Is Openness Penalized?
Stock Returns around Earnings Warnings

Jennifer W. Tucker

Assistant Professor
Fisher School of Accounting
Warrington College of Business
University of Florida
(352) 273-0214 (Office)
(352)392-7962 (Fax)
E-mail: jenny.tucker@cba.ufl.edu

October 2006

I am indebted to Jim Ohlson, Bill Greene, Carol Marquardt, Stephen Ryan and Paul Zarowin for their guidance and constant support. I thank Chunrong Ai, Eli Bartov, Monika Causholli, Joel Demski, Liang Fu, Joel Houston, Baruch Lev, Haijin Lin, Jason MacGregor, Burt Porter, Jay Ritter, Alan Reinstein, Surjit Tinaikar, Senyo Tse, Pete Wilson, and the participants at the Florida State University workshop. This paper has benefited greatly from the constructive comments of two anonymous referees. I also thank Thomson Financial, especially Pamela Grant, for the First Call Company Issued Guideline database as well as I/B/E/S analyst forecast data.

Is Openness Penalized?
Stock Returns around Earnings Warnings

ABSTRACT

Prior research finds that firms that warn investors of an earnings shortfall experience lower returns in the event quarter than those that have similar risks and earnings news but do not warn. Openness appears to be penalized by investors. In this study, I examine whether warning firms' lower returns are due to their having more unfavorable nonearnings news ("other bad news") and whether investors are able to completely understand such news in the event quarter. My evidence suggests that warning firms indeed have a larger amount of other bad news than nonwarning firms. After controlling for risks, earnings news, and other bad news, I find that firms are worse off in the event quarter for having warned; however, the decision to warn is ultimately not penalized in the long run.

Keywords: earnings warning; self-selection; warning effect; voluntary disclosure

Is Openness Penalized?
Stock Returns around Earnings Warnings

I. INTRODUCTION

This paper examines whether firms that warn of an earnings shortfall experience lower stock returns than those that have similar risks, earnings news, and *nonearnings news* but do not warn. Firm managers routinely face the decision of being open vs. silent about forthcoming earnings shortfalls and are concerned about investors' responses to their disclosure decisions. After controlling for risks and earnings news, Kasznik and Lev (1995, "KL") find that in the event quarter investors react to warning firms more negatively than to those that likely anticipate an earnings shortfall but do not warn ("nonwarning firms"). This result has been interpreted as a market penalty for openness.[1] If the market indeed penalizes openness, firms facing an earnings shortfall would be less willing to voluntarily issue warnings and, as a result, warnings should diminish over time. Surprisingly, the number of warnings has increased in the past decade (Figure 1).

In this paper I examine whether KL's result is, in fact, due to unfavorable nonearnings news ("other bad news") that has made firms more likely to warn and whether investors' responses to such news are incomplete or incorrect in the event quarter. Examples of "other bad news" would be the discontinuation of new product development, plans for store closings, trouble with alliances, change in management, lawsuits, etc.[2] Such news probably affects a manager's decision to warn as well as investors' stock valuation. However, the news is unobservable to researchers without incurring substantial costs. Using the Heckman selection

---

[1] See "Silence is Golden," *The Economist*, 3/4/1994, and Core (2001, 448).

[2] Although earnings surprise, especially the change in core earnings forecasted by analysts, is the common metric of news that researchers examine, firms often disclose a great deal of other news in earnings warnings, earnings announcements, earnings conference calls, etc. For example, when Rambus Inc. warned on January 14, 1999 (*Business Wire*) about earnings in the next three quarters, it also disclosed the discontinuation of development of a prospective product.

model that deals with this unobservability problem, I find that the decision to warn is penalized by investors in the event quarter, but not in the long run. The short-term penalty is due to investors' initial failure to adjust stock prices for other bad news.

My tests proceed as follows. I collect 3,869 warnings about quarterly earnings issued by U.S. firms between 1996 and 2003. The nonwarning observations are the firm-quarters in which the forthcoming earnings are lower than the most recent analyst consensus before the third fiscal month but the firms do not warn. In my first step, I examine returns by ignoring the effect of other bad news on stock price. After controlling for risks and earnings news, I find that warning firms' average return is 10.1% lower than that of nonwarning firms. This difference, referred to as the "between-group return difference," does not change from the short-term window, which runs from the beginning of the third fiscal month to 5 days after earnings announcement, to various windows that extend the short-term window by 1 to 12 months ("long term") (Figure 2).[3] The absence of return reversal or drift suggests that warning firms as a whole are not mispriced relative to nonwarning firms.

The lower average return of warning firms than that of nonwarning firms, however, is *not* evidence for a market penalty for warning. Suppose that managers are more likely to warn when they have a larger amount of other bad news, all else being equal. Investors should adjust each firm's stock price for other bad news. As a result, the observed average stock return of warning firms is lower than that of nonwarning firms, *ceteris paribus*. This amount of negative return difference between warning and nonwarning firms exists because firms select themselves into the warning or nonwarning group. The difference, referred to as the "self-selection effect," would exist even if no firm warns and thus does not represent a penalty for warning. After removing this effect, any remaining between-group return difference is the

[3] This statement holds for the 24- and 36-month extensions as well (unreported).

warning effect – the difference between warning firms' returns and what the returns would have been had these firms not warned (Heckman 2001, 718; Greene 2003, 788). Intuitively, the warning effect estimates the average return difference between warning and nonwarning firms *after* other bad news is also controlled for. It is the warning effect rather than the between-group return difference that measures the market penalty for warning.

In my second step, I control for other bad news and examine the warning effect. I find that the warning effect in the event quarter is significantly negative at –6.4%, but disappears after the short-term window is extended by 3 months. Recall that the between-group return difference (i.e. about –10.1% for both short term and long term) consists of the warning effect and the self-selection effect. Subtracting the warning effect from the between-group return difference, I estimate the self-selection effect to be –3.7% in the short term and about –10% in the long term. These results suggest that the observed lower average return of warning firms than that of nonwarning firms in the short term is due to both a negative warning effect (–6.4%) and a negative self-selection effect (–3.7%), but that in the long run is due purely to self-selection (–10%). Thus, the decision to warn is ultimately not penalized by investors.

Furthermore, I investigate why a market penalty for warning exists in the short term. My evidence suggests that investors initially cannot fully adjust stock prices for other bad news. By the end of the short-term window, investors, who have a larger information set than researchers, possibly observe other bad news for some firms and can presumably infer such news for other firms. The argument for the latter is as follows. Suppose the amount of bad news is all that matters when managers decide whether to warn and, because of litigation costs from withholding bad news, a firm is more likely to warn when it has a larger amount of bad news that includes the observable earnings shortfall and the unobservable other bad news.

If a firm warned even though its earnings shortfall is small, this firm is likely to have a larger amount of other bad news than a warning firm that has a large earnings shortfall and thus warned expectedly in hindsight. Likewise, if a firm did not warn despite its large earnings shortfall, the firm is more likely to have a larger amount of other good news (i.e. a smaller amount of other bad news) than a nonwarning firm that has a small earnings shortfall and thus whose decision of not issuing a warning was expected. Hence, investors could infer the amount of other bad news and should adjust each firm's stock price for such news.

I find that, in the short term, after responding to earnings news investors apply a larger price cut (but do not go far enough) to the firms within the warning group that arguably have more other bad news than to those that have less. Yet, such price adjustments within the nonwarning group are the opposite of what they should be and the initial adjustments are subsequently reversed in the long term. These results suggest that investors' short-term price adjustments within the warning group were incomplete and those within the nonwarning group were incorrect. I refer to this phenomenon as "within-group mispricing."[4] As a result, firms are worse off in the short term for having warned. A trading strategy that exploits within-group mispricing earns a median abnormal return of 2.4% from the warning group and 2.5% from the nonwarning group in the 3 months after the event quarter. The existence of such abnormal returns, however, may not be inconsistent with market efficiency when transaction costs are considered (Korajczyk and Sadka 2004).

My paper makes three contributions to the literature. First, the evidence that openness is ultimately not penalized contradicts the general perception and explains why firms voluntarily

[4] In any heterogeneous group, some members are higher than the population mean and others are lower. In a standard case the deviations from the population mean are random and their sum is 0. In my case the deviations are not random: on average, warning (nonwarning) firms have more (less) other bad news than the population mean. A failure to adjust stock prices of firms *within* each group for other bad news would result in lower average abnormal return of warning firms than that of nonwarning firms.

warn despite their experiencing a larger price decline than nonwarning firms, after risks and earnings news are controlled for. Although prior studies by Shu (2003) and Xu (2003) address a similar research issue, none of them extends the return window beyond the event quarter and also controls for firms' self-selection. In addition, my study uses a substantially larger sample and thus has more test power.

Second, I document that by the end of the event quarter investors cannot fully adjust stock prices for other bad news. Such a failure may be due to softness of other bad news – it takes investors time to appreciate the implications of such news for future cash flows. This failure largely occurs to nonwarning firms probably because their negative earnings surprises arrive later (i.e. at earnings announcement date) than earnings warnings. In addition, investors may pay limited attention to these announcements when they are busy digesting many other firms' earnings announcements (Simon 1978; Hirshleifer and Teoh 2003). Previous accounting and finance studies have documented market overreaction or underreaction to a group of firms that go through particular corporate events.[5] What I find in this paper is not mispricing of warning firms relative to nonwarning firms, but mispricing *within* the warning group and especially *within* the nonwarning group. This finding is unique in the literature.

The third contribution of the study is the application of the Heckman model. Although the model is not new to the accounting literature[6], my study uses it in three new ways. Foremost, I decompose the observed between-group return difference into the warning effect and the self-selection effect. This decomposition is fundamental in studies that address self-selection. My study is the first that explicitly uses this relation to design and interpret tests.

[5] For example, earnings announcement (Bernard and Thomas 1989, 1990), dividend initiations and omissions (Michaely, Thaler and Womack 1995), IPO (Loughran and Ritter 1995, 2000), tender offer (Ikenberry, Lakonishok and Vermaelon 1995), stock splits (Ikenberry and Ramnath 2002), etc.

[6] See Shehata (1991), Hogan (1997), Leuz and Verrecchia (2000), Beatty, Ramesh and Weber (2002), Weber and Willenborg (2003), Khurana and Raman (2004), etc.

Furthermore, as Heckman pointed out in his Nobel Lecture (Heckman 2001), unobservable heterogeneity is widespread; statistical inferences may be incorrect if this issue is not addressed adequately. My study specifically tests the price effects of other bad news, where such news varies between the warning and nonwarning groups as well as within each group. This method can be used to examine other interesting accounting issues. Finally, my study serves as an example of how a failure to control for self-selection leads to dramatically different conclusions in studies where selection issues are apparent. Maddala (1991, 799 and 801) pointed out that the accounting studies that use self-selection models "thus far do not show any strong evidence of selection bias" because the conclusion with self-selection control is the same as the conclusion without such a control; therefore, "by definition, there is no selection bias." His criticism remains applicable to the subsequently published accounting studies that use selection models. My study shows that *without* the control for self-selection, one would conclude that openness is penalized, when in fact it is not.

The rest of the paper proceeds as follows. Section II reviews prior research on the motives for bad-news disclosure and on investors' responses to warnings. I next present the econometric model, empirical predictions, variables identifications and measurements, data, and test results sequentially. Section VIII concludes.

II. PRIOR RESEARCH

The most popular motive for voluntary bad-news disclosure is to reduce the expected settlement costs by shortening the class-action period (Skinner 1994, 1997). KL find evidence that is consistent with this argument. Using 219 warning events about large earnings surprises, they find that, in the face of bad news, a firm's characteristics suggesting higher litigation risk or costs are positively associated with the likelihood of warning. Other studies report mixed evidence on the argument. For example, Soffer, Thiagarajan and Walther (2000) and Baginski, Hassell and Kimbrough (2002) provide supportive evidence, whereas Francis, Philbrick and Schipper (1994) and Johnson, Kasznik and Nelson (2001) do not.

The second motive for voluntary bad-news disclosure is to maintain a strong reputation with analysts and fund managers (Skinner 1994). This argument implicitly views a firm's disclosure decision as a mid-game phenomenon in a multi-period game. Following this conjecture, I expect that firms with many voluntary disclosures in the past are more likely to warn in the current quarter to maintain a reputation for transparency. Similarly, firms with higher analyst following have a higher propensity of issuing warnings because the damage caused by being silent would be greater.[7]

Several studies have examined the capital market consequences of warnings. KL find that warning firms experience a larger price cut than nonwarning firms in the window that covers both the warning and subsequent earnings announcement.[8] KL tentatively attribute this finding to the possibility that warnings signal a permanent earnings decline. They propose market overreaction as an alternative explanation (p.132).

Two subsequent studies examine a research question similar to mine. Shu (2003) uses 104 warning firms that have a large earnings shortfall in the 1994-1995 quarters. She finds that warning firms' short-term returns are weakly significantly lower than nonwarning firms', but the warning effect is positive after self-selection is controlled for. Xu (2003) collects 151 warnings about a large earnings shortfall during 1991-1994. She finds that warning firms

---

[7] The expectations management literature (Matsumoto 2002; Bartov, Givoly and Hayn 2002) suggests that managers have an incentive to guide market expectations downward so that they can otherwise meet or beat the market at the earnings announcement date. After controlling for future earnings information impounded in stock prices, Kasznik and McNichols (2002) find that the market rewards only the firms that have been *consistently* meeting or beating the market. A public warning conveys that the meet-or-beat streak, if it exists, cannot continue; therefore, using public warnings for the meet-or-beat purpose is implausible.

[8] In a large sample during 1995-1999, Atiase, Supattarakul and Tse (2006) report a similar finding for firms with small negative earnings surprises.

have larger downward analyst revisions and lower operating income than nonwarning firms in the year after the event quarter, supporting KL's permanent-earnings-decline argument. In the short-term return test, she finds that warning firms have weakly significantly lower returns than nonwarning firms both before and after the control for self-selection. Furthermore, she reports that warning firms earn lower excess returns than nonwarning firms over a size-M/B-momentum benchmark in the 12, 24, and 36 months after the event-quarter, concluding that investors have, in fact, underreacted to warnings in the short term.

The above studies as a whole do not provide a clear answer to my research question. In the short-term window Shu (2003) and Xu (2003) provide contradictory evidence. In the long-term window Xu (2003) does not control for firms' self-selection. It is thus unclear whether a firm is ultimately worse off for having warned, given its risks, earnings news, and nonearnings news known to managers at the time of the warning decision.

III. ECONOMETRIC MODEL

To determine whether the decision to warn is penalized by investors, I use the Heckman selection framework (Heckman 1979, 2001) to control for the effect of unobservable other bad news. My application of the model is new in three ways that require discussions in this section. First, I allow for differential investor responses (i.e. coefficients) to firms within the warning group and within the nonwarning group, whereas previous studies typically use a treatment-effect model that constrains the responses to be the same (Greene 2003). Second, by comparing self-selection terms in the short-term tests with those in the long-term tests, I draw inferences about investors' behavior, whereas previous studies generally stop short of interpreting the term(s). Last, I decompose the observed between-group return difference into the warning effect and the self-selection effect, demonstrating why KL's result cannot be interpreted as a market penalty for warning.

Set Up

I model investors' decisions and managers' warning choice in the following system of equations:

$$R_{1i} = \alpha_1 + X_i\beta + v_{1i} \qquad \text{(if } Warn_i^* > 0\text{)} \qquad (1)$$

$$R_{0i} = \alpha_0 + X_i\beta + v_{0i} \qquad \text{(if } Warn_i^* \leq 0\text{)} \qquad (2)$$

$$Warn_i^* = Z_i\gamma + \varepsilon_i \qquad (3)$$

In the first two equations, *R* is the stock return and *X* is the vector of asset-pricing control variables that include common risk factors, accounting characteristics that likely capture risks, and earnings news. Here, v_1 and v_0 are price-relevant nonearnings news that investors use but that is unobservable to researchers. Equation (3) is the warning model. A manager's decision process is unobservable (i.e. $Warn_i^*$ is a latent variable.). What researchers can observe is whether a firm warns (*Warn* =1, when $Warn_i^*$ exceeds the threshold of 0) or does not warn (*Warn* = 0, when $Warn_i^*$ stays below the threshold). I assume that *Z* includes all the factors that affect a manager's warning decision and are observable to researchers, such as the earnings shortfall. So, ε represents the factors that affect the manager's decision but are unobservable to researchers, for example, nonearnings news. Given that a firm has warned, ε_i is likely to be high when $Z_i\gamma$ is low, thereby triggering the threshold. Given that a firm did not warn, ε_i is likely to be low when $Z_i\gamma$ is high, making the firm stay below the threshold. Therefore, within both the warning group and the nonwarning group, high ε_i is associated with low $Z_i\gamma$ and ε_i varies within each group.

The three unobservables v_1, v_0, and ε are the focus of the Heckman model. If the nonearnings news represented by v_1 and v_0 does not intersect at all with the factors represented by ε, the whole system collapses to traditional standalone regressions. Otherwise, (1) and (2) are misspecified. Assume that managers are more likely to warn when they have more other bad news (for investors) and less likely to warn when they have more other good news (i.e. less other bad news). The following statements then hold: (i) ε is higher in the presence of more other bad news, (ii) the intersection of the factors represented by ε and v_1 (or v_0) is other bad news, and (iii) ε and v_1 (or v_0) are negatively correlated.

Interpretation of the Self-Selection Terms

Further, assume that the warning model can be implemented by fitting the data into a cumulative probability function of a normal distribution and that (ε, v_1) and (ε, v_0) each follow a joint-normal distribution with covariances $\sigma_{\varepsilon v_1}$ and $\sigma_{\varepsilon v_0}$, respectively. Given that a firm has warned, its valuation should be (4); given that a firm did not warn, (5) should be used.[9] Here, $\phi(.)$ is the p.d.f. and $\Phi(.)$ is the c.d.f. of the standard normal distribution.

$$E(R_{1i} |_{warn_i=1}) = \alpha_1 + X_i\beta + E(v_{1i} |_{\varepsilon_i > -Z_i\gamma}) = \alpha_1 + X_i\beta + \sigma_{\varepsilon v_1} \frac{\phi(Z_i\gamma)}{\Phi(Z_i\gamma)} \qquad (4)$$

$$E(R_{0i} |_{warn_i=0}) = \alpha_0 + X_i\beta + E(v_{0i} |_{\varepsilon_i \le -Z_i\gamma}) = \alpha_0 + X_i\beta + \sigma_{\varepsilon v_0} \frac{-\phi(Z_i\gamma)}{1-\Phi(Z_i\gamma)} \qquad (5)$$

The last terms in (4) and (5) are investors' price adjustments for a firm's other bad news. Under the assumption that firms are more (less) likely to warn when they have more (less) bad news, both $\sigma_{\varepsilon v_1}$ and $\sigma_{\varepsilon v_0}$ should be negative if investors can fully adjust stock prices for other bad news. The adjustment for a warning firm is therefore downward and that for a

[9] The properties of truncated normal distributions are used in deriving (4) and (5). See Greene (2003, 788).

nonwarning firm upward. Moreover, $\frac{\phi(Z_i\gamma)}{\Phi(Z_i\gamma)}$ decreases with $Z_i\gamma$ and thus increases with ε_i; $\frac{\phi(Z_i\gamma)}{1-\Phi(Z_i\gamma)}$ increases with $Z_i\gamma$ and thus decreases with ε_i (Appendix A). As a result, within the warning group, the firms with more other bad news (i.e. a higher ε_i) receive a larger price cut than those with less other bad news. Within the nonwarning group, the firms with more other good news (i.e. less other bad news) receive a larger price increase. For this reason, the last terms in (4) and (5) are referred to as the "self-selection correction terms."

Empirical Model and the Decomposition Relation

Define *Mill* (the inverse Mills ratio) as $\frac{\phi(Z_i\gamma)}{\Phi(Z_i\gamma)}$ for a warning firm and $\frac{-\phi(Z_i\gamma)}{1-\Phi(Z_i\gamma)}$ for a nonwarning firm. Adding the self-selection correction terms to (1) and (2) corrects the model misspecification. Stacking up the corrected regressions yields Equation (6), where $\alpha_{TT} = \alpha_0$ and $\theta_{TT} = \alpha_1 - \alpha_0$. Equation (6) is my empirical model, in which θ_{TT} estimates the warning effect.

$$R_i = \alpha_{TT} + \theta_{TT}\, Warn_i + \beta X_i + \sigma_{\varepsilon v_1} Mill_i * Warn_i + \sigma_{\varepsilon v_0} Mill_i * (1 - Warn_i) + u_i \qquad (6)$$

KL estimate the coefficient on *Warn* when the self-selection correction terms are *excluded*. This coefficient, referred to as θ_{OLS}, measures the between-group return difference after risks and earnings news are controlled for. As shown in (7), θ_{OLS} has two components: θ_{TT} and the self-selection effect ("SS") (Greene 2003, 788).[10] In other words, θ_{TT} is the

[10] $\theta_{OLS} \equiv$ E(R1|warn=1) – E(R0|warn=0) = [E(R1|warn=1) – E(R0|warn=1)] + [E(R0|warn=1) – E(R0|warn=0)]
Adding and subtracting of E (R0|warn=1) makes this relation clear. On the right-hand side, the first bracket is θ_{TT}, generally called the "treatment effect on the treated group;" the second bracket is SS (Heckman 2001, 718).

remaining between-group return difference after the price effect of other bad news is removed.

$$\theta_{OLS} = \theta_{TT} + E[\sigma_{\varepsilon v_1} \frac{\phi(Z_i\gamma)}{\Phi(Z_i\gamma)} - \sigma_{\varepsilon v_0} \frac{-\phi(Z_i\gamma)}{1-\Phi(Z_i\gamma)}] \tag{7}$$

IV. EMPIRICAL PREDICTIONS

Overall, I predict that warnings are not penalized by investors *in the long run* (i.e. θ_{TT} =0 in the long-term window) because warnings are voluntary.[11] Otherwise, managers who are presumably rational in making disclosure decisions would stop issuing warnings and the warning phenomenon would disappear. I expect four major market scenarios that are consistent with this prediction.[12] These scenarios are organized around (i) whether self-selection exists and (ii) whether stock prices behave as if investors process news efficiently. In Figure 3, I summarize the scenarios and my predictions for θ_{OLS} , θ_{TT} , and SS in the short- and long-term windows in each scenario. In the following, I first explain what types of inefficient market behavior to expect and then discuss the predictions in each scenario.

Inefficient Market Behavior – Overreaction to Warnings

Investors are often panicked by bad news because a risk-averse individual's value function is commonly convex for losses (Kahneman and Tversky 1979). The market may be more alarmed by warnings than by negative earnings news released by nonwarning firms in earnings announcements. For a small number of firms, a warning only foreshadows what is to

[11] Investors require higher returns from the firms for which public information is lacking (Klein and Bawa 1976; Barry and Brown 1985; Easley and O'Hara 2004). Thus, firms that are forthright with investors receive a market reward. The reward for warnings may be too small to be empirically detected, because a warning is only one disclosure and the incremental transparency is minimal if a firm has been open in the past. Tucker (2006) finds that, relative to warnings firms, nonwarning firms lose analyst coverage after failing to warn.

[12] In an unreported post-event return test, I find no evidence that investors underreact to nonwarning firms' event-quarter earnings surprise. For brevity, this scenario is not proposed in this section.

come, such as deteriorating operations, earnings restatements, SEC investigations, and lawsuits. For example, the warnings by Motorola on July 10, 1996; Gillette on August 14, 1997; and Hewlett-Packard on May 13, 1998 were only the beginning of a series of strategic mishaps and operational failures. The media coverage of such firms may cause investors to overestimate the proportion of troubled firms among warning firms and overreact to warnings (e.g. the representativeness bias in psychology).

Inefficient Market Behavior – Incomplete or Incorrect Adjustments for Other Bad News

Traditional economics assumes that agents are globally rational (i.e. they are endowed with information and unlimited ability to process information). In reality, investors are perhaps bounded rational – they make optimal decisions within the constraints of information costs (Simon 1955, 1959; March 1978). Consequently, price discovery takes time as information gathering and digestion evolves (Gonedes 1976; Lee 2001; O'Hara 2003).[13]

By the end of the short-term window, investors may observe or infer other bad news about each firm. The direct impact of other bad news on future cash flows is often unclear, so investors may need extra time to fully understand the implications of other bad news. Furthermore, investors' short-term price adjustments for other bad news may be incomplete or incorrect to a larger extent for nonwarning firms than for warning firms for two reasons. First, nonwarning firms' negative earnings surprises, released at the earnings announcement date, arrive much later than earnings warnings. Second, investors may pay limited attention to an earnings shortfall disclosed in an earnings announcement because they are occupied with digesting many other firms' announcements (Simon 1978; Hirshleifer and Teoh 2003).

[13] When the searching and processing of information is costly, if such information is impounded in stock price immediately, there would be no demand for such information (Grossman and Stiglitz 1980).

Scenario A – Efficient Market and No Self-Selection

If managers do not consider nonearnings news when deciding whether to warn, there is no self-selection issue and therefore the self-selection effect SS is 0 in both the short term and long term. In the absence of SS, the between-group return difference θ_{OLS} is the same as the warning effect θ_{TT}, which is predicted to be 0 in the long run. Thus, in this scenario θ_{OLS}, θ_{TT}, and SS are all predicted to be 0 in the long run. If investors fully process news in the event quarter, the short-term returns should be no different from the long-term returns, so my predictions for the short-term θ_{OLS}, θ_{TT}, and SS in this scenario are also 0. Note that KL's finding of a negative θ_{OLS} indicates that Scenario A is unlikely. I discuss this scenario mainly for benchmarking.

Scenario B – Inefficient Market and No Self-Selection

Investors may overreact to warnings. Compared with Scenario A, θ_{OLS} in Scenario B is predicted to be negative in the short term because of the overreaction. Market overreaction is doomed to be corrected in the long term; therefore, θ_{OLS} should be 0 in the long term. Because self-selection is not an issue in this scenario, SS is 0 and $\theta_{TT} = \theta_{OLS}$ in both the short term and the long term.

Scenario C – Efficient Market and Self-Selection

Assume that managers are more (less) likely to warn when they have more (less) other bad news. When investors eventually observe, infer, and digest such news, the average return of warning firms should be lower than that of nonwarning firms even if both have the same risks and earnings news; that is, SS is negative in the long run. Because my overall prediction for the long-run θ_{TT} is 0, θ_{OLS} is expected to be equal to SS and both are negative in the long run. In this efficient market scenario, my predictions for the short-term θ_{OLS}, θ_{TT}, and SS are the same as those for the long term.

Scenario D – Inefficient Market and Self-Selection

This scenario deviates from Scenario C in that investors do *not* fully infer and process other bad news in the short term. All the predictions for the long term remain the same as those in Scenario C. For the short term θ_{OLS} is likely to be negative based on KL's finding. Because SS is the difference between investors' average price adjustment for warning firms ($\sigma_{\varepsilon v_1} \frac{\phi(Z_i\gamma)}{\Phi(Z_i\gamma)}$) and that for nonwarning firms ($\sigma_{\varepsilon v_0} \frac{-\phi(Z_i\gamma)}{1-\Phi(Z_i\gamma)}$), SS depends on the sign and magnitude of $\sigma_{\varepsilon v_1}$ and $\sigma_{\varepsilon v_0}$. Thus, the prediction for the short-term SS is unclear, as is the prediction for the short-term θ_{TT}, which is the residual in θ_{OLS} after SS is removed.

V. VARIABLES

Dependent Variables

The dependent variable in my tests is a firm's buy-and-hold return over various windows.[14] The short-term return (R^{Short}) is measured from the beginning of the third fiscal month of the event quarter to 5 days after the event-quarter earnings announcement date. The long-term window extends this window by 1, 2, 3, 6, 9, or 12 months (R^{Long1}, R^{Long2}, R^{Long3}, etc.) after the end of the event-quarter earnings-announcement month. I use various lengths

[14] I prefer buy-and-hold to cumulative returns because the former is closer to the true payoff. For example, a stock is traded for $10, $5, and $10 at t=1, 2, and 3, respectively. In this example, the buy-and-hold return is [(5/10)*(10/5) –1] = 0%, whereas the cumulative return is (–0.5+1.0) =50%.

because the speed of price adjustments is unknown. I stop at 12 months out of concern for potential measurement-error problems in long-run returns (Fama 1998, 291).

Control Variables – Risk Factors

The commonly recognized risk factors in the finance literature are market risk, firm size, market-to-book ("M/B"), momentum, and industry. I use a portfolio approach to control for these risks. At the beginning of each month, I form beta deciles, 20 size groups, M/B deciles, momentum deciles, and 48 Fama-French (1997) industry groups using all US common stocks covered by CRSP and Compustat. Beta is the coefficient on market returns in the market model that uses daily returns in the 1-year period before that month. Size is the beginning-of-month market value of equity. M/B is the ratio of the beginning-of-month market value of equity over the book value of equity reported in the most recent quarter. Return momentum is measured by the cumulative stock return in the past 6 months.

For the control of each risk factor, I assign a sample observation to a portfolio to which the firm belongs at the beginning of the return window. I purge the sample firm-quarters from the benchmark portfolios (Loughran and Ritter 2000, 372 and 382). From each benchmark portfolio, I randomly choose 100 members, calculate the buy-and-hold returns for each member over the same holding period as the dependent variable, delete the top and bottom 2% of the distribution, and use the mean as the control portfolio return, denoted as R^{beta}, R^{size}, R^{mb}, R^{mom}, and R^{ind}, respectively. [15]

[15] Finance studies commonly control for risks by subtracting the control portfolio (the same size, M/B, and momentum group as the sample firm) return from the sample firm's return and use the excess return as the dependent variable. This approach yields a control portfolio with a small number of firms when the number of risk factors is large. For example, if I first sort all the public companies (about 10,000) into five size groups, then five M/B groups for each size group, and finally three momentum groups for each M/B group, I only have 133 firms available for further sorting into 10 beta and 48 industry groups.

Control Variables – Accounting Characteristics

Previous research finds that accounting information helps investors evaluate risks (Hamada 1969; Beaver, Kettler and Scholes 1970). I control for three accounting metrics so that in the test a firm's stock return is compared with the return of firms with similar accounting characteristics, in case these metrics capture risks. The three accounting metrics are leverage, accounting return on assets, and earnings volatility. They represent three aspects of a firm's business: financial structure, performance, and performance variability. *Leverage* is the ratio of total liabilities over total assets, averaged in the four quarters before the event quarter. *ROA* is the ratio of earnings before extraordinary items over beginning-of-quarter assets, averaged in the four quarters before the event quarter. *EarnVolt* is the standard deviation of ROA in the eight quarters before the event quarter.

Control Variables – Earnings Innovations

I use two information variables. The first variable is the event-quarter earnings surprise (*Surprise*), measured as the difference between the forthcoming earnings and analyst consensus (both from I/B/E/S). For warning firms, the consensus is the most recent one compiled before the warning; for nonwarning firms, the consensus is the last one before the beginning of the return window.

The second variable is future earnings change (*FutureEPS*). I control for *FutureEPS* in the short-term window because price leads earnings: a portion of current price change is due to information about future earnings before they are recognized in accounting books (Beaver, Lambert and Morse 1980). I control for *FutureEPS* in the long-term windows because stock prices after the event quarter are affected by the portion of future earnings change that investors did not anticipate in the event quarter (Elton 1999, 1214). I measure *FutureEPS* as the change in average diluted EPS before extraordinary items from the four quarters before the event to the four quarters after the event. As with KL, I deflate both earnings variables by the split-adjusted stock price at the beginning of the event quarter.

Control Variable – Mill

I identify the observable variables that reflect managers' litigation-, reputation-, and earnings-torpedo-related motives for issuing warnings. For the litigation-related motive, I include (i) general litigation risk (*LitigRisk*), (ii) expected legal settlement costs (*LogMVE* and *LogSurprise*), and (iii) specific litigation risk from firms' failure to update a previously issued projection for the event quarter (*Forecast*). *LitigRisk* is the probability of being sued (Appendix B). Due to skewness, I use the fractional rankings of this probability in the full-sample for the empirical tests.

Expected legal costs are higher for larger firms and firms with more severe bad news. The two components arguably have a multiplicative relation (Skinner 1997). Thus, I include the log transformations of both components. *LogMVE* is the log transformation of market value of equity at the beginning of the event quarter. *LogSurprise* is the log transformation of the absolute value of *Surprise*. If a firm has issued an earnings forecast about the event quarter, managers may be concerned about potential lawsuits from failing to update or correct the previous projection.[16] This situation presents a specific litigation risk for managers; I include a dummy variable *Forecast* to capture this risk. The variable is 1 if a firm has issued a forecast about the event quarter before the third fiscal month and 0 otherwise.

[16] In the 1979 safe-harbor, firms had no duty to update a projection included in a previous SEC filing, but were required to correct the previous projection once it was found false or misleading in light of subsequent events. The Private Securities Litigation Reform Act of 1995 maintains that firms have no duty to update previously released forward-looking information, but the law is quiet about whether firms have a duty for correction. The distinction between "update" and "correct" is ambiguous, causing controversy among legal experts.

For the reputation-related motive, I use past disclosure frequency (*PastDisclosure*) and pre-event analyst following (*PastFollow*). *PastDisclosure* is the number of positive or negative guidelines about quarterly earnings issued by a firm in the 360 days before it announces earnings for the quarter before the event quarter. If First Call does not have records for a firm in this period, the prior disclosure level is considered 0. *PastFollow* is the average number of analysts whose forecasts are included in the most recent consensus compiled before earnings announcements for the four quarters before the event. As with Frankel and Li (2004), analyst coverage is considered 0 if a firm is not covered by I/B/E/S. I convert *PastDisclosure* and *PastFollow* each into fractional rankings among all Compustat, CRSP, and I/B/E/S firms in the event quarter. This conversion controls for potential time trends in First Call, media, and analyst coverage and fluctuations in firms' disclosures due to market conditions or the legal environment.

For growth firms' motive for softening the earnings-torpedo effect, I use M/B (Skinner and Sloan 2002; Anilowski, Feng and Skinner 2006). Finally, I control for earnings volatility because managers with more volatile earnings may feel less need to warn investors of an earnings fluctuation. Equation (8) implements the warning model from which *Mill* is calculated. [17]

$$\Pr(\text{Warn}_i = 1) = \Phi\,(a_0 + a_1\,\text{LitigRisk}_i + a_2\,\text{LogMVE}_i + a_3\,\text{LogSurprise}_i + a_4\,\text{Forecast}_{i\,i} + a_5\,\text{PastDisclosure}_i + a_6\,\text{PastFollow}_i + a_7\,\text{M/B}_i + a_8\,\text{EarnVolt}_i + \varepsilon_i) \quad (8)$$

To summarize, Equation (9) is the return regression that I estimate in various windows.

$$\begin{aligned} \text{R}_{\,i} = c_0 + \theta_{TT}\,\text{Warn}_{\,i} + c_2\,\text{R}^{\text{beta}}{}_{i} + c_3\,\text{R}^{\text{size}}{}_{i} + c_4\,\text{R}^{\text{mb}}{}_{i} + c_5\,\text{R}^{\text{mom}}{}_{i} + c_6\,\text{R}^{\text{ind}}{}_{i} \\ + c_7\,\text{Leverage}_{\,i} + c_8\,\text{ROA}_{\,i} + c_9\,\text{EarnVolt}_{\,i} + c_{10}\,\text{Surprise}_{\,i} + c_{11}\,\text{FutureEPS}_{\,I} \quad (9) \\ + \sigma_{\varepsilon v_1}\,\text{Mill}_{\,i} * \text{Warn}_{\,i} + \sigma_{\varepsilon v_0}\,\text{Mill}_{\,i} * (1\text{-Warn}_{\,i}) + \text{u}_{\,i} \end{aligned}$$

---

[17] Aboody and Kasznik (2000) find that managers have an incentive to release pessimistic earnings forecasts before the option award date. I control for option grant but find it insignificant (unreported). Because the use of option data substantially reduces the sample and the variable is insignificant, I do not include this variable.

VI. DATA

Data Collection

I collect earnings warnings from the First Call Company Issued Guideline (CIG) database and use 1996Q1 to 2003Q2 (calendarized) as my sample period.[18] This period starts with 1996 because the passage of the Private Securities Litigation Reform Act of 1995 expanded the "safe harbor" protection to firms that issue forward-looking information, raised the bar for class action suits, and thus changed firms' legal and information environment. During the sample period, US companies issued 8,692 negative guidelines about quarterly earnings.[19]

Table 1 summarizes warning event collection. I exclude 3,214 negative quarterly guidelines issued before the third fiscal month because my study focuses on warnings, not forecasts.[20] I also exclude 469 extreme small-news events, defined as the event-quarter earnings surprise higher than –0.001 because managers may be unaware of such a small shortfall. To avoid spurious results by penny stocks, which are extremely illiquid and for which market arbitrage is weak (D'Avolio 2002), I delete 24 events whose beginning-of-event-quarter stock price is less than $2. The final number of warnings is 3,869.

The nonwarning observations are the firm-quarters in which the forthcoming earnings are lower than analysts' consensus before the third fiscal month but the firms do not warn. If, according to First Call, a firm issues negative guidance about sales, cash flows, earnings

[18] "Quarter" is calendarized. A firm's fiscal quarter is relabeled to the calendar quarter with which it overlaps most. This procedure improves the control for time effect because only about 67% of the firms covered by Compustat end their fiscal years on December 31 and the next most popular fiscal-year-end is June 30.

[19] First Call collects company guidelines from press releases and interviews, compares the disclosure with existing market expectations, and codes them as "D" for negative news for Variable "Code2" in the CIG data file. To check First Call's positive/negative classification accuracy, I searched the Factiva news database for 100 events randomly chosen from the First Call 2004 data. I found confirmation in the news for 87 events. Among the 13 unconfirmed events, I could not find guidance news for 7 events; for the remaining 6 events, I found news that contradicted First Call's classification. My subsequent deletion of events with earnings surprise that is higher than –0.001 likely excludes the misclassified events.

[20] Firms issue negative guidelines about quarterly earnings around two time points. The first time point is at the end of the first fiscal month and I refer to these disclosures as "forecasts." The second time point is at the fiscal quarter end and I refer to these disclosures as "warnings."

growth, or EBITDA, this firm-quarter is excluded from the nonwarning group. I then follow the same procedures used for warning events to exclude extremely small news, penny stocks, and the observations that have insufficient data. These procedures result in 23,158 nonwarning observations.

Descriptive Statistics

Table 2 describes the warning and nonwarning groups. Warnings are issued by almost all 48 Fama–French industries. Business services, retail, chips, computer, and machinery industries have the highest number of warnings (Panel A).

To compare with prior research, in Panel B I report the cumulative market-adjusted returns in the warning and earnings announcement windows as well as in the short-term window. Warning firms experience a mean return of –13.2% during the 3 trading days around the warning but the returns around the subsequent earnings announcement date is close to 0, suggesting that warning is a timing issue rather than an issue of piecemeal disclosure. For those that have waited to give bad news until earnings announcement, the average 3-trading-day return is –2.1%. Warning and nonwarning firms experience a mean return of –19.5% and –8.4% in the short-term window, respectively. These returns are more negative than the 3-day event returns, probably because of intra-industry information transfer (i.e. a price decline upon peers' warnings) and the arrival of other negative news. [21]

Panel C describes the main variables used in the empirical tests. Warning firms have lower buy-and-hold returns, worse event-quarter earnings news (median), and a larger decline in future earnings than nonwarning firms. As Barber and Lyon (1997) notes, returns tend to be positively skewed when the window is long (e.g. R^{Long12}). Panel D reports the pairwise

[21] The "cockroach theory" is popular on Wall Street: there is rarely one adverse surprise; like cockroaches, many surprises lie hidden for every one that is visible.

correlations of the main variables. As predicted, realized returns are positively correlated with both the event-quarter earnings surprise and the change in future earnings.

Panel E presents the correlations between firms' buy-and-hold returns and their control portfolio returns. For brevity, I only report the 3-month extension window; the correlations for other windows are similar. The correlation between realized returns and industry control portfolio returns is the highest among all correlations involving realized returns; beta control portfolio returns come next. The correlations between the control portfolio returns themselves range from 0.660 to 0.794, yet multicollinearity is not a concern in the multivariate tests according to my variance-inflation-factor analysis (unreported).

VII. RESULTS

In this section I first report the warning model estimation. I next present the test results in the short- and long-term windows when self-selection is *not* controlled for. As the primary results of this study, I then report the tests in both windows when self-selection *is* controlled for and briefly note the robustness tests concerning subsamples and model specifications. Finally, I examine post-event returns and evaluate trading strategies to provide supplementary evidence for the primary results.

Warning Model Estimation

Table 3 reports the estimation results. As predicted, firms are more likely to warn if they are riskier, larger, have a larger earning shortfall, have issued a forecast for the event quarter early on, gave more disclosures, and had higher analyst following in the past quarters. Firms with more volatile earnings are less likely to warn. The pseudo R^2 is 9.15%.

Return Tests without Control for Self-Selection

I first estimate KL's short-term return regression (Table 4, Panel A). The coefficient on *Warn* is significantly negative, consistent with KL and *contradicting Market Scenario A*. The earnings response coefficient is significantly positive at 0.944, whereas KL, Shu (2003), and Xu (2003) all report insignificant coefficients probably because of their small samples.

Panel B of Table 4 reports on my return regression without the control for self-selection. The coefficient on *Warn*, θ_{OLS}, is not only significantly negative but is also surprisingly close to –10% across all the windows (including the 6-, 9-, and 12-month extensions, unreported). In particular, θ_{OLS} is –10.1% for the short-term window and –9.7% for the 3-month-extension long window. The fact that θ_{OLS} does not change as the window is extended suggests that investors have initially correctly valued the warning group relative to the nonwarning group. The negative θ_{OLS} in both the short term and long term *invalidates Market Scenario B*.

Return Tests with Control for Self-Selection

Panel C of Table 4 reports on my return regression when the self-selection correction terms are included and the t-statistics are robust to heteroskedasticity (Huber/White/Sandwich standard error estimator). In Panel D of Table 4, I present two alternative estimations that additionally allow for cross-sectional error correlations.[22] I draw inferences from Panel C not from Panel D because the latter uses only 30 cross-sections and, as a result of low power, the tests may be biased toward finding no warning effect in the long run – my main prediction.

Panel C shows that the coefficient on *Warn*, the warning effect θ_{TT}, is significantly negative in the short term (coefficient = –6.4%, t-statistic = –4.20). The negative

[22] The results from allowing for within-firm error correlations are very similar to Panel C.

θ_{TT} indicates that warning firms earn lower returns in the short term than those that have similar risks, earnings news, and *nonearnings news* but do not warn; that is, the market penalizes the act of warning in the short term. The penalty, however, declines as the return window is extended and disappears for the 2- and 3-month extensions (also for the 6-, 9-, and 12-month extensions, unreported).

Using the decomposition in Equation (7), I estimate the self-selection effect SS to be –3.7% for the short-term window and –10.8% for the 3-month extension after subtracting θ_{TT} from θ_{OLS}.[23] The negative SS suggests that, on average, warning firms have a larger amount of other bad news than nonwarning firms. The patterns for θ_{OLS}, θ_{TT}, and SS indicate that the lower return of warning firms than that of nonwarning firms in the short term is due to both a negative warning effect and a negative self-selection effect, but that in the long run is due purely to self-selection. *The evidence is consistent with Scenario D, not with Scenario C.*

Panel C also shows the coefficients on *Mill* for the warning group and for the nonwarning group, respectively. Note that the coefficient on warning firms' *Mill* is the estimate of $\sigma_{\varepsilon v_1}$, the covariance of the error terms of the return regression (1) and the warning model (3). Similarly, the coefficient on nonwarning firms' *Mill* is the estimate of $\sigma_{\varepsilon v_0}$, the covariance of the error terms in the return regression (2) and the warning model (3). If firms are more (less)

[23] The table below summarizes the key results in Panels B and C of Table 4 as well as Table 5. Except for SS, "***" and "**" indicate statistical significance at 1% and 5%, respectively.

	Short Term	Long Term (3-month extension)	Post Event (3 months)
θ_{OLS}	–0.101***	–0.097***	0.006
θ_{TT}	–0.064***	0.011	0.078***
SS	–0.037	–0.108	–0.071
$\sigma_{\varepsilon v_1}$	–0.034***	–0.068***	–0.034**
$\sigma_{\varepsilon v_0}$	0.050***	–0.042**	–0.091***

likely to warn when they have more (less) other bad news and investors can fully observe, infer, and digest such news, both covariances should be negative. The tests show that the estimates of these two covariances change from the short term to the long term, suggesting within-group mispricing.

Specifically, $\sigma_{\varepsilon v_1}$ is significantly negative in both the short term (–0.034) and 3-month-extension long-term window (–0.068); however, the magnitude in the short term is only half of that in the long term. This result suggests that the positions that investors initially take to adjust warning firms' prices were correct but did not go far enough. For the nonwarning group, $\sigma_{\varepsilon v_0}$ is significantly positive in the short term (0.050) but becomes significantly negative for the 3-month-extension long-term window (–0.042). The reversal suggests that investors initially made a mistake in valuing other bad news within the nonwarning group.[24] In sum, investors are supposed to cut the stock prices of the firms that arguably have more bad news relative to those that have less both within the warning group and within the nonwarning group. Yet, the price cut did not happen in the short term within the nonwarning group (and the degree of the price cut within the warning group is insufficient). Therefore, a firm is worse off for having warned in the short term.

In unreported tests I partition the sample by Regulation Fair Disclosure ("FD") and by the magnitude of earnings surprise. In the pre-FD era, some firms might have warned privately but are misclassified to the nonwarning group. After FD, managers' private communication channels are legally suppressed, so the potential classification problem is mitigated. I find that the post-FD warning model indeed has a much higher pseudo R^2 (16.6%) than that obtained from the full sample and that the primary results largely hold in the both the pre- and

[24] I view the 3- rather than 2-month extension as the primary long-term window because $\sigma_{\varepsilon v_0}$ is significantly negative in the 3-month-extension long-term window as well as in the 3-month post-event window (Table 5).

post-FD subsamples. Partitioning by the earnings surprise, using 1% price-deflated earnings surprise as the cutoff, I obtain consistent results with the primary results.

The use of the Heckman model has a caveat: The results may be sensitive to the specification of Z because $Z_i\gamma$ determines the magnitude of price adjustments for self-selection within the warning group and within the nonwarning group. For example, if the warning model is weak, the self-selection correction terms would act like noise such that SS is 0 and $\theta_{TT} = \theta_{OLS}$. My evidence of significant SS alleviates this concern. Although I use the best warning model based on extant evidence, my study may be sensitive to including new warning factors identified from future theories. [25]

Supplementary Test – Post-Event Returns

I examine post-event returns to corroborate the primary results. The post-event windows (e.g. R^{Post1}, R^{Post2}, etc.) start on the sixth day following the event-quarter earnings announcement date and end at various points as the long windows end. Panel A of Table 5 shows that the post-event warning effect is significantly positive (coefficient = 0.078, t-statistic = 3.69 for the 3-month window). The positive warning effect in the post-event windows following the negative warning effect in the short-term window explains why the overall warning effect in the long run is nil. Moreover, the estimate of $\sigma_{\varepsilon v_0}$ is significantly negative, consistent with its reversal from being positive in the short term to negative in the long term, suggesting a correction of short-term mispricing within the nonwarning group.

[25] I check how the results are sensitive to the inclusion of already identified warning factors. When the warning model includes only litigation risk, firm size, and earnings surprise, the pseudo R^2 is 4.4% and the warning effect is significantly negative even in the long run. After adding *Forecast*, the pseudo R^2 is improved to 6.1% and the warning effect is qualitatively similar to my primary results. The warning effect is insensitive to further adding other identified warning factors. My primary results are also insensitive to adding *FutureEPS* to the warning model or excluding *FutureEPS*, *Leverage*, *ROA*, and *Earnvolt* from the return regression.

The estimate of $\sigma_{\varepsilon v_1}$ is significantly negative in the 3-month post-event window, suggesting that 3 months after the event quarter investors have better understanding of warning firms' other bad news even though their initial positions were correct.

In the second-to-last row, I *exclude* the self-selection correction terms and report the coefficient on *Warn*. The coefficient is insignificantly different from 0. The absence of return reversal or drift is consistent with my previous finding that investors' initial valuation of warning firms relative to nonwarning firms was correct. In the last row, I reestimate the model after further excluding *FutureEPS* and report the coefficient on *Warn*. Because all the variables are publicly known at the beginning of the return window, this coefficient essentially estimates the portfolio abnormal return from a zero-investment trading strategy of longing warning stocks and shorting nonwarning stocks after the event quarter. The insignificance of the coefficient indicates that the strategy based solely on the dichotomy of warning vs. nonwarning is unprofitable, confirming that warning firms as a group is correctly priced relative to the nonwarning group. [26]

Furthermore, I form a trading strategy to exploit within-group mispricing. Note that the predicted warning probability (*P*) increases with $Z_i\gamma$. Within the warning group and within the nonwarning group, low $Z_i\gamma$ is associated with high ε_i, so low *P* is associated with a large amount of other bad news. I sort warning firms and nonwarning firms separately into three groups (high, medium, and low) by *P*. If investors initially did not understand that within the warning group the firms with low *P* have a high likelihood of having a large amount of other

[26] This result differs from Xu (2003). To replicate her study, I retain only the size, M/B, and momentum factors in the post-event return regression and further retain only large-surprise firms. I find that the coefficient on *Warn* is significantly positive (coefficient = 0.025, t-statistic=2.42 for the 3-month window; coefficient = 0.040, t-statistic=1.77 for the 12-month window), contrary to the drift documented by Xu (2003). The difference in our findings may be due to the different sample period.

bad news, selling the firms with low *P* and buying those with high *P* after the event quarter will generate profit. A similar strategy within the nonwarning group should be profitable.

Panel B of Table 5 shows, by subtracting "Low *P*" returns from "High *P*," that this strategy earns a mean (median) abnormal return of 1.2% (2.4%) from the warning group and 2.2% (2.5%) from the nonwarning group. The profit from the nonwarning group is significantly positive (T = 4.54; Wilcoxon Z = 5.64); the profit from the warning group is weakly significantly positive in the Wilcoxon test (Z = 1.76). The evidence supports my finding in the primary tests that mispricing exists within the warning group and especially within the nonwarning group. When transaction costs are considered, however, this trading strategy probably yields no net profit. Thus, the evidence of these abnormal returns may not be inconsistent with market efficiency.

VIII. CONCLUSION

My study examines the capital market consequences of voluntary warnings. Using the Heckman selection model to control for price declines due to other bad news, I document three findings. First, warning firms on average have a larger amount of other bad news than nonwarning firms. Second, investors initially cannot fully infer and digest other bad news such that the short-term price adjustments for such news were incomplete within the warning group and incorrect within the nonwarning group. As a result, firms are worse off in the short term for issuing warnings. Openness, however, is ultimately not penalized in the long run. My study provides current, relevant evidence that should alleviate managers' concerns about a market penalty for openness. Moreover, the study demonstrates the importance of controlling for unobservable factors that influence both a firm's choice and the subsequent metric which researchers examine.

REFERENCES

Aboody, D., and R. Kasznik. 2000. CEO stock options awards and the timing of corporate voluntary disclosures. *Journal of Accounting and Economics* 29: 73–100.

Anilowski, C., M. Feng, and D. J. Skinner. 2006. Does earnings guidance affect market returns? The nature and information content of aggregate earnings guidance. Working paper.

Atiase R. K., S. Supattarakul, and S. Tse. 2006. Market reaction to earnings surprise warnings: The incremental role of shareholder litigation risk on the warning effect. *Journal of Accounting, Auditing and Finance* (Spring): 191-222.

Baginski, S., J. M. Hassell, and M. D. Kimbrough. 2002. The effect of legal environment on voluntary disclosure: Evidence from management earnings forecasts issued in U.S. and Canadian markets. *The Accounting Review* 77 (1): 25-50.

Barber, B. M., and J. D. Lyon. 1997. Detecting long-run abnormal stock returns: The empirical power and specification of test statistics. *Journal of Financial Economics* 43 (3): 341-372.

Barry, C., and S. Brown. 1985. Equilibrium information and security market equilibrium. *Journal of financial and Quantitative Analysis* 20 (4): 407-422.

Bartov, E., D. Givoly, and C. Hayn. 2002. The rewards to meeting or beating earnings expectations. *Journal of Accounting and Economics* 33 (2): 173-204.

Beaver, W., P. Kettler, and M. Scholes. 1970. The association between market determined and accounting determined risk measures. *The Accounting Review* 45 (4): 654-682.

Beaver, W., R. Lambert, and D. Morse. 1980. The information content of security prices. *Journal of Accounting and Economics* 2 (1): 3-28.

Beatty, A., K. Ramesh, and J. Weber. 2002. The importance of accounting changes in debt contracts: The cost of flexibility in covenant calculations. *Journal of Accounting and Economics* 33: 205-227.

Bernard, V. L., and J. K. Thomas. 1989. Post-earnings-announcement drift: Delayed price response or risk premium? *Journal of Accounting Research* 27 (3): 1-36.

_______. 1990. Evidence that stock prices do not fully reflect the implications of current earnings for future earnings. *Journal of Accounting and Economics* 13 (4): 305-340.

Core, J. E. 2001. A review of empirical disclosure literature: Discussion. *Journal of Accounting and Economics* 31 (1-3): 441-456.

D'Avolio, G. 2002. The market for borrowing stock. *Journal of Financial Economics* 66 (2-3): 271-306.

Easley, D., and M. O'Hara. 2004. Information and the cost of capital. *Journal of Finance* 59 (4): 1553-1583.

Elton, E. J. 1999. Expected returns, realized return, and asset pricing tests. *Journal of Finance* 54 (4): 1199-1220.

Fama, E. F. 1998. Market efficiency, long-term returns, and behavioral finance. *Journal of Financial Economics* 49 (3): 283–306.

Fama, E. F., and K. R. French. 1997. Industry costs of equity. *Journal of Financial Economics* 43 (2): 153–193.

Francis, J., D. Philbrick, and K. Schipper. 1994. Shareholder litigation and corporate disclosures. *Journal of Accounting Research* 32 (2): 137–164.

Frankel, R., and X. Li. 2004. Characteristics of a firm's information environment and the information asymmetry between insiders and outsiders. *Journal of Accounting and Economics* 37 (2): 229-259.

Gonedes, N. 1976. The capital market, the market for information, and external accounting. *Journal of Finance* 31 (2): 611-630.

Greene, W. 2003. *Econometric Analysis*. 5th edition. Prentice Hall.

Grossman, S. J., and J. E. Stiglitz. 1980. On the impossibility of informationally efficient markets. *The American Economic Review* 70 (3): 393-408.

Hamada, R. S. 1969**.** Portfolio analysis, market equilibrium and corporation finance. *Journal of Finance* 24 (1): 13-32.

Heckman, J. J. 1979. Sample selection bias as a specification error. *Econometrica* 47 (1): 153-161.

Heckman, J. J. 2001. Micro data, heterogeneity, and the evaluation of public policy: Nobel lecture. *Journal of Political Economy* 109 (4): 673-748.

Hirshleifer, D., and S. H. Teoh. 2003. Limited attention, information disclosure, and financial reporting. *Journal of Accounting and Economics* 36: 337-386.

Hogan, C. E. 1997. Costs and benefits of audit quality in the IPO market: A self-selection analysis. *The Accounting Review* 72 (1): 67-86.

Ikenberry, D., J. Lakonishok, and T. Vermaelen. 1995. Market underreaction to open market share repurchases. J*ournal of Financial Economics* 39: 181-208.

Ikenberry, D. L., and S. Ramnath. 2002. Underreaction to self-selected news events: The case of stock splits. *Review of Financial Studies* 15 (2): 489-526.

Johnson, M. F., R. Kasznik, and K. K. Nelson. 2001. The impact of securities litigation reform on the disclosure of forward-looking information by high technology firms. *Journal of Accounting Research* 39 (2): 297–327.

Kahneman, D., and A. Tversky. 1979. Prospect theory: An analysis of decision under risk. *Econometrica* 47 (2): 263-291.

Kasznik, R., and B. Lev. 1995. To warn or not to warn: Management disclosures in the face of an earnings surprise. *The Accounting Review* 70 (1): 113–134.

Kasznik, R., and M. F. McNichols. 2002. Does meeting earnings expectations matter? Evidence from analyst forecast revisions and share prices. *Journal of Accounting Research* 40 (3): 727–759.

Khurana, I., and K. K. Raman. 2004. Litigation risk and the financial reporting credibility of Big 4 versus Non-Big 4 audits: Evidence from Anglo-American countries. *The Accounting Review 79* (2): 473-495.

Klein, R., and V. Bawa. 1976. The effect of estimation risk on optimal portfolio choice. *Journal of Financial Economics* 3: 215-231.

Korajczyk, R., and R. Sadka. 2004. Are momentum profits robust to trading costs? *Journal of Finance* 39(3) (June): 1039-1082.

Lee, C. M. 2001. Market efficiency and accounting research: A discussion of 'capital market research in accounting' by S.P. Kothari. *Journal of Accounting and Economics* 31: 233-253.

Leuz, C., and R. E. Verrecchia. 2000. The economic consequences of increased disclosure. *Journal of Accounting Research* 38 (3): 91-124.

Loughran, T., and J. R. Ritter. 1995. The new issues puzzle. *Journal of Finance* 50 (1): 23-51.

Loughran, T., and J. R. Ritter. 2000. Uniformly least powerful tests of market efficiency. *Journal of Financial Economics* 55: 361-389.

Maddala, G. S. 1991. A perspective on the use of limited-dependent and qualitative variables models in accounting research. *The Accounting Review* 66 (4): 788-807.

March, J. G. 1978. Rounded rationality, ambiguity, and the engineering of choice. *The Bell Journal of Economics* 9 (2): 587-608.

Matsumoto, D. A. 2002. Management's incentives to avoid negative earnings surprises. *The Accounting Review* 77 (3): 483–514.

Michaely, R., R. H. Thaler, and K. L. Womack. 1995. Price reactions to dividend initiations and omissions: Overreaction or drift? *Journal of Finance 50*: 573-608.

O'Hara, M. 2003. Presidential address: Liquidity and price discovery. *Journal of Finance* 38 (4): 1335-1354.

Rogers, J. L., and P. C. Stocken. 2005. Credibility of management forecasts. *The Accounting Review* 80 (4): 1233-1260.

Shehata, M. 1991. Self-selection bias and the economic consequences of accounting regulation: An application of two-stage switching regression to SFAS No. 2. *The Accounting Review* 66 (4): 768-787.

Shu, S. 2003. Why do firms issue earnings in the face of earnings disappointments: A self-selection analysis. Working paper. Boston College.

Simon, H. A. 1955. A behavioral model of rational choice. *The Quarterly Journal of Economics* 69 (1): 69-118.

Simon, H. A. 1959. Theories of decision-making in economics and behavioral science. *The American Economic Review* 49 (3): 253-283.

Simon, H. A. 1978. Rationality as process and as product of thought. *The American Economic Review* 68 (2): 1-16.

Skinner, D. J. 1994. Why firms voluntarily disclose bad news? *Journal of Accounting Research* 32 (1): 38–60.

_______. 1997. Earnings disclosures and stockholder lawsuits. *Journal of Accounting and Economics* 23: 249–282.

Skinner, D. J., and R. G. Sloan. 2002. Earnings surprises, growth expectations, and stock returns or don't let an earnings torpedo sink your portfolio. *Review of Accounting Studies* 7: 289-312.

Soffer, L. C., S. R. Thiagarajan, and B. R. Walther. 2000. Earnings preannouncement strategies. *Review of Accounting Studies* 5: 5–26.

Tucker, J. W. 2006. Is silence golden? Earnings warnings and change in subsequent analyst following. Working paper. University of Florida.

Weber, J., and M. Willenborg. 2003. Do expert informational intermediaries add value? Evidence from auditors in microcap initial public offerings. *Journal of Accounting Research* 41 (4): 681-720.

Xu, W. 2003. Market reaction to warnings of negative earnings surprises: Further evidence. Working paper. University of Vanderbilt.

APPENDIX A
Statement and Proofs

Statements:

(A1): $\frac{\phi(x)}{\Phi(x)}$ is a decreasing function of x. (A2): $\frac{\phi(x)}{1-\Phi(x)}$ is an increasing function of x.

Here, $\phi(.)$ is the p.d.f. and $\Phi(.)$ is the c.d.f. of the standard normal distribution.

Proofs:

(A1): Suppose a random variable w that follows the standard normal distribution is truncated at x from above: E (w | w<x) = $-\frac{\phi(x)}{\Phi(x)}$ (Greene 2003. Equation 22-3b)

The mean of the truncated distribution is smaller than the truncation point x:
E (w | w<x) < x. Therefore, $\phi(x)+x\Phi(x)>0$ (A1a)

Take the derivative of $\frac{\phi(x)}{\Phi(x)}$ with respect to x and use A1a in the last step:

$$\frac{d(\frac{\phi(x)}{\Phi(x)})}{dx}=\frac{\phi'(x)\Phi(x)-\phi(x)\Phi'(x)}{\Phi^2(x)}=\frac{(-x)\phi(x)\Phi(x)-\phi(x)\phi(x)}{\Phi^2(x)}=\frac{-\phi(x)[\phi(x)+x\Phi(x)]}{\Phi^2(x)}<0$$

Thus, $\frac{\phi(x)}{\Phi(x)}$ is a decreasing function of x.

(A2): Suppose a random variable w that follows the standard normal distribution is truncated at x from below: E (w | w>x) = $\frac{\phi(x)}{1-\Phi(x)}$ (Greene 2003. Equation 22-3a)

The mean of the truncated distribution is greater than the truncation point x:
E (w | w>x) > x. Therefore, $\phi(x)-x[1-\Phi(x)]>0$ (A2a)

Take the derivative of $\frac{\phi(x)}{1-\Phi(x)}$ with respect to x and use A2a in the last step:

$$\frac{d(\frac{\phi(x)}{1-\Phi(x)})}{dx}=\frac{\phi'(x)[1-\Phi(x)]-\phi(x)[1-\Phi(x)]'}{[1-\Phi(x)]^2}=\frac{-x\phi(x)[1-\Phi(x)]-\phi(x)[-\phi(x)]}{[1-\Phi(x)]^2}$$

$$=\frac{\phi(x)\{\phi(x)-x[1-\Phi(x)]\}}{[1-\Phi(x)]^2}>0$$

Thus, $\frac{\phi(x)}{1-\Phi(x)}$ is an increasing function of x.

APPENDIX B
Litigation Risk Estimation

The litigation risk model in this paper is similar to those used by Rogers and Stocken (2005) and Johnson et al. (2001). The model estimation uses class-action filings data (1996-2000) obtained from the Stanford Securities Class Action Clearinghouse website. The dependent variable *Lawsuit* is 1 for any firm-year in which the firm is a defendant in a class action lawsuit filed in that year and 0 otherwise.

The explanatory variables include firm size, stock turnover, market beta, cumulative stock return, return volatility, and minimum return. For a litigated firm-year, the variables are measured in the 1-year period before the filing date. For a nonlitigated firm-year they are measured over the calendar year. *Size* is the log transformation of average daily market value of equity (in millions of dollars). *Turnover* is the average daily trading volume deflated by the number of shares outstanding. *Beta* is the coefficient on market returns in the market model. *CumRet* is the cumulative daily raw returns. *StdRet* is the standard deviation of daily raw returns. *MinRet* is the minimum daily raw return. Firm size, stock turnover, beta, and return volatility are predicted to be positively associated with litigation risk. Cumulative stock return and minimum return are predicted to be negatively associated with litigation risk.

The model includes three dummy variables for high-tech industries because prior research finds high-tech firms are more likely to be sued. *BusinessService* is 1 if a firm is a member of Fama-French (1997) Industry Group 35 and 0 otherwise. *Computer* is 1 if a firm is a member of Fama-French Industry Group 36 and 0 otherwise. *Chips* is 1 if a firm is a member of Fama-French Industry Group 37 and 0 otherwise.

$$\Pr(\text{Lawsuit}_i = 1) = F\,(d_0 + d_1\,\text{Size}_i + d_2\,\text{Turnover}_i + d_3\,\text{Beta}_i + d_4\,\text{CumRet}_i + d_5\,\text{StdRet}_i + d_6\,\text{MinRet}_i + d_7\,\text{BusinessService}_i + d_8\,\text{Computer}_i + d_9\,\text{Chips}_i + \varepsilon_i)$$

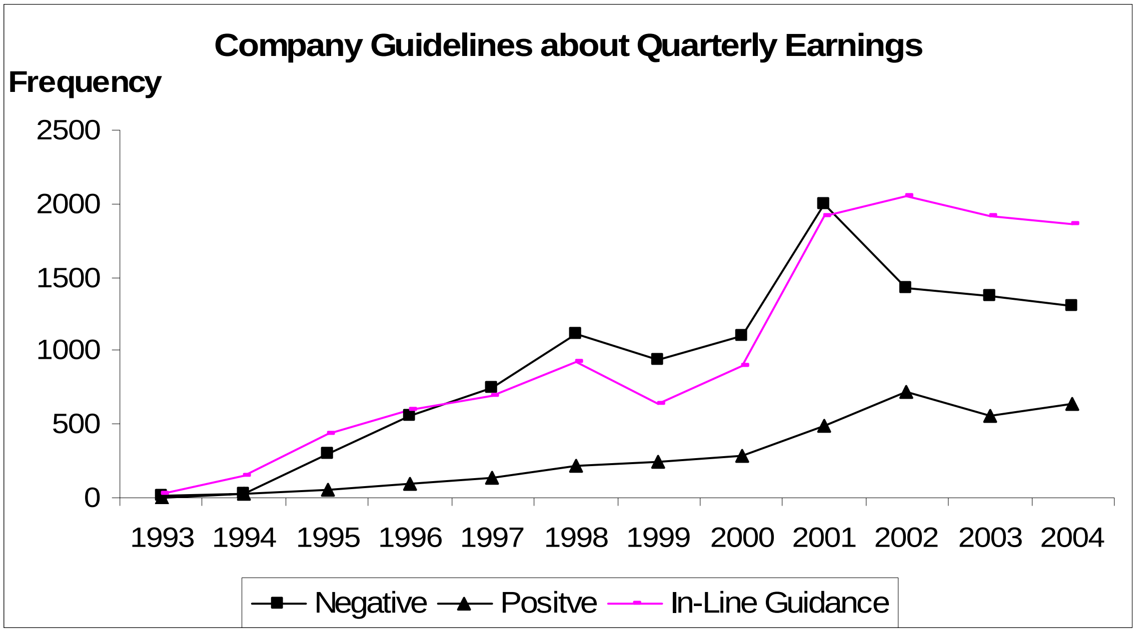
Table B reports the estimation. The coefficients on *Size*, *Turnover*, *Beta*, *CumRet*, and *MinRet* are consistent with the predictions. The coefficient on *StdRet* is significantly negative, contrary to my prediction. Firms in the computer industry face significantly higher risk. For Equation (8), I estimate a firm's probability of being sued using this litigation model out-of-sample with the input variables measured in the 1-year period before the event quarter.

TABLE B
Litigation Model Estimation

Variable	Coefficient	*Chi-Square*
Intercept	–3.531	1106.71***
Size	0.110	67.72***
Turnover	18.330	136.57***
Beta	0.119	24.85***
CumRet	–0.206	49.62***
StdRet	–14.061	101.13***
MinRet	–4.570	531.79***
BusinessService	–0.010	0.02
Computer	0.233	8.45***
Chips	0.041	0.22
McFadden Pseudo R^2		0.267
426 litigated and 38,150 nonlitigated firm-year observations		

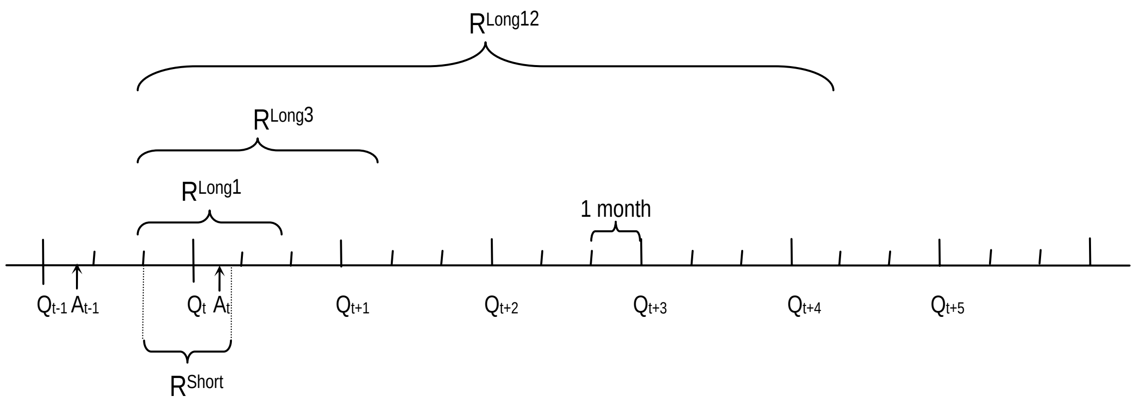
Note: "***"indicates statistical significance at 1% in a two-tailed test.

FIGURE 1
Company Guidance about Quarterly Earnings over Time



Notes: The data source is the First Call Company Issued Guideline (CIG) database. First Call collects company guidelines from press releases and interviews and codes them as "D" for negative news, "E" for positive news, and "M" for in-line guidance for Variable "Code2" by comparing the disclosure with existing market expectations. The events in 2004 are underrepresented because the data for the fourth quarter are incomplete.

FIGURE 2
Return Measurement Windows



Notes:

"Q" represents the fiscal quarter end and "A" represents earnings announcement.

R^{Short} is measured from the beginning of the third fiscal month of the event quarter to 5 days after the event-quarter earnings announcement date.

R^{Long1} is measured from the beginning of the third fiscal month of the event quarter to 1 month after the end of the event-quarter earnings-announcement month.

R^{Long3} is measured from the beginning of the third fiscal month of the event quarter to 3 months after the end of the event-quarter earnings-announcement month.

R^{Long12} is measured from the beginning of the third fiscal month of the event quarter to 12 months after the end of the event-quarter earnings-announcement month.

FIGURE 3
Market Behavior Scenarios and Empirical Predictions

Model: $R_i = \alpha_{TT} + \theta_{TT}\, Warn_i + \beta X_i + \sigma_{\varepsilon v_1}\, Mill_i * Warn_i + \sigma_{\varepsilon v_0}\, Mill_i * (1 - Warn_i) + u_i$

θ_{OLS} is the average return difference between warning and nonwarning firms after controlling for risks and earnings news (represented by *X*). θ_{OLS} can be decomposed as:

$$\theta_{OLS} = \underbrace{\theta_{TT}}_{\text{Warning Effect}} + \underbrace{E[\sigma_{\varepsilon v_1} \frac{\phi(Z_i\gamma)}{\Phi(Z_i\gamma)} - \sigma_{\varepsilon v_0} \frac{-\phi(Z_i\gamma)}{1-\Phi(Z_i\gamma)}]}_{\text{Self-Selection Effect (SS)}}$$

Warning Effect   Self-Selection Effect (SS)

Empirical Predictions for Market Scenarios A (benchmark), B, C, and D:

	Efficient Market			Inefficient Market		
No Self-Selection	**A**	ST	LT	**B**	ST	LT
	θ_{OLS}	0	0	θ_{OLS}	–	0
	θ_{TT}	0	0	θ_{TT}	–	0
	SS	0	0	SS	0	0
Self-Selection	**C**	ST	LT	**D**	ST	LT
	θ_{OLS}	–	–	θ_{OLS}	–	–
	θ_{TT}	0	0	θ_{TT}	?	0
	SS	–	–	SS	?	–

Notes:

1. In the model, *R* is stock return, *X* is the vector of variables controlling for risks and earnings news, and *Warn* is 1 for warning firms and 0 for nonwarning firms. *Mill* is the inverse Mills ratio, defined as $\frac{\phi(Z_i\gamma)}{\Phi(Z_i\gamma)}$ for a warning firm and $\frac{-\phi(Z_i\gamma)}{1-\Phi(Z_i\gamma)}$ for a nonwarning firm, where $\phi(.)$ is the p.d.f. and $\Phi(.)$ is the c.d.f. of the standard normal distribution and z is the vector of warning factors observable to researchers. $\sigma_{\varepsilon v_1}$ ($\sigma_{\varepsilon v_0}$) is the covariance of the error term of the warning model and the error term of regressiong *R* on *X* and the constant term using the warning (nonwarning) observations.
2. Scenarios A and B are self-explanatory. Scenario C allows for market overreaction to warnings. Scenario D allows for incomplete or incorrect price adjustments for other bad news.
3. The short-term (ST) window is from the beginning of the third fiscal month of the event quarter until 5 days after the event-quarter earnings announcement date. The long-term (LT) window extends the short-term window by 1, 2, 3, 6, 9, or 12 months after the end of the event-quarter earnings-announcement month.

TABLE 1
Sample Collection

Panel A: Collection of Warning Events

Procedures	Change	Remaining Events
Negative guidance (see the note below) about *quarterly* earnings for 1996Q1– 2003Q2		8,692
Negative guidance about *annual* earnings issued after the beginning of the last fiscal month (treated as warnings for Q4)	+181	8,873
Negative guidance about quarterly earnings issued *before* the beginning of the third fiscal month	–3,214	5,659
Missing identifying variables in Compustat, CRSP or I/B/E/S	–907	4,752
Duplicate warnings for the same fiscal quarter	–75	4,677
Warnings issued since three days before earnings announcement	–100	4,577
Unavailable recent analyst consensus before the warning	–25	4,552
Price-deflated earnings surprise higher than –0.001	– 469	4,083
Insufficient data for the warning model and return tests	–190	3,893
Penny stocks (stock price less than $2)	– 24	3,869
Warning events		3,869

Panel B: Examples of Earnings Warnings

Example 1 — Quintiles

"In yet another setback for companies which manage clinical research for drug companies, sector stalwart Quintiles Transnational Corp. took a stock-market beating Thursday after warning Wednesday it would miss third- and fourth-quarter earnings by a wide margin. The announcement caught Wall Street by surprise and sent tremors throughout the industry. Quintiles fell as low as $16.875 at one point Thursday, slightly more than half the previous 52-week low of $30.25, set Aug. 24. Quintiles shares (QTRN) closed down $14.75, or 42%, at $20. Volume was 39.8 million shares, compared with a daily average of 1.5 million. Quintiles said it expects third-quarter earnings of about 27 cents a share, below First Call/Thomson Financial's mean estimate of 36 cents, as a result of early terminations of trials for certain cardiovascular drugs." (*Dow Jones Business News*, "Investors slam Quintiles shares in aftermath of earnings warning", 9/16/1999)

Example 2 — Molecular Dynamics Inc.

"A warning its fourth-quarter earnings will come in below analysts' expectations sent shares of Molecular Dynamics Inc. down 38% Friday. The near-term uncertainty of the company's Japan business because of financial turmoil in Asia is also a factor, it said. Molecular Dynamics expects its fourth quarter to come in under analysts' forecasts, and down sequentially from earnings of $1.4 million, or 12 cents a share, in the third quarter. A survey of four analysts polled by First Call projected a mean estimate of 14 cents a share for the fourth quarter …." (*Dow Jones Online News*, "Molecular Dynamics's shares plunge 38% on lower earnings forecast", 1/9/1998)

Note: First Call collects company guidelines from press releases and interviews, compares the disclosure with existing market expectations, and codes them as "D" for negative news for Variable "Code2" in the CIG database.

TABLE 2
Descriptive Statistics

Panel A: Top 10 Industries with the Largest Number of Warnings

		Warning		Nonwarning		Total	
No.	Industry	#1	%	#0	%	#T	#1/#T
1	Business Services	530	13.7%	3,002	13.0%	3,532	15.0%
2	Retail	346	8.9%	1,126	4.9%	1,472	23.5%
3	Chips	343	8.9%	1,529	6.6%	1,872	18.3%
4	Computer	267	6.9%	1,177	5.1%	1,444	18.5%
5	Machinery	191	4.9%	802	3.5%	993	19.2%
6	Wholesale	172	4.4%	837	3.6%	1,009	17.0%
7	Steel Works	118	3.0%	577	2.5%	695	17.0%
8	Transportation	113	2.9%	732	3.2%	845	13.4%
9	Chemicals	111	2.9%	419	1.8%	530	20.9%
10	Lab Equipment	111	2.9%	541	2.3%	652	17.0%
	Other	1,567	40.5%	12,416	53.6%	13,983	11.2%
	Total	3,869	100%	23,158	100%	27,027	14.3%

Notes: The industries are classified according to Fama and French (1997). See Table 1 for the identification of warning observations. Nonwarning observations are the firm-quarters for which the forthcoming earnings are less than the most recent analyst consensus before the third fiscal month but the firms do not warn, according to the First Call CIG database.

Panel B: Stock Returns

Group	Warning (3-Trading-Day)	Earnings Announcement (3-Trading-Day)	Short-Term Window
Warning	–0.132***	–0.004***	–0.195***
Nonwarning	N/A	–0.021***	–0.084***

Note: The returns are the average cumulative market-adjusted return for each group. The short-term window runs from the beginning of the third fiscal month of the event quarter to 5 days after the event-quarter earnings-announcement date. "***"indicates statistical significance at 1% in a two-tailed test.

TABLE 2
(Continued)

Panel C: Main Variables

	Warning (3,869)		Nonwarning (23,158)		Between-Group Test (warning – nonwarning)	
	Mean	Median	Mean	Median	T-Test	Wilcoxon Z
R^{Short}	–0.164	–0.159	–0.057	–0.054	–27.24***	–26.85***
R^{Long1}	–0.156	–0.160	–0.047	–0.051	–22.28***	–21.65***
R^{Long3}	–0.138	–0.155	–0.035	–0.057	–16.81***	–16.67***
R^{Long12}	–0.056	–0.142	0.064	–0.042	–11.20***	–9.24***
Beta	1.295	1.152	1.247	1.085	3.57***	4.65***
MVE	2,585	432	1,495	226	6.15***	21.42***
M/B	2.872	2.106	2.811	1.825	1.20	9.67***
Leverage	0.483	0.492	0.499	0.510	–4.25***	–3.86***
ROA	0.011	0.013	–0.007	0.005	36.00***	29.02***
EarnVolt	0.019	0.011	0.027	0.012	–15.51***	–7.30***
Surprise	–0.014	–0.007	–0.014	–0.005	1.23	–13.94***
FutureEPS	–0.016	–0.008	–0.009	–0.004	–10.04***	–14.36***

Note: "***"indicates statistical significance at 1% in a two-tailed test.

Variable Definitions:

R^{Short} is a firm's buy-and-hold return from the beginning of the third fiscal month of the event quarter to 5 days after the event-quarter earnings announcement. R^{Long1}, R^{Long3}, and R^{Long12} are a firm's buy-and-hold return from the beginning of the third fiscal month of the event quarter to 1, 3, and 12 months after the end of the event-quarter earnings-announcement month, respectively. *Beta* is the coefficient on market returns in a market model using daily returns in the 1-year period before the event quarter. *MVE* is the market value of equity at the beginning of the event quarter (in millions). *M/B* is the market-to-book ratio at the beginning of the event quarter. *Leverage* is the debt-to-assets ratio, averaged in the four quarters before the event quarter. *ROA* is the accounting return on assets, averaged in the four quarters before the event quarter. *EarnVolt* is the standard deviation of accounting return on assets in the eight quarters before the event quarter. For a warning observation, *Surprise* is the difference between the forthcoming EPS and the most recent analyst consensus before the warning (both earnings are from I/B/E/S). For a nonwarning observation, *Surprise* is the difference between the forthcoming EPS and the last analyst consensus before the return window. Earnings surprise is deflated by the split-adjusted beginning-of-event-quarter stock price. *FutureEPS* is the change in average diluted EPS (before extraordinary items) from the four pre- to the four post-event quarters, deflated by the split-adjusted stock price at the beginning of the event quarter. All variables except for *MVE* are winsorized at 1% and 99% in the full sample.

TABLE 2
(Continued)

Panel D: Pairwise Correlations (Pearson in the lower triangle and Spearman in the upper triangle)

	R^{Short}	R^{Long3}	Leverage	ROA	EarnVolt	Surprise	FutureEPS
R^{Short}		0.609	0.111	0.025	–0.127	0.179	0.124
R^{Long3}	0.611		0.121	0.060	–0.154	0.162	0.231
Leverage	0.083	0.073		–0.046	–0.449	0.040	0.028
ROA	0.035	0.060	0.142		–0.350	0.260	–0.257
EarnVolt	–0.083	–0.084	–0.271	–0.651		–0.264	0.083
Surprise	0.102	0.087	–0.014	0.161	–0.144		0.167
FurtureEPS	0.100	0.192	–0.012#	–0.234	0.185	0.152	

Notes: See Panel C for variable definitions. "#" indicates statistical insignificance at 5% in a two-tailed test. The unmarked correlations are statistically significant at 5% in a two-tailed test.

Panel E**:** Pearson Correlations between Stock Returns and Control Portfolio Returns

3-Month Extension	R^{Long3}	R^{beta}	R^{size}	R^{mb}	R^{mom}
R^{beta}	0.396				
R^{size}	0.321	0.726			
R^{mb}	0.350	0.764	0.794		
R^{mom}	0.343	0.769	0.776	0.761	
R^{ind}	0.430	0.696	0.660	0.681	0.677

Notes: All the correlations are statistically significant at 1% in a two-tailed test. The Spearman correlations are similar and thus are not reported.

Variable Definitions:

R^{Long3} is a firm's buy-and-hold return from the beginning of the third fiscal month of the event quarter to 3 months after the end of the event-quarter earnings-announcement month. R^{beta}, R^{size}, R^{mb}, R^{mom}, and R^{ind} are the control portfolio returns. At the beginning of each month, I form beta deciles, 20 firm-size groups, M/B deciles, momentum deciles, and 48 Fama and French (1997) industry groups. Beta is estimated in a market model using daily returns in the 1-year period before that month, size is the market value of equity at the beginning of the month, M/B is the ratio of the market value of equity at the beginning of the month over the book value of equity reported in the most recent quarter, and the momentum factor uses the cumulative stock returns in the past six months. For each warning and nonwarning observation, I determine its control portfolio affiliation at the beginning of the third fiscal month. I purge all warning and nonwarning observations from the control portfolio, randomly choose 100 members from each benchmark portfolio, calculate the buy-and-hold returns for each member over the same holding period as the dependent variable, delete the top and bottom 2% of the distribution, and use the mean as the control portfolio return.

TABLE 3
Probit Warning Model

$$\Pr(\text{Warn}_i = 1) = \Phi(a_0 + a_1 \text{ LitigRisk}_i + a_2 \text{ LogMVE}_i + a_3 \text{ LogSurprise}_i + a_4 \text{ Forecast}_i + a_5 \text{ PastDisclosure}_i + a_6 \text{ PastFollow}_i + a_7 \text{ MB}_i + a_8 \text{ EarnVolt}_i + \varepsilon_i)$$

	Coefficient	z-statistic
Intercept	–2.657	–23.21***
LitigRisk	0.170	4.08***
LogMVE	0.093	10.13***
LogSurprise	0.208	21.41***
Forecast	0.587	16.70***
PastDisclosure	2.033	17.50***
PastFollow	0.474	8.25***
M/B	0.019	5.73***
EarnVolt	–4.500	–11.70***
McFadden Pseudo R^2		9.15%

Note: "***" indicates statistical significance at 1% in a two-tailed test. The number of warning and nonwarning observations is 3,869 and 23,158, respectively.

Variable Definitions:

Warn is 1 for the warning group and 0 for the nonwarning group. *LitigRisk* is the full-sample rankings (0 for the lowest and 1 for the highest) of the likelihood of being sued (Appendix B), estimated with the input variables measured in the 1-year period before the event quarter. *LogMVE* is the log transformation of market value of equity at the beginning of the event quarter (in millions). *LogSurprise* is the log transformation of the absolute value of event-quarter earnings surprise. For a warning observation, earnings surprise is the difference between the forthcoming EPS and the most recent analyst consensus before the warning (both from I/B/E/S). For a nonwarning observation, it is the difference between the forthcoming EPS and the last analyst consensus before the third fiscal month. *Surprise* is deflated by the split-adjusted beginning-of-event-quarter stock price. *Forecast* is 1 if a firm has issued earnings forecast about the event quarter before the third fiscal month and 0 otherwise. *PastDisclosure* is the number of positive or negative guidelines about quarterly earnings issued by a firm in the 360 days before the event quarter. *PastFollow* is the average number of analysts whose earnings forecasts are included in the most recent consensus before earnings announcement for the four quarters before the event quarter. Each of the above two measures is converted into rankings (between 0 and 1) among all firms (good-news, no-news, bad-news firms) in the event quarter. *M/B* is the market-to-book ratio at the beginning of the event quarter. *EarnVolt* is the standard deviation of accounting return on assets in the eight quarters before the event quarter. The above two measures are each winsorized at 1% and 99%.

TABLE 4
Primary Tests

Panel A: KL return regression (t-statistics in parentheses. Adjusted R^2= 3.3%):

$$CAR_i = b_0 + b_1 Warn_i + b_2\, Surprise_i + b_3 Warn_i * Surprise_i + b_4 LogMVE_i + w_i$$

	b_0	b_1	b_2	b_3	b_4
Coefficient	–0.043***	–0.107***	0.944***	0.078	–0.005***
t-statistic	(–7.15)	(–20.83)	(14.51)	(0.38)	(–5.02)

Notes: *CAR* is the cumulative market-adjusted return from the beginning of the third fiscal month of the event quarter to 5 days after the event-quarter earnings announcement date. *Surprise* is the earnings surprise, deflated by the beginning-of-quarter stock price. *LogMVE* is the log transformation of market value of equity at the beginning of the event quarter (in millions).

Panel B: Regression with <u>No Control</u> for Self-Selection

$$R_i = c_0 + \theta_{OLS}\, Warn_i + c_2 R^{beta}_i + c_3 R^{size}_i + c_4 R^{mb}_i + c_5 R^{mom}_i + c_6 R^{ind}_i + c_7\, Leverage_i + c_8\, ROA_i + c_9\, EarnVolt_i + c_{10}\, Surprise_i + c_{11}\, FutureEPS_i + \nu_i$$

R =	R^{Short}	R^{Long1}	R^{Long2}	R^{Long3}
Intercept	–0.070***	–0.063***	–0.068***	–0.061***
	(–17.23)	(–12.46)	(–11.89)	(–9.55)
Warn	**–0.101*** **	**–0.100*** **	**–0.101*** **	**–0.097*** **
	(–28.13)	**(–22.98)**	**(–20.49)**	**(–17.58)**
R^{beta}	0.312***	0.366***	0.421***	0.379***
	(10.73)	(14.65)	(17.31)	(14.43)
R^{size}	0.025	–0.087***	–0.059**	–0.102***
	(0.79)	(–2.96)	(–2.08)	(–3.43)
R^{mb}	0.018	0.059*	0.124***	0.142***
	(0.49)	(1.84)	(3.91)	(4.52)
R^{mom}	0.180***	0.096***	0.030	–0.043
	(5.63)	(3.36)	(1.13)	(–1.54)
R^{ind}	0.527***	0.520***	0.454***	0.576***
	(23.07)	(24.49)	(27.34)	(27.88)
Leverage	0.045***	0.040***	0.038***	0.037***
	(7.71)	(5.45)	(4.57)	(4.03)
ROA	0.002	0.144**	0.260***	0.424***
	(0.04)	(2.22)	(3.69)	(5.42)
EarnVolt	–0.457***	–0.519***	–0.495***	–0.501***
	(–7.76)	(–6.92)	(–5.91)	(–5.29)
Surprise	0.902***	0.901***	0.873***	0.901***
	(12.18)	(9.57)	(8.37)	(8.07)
FutureEPS	0.449***	0.889***	1.139***	1.452***
	(11.91)	(18.04)	(20.60)	(23.76)
R^2	23.6%	25.7%	25.7%	25.2%

Notes: The t-statistics are in the parentheses. "***," "**," and "*" indicate statistical significance at 1%, 5%, and 10% in a two-tailed test, respectively. The t-statistics are adjusted for heteroskedasticity (Huber/White/Sandwich estimator). See variable definitions after Panel D.

TABLE 4
(Continued)

Panel C: Regression with Control for Self-Selection

$$R_i = c_0 + \theta_{TT}\,\text{Warn}_i + c_2\,R^{beta}_i + c_3\,R^{size}_i + c_4\,R^{mb}_i + c_5\,R^{mom}_i + c_6\,R^{ind}_i + c_7\,\text{Leverage}_i + c_8\,\text{ROA}_i + c_9\,\text{EarnVolt}_i + c_{10}\,\text{Surprise}_i + c_{11}\,\text{FutureEPS}_i + \sigma_{\varepsilon v_1}\,\text{Mill}_i * \text{Warn}_i + \sigma_{\varepsilon v_0}\,\text{Mill}_i * (1\text{-Warn}_i) + u_i$$

R =	R^{Short}	R^{Long1}	R^{Long2}	R^{Long3}
Intercept	–0.058***	–0.059***	–0.074***	–0.069***
	(–12.49)	(–10.04)	(–13.09)	(–9.51)
Warn	**–0.064*** **	**–0.040** **	**–0.026**	**0.011**
	(–4.20)	**(–2.17)**	**(–1.25)**	**(0.46)**
R^{beta}	0.311***	0.365***	0.420***	0.377***
	(10.72)	(14.64)	(17.28)	(14.38)
R^{size}	0.030	–0.086***	–0.058**	–0.102***
	(0.93)	(–2.91)	(–2.07)	(–3.45)
R^{mb}	0.018	0.059*	0.124***	0.142***
	(0.50)	(1.82)	(3.90)	(4.53)
R^{mom}	0.177***	0.095***	0.031	–0.042
	(5.55)	(3.34)	(1.14)	(–1.49)
R^{ind}	0.526***	0.520***	0.454***	0.577***
	(23.03)	(24.49)	(27.33)	(27.92)
Leverage	0.046***	0.039***	0.036***	0.034***
	(7.92)	(5.39)	(4.37)	(3.66)
ROA	0.017	0.151**	0.262***	0.418***
	(0.33)	(2.33)	(3.71)	(5.34)
EarnVolt	–0.488***	–0.523***	–0.479***	–0.455***
	(–8.24)	(–6.91)	(–5.67)	(–4.77)
Surprise	0.843***	0.888***	0.894***	0.971***
	(11.13)	(9.20)	(8.34)	(8.46)
FutureEPS	0.445***	0.890***	1.144***	1.462***
	(11.80)	(18.05)	(20.68)	(23.90)
Mill*Warn	**–0.034*** **	**–0.045*** **	**–0.051*** **	**–0.068*** **
	(–3.44)	**(–3.67)**	**(–3.67)**	**(–4.35)**
Mill*(1-Warn)	**0.050*** **	**0.017**	**–0.008**	**–0.042*** **
	(4.75)	**(1.31)**	**(–0.53)**	**(–2.58)**
R^2	23.7%	25.7%	25.7%	25.3%

Notes: The estimation takes two steps. In the first step, I estimate the probit warning model and calculate *Mill*. In the second step, I add the self-selection terms to the return regression, which is estimated allowing for heteroskedasticity (Huber/White/Sandwich estimator). In the second step, the statistical package does not correct the variance for the sampling error from the first step. I compare the test results of a typical treatment-effect model, for which the statistical package makes the variance correction, with those without variance correction and find little difference. The t statistics are in the parentheses. "***," "**," and "*" indicate statistical significance at 1%, 5%, and 10% in a two-tailed test, respectively. See variable definitions after Panel D.

TABLE 4
(Continued)

Panel D: Correlation-Robust Standard Error (S. E.) and Fama-MacBeth Regressions

R =	R^{Short}		R^{Long3}	
	Robust S. E.	Fama-MacBeth	Robust S. E.	Fama-MacBeth
Intercept	–0.058***	–0.049***	–0.069***	–0.078***
	(–7.42)	(–5.06)	(–6.25)	(–3.71)
Warn	**–0.064****	**-0.061*****	**0.011**	**–0.019**
	(–2.57)	**(–2.90)**	**(0.25)**	**(–0.56)**
R^{beta}	0.311***	0.438***	0.377***	0.338***
	(3.11)	(7.99)	(7.32)	(6.34)
R^{size}	0.030	0.076	–0.102	0.083
	(0.35)	(1.61)	(–1.32)	(1.21)
R^{mb}	0.018	0.025	0.142***	0.165**
	(0.19)	(0.30)	(3.09)	(2.48)
R^{mom}	0.177**	0.047	–0.042	–0.017
	(2.50)	(0.73)	(–1.03)	(–0.38)
R^{ind}	0.526***	0.481***	0.577***	0.512***
	(11.23)	(12.47)	(15.43)	(14.62)
Leverage	0.046***	0.038***	0.034	0.007
	(3.88)	(3.31)	(1.58)	(0.33)
ROA	0.017	–0.017	0.418***	0.367***
	(0.17)	(–0.20)	(2.94)	(2.71)
EarnVolt	–0.488***	–0.446***	–0.455***	–0.320***
	(–7.70)	(–7.21)	(–4.18)	(–2.67)
Surprise	0.843***	0.849***	0.971***	1.000***
	(6.38)	(5.91)	(6.09)	(4.76)
FutureEPS	0.445***	0.487***	1.462***	1.516***
	(8.13)	(10.02)	(18.02)	(20.93)
Mill*Warn	**–0.034*****	**–0.022****	**–0.068*****	**–0.052*****
	(–2.76)	**(–2.15)**	**(–2.72)**	**(–2.78)**
Mill*(1-Warn)	**0.050****	**0.058*****	**–0.042**	**–0.028**
	(2.25)	**(2.76)**	**(–1.24)**	**(–0.77)**
R^2	23.7%	21.3%	25.3%	21.7%

Notes: The robust-standard-error estimation allows for heteroskedasticity and within-year-quarter error-term correlations (the "reg" procedure in Stata with the "cluster" option). The Fama-MacBeth estimation uses the time-series of coefficient estimates from 30 year-quarter cross-sections. The t statistics are in the parentheses. "***," "**," and "*" indicate statistical significance at 1%, 5%, and 10% in a two-tailed test, respectively. See the next page for variable definitions.

TABLE 4
(Continued)

Variable Definitions:

Warn is 1 for a warning observation (3,869) and 0 for a nonwarning observation (23,158).

R^{Short} is a firm's buy-and-hold return from the beginning of the third fiscal month of the event quarter to 5 days after the event-quarter earnings announcement.

R^{Long1}, R^{Long2}, and R^{Long3} are a firm's buy-and-hold return from the beginning of the third fiscal month of the event quarter to 1, 2, and 3 months after the end of the event-quarter earnings-announcement month, respectively.

For each warning or nonwarning observation, R^{beta}, R^{size}, R^{mb}, R^{mom}, and R^{ind} are the buy-and-hold return of a portfolio of firms in the same beta group (10), firm-size group (20), M/B group (10), past-6-months returns group (10), and Fama-French industry group (48) as the sample firm at the beginning of the third fiscal month, respectively. At the beginning of each month, beta is estimated in a market model using daily returns over the 1-year period before that month, size is the market value of equity at the beginning of the month, M/B is the ratio of the market value of equity at the beginning of the month over the book value of equity reported in the most recent quarter, and the momentum factor uses the cumulative returns in the past 6 months. I purge all warning and nonwarning observations from the control portfolio, randomly choose 100 members from each benchmark portfolio, calculate the buy-and-hold returns for each member over the same holding period as the dependent variable, delete the top and bottom 2% of the distribution, and use the mean as the control portfolio return.

Leverage is the debt-to-assets ratio, averaged in the four quarters before the event quarter.

ROA is the accounting return on assets, averaged in the four quarters before the event quarter.

EarnVolt is the standard deviation of accounting return on assets in the eight quarters before the event quarter.

For a warning observation, *Surprise* is the difference between the forthcoming EPS and the most recent analyst consensus before the warning (both from IBES). For a nonwarning observation, it is the difference between the forthcoming EPS and the last analyst consensus before the return window. *Surprise* is deflated by the split-adjusted beginning-of-event-quarter stock price.

FutureEPS is the change in average diluted EPS (before extraordinary items) from the four pre- to the four post-event quarters, deflated by the split-adjusted beginning-of-event-quarter price.

Mill is the inverse Mills ratio, defined as $\frac{\phi(Z_i\gamma)}{\Phi(Z_i\gamma)}$ for a warning firm and $\frac{-\phi(Z_i\gamma)}{1-\Phi(Z_i\gamma)}$ for a nonwarning firm, where ϕ and Φ are standard normal p.d.f. and c.d.f. respectively, Z is the vector of explanatory variables in the warning choice model, and γ is the vector of coefficients estimated in Table 3.

All continuous variables except for *Mill* and *LogMVE* are winsorized at 1% and 99%.

TABLE 5
Post-Event Returns

Panel A: Regression Estimation

R =	R^{Post1}	R^{Post2}	R^{Post3}
Intercept	–0.010**	–0.013**	–0.017***
	(–2.26)	(–2.55)	(–2.68)
Warn	**0.027****	**0.037****	**0.078*****
	(1.97)	**(2.16)**	**(3.69)**
R^{beta}	0.236***	0.258***	0.203***
	(13.08)	(12.42)	(8.89)
R^{size}	0.063***	0.082***	0.028
	(3.01)	(3.63)	(1.06)
R^{mb}	–0.036	–0.029	0.049*
	(–1.50)	(–1.11)	(1.72)
R^{mom}	–0.004	–0.027	–0.020
	(–0.22)	(–1.19)	(–0.80)
R^{ind}	0.314***	0.395***	0.481***
	(20.37)	(23.86)	(26.10)
Leverage	–0.004	–0.007	–0.013
	(–0.80)	(–1.02)	(–1.60)
ROA	0.250***	0.355***	0.498***
	(5.40)	(6.27)	(7.43)
EarnVolt	–0.064	–0.095	–0.066
	(–1.12)	(–1.40)	(–0.80)
Surprise	0.045	–0.040	–0.012
	(0.64)	(–0.45)	(–0.11)
FutureEPS	0.516***	0.811***	1.140***
	(15.09)	(18.82)	(21.66)
Mill	**–0.013**	**–0.016**	**–0.034****
	(–1.39)	**(–1.35)**	**(–2.42)**
Mill*(1-Warn)	**–0.032*****	**–0.051*****	**–0.091*****
	(-3.49)	**(–4.26)**	**(–6.38)**
R^2	18.2%	20.1%	20.4%
θ_{OLS}	0.001	0.002	0.006
	(0.19)	(0.47)	(1.17)
$\theta_{OLS\ (Excluding\ FutureEPS)}$	–0.001	0.000	0.003
	(–0.17)	(0.05)	(0.64)

TABLE 5
(Continued)

Notes:

1. The estimation uses 3,869 warning (*Warn*=1) and 23,158 nonwarning (*Warn*=0) observations.
2. The estimation uses a two-step procedure. In the first step, I estimate the warning probit model and calculate *Mill*. In the second step, I add the self-selection terms to the return regression, which is estimated allowing for heteroskedasticity (Huber/White/Sandwich estimator). The t statistics are in the parentheses. "***," "**," and "*" indicate statistical significance at 1%, 5%, and 10% in a two-tailed test, respectively.
3. R^{Post1}, R^{Post2}, and R^{Post3} are a firm's buy-and-hold return from the sixth day after the event-quarter earnings announcement to 1, 2, and 3 months after the end of the event-quarter earnings announcement month, respectively. The three new variables are winsorized at 1% and 99% in the full sample. See the end of Table 4 for other variable definitions.
4. θ_{OLS} is the least squares coefficient on *Warn* when *Mill* is excluded. The t-statistic is robust to heteroskedasticity.
5. $\theta_{OLS\ (excluding\ FutureEPS)}$ is the least squares coefficient on *Warn* when *Mill* and *FutureEPS* are excluded. The t-statistic is robust to heteroskedasticity.

Panel B: Trading Strategy to Exploit Within-Group Mispricing

Mean (Median)	Warning Firms			Nonwarning Firms		
	High *P*	Low *P*	T-Test (Wilcoxon)	High *P*	Low *P*	T-Test (Wilcoxon)
P	0.351 (0.319)	0.098 (0.102)		0.233 (0.208)	0.051 (0.053)	
Abnormal Return	0.011 (–0.011)	–0.001 (–0.035)	1.02 (1.76*)	0.011 (–0.011)	–0.011 (–0.036)	4.54*** (5.64***)
Observations	1,291	1,290		7,722	7,721	

Notes:

1. *P* is the predicted warning probability from the warning model estimated in Table 3. The firms in the warning group and nonwarning group are sorted separately into three subgroups (i.e. high, medium, and low) according to *P*.
2. *Abnormal Return* is the residual obtained from regressing R^{Post3} on R^{beta}, R^{size}, R^{mb}, R^{mom}, R^{ind}, *Leverage*, *ROA*, and *Surprise* (all publicly available information). See the end of Table 4 and Panel A of Table 5 for the variable definitions.
3. "***," "**," and "*" indicate statistical significance at 1%, 5%, and 10% in a two-tailed test, respectively.