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The Contagion Effect of Low-Quality Audits

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ABSTRACT: We investigate if the existence of low-quality audits in an auditor office indicates the presence of a "contagion effect" on the quality of other (concurrent) audits conducted by the office. A low-quality audit is defined as the presence of one or more clients with overstated earnings that were subsequently corrected by a downward restatement. We document that the quality of audited earnings (abnormal accruals) is lower for clients in these office-years (when the misreporting occurred) compared to a control sample of office-years with no restatements. This effect lasts for up to five subsequent years, indicating that audit firms do not immediately rectify the problems that caused contagion. We also find that an office-year with client misreporting is likely to have subsequent (new) client restatements over the next five fiscal years. Overall, the evidence suggests that certain auditor offices have systematic audit-quality problems and that these problems persist over time.

Keywords: *audit quality*; *auditor offices*; *contagion*.

Data Availability: *All data are publicly available*.

I. INTRODUCTION

We investigate if the existence of at least one low-quality audit in an auditor office location indicates a more systematic problem in office-level audit quality for publicly traded clients. The term "audit failure" is used to refer to audit engagements in which there is a downward restatement of previously audited client earnings.[1] A "contagion" of

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[1] The term "audit failure" can be defined more narrowly, such as court judgments or SEC enforcement actions against auditors, although the Panel on Audit Effectiveness (2000, paras. 1.6 and 3.26) notes that a restatement is strongly suggestive that the audit of the originally issued financial statements was of unacceptably low quality. We also believe the use of accounting restatements can provide insight on a much wider range of potentially low-quality audits than a narrower definition of audit failures.

low-quality audits could occur in an auditor office location due to office-specific characteristics including personnel and quality-control procedures. Gleason et al. (2008, footnote 8) define contagion as occurring "when an adverse event at one firm also conveys negative information about . . . other firms." In our test setting, contagion occurs if the presence of one low-quality audit in an engagement office conveys negative information about the quality of other concurrent audits conducted by the office.

Prior research provides evidence that differences in characteristics across offices of accounting firms are an important determinant of audit quality, and that differences in audit quality can exist even within the same audit firm, depending on office-level characteristics. For example, Francis and Yu (2009) and Choi et al. (2010) show that audit quality is higher in larger Big 4 auditor offices. Research also shows that industry expertise within an office is positively associated with engagement-specific audit quality and audit pricing (Francis et al. 2005; Reichelt and Wang 2010). This research highlights the importance of investigating auditor office-level characteristics and their effects on audit quality, and is consistent with the view that offices are the primary decision-making units in accounting firms (Wallman 1996).

Our results show that offices with an audit failure are more likely to have additional (new) audit failures in the subsequent five years, suggesting a longitudinal contagion of audit failures over time. We also find that concurrent clients in offices with audit failures have a higher level of abnormal accruals compared to offices with zero audit failures, which is suggestive of lower earnings quality (Francis et al. 1999a; Francis and Yu 2009; Reichelt and Wang 2010). These results hold for all but the largest quartile (top 25 percent) of auditor office size. Last, in a separate analysis of Big 4 offices we document that the contagion effect is mitigated for the smallest 75 percent of offices when a large portion of audits are in those industries in which the office is the city-level industry leader. Thus, there is interplay between office size and the office's level of industry expertise, and their effect on audit quality.

Our study can help multiple parties in assessing office-specific audit quality. Regulators such as the PCAOB can focus inspections on auditor office locations that are more likely to be problematic. Audit standard-setters may formulate auditing standards to better address audit-quality problems at the office level, and audit firms can allocate their resources more effectively to improve quality-control procedures in those offices more likely to conduct persistently low-quality audits.[2] Finally, investors may be able to use the results to assess current earnings quality based on the auditor office's prior history of audit failures.

The paper proceeds as follows. We develop our hypotheses in Section II, and Section III presents the sample, research design, and descriptive statistics. Section IV reports the results, and the paper concludes in Section V.

II. BACKGROUND AND HYPOTHESES

Background

Prior research argues that offices are the primary decision-making units in accounting firms (Francis et al. 1999b; Francis and Yu 2009; Wallman 1996). However, the extant literature that investigates the determinants of audit quality at the auditor office-level is relatively scant. Francis and Yu (2009), Choi et al. (2010), and Francis et al. (2012) are the only studies that currently

[2] While audit failure information has obviously been available to national offices in the past, ours is the first study that empirically investigates whether any party, including the national office of audit firms, can infer something systematic about an office's audit quality by identifying a specific audit failure using publicly available information.

provide a way to distinguish *overall* audit quality at the office level by providing evidence that the size of an auditor office is positively associated with audit quality. While these studies attempt to look into the "black box" of auditor offices to investigate office-level characteristics associated with differential audit quality, office size is a somewhat crude tool that may not be as useful to outsiders as a measure that is more specific. Further, given that auditor office size is likely to be very stable from year to year, this measure is not able to discern yearly variations in office-level audit quality. Given the relatively high amount of turnover within audit firms (Hiltebeitel et al. 2000), a measure that provides an indication of overall audit quality within an office in a particular year is likely to be more useful compared to office size alone. We show how our measure can be used in conjunction with auditor office size, thus providing an important contribution to the literature as well as a more refined way to assess office-level audit quality. Our study examines audit failures separately for offices of Big 4 and non-Big 4 accounting firms. This is important because we know relatively little about non-Big 4 firms, yet they conduct audits for about 30 percent of publicly traded companies and their market share has grown since the collapse of Arthur Andersen in 2002.

Prior research also documents differences in engagement-specific audit quality based on an auditor office's industry expertise (e.g., Reichelt and Wang 2010). However, while an auditor office is classified as an expert in particular industries, that office will typically audit many clients outside of its areas of industry expertise. In other words, the unit of analysis in these studies is engagement-specific industry expertise, not a more general office-wide measure of auditor expertise. In contrast, we compare the quality of all audits in offices where audit failures are identified, with all audits in those offices where no audit failures are identified. Therefore, we are investigating inter-office variation in an office characteristic—the presence or absence of an audit failure—instead of variation in engagement-specific industry expertise. However, we control for engagement-specific industry expertise in the primary tests, and we also conduct an additional analysis to determine if the overall use of such expertise with an office mitigates office-level contagion.

Hypotheses Development

An audit failure in an auditor office-year may indicate one of two possibilities. First, it may indicate that a one-off audit engagement was of low quality for engagement-specific or idiosyncratic reasons. The second possibility, and the one that we investigate, is that one audit failure may *reveal* a more systematic problem in an office due to general characteristics of the office. We term this a *contagion*. Specifically, it is possible that general characteristics of office-level personnel, including an office's level of auditor expertise or the lack of office-level, quality-control procedures, led to the specific audit failure, as well as other low-quality audits. If this is the case, then audit failures may provide useful information about the quality of concurrent audits performed in the office.[3]

We test whether a contagion effect exists at the office level based on the identification of one or more audit failures as having occurred in a specific auditor office in a given fiscal year. This requires us to identify and measure audit failures. Palmrose and Scholz (2004) and Kinney et al. (2004) argue that a material restatement of originally audited financial statements is strongly suggestive that the audit of the original, misstated financial statements was of low quality. This view is reinforced by the Panel on Audit Effectiveness (2000, para. 1.6), which says that "Restatements also raise the question, 'Where were the auditors?'" The report goes on to suggest

[3] For example, Krishnan (2005) analyzes office-level audit quality and finds that the clients of the Houston office of Arthur Andersen, which audited Enron, exhibit less timely reporting of bad news compared to a sample of Houston-based clients audited by other Big 6 audit firms, as well as clients of Andersen's Atlanta office, in the same year as the Enron audit failure.

"Restatements of previously audited financial statements raise questions about whether the system that provides assurances about both the *quality of audits* and the reliability of financial reports is operating effectively" (para. 3.26; emphasis added).

A company may have accounting restatements for various reasons. Plumlee and Yohn (2010) examine 3,744 restatements from 2003–2006 and identify four main causes: the majority (57 percent) of restatements are caused by internal company error followed by characteristics of accounting standards (37 percent), which includes complexity, lack of clarity in the standard, and the need to use judgment in applying the standard. The remaining restatements are due to fraud (3 percent) and transaction complexity (3 percent). We believe that a company's external auditor bears some responsibility for allowing a company to issue financial statements that are misstated due to any of these four causes because auditors have "a responsibility to plan and perform the audit to obtain reasonable assurance about whether the financial statements are free of material misstatement, whether caused by error or fraud" (SAS No. 1, AICPA 1972). Consequently, a high-quality audit should, *ceteris paribus*, detect misstatements due to any of the above reasons at a higher rate compared to a low-quality audit, including the professional judgment required to deal with complexity and the interpretation and implementation of accounting standards. We conclude that the presence of an accounting restatement is indicative that a relatively low-quality audit was performed when the misstated financial statements were originally issued.

We predict that in auditor office-years where at least one client misreports (as evidenced by the subsequent downward restatement of earnings), audit quality is lower on average for other clients audited by that office in the same year. The first hypothesis in alternative form is that the presence of one audit failure reveals a contagion effect on the quality of concurrent audits:

H1: The existence of an audit failure in an auditor office is indicative of a contagion effect that reveals the presence of other concurrent low-quality audits in the office.

Francis and Yu (2009), Choi et al. (2010), and Francis et al. (2012) find that audited earnings are of higher-quality for clients in larger Big 4 auditor offices compared to smaller offices. If larger offices perform higher-quality audits, then we would expect to observe less contagion in larger offices. Francis and Yu (2009) attribute the office-size effect to larger offices possessing more in-house experience with public companies, and therefore greater human capital in the office, while Choi et al. (2010) attribute the result to larger offices being subject to lower economic dependence on any one client. In the context of a contagion effect, these arguments suggest that in large offices an audit failure (on a specific engagement) is more likely to be idiosyncratic rather than symptomatic of widespread problems, and the second hypothesis in alternative form is:

H2: There is less contagion in large Big 4 offices than in small Big 4 offices.

While H2 specially focuses on Big 4 firms, for completeness we also compare large and small offices of non-Big 4 accounting firms.

Finally, the literature on auditor industry expertise indicates that office-specific industry expertise is an important determinant of engagement-level audit quality (Reichelt and Wang 2010). However, audits at the office level are conducted for clients that operate within the office's areas of industry expertise as well as in other industries. Furthermore, the number of audits for which the office is an industry expert, as a percentage of the total number of audits in the office, likely varies across offices. In offices where the vast majority of audits are in the office's areas of industry expertise, engagement personnel are more able to apply their industry-specific knowledge and human capital to the office's overall client portfolio, which should result in high-quality audits. In contrast, in offices where relatively few audits are conducted within the office's areas of industry expertise, audit personnel make less use of their industry-specific knowledge. This leads to the third hypothesis stated in alternative form:

H3: There is less contagion in a Big 4 office where relatively more audits are conducted in the office's areas of industry expertise, compared to Big 4 offices where relatively fewer audits are conducted in the office's areas of industry expertise.

We do not test H3 for non-Big 4 auditors as prior literature considers only Big 4 offices to be city-level industry experts (Francis et al. 2005; Reichelt and Wang 2010).

We test for a contagion effect by comparing the quality of clients' audited earnings in those office-years with an audit failure (treatment sample), with the quality of clients' earnings in office-years with *no* audit failures (control sample). Earnings are jointly produced by the client and the auditor (Antle and Nalebuff 1991). Clients are responsible for preparing the financial statements in accordance with GAAP (Generally Accepted Accounting Principles), and the auditor's role is to enforce compliance with GAAP. The research design linking statistical properties of earnings with audit characteristics is described by Francis (2011):

$$\textit{Audited Earnings Quality} = f(\textit{Audit Attributes} + \textit{Controls}),$$

where earnings quality is measured by cross-sectional variation in statistical properties of audited earnings. In this equation, audited earnings quality is a function of specific audit attributes such as the type of auditor (Big 4 or non-Big 4), auditor office characteristics, or engagement-specific factors. Controls refer to client-level controls and other attributes of audit firms (other than the test variable) that are likely to affect the specific measure of audited earnings quality used in a given analysis. It is important to emphasize that earnings quality metrics are not a direct measure of audit quality. Rather, given that earnings are jointly produced by clients and auditors, cross-sectional differences in the statistical properties of audited earnings suggest that there are differences in the underlying quality of audits, based on systematic auditor characteristics.

Following prior research, we test if auditor characteristics are associated with abnormal accruals (Becker et al. 1998; Francis and Yu 2009; Frankel et al. 2002; Reichelt and Wang 2010). Earnings are assumed to be of higher-quality when abnormal accruals are smaller in magnitude, *ceteris paribus*, and the audit attribute we test is whether the engagement office has an audit failure as evidenced by a subsequent downward earnings restatement. A contagion effect is evidenced if earnings quality is lower on average (larger abnormal accruals) for clients in offices with an audit failure, compared to clients in offices with no audit failures.

III. RESEARCH DESIGN AND DESCRIPTIVE STATISTICS

Sample

As described in more detail in the next subsection, an audit failure occurs in an office when there is a downward restatement of net income by a client subsequent to the statutory audit. The year of the audit failure is the year in which the misstated earnings were *originally* reported. We use the Audit Analytics database to identify restatements and the original filing year for which the financial statements were subsequently restated. We use the Compustat Unrestated U.S. Quarterly Data File to obtain originally released as well as subsequently restated accounting data in order to identify the yearly restatement amount, if any.[4] This database provides originally reported quarterly

[4] The Audit Analytics database provides information about a restatement "period" for each firm, which can be one year or more than one year in length. Further, it identifies the effect on net income, if any, only for the entire restatement period, not for each individual year. Conversely, the Compustat Unrestated U.S. Quarterly file identifies the net income effect for each quarter of each fiscal year for each firm. Studies that use the Compustat Unrestated Quarterly file to obtain originally released accounting data include Bronson et al. (2011), Price et al. (2011), and Comprix et al. (2012), among others.

financial statement data, including net income, and many of the data items available in the Compustat Fundamentals Quarterly and Annual Files. Finally, we limit the restatements to those that cross-map to the Audit Analytics restatements database. The reason is that Audit Analytics restricts its database to accounting restatements related to accounting errors, fraud, and GAAP misapplications. In contrast, there are additional restatements in Compustat due to GAAP changes as well as entity changes due to mergers and acquisitions.

The advantage of using the Compustat database (in conjunction with Audit Analytics) is that it readily identifies the yearly dollar amount of accounting restatements. The Compustat Unrestated Quarterly file also includes the originally released as well as the most current restated values for each data item. For companies for which no restatement took place, the data value is exactly the same in both the unrestated and restated item columns. A company's annual earnings (both unrestated and restated) is computed by summing the four quarters of the fiscal year.[5]

The sample period begins in the year 2000, the first year data on the specific auditor office location is available in the Audit Analytics database. We cut off the sample in 2008 because Cheffers et al. (2010) show that the average time lag between the original financial statement release and a restatement in the years 2005 to 2007 is about 700 days, or roughly two years. Therefore, cutting off the sample in 2008 provides confidence that we are correctly classifying the vast majority of restating and non-restating companies.

Table 1, Panel A summarizes the sample selection. There are 87,890 annual firm-year observations in the Compustat Unrestated Quarterly data file for the years 2000 through 2008 with non-missing assets or income. We delete 25,019 financial and utility companies due to the specific operating and accounting characteristics of these firms. Central Index Key (CIK) numbers are used to merge accounting data with auditor office location information drawn from Audit Analytics and are missing for 7,081 observations, and the specific auditor office location data are missing in Audit Analytics for 11,637 observations. There are 2,260 observations that have a downward restatement of net income, and these are used to measure audit failures in specific office-years as described below. In the contagion tests, we delete these 2,260 observations because we test if a contagion effect occurs for the other concurrent clients of the office.[6] Finally, we delete 19,267 observations due to missing information necessary to compute firm-level accounting variables, including abnormal accruals and stock price-based variables. The final sample is comprised of 22,626 firm-year observations for 4,765 unique companies from 2000 through 2008. Table 1, Panel B indicates there are 2,475 Big 4 office-years in the sample with an average of 275 unique offices per year, while there are 1,997 non-Big 4 offices years with an average of 222 unique offices per year. Untabulated results show that Big 4 (non-Big 4) offices have an average of 15 (8) clients, and the largest Big 4 (non-Big 4) office has 567 (408) clients.

Office-Level Audit Failures

Our measure of an audit failure involves identifying whether one or more clients of a specific auditor office in a given year subsequently have a downward restatement of net income. For these offices we calculate the percentage restatement of a company's annual net income by measuring the dollar value difference in net income between the originally released financial information and the

[5] A small percentage of restatements identified in Audit Analytics are communicated to the SEC before the company's fiscal year-end. Specifically, out of a total of 16,175 restatement-years identified by Audit Analytics, only 628, or 3.88 percent, are reported to the SEC before the company's fiscal year-end date. We do not consider these to be audit failures when calculating our main variable of interest, *AUD_FAIL_X*.

[6] We retain the 1,313 upward restatements in the control sample as we do not consider these to be audit failures. However, all results are very similar if we delete these upward restatements from the control sample.

TABLE 1
Sample

Panel A: Sample Selection

	n
Observations available in the Compustat Unrestated Quarterly Data File from the years 2000–2008 with non-missing assets or income	87,890
Less:	
Financial and Utility Companies (SIC 4400–4999 and 6000–6999)	(25,019)
Observations with missing CIK number to merge with Audit Analytics	(7,081)
Observations with missing auditor location data in Audit Analytics	(11,637)
Observations with a downward restatement of net income	(2,260)
Observations with missing data necessary to calculate firm-level variables	(19,267)
Final Sample	22,626

Panel B: Number of Unique Auditor Office Locations by Year

Year	Non-Big 4	Big 4	Total
2000	99	317	416
2001	161	343	504
2002	196	292	488
2003	235	272	507
2004	272	263	535
2005	294	260	554
2006	283	249	532
2007	226	244	470
2008	231	235	466
Total Office-Years	1,997	2,475	4,472
Mean Number of Offices	222	275	497

(continued on next page)

TABLE 1 (continued)

Panel C: Classification of Office-Years with Audit Failures

Number of Offices With At Least One Audit Failure	**Restatement Threshold (X)** $> 0\%$ **Non-Big 4**	$> 0\%$ **Big 4**	$> 10\%$ **Non-Big 4**	$> 10\%$ **Big 4**
$AUD\_FAIL\_X = 0$	1,684	1,599	1,684	1,599
$AUD\_FAIL\_X = 1$	313	876	199	515
Total Office-Years	1,997	2,475	1,883	2,114

(continued on next page)

TABLE 1 (continued)

Panel D: Office-Level Descriptive Statistics

	Non-Big 4				Big 4			
	AUD_FAIL_0 = 0 n = 1,684		*AUD_FAIL_0* = 1 n = 313		*AUD_FAIL_0* = 0 n = 1,599		*AUD_FAIL_0* = 1 n = 876	
	Mean	**Median**	**Mean**	**Median**	**Mean**	**Median**	**Mean**	**Median**
OFFICE_SIZE	599	164	1,702***	706***	7,491	2,505	2,290***	11,602***
M_RISK_PORT	1,979	1.055	2.551***	1.944***	−0.443	−0.635	−0.472	−0.538
M_CITY_IND_EXP	NA	NA	NA	NA	0.635	0.667	0.520***	0.500***
M_NAT_IND_EXP	0.005	0	0.006	0***	0.198	0	0.227**	0.119***
M_INFLUENCE	0.794	0.832	0.274***	0.167***	0.542	0.321	0.113***	0.052***
M_SIZE	2.690	2.758	2.667	2.685	5.991	5.997	5.823***	5.819***
M_LAG_TOT_ACC	−0.169	−0.071	−0.225***	−0.097***	−0.065	−0.054	−0.062	−0.057**
M_CFO	−0.257	−0.043	−0.342*	−0.094***	0.057	0.076	0.057	0.071*
M_CFO_VOL	0.270	0.162	0.306**	0.193***	0.101	0.078	0.096	0.082**
M_SALES_GROWTH	0.124	0.052	0.127	0.070	0.102	0.011	0.086**	0.076
M_SALES_VOL	0.071	0.026	0.072	0.025	1.011	0.384	0.536***	0.300***
M_PPE_GROWTH	0.038	−0.034	−0.018**	−0.067**	0.056	0.028	0.024***	0.011***
M_LEV	0.223	0.131	0.241	0.154**	0.216	0.194	0.183***	0.186***
M_MB	1.814	0.802	10.737	1.250**	1.579	1.304	1.707*	1.678***
M_RETURN	0.242	−0.021	0.242	0.011	0.114	0.049	0.112	0.062
M_RET_VOL	0.223	0.175	0.223	0.180	0.133	0.116	0.130	0.116
M_SHARE_ISSUE	0.752	1.000	0.836***	1.000	0.684	0.750	0.721***	0.750
M_LOSS	0.550	0.592	0.625***	0.667**	0.308	0.250	0.343***	0.333***
M_LITIGATE	0.342	0.142	0.376	0.334***	0.261	0.200	0.286**	0.259***
M_BANKRUPTCY	−2.268	1.010	−3.741***	−0.578***	1.742	1.946	1.754	1.837***
M_#_OPER_SEGS	1.072	1.000	1.048	1.000	1.331	1.000	1.283	1.072***
M_#_GEO_SEGS	1.810	1.000	1.869	1.583***	2.343	2.000	2.452**	2.333***

(continued on next page)

TABLE 1 (continued)

Panel E: Office-Level Audit Failures across SEC Regional Office Locations

		AUD_FAIL_X = 0	*AUD_FAIL_0* = 1		*AUD_FAIL_10* = 1	
SEC Region	**SEC Regional Office**	**Number**	**Number**	**% of Total In Regional Office**	**Number**	**% of Total In Regional Office**
Northeast	New York	156	40	20.4%	18	10.3%
	Philadelphia	583	189	24.5	127	17.9
	Boston	248	74	23.0	47	15.9
Midwest	Chicago	621	218	26.0	122	16.4
Southeast	Atlanta	517	139	21.2	72	12.2
	Miami	96	22	18.6	8	7.7
Central	Dallas-Fort Worth	139	53	27.6	32	18.7
	Denver	282	122	30.2	81	22.3
	Salt Lake City	53	32	37.6	19	26.4
Pacific	San Francisco	498	259	34.2	164	24.8
	Los Angeles	90	41	31.3	24	21.1
	Total	3,283	1,189	26.6%	714	17.9%

*, **, *** Indicate significance at the 0.10, 0.05, and 0.01 levels, respectively, using two-tailed tests.

Big 4 indicates that the auditor office is that of a Big 4 audit firm in year t. Non-Big 4 indicates that the auditor office is that of a non-Big 4 audit firm in year t. The auditor office-year test variable *AUD_FAIL_X* is coded 1 when at least one low-quality audit failure occurs within the same office of a company's external auditor during year t, and 0 otherwise. A low-quality audit is defined as existing when a client company restates net income downward by a material amount subsequent to the audit. X refers to the materiality level of the restatement (i.e., 0 for a greater than 0 percent downward restatement of net income and 10 for a greater than 10 percent downward restatement of net income). "*M_*" before a variable indicates that it is the median value of the variable across all audit clients in an auditor office during a year.

See Appendix A for all variable definitions.

most recent, restated net income number. We then scale this by the absolute value of the originally released net income number to obtain a restatement percentage, either positive or negative.[7] Finally, we cross-map the downward restatements from Compustat to the Audit Analytics database and use only those restatements that are in both databases.

We initially define an audit failure as having occurred when a client company restates net income *downward* by any amount compared to the originally reported value.[8] We also examine a second and more extreme restatement threshold of 10 percent (or more) of originally reported net income to ensure the results are robust to a higher materiality level. It turns out the results are consistent across both thresholds. Downward restatements indicate that the company's originally released net income was "too high" as originally audited. Given that auditors are most concerned with overstatements of net income due to liability concerns (Basu 1997; Kothari et al. 1989; Skinner 1994), and given that income-increasing accruals are more likely to result in auditor reporting conservatism (Francis and Krishnan 1999), we consider only an overstatement of originally reported net income to be an audit failure. While we consider only downward restatements to be an audit failure in our main analyses, all results are similar if we re-code an office with either downward or upward restatements of net income as being an audit failure.

The year of an audit failure is the fiscal year in which the client's restated net income was *originally* reported in the 10-K. This identification is important because we are testing whether the existence of an audit failure in a given year for an auditor office is indicative of other lower quality audits throughout that office in the same fiscal year (i.e., a contagion effect). Auditor office locations that have one or more clients with a downward restatement in net income for a particular year are coded 1 for the test variable *AUD_FAIL_X*, where *X* indicates the particular percentage restatement threshold being used in a particular model: i.e., $X = 0$ for any downward restatement of net income (0 percent threshold), and $X = 10$ for 10 percent or greater downward restatements of net income. The control sample of auditor office-years with no downward restatements of net income in a particular year are coded 0 ($AUD\_FAIL\_X = 0$).

Table 1, Panel C summarizes the coding of office-year audit failures. For Big 4 accounting firms at the 0 percent materiality threshold, 876 of 2,475 total office-years are coded 1, indicating the presence of one or more downward restatements of clients' earnings in 35.4 percent of office-years. For non-Big 4 firms at the 0 percent materiality threshold, 313 of 1,997 total office-years (15.7 percent) are coded 1. Untabulated results show that of the 313 non-Big 4 office-years with at least one audit failure, 217 of these office-years have exactly one audit failure and 96 office-years (31 percent) have more than one failure: 59 offices have two failures, 27 have three failures, eight have four failures, and two have five failures. For the 876 Big 4 office-years with audit failures, 414 have a single failure and 462 (52 percent) have more than one failure: 237 offices have two failures, 103 offices have three failures, 49 offices have four failures, 34 offices have five failures, and 39 have more than five failures. For Big 4 offices in particular, the presence of one audit failure indicates a strong likelihood of others. In total, 30 office-years have more than five audit failures, and the maximum is 17 failures in a single office.

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[7] Scaling by the absolute value of the originally released net income value ensures that all decreases (increases) in net income due to the restatement are calculated to be a negative (positive) percentage restatement.

[8] A restatement may have occurred for any of the company's four fiscal quarters for a fiscal year. Quarter-end financial statements for the first three quarters are typically reviewed instead of audited. However, given that fiscal year-end financial statements are always audited, and given that fiscal year-end net income includes cumulative net income for all four fiscal quarters, each quarter is, in effect, audited at year-end. Therefore, a restatement of net income for any of the company's fiscal quarters can be considered a restatement of *audited* annual earnings.

Table 1, Panel D presents office-level characteristics and compares offices with and without audit failures. All variables, except *OFFICE_SIZE*, are calculated by taking the mean and median value across all client firms within an office each year. Panel C shows statistically significant differences between offices with/without audit failures for about two-thirds (three quarters) of the variables for non-Big 4 (Big 4) offices, indicating the need to control for these client characteristics in the regression models. Finally, Panel E reports the number of auditor offices with and without audit failures located in each of the SEC's 11 regional offices/districts. Kedia and Rajgopal (2011) find that geography is important in explaining corporate misreporting and SEC enforcement activity. A Kolmogorov-Smirnov test indicates that the distribution of auditor offices with failures versus offices without audit failures is significantly different ($p < 0.01$) across the 11 SEC regions. In particular, there is a higher rate of auditor office failures in the Central and Pacific regions (Dallas-Fort Worth, Denver, Salt Lake City, San Francisco, and Los Angeles) as all of these regions are above the sample mean. To control for potential regional differences, we include SEC regional office fixed effects in our models, as described later.

Empirical Model

The OLS regression model in Equation (1) is estimated for separate samples of Big 4 and non-Big 4 client companies to test if a contagion effect exists in auditor offices:

$$\begin{aligned} ABS\_ABN\_ACC \text{ or } ABN\_ACC = {} & \beta_0 + \beta_1 AUD\_FAIL\_X + \beta_2 OFFICE\_SIZE + \beta_3 RISK\_PORT \\ & + \beta_4 CITY\_IND\_EXP + \beta_5 NAT\_IND\_EXP + \beta_6 INFLUENCE \\ & + \beta_7 SIZE + \beta_8 LAG\_TOT\_ACC + \beta_9 CFO + \beta_{10} CFO\_VOL \\ & + \beta_{11} SALES\_GROWTH + \beta_{12} SALES\_VOL \\ & + \beta_{13} PPE\_GROWTH + \beta_{14} LEV + \beta_{15} MB + \beta_{16} RETURN \\ & + \beta_{17} RET\_VOL + \beta_{18} SHARE\_ISSUE + \beta_{19} LOSS \\ & + \beta_{20} LITIGATE + \beta_{21} BANKRUPTCY + \beta_{22} \#\_OPER\_SEGS \\ & + \beta_{23} \#\_GEO\_SEGS + Year\ Fixed\ Effects \\ & + Industry\ Fixed\ Effects + SEC\ Regional\ Office\ Fixed\ Effects \\ & + \varepsilon, \end{aligned} \tag{1}$$

where the dependent variable *ABS_ABN_ACC* (*ABN_ACC*) is the absolute value (signed value) of a company's abnormal accruals in year *t*, controlling for concurrent performance using a modified Jones model (Dechow et al. 1995; Jones 1991; Kothari et al. 2005). We analyze both absolute abnormal accruals and the subsample of income-increasing abnormal accruals because Hribar and Nichols (2007) demonstrate that the analysis of absolute accruals may be problematic due to a correlated omitted variable problem. The calculation of abnormal accruals is detailed in the next subsection.

Other auditor office characteristics are controlled for as prior research shows these to be important. Office size is controlled for because Francis and Yu (2009) and Choi et al. (2010) show that Big 4 office size is negatively associated with client abnormal accruals. Consistent with their studies, the variable *OFFICE_SIZE* is the natural log of the total dollar amount of audit fees charged to all audit clients within an auditor office in year *t*. A dichotomous version of this variable is also used as a test variable in Table 5 to investigate whether auditor office size affects the extent to which an audit failure is indicative of a contagion effect (test of H2).

We also control for the average clientele portfolio risk within an auditor office (*RISK_PORT*) to mitigate the concern that client-specific characteristics may be driving either the likelihood that at least one restatement occurs within an office, or the level of abnormal accruals of those clients, or both. We compute an office's client portfolio risk by first calculating the median level of client assets, leverage, and return on assets within each office-year, similar to prior research (Johnstone and Bedard 2004).[9] We then standardize these values, which results in a mean of 0 and standard deviation of 1 (for each variable) so as to not under-/over-weight any individual variable. Finally, we add together the standardized mean values of assets and return on assets, subtract the mean value of leverage, and then multiply this sum by −1 (so that a higher value reflects a riskier portfolio) to obtain the final value of *RISK_PORT*. Results are similar if mean values are used, and we make no prediction for the sign on this variable.

We create an additional office-level test variable for H3, which examines if the percentage of audits conducted within a Big 4 office where that office is the city-level industry leader, has an impact on the contagion effect. The variable *OFFICE_EXP_#* is measured as the number of audits conducted in a Big 4 office in a year where that office is the city-level industry leader, scaled by the total number of audits conducted by the office in the year.

Engagement-specific auditor industry expertise is controlled using both city- and national-level measures in which the industry leader (auditor with the largest dollar amount of audit fees) is considered to be the industry expert (Francis et al. 2005; Reichelt and Wang 2010). Only Big 4 auditors are city and/or national industry leaders. National (city) industry leadership is based on each audit firm's market share of audit fees in a two-digit SIC category in the United States (within a two-digit SIC category in a specific city). Following Francis et al. (2005) and Reichelt and Wang (2010), we define a city using the Metropolitan Statistical Area (MSA) as classified by the U.S. Census Bureau. Auditor cities are collected from Audit Analytics and are then categorized by MSA using the U.S. Census Bureau's MSA cross-map.[10] Both city and national industry leaderships are recalculated each year. The variable *CITY_IND_EXP* is coded 1 if the auditor on a specific client engagement is the city-specific market share leader in terms of audit fees in a given year, and *NAT_IND_EXP* is coded 1 if the auditor is the national market share leader in terms of audit fees in a given year. Results on the association between abnormal accruals and both city and national industry leadership are mixed in prior research so we do not make a prediction for these variables (Francis and Yu 2009; Reichelt and Wang 2010).

The variable *INFLUENCE* is the total dollar amount of audit and nonaudit fees charged to a specific client in year *t*, scaled by the total fees charged by the auditor office in a year. Francis and Yu (2009) include this variable to control for the possibility that a specific client that provides a relatively high percentage of total fees to an auditor office may affect auditor objectivity and audit quality for that client. In most of their analyses this variable is not significant, so we do not predict a sign for the coefficient on *INFLUENCE*.

Firm-level variables used in prior studies are included in all analyses to control for the various characteristics that affect a company's level of abnormal accruals (see Appendix A for detailed definitions for all control variables). Based on prior research, we expect *SIZE*, *LAG_TOT_ACC*, *CFO*, *LOSS*, and *BANKRUPTCY* to be negatively associated with abnormal accruals, while we expect *CFO_VOL*, *SALES_GROWTH*, *SALES_VOL*, *PPE_GROWTH*, *MB*, *RET_VOL*, and *LITIGATE* to be positively associated with abnormal accruals (Choi et al. 2010; Francis and Yu 2009; Hribar and Nichols 2007; Reichelt and Wang 2010). We do not predict a sign for *LEV*, *RETURN*, *SHARE_ISSUE*, *#_OPER_SEGS*, and *#_GEO_SEGS* due to absent or conflicting results

[9] Johnstone and Bedard (2004) include additional variables in calculating their risk portfolio measure. However, these variables were obtained through a questionnaire specific to their study. We use variables that are available in Compustat. Further, Johnstone and Bedard (2004) do not include client assets as a risk variable, although they do include it as a control variable in their analyses.

[10] The MSA cross-map (2008 definition) is available at: http://www.census.gov/population/www/metroareas/metroarea.html. For cities not listed on the cross-map, we hand-collect the closest MSA using the 2008 map available at the website listed above and Google Maps. We thank Brett Kawada and Sarah Stein for their help in this hand-collection.

in prior studies (Francis and Yu 2009).[11] As in prior research, we include year and industry fixed effects. In addition, SEC regional office fixed effects are used to control for geographic patterns in misreporting and detection by the SEC (Kedia and Rajgopal 2011).

Abnormal Accruals

Firm-year abnormal accruals are calculated using a modified Jones model (Dechow et al. 1995; Jones 1991), controlling for concurrent performance (Kothari et al. 2005) within industry-year groups for separate samples of Big 4 and non-Big 4 clients, where industries are defined by a company's two-digit SIC code. The model in Equation (2) is estimated separately for each industry-year-auditor group, and requires a minimum of 20 observations:[12]

$$TOT\_ACC = \alpha_0 + \alpha_1(1/ASSETS) + \alpha_2(\Delta SALES - \Delta AR) + \alpha_3 PPE + \alpha_4 ROA + \varepsilon. \quad (2)$$

The variable *TOT_ACC* is calculated as a company's net income before extraordinary items less cash flows from operations; *ASSETS* is a company's total assets at the end of year $t-1$; *SALES* is a company's sales in year t and $t-1$ scaled by lagged total assets; *AR* is a company's net total receivables at the end of year t and $t-1$ scaled by lagged total assets; *PPE* is net property, plant, and equipment at the end of year t scaled by lagged total assets; and *ROA* is net income in year t scaled by lagged total assets. Equation (2) is estimated separately for clients of Big 4 and non-Big 4 accounting firms (for separate industry-year subsamples) as these clients exhibit different operating and accounting characteristics (Francis et al. 1999a), although the results are qualitatively the same if Equation (2) is estimated for the full sample.

Equation (2) uses only those firm-year observations that do not have an earnings restatement, either income-increasing or income-decreasing, as the inclusion of companies with misstated earnings could bias the calculation of the coefficient parameters. We then apply these parameter values to firm-year observations in the treatment and control samples to derive expected accruals. Abnormal accruals are the difference between expected and actual accruals.

Descriptive Statistics

Table 2, Panel A presents the descriptive statistics of the variables in the study. *ABS_ABN_ACC* has a mean (median) value of 0.091 (0.052), which is similar to other studies (Reichelt and Wang 2010; Reynolds and Francis 2000). The mean value of *ABN_ACC* for all observations is 0 by construction (Kothari et al. 2005), and the median value is also close to 0 (−0.003). The office-level test variable *AUD_FAIL_0* is coded 1 when at least one client restates net income downward by any amount within an auditor office in a year, and 0 otherwise. There are 4,472 total office-years in the sample, and the mean value of 0.265 indicates that 26.5 percent of auditor office-years have an audit failure at the 0 percent materiality threshold). At the 10 percent threshold (*AUD_FAIL_10*), 16.0 percent of auditor office-years have an audit failure.

[11] Francis and Yu (2009) also include the variable *TENURE* that indicates whether a company has been audited by the same audit firm for at least three years, based on Johnson et al. (2002). They are able to include this variable for their entire sample because they begin their analysis in the year 2003. However, given that this variable requires two years of lagged data to compute, its inclusion would force us to eliminate observations in the years 2000 and 2001 because specific auditor information is not available in Audit Analytics prior to 2000. Given the effect on sample size, we do not present analyses including *TENURE*. However, we note that all results are qualitatively the same when *TENURE* is included for a reduced sample.

[12] Observations for which any value of the variables in Equation (2) is above the 0.99 value or below the 0.01 value of all companies are excluded from the calculation of parameter values for Equation (2) to mitigate the effect of these extreme values on the calculation of expected accruals. However, these companies are included in the final sample of 22,626 company-year observations.

TABLE 2

Descriptive Statistics

Panel A: Distributional Properties of Variables

Variable	n	Mean	Std. Dev.	25%	Median	75%
ABS_ABN_ACC	22,626	0.091	0.115	0.023	0.052	0.108
ABN_ACC	22,626	0	0.126	−0.056	−0.003	0.049
AUD_FAIL_0	4,472	0.265	0.440	0	0	1.000
AUD_FAIL_10	4,472	0.160	0.366	0	0	0
B4	22,626	0.803	0.397	1.000	1.000	1.000
OFFICE_SIZE (n = office-years)	4,472	7,510	18,200	201	1,136	5,664
RISK_PORT (n = office-years)	4,472	0.669	2.725	−1.012	−0.032	1.487
OFFICE_EXP_% (Big 4 only)	2,475	0.682	0.329	0.400	0.740	1.000
CITY__IND_EXP (Big 4 only)	18,164	0.505	0.499	0	1.000	1.000
NAT_IND_EXP	22,626	0.185	0.389	0	0	0
INFLUENCE	22,626	0.243	0.495	0.020	0.066	0.234
SIZE	22,626	1,457	3,632	43	202	927
LAG_TOT_ACC	22,626	−0.151	1.471	−0.115	−0.057	−0.015
CFO	22,626	−0.052	0.488	−0.100	0.056	0.151
CFO_VOL	22,626	0.165	0.232	0.045	0.089	0.182
SALES_GROWTH	22,626	0.135	0.384	−0.045	0.075	0.219
SALES_VOL	22,626	1.108	3.047	0.041	0.182	0.781
PPE_GROWTH	22,626	0.077	0.386	−0.112	0.007	0.165
LEV	22,626	0.207	0.224	0.005	0.146	0.328
MB	22,626	2.197	3.224	0.126	1.349	2.693
RETURN	22,626	0.236	0.989	−0.283	0.031	0.407
RET_VOL	22,626	0.165	0.140	0.085	0.128	0.199
SHARE_ISSUE	22,626	0.733	0.441	0	1.000	1.000
LOSS	22,626	0.403	0.490	0	0	1.000
LITIGATE	22,626	0.315	0.464	0	0	1.000
BANKRUPTCY	22,626	0.399	5.740	0.508	1.791	2.819
#_OPER_SEGS	22,626	1.241	1.046	1.000	1.000	1.000
#_GEO_SEGS	22,626	2.486	2.087	1.000	2.000	3.000

Panel B: Differences in Means/Medians of Abnormal Accruals in Non-Big 4 Auditor Offices

	ABS_ABN_ACC			***ABN_ACC* > 0**		
	Mean	**Median**	**n**	**Mean**	**Median**	**n**
AUD_FAIL_X = 0	0.139	0.079	3,193	0.122	0.080	1,642
AUD_FAIL_0 = 1	0.158***	0.087***	1,269	0.132*	0.086	602
AUD_FAIL_10 = 1	0.159**	0.088*	853	0.128	0.086	398

(continued on next page)

Approximately 80 percent of companies in the sample use a Big 4 auditor, which is consistent with prior research (Francis et al. 1999a). *OFFICE_SIZE* is presented in Table 2 in raw form as is the total dollar amount of audit fees (in \$ thousands) charged by an auditor office-year. The mean (median) value of audit fees charged is about \$7.5 million (\$1.1 million). The mean (median) values of the variable that measures an office's risk portfolio (*RISK_PORT*) are 0.669 (–0.032). The variable *OFFICE_EXP_%* is the percentage of total audits conducted by an office in a year where the Big 4

TABLE 2 (continued)

Panel C: Differences in Means/Medians of Abnormal Accruals in Big 4 Auditor Offices

	ABS_ABN_ACC			*ABN_ACC* > 0		
	Mean	**Median**	**n**	**Mean**	**Median**	**n**
AUD_FAIL_X = 0	0.072	0.045	6,846	0.071	0.044	3,344
AUD_FAIL_0 = 1	0.081***	0.050***	11,318	0.082***	0.049***	5,324
AUD_FAIL_10 = 1	0.084***	0.052***	7,839	0.086***	0.051***	3,701

*, **, *** Indicate significance at the 0.10, 0.05, and 0.01 levels, respectively, using two-tailed tests.
ABS_ABN_ACC is the absolute value of a company's abnormal accruals as calculated in Kothari et al. (2005). *ABN_ACC* is the signed value of a company's abnormal accruals as calculated in Kothari et al. (2005). *AUD_FAIL_X* is 1 when at least one low-quality audit occurs within the same office of a company's external auditor during year *t*, and 0 otherwise. A low-quality audit is defined as existing when a client company restates net income downward by a material amount subsequent to the audit. *X* refers to the materiality level of the restatement (i.e., 0 for a greater than 0 percent downward restatement of net income and 10 for a greater than 10 percent downward restatement of net income). *B4* is 1 if the company hires a Big 4 auditor in year *t*, and 0 otherwise.
See Appendix A for definitions of all other variables.

auditor is the city-specific industry leader. Therefore, *OFFICE_EXP_%* is a continuous variable that is specific to each auditor-office-year observation. The mean (median) values for *OFFICE_EXP_%* over the 2,475 Big 4 auditor-office-year observations is 0.682 (0.740). This indicates that the average Big 4 office is the city-level industry leader on approximately 70 percent of its audit engagements.

The variable *CITY_IND_EXP* is a firm-year specific variable that takes on a value of 1 when a company is audited by the city-level industry leader in a year, and 0 otherwise. The mean value of *CITY_IND_EXP* is 0.505, indicating that about half of all Big 4 audit engagements in the sample are conducted by a city-level industry leader. The mean value of *NAT_IND_EXP* is 0.185 indicating that for about 18 percent of Big 4 audits, the auditor is classified as the national industry leader.

The variable *INFLUENCE* has a mean (median) value of 0.243 (0.066) indicating that the average client company represents 24.3 percent of the total fees charged to all clients of an office. The median value is only 6.6 percent, which indicates that this variable is skewed and that some particularly highly influential clients are driving the mean.[13]

Table 2, Panels B and C present univariate results comparing the mean and median values of companies' absolute and income-increasing abnormal accruals in offices with at least one audit failure compared to offices with zero audit failures. Panel B presents t-tests (rank-sum tests) for differences in means (medians) for non-Big 4 offices. The values for *AUD_FAIL_X* = 0 are the mean and median levels of client company absolute abnormal accruals (*ABS_ABN_ACC*) and income-increasing abnormal accruals (*ABN_ACC* > 0) audited by auditor offices with no audit failures in a year. *AUD_FAIL_0* = 1 and *AUD_FAIL_10* = 1 present the same values for office-years with at least one audit failure at the 0 and 10 percent threshold levels, respectively. Results at both thresholds indicate the mean and median level of client absolute abnormal accruals are significantly larger in non-Big 4 offices with at least one audit failure compared to non-Big 4 offices with no audit failures: at the 10 percent threshold, mean abnormal accruals are larger by 2.0 percent of lagged assets. Results for income-increasing abnormal accruals are less clear as only one out of four differences is significant. However, this is before controlling for other important factors that likely affect abnormal accruals. Panel C presents mean/median values for Big 4 offices and all differences are significant ($p < 0.01$). At the 10

[13] All results on our analyses of a contagion effect are virtually identical if we use either the log or the square root of *INFLUENCE* as a control variable.

percent threshold, mean abnormal accruals are larger by 1.2 percent of lagged assets, and mean income-increasing accruals are larger by 1.6 percent of lagged assets. Taken together, the results in both panels present univariate evidence consistent with a contagion effect in non-Big 4 and Big 4 offices.

Untabulated Pearson and Spearman correlations show the correlations among the independent variables are all below 0.50, indicating that multicollinearity is not likely to be of concern. This conclusion is supported by variance inflation factors in the model estimations that are all less than 6.5, well below the threshold of 10 suggested in Kennedy (1992).

IV. RESULTS

Before testing the study's three hypotheses, we first examine if an audit failure in a prior office-year increases the likelihood of observing an audit failure (misstated client earnings) in a subsequent office-year. Given that auditor office-years are the unit of analysis, to control for office-specific clientele characteristics we calculate the median level of all firm-level control variables used in Equation (1) for clients of the office-year.[14] We do not predict a sign on the office-level control variables as we know of no prior research to rely on that predicts client restatements at the office level. However, it is important to control for office-level characteristics that may affect the likelihood that one or more clients of an office issues a restatement. Table 3 reports this analysis at the 0 percent materiality threshold using a Probit regression.[15] For both Big 4 and non-Big 4 auditors, an audit failure in office-year *t* is more likely to occur if there were prior failures in years *t*–1 through *t*–5. Thus, when we observe an audit failure in an office, there is a significant likelihood there will be future (new) audit failures for up to five subsequent years. Results are similar at the 10 percent materiality threshold. It appears that certain engagements offices experience "serial problems" with audit quality.[16]

This result may seem surprising because a restatement is publicly disclosed and one might expect an audit firm to respond quickly to such an event by strengthening quality-control procedures within the office.[17] Even though it takes, on average, two years for a restatement to occur, it is remarkable that even after five years there is still a significant likelihood of another audit failure in the office. The lack of timely remediation could be due to the perception that a specific restatement was due to unique client-specific characteristics of the audit engagement (poor internal controls, weak financial reporting oversight, or even fraud that was not detected), or to idiosyncratic engagement-specific auditor characteristics, rather than suggestive of a more systemic problem in office-level audit quality. Our results suggest the possibility that audit firms can use client restatements as another input to their national quality-control procedures to identify problems and to improve audit quality in offices throughout the firm.

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[14] Results are very similar if mean values are used in place of median value for all office-level control variables.

[15] For this analysis we delete consecutive multi-year restatements by the same company as such restatements would bias in favor of the test. For a specific company we keep only the first restatement and recode *AUD_FAIL_X* accordingly for the analysis in Table 3.

[16] For both Tables 3 and 4 we also run models that pool Big 4 and non-Big 4 clients together. We run these pooled models in two ways: (1) We add a *B4* indicator variable and the interaction between *B4* and all five lagged versions of *AUD_FAIL_0* (Table 3), and a *B4* indicator variable along with the interaction between *B4* and *AUD_FAIL_X* (Table 4) and (2) we add the *B4* indicator variable as well as its interaction with all other control variables (except fixed effects) in Tables 3 and 4. In all cases inferences drawn from these results are virtually identical to those reported in the study. Further, the negative and significant coefficients on *B4* in these estimations indicate that Big 4 offices are less likely to have an audit failure overall (Table 3), and clients of Big 4 offices exhibit lower levels of abnormal accruals overall (Table 4).

[17] Correlations among the office-level variables show that fewer (more) audit failures occur, on average, in larger Big 4 (non-Big 4) offices, and in offices that have a smaller percentage of audits in industries for which the office is the city-level industry leader. The average clientele risk of an office (*RISK_PORT*) does not seem to be related to audit failures after controlling for specific client risk characteristics in our multivariate analyses. However, the univariate correlation indicates that the average clientele risk of an office (*RISK_PORT*) is negatively correlated with the presence of at least one audit failure. This suggests that auditors adjust their audit procedures to be more stringent when clients are higher risk, which is consistent with Generally Accepted Auditing Standards.

TABLE 3

Current Audit Failures in an Office Predicted by Past Audit Failures
Auditor Office-Level Analysis
(dependent variable = *AUD_FAIL_0*)

Variable	Pred.	Non-B4	B4	Non-B4	B4	Non-B4	B4	Non-B4	B4	Non-B4	B4
Test Variables											
LAG_1_AUD_FAIL_0	+	0.103***	0.286***								
LAG_2_AUD_FAIL_0	?			0.117***	0.194***						
LAG_3_AUD_FAIL_0	?					0.046*	0.183***				
LAG_4_AUD_FAIL_0	?							0.093***	0.146***		
LAG_5_AUD_FAIL_0	?									0.127***	0.120***
(p-value)		(0.000)	(0.000)	(0.000)	(0.000)	(0.072)	(0.000)	(0.003)	(0.000)	(0.000)	(0.000)
Control Variables											
OFFICE_SIZE	+	0.002***	0.003***	0.002***	0.004***	0.002***	0.004***	0.002***	0.004***	0.002***	0.004***
M_CITY_IND_EXP	?		0.033		0.008		0.012		−0.008		−0.017
M_NAT_IND_EXP	?		0.018		0.031		0.013		0.009		−0.011
M_INFLUENCE	?	−0.218***	−0.590***	−0.255***	−0.613***	−0.292***	−0.571***	−0.287***	−0.553***	−0.277***	−0.534***
M_AB_ACC	?	0.006	−0.095	0.008	0.031	0.000	0.103	−0.017	−0.322	−0.009	−0.221
M_SIZE	?	0.005	0.030*	0.000	0.030*	−0.005	0.036*	−0.011	0.032*	−0.005	0.023*
M_LAG_TOT_ACC	?	−0.016	0.412**	−0.023	0.311*	0.018	0.215**	0.026	0.326**	0.036	−0.011
M_CFO	?	−0.005	0.036	−0.001	0.130	−0.003	0.134	0.019	0.142	0.025	0.334**
M_CFO_VOL	?	0.018	0.242	0.011	0.202	0.036	0.355	0.072*	0.518*	0.041	0.551*
M_SALES_GROWTH	?	0.018*	0.008	0.028	−0.004	0.040*	0.000	0.062*	0.136	0.062*	0.113
M_SALES_VOL	?	0.025	0.006	0.095**	0.004	0.132**	0.001	0.136**	−0.005	0.097*	−0.004
M_PPE_GROWTH	?	−0.004	−0.067	−0.007	−0.148	−0.001	−0.111	0.030	−0.274*	0.039	−0.205*
M_LEV	?	−0.003	−0.088	−0.025	−0.089	−0.028	−0.089	−0.003	−0.072	−0.013	−0.073
M_MB	?	−0.002	−0.016	−0.006*	−0.024*	−0.007*	−0.019	−0.014***	−0.018	−0.020***	−0.032*
M_ RETURN	?	0.006**	0.036	0.010	0.044	0.012	0.022	0.010	0.017	0.014	−0.008
M_RET_VOL	?	0.005	0.182	0.001	0.002	−0.040	0.204	−0.138	0.507	0.004	0.264
M_SHARE_ISSUE	?	0.049**	0.060	0.074***	0.080	0.079**	0.034	0.038	0.014	0.017	0.027
M_LOSS	?	−0.014	−0.045	0.001	−0.007	0.010	0.019	0.051	−0.007	0.072	0.016
M_LITIGATE	?	0.015	0.044	0.015	0.043	0.018	−0.012	0.003	−0.038	0.022	−0.045
M_BANKRUPTCY	?	−0.001	−0.012*	−0.001	−0.011*	−0.001	−0.007*	−0.000	0.003	0.002	0.000

(continued on next page)

TABLE 3 (continued)

Variable	Pred.	Non-B4	B4	Non-B4	B4	Non-B4	B4	Non-B4	B4	Non-B4	B4
M_#_OPER_SEGS	?	−0.009	−0.014	0.002	−0.015	−0.007	−0.022	−0.005	−0.057*	−0.011	−0.028
M_#_GEO_SEGS	?	−0.008	0.003**	−0.010	0.006**	−0.020**	0.006**	−0.027**	0.003	−0.041***	0.003
Intercept	?	0.070	0.050	0.051	0.103	0.033	0.127*	−0.002	0.124	0.101	0.106
Year Fixed Effects		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SEC Regional Office Fixed Effects		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
n (treatment)		314	814	251	740	197	648	144	539	94	412
n (control)		1,451	1,345	1,233	1,065	993	858	762	689	537	555
Model p-value		< 0.001	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001
Pseudo R^2		29.8%	33.9%	28.4%	30.9%	26.9%	29.0%	26.5%	28.1%	26.5%	27.0%

*, **, *** Indicate significance at the 0.10, 0.05, and 0.01 levels, respectively, using two-tailed tests.
t-statistics are calculated based on robust standards errors clustered at the auditor-office level. *AUD_FAIL_0* is 1 when at least one audit failure (defined as a downward restatement of income) occurs in the office of the company's external auditor during year *t*, and 0 otherwise. "Lag" denotes the value of *AUD_FAIL_0* in years *t*–1, *t*–2, *t*–3, *t*-4, and *t*–5, respectively. "*M_*" before a variable indicates that it is the median (or mean for dichotomous variables) value in an office-year.
See Appendix A for definitions of control variables.

Tests of H1—Contagion in Non-Big 4 and Big 4 Auditor Offices

The results in Table 3 provide evidence of longitudinal contagion of audit failures in offices over time. H1 tests if there is cross-sectional contagion on the quality of audited earnings for other concurrent clients in office-years with an audit failure. Table 4, Panel A presents the first set of regression results testing H1 where the dependent variable is a company's level of absolute abnormal accruals (*ABS_ABN_ACC*). The p-values on the test variable of interest (*AUD_FAIL_X*) are reported conservatively as two-tailed values, even though directional predictions are being made with respect to contagion effects. All models are significant at $p < 0.01$ with R^2s around 47 percent for clients of non-Big 4 offices, and 27 percent for clients of Big 4 offices. These and all subsequent models include industry (two-digit SIC codes), year, and SEC regional offices fixed effects, and t-statistics are based on standard errors clustered at the auditor-office level.[18] Auditor and company-level control variables are generally consistent with predictions.

In Table 4, Panel A, the positive and significant ($p < 0.05$, two-tailed) coefficients on *AUD_FAIL_X* for Big 4 clients for both thresholds indicate that clients audited by Big 4 offices with at least one audit failure have higher absolute abnormal accruals, on average, compared to Big 4 offices where no audit failures occurred. These results support H1 and are consistent with the univariate tests in Table 2, Panel C, and provide evidence that a contagion effect exists in Big 4 auditor offices. In contrast, for non-Big 4 offices the coefficients are not significant at either threshold, indicating that absolute abnormal accruals are not different in non-Big 4 offices with an audit failure compared to offices with no failures.

Table 4, Panel B analyzes whether these results hold when looking only at companies that exhibit positive or income-increasing abnormal accruals. Results for Big 4 auditors are similar to Panel A, with the coefficients on *AUD_FAIL_X* being significant at the 0.01 level (two-tailed). The results for non-Big 4 offices are significant at both materiality thresholds ($p < 0.10$, two-tailed) indicating that income-increasing abnormal accruals are higher in non-Big 4 offices with at least one audit failure. Taken together, the results in both panels of Table 4 indicate that contagion occurs in the offices of both non-Big 4 and Big 4 accounting firms, with the caveat that results are not significant for absolute abnormal accruals in non-Big 4 offices. We conclude that the presence of a downward material client restatement of net income provides a method for assessing the earnings quality of clients in office-years by analyzing very simple, publicly available information.

The results in Table 4 are also economically significant. Given that *AUD_FAIL_X* is an indicator variable, the results in the fourth model of Panel A indicate that a Big 4 client company's level of absolute abnormal accruals is higher by a magnitude of 0.004 when audited by an office where at least one audit failure occurs (at the 0 percent threshold). This magnitude represents an increase of 5.1 and 4.7 (8.3 and 6.5) percent over Big 4 client companies' mean (median) values of absolute abnormal and total accruals, respectively.[19] For Panel B, the magnitude of the coefficient on *AUD_FAIL_X* in the third model of 0.009 represents a 7.2 and 4.4 (11.1 and 12.9) percent increase over non-Big 4 client companies' mean (median) values of income-increasing abnormal and total accruals, respectively.[20] Similarly, the 0.007 magnitude in the fourth model represents a 9.0 and 8.2 (14.9 and 11.4) percent increase over Big 4 client companies' mean (median) values of

[18] We cluster standard errors at the auditor-office level instead of the company level because our variables of interest vary at the office level, not the company level. Therefore, standard errors that are not clustered may be inflated due to including multiple observations of the same auditor office in the sample. However, if standard errors are clustered at the company level instead, all results are very similar in terms of the statistical significance on all test variables.

[19] The untabulated mean/median values of absolute abnormal and (total) accruals for Big 4 client companies are 0.0778/0.0481 and (–0.0851/–0.0616).

[20] The untabulated mean/median values of income-increasing abnormal and (total) accruals for non-Big 4 client companies are 0.1246/0.0811 and (–0.2063/–0.0700).

TABLE 4

Audit Failures within an Auditor Office and Abnormal Accruals
Client Company-Level Analysis

Panel A: Dependent Variable = *ABS_ABN_ACC*

		Audit Failure Threshold (*AUD_FAIL_X*)			
		> 0%		> 10%	
Variable	**Pred.**	**Non-B4**	**B4**	**Non-B4**	**B4**
Test Variable					
AUD_FAIL_X	+	0.004	0.004**	0.002	0.004**
(p-value)		(0.303)	(0.011)	(0.683)	(0.015)
Control Variables					
OFFICE_SIZE	−	0.000	−0.002**	−0.001	−0.003***
RISK_PORT	?	−0.001	−0.001	0.001	−0.001
CITY_IND_EXP	?		−0.000		−0.004
NAT_IND_EXP	?		0.001		−0.003
INFLUENCE	?	−0.001	0.001	−0.001	0.000
SIZE	−	−0.014***	−0.008***	−0.013***	−0.008***
LAG_TOT_ACC	−	−0.049***	−0.019**	−0.053***	−0.016*
CFO	−	−0.060***	−0.036***	−0.059***	−0.036***
CFO_VOL	+	0.089***	0.068***	0.092***	0.068***
SALES_GROWTH	+	0.016***	0.012***	0.017***	0.011***
SALES_VOL	+	0.015**	0.001***	0.013*	0.001***
PPE_GROWTH	+	0.001	0.017***	0.001	0.019***
LEV	?	−0.008	−0.019***	−0.008	−0.023***
MB	+	0.001	0.001***	0.001*	0.002***
RETURN	?	0.002*	0.002***	0.001	0.002**
RET_VOL	+	0.044***	0.043***	0.048***	0.051***
SHARE_ISSUE	?	−0.003	−0.001	−0.003	−0.001
LOSS	−	−0.028***	−0.001	−0.028***	−0.002
LITIGATE	+	−0.003	0.002	−0.002	0.003
BANKRUPTCY	−	−0.002***	−0.003***	−0.001***	−0.004***
#_OPER_SEGS	?	−0.003	0.000	−0.002	0.000
#_GEO_SEGS	?	−0.000	−0.001***	−0.000	−0.001***
Intercept	?	0.167***	0.145***	0.150***	0.140***
Year Fixed Effects		Yes	Yes	Yes	Yes
Industry Fixed Effects		Yes	Yes	Yes	Yes
SEC Regional Office Fixed Effects		Yes	Yes	Yes	Yes
n (treatment)		1,269	11,318	853	7,839
n (control)		3,193	6,846	3,193	6,846
Model p-value		< 0.001	< 0.001	< 0.001	< 0.001
R^2		46.6 %	26.9 %	46.3 %	27.9 %

(continued on next page)

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TABLE 4 (continued)

Panel B: Dependent Variable = *ABN_ACC* (> 0)

		Audit Failure Threshold (*AUD_FAIL_X*)			
		> 0%		> 10%	
Variable	**Pred.**	**Non-B4**	**B4**	**Non-B4**	**B4**
Test Variable					
AUD_FAIL_X	+	0.007*	0.006***	0.009*	0.007***
(p-value)		(0.098)	(0.003)	(0.081)	(0.003)
Control Variables					
OFFICE_SIZE	–	−0.000	−0.001	−0.001	−0.001
RISK_PORT	?	0.001	0.001	0.001	0.001
CITY_IND_EXP	?		−0.000		0.001
NAT_IND_EXP	?		−0.000		−0.001
INFLUENCE	?	0.002	0.005**	0.002	0.005**
SIZE	–	−0.009***	−0.011***	−0.008	−0.012***
LAG_TOT_ACC	–	0.012**	−0.021***	0.017**	−0.016**
CFO	–	−0.036***	−0.025***	−0.033***	−0.025***
CFO_VOL	+	0.038***	0.048***	0.037***	0.048***
SALES_GROWTH	+	0.007*	0.014***	0.008**	0.013***
SALES_VOL	+	0.006	0.002***	0.004	0.002***
PPE_GROWTH	+	−0.004	0.014***	−0.001	0.014***
LEV	?	−0.007	−0.004	−0.011	−0.004
MB	+	0.001	0.001	0.001**	0.001
RETURN	?	0.005***	0.001	0.004**	0.002
RET_VOL	+	−0.001	0.059***	−0.006	0.063***
SHARE_ISSUE	?	−0.001	0.003*	−0.002	0.002
LOSS	–	−0.011***	−0.012***	−0.011***	−0.012***
LITIGATE	+	0.001	−0.003	0.001	−0.001
BANKRUPTCY	–	−0.003***	−0.003***	−0.003***	−0.004***
#_OPER_SEGS	?	−0.004	0.001	−0.004	0.001*
#_GEO_SEGS	?	0.000	−0.001**	0.000	−0.001*
Intercept	?	0.127	0.152***	0.129	0.152***
Year Fixed Effects		Yes	Yes	Yes	Yes
Industry Fixed Effects		Yes	Yes	Yes	Yes
SEC Regional Office Fixed Effects		Yes	Yes	Yes	Yes
n (treatment)		602	5,324	398	3,701
n (control)		1,642	3,344	1,642	3,344
Model p-value		< 0.001	< 0.001	< 0.001	< 0.001
R^2		29.1%	31.1%	30.3%	31.7%

*, **, *** Indicate significance at the 0.10, 0.05, and 0.01 levels, respectively, using two-tailed tests.
t-statistics are calculated based on robust standards errors clustered at the auditor-office level. *ABS_ABN_ACC* is the absolute value of a company's abnormal accruals as calculated in Kothari et al. (2005). *ABN_ACC* is the signed value of a company's abnormal accruals as calculated in Kothari et al. (2005). *AUD_FAIL_X* is 1 when at least one low-quality audit failure occurs within the same office of a company's external auditor during year *t*, and 0 otherwise. A low-quality audit is defined as existing when a client company restates net income downward by a material amount subsequent to the audit. *X* refers to the materiality level of the restatement (i.e., 0 for a greater than 0 percent downward restatement of net income and 10 for a greater than 10 percent downward restatement of net income).
See Appendix A for definitions of control variables.

income-increasing abnormal and total accruals.[21] We conclude that the accrual differences in treatment and control offices are both economically and statistically significant.

In untabulated results we also examine the effect of lagged values of *AUD_FAIL_0* on current (year *t*) abnormal accruals in order to test if the contagion effect on earnings quality persists over time for auditor offices having an audit failure. These results show a significant lagged contagion effect for Big 4 auditors in years *t*–1, *t*–3, and *t*–4, and a significant lagged contagion effect for non-Big 4 auditors in years *t*–2, *t*–3, and *t*–5. Given that, for both auditor types, we find a significant contagion effect in three of the five lagged years, we conclude that there exists a greater likelihood of lower earnings quality in the current year *t* for those auditor offices with recent past audit failures. These results complement the analysis in Table 3 and further indicate that some offices have a serial problem with low-quality audits.

Tests of H2—Contagion Effect for Larger versus Smaller Offices

Table 5 investigates whether the contagion effect in auditor offices is conditioned by the size of the office (H2), and is motivated by the findings that larger Big 4 offices produce higher-quality audits. For completeness we analyze both Big 4 and non-Big 4 offices in separate tests. For this analysis we create an indicator variable *LARGE_OFFICE*, and this variable is coded 1 for auditor offices that are in the largest quartile of offices (*OFFICE_SIZE*), and 0 otherwise. For Big 4 offices, the cutoff is $13.5 million of audit fees (using the size distribution of Big 4 offices), and the cutoff is $667,000 in audit fees for non-Big 4 offices (using the size distribution of non-Big 4 offices). By comparing the largest quartile of auditor office size to the other 75 percent of offices within each group, the test is biased against finding a contagion effect in smaller offices because many of the offices coded as "small" are still quite large and well above median office size.

For brevity, Table 5 reports results only for the 10 percent threshold; however, signs and significance levels are comparable at the 0 percent materiality threshold. Further, we do not report the coefficients on the control variables, although all controls that are in the models in Table 4 are included. In Table 5 the test variable *AUD_FAIL_10*, by itself, determines if a contagion effect exists in the smallest 75 percent of auditor offices. The interaction coefficient *AUD_FAIL_10* $*$ *LARGE_OFFICE* tests the incremental difference for the largest quartile of offices relative to the smallest 75 percent of offices. The total contagion effect for larger offices is the sum of the coefficients on *AUD_FAIL_10* and *LARGE_OFFICE* $*$ *AUD_FAIL_10*, and an F-statistic test if the sum is different from 0 (test of H2).

In the test of absolute abnormal accruals (first two models), the coefficients on *AUD_FAIL_10* are positive and significant at $p < 0.01$ for both auditor groups, and the coefficients on the interaction term *AUD_FAIL_10* $*$ *LARGE_OFFICE* are negative and significant ($p < 0.01$ and $p < 0.05$, two-tailed, respectively) for both Big 4 and non-Big 4 offices. This means that earnings quality is lower (larger absolute abnormal accruals) for offices that are in the smallest 75 percent of the distribution for each auditor type. In contrast, there is no evidence of contagion for the largest quartile of office size for either Big 4 or non-Big 4 offices. The interaction terms are negative and significant, indicating smaller abnormal accruals relative to smaller office. The F-test of the sum of the coefficients (*AUD_FAIL_10* $+$ *LARGE_OFFICE* $*$ *AUD_FAIL_10*) is statistically insignificant at the 0.10 level in both cases, which means the sum of the coefficients is not different from 0. We conclude that office size appears to mitigate contagion effects.

For the test of income-increasing abnormal accruals (third and fourth models), the results are comparable to those for the full sample of absolute abnormal accruals. The clients of smaller offices

[21] The untabulated mean/median values of income-increasing abnormal accruals for Big 4 client companies are 0.0780/0.0469. See footnote 23 for mean and median values of total accruals for Big 4 client companies.

TABLE 5

Audit Failures within an Auditor Office and Abnormal Accruals Dependent on Auditor Office Size

		Dependent Variable is			
		ABS_ABN_ACC		*ABN_ACC* > 0	
Variable	**Pred.**	**Non-B4**	**B4**	**Non-B4**	**B4**
Test Variables					
LARGE_OFFICE	?	0.007	0.001	−0.002	0.006**
AUD_FAIL_10	+	0.012***	0.008***	0.013**	0.011***
(p-value)		(0.005)	(0.002)	(0.024)	(0.001)
AUD_FAIL_10 ∗ *LARGE_OFFICE*	−	−0.014***	−0.008**	−0.013	−0.010**
(p-value)		(0.001)	(0.036)	(0.196)	(0.018)
F-test [*AUD_FAIL_10* + *AUD_FAIL_10* ∗ *LARGE_OFFICE*]		1.7	0.1	0.0	0.3
(p-value)		(0.121)	(0.891)	(0.982)	(0.599)
All Control Variables		Yes	Yes	Yes	Yes
Year Fixed Effects		Yes	Yes	Yes	Yes
Industry Fixed Effects		Yes	Yes	Yes	Yes
SEC Regional Office Fixed Effects		Yes	Yes	Yes	Yes
n		4,046	14,685	2,040	7,045
R^2		45.1%	27.9%	29.3%	31.7%

*, **, *** Indicate significance at the 0.10, 0.05 and 0.01 levels, respectively, using two-tailed tests.
t-statistics are calculated based on robust standards errors clustered at the auditor-office level. *ABS_ABN_ACC* is the absolute value of a company's abnormal accruals as calculated per Kothari et al. (2005). *ABN_ACC* is the signed value of a company's abnormal accruals as calculated per Kothari et al. (2005). Only observations with values of *ABN_ACC* greater than 0 (income-increasing abnormal accruals) are used in the third and fourth models. *AUD_FAIL_X* is 1 when at least one audit failure occurs within the same office of a company's external auditor during year *t*, and 0 otherwise. An audit failure is defined as existing when at least one of the auditor's client companies within an auditor office subsequently restates net income downward in the future by threshold level *X*. *X* refers to the threshold level of the restatement (i.e., 0 (10) for a greater than 0 (10) percent restatement of net income). *LARGE_OFFICE* is 1 for Big 4 auditor offices that are larger than the 75th percentile value of *OFFICE_SIZE*, and 0 otherwise. All control variables presented in Table 4 are included in the models in this table, but are not reported for brevity.
See Appendix A for definitions of control variables.

with audit failures have larger abnormal accruals as evidenced by the positive and significant coefficients on *AUD_FAIL_10* ($p < 0.05$ and $p < 0.01$, two-tailed, respectively). In contrast, the F-tests indicate no evidence of contagion for larger offices. The results in Table 5 are consistent with prior studies that find that larger offices conduct audits of higher-quality (Francis and Yu 2009; Choi et al. 2010).[22] It also appears that the insignificant results for absolute abnormal accruals of non-Big 4 auditor clients in Table 4, Panel A are driven by the largest offices because the set of smaller non-Big 4 offices in Table 5 clearly show a contagion effect with respect to both absolute and income-increasing abnormal accruals.

[22] As an alternative to creating an indicator variable that splits offices into small and large, we use the continuous variable *OFFICE_SIZE* and interact this with *AUD_FAIL_X*. In a separate model we also interact *B4* with all other control variables. The interaction coefficients in all models as presented in Table 5 are negative and significant ($p < 0.05$), indicating that the contagion effect diminishes as office size increases.

Tests of H3—Contagion Effect and Big 4 Office-Level Industry Expertise

Table 6 presents the results of testing if a contagion effect in a Big 4 office depends on the percentage of audits performed in its areas of city-level industry leadership (H3). For brevity, Table 6 only reports the tests of income-increasing abnormal accruals at the 10 percent materiality threshold, but the results are similar when testing absolute abnormal accruals and when testing both types of accruals at the zero percent threshold. The presentation of included control variables is, again, suppressed.

The indicator variable *HIGH_OFFICE_EXPERTISE* in Table 6 is coded 1 when the value of *OFFICE_EXP_%* for a Big 4 office is greater than the sample median of 0.740.[23] The model specification also includes *AUD_FAIL_10*, and the interaction term *AUD_FAIL_10* $*$ *HIGH_OFFICE_EXPERTISE*. Given this specification, the variable *AUD_FAIL_10* by itself tests if a contagion effect occurs in Big 4 offices in which the office conducts less than 74 percent of audits in its areas of city-level of industry leadership. The interaction term (*AUD_FAIL_10* $*$ *HIGH_OFFICE_EXPERTISE*) tests if the results are incrementally different for auditor offices with high expertise, i.e., 74 percent or more of its audits are in its areas of city-level industry leadership. The F-statistic on the sum of the coefficients (*AUD_FAIL_10* + *AUD_FAIL_10* $*$ *HIGH_OFFICE_EXPERTISE*) tests the total contagion effect for offices where the auditor conducts a relatively *high* percentage of audits in its areas of city-level of industry leadership.

The first model in Table 6 analyzes all Big 4 offices. The positive and significant ($p < 0.01$) coefficient on *AUD_FAIL_10* indicates that a contagion effect exists in the Big 4 auditor offices that conduct a lower percentage of audits in its areas of industry expertise. In contrast, the F-statistic on the sum of *AUD_FAIL_10* and *AUD_FAIL_10* $*$ *HIGH_OFFICE_EXPERTISE* is not significant, indicating no contagion for offices that conduct a larger percentage of audits in their areas of industry expertise. Thus, H3 is supported.

The second model in Table 6 examines just the largest quartile of Big 4 offices, and tests if contagion occurs in large offices that have a lower percentage of audits in the office's areas of industry leadership. Using the same model specification, none of the test variables are statistically significant at the 0.10 level, which means that the level of industry expertise has no effect for larger offices. Taken together, the results in Table 6 indicate that the use of industry expertise is important for the smaller 75 percent of Big 4 offices, but not for the largest quartile of office size. That is, the negative effect of smaller Big 4 office size, documented in Table 5, is mitigated when these offices conduct a larger percentage of audits in its areas of city-level industry leadership. In contrast, for the largest quartile of Big 4 offices, industry leadership has no statistical relation. An explanation is that the better human capital and/or quality control in the largest quartile of offices compensates for the relatively lower use of industry expertise. This finding extends Francis and Yu (2009) and Choi et al. (2010) by showing that a greater use of industry expertise mitigates the small-office effect documented in these studies.[24]

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[23] We exclude all offices with less than ten clients in the analyses in Table 6 to avoid the possibility that very small offices are significantly influencing the calculation of *OFFICE_EXP_%*. This is possible because a very small denominator (total number of clients) for this variable calculation could create very large expertise percentages and possibly bias results.

[24] Similar to Table 5, as an alternative to creating an indicator variable that splits offices into low and high use of industry expertise, we use the continuous variable *OFFICE_EXP_%* and interact this with *AUD_FAIL_X*. In a separate model we also interact *B4* with all other control variables. The interaction coefficient in model one is negative and significant ($p < 0.10$) indicating that the contagion effect diminishes as offices make use of more industry expertise. The interaction coefficient is insignificant in model two, which is consistent with our tabled results.

TABLE 6

Audit Failures in Big 4 Offices and Income-Increasing Abnormal Accruals Dependent on City-Level Industry Expertise

		Dependent Variable is *ABN_ACC* > 0	
Variable	**Pred.**	**All Big 4 Offices**	**Largest Big 4 Offices**
Test Variables			
HIGH_OFFICE_EXPERTISE	?	−0.005	−0.013
AUD_FAIL_10	+	0.010***	0.001
(p-value)		(0.003)	(0.875)
*AUD_FAIL_10 * HIGH_OFFICE_EXPERTISE*	−	−0.005	0.001
(p-value)		(0.209)	(0.856)
F-stat [*AUD_FAIL_10 + AUD_FAIL_10 * HIGH_OFFICE_EXPERTISE*]		2.1	0.1
(p-value)		(0.172)	(0.720)
All Control Variables		Yes	Yes
Year Fixed Effects		Yes	Yes
Industry Fixed Effects		Yes	Yes
SEC Regional Office Fixed Effects		Yes	Yes
n		5,664	3,821
Model p-value		< 0.001	< 0.001
R^2		33.0%	30.8%

*, **, *** Indicate significance at the 0.10, 0.05 and 0.01, respectively, using two-tailed tests.
t-statistics are calculated based on robust standards errors clustered at the auditor-office level. The "Largest Big 4 Offices" are Big 4 auditor offices that are larger than the 75th percentile value of *OFFICE_SIZE*, and 0 otherwise. The 75th percentile value for Big 4 offices is $4.9 million in audit fees. *ABN_ACC* is the signed value of a company's abnormal accruals as calculated per Kothari et al. (2005). Only observations with values of *ABN_ACC* greater than 0 (income-increasing abnormal accruals) are analyzed. *AUD_FAIL_10* is 1 when at least one audit failure occurs within the same office of a company's external auditor during year *t*, and 0 otherwise. An audit failure is defined as existing when at least one of the auditor's client companies within an auditor office subsequently restates net income downward in the future by 10 percent or more of originally reported net income. *HIGH_OFFICE_EXPERTISE* is 1 when the percentage of audits conducted within an auditor office in a year where the auditor is the city-level industry expert is greater than the median value of 0.740 for all Big 4 auditor-office-year observations, and 0 when it is below the median value. The percentage is calculated for each office-year by scaling the number of audits conducted within an office in a year where the auditor is the city-level industry expert by the total number of audit engagements in the office in the same year. An auditor is the city-level industry expert when it has the highest market share of audit fees within a Metropolitan Statistical Area in an industry defined by two-digit SIC codes. All control variables presented in Table 4 are included in the models in this table, but are not reported for brevity.
See Appendix A for definitions of control variables.

Sensitivity Analyses

The results are robust to an alternative coding of the office-year test variable *AUD_FAIL_X*, which takes into consideration the size of the office. Specifically, we use the variable *PERC_FAIL_X*, which is calculated as the number of downward restatements in an office-year scaled by the number of clients in the office-year. Therefore, this variable reflects the percentage of clients that restate in an auditor office-year. The results in Tables 4 through 6 are robust to this alternative coding of the office-year test variable (note this analysis is not pertinent to Table 3). More specifically, results are virtually identical to those tabulated if we use a ranked version of *PERC_FAIL_X*, where *PERC_FAIL_X* is sorted into 11 ranks (the ranked variable equals 0 if there are zero audit failures in an office-year, and is then ranked into 10 additional partitions where the number of audit failures in an office-year is greater than 0, with an equal number of observations within each partition). Results are qualitatively similar if we use the raw form of *PERC_FAIL_X* instead of the ranked form.

We also investigate whether our results are robust to controlling for whether a company is an accelerated filer, and controlling for the quality of client companies' internal controls. We do this by creating two new variables: *ACCEL*, which equals 1 for firm-year observations where the company is an accelerated filer, and *SOX404FAIL*, which equals 1 when the audit report deems the company's internal controls as ineffective. Given that auditors began issuing audit opinions regarding internal controls only after Sarbanes-Oxley, and that data on this variable appear in Audit Analytics beginning only in 2004, we code *SOX404FAIL* as equal to 1 for a company in the years 2000 through 2003 if *SOX404FAIL* is equal to 1 in 2004. We do this to retain our full sample because it seems reasonable to assume that if a company's internal controls were ineffective in 2004 it is very likely they were also ineffective in the recent years prior to 2004. In all analyses *ACCEL* is always negative, but is significant in only about a quarter of the regression tests. Further, *SOX404FAIL* is never significant. Finally, and most importantly, our results in Tables 3 through 6 are not affected by the addition of these two variables and our results remain very similar to those tabulated.[25]

We also test whether our results are robust to calculating materiality with respect to originally reported sales rather than to net income. Specifically, we scale the downward change in net income by a company's net sales and consider materiality levels of 1, 2, and 10 percent of net sales to be an audit failure. All tabulated results in the study are very similar using all three sales-based materiality thresholds.

As an additional control, the models are re-estimated with auditor-office fixed effects, and these results are qualitatively similar to those reported in the tables. This procedure controls for potential omitted correlated variables with respect to office characteristics, and gives additional confidence the results in the study are not the consequence of omitted office variables.

We also investigate if one or more specific Big 4 audit firms is systematically driving the results, or whether the contagion effect is a characteristic of all of the Big 4 firms. To do so, we rerun all of our regression models in Tables 3 through 6 on separate samples of each individual Big 4 auditor. These auditor-specific regressions are qualitatively the same as the pooled results roughly two-thirds of the time. Further, differing results are spread evenly among the individual Big 4 audit firms. We conclude that there exists no systematic difference in the contagion effect across the Big 4 firms.

Many non-Big 4 accounting firms have only a few office locations. Untabulated analysis shows that out of the total of 1,997 non-Big 4 offices in the sample, 794 are single-office audit firms. Therefore, it is possible that these very small audit firms are driving the results for non-Big 4 auditors. In order to test whether the main results are sensitive to this we delete all observations where a company is audited by a non-Big 4 audit firm that has only one office location. All tabulated results are very similar using this reduced sample.

Similarly, it is possible that auditor offices with very few clients are driving the results. To address this concern we delete all companies audited by a non-Big 4 (Big 4) auditor office where the total number of clients of the office in a year is less than two (four). These client numbers represent the 25th percentile level for each auditor type (i.e., 25 percent of non-Big 4 and Big 4

[25] The *SOX404FAIL* variable used in the re-analyses of models in Table 3 is the mean value of this variable for all clients within an office-year.

auditor offices have less than two and four clients, respectively). Results are very similar compared to tabulated results.

Audit Analytics (AA) identifies a subset of firm-level restatements as occurring specifically due to accounting irregularities. As a sensitivity test we consider only downward restatements of net income due to these irregularities to be an audit failure. All results are qualitatively the same for these restatements compared to the tabulated results in terms of coefficient signs and statistical significance.

Finally, a concurrent study by Francis et al. (2012) makes a limited comparison of income restatements in the Audit Analytics and Compustat Unrestated Quarterly databases and finds that both contain some errors, but when the two databases are in agreement, they correspond to SEC filings. Consequently, we perform a robustness test by keeping only restatement observations where Audit Analytics and Compustat are in agreement. Since Audit Analytics reports only the cumulative amount of restatements, we average this over the restatement period to estimate the yearly effects that are then compared with Compustat. We retain only those restatements where the office-year test variable *AUD_FAIL_X* is coded the same using both databases. For this reduced sample, untabulated results indicate that the tests are virtually the same as those tabulated for Big 4 offices. Specifically, all Big 4 results in Tables 3 through 6 are the same in terms of the signs on the coefficients and significance levels for *AUD_FAIL_X* except for one model where significance is at the 0.10 level compared to the tabulated 0.05 level. Results for non-Big 4 offices are qualitatively the same for about three-quarters of the models in Tables 3 through 6. We conclude that potential measurement error in the restatement databases does not affect the study's results.

V. CONCLUSION

We investigate if the presence of an audit failure in an office-year reveals the likelihood of a "contagion effect" that results in other concurrent low-quality audits in the office. We test this by determining if the presence of at least one audit failure, measured as the downward restatement of client earnings, indicates a systematic problem in the average quality of concurrent audits performed in the same office for the same year that the misstated earnings were originally reported. We first show that offices with client restatements in the past are more likely to have new client restatements for up to five years in the future, suggesting a contagion of audit failures over time, and that accounting firms do not seem to quickly identify and remediate the quality-control problems in these offices.

Next we examine if the earnings quality (abnormal accruals) of clients in offices with audit failures is lower, on average, than clients in those offices with no audit failures. We find that in auditor offices where an audit failure occurred, the concurrent clients of that office (in the same year) have a higher level of abnormal accruals compared to offices with zero audit failures. These results hold for all but the largest quartile of office size, which indicates that office size is also an important factor in contagion. Last, we find that a relatively high use of industry expertise in a Big 4 office can mitigate the negative effect of small office size.

As discussed in the introduction, our findings should be of interest to regulators, audit standard-setters, accounting firms, and investors because we provide a method to infer the overall quality of an auditor office location through the use of easily obtainable and publicly available information on restatements. Regulators can use this information to identify offices where audits are more likely to be of lower quality (and perhaps even below minimum standards), and standard-setters may be able to use this information to develop standards that emphasize the potential for quality-control problems in the offices of multi-location audit firms. Accounting firms can benefit from this method as those in charge of quality-control processes in a firm's national office can use it to identify specific offices that may not be implementing the firm's quality-control procedures appropriately. Finally, investors may be able to use it as one piece of additional information with which to infer something about the earnings quality of a particular company based on the history of the auditor office that performs the audit.

The study also has some limitations. First, our measure of an audit failure within an auditor office relies on the assumption that every company that "should" restate earnings actually does so. This is not likely to be the case. However, we note that in cases where an audit engagement is misclassified as not being an audit failure (no restatement is issued) when it may in fact be a poor-quality audit (that is unobservable) biases against finding a statistically significant result. Second, our sample is limited to audits of publicly traded companies. To the extent that a particular auditor office also performs audits of smaller, private companies, our results cannot be generalized to this client base.

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APPENDIX A
VARIABLE DEFINITIONS

Dependent Variables

ABS_ABN_ACC = absolute value of a company's abnormal accruals as calculated in Kothari et al. (2005), with control for concurrent return on assets; and

ABN_ACC = signed value of a company's abnormal accruals as calculated in Kothari et al. (2005). Analyses using *ABN_ACC* examine only income-increasing abnormal accruals.

Test Variables

B4 = 1 if the company hires a Big 4 auditor in year *t*, and 0 otherwise;

AUD_FAIL_X = 1 when at least one low-quality audit failure occurs within the same office of a company's external auditor during year *t*, and 0 otherwise. A low-quality audit is defined as existing when a client company restates net income downward by a material amount subsequent to the audit. *X* refers to the materiality level of the restatement (i.e., 0 for a greater than 0 percent downward restatement of net income and 10 for a greater than 10 percent downward restatement of net income). Auditor office locations are taken from Audit Analytics;

LARGE_OFFICE = 1 for Big 4 auditor offices that are larger than the 75th percentile value of *OFFICE_SIZE* (see below for the definition of *OFFICE_SIZE*), and 0 otherwise. The 75th percentile value for non-Big 4 (Big 4) offices is $667,000 ($13.5 million) in audit fees. This variable is used only in Tables 5 and 6; and

HIGH_OFFICE_EXPERTISE = 1 when the percentage of audits conducted within an auditor office in a year where the auditor is the city-level industry expert is greater than the median value of 0.740 for all Big 4 auditor-office-year observations, and 0 when it is below the median value. The percentage is based on the definition of *OFFICE_EXP_%*, which is defined below. An auditor is the city-level industry expert when it has the highest market share of audit fees within a Metropolitan Statistical Area in an industry defined by two-digit SIC codes (see the variable definition for *CITY_IND_EXP* below). This variable is used only in Table 6.

Auditor Office-Level Control Variables

OFFICE_SIZE = natural log of the total dollar amount of audit fees charged to all audit clients within an auditor office in year *t*. Auditor office locations are taken from Audit Analytics;

RISK_PORT = mean value of the average of client assets, leverage, and return on assets for an auditor office in year *t*. The average is calculated by taking the mean level of client assets, leverage, and return on assets. These means are then standardized so that each has a mean of 0 and a standard deviation of 1. These standardized values are then averaged together to form the overall client risk portfolio for the auditor office in a year; and

OFFICE_EXP_% = number of audits conducted in a Bog 4 office in a year where that office is the city-level industry leader, scaled by the total number of audits conducted by the office in the year.

Firm-Level Control Variables

CITY_IND_EXP = 1 if the company's auditor is the city-level industry expert auditor, and 0 otherwise, where industry expertise is calculated based on total audit fees charged by the audit firm to clients within a particular Metropolitan Statistical Area and industry (similar to Francis et al. 2005). The audit firm with the highest amount of audit fees within an industry in a city-year is classified as the city-level industry expert. Industries are defined at the two-digit SIC code level;

NAT_IND_EXP = 1 if the company's auditor is the national-level industry expert auditor, and 0 otherwise, where industry expertise is calculated based on total audit fees charged by the audit firm to clients in a particular industry within the U.S. The audit firm with the highest amount of audit fees within an industry-year is classified as the national industry expert. Industries are defined at the two-digit SIC code level;

INFLUENCE = total dollar value of both audit and nonaudit fees charged to a specific client in year *t*, scaled by the total audit fees charged by the auditor office in the same year;

SIZE = natural log of a company's total assets in year *t*;

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LAG_TOT_ACC = company's total accruals scaled by total assets in year *t*–1;
CFO = company's cash flows from operations in year *t* scaled by lagged total assets;
CFO_VOL = standard deviation of a company's cash flows from operations from year *t*–2 through year *t*;
SALES_GROWTH = one-year percentage growth in a company's sales from year *t*–1 to year *t*;
SALES_VOL = standard deviation of a company's sales from year *t*–2 through year *t*;
PPE_GROWTH = one-year percentage growth in a company's net property, plant, and equipment from year *t*–1 to year *t*;
LEV = company's total debt in year *t*, scaled by lagged total assets;
MB = company's market value of equity scaled by book value of equity at the end of year *t*;
RETURN = company's 12-month stock return during year *t*–1;
RET_VOL = standard deviation of a company's monthly stock returns during year *t*;
SHARE_ISSUE = 1 if the company issues additional shares in year *t*, and 0 otherwise;
LOSS = 1 if the company records net income below 0 in year *t*, and 0 otherwise;
LITIGATE = 1 if a company is within the following SIC codes: 2833–2836, 3570–3577, 3600–3674, 5200–5961, and 7370), and 0 otherwise;
BANKRUPTCY = probability of bankruptcy using the Altman-Z score [(0.717 ∗ net working capital/assets) + (0.847 ∗ retained earnings/assets) + (3.107 ∗ earnings before interest and taxes/assets) + (0.42 ∗ book value of equity/liabilities) + (0.998 ∗ sales/assets)] (Altman 1983);
#_OPER_SEGS = number of operating segments the company operates in during year *t*; and
#_GEO_SEGS = number of geographic segments the company operates in during year *t*.