present results when the dependent variable is the probability that insiders are net sellers (*InsiderSeller*). Columns (3) and (4) present results when the dependent variable is the insider buy-sell imbalance (*InsiderBSI*). Columns (2) and (4) present results from including firm-quarter fixed effects. *Day[−10, −6]*, *Day[−5, −1]*, *Day[0, +5]*, and *Day[+6, +10]* are indicator variables equal to one if the day falls in the respective window relative to the audit report date and zero otherwise. All other variables are as defined in Table 1. For parsimony, we do not tabulate coefficients on control variables or main effects. *t*-statistics appear in parentheses and are clustered by firm and date. *, **, and *** indicate statistical significance (two-sided) at the 0.1, 0.05, and 0.01 levels, respectively. *p* values for tests of differences between coefficients appear in brackets

prediction model from Armstrong et al. (2013) to our setting.[21] In particular, we estimate the following regression on the sample of all firm-years in our event study sample:

$$Restate = \alpha + \beta_1 \, ModifiedAudit + \beta_2 \, InsiderTrade[-5, +5]$$

$$+ \beta_3 \, InsiderTrade\_PriorYear + \theta \, Controls + \varepsilon. \qquad (3)$$

where *Restate* is an indicator variable equal to one if financial results for the year are subsequently restated, *ModifiedAudit* is as previously defined, *InsiderTrade[−5,+5]* is either the probability that insiders are net sellers or the insider buy-sell imbalance during the short window around the audit report date (*InsiderSeller[−5,+5]* or *InsiderBSI[−5,+5]*), and *InsiderTrade_PriorYear* is either the probability that insiders are net sellers or the insider buy-sell imbalance during the fiscal year (*InsiderSeller_PriorYear* or *InsiderSeller_PriorYear*). Following Armstrong et al. (2013), we include the following vector of control variables when estimating Eq. (3): *Size, BM, Surprise, AbReturn, Volatility, LnEAtoAR, Leverage, FirmAge, InterestCov, Acquisition, Financing, Capital,* and *Intangibles*. All variables are defined in Table 7, and standard errors are clustered by firm and year. Panel A of Table 7 presents descriptive statistics for the 1916 firm-years in our sample with non-missing controls, which are generally similar to those in Armstrong et al. (2013).

Panel B presents results from estimating Eq. (3) measuring insider trading activity using an indicator for whether insiders are net sellers over the respective window. Column (1) presents the results from examining the ability of modified audit opinions to predict restatements. Column (2) presents results after simultaneously including trades around the audit report date. Column (3) presents results after controlling for insider trading activity over the fiscal year. By including the audit opinion and insider trading over the fiscal year in the regression, the coefficient on *InsiderTrade[−5,+5]* measures the incremental predictive ability of the trades around the audit report date for restatements (i.e., the extent to which they predict restatements more than other trades during the year). Column (4) presents results after including control variables, and Column (5) presents results after including year and Fama-French 12 industry fixed effects.

Several findings are noteworthy. First, consistent with prior research suggesting that modified opinions predict restatements (e.g., Czerney et al. 2014, 2019), across all specifications, we find that the coefficient on *ModifiedAudit* is economically and statistically significant (coefficients range from 0.051 to 0.089, *t*-stats range from 3.24 to 4.56). This suggests that, conditional on a modified audit, the probability of restatement increases by over 50% relative to the unconditional probability of a restatement (0.05 divided by 0.09, where 0.09 is the mean value of *Restate* in Panel A). Second, across all specifications, we

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[21] For example, SOX 404 material weakness disclosures represent an indication that the internal control system surrounding the financial reporting process likely will not be able to prevent a material misstatement in the final financial statement. While conceptually the financial statement audit should catch these material misstatements (i.e., through detection risk in the audit risk model), this is not always the case in practice. As such, the accounting literature documents a positive association between firms reporting a material weakness and the likelihood that the current period financial statements contain a material misstatement that is restated during future periods (e.g., Bhaskar et al., 2019).

Table 7 Modified Audits, Insider Trading, and Restatements

Panel A. Descriptive Statistics

Variable	Mean	Median	Std	N-obs
Financial Restatement				
Restate	0.099	0.000	0.299	1916
Controls				
Size	6.073	5.893	2.016	1916
BM	0.579	0.499	0.464	1916
Surprise	0.010	0.000	0.113	1916
AbReturn	0.129	−0.012	0.612	1916
Volatility	0.105	0.082	0.076	1916
LnEAtoAR	2.691	2.944	0.978	1916
Leverage	0.587	0.581	0.272	1916
FirmAge	22.582	18.000	14.982	1916
InterestCov	0.587	0.118	0.800	1916
Acquisition	0.025	0.000	0.156	1916
Financing	0.120	0.032	0.258	1916
Capital	0.202	0.104	0.233	1916
Intangibles	0.164	0.011	1.572	1916
Audit Opinion				
ModifiedAudit	0.374	0.000	0.484	1916
Insider Trading Activity				
InsiderSeller[−5,+5]	0.269	0.000	0.444	1916
InsiderSeller_PriorYear	0.604	1.000	0.489	1916
InsiderBSI[−5,+5]	−0.185	0.000	0.550	1916
InsiderBSI_PriorYear	−0.344	−0.772	0.756	1916

Panel B. Insider Selling

Variable	Dependent Variable: Pr($Restate_{i,t}$)				
	(1)	(2)	(3)	(4)	(5)
ModifiedAudit	0.089***	0.086***	0.088***	0.069***	0.051***
	(4.30)	(4.26)	(4.56)	(3.52)	(3.35)
InsiderSeller[−5,+5]	.	0.034*	0.041**	0.046**	0.040**
	.	(1.65)	(2.22)	(2.67)	(2.55)
InsiderSeller_PriorYear	.	.	−0.018	−0.001	−0.015
	.	.	(−1.24)	(−0.09)	(−1.19)
Controls	no	no	no	yes	yes
Fixed Effects	none	none	none	none	year, industry
F	18.53	9.463	26.19	6.91	3.56
N-obs	1916	1916	1916	1916	1916

Table 7 (continued)

Panel C. Buy-Sell Imbalance

	Dependent Variable: Pr($Restate_{i,t}$)				
Variable	(1)	(2)	(3)	(4)	(5)
ModifiedAudit	0.089***	0.085***	0.087***	0.069***	0.050***
	(4.30)	(4.17)	(4.42)	(3.43)	(3.24)
InsiderBSI[−5,+5]	.	−0.028*	−0.035**	−0.035**	−0.029**
	.	(−1.83)	(−2.78)	(−2.68)	(−2.73)
InsiderBSI_PriorYear	.	.	0.013	0.006	0.014*
	.	.	(1.47)	(0.56)	(1.87)
Controls	no	no	no	yes	yes
Fixed Effects	none	none	none	none	year, industry
F	18.53	9.893	59.53	6.81	3.51
N-obs	1916	1916	1916	1916	1916

This table presents results from regressions of financial restatement on audit outcomes and insider trading activity. Panel A presents descriptive statistics. Panel B presents results using the probability that insiders are net sellers to measure insider trading activity. Panel C presents results using the insider buy-sell imbalance to measure insider trading activity. Column (1) present results for examining the ability of modified audit opinions to predict restatements. Column (2) presents results after simultaneously including a measure of insider trading activity around the audit report date. Column (3) presents results after controlling for the normal level of insider trading activity over the fiscal year. Column (4) presents results after including control variables. Column (5) presents results after including year and Fama-French 12 industry fixed effects. *ModifiedAudit* is an indicator variable that equals one if the audit opinion is anything other than a clean unqualified opinion and zero otherwise. *InsiderBSI[−5,+5]* is the insider buy-sell imbalance over the [−5,+5] day window around the audit report date, calculated as the number of shares bought by insiders minus the number of shares sold by insiders, divided by total insider volume. *InsiderBSI_PriorYear* is the insider buy-sell imbalance over the twelve months prior to fiscal year-end. *InsiderSeller[−5,+5]* is an indicator variable equal to one if *InsiderBSI[−5,+5]* is negative and zero otherwise. *InsiderSeller_PriorYear* is an indicator variable equal to one if *InsiderBSI_PriorYear* is negative and zero otherwise. *FirmAge* is the number of years the firm appears on Compustat. *InterestCov* is the ratio of interest expense to net income. If net income for the year is negative or interest expense is more than twice net income, *InterestCov* is set to 2. *Acquisition* is an indicator variable for whether an acquisition accounts for 20% or more of total sales. *Financing* is the amount raised from stock and debt issuances during the year scaled by total assets. *Capital* is net plant, property, and equipment scaled by total assets. *Intangibles* is the ratio of research and development and advertising expense to sales. All other variables are as defined in Table 1. For parsimony, we do not tabulate coefficients on control variables. *t*-statistics appear in parentheses and are clustered by firm and year. *, **, and *** indicate statistical significance (two-sided) at the 0.1, 0.05, and 0.01 levels, respectively. Sample of 1916 unique firm-years

find that the coefficient on *InsiderSeller[−5,+5]* is economically and statistically significant (coefficients range from 0.034 to 0.046, *t*-stats range from 1.65 to 2.67). This suggests that, conditional on insiders selling around the audit report date, the probability of restatement increases by over 35% relative to the unconditional probability of a restatement (0.034 divided by 0.09). These results are robust to controlling for insider selling over the year and are unique to those trades in a short window around the audit report date. Panel C shows similar inferences from estimating Eq. (3) measuring insider trading activity using the buy-sell imbalance over the respective window. Collectively, the results in Table 7 suggest that trades around the audit report date are opportunistic in the sense that they are a leading indicator that the period's financial results are subsequently restated.

4.6.2 Predictive ability of trades for future stock prices

Given that insider sales around the audit report date predict restatements, we expect such trades to avoid losses.[22] We formally test this conjecture by examining buy-and-hold abnormal returns subsequent to the trade (e.g., Jagolinzer et al. 2011). We calculate trade-specific abnormal returns as the market-adjusted return over the 180 days after the trade, multiplying by −1 for sales.[23] We estimate trade-specific abnormal returns for all 1582 sales in the [−5,+5] interval around the audit report date. Given that prior research suggests that insider trades are informed, to judge the *relative* opportunism of these trades, we follow Blackburne et al. (2020) and compare the trade-specific abnormal returns to two benchmarks: (i) the trade-specific abnormal returns of all other insiders within the same industry-year ("Industry Benchmark"), and (ii) the trade-specific abnormal returns on the insider's own trades over the prior two years ("Own Trades Benchmark").

Table 8 presents results after partitioning trades into four groups based on whether the firm subsequently reports a modified opinion and subsequently restates the year's financial results (2 × 2). Three findings are noteworthy. First, both the Industry Benchmark and the Own Trades Benchmark are reliably negative. This is consistent with the notion that insider sales are generally not informed, but are driven by liquidity and diversification needs.

Second, insider sales around the audit report date earn abnormal positive returns only within the subset of firms that subsequently restate financial results (*p* values for test of differences between Industry and Own Trades Benchmarks ≤0.06). This is consistent with such sales avoiding the losses associated with the restatement. Finally, *within the firms that subsequently restate their financial statements*, sales around the audit report date are approximately three times more profitable in firms with modified audit opinions than in firms without modified audit opinions (10.98% vs. 3.68%; untabulated *p* value for test of differences is 0.05). Taken together, the evidence in Tables 7 and 8 suggests that the trading patterns we document appear highly opportunistic: they predict restatements and, as a consequence, avoid significant losses.

5 Conclusion

Although a key purpose of financial statement audits is to protect shareholders, an unintended consequence of the audit process is that it endows corporate insiders with an information advantage. In this paper, we examine whether corporate insiders exploit this information advantage and trade based on private information about audit findings. We focus our analysis on insider trading in a short window around the audit report date. By examining insider trading in a tight window around the audit report date, these tests

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[22] For example, Czerney et al. (2014) find that modified opinions (i.e., unqualified with explanatory language) are predictive of future restatements of that year's financial statements, which prior studies have found to be associated with negative stock price reactions (e.g., Karpoff et al. 2008).

[23] Prior research generally computes abnormal returns over a six-month horizon, since the "short-swing rule" penalizes insiders for profits earned on trades with horizons shorter than six months. In untabulated analyses, we find that our inferences are unchanged if we measure abnormal returns over a three-year horizon after each trade.

Table 8 Abnormal Returns to Insider Sales around the Audit Report Date

Market-adjusted buy-and-hold return [x (−1)] over the 180-days after the sale	[−5,+5] Window Sales	Industry-Year Benchmark	Diff. p-val	Own Trades Benchmark	Diff. p-val
	(1)	(2)	(1)–(2)	(3)	(1)–(3)
Trades without Restatement					
Restate=0; ModifiedAudit=0	−1.24	−2.56	[0.40]	−6.21	[0.03]
	(−0.78)	(−5.10)		(−3.09)	
Restate=0; ModifiedAudit=1	−2.25	−4.02	[0.15]	−5.96	[0.08]
	(−1.88)	(−13.44)		(−2.95)	
Trades with Restatement					
Restate=1; ModifiedAudit=0	3.68	−0.89	[0.06]	−5.75	[<0.01]
	(1.59)	(−0.94)		(−2.21)	
Restate=1; ModifiedAudit=1	10.98	−4.19	[<0.01]	−10.77	[<0.01]
	(3.80)	(−5.85)		(−2.58)	

This table presents trade-specific abnormal returns for 1582 sales during the [−5,+5] window around the audit report date. Sale-specific abnormal returns are measured as the market-adjusted return over the 180 days after each sale, multiplied by −1. Column (1) presents average abnormal returns for sales in the [−5,+5] interval. Column (2) presents average abnormal returns for all sales of officers in the same industry-year, "Industry-Year Benchmark." Column (3) presents average abnormal returns for all sales made by the same officer over the prior two years, "Own-Trades Benchmark." *t*-statistics (*p*-values) appear in parentheses (brackets) and are clustered by executive

mitigate concerns that our results are attributable to either (i) the audit findings themselves being influenced by insider trading, or (ii) omitted firm characteristics correlated with the audit findings. Evidence of a change in insider trading activity in a short window around the audit report date—*when audit findings are known to insiders but not to the market*—suggests that insiders are trading based on private information about audit findings.

We find an abnormally large increase in insider trading around the audit report date for firms that subsequently report modified opinions. This abnormal pattern disappears shortly before the public disclosure of the report in the firm's annual 10-K filing and does not appear in other quarters. In contrast to the trading activity of insiders, we find no evidence of a capital market reaction on the audit report date. The presence of significant insider trading activity, coupled with the absence of a capital market reaction, suggests that insiders are trading on an internal, nonpublic information event in close proximity to the audit report date (when the final results of the audit are communicated to management and members of the board).

We conduct a number of additional tests to sharpen our empirical identification and triangulate our inferences. We find that the abnormal trading activity around the audit report date is most pronounced for senior managers and for independent directors that are *not* on the audit committee, and is most pronounced in the settings where insiders are most likely to have significant private information about the audit (e.g., first-time modified opinions and modified opinions in years when financial results are subsequently restated). These trades are highly opportunistic: they predict restatements, and,

by virtue of this, we show that they avoid significant losses and earn sizable abnormal returns.

Collectively, our findings provide novel evidence that insiders at some firms appear to exploit the audit process—a process ostensibly designed to protect shareholders—for opportunistic gain. Uncovering such opportunism furthers our understanding of the audit process and the internal controls over trading by corporate insiders and suggests a more nuanced understanding of the extent to which audits mitigate agency conflicts.

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