The Effect of the Amount and Configuration of News on Inferences about Firm-Specific Events

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We examine how the amount and configuration of firm-specific news events affects inferences about the informativeness of eight types of firm-specific announcements. After establishing that confounding news events are neither infrequent nor random around these announcements, we investigate how the presence of confounding news events affects measures of announcement period market reactions. We find that the residual variation of asset pricing models (that are used to extract systematic components of returns) are largely unaffected by confounding news events. This aggregate result does not, however, extend to inferences about the informativeness of specific announcements. In particular, while conclusions about the *statistical significance* of market reactions to firm-specific announcements remain largely intact for most of the events that we consider, the *magnitudes* of those reactions (that is, their economic significance) are reliably smaller in absolute terms once we control for confounding news events.

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1. Introduction

We examine how the amount of news about firms (what we call firm-specific news events) and patterns in that news affect inferences about the informativeness of eight types of news events. The eight events we examine are chosen because they affect most firms and have been extensively studied by previous research; the eight events are analyst reports, conference calls, dividend declarations, earnings announcements, insider trades, merger and acquisition announcements, management forecasts, and seasoned equity offerings. Our aim is to provide evidence on the extent to which inferences about the informativeness of any one of the eight news events is affected by the configuration of the seven other news events. Specifically, what is the incidence of other news events in the same event window as the scrutinized news event (and what is the incidence of news events in non-event periods, including periods in which asset pricing models are estimated), and how does this incidence affect measures of market reactions to scrutinized events? We recognize that other news events that we do not consider would also be expected to affect the pattern of returns, for example, press releases announcing new products or changes in management. We chose the eight events because we believe that they are among the most important for a firm (in terms of market responses) and are also among the most analyzed in event studies.

Our analysis has implications for the design and interpretation of event studies which examine whether the market reaction to a given firm-specific news event (e.g., an earnings announcement) is reliably different from some benchmark level of expected return. Evidence of a reliable difference supports the inference that the scrutinized event is informative by virtue of its having shifted the market price of the firm that is the subject of the news event. In other words, event studies analyze whether returns during a window that contains the scrutinized event are reliably different from returns during periods that do not contain the event.[1] The components of an event study are thus the extraction of systematic (i.e., non-news) components using an asset pricing model, the calculation of the market response to a specific scrutinized event, and the measurement of expected (non-event) returns.

We consider how firm-specific news events (hereafter, news events) affect all three components of an event study analysis. In terms of the first component, we analyze whether the inclusion of news events in the estimation of asset pricing models that are used to extract systematic components of returns (so that the researcher can focus on the effects of events on returns) affects the explanatory power of and the residuals from those models sufficiently so as to call into question the estimation of the models in an event study context. We analyze the extent to which controlling for the eight types of news events influences both the R^2s of asset pricing regressions and the correlation between CAPM, 3- and 4-factor model residuals measured with and without firm-specific news days in the regression estimations. In an efficient market, both excluding news events from asset pricing regressions and controlling for news events explicitly should increase adjusted R^2s. With regard to the effect of excluding news events, we find that adjusted R^2s increase when we exclude news events from the returns used to estimate CAPM, 3-factor and 4-factor asset pricing regressions. While the increase is reliably non-zero, it is modest in economic terms – between 0.8 and 3.4 percentage points.[2] With regard to the effect of controls for these events, we find that the average adjusted explanatory power increases when we include explicit controls for news events in the asset pricing regressions, but again, the amount of the increase is modest in economic terms (under three percentage points). Finally, we find that residuals from asset pricing regressions estimated with and without news events are correlated at the 0.97 level or better. On the whole, we believe these results suggest that inferences from event studies should not be strongly affected by the number of news event days included in the estimation of asset pricing regressions.

[1] Some event studies examine whether market prices of non-announcing firms also shift. We do not examine such inter-firm information transfers.

[2] The increase in explanatory power is higher for value-weighted results than for equal-weighted results, suggesting that large firms have more information activity which gives rise to greater firm-specific price variation.

In terms of the second and third components of an event study, we focus on whether the configuration of other news events, shown by previous research to have meaningful price effects of their own, affects the inferences that can be drawn from a market reaction analysis of each of the eight news events. Inferences might be affected in any of three ways: 1) because the other news events overlap in the sense of occurring concurrent with the scrutinized event (so the event day market response is a combination response to multiple news events and cannot be uniquely attributed to the scrutinized event); 2) because the other news events interact with the scrutinized event, and possibly each other, in ways that affect the measured market reaction (so that the measured market reaction is not the sum of the market reactions to each event taken alone); or 3) because the other news events occur on non-event days (so they affect the benchmark level of expected returns).[3]

To shed light on the configuration of news events, we provide descriptive data on the extent to which each of our eight news events is confounded by (that is, overlaps with) other news events on the same day (or in a short event window centered on the event date). For example, we provide evidence on the percentage of management forecast days on which at least one of the other seven news events we consider occurs. These "confounding percentages" speak directly to the validity of an (implicit) assumption that is commonly made in event studies concerning the incidence of concurrent news events, namely, that they are either (or both) infrequent and randomly distributed across event and non-event days such that in large samples, the two effects cancel.[4] We find an unusually high incidence of other news events close in time to all of the eight news events we analyze. For example, both conference calls and management forecasts are accompanied by at least one other news event over 80% (93%) of the time for 1-day (3-day) event windows. On the issue of random distributions, we find that news events are not

[3] Because we use unsigned market reactions to assess informativeness, the expected return will not be zero by definition; however, an event study that uses signed reactions still faces the issue of whether news events affect expected returns in non-event periods and if so how much.

[4] The canceling argument is based on the measurement of event day abnormal returns. Specifically, many event studies use, as a benchmark, the average abnormal return calculated over a non-event (estimation) period. If other news events are equally likely to occur on event days and non-event days, then both the event day return and the benchmark non-event day return will be equally affected. A comparison of the event day return to the non-event day return then effectively removes the effect of other news events from the measured *relative* magnitude of the event day return. This argument assumes that the magnitude of a confounding event does not depend on the occurrence of the scrutinized event.

randomly distributed across scrutinized event and non-event days, indicating that the effects of other disclosures on non-event days are unlikely to cancel the effects of other disclosures on event days.

This configuration of overlapping and non-randomly distributed news events has a discernible effect on event study results. We estimate that the effect of other news events occurring during event windows is to increase the apparent magnitude of event window absolute one-factor model residuals by an average of 35% for a 1-day window and 36% for a 3-day window.[5] In contrast, we estimate that the effect of other news events on non-event days is to increase the non-event day absolute one-factor model residuals by an average of 23% for the 1-day window and 4% for the 3-day window. Purging both event days and non-event days of any other news events allows us to base inferences on the difference between absolute one-factor model residuals on *clean* event days (i.e., days purged of other news events) and those on *clean* non-event days (we refer to this difference as the absolute *abnormal* return on the event day). We find that eliminating the effects of other news events does not eliminate the statistical significance of the eight news events we examine. However, it does noticeably reduce their economic significance, as measured by the size of the absolute abnormal return. For example, the 1-day (3-day) absolute abnormal return to clean conference calls is 34% (44%) smaller in magnitude than the return to all conference calls.

We also conduct tests designed to shed light on the extent to which measured market reactions to scrutinized events are affected by their interaction effects with confounding events. Our tests, which consider up to three way interactions among events, show that most interaction effects are not reliably different from zero. Moreover, when interaction terms are reliably non-zero, we discern no pattern to their sign: that is, roughly half have positive coefficients (indicating that these events corroborate scrutinized events) and roughly half have negative coefficients (indicating that these events refute scrutinized events).

Taken together, we believe our results suggest that the amount and configuration of news events has its greatest effects on event study results because of overlapping (i.e., confounding) announcements coincident with, or near in time to, the scrutinized event. Although we document discernible effects of

[5] We examine absolute abnormal returns rather than signed abnormal returns because we are interested in a measure of market response rather than a directional market reaction.

news events on both the estimation of benchmark (non-event) returns and asset pricing models, as well as on their interaction with other news events, it is the overlap of news events that has the greatest effect on the economic significance of firm-specific news events, at least for the eight events we consider.

The rest of the paper is organized as follows. Section 2 discusses our analyses in relation to prior research. Section 3 describes the sample and data, and details the construction of the news event variables. Section 4 presents results pertaining to the incidence of other news events around scrutinized events, and section 5 reports the empirical analyses of whether and how confounding news affects the measured market reaction to scrutinized events. Section 6 summarizes the key findings.

2. Motivation and Contributions to Existing Literature

Our research question of how firm-specific news events affect inferences from event studies is related to two streams of capital markets research. Section 2.1 discusses research which examines the informativeness of specific information events, using the abnormal market reaction to the event as the indicator of informativeness. Section 2.2 discusses research which analyzes the explanatory power of asset pricing models.

2.1 Research on share price effects of firm-specific news events (event studies).

Finance and accounting researchers have examined the informativeness (as captured by share price responses) of numerous firm-specific news events, for example, earnings announcements, takeover announcements and conference calls with analysts.[6] A common assumption in this research is that the only item influencing the market reaction during the event window is the event being analyzed (the scrutinized event). This assumption is valid to the extent other news events during the event window are infrequent in the sample (so their effects, if any, are immaterial to the outcome) and/or randomly distributed across event and non-event days (so their effects are eliminated when the event period return is benchmarked against a measure of returns on non-event days). Our study presents evidence on both the

[6] Researchers have also measured informativeness using trading volume. Our focus in this paper is on price responses not volume responses, but our approach could be extended to tests of trading volume.

frequency and randomness of news events that occur concurrently with eight commonly scrutinized events (that is during the same event window as a scrutinized event), and examines the impact of concurrent events on inferences about the magnitude of market reactions to scrutinized events.

Previous research has examined concurrent news disclosures, but not in general with the same objective as our paper. For example, Thompson, Olsen and Dietrich [1987] examine the incidence and type of news reported in the *Wall Street Journal Index* for 2,358 firms in calendar year 1983. Although it is not the focus of their study, the authors report on the incidence of concurrent information revealed about earnings announcements, dividend announcements, accounting/corporate events, capital/ownership changes, asset changes, management-related events, labor-related events, forecasts or analysis, product-related events, financial distress, income tax-related events, and other. They show, for example, that 1,017 of the 8,807 earnings announcements made in 1983 are accompanied by dividend announcements. However, because Thompson et al. do not aggregate news disclosures across categories, it is not possible to deduce from their data what percentage of all earnings announcements are confounded by one or more other news events on the same day.

Previous research examining the market effects of concurrent news events has often focused on earnings and news that is closely related to earnings, sometimes appearing in the same press release as the earnings number. For example, Kane, Lee and Marcus [1984], Hoskins, Hughes and Ricks [1986] and Francis, Schipper and Vincent [2002] all examine the effect on share price responses to earnings announcements of other news disclosures made at the same time (or very near in time) to those announcements. [7] Kane et al. focus on earnings announcements accompanied by dividend changes for a sample of 352 quarterly earnings announcements made in 1979-1981. They document a significant corroboration effect: investors place greater weight on unexpected dividend changes when the direction of unexpected earnings news is in the same direction as the dividend change. Hoskins et al. analyze whether disclosures made on the day of or day after an earnings announcement add to earnings news in explaining

[7] We discuss these studies because their event study design is closest to the design that we use. Other studies that examine the effects of concurrent disclosures on stock returns include Griffin [1976] and Gonedes [1976].

the two day market reaction to earnings announcements. Using data from Dow Jones News Retrieval Service for 676 firms over 1979-1981 they find that contemporaneously announced earnings components, dividend increases and officer comments have significant incremental explanatory power, controlling for unexpected earnings news. Francis et al. test whether Landsman and Maydew's [2002] finding that absolute market reactions to earnings announcements have increased over time is attributable to an over-time expansion in the amount of other information disclosed in press releases announcing quarterly earnings. For a sample of 2,109 earnings announcement press releases issued by 30 randomly selected firms during 1980-1999, they find that the over-time increase in absolute abnormal returns on earnings announcement event days is driven by increases in voluntary disclosures included in these press releases. Particularly pertinent to our analysis is the inference that the other information disclosed in earnings press releases increases the perceived informativeness of earnings announcements, as measured by short-window share price responses.

In a different context, Chen, Francis and Schipper [2005] reach a similar conclusion about the measured informativeness of analyst reports. They analyze the effects of three concurrent news events (other analyst reports, earnings announcements and management earnings forecasts) on conventional measures of the informativeness of analyst reports. After removing days on which any of the three concurrent news events occurred, they report that the incidence of *insignificant* (at the 10% level) market responses to analyst reports is between 86% and 99%. For purposes of our analyses, the inference is that the presence of at least three kinds of concurrent news events has a material impact on measures of the informativeness of analyst reports.

We extend previous research on concurrent disclosures and their effects on inferences from event studies in several ways. First, we examine the frequency of concurrent disclosures using a more comprehensive set of firm-specific news events, including both firm-supplied disclosures and analyst-supplied reports, for a broad sample of firms over a significant (10-year) time period. Second, whereas Francis et al. consider only the informativeness of quarterly earnings announcements, and analyze only press releases for concurrent news events, we examine the influence of concurrent news events on the measured informativeness of eight types of scrutinized events. Third, and building on Kane et al.'s analysis of corroboration effects, we examine interaction effects among news events. Our tests allow us to comment, for example, on how the market reaction to an earnings announcement is affected if an analyst report appears and a conference call occurs on the same day as that announcement. Fourth, we consider how the amount, type and configuration of news events affects both returns calculated over event windows and returns calculated over non-event periods; the latter is important because a non-event return benchmark is often used in analyses of unsigned market reactions.

2.2 Effects of firm-specific news on the explanatory power of asset pricing regressions.

We are also concerned with how the amount (as opposed to the configuration) of news events affects event study results by virtue of their effect on the explanatory power and residuals of asset pricing regressions that are used to extract the systematic (i.e., not driven by news) component of returns. Our interest in explanatory power is related to Roll's [1988] observation that high explanatory power of an asset pricing model implies that much of the firm's variation in returns is driven by systematic risk factors not firm-specific news. This means that for a firm whose returns are largely explained by systematic risk factors (low residual variation of the asset pricing regression), a smaller in magnitude market reaction to a specific news event is more likely to be reliably non-zero. Our study of news events therefore also relates to research that examines the role of firm-specific news in explaining stock returns over longer periods chosen without regard as to whether they contain a specific scrutinized news event. This research, which began with Roll's [1988] critique, has been extended by Durnev, Morck, Yeung and Zarowin [2003] and Piotroski and Roulstone [2004].

Roll's R^2 critique showed that systematic risk factors explain little of the variation in firm-specific stock returns, even after removing days containing news events. In particular, the average R^2 of firm-specific CAPM regressions of daily returns on the market return increases from 16.3% when all daily trading data are used to 17.7% when trading days with news events are excluded.[8] He finds similar results for firm-specific arbitrage pricing theory (APT) regressions which include five factors as independent variables: the R^2 increases from 20.5% (for all daily trading data) to 22.1% (excluding trading days with news events). Roll characterizes these increases as modest, and concludes that the relatively low explanatory power of both the CAPM and APT model is not due to firm-specific news.

Durnev et al. extend Roll's work by examining whether the low explanatory power of asset pricing regressions is due to noise trading or to market efficiency in incorporating firm-specific information into share prices. They show that firms with high firm-specific price variation (as proxied by one minus the R^2 from a regression of returns on market and industry return factors) have high earnings informativeness (as proxied by the magnitude of the coefficients relating returns to future earnings and, separately, by the R^2 from the returns-earnings regression). They conclude that firm-specific price variation is attributable to firm-specific information (i.e., earnings) not to noise trading.[9] Thus, Durnev et al.'s result suggest an explanation for Roll's findings – notably, that the low explanatory power of firm-specific asset pricing regressions is due to investors' efficient incorporation of firm-specific news.[10]

We extend this research along several dimensions. First, we repeat Roll's CAPM R^2 test on a much larger sample of firms (our sample contains 14,280 distinct firms versus 96 firms in Roll's sample) over a longer and more recent time period (our sample period is 1995-2004 versus 1982-1986 in Roll's study) and we consider both firm-supplied and analyst-supplied news events. The broader sample, inclusion of analyst-supplied disclosures, and more recent time period are important for at least two

[8] Roll's analysis is conducted on 96 large firms over 1982-1986, and his news events are collected from the *Broad Tape* and the *Wall Street Journal*. He reports that information events occur on 23.7% of the sample firms' trading days over 1982-1986.

[9] Extending this line of reasoning, Piotroski and Roulstone [2004] examine how three types of market participants contribute to high firm-specific price variation. They find that analyst activities lead to lower firm-specific price variation consistent with a view that analysts facilitate the incorporation of market- and industry-wide information (as opposed to firm-specific news) into stock prices. They report the opposite for insider activities: insider trading leads to higher firm-specific price variation. For institutions they find no consistent relation between firm-specific price variation and institutional trading.

[10] However, recent work by Chan [2003] questions this market efficiency conclusion. Chan documents significantly greater momentum in stocks with public news events versus stocks without public news events. Most of the post-news drift is due to downward drift following bad news. Further, the effects are more pronounced in smaller, less liquid stocks.

reasons. First, we would expect the very large firms, which were the focus of Roll's analysis, to have more news events than other firms. Consistent with this prediction, we find that the average fraction of event days to total trading days in our sample is 14.5% compared to 23.7% in Roll's sample. Second, firm specific news events that do not originate with the firm (such as analysts' reports issued about specific firms) are a significant source of news; in particular, we find that of an average 35 news-event days per firm-year, 26 days, or 74%, are analyst report dates. The frequency of analyst reports (which do not appear on the *Broad Tape* or in the *Wall Street Journal*) suggests that the non-event days examined by Roll may very well contain news items. We include analyst reports as news-events with the intent of creating a cleaner sample of non-event days. At the same time, we recognize that we do not examine all possible types of news events, so our non-event days could contain news events that we do not examine (and which were examined by Roll). Finally, relative to Durnev et al. who use a proxy for firm-specific news (attributes of the earnings-returns relation), we consider explicit disclosures of eight types of news events that previous research has analyzed for informativeness. In addition, we incorporate the firm-specific news directly in the regressions that are used to measure firm-specific price variation, so our tests control for interaction effects between firm-specific news events.

3. Description of Sample and Data

The period of our analysis is January 1995 to December 2004 selected because the databases containing the eight news events we have chosen for analysis are available and complete for this time frame.[11] A key feature of our study is the comprehensiveness of the news events that we consider for a broad sample of firms. We chose eight types of information events: analyst reports, conference calls, dividend declarations, earnings announcements, insider trading, merger and acquisition activity, management forecasts, and seasoned equity offerings. These events have been analyzed by many

[11] We repeat our tests on periods which begin earlier (1980-2004), using databases available for this longer period (i.e., Compustat, CRSP, Zacks, SDC). Management forecast data and conference call data are not available in computer readable form prior to 1995; Thomson Financial data on insider trades begin in 1986. Results for the longer period (containing fewer databases) are similar in all respects to those we report for the 1995-2004 period.

researchers, over long periods, and have been shown to be informative (in the sense of being associated with reliably nonzero market reactions). In this section, we describe the data that we obtain for each type of news event and how we transform the data for purposes of our analysis. For events where it is possible to estimate the sign of the news conveyed without relying on the market reaction to the news itself, we describe the procedure we use to sign the news event.

3.1. Analyst reports

Data on analyst report activity are obtained by taking the union of the Zacks Research database and the FirstCall database. We combine the two databases to increase the completeness of analysts' reports covered by our sample. We retain all forecasts issued by distinct analysts about firm j on the same day, yielding a total of 5,433,145 analyst forecast observations.[12] To identify firm-specific analyst forecast activity for each day t, we set the analyst report indicator variable for firm j ($Analyst_{j,t}$) equal to one if at least one of any of five indicator variables capturing specific components of the analyst's report about firm j (quarterly EPS forecast, annual EPS forecast, long-term growth forecast, target price, stock recommendation) is one; otherwise $Analyst_{j,t}$ is set equal to zero. For the quarterly EPS forecast indicator variable ($Analyst1_{j,t}$) to equal one, we require at least one numeric quarterly EPS forecast for firm j on day t and that this forecast correspond to a fiscal quarter contained in the dataset. In total, there are 1,223,666 distinct firm-days in our sample with $Analyst1_{j,t}$ =1. Note that this variable is unsigned and is calculated at the firm-day level not at the analyst-firm-day level (i.e., multiple EPS forecasts issued on day t about firm j by different analysts are collapsed into a single event day.)

Our coding of the annual EPS forecast indicator variable ($Analyst2_{j,t}$) is similar to that for Analyst1. The final sample consists of 1,480,558 firm-days with $Analyst2_{j,t}$ =1. For the long term growth indicator variable ($Analyst3_{j,t}$) to equal one, we require that at least one analyst supply a

[12] Because FirstCall does not contain an analyst identifier (Zacks does), we develop an algorithm to distinguish between redundant and distinct forecasts issued about firm j on day t. Specifically, we delete cases where both databases have the same forecast figure for firm j for quarter q on day t.

numeric growth estimate for firm j on day t. Data on analysts' long term growth estimates are available from Zacks only. In total, 358,089 firm-days have $Analyst3_{j,t}$=1. The stock recommendation indicator variable ($Analyst4_{j,t}$) equals one on any firm-day when an analyst reported a stock recommendation. In total, 752,012 firm-days have $Analyst4_{j,t}$=1. Finally, the target price indicator variable ($Analyst5_{j,t}$) equals one if FirstCall data show a numeric target price issued about firm j by any analyst on day t. In total, $Analyst5_{j,t}$=1 for 449,089 firm-days.

Because an analyst report issued about firm j on day t may contain multiple components, the number of observations where $Analyst_{j,t}$=1 is smaller than the combined total of the five subcategories. For our sample period, $Analyst_{j,t}$=1 for 2,221,671 distinct firm-days. This sample is smaller than the original EPS forecast sample (of 5,433,145 forecasts) because we have aggregated into a single event day, days on which multiple analysts issue reports about the same firm.

The $Analyst_{j,t}$ sample (of 2,221,671 firm-days) identifies the event-day only; it does not provide information about the sign of the news conveyed on this day. Signing the nature of the news conveyed on any given event day is complicated by at least three factors: 1) the components of analysts' report do not have common units, so it is not straightforward to aggregate, say, the news in a quarterly EPS forecast with the news in a stock recommendation; 2) because analyst reports often convey information about multiple fiscal quarters, it is not straightforward to determine which fiscal quarter's news should dominate, if any; 3) multiple analysts may issue reports on the same day and it is again not obvious how to combine forecasts across analysts. Acknowledging that any procedure we use to sign the news conveyed by analyst reports issued about firm j on day t is subject to measurement and aggregation error concerns, we put forward the following procedure for determining the direction of analyst report news.

We sign the news in analyst reports using quarterly EPS forecasts because they are the most common component of those reports. We treat each quarterly forecast issued on day t as being of equal importance. For each firm-day in our sample period, we calculate daily mean forecasts for each forecasted fiscal quarter across all analysts issuing forecasts that day, $F_{j,t}^{Q}$. We also calculate a rolling consensus forecast for each forecasted fiscal quarter for each firm-day in the sample, $Consensus_{j,t}^{Q}$. The rolling consensus forecast is calculated using all forecasts for that fiscal quarter made over the prior 30 days, excluding day 0. If the number of forecasts within the prior 30-days is less than three, we base the consensus forecast on the prior 60 days. Similarly, if the number of forecasts within the prior 60 day window is less than three, we base the consensus forecast on the past 90 days. If there is at least one forecast in the 90-day window, we use the value of $Consensus_{j,t}^{Q}$ calculated over the prior 90 days; if there are no forecasts available in the 90 day window, we drop the forecast event day from the signed sample (but retain these observations in tests that use the unsigned sample).

To determine the sign of the aggregate news conveyed by all analyst reports issued about firm j on day t, we compare the grand average quarterly forecast for firm j on day t over all fiscal quarters ($F_{j,t}^{AvgQ}$) to the average forecast consensus across fiscal quarters ($Consensus_{j,t}^{AvgQ}$). We set an indicator variable capturing the sign of analyst news on each event day ($SignAnalyst_{j,t}$) equal to +1 (0, -1) if $F_{j,t}^{AvgQ}$ on day t is above (equal to, below) $Consensus_{j,t}^{AvgQ}$.[13] Application of this method to our quarterly EPS sample yields 1,177,532 firm-days with signed analyst report news events.

3.2. Conference Calls

We obtain data about conference calls initiated by firm j on day t by combining information from BestCalls.com and FirstCall. We identify 118,327 firm-days with conference calls; for each of these days, we code the conference call indicator variable ($ConfCall_{j,t}$) as having a value of one (and zero on all other days). Because we do not have access to the text of the conference call, we are not able to sign the news conveyed by conference calls.

[13] We equally-weight the forecasts as opposed to equally-weighting the fiscal quarters; results that do the opposite yield similar inferences and are not reported. We also repeat our tests using the difference between the average forecast and the consensus forecast for the earliest of all fiscal quarters forecasted. Results (not reported) are similar in all respects to those shown.

3.3. Dividend Declarations

Dividend data are obtained from CRSP and are required to be numeric. The unsigned indicator variable for the existence of a dividend declared by firm j on day t ($Div_{j,t}$) is set to one on each declaration date, and to zero otherwise. Our sample contains 135,740 firm-days where $Div_{j,t}$=1.

To sign the news conveyed by a dividend declaration event, we use the following procedure. Dividends with the same payout date declared on the same day are summed and compared to last quarter's dividends. For example, if both a regular dividend and a special dividend are declared (on the same day) for quarter q, we sum them and compare their total to quarter q-1's declared dividend. If the difference is positive (zero, negative), we code the indicator variable capturing the sign of dividend news ($SignDiv_{j,t}$) as +1 (0, -1). If dividends for different payment dates are declared on the same day, only the next dividend payment is used to determine the sign of the dividend declaration event. If no dividends were declared in quarter q-1, we drop the event day from the signed sample. In total, 135,187 dividend declaration event days could be signed.

3.4. Earnings announcements

We set the indicator variable capturing earnings announcements ($EA_{j,t}$) equal to one if data on Compustat reveal that firm j announced quarterly earnings on day t. In total, $EA_{j,t}$=1 for 257,732 firm-days in our sample. We use two procedures to sign the news conveyed by earnings announcements. The first method compares the announced earnings by firm j for quarter q ($A^Q_{j,t}$) with the rolling analyst consensus earnings forecast for firm j and quarter q ($Consensus^Q_{j,t}$, described earlier). If this difference is positive (zero, negative), we code the indicator variable capturing the sign of the earnings news ($SignEA^Q_{j,t}$) as +1 (0, -1). Because this method requires consensus analyst forecast data, it cannot be used to sign earnings news for firms without analyst following and for firms that, while followed by one or more analysts during the year, do not have a consensus analyst forecast measure for this quarter. Using this method, we are able to calculate $SignEA^{Q}_{j,t}$ for 155,976 firm-days. The second method determines the sign of the earnings news by comparing firm j's quarter q earnings to its earnings for quarter q-4. This method cannot be use to sign earnings news for firms without prior earnings data. Using the second method, we are able to sign earnings news for 251,512 firm-days. Results based on the second method are qualitatively similar to those found for the first method, so we report results only for the first (analyst-based) approach.

3.5. Insider trading

Data on trades executed by corporate insiders are taken from Thomson Financial which includes insider trades reported on SEC forms 3, 4, 5, and 144. Corporate insiders are defined as all persons with "access to non-public, material, insider information." We restrict our insider trading indicator variable ($Insider_{j,t}$) to transactions involving private stock purchases or private stock sales, i.e., we exclude grants, exercises, and other similar transactions. We also do not distinguish news events based on the insider's position in the firm (for example, we include purchases and sales by the CEO, president, beneficial owners and directors). We code $Insider_{j,t}$ as one if any insider trades for firm j are *filed* on day t, and as zero otherwise. The sample of event days where $Insider_{j,t}=1$ consists of 367,121 firm-days.

The signed version of $Insider_{j,t}$ ($SignInsider_{j,t}$) is measured using the average number of shares traded, with purchased shares treated as positive numbers and sold shares treated as negative numbers. If the average number of firm j's shares traded by insiders on day t is positive (negative), we set $SignInsider_{j,t}$ equal to +1 (-1). In total, there are 366,704 firm-days with signed values of insider trades. (The difference in sample size, 367,121 versus 366,604, is due to the absence of data on number of shares traded.)

3.6. Merger and acquisition activity

Data on merger and acquisition activity are obtained from the Securities Data Corporation (SDC) database. We exclude cross-border transactions and transactions with denominations not in US dollars.

We set the M&A indicator variable ($M\ \&\ A_{j,t}$) equal to one for firm j on day t if on day t there is an announcement, a significant update, or the withdrawal of a bid in which firm j is either the target or the bidder. If the immediate or ultimate parent company of the target or the bidder is known and publicly listed, $M\ \&\ A_{j,t}$ is also set equal to one for the parent company on day t. On all other days, we set $M\ \&\ A_{j,t}$=0. In total, we identify 111,311 firm-days with M&A news events. We do not create a signed measure of M&A activity.

3.7. Management EPS forecasts

Management forecast data are collected from the FirstCall database. To be included in our sample, the management forecast must be numeric. If a range forecast is provided, we measure the management forecast as the average of the range. We assign the indicator variable capturing a management forecast news event ($MF_{j,t}^{Q}$) the value of one if firm j reports either a point estimate or a range forecast on day t; otherwise, $MF_{j,t}^{Q}$=0. In total, we identify 22,110 firm-days where $MF_{j,t}^{Q}$ =1.

To sign the news in a management forecast, we follow the same procedure as previously described for analysts' earnings forecasts. Specifically, we compare the management forecast of earnings for quarter q (or for year T) to the rolling analyst consensus earnings forecast for that same quarter (or year), $Consensus_{j,t}^{Q}$. If $MF_{j,t}^{Q} > Consensus_{j,t}^{Q}$, we code the forecast as conveying positive news and assign a value of +1 to $SignMF_{j,t}^{Q} = MF_{j,t}^{Q} - Consensus_{j,t}^{Q}$. If $MF_{j,t}^{Q} < Consensus_{j,t}^{Q}$, we set $SignMF_{j,t}^{Q} = -1$; finally, if $MF_{j,t}^{Q} = Consensus_{j,t}^{Q}$, we set $SignMF_{j,t}^{Q} = 0$. The final sample of management forecasts with signed news consists of 17,876 firm-days.

3.8. Seasoned equity offerings

Data on seasoned equity offerings (SEOs) are obtained from Securities Data Corporation. We require that the issuer nation be the United States and that the issue be denominated in US dollars. Penny stocks (with an offering price less than $1) are excluded. Following the initial public offering literature, we exclude REITs and closed-end funds (SIC codes 6726 and 6798). Because announcement dates for SEOs are not available on the SDC database, the indicator variable capturing SEO activity ($SEO_{j,t}$) is set to one for all *issue* dates for issuing firm j (and the parent of issuing firm j if the parent is a public company). For all other dates, $SEO_{j,t}$ =0. The final sample consists of 3,529 firm-days with $SEO_{j,t}$ =1. Like M&A activity, we do not develop a signed measure of SEO activity.

4. Frequency of concurrent events in event study settings

For each firm in our sample, we merge daily stock return data for the period January 1, 1995 to December 31, 2004 (or a shorter period, if the firm is not traded over this entire ten year period) with the indicator variables created for the eight types of news events described in section 3. In this section, we report descriptive statistics on configurations of concurrent news events for each type of scrutinized event. In particular, we analyze the extent to which each of eight types of news events overlaps with, and therefore is confounded by, the other seven types of news events. We consider two approaches to determine whether firm j's news event of type *k* overlaps with another news event that also pertains to firm j.[14] The first defines a concurrent (confounding) news event as one that occurs on *the same day* as news event *k*; the second defines a confounding news event as one that occurs *in the 3-day window* centered on the announcement date of news event *k*. The first approach identifies fewer concurrent news events than the second approach, so tests using the first approach are less likely to find evidence that confounding news events are frequent and affect market reactions to disclosure *k*. We include the second approach because it is based on an event window that is often used in event studies; hence, if the market reaction to disclosure *k* over a 3-day window is of interest, then the tests for determining whether there are confounding events should be based on the longer 3-day window.

Results for the first (1-day window) approach are reported in Panel A of Table 1; results using the second (3-day window) approach are reported in Panel B. Specifically, the event day (day 0) in Panel A is

[14] It is also possible that firm j's news release of type *k* is confounded by news releases made by other firms on the same day. We do not consider such inter-firm transfers of information in this study.

defined as confounded when another news event occurs on day 0; in Panel B, the event day is labeled as confounded when another news event occurs on day -1, 0, or +1. To ensure that the percentages (summing over all other disclosures) sum to 100%, multiple confounding events in the same category (i.e., multiple analyst reports) count are counted as one confounding event.

In determining whether scrutinized event *k* is confounded by another event in a 3-day window, we apply the 3-day window to only the scrutinized event; we do not apply it to the confounding event(s). To illustrate, suppose that the scrutinized event occurred on Tuesday of a week with five trading days. Then the 3-day event window consists of Monday (day -1), Tuesday (day 0) and Wednesday (day +1). For the 3-day window, we say this scrutinized event is confounded if the release of another news event occurred on any of these three days. We do not consider the scrutinized event confounded if, for example, another news event occurred on Thursday, even though a 3-day window centered on Thursday overlaps with the 3-day window centered on the Tuesday scrutinized event. This approach essentially assumes that investors rapidly impound other news events into stock prices (i.e., in a single day), while allowing investors more time (3-days) to impound the information in the scrutinized event. We opt for this asymmetry because it biases against finding a large number of confounding events and therefore, biases against finding that such events influence the perceived market reaction to the scrutinized event.

Table 1 provides descriptive statistics on the extent of confounding events (columns), by each scrutinized event (rows). Across both panels, percentages are calculated by rows; for example, the first row of Panel A indicates that 2.98% of analyst report event days are confounded by conference calls made on the same day as the analyst report; the second row of Panel A indicates that 55.95% of all conference call event days are confounded by analyst reports on the same day. The rightmost column of Table 1 is a summary calculation that shows the number and percentage of event days that are confounded by one or more concurrent news events.

Table 1 reveals substantial variation in the extent of confounding events across scrutinized events. Using the summary measure in the rightmost column and focusing on the 1-day event window (Panel A), we find that analyst reports are the least confounded, with 9.61% having a concurrent news event. In contrast, the most confounded events are management forecasts and conference calls where at least one other concurrent news event occurs over 80% of the time. Moving across the types of confounding events, we see that analyst reports are the most likely confounding event for all scrutinized events. This result is not surprising given that analyst reports are by far the most common event, with a total of 2,221,671 event days in the sample. The *least* likely confounding event is SEO activity, where confounding percentages are less than a tenth of a percent for all disclosure types; note that SEO activity is also the least common scrutinized event in the sample, with only 3,529 event days.

Generally speaking, the extent to which a given type of news event occurs concurrent with, and therefore confounds, a scrutinized event is a function of frequency of occurrence – the more frequent is a type of news event, the more likely it confounds other events. However, there is also evidence of clustering among news events, with the common factor (possibly) being earnings. In particular, management forecasts are relatively rare news events, with only 22,110 event days in the ten year sample period. However, management forecasts confound 8.70% of conference calls and 5.01% of earnings announcements. Subsequent analyses examine whether these percentages differ from chance.

Inspection of the results in Panel B shows that, as expected, there are more concurrent news events (and therefore more confounding) for 3-day event windows. In particular, Panel B shows that other news events confound conference calls and management forecasts over 90% of the time (93% for conference calls and 94% for management forecasts), earnings announcements 64% of the time, and M&A and SEO events about 57% of the time. Dividend events and insider trades are confounded about one-third of the time, and analyst reports are confounded 24% of the time.

These results, which show high percentages of overlapping news events in 3-day windows, call into question an implicit assumption in event studies that ignore the possible effects overlapping news events. The implicit assumption is that these other news events are some combination of infrequent and/or randomly distributed across calendar days (the latter implying that, over large samples, the effects will be immaterial). However, the results in Table 1 suggest that overlapping news events even in the shortest (1-day) event windows are very common; in fact, in the case of conference calls and management forecasts, such overlaps are to be *expected*. When the event window is widened to 3-days, the fraction of event days with concurrent news events increases. In some cases, this increase is substantial: for example, confounding percentages for analyst reports, insider trades, and SEO activity more than double.

To provide formal evidence on the non-randomness of confounding events, we perform two tests; for brevity, we describe but do not tabulate the results of these tests. The first is a *non-directional* chi-square test by event category, which tests if the frequency of (any) confounding events on scrutinized event days is *different* from their frequency in the overall sample of trading days. For all scrutinized events, the resulting chi-squares statistics are significant at the 0.0001 level. The second test examines the *directional* prediction that the observed frequency of confounding events is *higher* on scrutinized event days than expected. The statistic here is a signed z-test for a population proportion (the z-score approximately follows a standard normal distribution for large samples such as ours). For all scrutinized events, we find that ratio of the observed frequency of confounding events to the expected frequency of confounding events is greater than one (the ratio ranges between 1.35 and 5.89), with all z-statistics significant at the 0.0001 level. These findings indicate that confounding events are not randomly distributed across event and non-event days; rather, there is a significantly greater likelihood of observing confounding events on scrutinized event days.

Table 2 presents the same type of information as Table 1, on a firm-year basis. These firm-year results provide a more intuitive feel for the data and provide a context for our subsequent tests that are conducted on a firm-year basis. The first column in Table 2 shows the average number of all event days during the firm-year. Turning to the last row in each panel, these data indicate that on average, a firm has one or more events on 34.58 trading days per year. Analyst reports (with 26.20 event days per year) dominate this average, followed by insider trading announcements (4.49 days) and earnings announcements (3.05 days). In contrast, management forecasts (0.3 days) and SEO announcements (0.04) are relatively rare events.

The second and third columns of Table 2 break down the average number of event days into "clean event days" and "confounded event days". We define a clean event day as an event day that has no concurrent news events and we define a confounded event day as one with one or more other news events. The breakdown between confounded and clean event days follows the same relative frequencies presented in the rightmost column of Table 1. Specifically, the percentages of confounded event days (reported in Table 2) are qualitatively similar to the percentage of event days with any confounding event (reported in Table 1).[15] Again, we report results using both a 1-day window for the scrutinized event (Panel A) and a 3-day window (Panel B). Similar to the findings in Table 1, the data show that the 3-day window results in fewer clean event days than does the 1-day event window (because of the greater likelihood of identifying another news event in a 3-day window versus a 1-day window). For example, whereas there are 1.62 clean earnings announcements per firm, on average, using a 1-day window, there are only 1.08 clean earnings announcements per firm using a 3-day window (or a decline of about one-third).

Table 2 also presents information about clean and confounded *non-event* days; we define these terms shortly. We investigate the effect of news events on so-called non-event days because confounding news events affect not only inferences about the market effects of scrutinized events, but also the benchmark for establishing whether those market reactions meaningfully differ from share price changes that would occur in the absence of the scrutinized events. In particular, it is common in event studies to identify the scrutinized event days (for example, to identify earnings announcement dates) and to assume that the mean or median market-adjusted stock return for the announcing firm over all other (non-event) trading days is an appropriate benchmark for establishing the magnitude and significance of the market reaction to the earnings announcement. This approach assumes that no news events occur on non-event days, or if they do, they have no meaningful effect on the announcing firm's stock returns on those days. While intuition suggests that some news events likely occur on so-called non-event days we believe that

[15] The percentages are similar but not exact because Table 2 counts the incidence of events at the firm-year level, then averages these frequencies by firm-year, then by year, and then over the 10 year sample period. (This method yields results that are comparable to the Fama-MacBeth tests of inference that are used to evaluate abnormal returns in subsequent tests.) In contrast, the percentages in Table 1 reflect simple averages (i.e., equal weighting of all events).

our study is the first to document the extent of occurrence and to measure their influence on market reactions to scrutinized news events.

The three columns in Table 2, labeled "Non-Event Days," present information pertaining to non-event days and the extent to which a non-event day is a day on which a (non-scrutinized) news event occurs. We term the latter a "confounded" non-event day, to distinguish it from a "clean" non-event day on which no news event (of the eight we consider) occurs. For our purposes, a non-event day is characterized by the absence of the scrutinized event indicated by a row in the table. For example, the *Analyst* row (in panel A or panel B) indicates that, on average, each sample firm had 211.48 days during the year with no analyst report news events. While these 211.48 days are "non-events" relative to analyst report activity, they may not be non-events relative to other news events. The remaining two columns show the percentage of non-event days where no other event occurred ("clean non-event days") and where at least one other type of event occurred ("confounded non-event days"). For analyst reports, there is a relatively small incidence of confounding events; only 8.38 non-event days, or less than 4% of all non-event days, have news events. For all other event categories, however, non-event days are significantly more contaminated: specifically, between 13% and 15% of all non-event days are confounded by other news events.[16]

Overall, we believe the results in Tables 1 and 2 indicate that confounding events are both more common and less random than would be suggested by assumptions typically made in event studies. The degree of confounding is most severe for management forecasts and conference calls, with over 80% (90%) of these scrutinized events overlapping with, and therefore confounded by, other news events in a 1-day (3-day) window. Analyst reports are the least confounded (about 10% of the time for a 1-day event window and 24% for a 3-day window). For non-event days, the confounding issue tends to be less severe

[16] Data on the number of clean and confounded non-event days is the same in panels A and B. This is because the number of confounded non-event days is invariant to the length of the event window (1-day versus 3-days), due to the fact that we apply the 3-day window only to the scrutinized event, not to the other news events. Other news events are evaluated using a 1-day window.

for all information categories, with a range of 4% (for analyst forecasts) to 13-15% for other information events.

5. *Effect of confounding events on perceived market reactions to scrutinized events*

Our remaining tests examine the influence of confounding events on inferences drawn from the market reactions to the scrutinized events. We examine three avenues through which other news events might affect measures of market reactions to scrutinized events: 1) the role that news days play in the estimation of the asset pricing model that underlies all event study tests; 2) the role of news events in measures of benchmark, i.e., non-event, expected returns; and 3) the role of non-scrutinized news events that overlap with the scrutinized news event in affecting inferences about the informativeness of the scrutinized event. We evaluate different ways of analyzing these issues and addressing them in an event study context; for example, excluding confounding events entirely or retaining them and controlling for their information content. Because it is not possible (in some cases) or not fruitful (in other cases) to treat each of these three avenues separately, our tests sometimes combine two of the avenues.

5.1. The influence of news events on the estimation of asset pricing models

We begin by examining the role that news events play in affecting the firm-specific variation associated with the asset pricing regressions that are commonly used to calculate the residuals examined in event studies. For these tests, we require a firm-year to have at least 50 trading days to estimate an asset pricing regression, and we require that the firm have at least one event day and one non-event day in the same year (the latter is a non-binding constraint).[17] To calculate firm-specific price variation, we estimate 1-factor (CAPM), 3-factor and 4-factor pricing regressions for the 14,280 firms with at least 50 daily returns over 1995-2004:

[17] The requirement that each firm has at least one event day facilitates a within-firm design, which is important because firms have different returns volatilities even absent information events. Such non-information related volatility will translate into different non-event day *absolute* market adjusted returns ($\left|R_{j,t}^{MktAdj}\right|$) which are the focus of some of our subsequent tests. We also perform across-firm tests which do not require that a firm have both an event day and a non-event day to be included in the sample. Results are similar and are not reported.

$$R_{j,t} - R_{F,t} = \alpha_j^{CAPM} + \beta_j RMRF_t + \varepsilon_{j,t}^{CAPM} \tag{1}$$

$$R_{j,t} - R_{F,t} = \alpha_j^{3f} + b_j^{3f} RMRF_t + s_j^{3f} SMB_t + h_j^{3f} HML_t + \varepsilon_{j,t}^{3f} \tag{2}$$

$$R_{j,t} - R_{F,t} = \alpha_j^{4f} + b_j^{4f} RMRF_t + s_j^{4f} SMB_t + h_j^{4f} HML_t + m_j^{4f} MOM_t + \varepsilon_{j,t}^{4f} \tag{3}$$

where $R_{j,t}$ is the daily return for security j, $R_{F,t}$ is the daily risk-free rate, $RMRF_t$ is the daily excess market return, SMB_t and HML_t are the daily returns of Fama and French's [1993] factor-mimicking portfolios for size and book-to-market, respectively, and MOM_t is a factor-mimicking portfolio designed to capture returns momentum (Carhart [1997]).[18] We include single-factor and multi-factor models because we wish to be agnostic about the 'correct' asset pricing model, and because we want to capture factors that have been shown to cause systematic co-movements in returns, which by definition are unlikely to be caused by firm-specific information events. Roll [1988] makes a similar argument for investigating multi-factor (arbitrage pricing theory, APT) models.

Table 3, Panel A, shows the equal-weighted (across firms) average adjusted R^2 for each model when estimated using all trading days in a firm-year and the average equal-weighted adjusted R^2 when estimated on non-information days only. Consistent with Roll, we find that the explanatory power of the CAPM is significantly smaller when information days are included in the estimation: the average adjusted R^2 is 6.74% for all days versus 7.50% excluding event days (the t-statistic for the difference is 72.66). Similar results are found for the 3-factor and 4-factor models, where the mean adjusted R^2 for all days is about 0.90 percentage points smaller than it is when event days are excluded (t-statistics for differences are about 77).

We note that the average adjusted R^2s that we document are smaller than those reported by Roll, who documents an average adjusted R^2 for the CAPM of 16.3% for all days versus 17.7% for non-event days. The higher explanatory power reported by Roll is due to the inclusion of small firms in our sample. To see this, Panel A also reports value-weighted results where each firm's adjusted R^2 for year t is weighted by the firm's average market capitalization in year t. Essentially, the value-weighting gives

[18] Data on risk-free rates, market returns and factor returns are from K. French, as available on WRDS.

greater weight to large firms, and as such, the value-weighted results are more comparable to Roll's investigation which was conducted on 96 large firms. As is evident from these data, the value-weighted explanatory power of all models is substantially higher than the equal-weighted values. For example, using all event days, the value-weighted adjusted R^2 is between 24% and 29%, compared to 27% and 32% for tests that use only non-event days; the differences, of roughly three percentage points, are reliably different from zero (t-statistics exceed 120).

Overall, we find the same pattern of results for our sample of 14,280 firms over 1995-2004 as documented by Roll for 96 firms for 1982-1986. We also draw the same inference: while adjusted R^2s are larger when event days are excluded from all trading days, the increase in explanatory power is modest.

An alternative way of characterizing the extent to which event days effect the underlie asset pricing model is to incorporate the returns on event days explicitly into the regression. We do this by augmenting equations (3), (4) and (5) with the event variables developed in section 3. Whenever it is possible to sign the event variable, we use the signed form of the variable rather than the unsigned form. This means, for example, that the signed event variable for analyst reports ($SignAnalyst_{j,t}$, which is based on the sign of quarterly earnings forecast news) replaces the unsigned analyst report indicator variable ($Analyst_{j,t}$) whenever $SignAnalyst_{j,t}$ exists for that event day. On all other days with analyst report activity (i.e., days with unsigned quarterly earnings forecasts or days with only annual forecasts, long-term growth forecasts, target prices or stock recommendations), the unsigned variable ($Analyst_{j,t}$) is used. In addition, because much empirical work shows that market reactions to good news and bad news differ, we include separate indicator variables for good news and bad news (for each type of news where we have a signed event variable). The resulting augmented regressions take the following form:

$$R_{j,t} - R_{F,t} = \alpha_j^{CAPM} + \beta_j RMRF_t + \sum_k \rho^k k_{j,t} + \varepsilon_{j,t}^{CAPM} \tag{4}$$

$$R_{j,t} - R_{F,t} = \alpha_j^{3f} + \beta_j^{3f} RMRF_t + s_j^{3f} SMB_t + h_j^{3f} HML_t + \sum_k \rho^k k_{j,t} + \varepsilon_{j,t}^{CAPM} \tag{5}$$

$$R_{j,t} - R_{F,t} = \alpha_j^{4f} + \beta_j^{4f} RMRF_t + s_j^{4f} SMB_t + h_j^{4f} HML_t + m_j^{4f} MOM_t + \sum_k \rho^k k_{j,t} + \varepsilon_{j,t}^{CAPM} \tag{6}$$

where $k \in \left\{ \begin{array}{c} \begin{pmatrix} Analyst_{j,t} \\ SignAnalyst_{j,t} \end{pmatrix}, ConfCall_{j,t}, \begin{pmatrix} Div_{j,t} \\ SignDiv_{j,t} \end{pmatrix}, \begin{pmatrix} EA_{j,t} \\ SignEA_{j,t} \end{pmatrix}, \begin{pmatrix} Insider_{j,t} \\ SignInsider_{j,t} \end{pmatrix}, \\ M\ \&\ A_{j,t}, \begin{pmatrix} MF_{j,t}^Q \\ SignMF_{j,t}^Q \end{pmatrix}, SEO_{j,t} \end{array} \right\}$

Panel B compares the average adjusted R^2s from the pure asset pricing regressions (these are identical to the average adjusted R^2s reported in the "All days" column in Panel A) with the adjusted R^2s from equations (4)-(6). We report results for equal-weighting and value-weighting of adjusted R^2s across firms. The increment in explanatory power from the inclusion of the event variables is about 2.20 percentage points for the equal-weighed results (from 6.7%-8.9% to 9.0%-11.1%), with t-statistics for this difference of about 96. For value-weighting, the increase in explanatory power is about 2.60 percentage points (from 24.2%-29.0% to 26.8%-31.6%), with t-statistics of about 142. While the increments are statistically significant, the increases are small in economic terms.

Perhaps of more relevance for event study tests than explained variation is the effect on the residuals themselves from including days with firm-specific news in the estimation of the asset pricing regressions. To provide evidence about this effect, we report the average correlation between the residuals (both signed and absolute) obtained from each asset pricing regression, estimated with and without returns on days when there are news events. Results are shown in Panel C of Table 3; we report only equal-weighted results because we draw similar conclusions from value-weighted results. For signed (absolute) residuals, Pearson and Spearman correlations are above 0.99 (0.97) and all correlations are highly significant. These high correlations indicate that the residuals obtained from asset pricing regressions estimated over periods which include returns on days with news events do not appear to markedly affect the ordering of residuals one would obtain if such event days were (more properly) excluded from the regression estimation.

Overall, we interpret the findings in Table 3 as showing little evidence that the inclusion of returns on news event days in estimating asset pricing models materially influences the residuals from these models. These tests speak, however, to *aggregate* effects; they do not speak to whether and how the configuration of news events influences the measured market reaction to specific scrutinized events. We turn to this issue in our remaining tests.

5.2. <u>Market reaction tests (univariate comparison)</u>

Our tests of the market's reaction to scrutinized events focus on the absolute value of firm j's market-adjusted return on event days, by calendar year, 1995-2004. Firm j's market-adjusted return on day t ($R_{j,t}^{MktAdj}$) is its CAPM residual for that day, where the parameters of the CAPM are estimated using all trading days in year t. (We use all trading days given the evidence in section 5.1 showing that both explained variability and residual correlation is not materially affected by the inclusion of days with news events in estimating the asset pricing regression.)[19]

Because we examine absolute market reactions which do not in expectation have a zero mean value even absent information events, we benchmark the event day absolute market-adjusted returns against non-event day absolute market-adjusted returns. We adopt a variant of Chen, Francis and Schipper's [2005] approach to calculating an absolute abnormal return ($\left|AAR_{j,t}\right|$) where we subtract the mean absolute market-adjusted return on firm j's non-event days in year t from firm j's mean absolute market-adjusted return on their event days. For purposes of these calculations, a non-event day is a day on which no news event (of any of the eight types of news events that we consider) occurred.

Summary information on the number of event days and non-event days and the mean and median value of $\left|R_{j,t}^{MktAdj}\right|$ on those event and non-event days is reported in Table 4. On average, 8,258 firms per year have at least one event day and one non-event day. The data also reveal a slight decreasing trend in

[19] We use CAPM residuals rather than 3- or 4-factor model residuals because most prior event study literature use the CAPM as the benchmark model of expected returns. We repeat our tests using 3- and 4-factor residuals (not reported) and obtain similar results. CAPM residuals and 3- and 4-factor model residuals are correlated at the 0.95 level or higher.

the number of paired firm-years, from a peak in 1997 (9,246 firms) to a low of 6,913 firm in 2004. The average [median] absolute market adjusted return is 0.0325 [0.0238] across all event days compared to 0.0261 [0.0188] on non-event days. The rightmost column in Table 4 shows the mean and median values of $\left|AAR_{j,t}\right|$; across the sample period, the mean [median] value of $\left|AAR_{j,t}\right|$ is 0.0064 [0.0049]. To determine the statistical significance of $\left|AAR_{j,t}\right|$, we examine the time series standard error of the ten annual mean [median] statistics (similar to the Fama-MacBeth technique for regression statistics).[20] The resulting t-statistics for both the mean and median value of $\left|AAR_{j,t}\right|$ (15.40 and 19.85) are reliably different from zero, at the 0.0001 level. This result is broadly consistent with prior studies' evidence that each of the scrutinized events is associated with a significant stock market response.

Our market tests so far combine all event events; we now examine each scrutinized event category separately. This analysis is important because, as shown in Tables 1 and 2, scrutinized events have different degrees of overlap with other news events. Table 5 shows mean and median values of $\left|R_{j,t}^{MktAdj}\right|$ by scrutinized event category; Panel A reports results for the 1-day event window and Panel B reports results for the 3-day event window. We present the average of the annual means [medians] along with the associated t-statistics (calculated as described above for all events). Results in the column labeled "All event days" include all event days for the noted information category regardless of any overlap between those events and other news events. As is evident from the results shown in the first column, the different event categories have substantial differences in $\left|R_{j,t}^{MktAdj}\right|$. For example, for the 1-day event window, the mean $\left|R_{j,t}^{MktAdj}\right|$ ranges from 0.0162 for dividend declarations to 0.0404 for conference calls.

The second and third columns in Table 5 show the event day $\left|R_{j,t}^{MktAdj}\right|$ for clean event days and confounded event days, respectively. With the exception of the 1-day event window results for SEO

[20] If anything, this technique is conservative because we do not use the large sample size in the annual cross-sections. Thus, cross-sectional dependence does not pose a problem for this test. We rely on the standard assumption that average daily returns are uncorrelated across years (Cochrane, 2005).

announcements, confounded event days always have larger $\left|R_{j,t}^{MktAdj}\right|$ than clean event days. For example, the mean 1-day $\left|R_{j,t}^{MktAdj}\right|$ for a confounded analyst report is 0.0373 compared to 0.0268 for a clean analyst report, or a difference of 39%. The difference in confounded versus clean event day reactions for the other news events averages about 35% for both the 1-day and 3-day event windows. In subsequent tables, we provide more details of these differences, including tests of statistical significance.

The finding that event days that are confounded by other news events have larger $\left|R_{j,t}^{MktAdj}\right|$ than clean event days, coupled with our previous evidence that such overlaps are not uncommon, raises the concern that inferences about the informativeness of a scrutinized event (as implied by the magnitude of the abnormal market reaction on that day) may be contaminated by the influence of other news events on the same day. Of course, if the benchmark against which event day returns are compared (i.e., non-event day returns) is equally contaminated, differencing of the two returns measures will control for the effects of confounding news events. That is, if there is an equal amount of confounding events on so-called "non-event" days as on event-days, then it is possible that the absolute abnormal return on event days ($\left|AAR_{j,t}\right|$) is, as an empirical matter, unaffected. Our results in Tables 1 and 2 suggest, however, that there is *not* an equal amount of confounding news on event and non-event days; rather, these results suggest a significantly greater incidence of confounding news on scrutinized event days.

To determine the degree to which the market reactions on non-event days and event days are contaminated by the effects of confounding news events, we begin by comparing the $\left|R_{j,t}^{MktAdj}\right|$ on confounded non-event days with the $\left|R_{j,t}^{MktAdj}\right|$ on clean non-event days. The columns labeled "Non-Event days" in Table 5 show the mean and median values of $\left|R_{j,t}^{MktAdj}\right|$ for all non-event days, for clean non-event days and for confounded non-event days. For all scrutinized event categories, these results show that $\left|R_{j,t}^{MktAdj}\right|$ is larger for confounded non-event days than for clean non-event days, indicating that, indeed, non-event day market reactions are also contaminated by the presence of news events. However, relative to the magnitude of differences in $\left|R_{j,t}^{MktAdj}\right|$ observed between clean event days and confounded event days (an average of 35% for both the 1-day and 3-day event windows), the difference in $\left|R_{j,t}^{MktAdj}\right|$ between clean and confounded non-event days is smaller in magnitude (an average of 23% for the 1-day window and 4% for the 3-day window).

Table 6 provides an analysis of the difference in $\left|AAR_{j,t}\right|$ observed when confounding events, on both event dates and non-event dates, are explicitly considered. Here we contrast the mean and median $\left|AAR_{j,t}\right|$ for *all* event dates and *all* non-event dates (i.e., this analysis ignores entirely the influence of other news events on both event dates and non-event dates) and for *only clean* event dates and *only clean* non-event dates (i.e., any difference is uniquely attributable to the scrutinized event). As is evident from the rightmost columns labeled "% Difference," values of $\left|AAR_{j,t}\right|$ that are calculated using only clean event and non-event days are generally substantially smaller than values of $\left|AAR_{j,t}\right|$ calculated including contaminated event and non-event days. In particular, Panel A shows that the mean [median] 1-day event day value of $\left|AAR_{j,t}\right|$ calculated using only clean event dates is 18.1% [19.9%] smaller than event day values of $\left|AAR_{j,t}\right|$ calculated using all event dates. For the 3-day event window (Panel B), the differences in abnormal returns are substantially, averaging -74.1% [-88.1%]. In most cases, the statistical significance of the clean event $\left|AAR_{j,t}\right|$ is also lower than the significance level observed for the all event days analysis $\left|AAR_{j,t}\right|$; however, in all cases but insider trades, $\left|AAR_{j,t}\right|$ remains reliably different from zero.

Overall, the results in Tables 4-6 indicate that event day absolute market-adjusted returns are larger in magnitude when the scrutinized news event is confounded by another news event, and that non-event day absolute market-adjusted returns are also larger in magnitude when the non-event day contains one or more confounding news events. These results are not surprising given that we expect new information to, on average, increase the volatility of share prices. Focusing on clean event days and clean non-event days controls for the effects of contamination and reveals the extent to which share price responses to contaminating disclosures in event periods and non-event periods differentially affect the measured market reaction to the scrutinized event. We find that contamination by confounding events is significantly greater for event day absolute market adjusted returns than for non-event day absolute market adjusted returns, implying that it is not the case that 'two wrongs make a right.' That is, merely subtracting the absolute market adjusted return on some average of non-event days from that on event days will insufficiently control for the greater tendency for contaminating news events to occur on the scrutinized event days.

5.3. Market reactions tests (multivariate comparison)

The tests in Tables 5 and 6 are intended to assess a particular event category when confounded or not confounded, without regard to the number or type(s) of confounding events. Our next tests are multivariate in nature, and (in our view) provide a better way to assess differences across types of confounding events. We believe that multivariate tests are preferable because they include more information, relative to our previous tests (reported in Table 6) which apply an extreme control for confounding events – they are dropped from the sample to obtain clean event and clean non-event days.

Table 7 provides the results of a series of multivariate tests that condition on each of the eight types of news events that we consider. Specifically, we regress the absolute market reaction on days when event *k* occurred on a series of indicator variables capturing other news events that take on the value of one if the noted confounding event occurs on the same day, and zero if it does not:

$$\left|R_{j,t}^{MktAdj}(k)\right| = \lambda_0 + \sum_{m \neq k} \lambda_j D_t^m + \varepsilon_t^{k=m} \tag{7}$$

For example, in the first row of each panel in Table 7, the dependent variable is the $\left|R_{j,t}^{MktAdj}\right|$ on analyst report dates, and the independent variables are indicator variables that take on the value one, respectively, if a conference call, dividend declaration, etc., occurs on the same day. This regression formulation is similar to an ANOVA, and the indicator variable coefficients (λ_m 's) are interpretable as the increment (decrement) in absolute market adjusted returns if confounding news event *m* occurs at the same time as scrutinized event *k*.

We begin by reporting the results of a pooled estimation of equation (7), shown in Panel A of Table 7. The intercept for each category *k* represents the 'clean' value of $\left|R_{j,t}^{MktAdj}\right|$ for that category and the slope coefficient represents the increment (if positive) or decrement (if negative) on $\left|R_{j,t}^{MktAdj}\right|$ associated with a confounding event. For example, the clean $\left|R_{j,t}^{MktAdj}\right|$ for earnings announcements is 0.0389. An analyst report issued on the same day *decreases* the average $\left|R_{j,t}^{MktAdj}\right|$ to earnings announcements by 0.0059. A conference call on the earnings announcement day *increases* the $\left|R_{j,t}^{MktAdj}\right|$ to the earnings announcement by 0.0082. Such disparate effects are also apparent for the other information categories. These data indicate that for pooled sample, controlling (or not) for confounding news events can materially affect the magnitude of any abnormal returns effect. Further, our results indicate that the type of confounding news event matters. From inspection of the categories of confounding news events, we note that most have different directional effects depending on the scrutinized event. The exception is dividend declarations which decrease the average $\left|R_{j,t}^{MktAdj}\right|$ for all scrutinized events except SEO announcements, where the confounding effect of dividend declarations is insignificant.

While the results in Panel A of Table 7 are important for research designs that use pooled samples, they do not inform about what is expected for a particular firm. For example, the fact that dividend declarations decrease the $\left|R_{j,t}^{MktAdj}\right|$ associated with earnings announcements may be due to the fact that earnings announcements are associated with smaller values of $\left|R_{j,t}^{MktAdj}\right|$ for dividend paying firms (a cross-sectional effect) or to the fact that dividend declarations generally decrease price volatility during earnings announcements (a within-firm effect). To isolate the within-firm effect (which is more pertinent to our research question), we perform firm-specific regressions of equation (7).

At least four issues arise in estimating firm-specific versions of equation (7) that do not arise in estimating pooled regressions. First, two or more confounding news event variables might be perfectly correlated (i.e., firm j always issues a management forecast and makes a conference call on earnings announcement days). When this occurs, the firm is dropped from the sample because of perfect collinearity of two or more independent variables. Second, if an indicator variable takes on the value of zero for all observations (i.e. there is no such event on any one of the required event days), the variable must be excluded from the regression model for this firm. For example, of the 10,519 firm-regressions where the required event is an earnings announcement, there are only 2,276 firm-specific coefficients on the management forecast indicator variable, implying that about 80% of the time, firms do not issue management forecasts at the same time they announce earnings. Third, we require a sufficient number of observations of each required event in order to estimate firm-specific regressions. For our purposes, we define sufficient number as at least five more event day observations than independent variables in the firm-specific regression model. Fourth, the interpretation of the intercept changes because it is now "variable-specific." By variable-specific, we mean that because we require an independent variable to have non-zero observations in order to be included in the regression for a particular firm, the sample regression intercepts will vary by variables included.

Results of our firm-specific tests of equation (7) are reported in Panel B of Table 7. Here we report six statistics for each scrutinized event: the mean intercept, the mean slope coefficient estimate and its associated t-statistic, the sum of the mean intercept and the mean coefficient estimate (the sum is the estimate of the $\left|R_{j,t}^{MktAdj}\right|$ for the information event in question in this multivariate analysis), the mean percentage increment (equal to the additional $\left|R_{j,t}^{MktAdj}\right|$ effect the confounding event has over and about the clean $\left|R_{j,t}^{MktAdj}\right|$ effect indicated by the intercept), and the number of firms with non-zero observations for the information variable in question.

The results in Panel B show that in most cases the increment to the clean $\left|R_{j,t}^{MktAdj}\right|$ (as indicated by the mean intercept) from having a confounding event is positive or not reliably different from zero. Overall, conference calls, M&A announcements, and management forecasts tend to have the largest confounding effects. For example, if an analyst report is confounded by a conference call, the existence of the conference call increases the average $\left|R_{j,t}^{MktAdj}\right|$ of analyst reports by 59% (from an $\left|R_{j,t}^{MktAdj}\right|$ or 0.0301 to 0.0480, t-statistic for increment is 24.19); if the analyst report is confounded by an M&A announcement, the incremental increase in $\left|R_{j,t}^{MktAdj}\right|$ for the M&A announcement is 81% (from an $\left|R_{j,t}^{MktAdj}\right|$ of 0.0273 to 0.0493, t-statistic of 20.44); and if the analyst report is confounded by a management forecast, the $\left|R_{j,t}^{MktAdj}\right|$ increase of the management forecast is 78% (from 0.0260 to 0.0463, t-statistic of 15.29). The smallest effects of confounding events are found for insider trades and dividend declarations, where the slope coefficients on these confounding effects are either insignificant or relatively small in economic magnitude (positive or negative). The only confounding event with a negative effect that is non-trivial in both economic magnitude and statistical significance is the effect of earnings announcements on management forecasts, where we find a reduction of the clean $\left|R_{j,t}^{MktAdj}\right|$ from 0.0331 to 0.0209 (t-statistic = -6.71).

Overall, we conclude from Table 7 that conference calls, M&A announcements, and management forecasts are non-trivial confounding events, the effects of which may severely distort inferences about the magnitude of market reactions to scrutinized events.

5.4 Interaction effects

While the multivariate tests in Table 7 control for the existence of multiple events on the same day, they do not control for interaction effects. That is, these tests do not provide an answer to the question of what is the returns effect of the combination of, say, an analyst report, a conference call, and a dividend declaration on day t. In addition, because the tests in Table 7 do not include a benchmark for the expected absolute market-adjusted return (i.e., the sample consists of event day observations only), it is not possible to draw inferences about whether the coefficient is reliably different from its expected value (under the null hypothesis of no information).

Our tests in this section address both concerns: specifically, we investigate the average absolute abnormal market reaction to each scrutinized event, while controlling for the information disseminated by confounding events. Our analysis uses a factorial design with interactions where we limit the number of interaction terms to three (hence, we refer to this test as a "fractional" factorial design).[21] Essentially, the fractional factorial design apportions the return on a confounded event day among all of the news events that occur on that day, including any interactions between those news events. The fractional factorial test amounts to estimating the following regression, by firm, for *all* available trading days (firm-subscripts are suppressed):

$$\left|R_t^{MktAdj}\right| = \alpha + \sum_{n=1}^{8}\beta_n A_{n,t} + \sum_{m=1}^{28}\gamma_m [A\times B]_{m,t} + \sum_{o=1}^{56}\delta_o [A\times B\times C]_{o,t} + \varepsilon_t \qquad (8)$$

where $A, B, C \in \left\{Analyst_{j,t}, ConfCall_{j,t}, Div_{j,t}, EA_{j,t}, Insider_{j,t}, M\ \&\ A_{j,t}, MF_{j,t}^{Q}, SEO_{j,t}\right\}$

and $A \neq B \neq C$.

In estimating equation (8), we impose the same procedures (as described in section 5.3) to handle perfect collinearity and the variable-specificity of the regressions. We continue to require a minimum of 50 trading days over 1995-2004 to estimate equation (8).

The interpretation of the coefficient estimates from equation (8) differs from the interpretation of the coefficients from equation (7) because (8) is estimated using all trading days not just event days. The

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[21] The number of *k*-factor interactions among eight events $\dfrac{\begin{pmatrix}8\\k\end{pmatrix}}{k!} = \dfrac{8!}{(8-k)!}$. We limit the number of interacting events to k=3 (implying 92 interaction terms) because of the large number of interaction terms possible with all eight factors, $\sum_{k=1}^{8}\dfrac{\begin{pmatrix}8\\k\end{pmatrix}}{k!} = 225$.

intercept, α, is the average absolute one-factor model residual on non-event days. The eight β coefficients are the main effects of each event (beyond the non-event day return, or the $\left|AAR_{j,t}\right|$), absent all other events. The 28 γ coefficients are the two-factor interaction effects, absent a third event, etc. Of the 56 possible three-way interactions, 33 coefficients (the δ's) are estimable for the 14,280 firms with at least 50 trading returns over 1995-2004.

Table 8 shows that the mean value of the intercept across the 14,280 firms, i.e., the average $\left|R_{j,t}^{MktAdj}\right|$ on non-event days, is 0.0292 (t-statistic =164.67). The panel labeled Main Effects shows the incremental $\left|R_{j,t}^{MktAdj}\right|$ (or the $\left|AAR_{j,t}\right|$) of each scrutinized event considered separately. The ordering of the main effect coefficient estimates generally lines up with our prior analyses in that conference calls, M&A announcements and management forecasts have the largest effects; the SEO announcement effect is also quite large. We observe, however, that for all but two scrutinized events (M&A and SEO announcements), the magnitude of the main effect is smaller than the average effect documented in Table 6 when all event days and all non-event days are considered. For example, whereas Table 6 shows that the average 1-day $\left|AAR_{j,t}\right|$ to analyst reports is 0.0062 (for the comparison based on all event and non-event days), Table 8 shows that controlling for news events that occur on the same day as the analyst report reduces the main effect of the analyst report to 0.0039, or a decline of 59%.

More important are the results showing what happens to market reactions once we control for interactions among confounding events. The panel labeled 2-Factor Interactions show that some interaction effects are positive while others are negative. To illustrate an example of how the total effect with interactions is calculated, assume that one is interested in the $\left|AAR_{j,t}\right|$ effect (that is, the absolute market adjusted return that is above and beyond the absolute market adjusted return found on non-event days) of days when there is both an earnings announcement and a conference call. The earnings announcement main effect is 0.0081 and the main effect of conference calls is 0.0128. However, the interaction effect between earnings announcements and conference calls is negative, -0.0096. Thus, the total effect is 0.0081 + 0.0128 - 0.0096 = 0.0113.

Now assume there is an additional effect of a confounding analyst report. To calculate the new expected market reaction we need to add to 0.0113 the main effect for analyst reports (of 0.0039), the two way interactions between analyst reports and earnings announcements (0.0001, insignificant) and between analyst reports and conference calls (0.0130) and the three way interaction between analyst reports, earnings announcements and conference calls in the 3-Factor interaction panel (of –0.0142). The total incremental returns effect of the analyst report is, therefore, 0.0039 + 0.0001 + 0.0130 - 0.0142 = 0.0028, implying that the total returns effect on days when there are analyst reports, earnings announcements and conference calls is 0.0113 + 0.0028 = 0.0141.[22]

Inspection of the results of the fractional factorial test reveals that 12 of the 28 estimable 2-way interaction effects (but only one of the 33 estimable 3-way interactions) are reliably non-zero, at the 0.10 level or better. (In unreported work, we find that 4 (or more)-way interaction effects also tend to be indistinguishable from zero, providing some validation of our decision to limit the factorial design to 3-way interactions.) Of the 12 significant 2-way interaction effects, six have reliably positive coefficients and six have reliably negative coefficients. We conclude from these results that while there appear to be some systematic interaction effects within our sample firms and sample information events, interaction effects are, for the most part, neither statistically significant nor consistent in their sign. However, these interactions could affect inferences in specific settings. For example, consider the combined reaction to an analyst report and a same-day management forecast, where their interaction effect (of +0.0218) exceeds the sum of their main effects (of +0.0039 and +0.0142), for a combined reaction of 0.0399.

[22] Note that this interpretation of the coefficients is valid under the assumption that the sample is constant across all coefficient estimates used in the calculation. This assumption is violated because we are not able to estimate all coefficients for all sample firms. Our inferences, therefore, are based on the further assumption that the changes in samples across the regressions do not significantly bias the coefficient estimates.

6. *Summary and Conclusions*

Taken as a whole, we believe our findings suggest the following conclusions. First, for some types of events, the frequency and configuration of confounding events call into question whether a measured reaction is reflective of the informativeness of the scrutinized event per se or to other disclosures occurring at the same time (for the 1-day event window results) or near in time (for the 3-day event window results). For example, using the same 3-day window employed in many event studies, we find that conference calls and management forecasts are contaminated 93% of the time; earnings announcements 64% of the time; and merger/acquisition and seasoned equity offering events 57% of the time. The configuration of confounding events also shows that these events are not equally distributed on event and non-event days: event days have a significantly greater incidence of confounding events, indicating that a simple adjustment that scales or subtracts a non-event day market return from the event day reaction is unlikely to control for this problem.

Second, when we purge both the event days and the non-event days to identify samples that are free of confounding events, we find that the measured absolute abnormal reaction to most of the news events that we consider is substantially smaller in magnitude. In particular, the mean difference in the measured 1-day (3-day) absolute abnormal reaction is 18% (74%) smaller when we exclude the effects of confounding events. The measured absolute abnormal return to the scrutinized event is also smaller when we include confounding event days and non-event days, but use a multivariate regression to control for their returns effects. Based on this evidence, we conclude that the informativeness of scrutinized events (as proxied by the magnitude of the abnormal market reaction) is, for at least some scrutinized events, substantially less than previously documented. However, in nearly all cases, the clean event reactions remain reliably different from zero, indicating that prior studies' conclusions about informativeness, in a statistical sense, are supported. Stated differently, the nature and existence of confounding events affect the economic magnitude of the market reaction, but not its statistical reliability.

Overall, we believe the results of this study are relevant to researchers, investors, and other users of the sorts of information events that we analyze. For researchers, our results indicate that greater care should be taken to identify other news events that occur concurrently with the scrutinized event. This cautionary advice is particularly aimed at research that examines 3-day (or longer) windows around conference calls, earnings announcements, management forecasts, merger and acquisition events, and seasoned equity offering announcements; for each of these events, there is a greater than 50% chance that the 3-day event window includes at least one other information event.

For investors, our results suggest potentially different investment strategies to news events than previously documented. For example, our finding that 64% of earnings announcements are confounded by another event (with that event typically being one or more analyst reports or a conference call) suggests the possibility that post-event window drifts in stock prices may be related to these other events rather than under-reaction to the earnings news released in the earnings announcement. In this regard, our findings suggest extending Chan [2003] to see whether post-announcement drifts can be linked to specific other news events that may systematically occur in the post-announcement period.

One user group which we think should be especially interested in our work is legal counsel involved in class action securities litigation over defective disclosures. Securities lawsuits, especially section 10b-5 litigation, often involve allegations of defective disclosures which *caused* stock prices to be inflated (such as intentionally upwardly biased management forecasts) which were then followed by some disclosure that revealed the true (and poorer) state of the firm, leading to a drop in stock price at the time of the curative disclosure. An important and largely assumed link in this reasoning is a causal relation between the defective disclosure and the inflated share price. For the most part, lawsuits establish this link by pointing to event studies demonstrating that a particular type of disclosure (e.g., management forecasts) is systematically associated with a reliably non-zero share price response. Our findings suggest caution in making this link: given the frequency with which, for example, management forecasts are confounded by other news events – notably analyst reports, conference calls and earnings announcements – it is quite possible that the market response on the day of the management forecast is not driven by the forecast news itself, but to news released in the confounding event(s) or its interaction with such news, neither of which may be controllable by management. In addition and irrespective of whether there is a statistically reliable link between a given news event and the market response, litigation turns on the economic magnitude of that response as the basis for calculating damages. We find that controlling for confounding events materially decreases most of these links—that is, the economic magnitude of the market response to the news events we consider generally declines when we control for each of the other news events.

Table 1

Confounded Events (Counts and Percentages)

Panel A: 1-Day Window (1995 - 2004) (# firm-days with at least one event = 2,919,138)

	Analyst	*ConfCall*	*Div*	*EA*	*Insider*	*M&A*	*MF*	*SEO*	**Any event**
Analyst	2,221,671	66,202	18,263	80,946	58,485	32,084	14,011	758	**213,521**
	100%	*2.98%*	*0.82%*	*3.64%*	*2.63%*	*1.44%*	*0.63%*	*0.03%*	***9.61%***
ConfCall	66,202	118,327	3,435	65,025	1,966	2,652	10,297	32	**95,590**
	55.95%	*100%*	*2.90%*	*54.95%*	*1.66%*	*2.24%*	*8.70%*	*0.03%*	***80.78%***
Div	18,263	3,435	135,740	10,347	1,982	920	1,024	17	**27,236**
	13.45%	*2.53%*	*100%*	*7.62%*	*1.46%*	*0.68%*	*0.75%*	*0.01%*	***20.06%***
EA	80,946	65,025	10,347	257,732	2,859	2,548	12,907	23	**118,699**
	31.41%	*25.23%*	*4.01%*	*100%*	*1.11%*	*0.99%*	*5.01%*	*0.01%*	***46.06%***
Insider	58,485	1,966	1,982	2,859	367,121	2,782	483	79	**64,408**
	15.93%	*0.54%*	*0.54%*	*0.78%*	*100%*	*0.76%*	*0.13%*	*0.02%*	***17.54%***
M&A	32,084	2,652	920	2,548	2,782	111,311	545	36	**36,468**
	28.82%	*2.38%*	*0.83%*	*2.29%*	*2.50%*	*100%*	*0.49%*	*0.03%*	***32.76%***
MF	14,011	10,297	1,024	12,907	483	545	22,110	13	**18,864**
	63.37%	*46.57%*	*4.63%*	*58.38%*	*2.18%*	*2.46%*	*100%*	*0.06%*	***85.32%***
SEO	758	32	17	23	79	36	13	3,529	**867**
	21.48%	*0.91%*	*0.48%*	*0.65%*	*2.24%*	*1.02%*	*0.37%*	*100%*	***24.57%***

Panel B: 3-Day Window (1995 - 2004)

	Analyst	*ConfCall*	*Div*	*EA*	*Insider*	*M&A*	*MF*	*SEO*	**Any event**
Analyst	2,221,671	174,316	56,300	236,943	157,267	79,549	37,136	2,661	**535,155**
	100%	*7.85%*	*2.53%*	*10.67%*	*7.08%*	*3.58%*	*1.67%*	*0.12%*	***24.09%***
ConfCall	93,576	118,327	7,390	89,003	5,297	4,831	13,523	73	**109,719**
	79.08%	*100%*	*6.25%*	*75.22%*	*4.48%*	*4.08%*	*11.43%*	*0.06%*	***92.73%***
Div	35,784	7,243	135,740	17,315	5,568	2,223	1,625	31	**46,280**
	26.36%	*5.34%*	*100%*	*12.76%*	*4.10%*	*1.64%*	*1.20%*	*0.02%*	***34.09%***
EA	138,776	87,652	17,296	257,732	8,009	5,149	13,565	79	**164,528**
	53.85%	*34.01%*	*6.71%*	*100%*	*3.11%*	*2.00%*	*5.26%*	*0.03%*	***63.84%***
Insider	121,128	5,918	6,135	8,644	367,121	7,052	1,460	261	**132,534**
	32.99%	*1.61%*	*1.67%*	*2.35%*	*100%*	*1.92%*	*0.40%*	*0.07%*	***36.10%***
M&A	56,957	5,389	2,568	6,097	7,539	111,311	1,119	121	**63,248**
	51.17%	*4.84%*	*2.31%*	*5.48%*	*6.77%*	*100%*	*1.01%*	*0.11%*	***56.82%***
MF	19,264	13,384	1,626	13,594	1,293	943	22,110	33	**20,761**
	87.13%	*60.53%*	*7.35%*	*61.48%*	*5.85%*	*4.27%*	*100%*	*0.15%*	***93.90%***
SEO	1,843	73	31	79	238	108	33	3,529	**2,012**
	52.22%	*2.07%*	*0.88%*	*2.24%*	*6.74%*	*3.06%*	*0.94%*	*100%*	***57.01%***

Variable definitions and sample description: *Analyst*=any of quarterly earnings forecast, annual EPS forecast, long-term growth forecast, target price, or stock recommendation; *ConfCall*=conference call; *Div*=dividend declaration; *EA* = quarterly earnings announcement; *Insider* = insider purchase or sale of shares; *M&A* = merger or acquisition activity for target firm or acquiring firm (or parent of either); *MF* = management forecast; *SEO* =seasoned equity offering by issuer or parent. The sample consists of all firms with available data over 1995-2004.

This table provides descriptive statistics on the extent of confounding events (horizontal axis), by each information category (vertical axis). Across both panels, percentages are calculated by rows. For example, the first row of Panel A indicates that 2.98% of analyst report event days are confounded by conference calls made on the same day as the analyst report; the second row of Panel A indicates that 55.95% of all conference call event days are confounded by analyst reports on the same day. The rightmost column is a summary calculation that shows the number and percentage of event days that are confounded by one or more other events. Panel A shows results when the period of confounding is limited to the same day as the scrutinized event; Panel B shows when the period of confounding is extended to the 3-day window centered on the announcement date of the scrutinized event.

Table 2

Confounded Events (Counts and Percentages) at Firm-Year Level

Panel A: 1-Day Window (1995 - 2004)

	Event Days			Non-Event Days		
	Avg. # all event days	Avg. # clean event days	Avg. # confounded event days	Avg. # all non-event days	Avg. # clean non-event days	Avg. # confounded non-event days
Analyst	26.20	23.63	2.57	211.48	203.11	8.38
% all days		*90.20%*	*9.80%*		*96.04%*	*3.96%*
ConfCall	1.48	0.29	1.20	236.20	203.11	33.09
% all days		*19.21%*	*80.79%*		*85.99%*	*14.01%*
Div	1.62	1.30	0.33	236.07	203.11	32.96
% all days		*79.94%*	*20.06%*		*86.04%*	*13.96%*
EA	3.05	1.62	1.44	234.63	203.11	31.52
% all days		*52.90%*	*47.10%*		*86.56%*	*13.44%*
Insider	4.49	3.70	0.79	233.20	203.11	30.09
% all days		*82.46%*	*17.54%*		*87.10%*	*12.90%*
M&A	1.29	0.86	0.43	236.40	203.11	33.29
% all days		*66.98%*	*33.02%*		*85.92%*	*14.08%*
MF	0.29	0.04	0.25	237.40	203.11	34.29
% all days		*13.89%*	*86.11%*		*85.56%*	*14.44%*
SEO	0.04	0.03	0.01	237.65	203.11	34.54
% all days		*75.35%*	*24.65%*		*85.47%*	*14.53%*
Any event	34.58	31.47	3.11	N/A	203.11	N/A
		91.00%	*9.00%*			

Panel B: 3-Day Window (1995 - 2004)

	Event Days			Non-Event Days		
	Avg. # all event days	Avg. # clean event days	Avg. # confounded event days	Avg. # all non-event days	Avg. # clean non-event days	Avg. # confounded non-event days
Analyst	26.20	19.80	6.40	211.48	203.11	8.38
% all days		*75.56%*	*24.44%*		*96.04%*	*3.96%*
ConfCall	1.48	0.11	1.38	236.20	203.11	33.09
% all days		*7.34%*	*92.66%*		*85.99%*	*14.01%*
Div	1.62	1.07	0.55	236.07	203.11	32.96
% all days		*66.09%*	*33.91%*		*86.04%*	*13.96%*
EA	3.05	1.08	1.97	234.63	203.11	31.52
% all days		*35.52%*	*64.48%*		*86.56%*	*13.44%*
Insider	4.49	2.87	1.62	233.20	203.11	30.09
% all days		*63.88%*	*36.12%*		*87.10%*	*12.90%*
M&A	1.29	0.55	0.73	236.40	203.11	33.29
% all days		*42.97%*	*57.03%*		*85.92%*	*14.08%*
MF	0.29	0.02	0.27	237.40	203.11	34.29
% all days		*5.71%*	*94.29%*		*85.56%*	*14.44%*
SEO	0.04	0.02	0.02	237.65	203.11	34.54
% all days		*43.08%*	*56.92%*		*85.47%*	*14.53%*
Any event	34.58	25.52	9.06	N/A	203.11	N/A
		73.80%	*26.20%*			

Variable definitions and sample description: See Table 1.

This table shows the average number (and percentage of total trading days) of event days and, separately, non-event days during the firm-year, aggregated first across all sample firm-years and then across calendar years. We also report the average number of clean event and non-event days, and the number of confounded event and non-event days. A clean event day is one when a single information event occurs; confounded event days consist of all other event days (i.e., event days with two or more information events). A clean non-event day is one when none of the eight information events occurs; a confounded non-event day is a non-event day when an information event other than the scrutinized event occurs.

Table 3
Comparison of Explanatory Power of Asset Pricing Models, With and Without Information

Panel A: Comparison of adjusted R-squares

	Equal-weighted R-squares				Value-weighted R-squares			
Model	All days	Non event days only	Diff	t-stat.	All days	Non event days only	Diff	t-stat.
CAPM	0.0674	0.0750	0.0075	72.66	0.2420	0.2714	0.0294	124.10
3-factor	0.0859	0.0950	0.0090	77.22	0.2801	0.3139	0.0338	140.18
4-factor	0.0890	0.0983	0.0093	77.66	0.2901	0.3237	0.0336	135.96

Panel B: Comparison of adjusted R-squares from regressions including event indicator variables

	Equal-weighted R-squares				Value-weighted R-squares			
Model	Excluding event variables	Including event variables	Diff.	t-stat	Excluding event variables	Including event variables	Diff.	t-stat
CAPM	0.0674	0.0896	0.0222	96.91	0.2420	0.2683	0.0263	142.82
3-factor	0.0859	0.1080	0.0220	96.85	0.2801	0.3061	0.0260	142.71
4-factor	0.0890	0.1110	0.0220	96.86	0.2901	0.3159	0.0258	142.27

Panel C: Correlation of signed and absolute residuals from models including (excluding) event days

	Signed residuals				Absolute value of residuals			
Model	Pearson	p-value	Spearman	p-value	Pearson	p-value	Spearman	p-value
CAPM	0.9990	0.0000	0.9971	0.0000	0.9962	0.0000	0.9878	0.0000
3-factor	0.9971	0.0000	0.9946	0.0000	0.9925	0.0000	0.9800	0.0000
4-factor	0.9961	0.0000	0.9932	0.0000	0.9906	0.0000	0.9761	0.0000

This table reports information about adjusted R^2s and correlations of residuals for asset pricing regressions estimated with and without event information. For each firm-year with at least 50 trading returns, we estimate a 1-factor (CAPM), 3-factor and 4-factor asset pricing regression using daily returns data over 1995-2004. Panel A shows the adjusted R^2s from regressions which are estimated using all trading days ("All days" column) and using the subset of all days which have no information events ("Non event days only" column). We report results which equally weight each firm's adjusted R^2 and results which value-weight the firm-specific R^2s. In Panel B, we augment the 1-factor, 3-factor, and 4-factor regressions with indicator variables capturing the news conveyed by each of the eight information events we consider. We report the mean adjusted R^2 across the firm-specific regressions, aggregating using both equal-weighting and value-weighting. Finally, Panel C shows the correlations between the signed (absolute) residuals obtained from each asset pricing regression, estimated with and without event day trading returns.

Table 4
Absolute Value of Market Model Residuals on Event and Non-Event Days

		Days with ≥ 1 event			Days with no event			Paired-sample difference (by firm)	
Year	# Paired firm-years	Avg. # days	Mean	Median	Avg. # days	Mean	Median	Mean	Median
1995	8,403	34.34	0.0302	0.0237	205.17	0.0257	0.0195	0.0045	0.0042
1996	8,975	35.37	0.0298	0.0229	203.75	0.0252	0.0189	0.0046	0.0041
1997	9,246	34.67	0.0299	0.0224	205.24	0.0249	0.0185	0.0049	0.0040
1998	9,195	36.03	0.0347	0.0250	202.64	0.0285	0.0201	0.0062	0.0049
1999	8,886	34.12	0.0362	0.0264	201.51	0.0289	0.0208	0.0073	0.0057
2000	8,644	33.18	0.0413	0.0298	203.09	0.0337	0.0242	0.0076	0.0056
2001	7,979	33.31	0.0385	0.0277	201.85	0.0307	0.0215	0.0078	0.0062
2002	7,382	36.60	0.0344	0.0242	204.98	0.0267	0.0188	0.0077	0.0054
2003	6,961	40.99	0.0270	0.0191	201.28	0.0200	0.0143	0.0070	0.0048
2004	6,913	36.16	0.0232	0.0162	205.10	0.0164	0.0119	0.0069	0.0043
Mean	8,258	35.48	0.0325	0.0238	203.46	0.0261	0.0188	0.0064	0.0049
t-stat			18.74	18.88		16.44	17.06	15.40	19.85

This table presents statistics on the absolute value of firm j's market-adjusted return on events days and non-event days, by calendar year, 1995-2004. Firm j's market adjusted return on day t ($R_{j,t}^{MktAdj}$) is its 1-factor model residual for that day, where the parameters of the firm's one-factor model are estimated using all trading days in year t. We require a firm to have at least 50 trading days to estimate a 1-factor model, and we require that the firm have at least one event day and one non-event day in the same year (the latter is a non-binding constraint) to enter into the sample that we use to examine event day returns. The second column of the table shows the number of firm-years meeting these requirements. The columns labeled "Days with no events" show the mean and median value of absolute market adjusted returns on clean non-event days (i.e., days on which none of the eight information events occurs). The rightmost column shows the mean and median value of the difference in event versus non-event absolute market-adjusted returns, or $\left|AAR_{j,t}\right|$. The t-statistic for the paired sample mean [median] difference is based on the time series standard error of the ten annual mean [median] statistics.

Table 5
Average Absolute Market Model Residuals on Event and Non-Event Days, By Information Event

Panel 1: 1-day event window

	Event days						Non-Event days					
	All event days		Clean event days		Confounded event days		All non-event days		Clean non-event days		Confounded non-event days	
	Mean	Median	Mean	Median	Mean	Median	Mean	Median	Mean	Median	Mean	Median
Analyst	0.0280	0.0163	0.0268	0.0159	0.0373	0.0205	0.0218	0.0141	0.0217	0.0141	0.0257	0.0152
	14.45	14.89	14.01	14.49	16.04	14.74	14.05	15.56	13.97	15.61	15.14	15.24
ConfCall	0.0404	0.0247	0.0341	0.0197	0.0436	0.0269	0.0243	0.0159	0.0234	0.0155	0.0286	0.0173
	15.46	15.37	14.32	13.77	17.09	16.67	11.62	11.57	11.43	11.77	11.27	11.54
Div	0.0162	0.0108	0.0145	0.0102	0.0183	0.0118	0.0142	0.0098	0.0136	0.0095	0.0163	0.0106
	18.87	18.75	19.33	17.96	18.24	20.55	16.69	15.83	16.68	15.69	17.04	14.71
EA	0.0349	0.0214	0.0319	0.0197	0.0377	0.0228	0.0249	0.0157	0.0242	0.0154	0.0301	0.0171
	18.65	16.07	17.04	15.91	20.01	18.09	14.94	15.66	14.83	15.61	15.06	15.63
Insider	0.0221	0.0138	0.0208	0.0134	0.0252	0.0152	0.0220	0.0140	0.0208	0.0135	0.0267	0.0156
	13.68	13.59	14.18	13.83	13.69	13.21	13.77	14.44	13.66	14.43	15.13	14.26
M&A	0.0287	0.0152	0.0241	0.0135	0.0343	0.0172	0.0211	0.0130	0.0200	0.0125	0.0247	0.0142
	13.58	12.52	13.34	12.31	13.29	12.55	12.46	13.22	12.37	13.29	12.64	12.85
MF	0.0361	0.0218	0.0296	0.0178	0.0402	0.0248	0.0192	0.0131	0.0181	0.0127	0.0230	0.0143
	20.41	19.17	13.42	12.99	24.38	15.86	17.36	17.02	16.99	16.81	17.96	16.39
SEO	0.0294	0.0275	0.0309	0.0232	0.0278	0.0272	0.0202	0.0140	0.0188	0.0134	0.0225	0.0154
	9.72	8.31	6.04	4.68	11.94	10.10	14.34	11.12	12.99	10.47	15.61	11.11
Any event	0.0292	0.0168	0.0280	0.0162	0.0378	0.0211	N/A	N/A	0.0229	0.0146	N/A	N/A
	14.66	14.87	14.37	14.73	16.87	14.87			14.18	15.29		

Panel B: 3-day event window

	All event days		Clean event days		Confounded event days		All non-event days		Clean non-event days		Confounded non-event days	
	Mean	Median	Mean	Median	Mean	Median	Mean	Median	Mean	Median	Mean	Median
Analyst	0.0235	0.0142	0.0227	0.0139	0.0298	0.0167	0.0237	0.0143	0.0236	0.0144	0.0241	0.0139
	13.25	13.23	13.01	13.15	13.48	12.93	15.90	17.67	15.87	17.69	15.98	17.49
ConfCall	0.0343	0.0199	0.0299	0.0176	0.0368	0.0213	0.0240	0.0149	0.0242	0.0150	0.0232	0.0144
	14.70	14.37	13.63	13.82	15.22	14.66	12.67	13.53	12.82	13.90	12.08	12.40
Div	0.0155	0.0099	0.0141	0.0092	0.0184	0.0115	0.0151	0.0099	0.0147	0.0096	0.0164	0.0108
	21.26	22.58	22.50	24.31	18.08	18.36	16.26	17.59	17.64	19.41	12.86	13.15
EA	0.0332	0.0191	0.0300	0.0174	0.0362	0.0209	0.0240	0.0145	0.0242	0.0146	0.0233	0.0140
	18.67	18.43	19.06	18.78	16.72	16.36	14.88	15.68	15.29	16.35	13.20	13.40
Insider	0.0220	0.0135	0.0217	0.0134	0.0228	0.0137	0.0229	0.0139	0.0226	0.0137	0.0240	0.0143
	14.45	14.63	15.11	15.39	12.58	12.52	14.74	14.99	15.07	15.68	13.41	12.83
M&A	0.0245	0.0134	0.0230	0.0130	0.0269	0.0142	0.0218	0.0133	0.0218	0.0133	0.0221	0.0134
	13.25	12.39	13.53	13.06	12.40	11.37	13.24	13.53	13.67	14.15	11.89	11.91
MF	0.0352	0.0186	0.0249	0.0153	0.0419	0.0213	0.0204	0.0131	0.0201	0.0130	0.0210	0.0133
	13.97	14.88	14.61	15.25	13.02	13.23	14.70	15.09	15.16	15.79	13.51	13.45
SEO	0.0318	0.0229	0.0314	0.0227	0.0323	0.0237	0.0242	0.0162	0.0235	0.0159	0.0261	0.0168
	12.16	11.02	11.05	10.29	13.02	12.02	11.24	10.58	11.27	10.79	11.16	10.06
Any event	0.0247	0.0146	0.0237	0.0143	0.0322	0.0180	N/A	N/A	0.0234	0.0142	N/A	N/A
	13.07	12.80	12.91	12.80	13.20	12.36			14.73	15.74		

This table shows values of $\left|R_{j,t}^{MktAdj}\right|$ by information event category. Results for the 1-day event window are shown in Panel A; results showing the average value of $\left|R_{j,t}^{MktAdj}\right|$ across the 3-day window centered on event day 0 are shown in Panel B. We report the average of the annual firm-year means [medians] along with the associated t-statistics for three classifications: all, clean and confounded event and non-event days. "Clean" event days pertain to days where the scrutinized event (given by the row) is the only news event on that day; "confounded" event days are those where one or more other news events occur on the same day as the scrutinized event; "all" event days is the union of clean and confounded event days. Similar classifications apply to non-event days: a "clean" non-event day is a day on which no news event (of the eight types we consider) occurred; a confounded non-event day is a day when there is no evidence of the scrutinized event, but there is evidence of one or more other news events; "all" non-event days is the union of clean and confounded non-event days.

Table 6
Paired-Sample Tests - Constant Sample by Event Category

Panel A: 1-Day Window (1995 - 2004)

	All Events Minus All Non-Events		Clean Events Minus Clean Non-Events		% Difference	
	Mean	Median	Mean	Median	Mean	Median
Analyst	0.0062	0.0020	0.0051	0.0017	-18.6%	-15.6%
	10.83	*9.68*	*11.08*	*9.41*	*-7.19*	
ConfCall	0.0161	0.0082	0.0107	0.0047	-33.6%	-42.0%
	15.59	*21.05*	*10.85*	*12.71*	*-9.88*	
Div	0.0019	0.0011	0.0009	0.0008	-54.4%	-31.5%
	16.84	*16.88*	*6.46*	*11.47*	*-9.30*	
EA	0.0100	0.0055	0.0077	0.0044	-22.3%	-19.7%
	27.31	*14.57*	*23.87*	*10.67*	*-6.48*	
Insider	0.0001	0.0001	0.0000	0.0002	-93.0%	52.4%
	1.08	*2.65*	*0.09*	*3.83*	*-1.32*	
M&A	0.0076	0.0021	0.0041	0.0010	-46.8%	-49.6%
	13.97	*9.86*	*13.02*	*9.86*	*-8.64*	
MF	0.0169	0.0094	0.0115	0.0055	-32.3%	-41.8%
	12.90	*9.59*	*8.14*	*7.27*	*-5.32*	
SEO	0.0092	0.0136	0.0121	0.0101	30.8%	-25.5%
	4.23	*4.90*	*2.93*	*2.52*	*1.14*	
Any event	0.0066	0.0021	0.0054	0.0017	-18.1%	-19.9%
	9.29	*8.86*	*9.26*	*8.19*	*-8.08*	

Panel B: 3-Day Window (1995 - 2004)

	All Events Minus All Non-Events		Clean Events Minus Clean Non-Events		% Difference	
	Mean	Median	Mean	Median	Mean	Median
Analyst	-0.0002	-0.0002	-0.0009	-0.0004	-387.6%	-157.6%
					-7.96	
ConfCall	0.0103	0.0050	0.0057	0.0025	-44.4%	-49.8%
					-20.50	
Div	0.0004	0.0000	-0.0006	-0.0005	-253.1%	-5335.1%
					-13.16	
EA	0.0092	0.0046	0.0058	0.0028	-36.4%	-38.6%
					-5.07	
Insider	-0.0009	-0.0004	-0.0008	-0.0003	7.2%	19.5%
					1.29	
M&A	0.0027	0.0002	0.0012	-0.0003	-55.1%	-269.9%
					-6.10	
MF	0.0148	0.0055	0.0048	0.0023	-67.5%	-58.1%
					-10.25	
SEO	0.0076	0.0067	0.0079	0.0068	4.4%	0.4%
					0.60	
Any event	0.0014	0.0004	0.0004	0.0000	-74.1%	-88.1%
					-7.11	

This table reports the difference in $\left|AAR_{j,t}\right|$ (equal to the absolute value of the market adjusted return in the event window less the absolute value of the market adjusted return over the non-event window) observed if one considers all event and non-event dates (so ignores entirely the influence of other disclosures on both event dates and non-event dates) and if one considers only clean event dates and clean non-event dates (so purges the effect of confounding events from the sample). Results are shown for the 1-day window (Panel A) and the 3-day window (Panel B). The far right column, labeled % Difference", shows the percentage change in the mean and median $\left|AAR_{j,t}\right|$ to each scrutinized event from focusing on only clean event days and clean non-event days. For the mean percentage difference, we report the t-statistic for the difference.

Table 7
Results of Regressions of Residuals on the 7 remaining dummies, requiring one of the events

Panel A: Pooled regression

Required Event			Intercept	*Analyst*	*ConfCall*	*Div*	*EA*	*Insider*	*M&A*	*MF*	*SEO*
Analyst	Coef. Est.		0.0241	Required	0.0165	-0.0085	0.0035	-0.0031	0.0060	0.0080	0.0080
	t-stat		945.90		98.85	-31.38	23.18	-20.31	29.64	25.04	6.04
ConfCall	Coef. Est.		0.0427	0.0011	Required	-0.0172	0.0000	-0.0061	0.0116	-0.0007	-0.0078
	t-stat		133.90	3.18		-16.86	-0.12	-4.59	10.01	-1.13	-0.75
Div	Coef. Est.		0.0123	0.0040	0.0060	Required	0.0060	0.0010	0.0114	0.0035	0.0236
	t-stat		207.73	24.39	15.44		26.61	2.28	17.42	5.48	4.93
EA	Coef. Est.		0.0389	-0.0059	0.0082	-0.0168	Required	-0.0034	0.0133	-0.0043	-0.0015
	t-stat		279.51	-23.81	30.38	-30.46		-3.30	12.18	-8.44	-0.13
Insider	Coef. Est.		0.0236	-0.0025	0.0106	-0.0092	0.0091	Required	0.0012	0.0053	0.0151
	t-stat		387.47	-16.39	12.87	-12.15	13.39		1.81	3.37	3.99
M&A	Coef. Est.		0.0331	-0.0034	0.0192	-0.0085	0.0130	-0.0083	Required	0.0065	0.0008
	t-stat		139.12	-7.63	13.81	-3.90	9.22	-6.58		2.23	0.07
MF	Coef. Est.		0.0385	0.0089	0.0115	-0.0102	-0.0163	-0.0055	0.0065	Required	-0.0134
	t-stat		51.12	10.84	12.22	-5.64	-17.69	-2.14	2.65		-0.86
SEO	Coef. Est.		0.0366	-0.0041	0.0016	0.0027	-0.0018	0.0024	-0.0018	-0.0017	Required
	t-stat		52.05	-2.68	0.23	0.30	-0.23	0.58	-0.30	-0.16	

Panel B: Firm-specific regressions, mean values and number of the coefficients and intercepts

Required Event		# firms	Grand intercept	*Analyst*	*ConfCall*	*Div*	*EA*	*Insider*	*M&A*	*MF*	*SEO*
Analyst	Intercept				0.0301	0.0187	0.0295	0.0285	0.0273	0.0260	0.0272
	Coef. Est				0.0179	0.0013	0.0038	-0.0017	0.0220	0.0203	0.0057
	t-statistic	9,909	0.0300	Required	24.19	3.00	8.17	-4.79	20.44	15.29	5.24
	Sum				0.0480	0.0199	0.0332	0.0268	0.0493	0.0463	0.0330
	% inc.				59%	7%	13%	-6%	81%	78%	21%
	# firms				4,776	3,056	7,851	6,038	4,786	2,470	609
ConfCall	Intercept			0.0407		0.0273	0.0424	0.0366	0.0368	0.0360	0.0229
	Coef. Est			0.0106		-0.0018	-0.0052	-0.0038	0.0167	0.0075	0.0061
	t-statistic	4,636	0.0421	15.81	Required	-1.34	-7.41	-2.50	5.95	6.11	0.90
	Sum			0.0513		0.0256	0.0373	0.0328	0.0534	0.0435	0.0289
	% inc.			26%		-6%	-12%	-10%	45%	21%	26%
	# firms			4,146		903	4,300	1,156	1,428	1,780	26
Div	Intercept			0.0145	0.0156		0.0164	0.0142	0.0138	0.0149	0.0130
	Coef. Est			0.0031	0.0052		0.0040	-0.0007	0.0096	0.0080	0.0002
	t-statistic	4,137	0.0141	8.83	4.13	Required	5.80	-1.50	3.20	3.43	0.08
	Sum			0.0176	0.0208		0.0203	0.0135	0.0234	0.0229	0.0132
	% inc.			21%	34%		24%	-5%	69%	54%	2%
	# firms			2,181	598		1,403	1,033	524	275	14
EA	Intercept			0.0341	0.0366	0.0235		0.0350	0.0338	0.0310	0.0174
	Coef. Est			0.0035	0.0057	-0.0001		-0.0007	0.0146	0.0037	0.0059
	t-statistic	10,519	0.0375	8.52	10.76	-0.23	Required	-0.74	6.98	3.99	1.30
	Sum			0.0376	0.0422	0.0234		0.0343	0.0484	0.0347	0.0234
	% inc.			10%	15%	-1%		-2%	43%	12%	34%
	# firms			7,162	4,934	1,971		1,999	1,809	2,276	21
Insider	Intercept			0.0233	0.0213	0.0145	0.0253		0.0207	0.0175	0.0228
	Coef. Est			0.0032	0.0106	0.0008	0.0065		0.0061	0.0078	0.0116
	t-statistic	8,647	0.0269	9.49	5.61	1.42	5.98	Required	4.36	1.68	3.56
	Sum			0.0265	0.0319	0.0153	0.0318		0.0268	0.0253	0.0344
	% inc.			14%	50%	6%	26%		30%	44%	51%
	# firms			5,487	816	1,049	1,578		1,408	213	67
M&A	Intercept			0.0265	0.0244	0.0159	0.0304	0.0244		0.0199	0.0186
	Coef. Est			0.0106	0.0230	0.0063	0.0042	-0.0004		0.0165	0.0056
	t-statistic	4,162	0.0341	10.51	10.40	2.97	2.21	-0.25	Required	2.75	0.92
	Sum			0.0371	0.0474	0.0222	0.0346	0.0240		0.0363	0.0243
	% inc.			40%	94%	40%	14%	-2%		83%	30%
	# firms			2,886	878	440	1,127	1,186		234	25
MF	Intercept			0.0319	0.0328	0.0279	0.0331	0.0308	0.0244		0.0234
	Coef. Est			0.0123	0.0086	-0.0013	-0.0122	-0.0063	0.0028		-0.0295
	t-statistic	758	0.0331	9.17	4.17	-0.46	-6.71	-1.83	0.78	Required	-1.16
	Sum			0.0442	0.0415	0.0266	0.0209	0.0245	0.0271		-0.0061
	% inc.			39%	26%	-5%	-37%	-21%	11%		-126%
	# firms			641	596	218	696	172	218		9
SEO	Intercept			0.0424	0.0478			0.0277	0.0527		
	Coef. Est			-0.0123	-0.0075			0.0148	-0.0467		
	t-statistic	5	0.0424	-1.39	-0.26			1.06	.		Required
	Sum			0.0301	0.0404			0.0424	0.0060		
	% inc.			-29%	-16%			53%	-89%		
	# firms			5	2			2	1		

This table shows the results of regressing the absolute market reaction on days when event *k* occurred on a series of indicator variables capturing other information disclosures and which take on the value of one if the noted confounding event occurs on the same day, and zero if it does not:

$$\left|R_{j,t}^{MktAdj}(k)\right| = \lambda_0 + \sum_{j \neq m} \lambda_j D_t^{k=m} + \varepsilon_t^{k=m}$$

The indicator variable coefficients (λ_m 's) are interpretable as the increment (decrement) in absolute market adjusted returns if confounding event *m* occurs at the same time as event *k*. Panel A shows results of estimating this regression for the pooled sample, and Panel B shows results of estimating the regression at the firm-level, and aggregating the firm-specific coefficients across the firms in the *k*'th regression sample.

Table 8
Average Firm-Specific Regression Estimates for Fractional Factorial Model
(Up to Three Interactions)

Regression Statistics (n=14,280 firms)			
Intercept	0.0292	164.67	0.0000
Main effects (8)			
	Mean Coefficient	t-statistic	P-value
Analyst	0.0039	20.27	0.0000
ConfCall	0.0128	21.32	0.0000
Div	0.0018	6.74	0.0000
EA	0.0081	35.56	0.0000
Insider	-0.0004	-1.85	0.0648
M&A	0.0215	25.73	0.0000
MF	0.0142	10.32	0.0000
SEO	0.0120	15.52	0.0000
2-Factor Interactions (27 out of 28 possible)			
	Mean Coefficient	t-statistic	P-value
*Analyst*ConfCall*	0.0130	11.50	0.0000
*Analyst*Div*	0.0010	2.58	0.0099
*Analyst*EA*	0.0001	0.18	0.8611
*Analyst*Insider*	-0.0007	-1.98	0.0478
*Analyst*M&A*	0.0098	9.33	0.0000
*Analyst*MF*	0.0218	10.05	0.0000
*Analyst*SEO*	-0.0071	-3.50	0.0006
*ConfCall*Div*	-0.0037	-1.30	0.1949
*ConfCall*EA*	-0.0096	-9.75	0.0000
*ConfCall*Insider*	-0.0034	-1.51	0.1324
*ConfCall*M&A*	0.0075	1.81	0.0703
*ConfCall*MF*	-0.0100	-2.02	0.0455
*ConfCall*SEO*	0.0054	0.33	0.7634
*Div*EA*	-0.0019	-2.35	0.0187
*Div*Insider*	-0.0001	-0.20	0.8442
*Div*M&A*	0.0032	1.66	0.0981
*Div*MF*	0.0000	-0.01	0.9929
*Div*SEO*	-0.0214	-1.23	0.2846
*EA*Insider*	-0.0015	-1.10	0.2714
*EA*M&A*	0.0006	0.23	0.8180
*EA*MF*	-0.0134	-5.71	0.0000
*EA*SEO*	0.0120	0.63	0.5952
*Insider*M&A*	0.0006	0.33	0.7399
*Insider*MF*	-0.0107	-1.14	0.2578
*Insider*SEO*	-0.0006	-0.10	0.9225
*M&A*MF*	0.0059	0.44	0.6645
*M&A*SEO*	0.0024	0.37	0.7158
*MF*SEO*	-0.0002	not estimable	not estimable

3-Factor Interactions (33 out of 56 possible)

	Mean Coefficient	t-statistic	P-value
*Analyst*ConfCall*Div*	-0.0001	-0.03	0.4375
*Analyst*ConfCall*EA*	-0.0142	-7.14	0.3734
*Analyst*ConfCall*Insider*	-0.0019	-0.57	0.5083
*Analyst*ConfCall*M&A*	-0.0009	-0.13	0.3867
*Analyst*ConfCall*MF*	-0.0038	-0.38	0.3460
*Analyst*Div*EA*	0.0012	0.67	0.4666
*Analyst*Div*Insider*	0.0013	0.95	0.6123
*Analyst*Div*M&A*	0.0013	0.37	0.5186
*Analyst*Div*MF*	0.0114	1.25	0.5006
*Analyst*EA*Insider*	0.0065	0.96	0.4658
*Analyst*EA*M&A*	-0.0284	-2.34	0.4235
*Analyst*EA*MF*	-0.0168	-2.91	0.3794
*Analyst*Insider*M&A*	-0.0002	-0.06	0.5521
*Analyst*Insider*MF*	-0.0031	-0.20	0.3574
*Analyst*M&A*MF*	-0.0209	-0.54	0.3042
*Analyst*M&A*SEO*	0.0062	0.31	0.5581
*ConfCall*Div*EA*	0.0085	1.84	0.3440
*ConfCall*Div*M&A*	-0.0147	-18.62	0.6674
*ConfCall*Div*MF*	-0.0163	-0.81	0.4028
*ConfCall*EA*Insider*	0.0005	0.04	0.4726
*ConfCall*EA*M&A*	0.0025	0.19	0.3296
*ConfCall*EA*MF*	0.0014	0.17	0.2823
*ConfCall*Insider*M&A*	-0.0128	-1.06	0.4043
*ConfCall*Insider*MF*	0.0452	not estimable	not estimable
*ConfCall*M&A*MF*	-0.0010	-0.04	0.1322
*Div*EA*Insider*	0.0154	1.79	0.5209
*Div*EA*M&A*	-0.0339	-1.39	0.2943
*Div*EA*MF*	0.0147	1.05	0.3787
*Div*Insider*M&A*	-0.0049	-0.44	0.3941
*EA*Insider*M&A*	-0.0105	-1.78	0.7084
*EA*Insider*MF*	0.0450	8.01	0.4224
*EA*M&A*MF*	0.0276	0.47	0.0850

This table reports the results of a fractional factorial design (limited to three interaction terms), estimated at the firm-level:

$$\left|R_t^{MktAdj}\right| = \alpha + \sum_{n=1}^{8}\beta_n A_{n,t} + \sum_{m=1}^{28}\gamma_m [A\times B]_{m,t} + \sum_{o=1}^{56}\delta_o [A\times B\times C]_{o,t} + \varepsilon_t$$

$$A, B, C \in \left\{Analyst_{j,t}, ConfCall_{j,t}, Div_{j,t}, EA_{j,t}, Insider_{j,t}, M\ \&\ A_{j,t}, MF^{Q}_{j,t}, SEO_{j,t}\right\}$$

and $A \neq B \neq C$. We report the mean value of each coefficient estimate across the sample of 14,280 firms with at least 50 trading returns over 1995-2004.

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