RETURNS-EARNINGS REGRESSIONS: AN INTEGRATED APPROACH

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ABSTRACT: This paper estimates earnings response coefficients (ERCs) for a large sample of companies using six methodological approaches, the simultaneous use of which is hypothesized to improve the estimation. First, we carefully match the windows over which abnormal returns and unexpected earnings are measured. Second, we control for the measurement error in unexpected earnings. Third, we use analyst earnings forecasts as a proxy for market expectations. Fourth, we control for nonlinear effects. Fifth, we consider the effect of losses, and sixth, we use firm-specific time-series regressions, in addition to pooled, cross-sectional regressions. The combined and simultaneous use of these six methodological approaches, which have not been employed simultaneously in prior research, results in a substantial increase in the estimated ERCs for firm-specific regressions as well as pooled time-series cross-sectional regressions. These results have important implications for both research and practice.

Key Words: Earnings response coefficient (ERCs), Earnings expectations, Dispersion of analyst earnings forecasts, Measurement error.

Data Availability: Data used in this study are available from public sources identified in the study.

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I. INTRODUCTION

The relation between earnings and stock returns has occupied much of the accounting literature during the past two decades. A rich variety of metrics are used in estimating the earnings response coefficient (ERC), such as returns and levels of earnings, typically scaled by prices, and returns and the unexpected portion of earnings, quantified using expectations models for earnings (time-series models, analysts' forecasts, etc.) and for returns (size-adjusted, market-model-predicted-returns, etc.) All studies, however, invariably report ERCs that seem implausibly small (as noted, e.g., by Easton, Harris, and Ohlson 1992, 120).

The question this study addresses is whether the simultaneous use of six methodological refinements that have already been employed in prior literature, but not in combination, can result in more plausible estimates. Specifically, it is hypothesized that the simultaneous use of six refinements in model specification and variable measurement would increase estimated ERCs (which invariably have been reported as implausibly small in prior research.) We find that this integrated approach results in substantial increases in estimated ERCs to more plausible levels.

In addition to possibly shedding light on the puzzling findings of small ERCs, addressing this question is important for any use accounting researchers or other professionals may wish to make of estimated ERCs. Researchers often attempt to draw inferences based on the comparison of magnitudes of ERC's of sets of firms with different pre-specified characteristics. Collins and Salatka (1993), for example, investigate the valuation relevance of alternative foreign currency methods by comparing the magnitudes of ERCs of two sets of firms on the basis of their choice of the functional currency. Teets (1992) studies the effects of regulation on the market valuation of earnings surprises by comparing the magnitudes of ERCs of non-regulated firms and regulated electric utilities. Such comparisons can lead to more meaningful inferences when the estimated ERCs are the result of employing methodological refinements that yield plausible levels of ERCs.

We employ simultaneously six methodological refinements using linear regressions of abnormal returns on unexpected earnings, properly scaled. Specifically, we (1) use analysts' forecasts rather than naïve, time-series models, as a proxy for the market's earnings expectations; (2) match closely the period over which unexpected earnings are measured and the period over which abnormal returns are accumulated in addition to using short windows for the accumulation of abnormal returns; (3) account for nonlinearity; (4) employ firm-specific as well as pooled cross-sectional regressions; (5) account for measurement error in unexpected earnings; and (6) consider the effect of losses.

We find that when (1) analysts' earnings forecasts are used as a proxy for the market's earnings expectations, (2) time windows of unexpected earnings and returns are aligned properly, (3) the proxy we use for measurement error in standardized unexpected earnings (SUE) is small, (4) only firms with positive earnings and forecasts are considered, (5) firm-specific, time-series regressions are used, and (6) nonlinear effects are controlled for, the median ERC is approximately 24. In contrast, prior research using linear regressions reports ERCs not exceeding 3 when using returns as dependent variables.

Incrementally, proper alignment of the period over which unexpected earnings and returns are measured leads to ERCs that are substantially greater than those obtained by prior studies. For example, Easton and Zmijewski (1989, table 2, panel B), whose attempt to align the windows is not entirely successful (see discussion below), report a median ERC of 1.702, whereas aligning the windows properly in our sample under a firm-specific regression (and without other methodological refinements) increases the median ERC from 1.95 to 7.05. Improper alignment thus is an important reason for the implausibly small ERCs reported in prior studies.

The results also show that controlling for the known nonlinearity effect increases the ERCs. Furthermore, while the nonlinearity effect partially overlaps with measurement error in expected earnings, a reduced measurement error is shown to increase ERCs across a variety of estimation methods and controls by increments ranging up to approximately 5.50 above and beyond the effect of nonlinearity.

Firm-specific estimation also results incrementally in higher ERCs vis-à-vis cross-sectional estimation. Depending on other variations in estimation methods and controls, using firm-specific regressions increases ERC estimates by increments ranging up to 5.58. ERC increments obtained by controlling for losses and for nonlinearity range up to 6.64 and 15.17, respectively.

Collectively, our results provide a new explanation for the implausibly small ERCs observed by prior researchers (e.g., Easton and Zmijewski 1989, Kothari and Sloan 1992, Lipe 1990, and Teets and Wasley 1996), as well as a methodology for a more reliable estimation of the coefficients. Specifically, the results suggest that the simultaneous (1) use of analysts' forecasts, (2) consideration of measurement errors in expected earnings, (3) proper alignment of windows, (4) use of firm-specific time-series regressions, (5) controlling for nonlinearity effects, and (6) controlling for losses, and not any single factor considered in isolation, will help produce more plausible estimates of ERCs. Additionally, while firm specific regressions yield higher ERCs, controlling for nonlinearity and measurement errors in unexpected earnings also increases substantially ERC estimates obtained from pooled time-series cross-sectional regressions. This last result is important because firm-specific estimation is feasible for only a small percentage of firms, and in particular it may not be used when studying firms with short histories (e.g., IPOs).

Our findings regarding the importance of measurement errors for estimating ERCs also have implications for investment analysis and management. If smaller measurement errors in unexpected earnings are a precursor of higher ERCs, information gathering and analysis designed to predict earnings are perhaps then best focused on companies associated with relatively small measurement errors. The return on investment analysis activities might then be maximized unless these firms happen also to be those for which there has already been a high amount of private or public information-gathering and analysis designed to predict earnings.

The next section discusses prior research, the motivation, and the research question. Section 3 presents the research design. Section 4 outlines the sample selection procedure, defines the variables, and describes the data. Section 5 reports the empirical findings, and the final section summarizes our findings and offers concluding remarks.

II. BACKGROUND, MOTIVATION, AND RESEARCH QUESTION

One of the most persistent features of the returns/earnings relation research is the finding of implausibly small earnings response coefficients. While it is a formidable task to theoretically determine a plausible range for ERCs, one can exploit the parsimonious theoretical model in Ohlson (1995) in conjunction with certain reasonable assumptions to at least provide a lower bound for a plausible ERC. We assume abnormal earnings follow a seasonal random walk with a trend process.[1] This implies in terms of the Ohlson model (equation 6, p. 670) that ω equals l

1. This assumption is not unreasonable in light of the evidence in, e.g., Bernard and Thomas (1990) and Bartov (1992), showing that the market behaves as if quarterly earnings follow a seasonal random walk with a trend.

and v is positive, where ω and v are the parameters of the stochastic process specified in his assumption 3 (A3, p. 663). Since the coefficient on unexpected earnings in equation (6) is $1+\omega/(R_f - \omega)$, we need to further specify the magnitude of R_f, which is the risk-free rate plus one. As a rough estimate of the risk-free rate, consider the average return on a one-year treasury bond over the years 1988 to 1998, which equals 0.068. That is, R_f can be approximated at 1.068. The resulting lower bound for ERC is thus 1 + (1/0.068) = 15.71. This is a lower bound because it does not incorporate information related to growth in future abnormal returns that may be anticipated by the market but not yet captured in past and current earnings. In other words, the "unexpected innovations" in "other information" that is the second source of uncertainty in Ohlson's equation (6) is typically absent in empirical regressions of returns on unexpected earnings. To the extent this omitted other information variable implies future growth in abnormal earnings, the observed ERC should be expected to be larger than the above-computed lower bound. Yet it is difficult to specify an upper bound. In theory, if the coefficient (say δ) on unexpected earnings of a regression of the omitted variable, i.e., unexpected innovations in other information (ε_{2t+1}/P_t) in Ohlson's equation (6), were known, we could compute the bias in the observed ERC. Since the theoretical coefficient on (ε_{2t+1}/P_t) in Ohlson's equation (6) is $\alpha_2 = R_f/(R_f-\omega)(R_f-\gamma)$, the bias in the observed ERC would equal $\delta R_f/(R_f-\omega)(R_f-\gamma)$ (see, e.g., Greene 1990, p. 259.) To illustrate, suppose δ=0.5, γ=0.5, and that R_f and ω have the same values assumed above (1.068 and 1 respectively); we then have a bias of 0.5 x 1.068/0.068 x 0.568 = 13.83. That is, the observed (biased) ERC would be expected to be 15.71 + 13.83 = 29.54. If, on the other hand, "other information" had zero persistence (γ=0), ERC would be 23. If, further, δ were only 0.1, ERC would be 17.18. The main point here is that a plausible range of estimated ERCs that does not explicitly incorporate "other information" such as earnings growth will exceed ERCs estimated in the literature to date.[2]

Indeed, extant empirical research findings briefly reviewed below point to considerably smaller ERCs, typically not exceeding 3 when using stock returns as a dependent variable.[3] While response coefficients estimated from cross-sectional regressions increase steadily with the length of the return interval in the study by Easton, Harris, and Ohlson (1992, p. 138), they still lie within a low range of 0.53 for a one-year interval and 1.66 for a ten-year interval.[4]

Time-series, firm-specific response coefficients are only slightly higher than those obtained from cross-sectional regressions, and still well below their plausible value. For example, Kothari (1992), using firm-specific time-series price-earnings regressions over one-year windows, estimates response coefficients with a mean of 2.61 and median of 2.00 using earnings levels scaled by prices, a mean of 3.31 and a median of 1.82 using earnings changes scaled by price, and a mean of 0.26 and a median of 0.13 using earnings changes scaled by lagged earnings. While Kothari (1992) does not attempt returns/earnings regressions that employ unexpected earnings as the explanatory variable, he claims that unexpected earnings are the logical variable

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2. Our study focuses on quarterly, not annual, earnings surprises. Nonetheless, under the widely accepted seasonal-random-walk model of quarterly earnings expectations quarterly response coefficients should not differ significantly from the annual ones. This follow because one dollar of quarterly earnings surprise is also one dollar of annual earnings surprise, and therefore quarterly and annual earnings surprises of the same magnitude should have the same impact on price and, hence, the same response coefficients.

3. The only exception to the findings of "small" ERCs is Freeman and TSE (1992). They hypothesize that the absolute value of unexpected earnings is negatively correlated with persistence, and their nonlinear regression yields results consistent with this premise, exhibiting an average ERC of 14.0 in cross-section. Also, Kothari and Zimmerman (1995) obtain an ERC estimate of 6 when regressing price on earnings and of 7.7 when regressing price divided by earnings on 1 (see their p. 180). Their study is not comparable to ours, as it does not estimate the impact of earnings surprises on returns (their return regression yields an ERC estimate of 0.45, and a differenced price regression yields an ERC estimate of 2.09, see their table 2).

4. Their research design is not directly comparable to that of others and to this paper's design. Their regressions have returns as the dependent variable and earnings as the independent variable, both scaled by price at the beginning of the accumulation. Moreover, theirs is not an attempt to estimate the price response to earnings innovation as is the case in this and related studies; rather, they effectively examine the association between ratios of earnings to price with returns. As the authors themselves state, they can offer no explanation for the observed pattern of coefficients.

to use in price-earnings regressions. That is, an accurate proxy for market-unexpected earnings should outperform both earnings-level and earnings-change-deflated-by-price variables. An accurate proxy for the market's expectations, however, may be difficult to identify: time-series-based proxies for earnings expectations fail to capture newly available information incrementally useful in predicting earnings.

Using analysts' forecasts (scaled by stock prices) instead, in an attempt to obtain a more accurate proxy for the market's earnings expectations, as well as closely matched windows around quarterly earnings announcements and firm-specific time-series regressions, Easton and Zmijewski (1989, table 2, panel B) estimate ERCs with a mean of 2.531 and median of 1.702.[5]

There are two major sets of factors that likely contribute to the small estimates of ERCs: the first is related to measurement error given a specified model, and the second is related to model specification.

The first set of factors can result from a variety of sources. First, choice of the proxy for the market's earnings expectations can be a source of measurement error. Time-series-based proxies for earnings expectations fail to capture new information useful in predicting earnings that has become available to the market since the previous earnings announcements. Use of analysts' forecasts can reduce this measurement error since these forecasts incorporate non-earnings information about future cash flows embedded in stock returns.

Second, even if based on analysts' forecasts, when unexpected earnings are associated with abnormal returns errors in measurement of unexpected earnings invariably confound the association and bias the estimated coefficient towards zero because of yet another two factors.

5. For an insightful review of the role of analysts' earnings forecasts in capital market research generally, see, e.g., Brown (1993).

Specifically, the measurement error can result from either (1) the difference between the proxy based on analysts' forecasts and the unobserved expectation (Brown et al. 1987), or (2) noise in the reported earnings (Beaver, Lambert and Morse 1980).

Third, ERCs may be attenuated when the period over which abnormal returns are accumulated is not properly matched with the period relative to which the unexpected earnings measure is derived. For example, suppose a firm announces its earnings on April 1st and a preceding analysts' (or consensus) forecast of these earnings made on March 1st is used as a proxy for the market's expectations. Unexpected earnings—announced earnings less the forecast—is thus measured relative to the period March 1st to April 1st. Abnormal stock returns should then be accumulated over the same period, March 1st to April 1st for the two variables, unexpected earnings and abnormal returns, to be perfectly aligned.

As to the second set of factors, those related to model specification, two issues stand out. First, cross-sectional estimation can lead to incorrect inferences about the magnitude of estimated earnings response coefficients because of the typically negative relation between firm specific earnings response coefficients and unexpected earnings variances (see Teets and Wasley 1996). If the unexpected earnings variance for any firm is approximately an inter-temporal constant, this problem does not arise in firm-specific regression.

The second model specification factor is the nonlinear relation between the absolute value of unexpected earnings and stock price changes.[6] This nonlinear relation follows because there is a negative relation between the absolute value of unexpected earnings and earnings persistence (Freeman and Tse 1992) and because transitory earnings have less value than persistent earnings

6. Freeman and Li (1999) discuss the relationship between nonlinearity, unexpected earnings variance, and the use of cross-sectional models. See below for further discussion.

(Miller and Rock 1985). Thus, the marginal stock price response to unexpected earnings (i.e., the ERC) declines as the absolute value of unexpected earnings increases. Consequently, estimation of ERCs that does not consider this relation tends to be biased downwards.

Prior research has not ignored these issues, but no study has addressed them simultaneously. For example, Imhoff and Lobo (1992) considered measurement errors in unexpected earnings hypothesized to originate from noise in financial reporting but used only a cross-sectional regression, did not match closely the abnormal returns and the unexpected earnings windows, and did not control for nonlinearity. Teets and Wasley (1996) employed firm-specific time-series regression but did not control for measurement errors in unexpected earnings, nor did they closely match the windows. Easton and Zmijewski (1989) used analysts' forecasts as a proxy for expectations, employed firm-specific time-series regressions, and attempted to align the windows (their table 2, panel B), but they did not control for measurement errors in unexpected earnings. Freeman and Tse (1992) employed nonlinear estimation. Indeed, all studies, with the exception of the latter, report implausibly small ERCs. We conjecture that incorporating the six factors mentioned above simultaneously into the analysis will lead to a substantial increase in the estimated ERCs.[7]

III. RESEARCH DESIGN

The studies reviewed above provide theoretical arguments supporting the use of control

7. Lipe et al. (1998) discuss and estimate the degree of overlap among three factors potentially explaining variations in stock returns, but not in ERCs: nonlinearity, differential information content of losses, and firm-specific coefficients. They find that each of the three factors is incrementally important in explaining stock return variation, but that firm-specific coefficient estimation has a larger effect than the other two. Yet, Lipe et al. used non-matched windows, a seasonal random walk expectation model for earnings rather than analysts' forecasts, and did not consider measurement error in unexpected earnings.

variables for nonlinearity and losses, for employing firm-specific regressions, and for using analysts' forecasts rather than time-series-based models to generate proxies for market expectations. However, none of the prior empirical studies adequately discussed or justified the choice of a proxy for the measurement error that inheres in the unexpected earnings variable. This section thus discusses the proxy we use and the theoretical justification for it, as well as other research design choices.

Proxy for Measurement Error in Unexpected Earnings

Our chosen proxy for measurement error in unexpected earnings is the cross-sectional dispersion of analysts' forecasts.8 The intuition underlying our approach can be derived from market equilibrium, as in Abarbanell et al. (1995). Using a noisy rational expectations equilibrium model with the standard assumptions, they show that the variance of measurement error created by using the mean (consensus) forecast to proxy for investors' average expectations of earnings increases in the dispersion of analysts. Abarbanell et al. also demonstrate that, even in the absence of measurement error and under the assumption that private information precision is endogenous, the earnings response coefficient increases in forecast precision, which decreases in forecast dispersion. This presents yet another basis for the hypothesis that ERCs decrease in dispersion. However, this latter explanation is not testable independently because neither the precision of private information nor the degree to which it is endogenously acquired is observable. More pointedly, regardless of the precision of private information or its endogeneity,

8 . Imhoff and Lobo (1992) also used analyst forecast dispersion as a control variable, but they employed this variable to proxy for noise in financial reporting and not for measurement error in unexpected earnings, given the reported earnings number actually observed. Understandably, therefore, they make no attempt to provide a rationale for using dispersion to proxy for measurement error in unexpected earnings.

the measurement error in using proxies is always present (average market expectations are not observable), and it always increases with dispersion. More recently, Barron, Kim, and Stevens (2000, henceforth BKS) developed a closed-form expression for the measurement error in the mean analysts' forecasts when used as proxy for investors' mean expectations, which is shown to be equal to the dispersion of analysts' forecasts divided by the number of forecasts. Hence, they conclude that forecast dispersion divided by the number of analysts' forecasts is an ideal proxy for the magnitude of measurement error when using the mean analysts' forecasts as a measure of the mean expectations of the larger set of investors who possess similar information.[9] While the two papers discussed above have used different approaches, both generally demonstrate that the estimated ERC relates to dispersion in analysts' earnings forecasts: as forecast dispersion decreases, the ERC approaches its theoretically expected value.[10]

Firm-Specific versus Pooled Time-Series Cross-Sectional Regressions

We use both firm-specific regressions and pooled time-series cross-sectional regressions. As discussed above, pooled estimation could lead to incorrect inferences about the magnitude of estimated earnings response coefficients because of the typically negative relation between firm-specific earnings response coefficients and unexpected earnings variances (see Teets and Wasley 1996). This implies that firm-specific regressions should yield more accurate ERCs. An additional reason for expecting firm-specific regressions to fare better is the potential effect of

9 . The measurement error in the BKS model reflects two distinct components. One reflects the difference between analysts' and investors' information sets. The other reflects the error resulting from sampling analysts' forecasts. The BKS model suggests the effect of these two components on the measurement errors of estimated ERCs could be substantial because these two components interact. Furthermore, their model suggests that the dispersion variable captures both components.

10. But may exceed it when measurement error is small and the supply (of security) noise is high (see BKS, p. 20).

firm-specific characteristics on the ERCs. Indeed, prior work has identified a subset of firm-specific determinants of ERCs, including growth, risk, and the persistence of earnings (see, e.g., Easton and Zmijewski 1989, and Collins and Kothari 1989).

On the other hand, Freeman and Li (1999) argue that differences between pooled-based ERCs and firm-specific-based ERCs, hypothesized by Teets and Wasley, result from a failure to control for the effect of nonlinearity. Freeman and Li further argue that pooled cross-sectional models would perform better than firm-specific estimation if nonlinearity were controlled for. Our controls for nonlinearity as well as for other potential model misspecifications (discussed below) should help distinguish between these two possible conjectures.

Close Matching of the Return Accumulation Window

Our choice of an abnormal return holding period (estimated forecast date to earnings-announcement date) reflects a trade-off. While using a short window (such as a two-day holding period) for accumulating abnormal returns minimizes the impact of confounding events, it increases the measurement error in unexpected earnings due to a mismatch between the return window and the horizon of the expected-earnings measure (see, e.g., Brown et al. 1987, and Easton and Zmijewski 1989). We conjecture that the confounding-event problem in stock returns is not as severe as the measurement error in unexpected earnings, particularly for relatively short windows spanning weeks rather than months or even years. Hence, we accumulate returns from the estimated date of the most recent earnings forecast (consensus or individual) made prior to the earnings announcement to one day after the date of the earnings announcement. This accumulation period closely matches the period over which unexpected earnings are measured and is not long enough to make the confounding-effect problem serious.[11]

Use of Analysts' Earnings Forecasts

Another research design choice concerns the selection of a proxy for expected earnings. We use analysts' earnings forecasts to improve on the measurement of market expectations of earnings relative to the use of mechanical models that generate expected earnings since the former incorporates non-earnings information about future cash flows that is presumably reflected in stock returns.[12]

Control for Nonlinearity

As mentioned, Freeman and Tse (1992) found a negative relation between the absolute value of unexpected earnings and marginal stock price response**.** They suggest as an explanation that the absolute value of unexpected earnings is negatively correlated with earnings persistence, and the latter is expected to be associated with higher ERCs. We control for this effect of nonlinearity by introducing a dummy independent variable (LIN) in our regressions reflecting ranges of the absolute value of unexpected earnings, as explained in detail in the next section.

Control for Losses

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11. The return accumulation period should arguably commence prior to the forecast estimated date because prices lead earnings (see Kothari and Sloan 1992). Note, however, that they used prior year earnings, not analysts' earnings forecasts, to proxy for expected earnings. Using analysts' forecasts should substantially alleviate, and perhaps even eliminate entirely, the problem of prices leading earnings since rational analysts are expected to incorporate information in prior stock returns in their forecasts.

12. We also estimated ERCs using "naive" market expectations of earnings (seasonal random walk). As implied by the discussion above, the results summarized in table 10 below show that models in which "naive" expectations of earnings were used yielded markedly lower ERCs than models based on analysts' consensus forecasts.

Hayn (1995) has hypothesized and shown that estimated ERCs are sensitive to the inclusion of loss firms in cross-sectional regressions; estimated ERCs were smaller when loss firms were included. We therefore control for differential effects of positive and negative earnings on ERCs by estimating ERCs separately for a sample excluding loss firms.

Potential Overlap Among the Methodological Refinements

In this section we consider the potential overlap among methodological refinements we simultaneously employ in this study. First, dispersion among analysts (DIS) likely overlaps with LIN because analysts, interpreting events that occurred during a given quarter, are likely to disagree more on the implication of these events for expected end-of-quarter earnings, the more unusual these events are, or the larger the impact they may have on the quarter's earnings per share. Consider, for example, a firm's announcement of a large contract with a customer. Analysts might reasonably disagree over how much (possibly large) gain will be recognized this quarter and how many future periods will benefit (persistence). Such events may lead simultaneously to the observation of (1) increased disagreement among analysts and (2) the possible occurrence of a large realization relative to the consensus estimate. Conversely, larger disagreement among analysts implies that some analysts forecast earnings that are widely divergent from the consensus. Consequently, it is more likely (than when the dispersion is low) that large earnings surprises (relative to the consensus) would be observed. Indeed, Daley et al. (1988) hypothesized and found a positive association between the variance of analysts' forecasts and the (absolute) magnitude of unexpected earnings. Even so, the correlation coefficient between the two is only 0.347, implying that each measure contains information not included in the other. This, in turn, suggests that each would incrementally contribute to the estimation of

ERCs.

Another potential overlap, that between LIN and the loss effect, has already been discussed by Lipe et al. (1998). As they explained, nonlinearity and losses are incrementally important. With respect to the loss effect on ERCs, Hayn's (1995) liquidation option hypothesis and Basu's (1997) conservatism hypothesis both imply $ERC_{XL} < ERC_{M}$ (where XL denotes extreme loss news and M denotes moderate news). And Freeman and Tse's (1992) nonlinearity hypothesis separately implies: $ERC_{XL} < ERC_{M}$ and $ERC_{XP} < ERC_{M}$, where XP denotes extreme gain news. Taken together, these theoretical considerations point to an overlap between the two effects; both imply $ERC_{XL} < ERC_{M}$. The overlap between LIN and the loss effect is also demonstrated empirically: both Hayn (1995, p. 142, table 6) and Lipe et al. (1998) show that, in fact, $ERC_{XL} < ERC_{XP} < ERC_{M}$.

A third potential overlap is between DIS and the loss effect. Hayn (1995), who investigated the loss effect on ERCs, has shown a positive relation between the ERC and the estimated distance between the share price implied by earnings and the firm's liquidation value (see her table 7, p. 144). If the gap is sufficiently small, the liquidation option kicks in and the ERC is attenuated. Yet, uncertainties in estimating the liquidation value and, hence, in the assessment of the potential impact of the liquidation option can be a source of disagreement among analysts. This, in turn, is likely to result in a high dispersion measure when the gap between the price and the liquidation value is small; we would expect a negative relation between dispersion and the price-liquidation value gap. In other words, the impact of extreme loss news on the ERC, and of dispersion on the ERC would overlap. Nonetheless, because of the measurement error implied by dispersion, we would expect losses and dispersion to be incrementally important, just as we expect nonlinearity and dispersion to be incrementally important.

As another potential overlap, consider close window matching and DIS. The kind of measurement error likely to be mitigated by using closely matched windows could be partially captured by our DIS measure. We conjecture that window mismatching may introduce error in the unexpected earnings measure because, when abnormal return accumulation commences later than the forecast date, news released to the market in the intervening period would change the market's expectation and render the forecasts "stale." Such staleness might be captured by DIS, as potentially stale forecasts might contribute to high DIS measures, so that the measure may reflect cross-sectional differences in the degree of "staleness" in the mean forecast. We expect, however, that DIS will contribute to estimation beyond the close matching of windows. While the staleness of the mean forecast would be mitigated in closely matched windows (relative to short windows) the remaining as-yet-unaccounted-for staleness inherent in mean forecasts that differs cross-sectionally would be reflected in the DIS measure.

The fifth potential overlap involves firm-specific regressions and nonlinearity. As Freeman and Li (1999) argue, the hypothesized superiority of firm-specific regressions as better specified than pooled regression (Teets and Wasley 1996) may be (partially?) attributable to the omission of nonlinearity in pooled regressions (such as those employed by Teets and Wasley 1996). They conclude that, based on out-of-sample prediction results, a cross-sectional nonlinear approach is most appropriate. A potential overlap thus exists between the use of firm-specific regression and the accounting for nonlinearity. Ultimately, the degree of such overlap remains an open empirical issue. We assume that there exist firm-specific characteristics beyond varying magnitudes of earnings surprises that may cause firm-specific estimation to contribute incrementally beyond the incorporation of nonlinearly in pooled regressions.

To sum up, we are mindful of commonalities underlying these factors. The primary goal of the study is not to estimate the degree of this overlap, but rather to examine whether incorporating all these factors will yield plausible ERCs, and to gauge the incremental contribution of each of these factors to the estimated ERCs.

IV. DATA

Sample Selection

Our sample spans the 15-year period, 1984-1998. Table 1 reconciles the size of our final sample of 19,664 firm-quarters (512 distinct firms) with the initial size of 206,027 firm-quarters (10,302 distinct firms). We exclude 156,361 firm-quarters (6,172 distinct firms) due to missing required data on IBES, reducing the sample size to 49,666 firm-quarters (4,130 distinct firms). 13,196 firm-quarters (1,075 distinct firms) were dropped due to missing required data on CRSP, decreasing the sample size to 36,470 firm-quarters (3,055 distinct firms). The requirement that at least 25 time-series observations be available for each distinct firm to make possible the estimation of ERCs using the time-series model further reduced the number of observations in the sample to 20,381 firm-quarters (531 distinct firms). Finally, we removed 717 firm-quarters (19 distinct firms) due to zero variation in a firm's time-series of our proxies for either the level of disagreement among market participants regarding the upcoming quarterly earnings (DIS) or nonlinearity (LIN), resulting in a final sample of 19,664 firm-quarters (512 distinct firms). A portion of the analysis was performed on a subsample of 17,335 firm-quarters (462 distinct firms). This subsample was formed by removing from our final sample firms with less than 25 quarters of data on the IBES tape with positive reported earnings and positive earnings forecasts (referred to as the positive subsample).

Variable Definitions and Descriptive Statistics

Our analysis involves calculations of unexpected earnings (SUE), disagreements among market participants regarding the upcoming quarterly earnings (DIS), a control for the nonlinear relation between abnormal returns and unexpected earnings (LIN), and cumulative abnormal returns (CARs).

Unexpected earnings of the i*th* firm in quarter *t* ($SUE_{i,t}$) are the reported earnings minus the forecasted (i.e., expected) earnings for this firm-quarter observation scaled by the stock price at the beginning of the corresponding CAR window.[13] Forecasted earnings (FE) are estimated in two ways using analysts' earnings forecasts retrieved from the IBES database. The first employs the latest consensus forecast in the period between the previous quarterly earnings announcement and the current announcement as reported in the IBES summary file. The forecast date is estimated as the middle of the month preceding the IBES "statistical date" due to publication lags; the time between the date of an analyst's forecast and the date the forecast first appears on IBES is approximately one month on average (see, e.g., O'Brien 1988, p. 59).[14] To illustrate, consider a firm reporting quarterly earnings on May 28, 1998. Referring to the timeline in panel A of Figure 1, the forecasted earnings used is the consensus estimate with a May 1998 statistical date, i.e., the one first appearing on the IBES summary tape on May 14, 1998. We, however, estimate the forecast date for the purpose of matching the return window with the unexpected earnings interval as April 15 (i.e., the middle of the month preceding the IBES statistical date.)

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13. Prices are retrieved from the CRSP daily stock file. The adjustment factors for stock dividends and stock splits as well as the actual and forecasted earnings are taken from IBES.

14. The statistical date refers to the month and year in which IBES recorded the consensus. For example, earnings forecast data with a May 1998 statistical date were recorded on the IBES summary tape on May 14, 1998. Each month IBES updates its summary tape on the Thursday preceding the third Friday of the month.

The second method uses the latest individual forecast made just before the current quarterly earnings announcement, with a forecast date estimated as the IBES estimate date, which is defined as the date on which the estimate was received by IBES (see the timeline in panel B of Figure 1). The individual forecasts are taken from the IBES detailed file. If several forecasts were reported as being made on the relevant date, we use the average.[15]

While we use both measures as proxies for the market's earnings expectations, we do not expect the individual forecast to perform as well as the consensus: those who happen to be the latest forecasters of a forthcoming earnings announcement may not be those to whom the market attaches a large weight. Indeed, articles in both the academic literature and the popular press emphasize the role of consensus forecasts; ostensibly managers manage earnings in an attempt to meet analysts' expectations, in particular the analysts' consensus earnings forecast (see Degeorge, Patel, and Zeckhauser 1999, p. 8).

As in Brown and Han (1992), disagreement among market participants regarding expected earnings of the i*th* firm in quarter *t* ($DIS_{i,t}$) is proxied for each firm- quarter by its forecasts' standard deviation quintile, computed within each quarter based on a cross-sectional ranking of the standard deviation of all the forecasts made within one month after the previous quarterly earnings announcement deflated by the number of forecasts in that period. DIS ranges between 0 (lowest quintile) and 4 (highest quintile).

LIN_{it} is a dummy variable that equals 0 if the absolute magnitude of SUE_{it} is between 0 and 0.001, 1 for magnitudes between 0.001 and 0.005, 2 for magnitudes between 0.005 and 0.01,

15. We assume that the publication lag problem is much less pronounced for individual forecasts than for the consensus forecasts and therefore use the IBES estimate date, i.e., the date IBES received the estimate from the analyst, as the forecast date. Replicating the tests that follow assuming a publication lag similar to that of the consensus forecasts yields only a marginal improvement in ERCs.

3 for magnitudes between 0.01 and 0.05, and 4 for magnitudes greater than 0.05. This variable controls for the observed S-shaped relation between abnormal returns and unexpected earnings, and its intervals are determined on the basis of the findings in Freeman and Tse (1992, table 1, panel B). In other words, in order to attain the best experimental control we use the same reported SUE intervals that they used to stratify their sample (see their discussion on pp. 193-196), rather than the straightforward quintile classification, as in the case of DIS.[16]

We obtain cumulative abnormal returns (CARs) from the CRSP NYSE/AMEX daily beta-excess-return file. This file contains daily returns for each stock in the database in excess of the daily returns on a portfolio of stocks with similar risk (i.e., same beta decile). CRSP determines risk using beta values estimated using the method developed by Scholes and Williams (1977). As Figure 1 illustrates, we compute CAR by summing the daily beta excess returns over a window that commences one day before the estimated forecast date as defined above and ends one day after the actual earnings announcement date as reported by IBES.[17]

V. TESTS AND RESULTS

Time-Series Estimation of ERCs

We estimate ERCs by firm, using the following time-series regression:

$$CAR_{i,t} = a_i + b_i{*}LIN_{i,t} + c_i{*}SUE_{i,t} + d_i{*}LIN_{i,t} * SUE_{i,t} + e_i{*}DIS_{i,t} * SUE_{i,t} + u_{i,t} \qquad (1)$$

where CAR is the sum of daily beta excess returns over a window that commences one day before the estimated forecast date as defined above and ends one day after the actual earnings

16. We also replicated the analyses that follow using quintiles (equal intervals) for LIN as in the case of DIS and obtained similar results (available from the authors upon request).

17. For the full sample, the medians of the length of the return accumulation period for the late consensus forecast and for the late individual forecast are 45 days and 15 days, respectively.

announcement date as reported by IBES, SUE is unexpected earnings scaled by stock price, LIN is a dummy variable ranging from zero (when SUE is low) to four (when SUE is high), and DIS is a dummy variable ranging from zero (when disagreement among analysts is low) to four (when disagreement is high). We include LIN as a distinct independent variable and as a SUE slope dummy because the S-shaped relation between abnormal stock returns and unexpected earnings observed by prior research (see, e.g., Freeman and Tse 1992) implies that different levels of SUE may have different intercepts.

Table 2 reports the sample medians of the parameter estimates for this time-series analysis along with their observed significance levels using the late consensus forecast as a proxy for expected earnings.[18] Panel A (B) reports results for the full sample (positive subsample). To begin, consider the results for the full sample, which consists of 512 distinct firms. There are four points to notice. First, the median ERC for model I, which contains SUE as the only explanatory variable (i.e., no controls for disagreement and nonlinearity), is 7.05. Contrasting this result with those of Easton and Zmijewski (1989, table 2, panel B) furnishes preliminary insights into the incremental effect of appropriate matching of the windows, alone. While, like us, they use time-series firm-specific regression and attempt to match the windows, they do not match the windows closely and obtain a median ERC of only 1.702. This preliminary evidence suggests that careful matching of the windows yields a threefold improvement in estimated

18. In addition to the parametric tests, we performed non-parametric tests of all regressions' estimates using the Wilcoxon Signed Rank statistic. The results of the two tests were nearly identical. For the sake of brevity, only the results of the parametric tests are tabulated. Additionally, as explained above, one important feature underlying our research design is the close matching of the windows used in computing abnormal returns and unexpected earnings. When the consensus forecast is used the CAR window begins at the middle of the month before the corresponding IBES statistical date (see the discussion in the variable definition section above) and ends one day after the actual earnings announcement date as reported by IBES.

ERCs.[19]

Second, the results for model II, which augments model I by adding LIN and SUE1*LIN as explanatory variables, show that, as expected, the effect of nonlinearity on estimated ERCs is substantial and statistically significant. The parameter estimate on SUE1 is 18.76, more than twice as large as that obtained from model I, and the estimate on SUE1*LIN, which captures the effect of nonlinearity on ERCs, is negative, –4.59, and highly significant. The estimate on LIN is positive and significant, consistent with our prediction that different levels of SUE have different intercepts. As expected, based on the observed S-shaped relation between abnormal returns and SUE, ERCs are decreasing in (the absolute value of) the earnings surprise (Freeman and Tse 1992).

Our third major finding, reflected in panel A of table 2, concerns the effects on estimated ERCs of measurement errors in unexpected earnings as proxied by our measure of the dispersion of analysts' forecasts, DIS. The results for model III show that the estimate on DIS is as expected negative and highly significant and that the ERC ranges from 11.64 when the measurement error in unexpected earnings is the lowest (i.e., DIS = 0) to 10.12 when the measurement error is the highest (i.e., DIS = 4). The ERCs for all levels of DIS are statistically significantly larger than the ERC in model I, indicating that adding DIS as an explanatory variable helps in obtaining more plausible estimated values for ERCs.

The results for model IV show that when LIN and DIS appear in the regression simultaneously the ERC is the highest of all four specifications, ranging from 22.91, when LIN = 0 and DIS = 0, to 3.55 when LIN = 4 and DIS = 4. The results also show that the DIS effect and

19. In their attempt to match the windows, Easton and Zmijewski begin their accumulation of abnormal returns from the day after the Value Lime report date. This ignores possible lags between the forecast date and the report date. Our methodology explicitly adjusts for such lags (see our discussion in the variable-definition section above).

the LIN effect are incremental to each other as the parameter estimates on both variables are, as expected, negative and statistically significant.

The results for the positive subsample (462 distinct firms) reported in panel B show that removing observations with negative reported and forecasted earnings has the most pronounced effect on the results of model I, where a highly statistically significant increase in the ERC from 7.05 to 10.70 is observed. Hayn (1995) found that the estimation of ERCs is sensitive to the inclusion of loss firms and concluded that losses are less informative than profits about the firm's future prospects. This effect of losses on estimated ERCs weakens once the control variable for nonlinearity is included; while the increase in the ERC between the two panels is over 50 percent for model I, the increases for models II and IV are statistically significant but much smaller (below 5 percent). Albeit still statistically significant, this lessening of the effect of losses on ERCs is less pronounced when DIS alone is included in the regression (cf. results for model III across panels A and B). These results demonstrate the overlap between the nonlinearity effect and the loss effect discussed above, as well as the lesser overlap between the loss effect and dispersion. At the same time, they reveal the incremental contribution of loss beyond controlling for the effects of both LIN and DIS, as indicated by the significant W-statistics. Thus, both an overlap and incremental contributions are observed.

Table 3 reports the results for the latest individual earnings forecast. Like the results for the late consensus, the estimated coefficients on LIN*SUE2 and DIS*SUE2 are negative and highly significant in all models. Specifically, using the full sample (panel A), the results for the full model show that the ERCs range from a high of 13.07 when both DIS and LIN are set to zero to a low of 2.19 when both LIN and DIS are set to 4. The corresponding range for the positive subsample (panel B) is from 14.03 to 1.63. Analogous to the late consensus results, the removal of loss firm-quarters leads to a substantial increase in the ERC for Model I and Model III. This increase is highly statistically significant as evidenced by the W-statistics. However, no significant change in ERCs is observed for Model II and Model IV. Finally, using the latest individual earnings forecast rather than the latest consensus forecast leads to consistently lower ERCs in all models. Specifically, ERCs range from 3.30 to 14.03 using the late individual forecast and from 7.05 to 24.24 using the late consensus forecast.

Overall, contrasting the results in tables 2 and 3 with findings of prior research provides preliminary insights into the importance of the incremental impact of the simultaneous use of five methodological refinements on estimating ERCs for firm-specific regressions. To gain insights into the importance of using firm-specific regressions vis-à-vis pooled firm-quarter regressions for estimating ERCs, we estimate equation (1) using data pooled over time and across firms; this is discussed in the next section.

Pooled Cross-Sectional Time-Series Estimation of ERCs

In this section we estimate ERCs by pooling the data across all sample firms and over the whole sample period, using the following regression:

$$CAR_{i,t} = a + b*LIN_{i,t} + c*SUE_{i,t} + d*LIN_{i,t} * SUE_{i,t} + e*DIS_{i,t} * SUE_{i,t} + u_{i,t} \qquad (2)$$

The variable definitions are as for equation (1). The two equations expectedly differ in that the subscript *i* is dropped from the parameters a, b, c, d, and e in equation (2).

Table 4 (5), where the latest consensus (individual) forecast is used as a proxy for earnings expectations, reports the results. Panel A (B) of each table displays the results for the full sample (positive subsample). The primary findings in panel A of table 4 corroborate the results in panel A of table 2 and highlight the importance of both control variables, LIN and DIS, for estimating ERCs. The ERC obtained from model I, which considers neither effect, is only 1.55. In contrast, model II, which considers the effect of nonlinearity on estimated ERC, generates a highly significant estimate on SUE1*LIN, indicating the importance of controlling for the nonlinearity effect. The ERCs of model II range from 16.72 when earnings surprises are the smallest (less than 0.1 percent of firm value) to 0.28 when earnings surprises are the highest (exceeding 5 percent of firm value).

The effect of disagreements among analysts (DIS) on estimated ERCs in model III, is, as expected, negative but insignificant at conventional levels.[20] The results for model IV, which considers the two effects simultaneously, show that both effects are important for estimating ERCs, as the estimates on both SUE1*LIN and SUE1*DIS have the predicted sign and are statistically significant at conventional levels.

How do the ERCs produced by the pooled regressions fare relative to those produced by the firm-specific regressions? Comparing panel A of tables 2 and 4 indicates that like prior research ERCs of the former (7.05) are substantially and significantly higher than that of the latter (1.55) when SUE is the only explanatory variable.[21] When both LIN and DIS are added to the model, however, the difference between the ERCs becomes less pronounced (22.91 vis-à-vis 17.33), but still highly statistically significant. Thus, the omission of the two variables, LIN and DIS, which has a stronger effect on pooled regressions perhaps because they vary more across

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20. Contrasting our estimated ERCs with those of Imhoff and Lobo (1992) provides insights into the incremental effect of appropriate matching of the return and unexpected earnings windows, alone. Like us, they estimate ERCs in cross section after controlling for the effect of disagreements among analysts. Unlike us, however, they mismatched the abnormal-return window and the unexpected-earnings window by cumulating abnormal returns over two-day windows but measuring unexpected earnings over one-month intervals. As a consequence, Imhoff and Lobo find an ERC of 0.077 in a pooled cross-section when disagreement is low and 0.04 when disagreement is high.

21. The statistical tests we use to compare ERCs generated by the two estimation methods, pooled and time series, are based on the T-statistic developed in Appendix A. We thank Gary Simon for help in developing this test statistic.

firms than over time, contributes to the observation of the higher ERCs yielded by the firm-specific regressions. Still, the inclusion of these two omitted variables in the regression does not fully close the gap because even for the full model the difference between the ERCs of the two alternative estimation procedures is highly significant.[22]

The results for the positive subsample reported in panel B of table 4 are consistent with the results for the full sample displayed in panel A, with two differences. First, in model III the estimate on SUE1*DIS is as expected negative and highly significant in panel B, while it is insignificant in panel A. This finding demonstrates the importance of DIS in estimating ERCs in pooled regressions as well. Second, relative to the firm-specific regressions, the removal of negative earnings observations from the sample leads to a more pronounced increase in estimated ERCs for model I. Specifically, while the former increases from 7.05 to 10.70 (approximately 52 percent), the latter increases by about 300 percent, from 1.55 to 6.22. Still, this substantial improvement in ERCs is observed only for models I and III; for models II and IV the improvement, although statistically significant, is relatively modest and not much different from the improvement observed in the firm-specific regressions. Thus, like Hayn (1995), we find that the estimation of ERCs is sensitive to the inclusion of loss firms in a cross-sectional regression, and this sensitivity is larger in the case of pooled regressions than in the case of firm-specific regressions. The effect of losses on estimated ERCs weakens once the control variable for nonlinearity is included, which again highlights the overlap between the loss effect and the nonlinearity effect discussed above.

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22. Thus, Freeman and Li's (1999) conjecture is only partially confirmed: the control for nonlinearity in the pooled cross-sectional model does not make the model superior to firm-specific estimation, although it makes the model less inferior. Arguably, other firm-specific characteristics, not explicitly controlled for in the cross-sectional estimation, play a role.

Table 5 displays the results using the latest individual earnings forecast instead of the consensus. The results for the former, like those for the latter, show that the ERCs in all models are (i) significantly higher in the positive subsample than in the full sample as indicated by the W-statistics, and (ii) significantly lower for the pooled regressions compared to firm-specific regressions as evidenced by the T-statistics. In addition, DIS*SUE2 and LIN*SUE2 are significantly negative at conventional levels in all models for both the full sample and the subsample. Finally, similar to the firm-specific results reported in tables 2 and 3, a comparison of the results in tables 4 and 5 shows that for all models the use of the latest individual forecast yields lower ERCs than the corresponding latest consensus forecast. For example, while the highest ERC in table 5 is 13.33, the highest one in table 4 is 23.97.

Does Close Window Matching Matter?

To explore the effect of closely matching the CAR window with the SUE interval vis-à-vis short CAR windows, we assess the performance of the various models considered above using a two-day return window around earnings announcements. Table 6 (7) reports the results for the firm-specific regressions (pooled time-series cross-sectional regressions). As before, panel A (B) of each table displays the results for the full sample (positive subsample). We note two points. First, the tenor of our previous results is preserved even in the presence of a substantial mismatch between the windows over which the abnormal-return variable and the unexpected-earnings variable are measured. Specifically, the ERCs of model I range from 0.52 to 2.84, and those for model IV range from 4.69 to 6.84. Second, a comparison of the magnitudes of the ERCs in tables 6 and 7 with those in tables 2 and 4, respectively, reveals that the mismatch between the two windows results in statistically significantly lower ERCs. This highlights the importance of appropriately matching the two windows.

Sensitivity Tests

Prior work exploring returns/earnings regressions has relied on a variety of data sources, sample-selection procedures, and research designs. We thus evaluate the generalizibility of our findings. To that end, we replicate the work of Easton and Zmijewski (1989; hereafter EZ), which retrieved a sample from Value Line covering the six-year period, 1975-1980, and employed firm-specific regressions, using our sample.[23] Panels A and B of table 8 report the results of replicating their table 2, panel A, as well as the effects of LIN and DIS in the context of their specification using, respectively, the full sample and the positive subsample.

Most noteworthy, even in the EZ specification the inclusion of SUE3*LIN and SUE3*DIS in the regression causes the estimates of ERCs to increase more than threefold, and the estimates on both variables are statistically significant. The results in panel B are similar to those in panel A, indicating that the exclusion of loss firms, which is associated with an approximately 10-percent reduction in sample size, does not change the inference.[24]

Finally, we consider the effect of controlling for SUE3*LIN and SUE3*DIS in the context of EZ's specification using pooled regressions (EZ do not report results using pooled regressions). The results, displayed in panels A and B of table 9, are similar to those reported in table 8: there is a substantial increase in the ERCs (up to eightfold) once the two control

23. Another difference between the two studies is that we use OLS to estimate the regression and EZ used a Swamy-random-coefficient-based approach (see their Appendix, pp. 137-140, for a discussion of this approach).

24. Note their use of the actual return as an additional explanatory variable (i.e., RET) in the regression. This variable surrogates for information released between the earnings forecast date and the date on which they begin the accumulation of abnormal returns. Our approach matches closely the abnormal return accumulation period and the period over which unexpected earnings are measured. Hence we have no need to include an actual-return explanatory variable in our models.

variables are included in the regression. This increase is statistically significant, as the estimates on both control variables are as expected negative and generally significant.

VI. DISCUSSION AND CONCLUDING REMARKS

We provide results exhibiting the degree to which controlling for factors affecting the estimation methods changes the estimated ERCs. Specifically, 1) using analysts' forecasts as proxies for expectations, 2) close matching between the windows over which abnormal returns are accumulated and the earnings surprises are measured, 3) controlling for nonlinearity, 4) controlling for errors in measuring unexpected earnings as proxied by dispersion of analysts' forecasts, 5) controlling for losses, and 6) distinguishing between firm-specific and pooled time-series cross-sectional estimation allow us to obtain ERCs that are both higher and closer to expected magnitudes.

Our results further allow us to gauge the incremental contribution of each of these factors to the improvement in the estimation of ERCs. Table 10 highlights the incremental contributions, all calibrated within the same sample of firms. Estimates based on analysts' forecasts are considerably larger than those based on naïve forecasts: under closely matched windows, for example, the difference in the estimates ranges from a low of 5.23 (full sample, firm-specific estimation with no control for nonlinearity and dispersion) to a high of 16.36 (positive only, controlling for both nonlinearity and dispersion). The comparable range in the pooled data is 0.93 to 14.86. Firm-specific estimation dominates pooled estimation in terms of the magnitude of ERCs under all three models and under both closely matched windows and non-matched windows. For example, under closely matched windows and the full model (i.e., where SUE-interactive terms with both LIN and DIS are included) the firm-specific ERC exceeds the pooled ERC by 5.58 in the full sample; the corresponding difference under the non-matched windows is 1.25. In firm-specific estimation, the incremental effect of closely matching windows under the full model is reflected in a difference in ERCs of 16.97 in the full sample and of 18.06 in the positive-only sample; the corresponding effect in the pooled estimation is 12.64 for the full sample and 17.13 in the positive-only sample. Controlling for firms with losses contributes 1.33 to the full model under firm-specific estimation and 6.64 under pooled estimation. The total effect of five factors combined (firm-specific estimation, closely matching windows, controlling for losses, controlling for nonlinearity, and controlling for dispersion) is 23.72.

Our proxy for measurement error in unexpected earnings contributes incrementally to ERCs beyond the effect of nonlinearity. For example, in firm-specific estimation, the incremental contribution over the effect of nonlinearity is 4.15 for the full sample and 4.82 for the positive-only sample when windows are closely matched, and it is, respectively, 0.74 and 0.54 when windows are not matched. In pooled estimation, the corresponding improvements are 0.61 (closely matched full sample), 2.86 (closely matched positive subsample), 0.04 (non-matched full sample), and 0.72 (non-matched positive subsample).

Overall, we see a significant increase in ERCs when employing the combination of refinements simultaneously. The resulting ERCs fall within the theoretical range of expected ERCs discussed in prior sections of this paper. Future research investigating ERCs across companies and environments is likely to yield more reliable results when employing the combination of these methodologies.

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Appendix A

Comparing pooled ERCs and firm-specific ERCs – Computing statistical significance[25]

1. Firm-specific estimation:

We have a problem in which data are collected on J firms. The index $j = 1, 2, \ldots, J$ will be used to count the firms.

For firm j a regression is done for the model $\boldsymbol{Y}_j = \boldsymbol{X}_j \boldsymbol{\beta}_j + \boldsymbol{\varepsilon}_j$. The vector $\boldsymbol{Y}_j$ is n_j-by-1, and the vector $\boldsymbol{\beta}_j$ is p-by-1. We will assume that $\boldsymbol{\varepsilon}_j \sim N(\mathbf{0}, \sigma_j^2 \mathbf{I})$.

We are interested in one particular coefficient. For the sake of notational simplicity, let us call this coefficient ω_j and call its unbiased least squares estimate w_j.

It is well known that (under the usual normal theory)

$$w_j \sim N(\omega_j, \sigma_j^2 \text{ [} (p, p) \text{ entry of } \left(\boldsymbol{X}_j' \boldsymbol{X}_j\right)^{-1} \text{]}$$

For simplicity, let $h_j =$ $[(p, p)$ entry of $\left(\boldsymbol{X}_j' \boldsymbol{X}_j\right)^{-1}]$. This quantity can be computed from the nonrandom part of the data (meaning that $\boldsymbol{Y}_j$ is not involved).

It should be pointed out that w_j is a linear combination of $\boldsymbol{Y}_j$. Assume that $w_j = \boldsymbol{a}_j' \boldsymbol{Y}_j$. We can calculate $\boldsymbol{a}_j$ directly from $\boldsymbol{X}_j$, and we can also obtain it from a simple linear combination of $\hat{\boldsymbol{\beta}}_j = \left(\boldsymbol{X}_j' \boldsymbol{X}_j\right)^{-1} \boldsymbol{X}_j' \boldsymbol{Y}_j$. The point here is that $\boldsymbol{a}_j$ is also a quantity that can be computed from the nonrandom part of the data (meaning that $\boldsymbol{Y}_j$ is not involved).

Suppose we form the null hypothesis that H_0: $\omega_1 = \omega_2 = \ldots = \omega_J$. If this hypothesis holds, then it is reasonable to estimate the common value (call it ω) by averaging the estimates $w_1, w_2, \ldots, w_J$. Let us form this estimate as

$$\overline{w} = \frac{\sum_{j=1}^{J} c_j w_j}{\sum_{j=1}^{J} c_j} \quad \text{(in our case } c_j = 1, \text{ for all } j)$$

Since $w_j = \boldsymbol{a}_j' \boldsymbol{Y}_j$, we can write the above as

25. A bold italic lower-case letter (e.g., $\boldsymbol{a}$) denotes a vector, and a bold upper-case letter (e.g., $\boldsymbol{X}$) denotes a matrix.

$$\overline{w} = \frac{\sum_{j=1}^{J} c_j \, \boldsymbol{a}_j' \, \boldsymbol{Y}_j}{\sum_{j=1}^{J} c_j}$$

If H_0 holds, then E $\overline{w}$ = ω.

2. Pooled estimation:

Now consider another way to estimate ω from the same data. Stack up the J firms so that:

$$\boldsymbol{Y} = \begin{pmatrix} \boldsymbol{Y}_1 \\ \boldsymbol{Y}_2 \\ \boldsymbol{Y}_3 \\ \vdots \\ \boldsymbol{Y}_J \end{pmatrix} \qquad \text{and also} \qquad \boldsymbol{X} = \begin{pmatrix} \boldsymbol{X}_1 \\ \boldsymbol{X}_2 \\ \boldsymbol{X}_3 \\ \vdots \\ \boldsymbol{X}_J \end{pmatrix}$$

Then form the linear model $\boldsymbol{Y} = \boldsymbol{X}\,\boldsymbol{\beta} + \boldsymbol{\varepsilon}$, where $\boldsymbol{\varepsilon} \sim N(\mathbf{0}, \tau^2\,\mathbf{I})$.

> The least squares arithmetic is completely indifferent to the value of τ^2 and to the relationship between τ^2 and $\sigma_1^2, \sigma_2^2, \ldots, \sigma_J^2$.

We can obtain the usual least squares estimate $\hat{\boldsymbol{\beta}} = \left(\boldsymbol{X}'\,\boldsymbol{X}\right)^{-1} \boldsymbol{X}'\boldsymbol{Y}$ and then obtain the usual least squares estimate for component ω as $\hat{w}$. Clearly $\hat{w}$ is a linear combination of $\boldsymbol{Y}$, which we write as $\hat{w} = \boldsymbol{g}'\,\boldsymbol{Y}$. We can obtain $\boldsymbol{g}'$ simply by picking off the appropriate row of $\left(\boldsymbol{X}'\,\boldsymbol{X}\right)^{-1} \boldsymbol{X}'$, which in our case is the second row. This matrix has p rows and $\sum_{j=1}^{J} n_j$ columns, so it is a very wide matrix. Therefore, $\boldsymbol{g}'$ is a very long vector. We can break up the entries of $\boldsymbol{g}'$ in $\mathbf{Y}$ into its first n_1 entries, its next n_2 entries, and so on. This enables us to write:

$$\hat{w} = \boldsymbol{g}'\,\boldsymbol{Y} = \sum_{j=1}^{J} \boldsymbol{g}_j'\,\boldsymbol{Y}_j \quad \text{(the } j \text{ subscripts resulting from the breakup)}$$

It is certainly clear that under H_0 we have E ($\hat{w}$) = ω.

3. Comparing firm-specific and pooled estimates:

Now consider the difference $\overline{w}$ - $\hat{w}$. Under H_0 this difference will estimate 0. What can we say about the variance of this difference? Write this difference as:

$$\overline{w} - \hat{w} = \frac{\sum_{j=1}^{J} c_j \, \boldsymbol{a}_j' \, \boldsymbol{Y}_j}{\sum_{u=1}^{J} c_u} - \sum_{j=1}^{J} \boldsymbol{g}_j' \, \boldsymbol{Y}_j \quad \text{(recall that } c_j = 1 \text{ for all } j\text{)}$$

The denominator sum in $\overline{w}$ was made to have counter *u* to avoid indexing confusion. It will be helpful to let $c_j^* = \dfrac{c_j}{\sum_{u=1}^{J} c_u}$. (in our case: c_j^* = 1/J, for all *j*)

This lets us write the step above as

$$\overline{w} - \hat{w} = \sum_{j=1}^{J} c_j^* \, \boldsymbol{a}_j' \, \boldsymbol{Y}_j - \sum_{j=1}^{J} \boldsymbol{g}_j' \, \boldsymbol{Y}_j = \sum_{j=1}^{J} \left(c_j^* \, \boldsymbol{a}_j' - \boldsymbol{g}_j' \right) \boldsymbol{Y}_j$$

We would like to test whether this takes a value that is significantly different from zero. Note that the values of $\left(c_j^* \, \boldsymbol{a}_j' - \boldsymbol{g}_j' \right)$ are known, non-random quantities. They are known because they can be computed from the $\boldsymbol{X}_j$ matrices and from the weights used to compute $\overline{w}$. They are non-random because they do not use the $\boldsymbol{Y}_j$ values.

For the sake of simplicity we assume that $\sigma_1^2 = \sigma_2^2 = \ldots = \sigma_J^2$ and use σ^2 for the common value.[26] Certainly $\tau^2 = \sigma^2$ in this case. Then

$$\text{Var}(\overline{w} - \hat{w}) = \text{Var}\left(\sum_{j=1}^{J} \left(c_j^* \, \boldsymbol{a}_j' - \boldsymbol{g}_j' \right) \boldsymbol{Y}_j \right) = \sum_{j=1}^{J} \text{Var}\Big(\left(c_j^* \, \boldsymbol{a}_j' - \boldsymbol{g}_j' \right) \boldsymbol{Y}_j \Big)$$

$$= \sum_{j=1}^{J} \Big(\left(c_j^* \, \boldsymbol{a}_j' - \boldsymbol{g}_j' \right) \text{Var}\left(\boldsymbol{Y}_j \right) \left(c_j^* \, \boldsymbol{a}_j' - \boldsymbol{g}_j' \right)' \Big)$$

26. This simplifying assumption leads to an approximate normal test rather than an exact *t* test, but it should have only a minor effect on the results.

$$= \sum_{j=1}^{J} \left(\left(c_j^* \, \boldsymbol{a}_j' - \boldsymbol{g}_j' \right) \left(\sigma^2 \, \mathbf{I}_j \right) \left(c_j^* \, \boldsymbol{a}_j' - \boldsymbol{g}_j' \right)' \right) \qquad \text{(here } \mathbf{I}_j \text{ is } n_j\text{-by-}n_j \text{)}$$

$$= \sigma^2 \sum_{j=1}^{J} \left(\left(c_j^* \, \boldsymbol{a}_j' - \boldsymbol{g}_j' \right) \left(c_j^* \, \boldsymbol{a}_j - \boldsymbol{g}_j \right) \right) \overset{\text{let}}{=} \sigma^2 \, Q$$

Now let s^2 be a chi-squared-based estimate of σ^2 with M degrees of freedom. It follows that

$$\frac{\overline{w} - \hat{w}}{s \sqrt{Q}} \sim t_M \qquad \left[\text{where } M = \sum_{j=1}^{J} \left(n_j - p \right) \right]$$

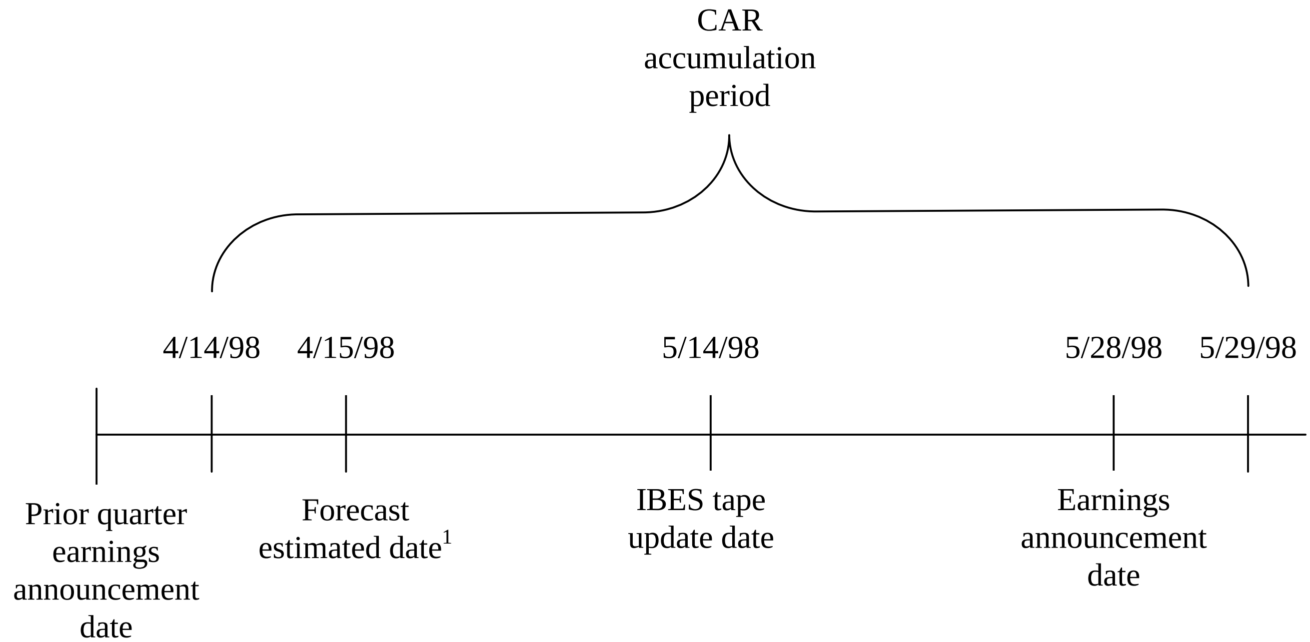
This statistic furnishes a comparison for the two different ways to estimate ω. We need only note how s^2 should be obtained. Let s_j^2 be the mean square residual from the regression involving only firm j.

Under the usual normal theory, we will have $\dfrac{\left(n_j - p \right) s_j^2}{\sigma^2} \sim \chi^2_{n_j - p}$. It follows that

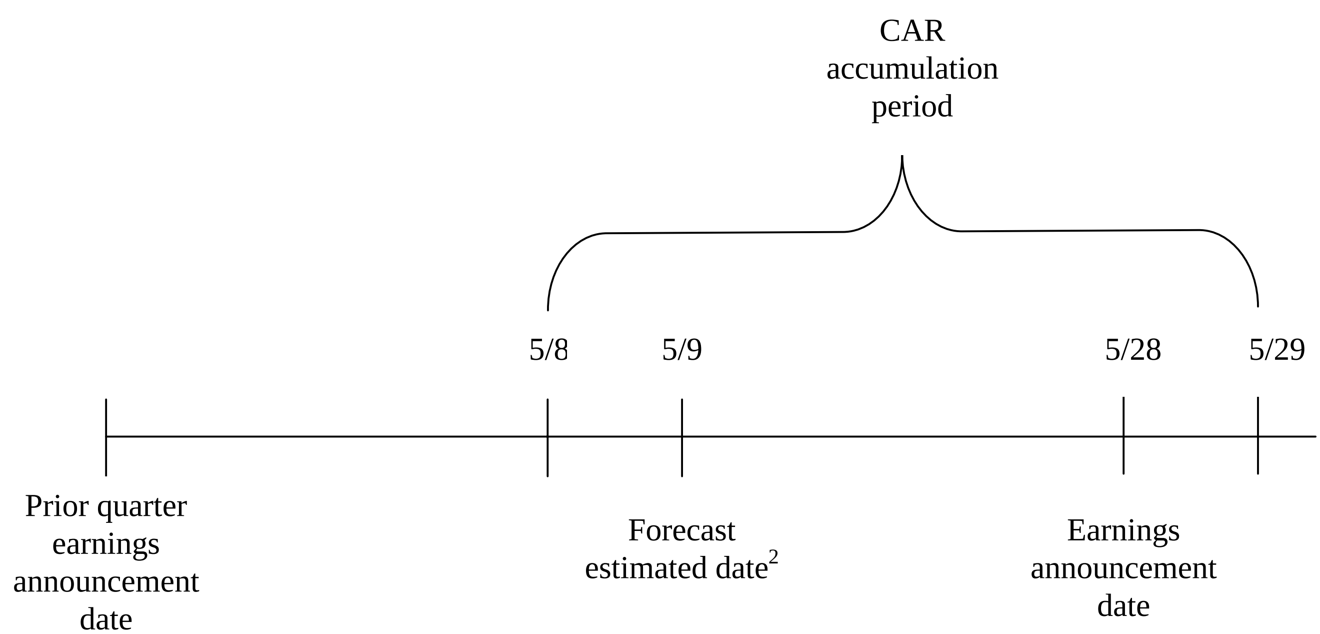
$\displaystyle\sum_{j=1}^{J} \frac{\left(n_j - p \right) s_j^2}{\sigma^2} \sim \chi^2_M$, where $M = \displaystyle\sum_{j=1}^{J} \left(n_j - p \right)$. Thus we let $s^2 = \dfrac{\sum_{j=1}^{J} s_j^2}{M}$.

Figure 1

Panel A: Consensus Forecast



Panel B: Individual Forecast



[1] Forecast estimated date is the middle of the month preceding the IBES statistical date (in this example, the IBES statistical date is 5/98).

[2] Forecast estimated date is defined as the date on which the individual estimate was received by IBES.

TABLE 1
Sample Selection

	Distinct firms	Firm quarters
[A] All observations on the IBES consensus tape	10,302	206,027
[B] Observations in [A] with no missing values for relevant data from IBES	4,130	49,666
[C] Observations in [B] with no missing values for relevant data from CRSP	3,055	36,470
[D] Observations in [C] after deleting firms with a time series of less than 25 observations	531	20,381
[E] Observations in [D] after deleting firms with perfect collinearity among the following expressions: SUE*j*, LIN, SUE*j* x LIN, and SUE*j* x DIS, where *j*=1,2,3, LIN computed based on SUE*j*.*	512	19,664

Notes to Table 1:

*This requirement is necessary in order to avoid a perfect collinearity problem in some of our firm-specific regressions.

Variable definitions:

SUE1 Standardized unexpected earnings based on late consensus forecasts. Unexpected earnings are calculated using the latest consensus forecast before the current quarterly earnings announcement as the proxy for expected earnings. The price by which unexpected earnings are deflated is the closing stock price at the beginning of the CAR1 window. SUEs are winsorized at three standard deviations.

CAR1 Cumulative abnormal returns are computed using a window that starts one trading day before the date of the latest consensus forecast after the previous quarter's earnings announcement and before the current announcement, and that ends one trading day after the actual earnings announcement date. The consensus forecast date is estimated as the midpoint (15^{th}) of the month preceding the “statistical date” reported by IBES.

SUE2 Standardized unexpected earnings based on the latest individual forecast taken from the IBES detailed tape. The IBES estimate date is used to date forecasts. Unexpected earnings are calculated using as the expected earnings proxy the latest forecast on the IBES detailed tape that lies between the previous and the current quarterly earnings announcement. An average is taken if there are multiple forecasts on the relevant date. The price by which unexpected earnings are deflated is the closing stock price at the beginning of the CAR2 window. SUEs are winsorized at three standard deviations.

CAR2 Cumulative abnormal returns computed over a window from one trading day before the latest IBES detailed tape forecast before the current quarter's earnings announcement to one trading day after the IBES-reported earnings announcement date.

SUE3 Standardized unexpected earnings based on late consensus forecasts. Unexpected earnings are calculated using the latest consensus forecast before the current quarterly earnings announcement as the proxy for expected earnings. The price by which unexpected earnings is deflated is the closing stock price at the beginning of the CAR3 window. SUEs are winsorized at three standard deviations.

CAR3 Cumulative abnormal returns are computed using a window that starts the last trading day before the current announcement and ends on the earnings announcement date (i.e., a two-day window around the earnings announcement date).

LIN This is a grouping variable based on the absolute magnitude of the appropriate SUEs *before* *winsorizing.* It equals 0 if the absolute magnitude is between 0 and 0.001, 1 for magnitudes between 0.001 and 0.005, 2 for magnitudes between 0.005 and 0.01, 3 for magnitudes between 0.01 and 0.05, and 4 for magnitudes greater than 0.05. The number of observations in each group from 0 to 4 respectively is (full sample): 9198, 7450, 1669, 1185 and 162 in the case of SUE1; 9337, 7224, 1704, 1228 and 171 in the case of SUE2; 9290, 7346, 1701, 1154 and 173 in the case of SUE3; 8588, 7794, 1865, 1274 and 143 in the case of SUE4, and 9115, 7471, 1738, 1180 and 160 in the case of SUE5. In the subsample the numbers are: 8788, 6681, 127, 583 and 8 in the SUE1 case; 8896, 6447, 1309, 668 and 15 in the SUE2 case, and 9290, 7346, 1701, 1154 and 173 in the SUE3 case.

DIS IBES standard error quintile, computed within each quarter based on a cross-sectional ranking of the standard error of all individual forecasts made within one month after the previous quarterly earnings announcement deflated by the number of individual forecasts in that period. DIS can range between 0 (lowest) and 4 (highest).

TABLE 2
Firm-specific regression (dependent variable=CAR1): median coefficient estimates and adjusted R^2 with Z-values in brackets

Panel A: Full Sample (N=512)

Explanatory variable (Predicted sign)	Model I – only SUE	Model II – LIN interaction	Model III - DIS interaction	Model IV – full model
Intercept (?)	-0.01 [-7.90***]	-0.01 [-10.31***]	-0.01 [-8.19***]	-0.01 [-10.56***]
SUE1 (+)	7.05 [37.55***]	18.76 [28.67***]	11.64 [20.70***]	22.91 [23.56***]
LIN (+)		0.01 [4.27***]		0.00 [4.27***]
SUE1 x LIN (-)		-4.59 [-17.31***]		-4.32 [-14.96***]
SUE1 x DIS (-)		--	-0.38 [-3.16***]	-0.52 [-2.54**]
Adjusted R^2	0.04	0.08	0.06	0.09

Panel B: Subsample with only positive values for actual and forecasted earnings (N=462)

Explanatory variable (Predicted sign)	Model I – only SUE	Model II – LIN interaction	Model III - DIS interaction	Model IV – full model
Intercept (?)	-0.01 [-8.09***]	-0.01 [-9.86***]	-0.01 [-8.29***]	-0.01 [-10.04****]
SUE1 (+)	10.70 [39.86***]	19.42 [25.01***]	15.15 [21.62***]	24.24 [21.48***]
LIN (+)	--	0.01 [4.22***]	--	0.01 [4.05***]
SUE1 x LIN (-)	--	--	--	-4.44 [-11.12***]
SUE1 x DIS (-)	--	-4.54 [-12.21***]	-0.19 [-2.59***]	-0.65 [-3.09***]
Adjusted R^2	0.06	0.08	0.08	0.09
W-statistic, comparing subsample and full-sample ERCs§	12.02***	4.16***	5.38***	3.98***

Notes to Table 2:

*(**, ***) significant at the 10% (5%, 1%) level two-tailed.

+The Z-statistic for a particular coefficient estimate is computed using the following formula, where *k* refers to the degrees of freedom for the t-statistic *t* corresponding to that coefficient estimate, and *N* refers to the total number of firms in the sample:

$$Z = \frac{1}{\sqrt{N}} \sum_{i=1}^{N} \frac{t_i}{\sqrt{k_i / (k_i - 2)}}$$

§A W-statistic is computed for testing whether the parameter on SUE was significantly greater for the subsample versus the full sample estimation. The statistic is obtained by aggregating firm-specific t-statistics for a test of the alternative $c_{i,p} > c_{i,n}$ in the following regression equation (or using the relevant subset of the regressors in Models I to III):

$$CAR_{i,t} = a_i + b_{i,p}*POS_{i,t}*LIN_{i,t} + b_{i,n}*(1-POS_{i,t})*LIN_{i,t} + c_{i,p}*POS_{i,t}*SUE_{i,t} + c_{i,n}*(1-POS_{i,t})*SUE_{i,t} + d_{i,p}*POS_{i,t}*LIN_{i,t}*SUE_{i,t} + d_{i,n}*(1-POS_{i,t})*LIN_{i,t}*SUE_{i,t} + e_{i,p}*POS_{i,t}*DIS_{i,t}*SUE_{i,t} + e_{i,n}*(1-POS_{i,t})*DIS_{i,t}*SUE_{i,t} + u_{i,t}$$

where *i* indexes firms, POS equals one if the observation makes it to the subsample and zero otherwise, and all other variables are as defined in equation (1) in the paper. The firms included in the estimation are those with at least 25 observations with POS=1 and at least four observations with POS=0 (71 firms).

The variable definitions of CAR1, SUE1, LIN, and DIS are given in table 1.

TABLE 3
Firm-specific regression (dependent variable=CAR2): median coefficient estimates and adjusted R^2 with Z-values in brackets

Panel A: Full Sample (N=512)

Explanatory variable (Predicted sign)	Model I – only SUE	Model II - LIN interaction	Model III – DIS interaction	Model IV – full model
Intercept (?)	-0.00 [-0.59]	-0.00 [-5.37***]	-0.00 [-1.34]	-0.00 [-5.33***]
SUE2 (+)	3.30 [29.00***]	11.08 [23.42***]	5.40 [16.44***]	13.07 [19.42***]
LIN (+)	--	0.00 [4.57***]	--	0.00 [4.29***]
SUE2 x LIN (-)	--	-2.58 [-15.11***]	--	-2.53 [-12.74***]
SUE2 x DIS (-)	--	--	-0.41 [-2.07**]	-0.19 [-1.97**]
Adjusted R^2	0.02	0.08	0.04	0.08

Panel B: Subsample with only positive values for actual and forecasted earnings (N=462)

Explanatory variable (Predicted sign)	Model I – only SUE	Model II - LIN interaction	Model III – DIS interaction	Model IV – full model
Intercept (?)	-0.00 [-1.33]	-0.00 [-4.70***]	-0.01 [-1.76]	-0.00 [-4.84***]
SUE2 (+)	5.31 [30.14***]	10.72 [20.18***]	7.79 [17.25***]	14.03 [17.76***]
LIN (+)	--	0.00 [4.25***]	--	0.00 [4.22**]
SUE2 x LIN (-)	--	-2.93 [-11.02***]	--	-2.87 [-9.86***]
SUE2 x DIS (-)	--	--	-0.51 [-2.09**]	-0.23 [-1.94*]
Adjusted R^2	0.03	0.07	0.05	0.08
W-statistic, comparing subsample and full-sample ERCs[§]	8.95***	1.14	3.62***	1.01

Notes to Table 3:

*(**,***) significant at the 10% (5%, 1%) level two-tailed.

[+]The Z-statistic for a particular coefficient estimate is computed using the following formula, where *k* refers to the degrees of freedom for the t-statistic *t* corresponding to that coefficient estimate, and *N* refers to the total number of firms in the sample:

$$Z = \frac{1}{\sqrt{N}} \sum_{i=1}^{N} \frac{t_i}{\sqrt{k_i / (k_i - 2)}}$$

[§]A W-statistic is computed for testing whether the parameter on SUE was significantly greater for the subsample than for the full-sample estimation. The statistic is obtained by aggregating firm-specific t-statistics for a test of the alternative $c_{i,p} > c_{i,n}$ in the following regression equation (or using the relevant subset of the regressors in Models I to III):

$$CAR_{i,t} = a_i + b_{i,p}*POS_{i,t}*LIN_{i,t} + b_{i,n}*(1-POS_{i,t})*LIN_{i,t} + c_{i,p}*POS_{i,t}*SUE_{i,t} + c_{i,n}*(1-POS_{i,t})*SUE_{i,t} + d_{i,p}*POS_{i,t}*LIN_{i,t}*SUE_{i,t} + d_{i,n}*(1-POS_{i,t})*LIN_{i,t}*SUE_{i,t} + e_{i,p}*POS_{i,t}*DIS_{i,t}*SUE_{i,t} + e_{i,n}*(1-POS_{i,t})*DIS_{i,t}*SUE_{i,t} + u_{i,t}$$

where *i* indexes firms, POS equals one if the observation makes it to the subsample and zero otherwise, and all other variables are as defined in equation (1) in the paper. The firms included in the estimation are those with at least 25 observations with POS=1 and at least four observations with POS=0 (71 firms).

The variable definitions of CAR2, SUE2, LIN, and DIS are given in table 1.

TABLE 4
Pooled time-series cross-sectional regression (dependent variable=CAR1): coefficient estimates and adjusted R^2 with White's corrected t-values in brackets

Panel A: Full Sample (N=19664)

Explanatory variable (Predicted sign)	Model I - only SUE	Model II – LIN interaction	Model III – DIS interaction	Model IV – full model
Intercept (?)	-0.00 [-6.01***]	-0.01 [-6.89***]	-0.00 [-6.00***]	-0.01 [-6.94***]
SUE1 (+)	1.55 [7.50***]	16.72 [28.82***]	3.18 [2.66***]	17.33 [27.32***]
LIN (+)	--	0.00 [1.57]	--	0.00 [1.66*]
SUE1 x LIN (-)	--	-4.11 [-26.77***]	--	-4.04 [-23.60***]
SUE1 x DIS (-)	--	--	-0.52 [-1.62]	-0.27 [-1.89*]
Adjusted R^2	0.02	0.09	0.03	0.09
T-statistic, comparing firm-specific with pooled ERCs	132.77***	38.67***	46.08***	28.15***

Panel B: Subsample with only positive values for actual and forecasted earnings (N=17335)

Explanatory variable (Predicted sign)	Model I - only SUE	Model II – LIN interaction	Model III – DIS interaction	Model IV – full model
Intercept (?)	-0.00 [-5.78***]	-0.01 [-7.13***]	-0.00 [-5.78***]	-0.01 [-7.34***]
SUE1 (+)	6.22 [15.62***]	21.11 [29.10***]	11.00 [8.72***]	23.97 [25.37***]
LIN (+)	--	0.00 [2.11**]	--	0.00 [2.46**]
SUE1 x LIN (-)	--	-5.84 [-18.81***]	--	-5.56 [-15.84***]
SUE1 x DIS (-)	--	--	-1.83 [-4.69***]	-1.37 [-4.38***]
Adjusted R^2	0.05	0.09	0.06	0.09
T-statistic, comparing firm-specific with pooled ERCs	106.55***	27.33***	30.31***	20.71****
W-statistic, comparing subsample and full-sample ERCs§	12.27***	6.66***	11.17***	8.91***

Notes to Table 4:

*(**,***) significant at the 10% (5%, 1%) level two-tailed.

§A W-statistic is computed for testing whether the parameter on SUE was significantly greater for the subsample than for the full-sample estimation. This is the t-statistic for a test of the alternative $c_p > c_n$ in the following regression equation (or using the relevant subset of the regressors in Models I to III):

$$CAR_{i,t} = a + b_p*POS_{i,t}*LIN_{i,t} + b_n*(1-POS_{i,t})*LIN_{i,t} + c_p*POS_{i,t}*SUE_{i,t} + c_n*(1-POS_{i,t})*SUE_{i,t} + d_p*POS_{i,t}*LIN_{i,t}*SUE_{i,t} + d_n*(1-POS_{i,t})*LIN_{i,t}*SUE_{i,t} + e_p*POS_{i,t}*DIS_{i,t}*SUE_{i,t} + e_n*(1-POS_{i,t})*DIS_{i,t}*SUE_{i,t} + u_{i,t}$$

POS equals one if the observation makes it to the subsample and otherwise zero, and all other variables are as defined in equation (1) in the paper. The firms included in the estimation are those with at least 25 observations with POS=1 and at least four observations with POS=0 (71 firms).

The variable definitions of CAR1, SUE1, LIN, and DIS are given in table 1.

TABLE 5
Pooled time-series cross-sectional regression (dependent variable=CAR2): coefficient estimates and adjusted R^2 with White's corrected t-values in brackets

Panel A: Full Sample (N=19664)

Explanatory variable (Predicted sign)	Model I – only SUE	Model II – LIN interaction	Model III – DIS interaction	Model IV – full model
Intercept (?)	0.00 [2.60***]	-0.00 [-2.27**]	0.00 [2.68***]	-0.00 [-2.40**]
SUE2 (+)	1.01 [7.16***]	9.27 [21.22***]	2.33 [2.73***]	9.67 [20.34***]
LIN (+)	--	0.00 [2.94***]	--	0.00 [3.15***]
SUE2 x LIN (-)	--	-2.23 [-18.18***]	--	-2.16 [-15.29***]
SUE2 x DIS (-)	--	--	-0.41 [-1.79*]	-0.20 [-1.64]
Adjusted R^2	0.02	0.05	0.02	0.05
T-statistic, comparing firm-specific with pooled ERCs	103.38***	32.88***	28.99***	21.16***

Panel B: Subsample with only positive values for actual and forecasted earnings (N=17335)

Explanatory variable (Predicted sign)	Model I – only SUE	Model II – LIN interaction	Model III – DIS interaction	Model IV – full model
Intercept (?)	0.00 [2.95***]	-0.00 [-2.59***]	0.00 [3.04***]	-0.00 [-2.90***]
SUE2 (+)	3.04 [13.15***]	11.59 [22.61***]	4.89 [7.47***]	13.33 [20.55***]
LIN (+)	--	0.00 [3.91***]	--	0.00 [4.40***]
SUE2 x LIN (-)	--	-3.16 [-15.93***]	--	-3.13 [-15.38***]
SUE2 x DIS (-)	--	--	-0.73 [-3.50***]	-0.72 [-4.96***]
Adjusted R^2	0.03	0.05	0.03	0.06
T-statistic for comparing firm-specific with pooled ERCs	90.33***	24.32***	25.38***	15.65***
W-statistic, comparing subsample and full-sample ERCs§	7.14***	4.23***	6.02***	5.54***

Notes to Table 5:

*(**,***) significant at the 10% (5%, 1%) level two-tailed.

§A W-statistic is computed for testing whether the parameter on SUE was significantly greater for the subsample than for the full-sample estimation. This is the t-statistic for a test of the alternative $c_{,p} > c_{,n}$ in the following regression equation (or using the relevant subset of the regressors in Models I to III):

$$CAR_{i,t} = a + b_p*POS_{i,t}*LIN_{i,t} + b_n*(1-POS_{i,t})*LIN_{i,t} + c_p*POS_{i,t}*SUE_{i,t} + c_n*(1-POS_{i,t})*SUE_{i,t} + d_p*POS_{i,t}*LIN_{i,t}*SUE_{i,t} + d_n*(1-POS_{i,t})*LIN_{i,t}*SUE_{i,t} + e_p*POS_{i,t}*DIS_{i,t}*SUE_{i,t} + e_n*(1-POS_{i,t})*DIS_{i,t}*SUE_{i,t} + u_{i,t}$$

POS equals one if the observation makes it to the subsample and otherwise zero, and all other variables are as defined in equation (1) in the paper. The firms included in the estimation are those with at least 25 observations with POS=1 and at least four observations with POS=0 (71 firms).

The variable definitions of CAR2, SUE2, LIN, and DIS are given in table 1.

TABLE 6
Firm-specific regression (dependent variable = CAR3): median coefficient estimates and adjusted R^2 with Z-values in brackets

Panel A: Full Sample (N=512)

Explanatory variable (Predicted sign)	Model I - only SUE	Model II – LIN interaction	Model III - DIS interaction	Model IV – full model
Intercept (?)	0.00 [8.32***]	0.00 [1.70*]	0.00 [8.02***]	0.00 [1.36]
SUE3 (+)	1.95 [30.93***]	5.20 [23.50***]	2.76 [15.53***]	5.94 [18.80***]
LIN (+)	--	0.00 [5.45***]	--	0.00 [5.55***]
SUE3 x LIN (-)	--	-1.18 [-13.24***]	--	-1.26 [-12.21***]
SUE3 x DIS (-)	--	--	-0.07 [-2.33**]	-0.07 [-1.29]
Adjusted R^2	0.03	0.06	0.04	0.07

Panel B: Subsample with only positive values for actual and forecasted earnings (N=462)

Explanatory variable (Predicted sign)	Model I – only SUE	Model II – LIN interaction	Model III – DIS interaction	Model IV – full model
Intercept (?)	0.00 [7.47***]	0.00 [1.69*]	0.00 [7.15***]	0.00 [1.38]
SUE3 (+)	2.84 [32.41***]	5.64 [20.51***]	4.09 [17.45***]	6.18 [17.27***
LIN (+)	--	0.00 [5.38***]	--	0.00 [5.29***]
SUE3 x LIN (-)	--	-1.21 [-9.14***]	--	-1.39 [-8.80***]
SUE3 x DIS (-)	--	--	-0.09 [-2.56**]	-0.10 [-1.35]
Adjusted R^2	0.04	0.07	0.04	0.07
W-statistic, comparing subsample and full-sample ERCs[§]	9.06***	3.18***	5.83***	3.31***

Notes to Table 6:

*(**,***) significant at the 10% (5%, 1%) level two-tailed.

[+]The Z-statistic for a particular coefficient estimate is computed using the following formula, where *k* refers to the degrees of freedom for the t-statistic *t* corresponding to that coefficient estimate, and *N* refers to the total number of firms in the sample:

$$Z = \frac{1}{\sqrt{N}} \sum_{i=1}^{N} \frac{t_i}{\sqrt{k_i / (k_i - 2)}}$$

[§]A W-statistic is computed for testing whether the parameter on SUE was significantly greater for the subsample than for the full-sample estimation. The statistic is obtained by aggregating firm-specific t-statistics for a test of the alternative $c_{i,p} > c_{i,n}$ in the following regression equation (or using the relevant subset of the regressors in Models I to III):

$$CAR_{i,t} = a_i + b_{i,p}*POS_{i,t}*LIN_{i,t} + b_{i,n}*(1-POS_{i,t})*LIN_{i,t} + c_{i,p}*POS_{i,t}*SUE_{i,t} + c_{i,n}*(1- POS_{i,t})*SUE_{i,t} + d_{i,p}*POS_{i,t}*LIN_{i,t}*SUE_{i,t} + d_{i,n}*(1-POS_{i,t})*LIN_{i,t}*SUE_{i,t} + e_{i,p}*POS_{i,t} *DIS_{i,t} * SUE_{i,t} + e_{i,n}*(1-POS_{i,t})*DIS_{i,t}*SUE_{i,t} + u_{i,t}$$

where *i* indexes firms, POS equals one if the observation makes it to the subsample and otherwise zero, and all other variables are as defined in equation (1) in the paper. The firms included in the estimation are those with at least 25 observations with POS=1 and at least four observations with POS=0 (71 firms).

The variable definitions of CAR3, SUE3, LIN, and DIS are given in table 1.

TABLE 7
Pooled time-series cross-sectional regression (dependent variable=CAR3): coefficient estimates and adjusted R^2 with White's corrected t-values in brackets

Panel A: Full Sample (N=19664)

Explanatory variable (Predicted sign)	Model I - only SUE	Model II – LIN interaction	Model III – DIS interaction	Model IV - full model
Intercept (?)	0.00 [8.51***]	0.00 [3.72***]	0.00 [8.54***]	0.00 [3.72***]
SUE3 (+)	0.52 [5.76***]	4.65 [20.50***]	0.73 [2.90***]	4.69 [16.14***]
LIN (+)	--	0.00 [2.81***]	--	0.00 [2.85***]
SUE3 x LIN (-)	--	-1.10 [-15.11***]	--	-1.10 [-15.97***]
SUE3 x DIS (-)	--	--	-0.07 [-0.88]	-0.01 [-0.27]
Adjusted R^2	0.02	0.05	0.02	0.05
T-statistic, comparing firm-specific with pooled ERCs	97.03***	28.35***	37.05***	23.03***

Panel B: Subsample with only positive values for actual and forecasted earnings (N=17335)

Explanatory variable (Predicted sign)	Model I – only SUE	Model II – LIN interaction	Model III – DIS interaction	Model IV - full model
Intercept (?)	0.00 [8.47***]	0.00 [3.88***]	0.00 [8.56***]	0.00 [3.73***]
SUE3 (+)	1.78 [12.99***]	6.12 [25.11***]	2.96 [8.03***]	6.84 [20.55***]
LIN (+)	--	0.00 [2.80***]	--	0.00 [3.14***]
SUE3 x LIN (-)	--	-1.66 [-17.85***]	--	-1.60 [-15.00***]
SUE3 x DIS (-)	--	--	-0.45 [-3.96***]	-0.33 [-3.89***]
Adjusted R^2	0.03	0.06	0.04	0.06
T-statistic, comparing firm-specific with pooled ERCs	80.30***	15.98***	26.32***	13.60***
W-statistic, comparing subsample and full-sample ERCs[§]	8.88***	4.91***	8.73***	6.60***

Notes to Table 7:

*(**,***) significant at the 10% (5%, 1%) level two-tailed.

[§]A W-statistic is computed for testing whether the parameter on SUE was significantly greater for the subsample than for the full-sample estimation. This is the t-statistic for a test of the alternative $c_{,p} > c_{,n}$ in the following regression equation (or using the relevant subset of the regressors in Models I to III):

$$CAR_{i,t} = a + b_p{*}POS_{i,t}{*}LIN_{i,t} + b_n{*}(1-POS_{i,t}){*}LIN_{i,t} + c_p{*}POS_{i,t}{*}SUE_{i,t} + c_n{*}(1-POS_{i,t}){*}SUE_{i,t} + d_p{*}POS_{i,t}{*}LIN_{i,t}{*}SUE_{i,t} + d_n{*}(1-POS_{i,t}){*}LIN_{i,t}{*}SUE_{i,t} + e_p{*}POS_{i,t}{*}DIS_{i,t}{*}SUE_{i,t} + e_n{*}(1-POS_{i,t}){*}DIS_{i,t}{*}SUE_{i,t} + u_{i,t}$$

POS equals one if the observation makes it to the subsample and otherwise zero, and all other variables are as defined in equation (1) in the paper. The firms included in the estimation are those with at least 25 observations with POS=1 and at least four observations with POS=0 (71 firms).

The variable definitions of CAR3, SUE3, LIN, and DIS are given in table 1.

TABLE 8

Firm-specific OLS estimation of Easton and Zmijewski (1989) model (dependent variable=CAR3): median coefficient estimates and adjusted R^2 with Z-values in brackets

Panel A: Full Sample (N=512)

Explanatory variable (Predicted sign)	Model I – EZ specification	Model II – LIN interaction	Model III – DIS interaction	Model IV - full model
Intercept (?)	0.00 [9.91***]	0.00 [3.74***]	0.00 [9.84***]	0.00 [3.53***]
SUE3 (+)	2.08 [32.07***]	5.87 [24.76***]	2.85 [16.34***]	6.56 [19.97***]
LIN (+)	--	0.00 [4.68***]	--	0.00 [4.87***]
SUE3 x LIN (-)	--	-1.30 [-14.02***]	--	-1.33 [-12.90***]
SUE3 x DIS (-)	--	--	-0.04 [-2.33**]	-0.10 [-1.71*]
RET (-)	-0.03 [-13.42***]	-0.04 [-14.90***]	-0.03 [-14.11***]	-0.04 [-15.34***]
Adjusted R^2	0.05	0.10	0.07	0.10

Panel B: Subsample with only positive values for actual and forecasted earnings (N=462)

Explanatory variable (Predicted sign)	Model I – EZ specification	Model II – LIN interaction	Model III - DIS interaction	Model IV – full model
Intercept (?)	0.00 [9.17***]	0.00 [3.68***]	0.00 [9.13***]	0.00 [3.48***]
SUE3 (+)	3.14 [33.97***]	6.53 [21.66***]	4.33 [18.44***]	7.09 [18.52***]
LIN (+)	--	0.00 [4.71***]	--	0.00 [4.79***]
SUE3 x LIN (-)	--	-1.39 [-9.61***]	--	-1.53 [-9.29***]
SUE3 x DIS (-)	--	--	-0.14 [-2.71**]	-0.21 [-1.89*]
RET (-)	-0.04 [-15.00***]	-0.04 [-15.24***]	-0.04 [-15.18***]	-0.04 [-15.42***]
Adjusted R^2	0.07	0.10	0.07	0.11

Notes to Table 8:

*(**,***) significant at the 10% (5%, 1%) level two-tailed.

[+]The Z-statistic for a particular coefficient estimate is computed using the following formula, where *k* refers to the degrees of freedom for the t-statistic *t* corresponding to that coefficient estimate, and *N* refers to the total number of firms in the sample:

$$Z = \frac{1}{\sqrt{N}} \sum_{i=1}^{N} \frac{t_i}{\sqrt{k_i/(k_i-2)}}$$

The variable definitions of CAR3, SUE3, LIN, and DIS are given in table 1;

RET Stock return for a firm from the trading day after the date for the consensus forecast used in calculating SUE3 through the date that is two trading days before the current quarterly earnings announcement date.

TABLE 9
Pooled time-series cross-sectional OLS regression, with control added for returns as in Easton and Zmijewski (1989) (dependent variable=CAR3): coefficient estimates and adjusted R^2 with White's corrected t-values in brackets

Panel A: Full Sample (N=19664)

Explanatory variable (Predicted sign)	Model I – EZ specification	Model II – LIN interaction	Model III – DIS interaction	Model IV – full model
Intercept (?)	0.00 [8.85***]	0.00 [5.05***]	0.00 [8.87***]	0.00 [5.06***]
SUE3 (+)	0.53 [5.89***]	4.90 [20.84***]	0.77 [2.90***]	4.95 [16.34***]
LIN (+)	--	0.00 [2.54**]	--	0.00 [2.59***]
SUE3 x LIN (-)	--	-1.16 [15.37***]	--	-1.16 [-16.16***]
SUE3 x DIS (-)	--	--	-0.07 [-0.93]	-0.02 [-0.39]
RET (-)	-0.02 [-4.23***]	-0.03 [-6.49***]	-0.02 [-4.24***]	-0.03 [-6.44***]
Adjusted R^2	0.02	0.06	0.02	0.06

Panel B: Subsample with only positive values for actual and forecasted earnings (N=17335)

Explanatory variable (Predicted sign)	Model I – EZ specification	Model II – LIN interaction	Model III – DIS interaction	Model IV – full model
Intercept (?)	0.00 [9.37***]	0.00 [5.64***]	0.00 [9.55***]	0.00 [5.52***]
SUE3 (+)	1.88 [12.95***]	6.55 [27.01***]	3.17 [8.10***]	7.36 [22.42***]
LIN (+)	--	0.00 [2.19**]	--	0.00 [2.54**]
SUE3 x LIN (-)	--	-1.78 [-19.27***]	--	-1.72 [-15.99***]
SUE3 x DIS (-)	--	--	-0.49 [-4.06***]	-0.36 [-4.22***]
RET (-)	-0.03 [-6.68***]	-0.03 [-8.62***]	-0.03 [-7.14***]	-0.04 [-8.95***]
Adjusted R^2	0.04	0.07	0.04	0.07

Notes to Table 9:

*(**,***) significant at the 10% (5%, 1%) level two-tailed.

The variable definitions of CAR3, SUE3, LIN, and DIS are given in table 1 and RET in table 8.

TABLE 10

ERCS UNDER DIFFERENT ESTIMATION METHODS

ESTIMATION METHOD	CAR1						CAR3					
	FULL SAMPLE			ONLY POSITIVE			FULL SAMPLE			ONLY POSITIVE		
	MODELS			MODELS			MODELS			MODELS		
	Plain	Lin	Full	Plain	Lin	Full	Plain	Lin	Full	Plain	Lin	Full
FIRM-SPECIFIC (ANALYST FORECASTS)	7.05	18.76	22.91	10.70	19.42	24.24	1.95	5.20	5.94	2.84	5.64	6.18
FIRM-SPECIFIC (NAIVE FORECASTS)	1.82	5.63	7.21	2.77	6.88	7.88						
LOSS EFFECT (1)	3.65	0.66	1.33				0.89	0.44	0.24			
CAR EFFECT (2)	5.10	13.56	16.97	7.86	13.78	18.06						
ANALYST EFFECT (9)	5.23	13.13	15.70	7.93	12.54	16.36						
LIN LESS PLAIN (3)		11.71			8.72			3.25			2.80	
FULL LESS LIN (4)			4.15			4.82			0.74			0.54
CROSS-SECTION (ANALYST FORECASTS)	1.55	16.72	17.33	6.22	21.11	23.97	0.52	4.65	4.69	1.78	6.12	6.84
CROSS-SECTION (NAIVE FORECASTS)	0.62	5.97	6.55	1.79	8.01	9.11						
LOSS EFFECT (1)	4.67	4.39	6.64				1.26	1.47	2.15			
CAR EFFECT (2)	1.03	12.07	12.64	4.44	14.99	17.13						
ANALYST EFFECT (9)	0.93	10.75	10.78	4.43	13.10	14.86						
LIN LESS PLAIN (3)		15.17			14.89			4.13			4.34	
FULL LESS LIN (4)			0.61			2.86			0.04			0.72
F-S LESS C-S (5)	5.50	2.04	5.58	4.48	-1.69	0.27	1.43	0.55	1.25	1.06	-0.48	-0.66
F-S,P LESS C-S,F (6)	9.15	2.70	6.91				2.32	0.99	1.49			
F-S,P,F LESS C-S,F,P (7)	22.69						5.66					
F-S,P,1 LESS C-S,F,3 (8)	23.72											

1 The excess of the ERC in the only-positive sample over the full sample.
2 The excess of the ERC in CAR1 estimation over the CAR3 estimation.
3 The incremental contribution of including the nonlinearity effect on the ERC.
4 The incremental contribution of including the dispersion measure to the ERC above and beyond the nonlinearity effects.
5 The incremental contribution of firm- specific estimation over cross-sectional estimation.
6 The incremental contribution of firm- specific estimation and controlling for losses over cross-sectional estimation without controlling for losses.
7 The incremental contribution of firm-specific estimation, controlling for losses, nonlinearity, and dispersion, over cross-sectional estimation without controlling for any of these.
8 The incremental contribution of firm-specific estimation, controlling for losses, nonlinearity and dispersion, and using closely matched windows over cross-sectional estimation without controlling for any of these and not using closely matched Windows.
9. The excess of the ERC in the estiamtion based on analyst forecasts over that based on naïve forecasts