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Perceived Auditor Quality and the Earnings Response Coefficient

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SYNOPSIS AND INTRODUCTION: An auditor's reputation lends credibility to the earnings report that he audits. An unresolved issue is whether auditor size is correlated with auditor quality, where a high-quality auditor is defined as one who brings about more credible earnings reports. According to basic intuition and a modified Holthausen-Verrecchia (1988) model, investors' response to an earnings surprise will depend on the perceived credibility of the earnings report. In this study, we examine whether the earnings response coefficient (*ERC*) differs between Big Eight (B8) and non-Big Eight (NB8) audited firms. This provides a test of the joint hypotheses that auditor size is a proxy for auditor credibility and of the modified H-V model.

Consistent with the joint hypotheses, we find that the *ERC*s of Big Eight clients are statistically significantly higher than for non-Big Eight clients. The result obtains in both a matched sample of firms paired according to industry membership, and a switch sample of firms grouped according to shifts from and to B8 and NB8 auditors. Furthermore, the result is robust with respect to the inclusion of other explanatory factors for *ERC* that have been suggested by previous studies: growth and persistence, risk, firm size, and predisclosure information environment.

Key Words: *Auditor quality, Auditor size, Earnings quality, Earnings response coefficient, Precision of information.*

Data Availability: *Analysts' forecasts may be requested from the Institutional Brokers Estimate System (IBES); all other data are from public sources.*

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THE remainder of the article is organized as follows. In section I, we present the motivation for the study by explaining the hypothesized relation between investor response to earnings surprises and auditor type (B8 and NB8). In section II, we provide a simple analytical formula that relates stock price response to the precision of the earnings signal. The selection criteria for the industry-matched and the switch samples are explained in section III. The test procedures for both samples are described in section IV, which also contains a description of the control variables in the regression equation and the empirical proxies for the earnings surprise and stock price response. Results are reported in section V and section VI contains our conclusions.

I. Motivation

Recent research in the capital markets area has focused on the determinants of the *ERC*, which is the multiple that correlates unexpected earnings with abnormal changes in stock prices in response. The *ERC* is commonly estimated as the slope coefficient in a regression of the abnormal stock returns on a measure of earnings surprise. It is therefore a measure of the extent to which new earnings information is capitalized in the stock price. Current evidence indicates that the *ERC* varies cross-sectionally and intertemporally; for example, see Kormendi and Lipe (1987), Collins and Kothari (1989), Easton and Zmijewski (1989), Biddle and Seow (1991), and Lipe (1990). Collectively, these studies have shown that the *ERC* varies with the degree of persistence in earnings, the predictability of earnings, the security's covariation with the market return, growth opportunities of the firm, and industry membership.

Researchers in this area have begun to examine whether the stock price reaction to earnings surprises is related to the quality of the reported earnings numbers. Imhoff and Lobo (1992) find that firms with low consensus in the analysts' forecasts of earnings tend to have a low *ERC*. Although it is possible that high prior uncertainty about the underlying value of the firm would also increase the dispersion in forecasts, Imhoff and Lobo conclude that it is more likely to proxy for the noise in accounting measures than for any prior uncertainty about underlying cash flows.

Lang and McNichols (1990) use the correlation between cash flow and earnings as a proxy for earnings report quality. To the extent that high-quality auditors assure a greater conformance of the financial report with generally accepted accounting principles (GAAP), less discretion in the management of accruals is permitted. Therefore, one might expect a greater correlation between expected future cash flows and accounting earnings with high-quality auditors.[1]

In a similar spirit, we expect earnings reports to have a greater effect on investors' valuation of the firm when the reported numbers more accurately reflect true economic value. This can be demonstrated theoretically with a simplified version of the Holthausen and Verrecchia (1988) model presented in the next section. The model pre-

[1] This is not, however, a logically necessary consequence. To give an extreme example, an auditor who incorrectly required the firm to record zero accruals always would cause a perfect correlation between cash flows and earnings. More generally, for a good auditor, the divergence of earnings from cash flows may capture meaningful economic events, in which case a lower correlation of earnings and cash flows could be associated with more informative earnings. Lang and McNichols (1990) find that the *ERC* is lower for firms with higher correlation between net income and contemporaneous cash from operations, and for firms with higher accruals, contrary to their predictions.

dicts that the magnitude of the stock price response increases with the precision (inverse of the variance of the noise term) of the information.

Since investors cannot directly observe the underlying true earnings of the firm, they have to rely on reported accounting numbers. To safeguard the credibility of these reported figures, external auditors must certify that they conform to GAAP, which assures investors of the reliability of financial data. This reflects the attestation role of auditing (see, e.g., Abdel-khalik and Solomon 1988). A more skillful auditor will presumably be able to bring closer concordance of the reported earnings with GAAP.[2] It seems plausible, therefore, that if the auditor's quality (skill) is perceived by investors as high, they will respond strongly to surprises in reported earnings.

Although a link between informativeness of earnings and the firm's conformance with GAAP is plausible, it is not crucial for the argument. So long as some auditors are perceived to follow policies that cause reported earnings to be more informative about value than other auditors, valuation theory predicts that the *ERC* will be different for different auditors. An auditor's "quality" can then be defined as the characteristic leading to greater informativeness of reported earnings.[3] Whether some auditors do in fact produce earnings reports with greater credibility for investors is an interesting issue. To examine this, we focus on an observable characteristic of the audit firm size, which managers have argued is associated with audit quality.

DeAngelo (1981) suggests that the initial client-specific setup costs confer economic rents to the incumbent auditor in later periods as a consequence of a first-mover advantage. Because a failure to detect any material omissions and misrepresentations in one client's records may cause the auditor to lose these rents from some or all clients, a larger audit firm has a greater commitment to accuracy. Dopuch and Simunic (1980, 1982) propose that investors may rationally perceive B8 auditors as of higher quality because larger auditors have more of the observable characteristics associated with quality (i.e., specialized training, accreditation by some reputable agency, and peer reviews). In a model on the determinants of optimal size and structure of the auditing firms, John (1991) shows that auditor quality increases with size.

A survey of management by Carpenter and Strawser (1971) concludes that the majority of firms issuing equity stock switch to B8 auditors to secure the best price for the shares. Other empirical studies that support the correlation of size and quality include Palmrose (1988) and Beatty (1989). Using the incidence of litigation as a measure of auditor quality, Palmrose finds that the NB8 auditors as a group experience more litigation than B8 auditors. This result holds despite the possibility of a higher tendency to sue larger auditors who have deeper pockets. Beatty finds that the initial return on an initial public offering is higher for NB8 than for B8 client firms (i.e., there is less underpricing for B8 client firms).

[2] This does not imply that the financial reports audited by low-quality auditors necessarily violate GAAP, but that the likelihood and amount of deviation of the reported numbers from true economic levels are lower for high-quality auditors.

[3] The literature on audit quality recognizes that the audit service has multiple attributes; for example, Simunic and Stein (1987) characterize the service in terms of (1) control (provision of reliable information for internal control purposes), (2) product line (availability of consulting services as well as the traditional audit service), and (3) credibility. In a study of private companies that voluntarily hire external auditors, O'Keefe and Barefield (1985) found that the majority (57 percent) cited enhanced credibility with outside parties as the major reason for the hire. We focus on credibility because of its importance, and because the other characteristics would seem to have relevance for the *ERC* primarily via their effects on the credibility of the financial statements.

Conversely, some studies have not found an association between size of the audit firm and measures of quality. Imhoff (1988) surveyed financial analysts and found that they perceive no differences in quality between B8 and NB8 clients. Event studies of auditor switches provide evidence that is hard to assess. Nichols and Smith (1983) find that the stock price reaction to an announcement of a change from a NB8 to a B8 auditor is larger than for a change from B8 to a NB8 auditor, but the difference is not statistically significant. They hypothesize that a higher-quality auditor will be more credible, and hence a switch to a B8 auditor should be good news. A theoretical justification for the link between firm value and auditor quality is provided by Titman and Trueman (1986) and Datar et al. (1991). However, these models analyze first-time selection of auditors for a firm that is going public, and therefore their prediction relating auditor quality to value does not necessarily apply to auditor switches. For example, Teoh (1992a) shows that auditor switches can convey either favorable or adverse information about firm value to investors even if all auditors are identical. Hence, a more complex model that considers the issues associated with switching *per se* as well as the quality of the chosen auditor is needed before one can predict whether cross-quality switches are good or bad news.[4]

This research differs from previous studies in that it examines the link between auditor size and a crucial ultimate output of the auditor, the credibility of the financial report. Important consequences related to auditor size may pertain to litigation and information revelations, but we believe it is also important to examine whether large auditors are more credible on the level of financial reporting. The empirical test considers the relation betweeen auditor size and the *ERC*. It is therefore a joint test of two hypotheses: that earnings report quality is an important determinant of the *ERC* and that auditor credibility increases with auditor size. If one maintains that auditor size and quality of the financial report are related, then the evidence provided here concerns whether the quality of reported earnings is an important determinant of the *ERC*. If one maintains that the *ERC* increases with earnings quality, then the evidence indicates whether auditor credibility increases with auditor size.

II. Precision of the Earnings Signal and the *ERC*

In this section, we develop a simple analytical formula that relates the stock price response to the precision of the earnings signal. We adapt a single-period with a single information signal model from Holthausen and Verrecchia (1988). Our aim is to provide the simplest setting sufficient for a test of the relation between the market response to an information signal and the precision of the signal. Some strong simplifying assumptions will be made, but we expect the basic results to hold in more general settings with multiple information signals. Therefore, we abstract from some pertinent factors that would be incorporated in a more general model, such as the firm's riskiness, the degree of earnings persistence and predictability, and other time series characteristics.[5]

[4] Using a larger sample of switches, Teoh (1992b) finds that the stock price reaction to cross-quality switches depends on the pre-switch audit opinion.

[5] For a more general analysis that incorporates these issues in deriving the theoretical measure of the *ERC*, see Kormendi and Lipe (1987) and Lipe (1990).

At the start of the period at date 0, the price of a firm i is P^0. (Since all parameters in the model refer to firm i, we do not use the subscript in this section for simplicity.) At the earnings announcement date 1, the price of the firm is P^1. The stock price response to the announcement is measured by $\delta = P^1 - P^0$. Let the unknown random value of firm i be $\tilde{u}$ and assume it is normally distributed with mean m and variance v.[6] At date 0, the price of firm i is:

$$P^0 = E[\tilde{u}] = m. \tag{1}$$

Let x be the earnings signal that is released at date 1, and let the earnings signal communicate the true value of the firm with noise, $\tilde{\epsilon}$: $\tilde{x} = \tilde{u} + \tilde{\epsilon}$. The random variable $\tilde{\epsilon}$ has normal distribution with mean zero and variance η. Hence, $x - m$ is a measure of the earnings surprise for firm i. At date 1:

$$P^1 = E[\tilde{u} \mid \tilde{x} = x] = m + \frac{v}{v+\eta}(x - m). \tag{2}$$

Therefore, the stock price response is:

$$\delta = P^1 - P^0 = \frac{v}{v+\eta}(x - m), \tag{3}$$

which is a positive linear function of the measure of earnings surprise. The *ERC* is the ratio $v/(v+\eta)$. It is clear that the higher the earnings signal x, the more favorable the investor response; that is, $\partial\delta/\partial x = (v/v+\eta) > 0$. More interestingly, the *ERC* will vary with the amount of prior uncertainty, v, and the noise in the earnings signal η. This can be seen by differentiating the *ERC* with respect to v and η, respectively:

$$\frac{\partial ERC}{\partial v} = \frac{\eta}{(v+\eta)^2} > 0, \tag{4}$$

$$\frac{\partial ERC}{\partial \eta} = -\frac{\eta}{(v+\eta)^2} < 0. \tag{5}$$

Thus, the stock price response increases with v and decreases with η. Note that v and η measure different types of uncertainty; v measures the prior uncertainty of investors about the underlying value u of the firm, whereas η measures the noise in the earnings signal x.

Equation (4) implies that the stock price response is larger for firms with high v, for given η, because the informational value of the earnings signal is greater when there is greater prior uncertainty.[7] In contrast, η measures the noise in the earnings signal, so that high η, for given v, implies a less accurate (credible) or lower quality earnings signal. Therefore, the *ERC* will increase with the quality of the earnings signal when holding constant differences in prior uncertainty about underlying cash flows, as implied by equation (5). We test this prediction by using auditor size as a proxy for earnings quality.

[6] The parameter m can also be viewed as representing the net present value of all future cash flows discounted at the appropriate rate of interest in a multiperiod setting. It would, therefore, incorporate any time series characteristics of the underlying cash flow process.

[7] Note that high v does not imply low predictability in the sense used by Lipe (1990, fn. 9).

III. Sample Selection

In the empirical test, we estimate a cross-sectional multiple regression model of abnormal stock returns on a measure of earnings surprise using ordinary least squares (OLS), and test for a difference in the *ERC* between the B8 and NB8 groups. Other explanatory variables included in the regression equation control for other determinants of the *ERC* that have been identified in previous studies.

The regression is estimated with two alternative samples of firms obtained from the COMPUSTAT industrial tapes, which include firms listed on the New York Stock Exchange (NYSE), the American Stock Exchange (AMEX), and NASDAQ (National Association of Security Dealers Automated Quotations). The first sample contains industry matched pairs of firms to control for differences in the information environment. A NB8 client firm is matched with the B8 client firm closest in size in the same industry. The second sample contains firms that have switched across auditor class groups, so the firm's past history acts as its control in the switch sample. The hypothesis test evaluates the difference in the *ERC* when the firm is audited by a B8 versus NB8 auditor.

We present results from both samples as a robustness check because the alternative designs control for different sets of conditions that may be relevant for the *ERC*. The matched-pair sample has the advantage of holding constant the calendar period and controlling for conditions common among firms in the same industry that might affect the *ERC*. The switch sample uses the same firm as the control, and so controls for firm-specific measurement errors in unexpected earnings and residual stock returns, as long as the distribution of these errors are the same before and after the switch.

The Matched-Pair Sample

Noise in accounting earnings, measurement error in unexpected earnings and variations in the information environment can result in differences in the estimated *ERC*s across firms, apart from the hypothesized factor of difference in auditor credibility. As shown in section II, tests need to control for differences in v, the variance of the underlying true cash flows of the firm. Since firms in the same industry face common demand and supply uncertainties, it is plausible that v will vary more among firms across different industries than within the same industry. Furthermore, if industry members engage in similar transactions and account for them with similar accounting methods, then the noise in accounting measures of true economic earnings could be related to industry membership. These factors suggest the need to match firms by industry membership. Empirical support is provided by Biddle and Seow (1991) and Teets (1992), who showed that *ERC*s vary across industries. We attempt to control for other relevant factors not captured by industry membership by including them as other explanatory variables in the regression equation, which we will describe later.

The sample is selected from the primary, secondary, tertiary (PST), and full coverage (FCI) files of the 1990 COMPUSTAT annual industrial tape, and is limited to firms with earnings information in any year in the period 1980-1989. Firms are also selected from the research file if earnings data are available during the 1981–1988 period for up to three years before deletion. We exclude the three years preceding deletion to prevent contamination of our results by circumstances related to the deletion.[8]

[8] For example, Lang and McNichols (1990) showed that *ERC*s might be affected by financial distress.

The PST and research files contain firms traded on major exchanges, whereas the FCI file contains NASDAQ firms. There were 12,940 firm-year observations from the PST file, 10,465 observations from the FCI file, and 4,579 observations from the research file.[9]

We use the analysts' consensus forecast as a measure of the market's expectation for earnings. Thus, we require that the firm be followed by the Institutional Brokers Estimate System (IBES) and that the most recent consensus forecast be made within 200 trading days of the earnings announcement date. These requirements result in 9,624 PST observations, 5,215 FCI observations, and 2,604 research file observations.

Next, we require that the firms have daily stock returns data available on tape from the Center for Research in Security Prices (CRSP) during the period between the IBES consensus forecast date and the earnings announcement date. This further reduces the sample to 9,264 PST observations, 4,998 FCI observations, and 2,513 research observations. Of this total sample, 15,480 observations are audited by B8 firms and 1,297 are audited by NB8 firms. The greater frequency of B8 clients is not surprising because these auditors tend to audit larger firms, and larger firms are more likely to be followed by analysts and by COMPUSTAT.

Industry matching is accomplished by first eliminating any four-digit SIC code industry if it is not represented by both B8 and NB8 client firms in our sample. Next, each NB8 firm is matched with a B8 firm in the same industry in the same fiscal year. If no B8 firm with the same four-digit SIC code can be found, the three- and then two-digit codes are used to find a match. If there are several B8 candidates, the firm with the closest total asset measure is selected.[10] Finally, we delete observations for which the absolute value of the measure of earnings surprise exceeds 100 percent (see sec. IV for a description of the earnings surprise measure). The final sample consists of 1,263 four-digit firm-year pairs of observations, four three-digit pairs, and 15 two-digit pairs.[11]

A wide spectrum of industries is covered in the matched-pair sample, and a total of 194 four-digit codes are represented. The extractive industries are the most heavily represented (e.g., gold and silver mining is 3.52 percent of the sample; crude oil and natural gas is 3.05 percent). Other major categories are the pharmaceutical (3.36 percent), telephone and telegraph (2.89 percent), and the eating establishment (2.58 percent) industries. Atiase (1985) suggested that the predisclosure information environment may be different for firms listed on different exchanges, but, as shown below, the distribution of firms across exchanges is not different between the auditor class groups. Consequently, we do not include an exchange dummy in the regression equations.

Auditor Class	*NYSE and AMEX firms*	*NASDAQ firms*
B8	778 (60.7 percent)	504 (39.3 percent)
NB8	800 (62.4 percent)	482 (37.6 percent)

[9] We rely on annual rather than quarterly data for our study because only the annual accounting numbers are required to be audited by external auditors for publicly traded firms.

[10] Since this does not fully ensure adequate size matching, size is further included as a control variable in the cross-sectional regression. See section IV for furthur discussion of the effect of size on the ERC.

[11] We also considered an industry-matched sample without the pairwise matching of firm-year observations. This sample has 11,208 observations consisting of 9,928 Big Eight firms and 1,282 non-Big Eight firms. This procedure utilizes more observations, but a disadvantage is that some industries carry relatively more weight between the auditor class groups in the cross-sectional regression equation. The results are qualitatively similar and are not reported.

Table 1
Types of Auditor Switches by Year

	Switches	
Switch Year	Big Eight to Non-Big Eight (B-N)	Non-Big Eight to Big Eight (N-B)
1973	1	3
1974	0	5
1975	1	1
1976	0	1
1977	1	0
1978	1	4
1979	0	3
1980	1	3
1981	3	3
1982	2	5
1983	0	6
1984	1	6
1985	4	8
1986	4	23
1987	1	35
1988	0	8
Total	20	114

Note: Three B-N switches and four N-B switches followed qualified opinions by former auditors.

The Switch Sample

An initial sample of 160 switches taken from Teoh (1992b) contains 59 switches from B8 to NB8 auditors (B-N) and 101 reverse switches (N-B). The switches from 1977–1985 are from *Who Audits America*, and those from 1973–1976 are from Disclosure, Inc. We supplement this sample with switches available from COMPUSTAT for 1985–1988 using the following selection criteria: (1) accounting and stock return data must be available on COMPUSTAT and CRSP tapes for the two years before and two years after the switch, (2) the switching firm must not be a subsidiary of a company, and (3) the firm must have retained the same auditor for two consecutive years preceding and following the switch.[12]

We further require that the firms have analyst forecasts of earnings during the test period. We use the IBES analyst consensus forecasts when these are available (1976 and after) and supplemented forecasts from *Value Line* for 15 switches occurring prior to 1976 (three are B-N and 12 are N-B switches). The final sample contains 134 switches (114 N-B and 20 B-N switches), and their distribution by type over the period 1973–1988 is presented in table 1.

The small number of B-N switches reflects the finding of the Public Accounting Report (February 1984) that B8 auditors have gained clients who trade on major exchanges relative to NB8 auditors. There is a trend towards more switches in more recent years, and there were considerably more N-B switches in 1986 and 1987 than in

[12] Note that the later switches include NASDAQ firms, whereas the initial sample contains only NYSE or AMEX firms. No firm switched auditors and moved to another exchange at the same time.

Table 2
Summary Characteristics of Matched-Pair and Switch Samples

	Mean		Median			
Variable	*Big Eight*	*Non-Big Eight*	*Big Eight*	*Non-Big Eight*	*T-statistic*	*p-value*
Matched-Pair sample:						
CAR1	−0.0229	−0.0182	−0.0087	−0.0086	−0.7761	0.4378
CAR2	−0.0212	−0.0186	−0.0081	−0.0103	−0.3977	0.6909
UE	−0.0248	−0.0218	−0.0012	−0.0018	−0.6946	0.4874
MB	2.5742	2.5377	1.5507	1.8391	0.0735	0.9414
LMV	4.4909	4.5184	4.3425	4.3811	−0.4700	0.6384
β	1.2042	1.2277	1.1652	1.1897	−1.0205	0.3076
N	5.6176	4.8293	3.0000	3.0000	3.3044	0.0010
CAR period	28.8029	29.2142	18.0000	16.0000	−0.3103	0.7564
Switch sample:						
CAR1	0.0123	−0.0247	0.0051	−0.0130	3.0627	0.0023
CAR2	0.0080	−0.0216	−0.0032	−0.0144	2.1203	0.0345
UE	−0.0201	−0.0242	−0.0012	−0.0018	0.4692	0.6391
MB	2.1604	2.0867	1.3871	1.5834	0.3299	0.7416
LMV	4.4616	4.5006	4.3530	4.3038	−0.2835	0.7769
β	1.2499	1.1923	1.1574	1.1481	1.1667	0.2439
N	6.8969	6.1734	5.0000	4.0000	1.2191	0.2233
CAR period	27.6527	30.9815	17.5000	17.0000	−1.1781	0.2393

Note: *CAR1* = cumulative market-adjusted return; *CAR2* = cumulative market model abnormal return; *UE* = unexpected earnings; *MB* = market value to book value; β = market model slope coefficient; and *N* = number of analysts' forecasts in the consensus forecast. The *T*-statistic tests the significance of the difference in the mean levels of the variables between Big Eight and non-Big Eight firms. The *p*-value measures the two-tailed significance level of the *T*-statistic.

any of the other years, which reflects the inclusion of NASDAQ switches in the later years (35 of 83 switches in 1984–1988 are by NASDAQ firms).[13]

The switch sample also encompasses a wide spectrum of industries. A total of 15 two-digit SIC codes are represented by 20 B-N switches. With at most two B-N switches in any one industry, there appears to be no clustering of the B-N switches. The 114 N-B switches comprise 44 two-digit SIC codes, and there are only three industries with more than six switches (the chemical industry, ten switches; industrial machinery and computer equipment, eight switches; and the electronic and electric industry, 13 switches). Furthermore, there is an overlap of B-N and N-B switches in 11 industries. Thus, as in the matched-pair sample, industry effects are unlikely to account for the results.

IV. Test Procedures and Data

A multiple regression of abnormal stock returns on earnings surprise and other control variables with a dummy variable for the auditor class is used. The regression

[13] Time clustering of switches does not appear to be important for the results of the study. Similar regression results are obtained when switches in 1986 and 1987 are dropped from the sample.

procedure constrains the regression coefficients to be identical for firms within the same auditor class, but they are allowed to vary between auditor classes. For the industry-matched sample, the data is pooled across firms and years so that the following cross-sectional time-series regression is estimated with the OLS method:

$$\begin{aligned} CAR_{it} = {} & \lambda_0 + \lambda_1 D_{it} + \lambda_2 UE_{it} + \lambda_3 UE_{it} D_{it} + \lambda_4 UE_{it} MB_{it} + \lambda_5 UE_{it} MB_{it} D_{it} \\ & + \lambda_6 UE_{it} \beta_{it} + \lambda_7 UE_{it} \beta_{it} D_{it} + \lambda_8 UE_{it} LMV_{it} + \lambda_9 UE_{it} LMV_{it} D_{it} \\ & + \lambda_{10} UE_{it} \frac{1}{N_{it}} + \lambda_{11} UE_{it} \frac{1}{N_{it}} D_{it} + \epsilon_{it}, \end{aligned} \tag{6}$$

where:

CAR_{it} = cumulative abnormal return for firm i, continuously compounded between the forecast date and the earnings date;
UE_{it} = earnings surprise for firm i, as calculated in equation (7) and described later;
D_{it} = dummy variable taking a value of 1 for a non-Big Eight client, 0 otherwise;
MB_{it} = market value to book value as a proxy for growth and persistence;
β_{it} = market model slope coefficient as a proxy for firm risk;
LMV_{it} = natural log of market value as a proxy for firm size;
N_{it} = number of analysts' forecasts included in the consensus forecast as a proxy for noise in the predisclosure environment; and
ϵ_{it} = error term assumed to be distributed $N(0,\sigma_i^2)$.

The average intercept of B8 firms is measured by λ_0; λ_1 measures the shift in the intercept for NB8 firms from that for B8 firms; λ_4, λ_6, λ_8, and λ_{10} are coefficients for the *ERC* contributed by the control variables for the B8 group. The shift in the control variable coefficients for the NB8 group from the slope coefficients of the B8 group is estimated by λ_5, λ_7, λ_9, and λ_{11}. The residual *ERC* left unexplained by these control variables is estimated by λ_2 for the B8 group, and the shift of this coefficient for the NB8 group from that of the B8 group is estimated by λ_3. Thus, the residual *ERC* for the NB8 group is estimated by the sum of $\lambda_2 + \lambda_3$. The hypothesis examines whether the residual *ERC* is smaller for NB8 than for B8 clients, which implies a test of whether the coefficient λ_3 is negative:

H_0: $\lambda_3 \geq 0$.
H_a: $\lambda_3 < 0$.

For the switch sample, we estimate the pooled cross-sectional regression with control variables as in equation (6), but with one modification for the dummy variable: $D_{it} = 1$ if the firm is a NB8 client in year t, 0 otherwise. In this case, λ_2 measures the average residual *ERC* in the years when the firm is audited by a B8 auditor. So, for B-N switches, λ_2 measures the residual *ERC* in the years prior to the switch; for N-B switches, the years after the switch. The difference in residual *ERC* between the years when a B8 auditor is used versus the *ERC* in the years when a NB8 auditor is used is measured by λ_3. The test of whether the residual *ERC* is smaller with a NB8 auditor than with a B8 auditor is operationalized as a test for whether $\lambda_3 < 0$, as in the industry-matched sample.

The control variables for growth and persistence, firm risk, firm size, and the noise in the predisclosure environment are suggested by previous studies of the determinants

of the *ERC*. These studies differ in many dimensions, such as the *ERC* determinants examined, the empirical measures for the determinants, and the test methods and cumulation periods. Kormendi and Lipe (1987) and Lipe (1990) suggest that earnings persistence and earnings predictability are important determinants. Collins and Kothari (1989) emphasize earnings growth and firm risk in addition to earnings persistence. Easton and Zmijewski (1989) consider the revision in analysts' forecasts (which they interpret as a measure of persistence) and expected rate of return (related to firm risk). Some studies have also considered the size of the firm but the evidence has been conflicting. For example, Easton and Zmijewski find firm size to be generally unimportant in determining the *ERC*, and Lipe finds it marginally significant (when measured by market value of equity). However, Shevlin and Shores (1990) report that firm size might be correlated with the other control variables discussed here, and Atiase (1985) suggests that predisclosure environments differ according to size.

B8 and NB8 clients may differ in these various dimensions because of differences in the underlying economic characteristics of the firms. If these factors are important for determining the *ERC*, a difference in the *ERC* may result even if auditor class is not relevant. Because we need to adjust for these confounding factors in the regression in order to measure the incremental effect of auditor class on the *ERC*, these other determinants are included in the regression if empirical proxies can be reasonably measured without unduly sacrificing observations (see the latter discussion on the empirical proxies). Therefore, λ_3 measures the effect of auditor class on the residual *ERC* that is not explained by these factors. Thus, the test procedure here will provide a conservative measure of the importance of credibility if differences in the underlying economic characteristics of the firm are related to the *ERC* by virtue of their correlations with auditor size (and hence, credibility).

Empirical Proxies

Unexpected earnings are measured as the actual earnings disclosed minus a measure of investors' prior expectation of earnings scaled by the stock price,

$$UE = \frac{\textit{Actual Earnings} - \textit{Expected Earnings}}{\textit{Price}}, \tag{7}$$

where *Price* is the stock price on the day prior to the start of the cumulation period for stock returns, if available; otherwise, it is the fiscal year-end stock price.[14] Christie (1987) and Kormendi and Lipe (1987) have suggested that price is the appropriate scaling factor from the theoretical derivation of the *ERC* based on the dividend and earnings capitalization formulae.

In selecting a proxy for expected earnings, we attempt to minimize measurement error by using the IBES analyst consensus (median) earnings forecasts issued before the earnings announcement and selected for a minimal time lag between the forecast and the earnings announcement. Timely forecasts are more likely to capture the market's expectation of earnings more accurately. We discard any observation when $|UE|$ exceeds 100 percent to avoid using any observation that may have an undue influence on the regression parameter estimates (see, e.g., Collins and Kothari 1989). We also

[14] This prevents losing about 7 percent of observations in the matched-pair sample and 3.6 percent in the switch sample. The prices could be missing because of infrequent trading of the NASDAQ firms.

eliminated forecasts made more than 200 trading days prior to the earnings announcement date because news announcements subsequent to the forecast are likely to have resulted in the market's revising its expectation.[15]

Prior evidence suggests that analyst forecasts are better proxies for expected earnings than time-series models of earnings and that more recent forecasts outperform older ones (see, e.g., Brown et al. 1985; O'Brien 1988). Analysts' forecasts are likely superior to an annual random-walk model for expected earnings for our study because our test period is shorter than a year. Unlike analysts' forecasts, a random-walk model for expected earnings would not incorporate new sources of information after the announcement of the previous year's earnings.[16]

The analyst consensus forecast of annual earnings is updated every third Thursday of the month. The most recent consensus forecast available prior to the earnings announcement is selected as follows: If the earnings announcement for a firm occurs after the third Thursday of the month, then the consensus forecast for that month is selected. Otherwise, the consensus forecast for the previous month is selected. This ensures a minimum time lag between the release of annual earnings and the most recent consensus forecast available prior to the firm's disclosure of earnings. The actual earnings numbers and dates for each firm are the earnings per share figures published in *The Wall Street Journal Index*.

The cumulative, continuously compounded abnormal return, CAR_{it}, is the empirical proxy for δ_i in equation (3). It measures the residual price change for firm i that is unexplained by the beta risk of the firm. This is presumed to be the investors' reaction to new information in the earnings announcement. We cumulate the abnormal return from the day of the consensus forecast to the day of the earnings announcement to minimize measurement error in the earnings surprise. Thus, we measure the abnormal return over the same period as the forecast horizon.[17] We estimate CAR_{it} in two ways. The first measure assumes that the market model β is unity for all firms in the sample, so the abnormal return is estimated as the raw return minus the market return of the same day, continuously compounded between the IBES consensus forecast date and the earnings announcement date:

$$CAR1_{it} = \sum_{t=IB}^{ED} \log(1 + R_{it} - R_{mt}),$$

where IB is the *IBES* consensus forecast date and ED is the earnings announcement date; the market return is the CRSP equally-weighted stock index if the firm was traded

[15] The results are similar when using a 100-day cutoff.

[16] O'Brien (1988) does note that analysts' forecasts are not necessarily best in association studies. Although a more sophisticated time-series model might be superior to the annual random-walk model, the data requirements for estimating such a model would be too restrictive for our study.

[17] A disadvantage is that the measure of abnormal return (the dependent variable) includes noise in the form of other announcements during this interval. The industry-matching design and switch-sample design, a 200-day maximum period, and large sample sizes are attempts to limit the noise introduced by other news. Alternatively, an analysis over, e.g., days −1 and 0 relative to the earnings announcement date, could be used. Unlike problem of noise in the *dependent* variable over a long period, the analysis for a short period has the serious disadvantage that the regression coefficients are biased because of measurement error in the *independent* variables. The size of the measurement error bias and the amount of correction for the bias when using the Easton and Zmijewski (1989) technique will differ between Big Eight and non-Big Eight subsamples. Since the empirical tests center on comparing regression coefficients between the groups, an analysis over a short period will in general lead to invalid inferences.

on the NYSE or AMEX, and is the NASDAQ market index for NASDAQ firms. The second measure is the residual from a market model regression and is calculated as:

$$CAR2_{it} = \sum_{t=IB}^{ED} \log(1 + R_{it} - \hat{\alpha}_i - \hat{\beta}_i R_{mt}),$$

where the market model parameters $\hat{\alpha}_i$ and $\hat{\beta}_i$ are estimated from a time-series firm-specific regression of the stock return on the market return defined as for $CAR1$. The estimation period for this regression covers the 360 days prior to the IBES consensus forecast date. There must be a minimum of 100 daily returns in the period for the market model regression.

We consider both measures of CAR_{it} to gauge whether the results are sensitive to errors in measuring the relevant variables, since different measures will introduce different types of noise in measurement. For example, $CAR1$ will be mismeasured if the true β of the firm is not unity, and $CAR2$ may be a noisy measure if estimates of the parameters are inaccurate because of infrequent trading among NASDAQ firms in the sample (see Marais et al. 1989). Thus, the simpler method of the first measure may be preferable to avoid introducing unnecessary noise into the regression equation.

We measure MB_{it} as the market to the book value of equity at the beginning of each year t. Because it is plausible that future earnings are affected by growth opportunities, the higher the market to book value, the larger is the expected earnings growth. Collins and Kothari (1989) suggest that this ratio is also affected by persistence. To the extent that this is the case, it provides a normalization for persistence as well.[18]

We obtain β_{it} from the market model estimation for expected returns discussed earlier, and it is a proxy for the riskiness of the firm's earnings. In Kormendi and Lipe (1987), and Easton and Zmijewski (1989), among others, the firm's riskiness enters the valuation models as the present-value discount factor for the revision in the expected earnings in each period. Thus, a negative relation between the ERC and firm risk is predicted. The empirical evidence, however, has been mixed in these previous studies. Easton and Zmijewski did not find a significant partial correlation between the ERC and β. Collins and Kothari report a significant positive relation between β and their return-response coefficient in a reverse regression of the earnings surprise on a variety of explanatory variables, including the raw return, and β. They interpret this as confirming a negative relation between the ERC and risk.[19] Another confounding factor in the relation between the ERC and risk might be gleaned from the model in section II. We might think of the firm's risk measure as a proxy for v in the model; more risky firms have higher variance of the underlying cash flows. If this is true, then equation (4) predicts that the ERC should be positively related to risk. A more general equilibrium model is required before the relation between risk and the ERC can be predicted. Thus, we include β in our regression as a control variable without imposing any priors about the sign of the coefficient.

[18] A separate persistence measure is not included because the earnings history of many firms is insufficient for adequate empirical measure for a large number of the firms in our sample. Kormendi and Lipe (1987) used 35 consecutive earnings observations per firm, and Easton and Zmijewski (1989) used 20 in their time-series measure of persistence. Since our study emphasizes audited earnings, the requirement of 20 consecutive annual earnings prior to the test period would eliminate a considerable number of our firms, especially the NASDAQ firms.

[19] Leamer (1978) suggests caution in interpreting results of multiple regressions when reverse regression methods are employed and there is correlation among the independent variables.

We also include a measure of firm size (the natural log of market value at the beginning of the year) in the regression equation, as a control variable for other possible missing factors. (As noted earlier, the economic rationale for including size is unclear, and previous studies have not found size to be important.) Finally, we include the number of analysts' forecasts used in computing the consensus forecast in an attempt to control for differences in the predisclosure environment. If there are more analysts following the firm, there may be more information about the firm, and so the market's expectation of the firm's earnings might be more accurately estimated.

We use a binary measure for these control variables because of the possibility of nonlinearity. Thus, MB_{it}, β_{it}, LMV_{it}, and $1/N_{it}$ take on the value 1 when the measures are above the median and 0 otherwise. The binary measures are intended to reduce noise from mismeasurement of these variables, and so reduce the possibility that large measures have an undue influence on the regression.[20] Care, therefore, must be exercised in interpreting the results for these other variables.

To check whether the B8 and NB8 groups differ in the dimensions of these control variables, we provide summary statistics in table 2 for each group separately, for both the matched-pair and the switch samples.[21] The data indicate that in the matched-pair sample, the groups do not differ significantly in the mean and median levels of the proxies for growth and persistence, firm risk, and firm size. The mean number of analysts following the firms is significantly different between the groups, with B8 firms having more visibility with analysts. This result should be interpreted with caution because the median number of analysts is the same for both groups. This suggests the presence of nonnormality in the distribution of analysts, and therefore, use of a binary measure may be appropriate. The mean levels of the control variables do not differ significantly between the auditor class groups in the switch sample.

Summary statistics for the dependent variable, the earnings surprise measure, and the length of the cumulation period for the abnormal returns are also included in table 2. None of the variables differs significantly in the mean levels between the B8 and the NB8 firms in the matched-pair sample. In the switch sample, the earnings surprise and the cumulation window also do not differ between the groups, but the cumulative abnormal returns are larger when B8 auditors are used.[22] Different degrees of noise in the measure of *CAR* between the auditor groups because of different *CAR* periods could have resulted in heteroscedasticity in the errors of the regression equations. The insignificant difference in the mean and median *CAR* periods between the groups in table 2 eliminates this potential source for heteroscedasticity in the regression equations. In addition, the Pearson correlation matrix for the variables is reported in table 3 for each auditor class group for the matched-pair and switch samples. Some

[20] Inclusion of these explanatory variables, if relevant, will improve the specification of the regression equation. However, if these variables are measured with error and these errors are correlated with the earnings surprise proxy, the *ERC* estimates will remain biased. Nevertheless, so long as the degree of bias is the same for both auditor class groups, the test for a difference in the *ERC* between the two will remain informative.

[21] The statistics are reported for continuous measures of these control variables, not the binary measures used in the regression equations. They are calculated for observations used in the regressions, which exclude observations with missing data for some control variables (2 percent of matched-pair sample and 3 percent of switch sample).

[22] This is consistent with the results of Johnson and Lys (1990) and Teoh (1992b) that, on average, non-Big Eight to Big Eight auditor switches are preceded over a 60 day period by positive abnormal stock returns, and Big Eight to non-Big Eight switches by negative abnormal stock returns. This difference is captured by a significant intercept dummy in the estimated regression equation (6) reported in table 4.

Table 3
Correlation Matrix of Earnings Surprises and Control Variables
(Two-tailed *p*-values)

Panel A. Industry-Matched Sample:

	Audited by Big Eight Auditors						*Audited by Non-Big Eight Auditors*				
	UE	*MB*	*LMV*	*β*	*N*		*UE*	*MB*	*LMV*	*β*	*N*
UE	1.000	0.015	0.133	−0.062	0.097	*UE*	1.000	−0.046	0.031	−0.023	0.024
	0.000	0.585	0.000	0.026	0.000		0.000	0.108	0.275	0.423	0.392
MB		1.000	0.025	0.079	−0.009	*MB*		1.000	0.092	0.191	−0.001
		0.000	0.368	0.005	0.740			0.000	0.001	0.000	0.962
LMV			1.000	0.069	0.740	*LMV*			1.000	0.058	0.678
			0.000	0.014	0.000				0.000	0.042	0.000
β				1.000	0.083	*β*				1.000	0.046
				0.000	0.003					0.000	0.107
N					1.000	*N*					1.000
					0.000						0.000

Panel B. Switch Sample:

	Audited by Big Eight Auditors						*Audited by Non-Big Eight Auditors*				
	UE	*MB*	*LMV*	*β*	*N*		*UE*	*MB*	*LMV*	*β*	*N*
UE	1.000	0.024	−0.030	0.013	0.077	*UE*	1.000	0.102	0.122	−0.086	0.142
	0.000	0.695	0.624	0.834	0.212		0.000	0.094	0.044	0.160	0.019
MB		1.000	0.062	0.213	−0.020	*MB*		1.000	0.081	0.141	0.018
		0.000	0.315	0.000	0.741			0.000	0.184	0.020	0.767
LMV			1.000	0.152	0.625	*LMV*			1.000	0.042	0.669
			0.000	0.014	0.000				0.000	0.492	0.000
β				1.000	0.048	*β*				1.000	−0.009
				0.000	0.443					0.000	0.880
N					1.000	*N*					1.000
					0.000						0.000

Note: *UE* = unexpected earnings; *MB* = market value to book value; *LMV* = log of market value of common stock outstanding; *β* = market model slope coefficient; and *N* = number of analysts' forecasts in the consensus forecast.

differences in the partial correlations among the variables between the B8 and NB8 groups suggest that it might be appropriate to allow the slope coefficients of the control variables to vary between the two as in regression equation (6). The presence of significant correlations among some of the control variables suggests that it may be difficult to interpret the coefficients for the control variables separately even though they may be important joint determinants of the *ERC*.

V. Empirical Results

The OLS regressions for the matched-pair and switch samples are reported in table 4 and include the results when the market-adjusted return is used as a measure of

Table 4
OLS Regression of CARs on Earnings Surprises and Control Variables

$$CAR_{it}=\lambda_0+\lambda_1 D_{it}+\lambda_2 UE_{it}+\lambda_3 UE_{it}\bullet D_{it}+\lambda_4 UE_{it}\bullet MB_{it}+\lambda_5 UE_{it}\bullet MB_{it}\bullet D_{it}$$
$$+\lambda_6 UE_{it}\bullet\beta_{it}+\lambda_7 UE_{it}\bullet\beta_{it}\bullet D_{it}+\lambda_8 UE_{it}\bullet LMV_{it}+\lambda_9 UE_{it}\bullet LMV_{it}\bullet D_{it}$$
$$+\lambda_{10} UE_{it}\bullet\frac{1}{N_{it}}+\lambda_{11} UE_{it}\bullet\frac{1}{N_{it}}\bullet D_{it}+\epsilon_{it}$$

Panel A. Matched-Pair Sample:

*CAR*1 = cumulative market-adjusted return (*F*-statistic = 12.372, 0.0001; χ^2 = 47.359, 0.0504; $\bar{R}^2$ = 0.0465)

Variable	*Parameter Estimate*	T *for* H0 (*parameter* = 0)	*Probability* > |t|	*White's z-statistic*	*Probability* > |z|
Intercept	−0.015	−3.451	0.0006	−3.930	0.0001
D	0.004	0.589	0.5556	0.636	0.5246
UE	0.421	4.348	0.0001	3.804	0.0001
UE•D	−0.401	−2.523	0.0117	−1.972	0.0486
UE•MB	0.789	3.391	0.0007	2.732	0.0063
UE•MB•D	−0.405	−1.593	0.1113	−0.987	0.3235
UE•β	−0.183	−2.120	0.0341	−1.275	0.2022
UE•β•D	−0.029	−0.232	0.8162	−0.130	0.8960
UE•LMV	−0.229	−2.490	0.0128	−2.511	0.0120
UE•LMV•D	0.307	1.848	0.0648	1.783	0.0746
$UE\bullet\frac{1}{N}$	0.060	0.689	0.4907	0.600	0.5488
$UE\bullet\frac{1}{N}\bullet D$	0.287	2.005	0.0450	1.505	0.1324

*CAR*2 = cumulative market model abnormal return (*F*-statistic = 3.813, 0.0001; χ^2 = 43.051, 0.1131; $\bar{R}^2$ = 0.0119)

Variable	*Parameter Estimate*	T *for* H0 (*parameter* = 0)	*Probability* > |t|
Intercept	−0.018	−3.669	0.0002
D	0.004	0.559	0.5765
UE	0.264	2.427	0.0153
UE•D	−0.317	−1.776	0.0759
UE•MB	0.428	1.637	0.1017
UE•MB•D	−0.102	−0.359	0.7197
UE•β	−0.101	−1.040	0.2985
UE•β•D	−0.134	−0.971	0.3317
UE•LMV	−0.212	−2.055	0.0400
UE•LMV•D	0.334	1.791	0.0734
$UE\bullet\frac{1}{N}$	0.017	0.169	0.8657
$UE\bullet\frac{1}{N}\bullet D$	0.291	1.815	0.0697

Continued

abnormal stock return (*CAR*1) and those for the market model residual as the measure of abnormal returns (*CAR*2). The regression specification appears to be adequate in both panels A and B. The *F*-statistics for the goodness-of-fit are significant at the 0.0001 level in all four regressions. In addition, the χ^2-statistic to test for the presence of

Table 4–*Continued*

Panel B. Switch Sample:

$CAR1$ = cumulative market-adjusted return (F-statistic = 5.672, 0.0001; χ^2 = 28.724, 0.6800; $\bar{R}^2$ = 0.0869)

Variable	*Parameter Estimate*	T *for* H0 (*parameter* = 0)	*Probability* > |t|
Intercept	0.014	1.581	0.1146
D	−0.033	−2.703	0.0071
UE	0.740	3.971	0.0001
$UE \cdot D$	−0.974	−4.290	0.0001
$UE \cdot MB$	−0.144	−0.503	0.6153
$UE \cdot MB \cdot D$	0.664	1.958	0.0508
$UE \cdot \beta$	0.216	1.194	0.2330
$UE \cdot \beta \cdot D$	−0.560	−2.222	0.0267
$UE \cdot LMV$	−0.606	−2.775	0.0057
$UE \cdot LMV \cdot D$	0.618	2.164	0.0309
$UE \cdot \frac{1}{N}$	−0.483	−2.569	0.0105
$UE \cdot \frac{1}{N} \cdot D$	1.032	3.953	0.0001

$CAR2$ = cumulative market model abnormal return (F-statistic = 4.374, 0.0001; χ^2 = 27.014, 0.7590; $\bar{R}^2$ = 0.0643)

Variable	*Parameter Estimate*	T *for* H0 (*parameter* = 0)	*Probability* > |t|
Intercept	0.010	1.006	0.3151
D	−0.030	−2.172	0.0303
UE	0.857	3.986	0.0001
$UE \cdot D$	−1.015	−3.874	0.0001
$UE \cdot MB$	−0.430	−1.305	0.1926
$UE \cdot MB \cdot D$	0.788	2.012	0.0447
$UE \cdot \beta$	0.078	0.371	0.7105
$UE \cdot \beta \cdot D$	−0.378	−1.299	0.1945
$UE \cdot LMV$	−0.626	−2.487	0.0132
$UE \cdot LMV \cdot D$	0.521	1.581	0.1145
$UE \cdot \frac{1}{N}$	−0.439	−2.024	0.0434
$UE \cdot \frac{1}{N} \cdot D$	0.811	2.691	0.0074

Notes: D = dummy variable with a value of 1 for a Big Eight auditor, 0 otherwise; UE = unexpected earnings; MB = market value to book value; β = market model slope coefficient; LMV = log of market value of common stock outstanding; and N = number of analysts' forecasts in the consensus forecast. The model is from equation (6); MB_{it}, β_{it}, LMV_{it}, and $1/N_{it}$ are binary variables with a value of 1 if above median, and 0 otherwise. For the switch sample, the regression is estimated from two fiscal years before to two fiscal years after the switch.

heteroscedasticity of the residual errors is not significant in three of the four regressions. The exception is where the χ^2 = statistic for $CAR1$ is significant at the 5 percent level. Thus, we employ White's general heteroscedasticity-corrected covariance matrix estimate and report the resulting z-statistic and corresponding p-values.

The adjusted $\bar{R}^2$ in all four regressions is in the range reported by Lev (1989) in a survey of literature on earnings studies (usually less than 10 percent and generally

between 1 percent and 5 percent in the majority of studies). In table 4, the adjusted $\bar{R}^2$ for the matched-pair sample is 4.65 and 1.19 percent, respectively, for the *CAR*1 and *CAR*2 regressions, and it is 8.69 and 6.43 percent, respectively, for the switch sample. The greater explanatory power for the switch sample than for the industry-matched sample suggests that using the firm's own past history as its control may better avoid missing determinants of the *ERC*. The explanatory power is higher for the *CAR*1 regression in both panels, which suggests, as noted earlier, that the market model may be a more noisy measure for expected returns.

In all four regressions, the evidence is consistent with the test hypothesis that the residual *ERC* is smaller for NB8 firms than for B8 firms. The λ_3 coefficient is negative, as predicted, and the magnitude is statistically significant at the 0.0001 level in both regressions of panel B, and at less than the 5 percent level (one-tailed) in panel A. The parameter estimate for λ_3 is -0.401 (White z-statistic $= -1.97$, $t = -2.53$) in panel A for the *CAR*1 regression and -0.317 ($t = -1.78$) for the *CAR*2 regression. In panel B, the λ_3 coefficient is -0.974 ($t = -4.29$) for the *CAR*1 regression and -1.015 ($t = 3.874$) for the *CAR*2 regression.

In all four regressions, the residual *ERC* for the B8 group is positive, ranging from 0.264 to 0.857, and is statistically significant at the 0.0001 level in three of the regressions and at 0.02 level in the fourth. In contrast, the residual *ERC* for the NB8 group (estimated by the sum of λ_2 and λ_3) is negative in three of the regressions and positive in the fourth. However, because the residual *ERC* measures only the part left unexplained by the included control variables, this does not mean that the *ERC* is positive for B8 groups and negative for NB8 groups.

Table 4 also provides some interesting evidence for comparison with previous studies on the determinants of the *ERC*. Because the qualitative results are similar for the *CAR*1 and *CAR*2 regressions, the remaining discussion comments only on *CAR*1 results. Consider first the results for the growth and persistence proxy. In panel A, the coefficient λ_4 for this proxy is positive and significant at the 1 percent level, as expected. The corresponding coefficient for the NB8 group is smaller, as evidenced by the negative λ_5, but also positive ($\lambda_4 + \lambda_5 > 0$). Thus, investors are more responsive to earnings surprises from firms that are growing and/or expected to persist.

In panel B of table 4, λ_4 is negative but not statistically significant, and λ_5 is positive and significant at the 5 percent level. They suggest that growth/persistence is relatively more important for the *ERC* of cross-class switches during the period when they are audited by NB8 auditors than when they are audited by B8 auditors. A partial explanation is that there is insufficient variation in the growth/persistence measure for these switches during the years when a B8 auditor is used and greater variation during the years when a NB8 auditor is used. For example, N-B switches might be experiencing high earnings growth just prior to the switch, and B-N switches might be experiencing declining earnings after the switch.

In panel A of table 4, the coefficient λ_6 for the risk proxy for the B8 group is negative though the White z-statistic is not statistically significant. The coefficient for the NB8 group is also negative and is not statistically different from the coefficient for the B8 clients. In panel B, λ_6 is positive though small and not statistically significant. The coefficient for the NB8 group is significantly more negative than for the B8 group, as evidenced by a statistically significant λ_7 coefficient. Thus, the risk proxy for the NB8 group in panel B is significantly negative. The generally weak evidence and inconsistent signs are not surprising, given the inconclusive results of previous studies and

the possibility that β might proxy for both systematic risk of the firm and the amount of prior uncertainty of information, as discussed in section IV.

As mentioned earlier, the empirical evidence for the relation between client firm size and the *ERC* has been conflicting. This is not surprising given (1) the absence of a theory for the relevance of client firm size for the *ERC*, (2) the possibility that client firm size proxies for many relevant factors, and (3) the alternative methods for studying size in previous studies.[23] The results in our study are also conflicting. In both panels of table 4, the coefficient for the firm size proxy is significantly negative for B8 clients at the 1 percent level but is positive for NB8 clients. The difference between the coefficients for the groups is statistically significant.

Finally, no consistent results are obtained for $1/N$, the reciprocal of the number of analysts following the firm. In panel A of table 4, the number of analysts appears unimportant for the *ERC*, as λ_{10} and λ_{11} are not statistically significant at the 5 percent level. In panel B, however, λ_{10} is significantly negative at the 1 percent level, $\lambda_{10}+\lambda_{11}$ is positive, and λ_{11} is significantly positive at the 1 percent level. This evidence suggests that the *ERC* increases (decreases) with the number of analysts for B8 (NB8) client firms. The model in section II predicts a positive relation between the *ERC* and the amount of prior uncertainty of the firm. If a small number of analysts following the firm implies that little is known about the underlying cash flows of the firm (greater prior uncertainty), then the model predicts a negative relation between the number of analysts and the *ERC*. Thus, the results for the NB8 group is consistent with the prediction of the model, but not those of the B8 group. Several explanations are possible for these weak and conflicting results. As seen in the correlation matrices (table 3), N is significantly correlated with firm size. The coefficients for $1/N$ and *LMV* therefore may be separately unreliable though both variables are important for the *ERC*. In addition, the relation between the amount of prior uncertainty for the firm and the number of analysts need not be monotonically decreasing (see O'Brien and Bhushan 1990). For example, greater uncertainty of the underlying cash flows might increase analysts' incentives to follow the firm because of the greater potential value of information.

VI. Conclusion

We have examined whether some auditors have higher quality than others, in the sense of generating earnings reports with greater credibility for investors. We tested this issue jointly with a prediction of a modified Holthausen and Verrecchia (1988) model, that earnings reports with less noise have higher *ERC*s. The empirical test is operationalized as a test for a difference in the *ERC* between B8 and NB8 clients in a regression of abnormal stock returns on earnings surprise and control variables for other determinants of the *ERC*. We find that B8 clients have statistically significantly larger *ERC*s than NB8 clients. The result obtains in both a matched-pair sample of firms paired according to industry membership and a switch sample of firms grouped according to auditor size. Thus, the evidence is consistent with the joint hypothesis that larger auditors generate more precise earnings and that the Holthausen-Verrecchia prediction relating *ERC* to earnings precision is correct.

[23] See Shevlin and Shores (1990, fn. 21) for a comprehensive summary of the treatment and findings for firm size in past earnings studies.

The study also provides new evidence regarding the determinants of the *ERC*. Previous studies have found that the *ERC* increases with growth and persistence, have obtained inconsistent findings for client firm size and risk across studies, and have not considered the number of analysts following a firm as a determinant. We find that the relation of the *ERC* with growth and persistence is positive, and with firm risk is weak and negative. The relation of the *ERC* with the number of analysts following the firm, as a proxy for the noise in the information environment, is found to differ between B8 and NB8 client firms; the relation is positive for B8 clients, negative for NB8 clients. The relation of the *ERC* with client firm size is also found to differ between the groups; it is negative for B8 clients and positive for non-Big Eight clients.

We provide robustness checks on the results by using two alternative sample designs and including known determinants of the *ERC* as control variables. However, the results may be biased if there are other determinants of the *ERC*, not fully captured by the included control variables, that vary systematically between the auditor class groups. In addition, measurement error of the included variables that differ between the groups will also bias the results. However, to the extent that the included control variables are related to the *ERC* by virtue of their correlation with auditor size and credibility, the test procedure provides a conservative estimate of the effect of auditor size on the *ERC*.

By linking differences in perceived auditor quality to the *ERC*, this study provides evidence for the view that larger auditors are more credible. The issue of whether auditor quality is related to size may be relevant for the current policy debate on whether to permit mergers among large audit firms. This study is also relevant for capital markets research because it provides evidence of additional determinants of the *ERC*, auditor size, and the number of analysts following the firm. Finally, the study provides the first empirical verification of the Holthausen-Verrecchia model of earnings precision and stock price changes.

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