Complementarity between Audited Financial Reporting and Voluntary Disclosure: The Case of Former Andersen Clients

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Abstract

Ball and Shivakumar (2008) suggest mandatory periodic reporting disciplines disclosure and encourages timely voluntary disclosure. We examine this hypothesis using the shock to reporting quality experienced by former Arthur Andersen clients after they were forced to switch auditors. Consistent with the confirmatory role of mandatory reporting, we find that former Andersen clients increase disclosure following the switch. They increase forecasting frequency and enhance forecasting precision and specificity. They also show less return concentration around earnings announcements in bad-news quarters, consistent with timelier release of bad news (Rowchowdury and Sletton 2012). Our findings demonstrate complementarity between audit quality and voluntary disclosures.

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1. Introduction

> *Accurate reporting of actual earnings outcomes exerts an accountability discipline on managers' and analysts' more-timely expectational statements, such as growth prospects and earnings forecasts.*
> *(Ball and Shivakumar 2008)*

We examine changes in voluntary disclosure around forced auditor switches due to the collapse of Arthur Andersen, to investigate how involuntary changes in financial reporting affect voluntary disclosure. Prior research finds that firms experience an increase in auditor scrutiny of reporting practices and thus more reliable financial statements following the forced switch in auditors. Cahan and Zhang (2006) and Krishnan (2007) find that the earnings quality of former Arthur Andersen clients improved after they were forced to switch auditors, as measured by a reduction in abnormal accruals and an increase in auditor conservatism. More recently, Dyck, Morse, and Zingales (2014) find increased fraud detection rates by the new auditors of Andersen clients.[1]

Consistent with complementarities between mandatory and voluntary disclosures, we find that previous Andersen clients increase management forecasting frequency, precision, horizon, and specificity following their forced switch to a new auditor. We also find increased response coefficients at forecast-release dates, suggesting that increased reliability enhances voluntary disclosure. Using a quarterly return-concentration test (Roychowdhury and Sletten 2012), we find that returns are less concentrated around earnings announcements in bad-news quarters. These

[1] Many Andersen partners and staff switched to the new audit firms, while retaining their client relations. Even so, the switch can still affect auditor scrutiny, because partners of the most questionable quality were not taken on by other audit firms, and because the supervisory structure for Andersen partners and staff changed when they switched. Moreover, the switching firms are considered "new clients" by the new audit firm, requiring more significant auditing efforts.

results are consistent with enhanced voluntary disclosure by former Andersen clients following their switch to a new auditor.

Research on the confirmatory role of financial reporting motivates our work. Gigler and Hemmer (1998) distinguish the confirmatory role of reporting from the view that financial reports are the primary information source. In their model, mandatory reports establish the credibility of voluntary disclosures. In this way, the reliable mandatory reports discipline managers and thus facilitate voluntary disclosure that preempts the less timely mandatory reports and promotes the informational efficiency of prices. The confirmatory role implies a complementary (positive) relation between disclosure frequency and earnings quality.[2] Lang and Lundholm (1996) find a negative correlation between disclosure quality and value relevance of earnings, while Francis, Nanda, and Olsson (2008) find a positive relation between voluntary disclosure and measures of earnings quality. Ball, Jayaraman, and Shivakumar (2012) try to isolate the confirmatory role empirically and find that management-forecast quality increases in excess audit fees—their proxy for the degree of financial statement verification. Li and Yang (2016) find increased voluntary disclosure following mandatory IFRS adoption. In light of the difficulty of isolating the exogenous component of audit fees and changes in financial reporting quality following the adoption of IFRS (e.g., Christensen et al. 2013), we complement these studies by examining a shock to audit quality in a single country where the institutional environment is held constant.

We attempt to identify the mechanism linking disclosure to financial reporting by using an exogenous shock to financial reporting quality caused by forced auditor changes. Following the collapse of Arthur Andersen (AA), firms had to switch auditors involuntarily, which resulted in more reliable financial statements (Dyck, Morse, and Zingales, 2014). The advantage of our setting

[2] Arya et al. (2004) also show how the disciplining role of accounting enhances interim communication through optimal contracting.

is that it generates an exogenous change in earnings reliability in the sense that the auditor change is not the firm's choice and is not driven by changes in factors that can also affect voluntary disclosure incentives.[3] We employ a difference-in-difference design around the shock, which allows us to better establish a causal link between the quality of mandatory and voluntary disclosure, which is necessary to test the confirmation hypothesis.

To examine the relation between voluntary and mandatory disclosure, we focus on management forecasts. We do so for several reasons. First, the confirmation hypothesis suggests that better earnings quality provides better confirmation of managements' voluntary forecasts. Second, using management forecasts, we can examine both the decision to disclose and the characteristics and quality of the disclosure. Finally, management forecasts are available for much of our sample firms and for a robust control group of firms.

Our findings suggest that, following an involuntary increase in mandatory reporting quality, firms provide better voluntary disclosures. Specifically, the propensity to provide a forecast and the number of forecasts increases following the auditor switch. The effect is economically significant. For example, compared to non-Arthur Andersen clients (non-AA firms), firms audited by Arthur Andersen (Andersen) increase the number of forecasts issued in a year by 13.0% of the average value for our sample, after they switch auditors. In addition, the forecasts of AA firms show greater increases in specificity and precision, compared with non-AA firms. For example, relative to non-AA firms, AA firms increase the precision of forecasts by 14.1% of the average value for our sample. Finally, our treatment firms increase the relative horizon of their forecasts as well.[4]

[3] In additional analysis, we confirm that our treatment firms do not experience a relative change in litigation risk. See Section 4.2.5 and Table 8.
[4] We find no significant increase in litigation risk for AA firms.

We also examine changes in investors' reaction to management forecasts. Consistent with an increase in mandatory reporting quality improving voluntary disclosure quality, our findings show that prices respond more to management forecasts for AA firms after they switch auditors. This result suggests that investors find these forecasts more informative and credible in comparison to our control sample.

To examine the more general disclosure behavior of our treatment firms, we follow the empirical approach of Roychowdhury and Sletten (2012) and examine how changes in audit quality, which affect the quality of earnings reporting, affect the differential earnings informativeness of bad news. In other words, we test whether bad news is delayed less for AA firms after they switch auditors. Our results imply that AA firms disclose bad news in a timelier fashion after they switch auditors, which is consistent with the relative increase in disclosure quantity and quality for AA firms. As an additional analysis, we examine whether AA firms gain in terms of cost of capital. Our findings are consistent with AA firms experiencing a relative decline in their cost of capital.

Finally, we examine how these effects relate to the costs of switching auditors. We expect a more significant response for firms that had higher costs of switching auditors, as these costs "locked-in" lower audit quality. To gauge the net costs of auditor switching, we use firm age. Older firms tend to have longer auditor tenures, more complex business models, and they tend to be less financially constrained. These inferences suggest larger Andersen clients would face greater switching costs and a lower benefit from the increased audit quality that can accompany the switch. Therefore older firms are more likely to be locked to their current auditors. Our findings suggest that, among AA firms, the effects of auditor switches on disclosure policy are concentrated among

the older firms. We find that these firms increase the frequency and precision of management forecasts, while younger firms do not.

The remainder of the paper is organized as follows. We develop the predictions in Section 2, describe the research design in Section 3, report the empirical results in Section 4, conduct additional tests in Section 5, and conclude in Section 6.

2. Literature and prediction development

Mandated financial reports are one key component of the information provided by the firm to investors. Both words, "one" and "key," matter. The FASB's conceptual framework states:

> Many existing and potential investors, lenders and other creditors cannot require reporting entities to provide information directly to them and must *rely on general purpose financial reports for much of the financial information they need*… However, *general purpose financial reports do not and cannot provide all of the information* that existing and potential investors, lenders and other creditors need. (FASB, 2010, p. 2, emphasis added)

Our objective is to advance understanding of the reciprocal relation between financial reports and other sources of information. Intuition suggests that alternative sources are substitutes. To find a movie time, one either checks the local paper or the internet. A lower cost source of movie times renders others redundant. This intuition is consistent with the empirical evidence of Lang and Lundholm (1996) that analysts rate the quality of other disclosure lower when the value relevance of earnings is higher. Alternatively, information from multiple sources can be complementary when the variable of interest is unobservable or uncertain. For example, increased sample size reduces the standard error of sample estimates. Moreover, the complementarity of information extends to the incentives of parties supplying and demanding it. In Gigler and Hemmer's (1998) model, financial reports give a noisy signal that investors use to verify the truthfulness of managers' voluntary disclosures. The financial report seems redundant, because it has been pre-

empted by more timely voluntary disclosure. Yet the mandated report supports timely disclosure of otherwise unverifiable information, leading to greater price efficiency.

Interactive effects between voluntary and mandated disclosure are difficult to isolate, because the demand and supply of information depends on the firm's investment opportunities and its related need to finance additional investments from external investors who require information (Myers and Majuf, 1984; Ruland, Tung, and George, 1990; Frankel, McNichols and Wilson, 1995; Lang and Lundholm, 2000). Other factors such as litigation costs (Skinner, 1994), proprietary costs (Verrecchia, 1983), the competitive structure of a firm's industry (Darrough and Stoughton, 1990,; Feltham and Xie, 1992), and ex-ante information asymmetry between insiders and outsiders (Lang and Lundholm, 1993; Tasker, 1998) can also affect the willingness of firms to supply information.[5]

We use a shock to the quality of the firm's mandated disclosure to isolate the interaction between changes in mandatory disclosure quality and voluntary disclosure. The demise of Arthur Andersen in 2002 forced its clients to switch to other public accounting firms or delist. Research suggests the switch improved the audit quality of former Andersen clients. Dyck, Morse, and Zingales (2014) find that the new auditors are approximately four times more likely to reveal that firms formerly audited by Arthur Andersen had a fraud in 2001 and 2002, compared to firms that did not have Andersen as their auditor during this period. These results confirm the findings of Fuerman (2006) that the audit quality of Andersen clients declined in the few years prior to the auditor's demise. He finds that Andersen was more likely to be sued than other Big-N accounting firms in the period from 1999 through 2004 but that there is no difference in litigation rates between Andersen and other Big-N firms in earlier periods (1996–1998). Moreover, Cahan and Zhang (2006) show that the earnings quality of former Andersen clients increased after they were

[5] Healy and Palepu (2001) and Core (2001) offer excellent reviews.

forced to switch auditors, as measured by decreases in abnormal accruals. They attribute this change to increased auditor conservatism. Relatedly, Krishnan (2007) finds that the earnings of former Andersen clients became more conservative after they were forced to switch auditors.

Prior research also suggests that audit quality deteriorated in the few years prior to Arthur Andersen's demise. Dyck et. al. (2014) find no difference in the m-scores (Beneish, 1999) between the clients of Andersen and other Big-N audit firms between a 1998 and 2001, and Eisenberg and Macey (2004) find that Andersen clients are no more likely to have restatements in the General Accounting Office database between 1997 and 2001.[6] Ball (2009) suggests that a combination of rogue partners and branch offices as well as culpability in the Enron and WorldCom events forced Andersen out of business.

If the shift to a new auditor strengthens the confirmatory nature of mandatory financial reports for Andersen clients, we expect an increase in the extent and quality of their voluntary pre-emptive disclosures. The logic of our tests resembles that of Ball, Jayaraman, and Shivakumar (2012), who measure the relation between voluntary disclosure and audit fees. They reason that audit fees capture cross-firm variation in financial reporting quality. They control for the endogenous choice of financial reporting quality using two-stage-least squares and a model for audit fees. They find that audit fees are positively correlated with the frequency and quality of management earnings forecasts. Our contribution is to use an alternative research design to isolate shocks to audit quality. Assuring adequate control for factors driving both reporting and disclosure quality is exceeding difficult. Moreover, the interaction between financial reporting quality and voluntary disclosure incentives is of sufficient interest to warrant additional study using an

[6] Hennes, Leone, and Miller (2008) suggest that the GAO restatements are an imprecise measure of fraud because nearly 74% stem from unintentional misapplications of accounting standards.

alternative method.[7] The reciprocal relation between reporting and disclosure incentives speaks to the trade-off between relevance and reliability, a central theme when evaluating the merit of accounting standards. In particular, the relevance stemming from the timeliness of mandatory reports can receive less weight than reliability of reports in the deliberations of regulators, given evidence that reports confirm and thereby encourage more timely voluntary disclosures.[8]

We note two additional considerations. First, Leuz and Schrand (2009) examine beta shocks surrounding Enron's $618 million loss disclosure on October 16, 2001. They find the announcements of accounting problems are associated with increased betas and increased 10-K disclosure for firms with positive cost of capital shocks and Arthur Andersen clients. Their conclusion is that voluntary disclosure palliates increased uncertainty. These significant events do not confound our interpretation because they appear in our pre-auditor switch sample. Therefore, we measure increased voluntary disclosure beyond increases resulting directly from the Enron effect studied by Leuz and Schrand (2009).

Second, our tests for detecting a confirmatory role for financial reporting assume investors correctly infer financial-reporting-quality level. This means that investors in firms that had used Andersen as their auditor recognized the improvement in financial reporting quality after the switch. If investors do not realize the improvement, we would expect them to misperceive the quality of firms' voluntary disclosures—reacting as strongly to voluntary disclosures by prior

[7] Li and Yang (2016) use a difference-in-difference design around the adoption of IFRS to examine the relation between changes in mandatory reporting and management forecasts. They find that the propensity to issue forecasts and the number of forecasts issued increases following the adoption of IFRS. The advantage of our setting is that it uses a single country where the institutional environment is held constant across firms. Moreover, the adoption of IFRS is associated with other regulatory changes that make it difficult to attribute the changes in voluntary disclosure following the adoption of IFRS to reporting quality per se (Christensen et al. 2013; Kalay 2014).

[8] Statement of Financial Accounting Concepts Statement 8 (FASB, 2010) mentions the "confirmatory value" of financial reports, but this term refers to the value of financial reports in confirming users' predictions (See BC3.15, p. 25). The Concept Statement seems to not acknowledge the existence of a reciprocal relation between mandatory and voluntary financial disclosures.

Arthur Andersen-audited firms as to the disclosures by other firms. To the extent that investors are not fully cognizant of the differences in audit quality, our tests have reduced power.[9] Moreover, to the extent that firms using Andersen choose lower quality audits, we would expect that they differ from other firms along other dimensions associated with the choice of low audit quality. For example, they might have lower external financing needs or have less need for financial reporting to verify the stewardship of managers. We attempt to exploit such potential differences in our cross-sectional analysis.

3. Research Design

3.1 Management forecasts characteristics

To examine how changes in audit quality change firms' voluntary disclosure, we first examine how Andersen clients' voluntary disclosure changed following their change in auditor. We use the following difference-in-differences model for each firm-year:

$$Disclosure_{i,t} = \beta_0 + \beta_1 AA_i * POST_{i,t} + \beta_2 AA_i + \beta_3 POST_{i,t} + Controls_{i,t} + \varepsilon_{i,t}, \quad (1)$$

where $Disclosure_{i,t}$ is one of the following five variables indicating different disclosure characteristics. (1) $Forecaster_{i,t}$ is a dummy variable that equals 1 if firm *i* issues at least one forecast in three out of four quarters in a year and zero otherwise (Rogers et al., 2009). (2) *Number of forecasts* is the natural log of (1+ average number of forecasts issued by firm *i* in year *t*). (3) *Precision* is the average precision of the forecasts issued by firm *i* in year *t*, where precision of a forecast equals 4 for point estimates, 3 for range estimates, 2 for open-ended estimates, 1 for qualitative estimates, and 0 for no forecasts (Armstrong et al., 2002). (4) *Specificity* is the average

[9]Giannetti and Wang (2016) find that households reduced stock holdings following the revelation of fraud by companies located in their states and that this decline was larger for states with more Arthur Andersen clients. Their results suggest that the lower audit quality of Arthur Andersen clients was not fully anticipated. Alternatively, the results could also imply overreaction by less sophisticated investors.

specificity of the forecasts issued by firm *i* in year *t* multiplied by –1, where the specificity for a range forecast equals the difference between the top and bottom of the range, scaled by the stock price one month prior to the forecast date. The specificity of a point forecast equals zero. This variable is multiplied by –1 so that higher values represent more specific forecasts. (5) *Horizon* is natural log of (1+ average horizon of forecasts in year *t*) by firm *i*, where the horizon of a forecast is the difference in days between the fiscal period end-date and the forecast date.

AA_i is an indicator variable equal to one if the firm's auditor was Andersen (AA firms) in either 2001 or 2002 (Dyck, Morse, and Zingales 2014). $POST_{i,t}$ is an indicator variable equal to 1 for the first year (*t*) following the auditor change. For AA firms and other firms that experienced auditor changes in 2001 or 2002, $POST_{i,t}$ equals 1 during 2002 or 2003, respectively. For all the switching firms, we include one pre- and one post-year in our sample and exclude the year when the firm changed auditors. For firms that did not experience auditor changes in 2001 or 2002, we use 2003 as a quasi-post-event year and 2001 as quasi-pre-event year, because most AA firms switch auditors in 2002. For these control firms, we once again only include one observation for the pre- and post-year (2001 and 2003) and exclude the pseudo change year (2002) from our sample.

The interaction term $AA_i*POST_{i,t}$ is our main variable of interest. The interaction terms measure the relative change in the disclosure policy of AA firms following their change in auditor, relative to non-AA firms. $\beta_1 > 0$ indicates that AA firms issue relatively more frequent, precise, specific, or timely forecasts following their change in auditor. $Controls_{i,t}$ are firm characteristics that research has found to be correlated with mangers' disclosure choices (e.g., Lang and Lundholm 1993, 1996; Skinner 1994; Leuz and Verrecchia 2000; Miller 2002; Li 2008). These variables include firm size, leverage, market-to-book, institutional ownership, analyst following,

age, return and earnings volatility, prior returns, ROA, the number of geographic and business segments, and inclusion in the S&P index. We also include industry fixed effects defined using two-digit SIC codes, to control for potential variation in disclosure policies across industries.

There are several advantages to the difference-in-differences design. First, while we believe that the audit and reporting quality of AA firms was lower than that of the control firms in the pre-period, our tests do not assume that AA firms have lower quality relative to non-AA firms prior to the switch in auditor. Our research design simply requires AA firms to experience a larger increase in audit quality compared to the peer firms (e.g., Dyck, Morse, and Zingales 2014). Second, our design is not affected by aggregate temporal variations that occurred during our sample period, as our research design compares different firms in the same period. In other words, our research design controls for the effect of concurrent regulations such as RegFD and SOX and any potential changes in the average/aggregate level of earnings quality in the economy. Third, our pre-period includes 2000 or 2001, depending on when firms switched auditors, which should incorporate the effects of any aggregate shocks to transparency that occurred following the revelation of the Enron and WorldCom frauds (e.g., Leuz and Schrand, 2009). This alleviates concerns that are results are driven by AA firms being disproportionally affected by an aggregate transparency shock.

3.2 The market's reaction to management forecasts

To further examine how changes in audit and earnings quality affect voluntary disclosure, we examine changes in the market's reaction to management forecasts following the auditor changes. We employ the following difference-in- differences model for each management forecast announcement day:

$$MEF\_CAR_{i,t} = \beta_0 + \beta_1 AA_i * POST_{i,t} * SURP_{i,t} + \beta_2 AA_i + \beta_3 POST_{i,t} + \beta_4 AA_i * POST_{i,t} + \beta_5 AA_i * SURP_{i,t} + \beta_6 POST_{i,t} * SURP_{i,t} + \beta_7 SURP_{i,t} + Controls_{i,t} + \varepsilon_{i,t} \quad (2)$$

MEF_CAR$_{i,t}$ is the three-day market-adjusted return around the management forecast announcement. *AA*$_i$ is an indicator variable equal to one if the firm's auditor was Andersen in either 2001 or 2002 (Dyck, Morse, and Zingales, 2014). *POST*$_{i,t}$ is an indicator variable that equals 1 in the year (*t*) following the auditor change. Similar to model (1), for AA firms and other firms that experienced auditor changes in 2001 or 2002, *POST*$_{i,t}$ equals 1 during 2002 and 2003 respectively. Also similar to model (1), for all the switching firms, we include forecasts made during the pre- and post-year and exclude forecasts made during the year when the firm changed auditors. For firms that did not experience auditor changes in 2001 or 2002, we use 2003 as a quasi-post-event year and 2001 as quasi-pre-event year. For these control firms, we once again only include forecasts made in the pre- and post-years (2001 and 2003) and exclude the forecasts made during the pseudo change year (2002) from the sample.

SURP$_{i,t}$ is the information content contained in the forecast, measured as the difference between managements' estimates and the most recent analyst consensus forecast, scaled by the magnitude of the management forecast.[10] We only retain point and range forecasts for this test and use the midpoints of range forecasts. The coefficient on the three-way interaction, β_1, is the coefficient of interest. $\beta_1 > 0$ is consistent with our conjecture that, conditional on the amount of new information in the management forecast, the market reaction to the forecast increases for AA firms after they switch auditors, relative to changes in the market reactions for non-AA firms. To control for other variables that affect the market's reaction, we include firm size, the market-to-book ratio, leverage, litigation risk, analyst following, the precision and horizon of the forecast,

[10] We find similar results when we scale the difference by the stock price in the prior quarter.

and the volatility of daily market-adjusted returns in the pre-forecast announcement period in the regression. The model also includes industry fixed effects, to control for time-invariant differences across industries, and quarter fixed effects, to control for potential time trends in disclosure across all firms.

3.3 Differential earnings informativeness

Consistent with the confirmation hypothesis, Roychowdhury and Sletten (2012) argue that the value of the earnings reporting process as an information source lies in limiting delays in the release of bad news, either by inducing managers to disclose it voluntarily or by directly releasing the negative news that managers have incentives to withhold. They support their argument by showing that the ratio of news released around earnings announcements, relative to non-announcement days, is larger for bad news than good news. We follow their empirical approach and examine how changes in audit quality, which affect the quality of earnings reporting, affect the differential earnings informativeness of bad news. Specifically, we test whether bad news is delayed less for AA firms after they switch auditors. We adopt the following difference-in-differences model for each firm-quarter:

$$Ln(NEWS\_RATIO_{i,t}) = \beta_0 + \beta_1 AA_i * POST_{i,t} * BNEWS_{i,t} + \beta_2 BNEWS_{i,t} + \beta_3 AA_i + \beta_4 POST_{i,t} + \beta_5 POST_{i,t} * AA_i + \beta_6 BNEWS_{i,t} * AA_i + \beta_7 BNEWS_{i,t} * POST_{i,t} + Controls_{i,t} + \varepsilon_{i,t}. \quad (3)$$

For this analysis, we include all the firm quarters available for our treated and control firms for the period 2000–2004.[11] $BNEWS_{i,t}$ is an indicator variable that equals 1 when the quarterly overall market-adjusted return is negative during quarter *t*. $NEWS\_RATIO_{i,t}$ is the ratio of the absolute value of market-adjusted returns in the three days around the earnings announcement,

[11] We find similar results if we exclude the switching year from the sample.

relative to the absolute value of non-earnings-announcement period returns. *Ln(NEWS_RATIO$_{i,t}$)* is the natural logarithm of the *NEWS_RATIO$_{i,t}$*. *POST$_{i,t}$* is an indicator variable equal to 1 for firms that experienced auditor changes in 2001 or 2002 prior to quarter *t*. For firms that did not experience an auditor change during 2001 and 2002, $POST_{i,t}$ equals 1 after the third quarter of 2002, because most AA firms switched auditors by the third quarter of 2002.[12] The three-way interaction term, *AA$_i$* POST$_{i,t}$* BNEWS$_{i,t}$* is the variable of interest. The coefficient on the three-way interaction term captures the relative change in the bad news concentration for AA firms after switching auditors. A timelier release of bad news results in a lower news ratio for bad news. Therefore $\beta_1 < 0$ is consistent with the conjecture that the increase in audit and earnings quality accelerates the disclosure of bad news. Following Roychowdhury and Sletten (2012), we control for firm size, book-to-market ratio, leverage, the number of trading days in quarter *t*, and the number of analysts following the firm, in the regressions.

3.4 Capital market consequences

Lastly, we examine the overall cost of capital effect of the improvement in auditor quality. Consistent with prior literature (e.g., Fama and French, 1992), we use monthly realized return as a proxy for the cost of capital or expected returns. We employ the following difference-in-differences model:

$$RET_{i,t} = \beta_0 + \beta_1 AA_i * POST_{i,t} + \beta_2 AA_i + \beta_3 POST_{i,t} + Controls_{i,t} + \varepsilon_{i,t}. \quad (4)$$

For this analysis, we include all the firm months available for our treated and control firms for the period 2000–2004. *RET$_{i,t}$* is the monthly return for firm *i* during month *t*. *POST$_{i,t}$* is an indicator variable that equals 1 if month *t* is within 24 months after the auditor change for firms

[12] Our results are robust to the use of different quarter cutoffs for $POST_{i,t}$. The results are available upon request.

that experienced auditor changes in 2001 or 2002. For firms that did not experience an auditor change during 2001 and 2002, $POST_{i,t}$ equals 1 if month *t* is after September 2002, because most AA firms changed their auditors during the third quarter in 2002.[13] $AA_i * POST_{i,t}$ is the variable of interest. The coefficient on this interaction term, β_1, captures the relative decline in the cost of capital for AA firms after they switch auditors, compared to non-AA firms. $\beta_1 < 0$ is consistent with our conjecture that improvements in audit and earnings quality ultimately reduces the cost of capital for AA firms. To control for firm characteristics that also affect stock returns, we control firm size, book-to-market ratio, stock return in the prior month, compounded returns 12 to two months before auditor changes, leverage, market beta, and factor loadings for the Fama-French five factors (Fama and French 2015).

4. Data and Empirical Results

4.1 Sample and Data

To identify firms that were affected by the demise of Arthur Andersen, we identify firms' auditors in the Audit Analytics database. A firm is defined as an AA firm if its auditor was Arthur Andersen in either 2001 or 2002. Our main sample includes all firms with data available on Compustat, nonmissing returns data on CRSP, and nonmissing data to compute the various control variables. All the variables, as well as their sources, are described in detail in the appendix. The management forecast data comes from the CIG database in First Call.

Because we use different horizons and control variables across our tests, the number of observations varies across the different tests. Therefore we report descriptive statistics for each sample separately (see Table 1). Some of our tests are executed at the firm-year level, some at the

[13] The results are robust if we use different cutoff month for $POST_{i,t}$. Results are available upon request.

announcement-day level, and some at the firm-quarter or firm-month level. We describe the sample selection steps involved for each test when describing our research design in Section 3. We winsorize all variables at 1% and 99% level and delete firms with prices lower than $2 to mitigate market microstructure concerns. Table 1 reports summary statistics for the main variables used in our paper.

[Insert Table 1 Here]

4.2 Results

4.2.1 Management forecasts characteristics

Our first analysis examines how firms that are forced to change auditors change various aspects of their management forecasts. We examine the univariate changes in the propensity to issue guidance and the number of forecasts issued in Table 2. The results show that AA firms were less likely to issue forecasts and issued fewer. While both non-AA firms and AA firms increased their disclosure following the demise of Andersen, AA firms increased their disclosure by relatively more, so that their behavior was more similar to that of non-AA firms, following their change in auditor. This results is consistent with AA firms experiencing a relative improvement in financial-reporting quality and thus increasing their voluntary disclosure. The difference in difference is positive for both the propensity to issue forecasts and the number of forecasts issued. However, the difference is only statistically significant when examining the number of forecasts issued.

[Insert Table 2 Here]

The results from our multivariate analysis (equation 1) are reported in Table 3. Columns (1) and (2) report the change in the likelihood of becoming a forecaster and forecast frequency for AA firms one year after the auditor change, relative to non-AA firms. The coefficients on the

interaction term *AA*POST* are all statistically significant at the 1% level. The coefficients for *Forecaster* and *Number of Forecasts* are 0.038 and 0.070, with *t*-statistics of 2.77 and 3.62, respectively. The effect of the auditor change on management's forecasting is economically significant. For example, compared with non-AA firms, AA firms increase their number of forecasts by 12.96% of the average value for our sample.

Consistent with our hypothesis, we also find that AA firms issue more precise and specific forecasts after they switch auditors. The coefficients on the interaction term *AA*POST* in columns (3) and (4) are positive and statistically significant, suggesting that the forecasts issued by managers in AA firms became more precise (point and range rather than open-ended or qualitative forecasts) and more specific (have a narrower range) following the auditor switch. The coefficients for *Precision* and *Specificity* are 0.154 and 0.001, with *t*-statistics of 6.33 and 2.67, respectively. These results are also economically significant. For example, compared with non-AA firms, AA firms increase the precision of forecasts by 14.1% of the average value for our sample.

We also find that AA firms issue more timely forecasts with longer horizons after they change auditors (column (5)). These results are also economically significant. Compared with non-AA firms, AA firms increase the timeliness of forecasts by 3.13% of the average value for our sample. Taken together, the results in Table 3 show the amount and quality of voluntary disclosure increases when firms experience an exogenous change in their audit/earnings quality.

[Insert Table 3 Here]

4.2.2 Market reaction to forecasts

The confirmation hypothesis implies that investors' reaction to voluntary disclosures increase when the reliability of the disclosure is improved. To test this idea, we examine whether the market reaction to management forecasts increased after AA firms switched auditors.

Specifically, we estimate equation (2) and report the results in Table 4. Our findings are consistent with our prediction: the market reaction to management forecasts increases following the auditor switch, controlling for the amount of information in the forecasts.

Our main variable of interest is the three-way interaction, *AA*POST*SURP*. It captures how the market reaction to the forecasts issued by AA firms, conditional on the amount of information in the forecast, changes after the AA firms switch auditors compared to the changes that occur in non-AA firms. The coefficients are positive and statistically significant (*t*-statistic of 3.12 in column (2)), consistent with our conjecture that AA firms' forecasts become more reliable after they switch auditors.

The main effect on *SURP* is positive and significant, indicating that on average management forecasts are informative. Our results also suggest that investors react more strongly to more precise and timely forecasts, as reflected by the positive and significant coefficients on *HORIZON* and *PRECISION*. The positive and significant coefficients on *POST*SURP* indicate that, on average, the market reacts more to forecasts issued by non-AA firms in 2003, compared to those issued by non-AA firms in 2001.

[Insert Table 4 Here]

4.2.3 Earnings differential informativeness

Next, we examine whether improved earnings (higher quality earnings) accelerate the disclosure of bad news. Specifically, we estimate equation (3) and report the results in Table 5. If managers disclose more bad news during a quarter, then the concentration of bad news around earnings announcements, relative to non-earnings announcement periods, will decline. If improvements in earnings accelerate the disclosure of bad news, they should result in a reduction

in the concentration of bad news. The results are consistent with this conjecture. The main effect on *BNEWS* is positive and significant, consistent with prior finding that earnings informativeness relative to other sources is higher in bad-news quarters than in good-news quarters (Roychowdhury and Sletten, 2012). The coefficient on *BNEWS*POST* is positive but statistically insignificant, indicating that, for non-AA firms, earnings differential informativeness does not change after the third quarter of 2002. Our main variable of interest is *AA*POST*BNEWS*, which captures how earnings differential informativeness changes for AA firms after they switch auditors, relative to non-AA firms. The coefficients are negative and statistically significant (*t*-statistic= –2.89 in column (2)), indicating lower levels of bad news concentration around earnings announcement relative to non-earnings announcement periods. These results imply that AA firms disclose bad news in a timelier fashion after they switch auditors, which is consistent with the relative increase in disclosure quantity and quality for AA firms, documented in Table 2.

[Insert Table 5 Here]

4.2.4 Capital market consequence

Finally, we test the capital market consequence of the increase in audit/earnings quality for AA firms. The results above suggest that, consistent with the confirmation hypothesis, managers make more frequent, precise, specific, and timely disclosures and investors respond more strongly to these disclosures since they become more reliable. In addition, the exogenous changes in audit/earnings quality also accelerate the release of bad news. Thus, if these improved disclosures are more informative and reduce investors' uncertainty and if information uncertainty is priced (Easley and O'Hara 2014), then we would expect a reduction in the cost of capital. Specifically, we examine equation (4) and report the results in Table 6. The results are generally consistent with

this conjecture. The coefficients on *AA*POST* are negative and statistically significant, indicating that, compared with non-AA firms, AA firms experienced a reduction in their cost of capital after they switch auditors. These results are also economically significant. This suggests that AA firms experience a 1% reduction in their cost of capital compared with non-AA firms. However, for both AA firms and non-AA firms, cost of capital increased, probably because investors required a higher risk premium following the Enron scandal.

[Insert Table 6 Here]

5. Additional Tests

5.1 The Role of Auditor Switching Costs.

Our tests assume the market is aware of the reduced financial-reporting-quality level of Arthur Andersen clients. This raises the question why would firms not switch auditors voluntarily to improve their financial-reporting quality? In equilibrium, firms choose their audit quality based on the various costs and benefits. Therefore firms may choose to have lower audit quality because the costs of switching auditors outweigh the potential benefits. Hence, once firms are forced to switch auditors and improve their financial-reporting quality, we can detect the potential benefits documented above.

To shed light on this tradeoff, we examine how the effects we document relate to the net costs of switching auditors. We predict the effects we document to be stronger for firms with high net switching costs. To measure net switching costs, we use firm age. Older firms tend to have a longer relationship with their auditor and a more complex business model and tend to be less

financially constrained (have easier access to capital).[14] Therefore they are more likely to choose lower audit quality.

To test our prediction, we re-estimate models (1) and (2) for two subsamples, mature and young firms. Mature firms are defined as those older than the median firm (age) in our sample. Young firms are defined as those younger than the median firm (age) in our sample. The results from this analysis are presented in Table 7.

The result related to model (1) are presented in Panel A. For brevity, we focus on the number of forecasts issued and their precision in this analysis.[15] We find that our results are concentrated among the mature firms that have higher switching costs. For example, the coefficient for AA*Post, related to the number of forecasts, is positive and significant at the 1% 1evel for the mature firms. The magnitude of the coefficient is double that of the coefficient reported in Table 3. Moreover, the coefficient for the young firms is insignificantly different from zero. The difference between the coefficients is also significant.

The results related to model (2) are presented in Panel B. Once again the results are concentrated among the mature firms. The coefficient for the mature firms is 75% larger than the coefficient for the young firms. Moreover, the result is only significant for the mature firms. However, in this analysis the difference between the coefficients is not significant.

[Insert Table 7 Here]

5.2 Auditor Tenure

As an additional robustness test, we examine whether firms that had a longer-term relationship with Arthur Anderson also changed their disclosure after they switched auditors.

[14] In untabulated analysis, we find that the correlation between firm age and audit tenure among AA firms is 0.67.
[15] Given the smaller subsamples, we focus on the measures that are not conditional on issuing a forecast, such as specificity and horizon. We draw similar inferences when examining the variable forecaster.

Firms with longer auditor tenures likely employed Arthur Andersen based on decisions made in the more distant past, which are less likely to be correlated with their disclosures prior to switching auditors. Specifically, we re-estimate model (1) using AA firms with an auditor tenure above the sample median and the original sample of control firms.

In untabulated analysis, we draw similar inferences to those based on the results in Table 3. The coefficients for *Forecaster, Number of forecasts,* and *Precision* are positive and statistically significant at conventional levels. The coefficients are slightly larger in this analysis. The coefficients for *Specificity* and *Horizon* are positive but not statistically significant. Taken together, the results help alleviate concerns related to the effect of the firm's choice of Arthur Anderson as an auditor on our inferences.

5.3. Litigation Risk

One potential concern related to the interpretation of our results is that AA firms experience changes in litigation risk following their change in auditor, which in turn affect their disclosure choices. To rule out this alternative explanation, we estimate model (1) using litigation risk as an alternative dependent variable. To estimate firms' litigation risk, we employ the measure developed by Kim and Skinner (2012).

The results from this analysis are presented in Table 8. The coefficients for the interaction term *AA*POST* are insignificant in both specifications, indicating that AA firms do not experience a relative change in litigation risk. This result helps alleviate concerns related to an alternative litigation risk channel.

[Insert Table 8 Here]

5.4. Pseudo Event Dates

An additional potential concern is that the disclosure policy of Arthur Anderson's clients evolved differently prior to their switch in auditor. This would violate the parallel trend assumption of our difference-in-differences design and make it hard to attribute changes in the disclosures of AA firms to the improvement in audit quality they experience following their change in auditor. To address this concern, we re-estimate model (1) using pseudo-event dates prior to the switching dates of the AA firms.

We first adjust the event date to 1999 and compare the change in disclosure of AA firms between 1998 and 2000, relative to the change in disclosure of the control firms for the same period. In contrast to the results presented in Table 3, we find, in untabulated analysis, that all the coefficients except for *Specificity* are negative and insignificant. The coefficient for *Specificity* is positive and significant (coefficient of 0.001 with a *t*-statistic of 3.29). Given this result, we conduct a second test comparing the change in disclosure between 1997 and 1999. In this analysis, the coefficients for *Forecaster*, *Number of forecasters* and *Horizon* remain negative. The coefficient is significant for *Forecaster*. The coefficient for *Precision* is positive and insignificant. Surprisingly, the coefficient for *Specificity* remains positive and significant (coefficient of 0.002 with a *t*-statistic of 3.87).

Taken together, our results alleviate concerns related to a violation of the parallel trend assumption driving our results. However, our results related to *Specificity* are harder to attribute the change in audit quality experience by AA firms after they switch auditors.

6. Conclusion

This paper exploits an exogenous shock to audit quality—the auditor switch forced by Arthur Andersen's collapse—to test the confirmation hypothesis. Under this hypothesis, more reliable mandatory statements facilitate voluntary disclosure by imposing accountability and discipline on managers. Consistent with the confirmatory role of accounting, we find that audit clients "shocked" with improved audit quality increased disclosure by providing more management forecasts and enhancing their precision and specificity. We also find less return concentration around earnings announcements in bad-news quarters, indicating that forced improvements in audit quality lead to timelier release of bad news.

Our results reinforce the idea that the benefits of mandatory financial reporting cannot be judged solely by the informativeness of the mandatory signal. Forcing firms to issue periodic reports can alter their incentives to disclose information voluntarily via other channels. Such voluntary disclosures can substitute for the information contained in mandatory reports. Therefore, while reaction to mandatory reports might account for a small fraction of the overall variation in returns for a firm in a given year (Ball and Shivakumar, 2008), this fraction can understate the effect of mandatory reporting on the firm's information environment.

However, our results should not be taken to suggest that increasing financial reporting or auditing requirements provides net benefits to shareholders. Forcing firms to increase audit quality might improve the timeliness of disclosure, but many shareholders might not benefit from more timely reporting—particularly when less costly governance mechanisms are available. A related, unresolved question is whether the combined changes in enforcement, accounting rules, and audit practices that accompany a change in reporting regulation can be replicated by the actions of individual firms.

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Appendix: Variable Definitions

Variable Definitions		
Variable	***Variable Name***	***Description***
Treatment Variables		
Arthur Andersen clients	AA	• An indicator variable that equals 1 if the firm's auditor was Arthur Andersen in 2001 or 2002 and 0 otherwise (Dyck et al. 2014), as identified in the AuditAnalytics database.
Post treatment	Post	• An indicator variable that equals 1 in the year following the year of the auditor switch. For firms that did not experience auditor changes, *Post*=1 in 2003 and *Post* =0 in 2001. • In Table 5 (the capital market consequence test), Post equals 1 if the month is within the 24 months following the auditor change.
Variables of Interest		
Management forecast activity	Forecaster	• An indicator variable that equals 1 if the firm issues at least one forecast in three out of four quarters in a given year and zero otherwise (Rogers et al. 2008). • Management forecast activity is obtained from First Call's Company Issued Guidelines (CIG) database.
Number of forecasts	Number of forecasts	• Natural log of (1+ the number of forecasts issued in a given year).
Management forecast precision	Precision	• The precision of a forecast equals 4 for point estimates, 3 for range estimates, 2 for open-ended estimates, 1 for qualitative estimates, and 0 for no forecasts (Armstrong et al. 2012). • The variable precision equals the average precision of the forecasts issued over the year.
Management forecast specificity	Specificity	• We measure specificity for all point and range forecasts issued in a given year. • For range forecasts, the specificity of the forecast is defined as the difference between the top and bottom of the range, divided by the stock price of the firm in the month prior to the forecast date. For point forecasts, specificity equals zero. • The variable specificity equals the average specificity of the forecasts issued in a given year multiplied by –1. The variable is multiplied by –1 so that higher values represent more specific forecasts.
Management forecast horizon	Horizon	• Natural log of (1+average horizon of forecasts in a given year) • The horizon of a forecast is calculated as the difference in days between the fiscal period end-date and the forecast date (Ball et al. 2012).
Market reaction to forecasts	MEF_CAR	• Three-day market-adjusted return around the management forecast announcement (Ball et al. 2012). Data is obtained from CRSP and Compustat.

Differential earnings informativeness	Ln(News_Ratio)	• Natural log of *News_Ratio*. • *News_Ratio* is the ratio of the absolute value of the market-adjusted returns in the three days around the earnings announcement, relative to the absolute value of the non-earnings-announcement period returns. Data is obtained from CRSP and Compustat.
Cost of capital		• Monthly realized stock return, as reported by CRSP.
Ex-ante litigation risk	Litigation Risk	• The ex-ante probability of being sued for violating rule 10b-5 (Kim and Skinner 2012). • The measure is computed using fitted values based on the coefficient estimates presented in Table 7 of Kim and Skinner (2012). • We compute the measure on an annual basis.
Control Variables		
Information in managers' forecast	SURP	• Difference between the manager's forecast and the most recent analyst consensus forecast, scaled by the magnitude of the management forecast. • We only use point and the midpoints of range forecasts in this test.
Firm Size	Size	• Natural log of the market cap of the firm measured at the end of the fiscal year (the month of the report date), as reported by CRSP.
Financial Leverage	Leverage	• Ratio of (debt in current liabilities + long term debt)/(total assets), as reported by Compustat.
Market to Book Ratio	MTB	• The market value of equity/the book value of equity. The market value of equity is measured at the end of the fiscal year (the month of the report date), as reported by CRSP. The book value of equity is obtained from Compustat.
Inclusion in the S&P Index	S&P Index	• An indicator variable equal to one if the firm is included in the S&P index in a given year and zero otherwise. • The data are obtained from the Compustat index constituents file.
Number of Analysts Following the Firm	Analysts Following	• Natural log of (1 + the number of annual earnings estimates (for the next fiscal period) present in the IBES summary file. • The most recent record in IBES prior to the data date in Compustat is employed. • If no data are present on IBES, the variable is set to zero.
Percent of Shares Held by Institutional Investors	Institutional Ownership	• The (%) of outstanding shares held by institutions based on quarter-end 13F filings, as of the end of the fiscal year (the report date on Compustat). • The variable is constructed by WRDS in its s34 database. • In cases where the (%) reported exceeds 100%, we redefine the variable to equal 100%.
Prior Returns	Return	• Total returns over the year, as reported by CRSP in the monthly file.
Return on Assets	ROA	• Annual earnings before extraordinary items scaled by total assets.

		• Data are obtained from Compustat.
Share Price Volatility	Return Volatility	• The standard deviation of monthly returns measured over the fiscal year. • Data are obtained from CRSP. At least 10 months of data are required to compute the variable. • In Table 3, we follow Ball et al. (2012) and use the standard deviation of daily returns measured over the fiscal year.
Volatility of Annual Earnings	Earnings Volatility	• The standard deviation of annual operating income after deprecation scaled by total assets, measured over five years, using a minimum of three years. • Data are obtained from Compustat.
Firm Age	Age	• The number of years since the firm's first observation in CRSP.
The number of business segments	# Business Seg	• The natural log of the number of business segments the firm operates in, for a given year. • The data are obtained from the segment file in Compustat. We delete observations with missing data.
The number of geographic segments	# Geographic Seg	• The natural log of the number of geographic segments the firm operates in, for a given year. • Data are obtained from the segment file in Compustat. We delete observations with missing data.
High litigation Industry	High Litigation Industry	• An indicator if the firm is in one of the industries with high litigation risk, i.e., in SIC codes 2833–2836, 3570–3577, 3600–3674, 5200–5961, 7370–7374, and 8731–8734)
News in management forecast	SURP	• The difference between managements' estimates and the most recent analyst consensus forecast, scaled by the magnitude of the management forecast. • The variable is computed for point and range forecasts.
Bad news	BNEWS	• An indicator variable that equals 1 when the quarterly overall market-adjusted return during the quarter is negative and 0 otherwise.
Number of tradings days	Trade_Days	• The number of trading days in the quarter.
Prior monthly return	Lag(Ret)	• Stock return during 1 month prior to auditor changes, as reported by CRSP.
Momentum	MOM	• Compounded stock return over month *t–12* through *t–2*. Data are obtained from CRSP
Fama-French Factor Loadings	Betamkt, Betasmb, Betahml, Betacma, Betarmw	• Factor loadings calculated by regressing firm-level return on Fama-French (2015) five factors using monthly data from the end of month *t–50* to the end of month *t.* • The factor loadings are estimated simultaneously. • Data are obtained from CRSP and Ken French's website.

Table 1 Summary Statistics

This table reports summary statistics for all the variables used in our analysis. We report summary statistics for the different samples employed in Tables 2–8. All the variables are described in the appendix. All firm-level variables are winsorized at the 1% and 99% levels. Panel A, B, C, and D report the variables we use in the management forecasting analysis (Table 2, 3, and 7), market reaction to management forecasts analysis (Table 4), bad news concentration test (Table 5), cost of capital test (Table 6), and litigation risk analysis (Table 8), respectively.

Panel A. Management Forecasts Sample

Variable	N	Mean	S.D.	1st Quartile	Median	3rd Quartile
Forecaster	4247	0.16	0.37	0.00	0.00	0.00
Number of forecasts	4247	0.54	0.79	0.00	0.00	1.10
Precision	4247	1.09	1.47	0.00	0.00	3.00
Specificity	1417	-0.00370	0.00669	-0.00412	-0.00197	-0.00078
Horizon	1577	4.66	0.88	4.25	4.89	5.25
Size	4247	5.52	1.86	4.20	5.59	6.77
MTB	4247	2.83	3.34	1.05	1.81	3.28
Leverage	4247	0.21	0.21	0.01	0.16	0.36
S&P Index	4247	0.07	0.25	0.00	0.00	0.00
Return Volatility	4247	0.16	0.11	0.09	0.14	0.21
Earnings Volatility	4247	0.09	0.12	0.02	0.05	0.10
Analyst Following	4247	1.19	0.98	0.00	1.10	1.95
Age	4247	13.27	12.60	5.00	9.00	18.00
ROA	4247	-0.02	0.19	-0.03	0.03	0.07
Returns	4247	0.46	0.97	-0.08	0.25	0.69
# Business Segments	4247	1.57	1.03	1.00	1.00	2.00
#Geographical Segments	4247	2.13	1.61	1.00	1.00	3.00
Institutional Ownership	4247	0.42	0.29	0.16	0.39	0.65
AA	4247	0.17	0.37	0.00	0.00	0.00
POST	4247	0.51	0.50	0.00	1.00	1.00

Panel B. Market Reaction Sample

Variable	N	Mean	S.D.	1st Quartile	Median	3rd Quartile
MEF_Ret	5714	-0.02	0.12	-0.06	-0.01	0.04
SURP	5714	0.02	0.89	-0.04	0.02	0.48
Analyst Following	5714	2.10	0.69	1.61	2.20	2.64
Horizon	5714	4.28	1.21	3.30	4.52	5.28
Precision	5714	3.16	0.36	3.00	3.00	3.00
Size	5714	7.20	1.70	6.02	7.07	8.34
MTB	5714	3.01	3.39	1.40	2.12	3.55
Leverage	5714	0.24	0.18	0.07	0.23	0.36
High Litigation Industry	5714	0.33	0.47	0.00	0.00	1.00
Return Volatility	5714	0.03	0.02	0.01	0.02	0.03
AA	5714	0.16	0.37	0.00	0.00	0.00
POST	5714	0.50	0.50	0.00	1.00	1.00

Panel C. Bad News Concentration Sample

Variable	N	Mean	S.D.	1st Quartile	Median	3rd Quartile
Ln(News_Ratio)	65006	3.38	1.59	2.41	3.39	4.35
BNEWS	65006	0.48	0.50	0.00	0.00	1.00
Size	65006	5.37	2.06	3.82	5.22	6.75
BTM	65006	0.75	0.73	0.29	0.57	0.98
Leverage	65006	0.22	0.21	0.02	0.17	0.34
Trade_Days	65006	61.84	10.17	59.00	63.00	66.00
Analyst_Follow	65006	1.05	0.98	0.00	1.10	1.79
AA	65006	0.12	0.32	0.00	0.00	0.00
POST	65006	0.32	0.46	0.00	0.00	1.00

Panel D. Cost of Capital Sample

Variable	N	Mean	S.D.	1st Quartile	Median	3rd Quartile
Ret	187269	0.01	0.15	-0.07	0.01	0.08
Size	187269	5.75	1.89	4.39	5.69	6.99
BTM	187269	0.75	0.66	0.31	0.57	0.96
AA*BM	187269	0.09	0.28	0.00	0.00	0.00
Lag(Ret)	187269	0.02	0.16	-0.06	0.01	0.09
MOM	187269	0.22	0.71	-0.20	0.10	0.42
Leverage	187269	0.22	0.20	0.02	0.18	0.36
Betamkt	187269	0.99	0.99	0.41	0.92	1.51
Betasmb	187269	0.65	0.86	0.16	0.57	1.08
Betahml	187269	0.31	1.49	-0.40	0.42	1.13
Betacma	187269	-0.24	1.84	-1.02	-0.16	0.64
Betarmw	187269	-0.17	1.42	-0.74	0.02	0.64
AA	187265	0.12	0.33	0.00	0.00	0.00
POST	187265	0.63	0.48	0.00	1.00	1.00

Panel E. Litigation Risk Sample

Variable	N	Mean	S.D.	1st Quartile	Median	3rd Quartile
Litigation Probability	3423	0.42	0.34	0.10	0.31	0.74
Forecaster	3423	0.17	0.38	0.00	0.00	0.00
Number of forecasts	3423	0.56	0.80	0.00	0.00	1.10
Precision	3423	1.12	1.48	0.00	0.00	3.00
Specificity	1173	-0.00370	0.00679	-0.00411	-0.00199	-0.00079
Horizon	1304	4.66	0.86	4.25	4.89	5.25
Size	3423	5.56	1.86	4.23	5.62	6.80
MTB	3423	2.78	3.13	1.08	1.80	3.24
Leverage	3423	0.21	0.21	0.01	0.17	0.36
S&P Index	3423	0.07	0.26	0.00	0.00	0.00
Return Volatility	3423	0.15	0.09	0.08	0.13	0.20
Earnings Volatility	3423	0.07	0.09	0.02	0.04	0.09
Analyst Following	3423	1.19	1.00	0.00	1.10	1.95
Age	3423	14.69	12.67	6.00	10.00	19.00
ROA	3423	0.00	0.15	-0.02	0.03	0.07
Return	3423	0.48	0.97	-0.04	0.26	0.68
# Business Segments	3423	1.58	1.01	1.00	1.00	2.00
#Geographical Segments	3423	2.07	1.56	1.00	1.00	3.00
Institutional Ownership	3423	0.44	0.29	0.18	0.41	0.67
AA	3423	0.18	0.38	0.00	0.00	0.00
POST	3423	0.53	0.50	0.00	1.00	1.00

Table 2 Auditor Change and Forecasting Activity – Univariate Evidence

This table reports the average values of *Forecaster* (Panel A) and *Number of forecasts* (Panel B) for AA clients and non-AA clients in the pre- and post-periods, respectively. We report *p-values* based on the nonparametric two-tailed Wilcoxon test static in parentheses.

Panel A Forecaster

	Pre	Post	Post-Pre
AA	10.51%	20.00%	9.49%
			(0.00)
non-AA	13.12%	19.56%	6.45%
			(0.00)
			3.04%
			(0.36)

Panel B Number of Forecasts

	Pre	Post	Post-Pre
AA	1.14	1.70	0.56
			(0.09)
non-AA	1.33	1.76	0.44
			(0.09)
			0.12
			(0.07)

Table 3 Auditor Change and Management Forecasts

This table reports results from the estimation of equation (1). The dependent variable is the forecasting characteristic examined. All the variables are defined in the appendix. All the specifications are estimated using OLS regressions and include industry fixed effects, defined at the two-digit SIC level. *t*-statistics, based on robust standard errors clustered by firm and year, are presented below the coefficient estimates.

	1	2	3	4	5
VARIABLES	Forecaster	Number of forecasts	Precision	Specificity	Horizon
AA*POST	**0.038*** **	**0.070*** **	**0.154*** **	**0.001*** **	**0.146*** **
	[2.77]	**[3.62]**	**[6.33]**	**[2.67]**	**[4.51]**
AA	-0.046**	-0.102***	-0.139***	-0.001***	-0.034
	[-2.51]	[-3.51]	[-3.61]	[-4.99]	[-1.20]
POST	0.050***	0.054***	-0.012	-0.002***	0.258
	[12.16]	[4.03]	[-0.36]	[-4.08]	[0.00]
Size	0.018***	0.065***	0.129***	0.002***	0.038***
	[7.89]	[12.06]	[11.29]	[26.51]	[3.19]
MTB	-0.000	-0.001	-0.003	-0.000	0.007
	[-0.18]	[-0.36]	[-0.53]	[-0.21]	[1.60]
Leverage	0.052***	0.107***	0.129	-0.004***	0.315**
	[3.44]	[2.73]	[1.47]	[-8.28]	[2.38]
S&P Index	0.052**	0.017	-0.207	-0.004***	0.038
	[1.96]	[0.27]	[-1.51]	[-29.61]	[0.33]
Return Volatility	-0.035	0.202***	0.641***	-0.004	0.125
	[-0.82]	[5.94]	[7.12]	[-0.75]	[0.80]
Earnings Volatility	-0.000	0.019	-0.136***	0.000	0.011
	[-0.03]	[0.72]	[-2.58]	[0.24]	[0.07]
Analyst Following	0.082***	0.241***	0.438***	0.001**	-0.032
	[14.75]	[14.30]	[10.26]	[2.13]	[-0.62]
Age	0.000	-0.001	-0.002	0.000	-0.000
	[0.24]	[-0.37]	[-0.58]	[0.80]	[-0.04]
ROA	0.113	0.346*	0.699***	0.005***	0.182*
	[1.03]	[1.86]	[3.68]	[3.13]	[1.95]
Returns	-0.002	-0.003	0.004	0.000	0.014
	[-0.98]	[-0.60]	[0.30]	[0.45]	[0.63]
#Business Segments	0.004	0.015**	0.034**	-0.000**	0.015
	[0.74]	[2.36]	[2.48]	[-2.10]	[0.67]
#Geographical Segments	-0.010	-0.033**	-0.039*	-0.000	-0.043***
	[-1.52]	[-2.09]	[-1.93]	[-0.62]	[-4.58]
Institutional Ownership	0.125***	0.250***	0.402***	0.001***	-0.039
	[10.17]	[5.41]	[6.13]	[2.88]	[-0.97]
Constant	-0.036	-0.108	-0.273	-0.013***	4.421***
	[-0.57]	[-0.64]	[-1.24]	[-11.13]	[23.55]
Observations	4,247	4,247	4,247	1,417	1,577
Fixed Effects	Industry	Industry	Industry	Industry	Industry
Cluster	Firm & Year	Firm & Year	Firm & Year	Firm & Year	Firm & Year
Adj. R-squared	0.206	0.319	0.278	0.207	0.157

*** p<0.01, ** p<0.05, * p<0.1

Table 4 Auditor Change and the Market Reaction to Management Forecasts

This table reports results from the estimation of equation (2). The dependent variable is the three-day market-adjusted return around the management forecast announcement. All the variables are defined in the appendix. Column (1) reports the results without the control variables, and column (2) reports the results including the control variables. All the specifications are estimated using OLS regressions and include quarter and industry fixed-effects. Industry fixed-effects are defined at the two-digit SIC level. *t*-statistics, based on robust standard errors clustered at the quarter and industry level, are presented below the coefficient estimates.

	1	2
VARIABLES	MEF_CAR	MEF_CAR
AA*POST*SURP	**0.042*** **	**0.040*** **
	[3.13]	**[3.12]**
AA	-0.006	-0.006
	[-0.68]	[-0.71]
POST	0.006	0.001
	[0.81]	[0.06]
AA*POST	0.002	0.003
	[0.23]	[0.30]
AA*SURP	0.003	0.003
	[0.34]	[0.46]
POST*SURP	-0.002	-0.003
	[-0.55]	[-1.12]
SURP	0.020***	0.018***
	[3.91]	[4.07]
Analyst Following		-0.026***
		[-7.71]
Horizon		0.010***
		[4.25]
Precision		0.010***
		[2.86]
Size		0.015***
		[10.48]
MTB		0.001***
		[2.82]
Leverage		0.020
		[1.59]
High Litigation Industry		-0.012
		[-1.27]
Return Volatility		0.324
		[1.34]
Constant	-0.058***	-0.173***
	[-4.65]	[-5.33]
Observations	5,714	5,714
Fixed Effects	Quarter & Industry	Quarter & Industry
Cluster	Quarter & Industry	Quarter & Industry
Adj. R-squared	0.080	0.110

*** p<0.01, ** p<0.05, * p<0.1

Table 5 Auditor Change and the Concentration of Bad News

This table reports results from the estimation of equation (3). The dependent variable is the natural logarithm of the ratio of the absolute value of the earnings announcement period returns, divided by the absolute value of the non-earnings-announcement period returns. All the variables are defined in the appendix. Column (1) reports the results without the control variables, and column (2) reports the results including the control variables. All the specifications are estimated using OLS regressions and include quarter and industry fixed-effects. Industry fixed-effects are defined at the two-digit SIC level. *t*-statistics, based on robust standard errors clustered at the quarter and industry level, are presented below the coefficient estimates.

	1	2
VARIABLES	Ln(News_Ratio)	Ln(News_Ratio)
AA*POST*BNEWS	**-0.157*** **	**-0.157*** **
	[-2.70]	**[-2.89]**
BNEWS	0.203***	0.210***
	[3.70]	[4.33]
AA	0.061***	0.050***
	[3.60]	[4.14]
POST	0.127*	0.148**
	[1.74]	[2.36]
AA*POST	0.050	0.055
	[0.91]	[1.02]
AA*BNEWS	0.017	0.016
	[0.68]	[0.67]
BNEWS*POST	0.073	0.066
	[1.02]	[0.96]
Size		0.012
		[0.85]
BTM		0.029**
		[2.00]
Leverage		-0.047
		[-1.10]
Trade_days		-0.007***
		[-3.13]
Analyst_Follow		0.068***
		[4.88]
Constant	3.021	3.396
	[0.00]	[0.00]
Observations	65,006	65,006
Fixed Effects	Quarter & Industry	Quarter & Industry
Cluster	Quarter & Industry	Quarter & Industry
Adj. R-squared	0.019	0.022

*** p<0.01, ** p<0.05, * p<0.1

Table 6 Auditor Change and the Cost of Capital

This table reports results from the estimation of equation (4). The dependent variable is the realized monthly stock return. All the variables are defined in the appendix. Column (1) reports the results with *t*-statistics clustered at firm level, and column (2) reports the results with *t*-statistics clustered at firm and month level.

	1	2
VARIABLES	Realized Return	Realized Return
AA*POST	**-0.010*** **	**-0.010***
	[-4.91]	**[-1.73]**
AA	0.009***	0.009
	[3.79]	[1.42]
POST	0.021***	0.021
	[26.70]	[1.19]
Size	-0.002***	-0.002***
	[-12.86]	[-3.42]
BTM	0.007***	0.007**
	[9.60]	[2.54]
AA*BTM	0.001	0.001
	[0.24]	[0.16]
Lag(Ret)	0.003	0.003
	[1.28]	[0.10]
Leverage	-0.002	-0.002
	[-1.03]	[-0.39]
MOM	0.003***	0.003
	[5.92]	[0.52]
Betamkt	-0.001*	-0.001
	[-1.81]	[-0.28]
Betasmb	-0.002***	-0.002
	[-3.82]	[-1.40]
Betahml	0.005***	0.005**
	[13.43]	[2.13]
Betacma	0.003***	0.003*
	[7.93]	[1.75]
Betarmw	0.005***	0.005*
	[13.45]	[1.79]
Constant	0.008***	0.008
	[4.89]	[0.56]
Observations	187,269	187,269
Cluster	Firm	Month & Firm
Adj. R-squared	0.010	0.010

*** p<0.01, ** p<0.05, * p<0.1

Table 7 Cross-Sectional Variation in the Effect of Auditor Change

This table reports the results from the estimation of equation (1) and equation (2) across two subsamples. We partition our sample based on firm age. Mature firms are defined as firms that are older than the median firm (age) in our sample. Young firms are defined as firms that are younger than the median firm (age) in our sample. All the variables are defined in the appendix. In Panel A, the dependent variables are the number of forecasts issued and the forecast precision. In Panel B, the dependent variable is the three-day market-adjusted return around the management forecast announcement. All the specifications are estimated with the control variables employed in Tables 2 and 3, respectively, and include industry fixed-effects defined at the two-digit SIC level. *t*-statistics, based on robust standard errors clustered at the firm level, are presented below the coefficient estimates.

Panel A Management Forecast Characteristics

	Number of forecasts		Precision	
VARIABLES	Mature	Young	Mature	Young
AA*POST	**0.142*** **	**-0.036**	**0.280** **	**-0.027**
	[2.64]	**[-0.46]**	**[2.42]**	**[-0.18]**
AA	-0.167***	-0.020	-0.207**	-0.078
	[-3.76]	[-0.31]	[-2.26]	[-0.63]
POST	0.030	0.083**	-0.033	-0.009
	[1.20]	[2.26]	[-0.67]	[-0.12]
p-value for difference in AA*POST coefficients	0.05		0.09	
Control Variables	Yes	Yes	Yes	Yes
Observations	2,461	1,782	2,461	1,782
Fixed Effects	Industry	Industry	Industry	Industry
Cluster	Firm	Firm	Firm	Firm
Adj. R-squared	0.367	0.281	0.326	0.241

*** p<0.01, ** p<0.05, * p<0.1

Panel B Market Reaction to Management Forecasts

	MEF_CAR	
VARIABLES	Mature	Young
AA*POST*SURP	**2.841****	**1.629**
	[2.23]	**[1.33]**
SURP	0.342***	0.668***
	[2.61]	[4.24]
AA*SURP	0.381	0.703
	[0.96]	[1.16]
p-value for difference in AA*POST*SURP coefficients	0.44	
Control Variables	Yes	Yes
Observations	3,979	1,835
Fixed Effects	Industry	Industry
Cluster	Firm	Firm
Adj. R-squared	0.071	0.104

*** p<0.01, ** p<0.05, * p<0.1

Table 8 The Role of Litigation Risk

This table examines the potential change in litigation risk following the switch in auditor by former AA clients. The dependent variable is firm-level litigation risk. All the variables are defined in the appendix. Column (1) reports the results without the control variables, and column (2) reports the results including the control variables. All the specifications are estimated using OLS regressions and include industry fixed-effects defined at the two-digit SIC level. *t*-statistics, based on robust standard errors clustered at the firm and year level, are presented below the coefficient estimates.

	1	2
VARIABLES	Litigation Risk	Litigation Risk
AA*POST	**-0.023**	**-0.014**
	[-0.88]	**[-0.56]**
AA	0.011	-0.001
	[0.46]	[-0.08]
POST	-0.091***	-0.040***
	[-12.06]	[-2.76]
SIZE		0.028***
		[3.62]
MTB		-0.004
		[-1.25]
Leverage		0.055
		[0.74]
S&P Index		0.032
		[0.00]
Return Volatility		1.361***
		[37.60]
Earnings Volatility		0.462***
		[8.24]
Analyst Following		0.067***
		[7.42]
Age		-0.003***
		[-6.25]
ROA		-0.054*
		[-1.91]
Returns		-0.016
		[-0.70]
#Business Segments		0.002
		[0.72]
#Geographical Segments		0.012**
		[2.52]
Institutional Ownership		-0.022
		[-0.72]
Constant	0.100***	-0.213***
	[128.89]	[-17.81]
Observations	3,423	3,423
Fixed Effects	Industry	Industry
Cluster	Year & Firm	Year & Firm
Adj. R-squared	0.239	0.449

*** p<0.01, ** p<0.05, * p<0.1