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Is Openness Penalized? Stock Returns around Earnings Warnings

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ABSTRACT: Prior research finds that firms warning investors of an earnings shortfall experience lower returns than non-warning firms with similar risks and earnings news. Openness thus appears to be penalized by investors. Yet, this finding may be due to a self-selection bias that occurs when firms with a larger amount of unfavorable non-earnings news ("other bad news") are more likely to warn. In this paper I use a Heckman selection model to infer the amount of other bad news and document that, on average, warning firms have a larger amount of other bad news than non-warning firms. After controlling for this effect, I find that warning firms' returns remain lower than those of non-warning firms in a short-term window ending five days after earnings announcement. When this window is extended by three months, however, warning and non-warning firms exhibit similar returns. My evidence suggests that openness is ultimately not penalized by investors.

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

I. INTRODUCTION

This paper examines whether firms that warn of an earnings shortfall experience lower stock returns than firms that have similar risks, earnings news, and non-earnings news but do not warn. After controlling for risks and earnings news, Kasznik and Lev (1995; hereafter, KL) find that warning firms' returns are significantly lower than the returns of those that likely anticipate an earnings shortfall but do not warn (''non-warning firms''). This finding has been interpreted as a market penalty for openness.[1] If investors indeed penalize openness, then firms that face an earnings shortfall would be less willing to warn and, as a result, the frequency of warnings should diminish. Surprisingly, the number of warnings increased in the past decade (see Figure 1).

In this paper I examine whether KL's finding is due to unfavorable non-earnings news, referred to as ''other bad news,'' that affects managers' warning decisions and investors'

[1] See *The Economist* (1994) and Core (2001, 448).

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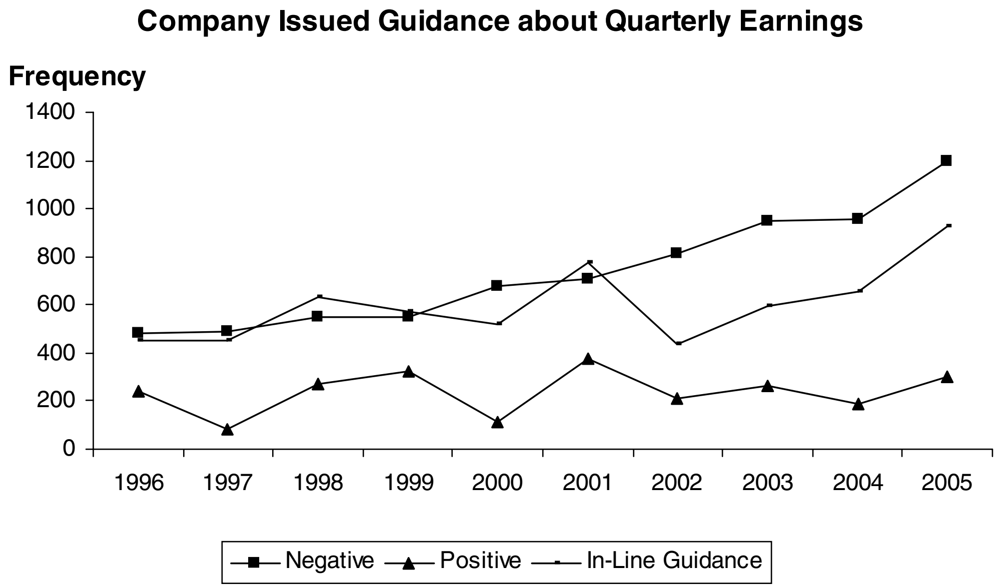
FIGURE 1
Earnings Guidance in the Confession Season



The graph shows the number of negative, positive, and in-line guidelines given by U.S. companies about quarterly earnings. These guidelines are issued after the beginning of the third fiscal month in the so-called "confession season." The data source is the First Call Company Issued Guidelines (CIG) database. First Call collects company guidelines from press releases and interviews, compares them with existing market expectations, and codes them as negative, positive, or in-line guidance (i.e., confirming guidance).

stock valuation, but is omitted from the researchers' return analysis. In addition, I examine whether stock prices immediately impound such news. Examples of other bad news include the discontinuation of new product development, plans for store closings, trouble with alliances, change in management, and lawsuits.[2] Such news is unobservable to researchers without incurring substantial data-collection costs. Using a Heckman selection model that addresses the unobservability problem, I find that, on average, warning firms have a larger amount of other bad news than non-warning firms. After controlling for risks, earnings news, and other bad news, I find that warning firms' returns remain lower than those of non-warning firms in a short-term window that ends five days after earnings announcement. When this window is extended by three months ("long-term"), however, warning and non-warning firms exhibit similar returns. Openness is ultimately not penalized by investors.

My tests proceed in two steps. First, as with KL, I do not control for other bad news. Warning firms' returns are, on average, 10.1 percent lower than those of non-warning firms that have similar risks and earnings news. This return difference, referred to as the "between-group return difference," does not change across the short- to long-term windows,

[2] Firms often disclose a great deal of other news along with earnings surprises in earnings warnings and earnings announcements. 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 the development of a prospective product.

suggesting that warning firms are correctly priced relative to non-warning firms—there is no between-group mispricing.[3]

The above negative between-group return difference, however, is *not* evidence of a market penalty for warning. It may be that managers are more likely to warn when they have a larger amount of other bad news, all else being equal. That is, firms self-select into the warning or non-warning groups depending on the amount of other bad news. If this is the case, then the average return of warning firms *should* be lower than that of non-warning firms, *ceteris paribus*. After removing this "self-selection effect," any remaining between-group return difference is the "warning effect." In other words, the between-group return difference consists of a self-selection effect and a warning effect. The warning effect is the difference between warning firms' returns and what the returns would have been had these firms not warned. Intuitively speaking, the warning effect estimates the average return difference between warning and non-warning firms after controlling for risks, earnings news, and other bad news. It is the warning effect, rather than the between-group return difference, that measures the market penalty for warning.

In my second step I add the control for other bad news. I find that the warning effect in the short-term window is significantly negative at −6.4 percent, but disappears in the long-term window. Subtracting the warning effect from the between-group return difference, I estimate the self-selection effect to be −3.7 percent in the short-term and about −10 percent in the long-term. The negative self-selection effect suggests that, on average, warning firms have a larger amount of other bad news than non-warning firms. Taken together, my results indicate that the negative between-group return difference in the short-term is due to both a negative warning effect and a negative self-selection effect, but the difference in the long-term is due purely to the self-selection effect. Therefore, the decision to warn is ultimately not penalized by investors.

Further, I investigate why a warning penalty exists in the short-term window. My evidence suggests that investors initially do not fully adjust stock prices for other bad news. By the end of the short-term window, investors (who typically have a larger information set than researchers) may observe other bad news or they could infer such news using the following logic. 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 if it has a larger amount of *total* bad news. If a firm warned even though its earnings shortfall is small, then the firm is likely to have a larger amount of other bad news than a warning firm that has a large earnings shortfall. 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 (or a smaller amount of other bad news) than a non-warning firm that has a small earnings shortfall. Hence, investors could infer the amount of other bad news about each firm and should adjust the stock price for such news.

In the short-term I find that while investors react more negatively to warning firms with more other bad news than to warning firms with less other bad news, they do not *completely* adjust for the difference in such news. For non-warning firms, on the other hand, investors initially increase, rather than decrease, the stock prices of those that have more other bad news relative to those that have less, and these price adjustments are later reversed. Thus, in the short-term investors' responses to other bad news are incomplete within the warning group and directionally incorrect within the non-warning group. I refer to this phenomenon

[3] This statement holds for the long-term windows that extend the short-term window by one to 12 months.

as "within-group mispricing."[4] As a result, firms with a large amount of other bad news, which therefore tend to warn, are worse off in the short-term for having warned than for being silent.

My paper makes four contributions to the literature. First, the evidence that openness is ultimately not penalized is relevant to firm managers. Managers routinely face the decision of being open versus being silent about forthcoming earnings shortfalls and are concerned about investors' responses to their disclosure decisions. My study adds to the literature beyond Shu (2003) and Xu (2003) who address a similar research issue. Shu (2003) controls for self-selection in the short-term but does not examine the long-term. Xu (2003) examines the long-term but does not control for self-selection. In addition, my study uses a substantially larger sample in the recent time period, thereby increasing statistical power and providing more current evidence than either Shu (2003) or Xu (2003).

Second, I document within-group mispricing; such evidence is unique in the literature. Accounting and finance studies offer much evidence for market overreaction or underreaction to firms that go through particular corporate events. For example, earnings announcements (Bernard and Thomas 1989, 1990), dividend initiations and omissions (Michaely et al. 1995), IPOs (Loughran and Ritter 1995, 2000), tender offers (Ikenberry et al. 1995), and stock splits (Ikenberry and Ramnath 2002). These studies document mispricing of the event firms relative to other firms, suggesting between-group mispricing. In contrast, what I find in this study is *within-group* mispricing.[5]

The third contribution is in my application of Heckman selection models. While previous studies have used selection models,[6] my application is new in two ways. Foremost, I decompose the between-group return difference into a warning effect and a self-selection effect. This decomposition is fundamental in studies that address self-selection. My study is the first of which I am aware that uses this relation explicitly to design empirical tests. Furthermore, I draw inferences about stock price adjustments for other bad news from the coefficients on the inverse Mills ratio (which is used to control for self-selection), and examine these coefficients across various windows, whereas previous studies largely stop short of interpreting these coefficients. My application also goes beyond the commonly used treatment-effect regression (Greene 2003) by allowing the warning and non-warning groups to have different coefficients on the inverse Mills ratio. Overall, the method that I employ can potentially be used to address other accounting issues.

Finally, my study serves as an example of how a failure to control for self-selection can lead to drastically different conclusions. Maddala (1991, 799 and 801) pointed out that the accounting studies that use selection models "thus far do not show any strong evidence of selection bias" because the conclusion with the control for self-selection 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 (of which I am aware) that use selection models. My study shows that without control for self-selection, one would conclude that openness is penalized, when in fact it is not.

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[4] I thank an anonymous referee for this insight. A trading strategy that exploits within-group mispricing earns a mean abnormal return of 2.3 percent from the warning group and 2.0 percent from the non-warning group in the three months after the event quarter. Such abnormal returns, however, may not be inconsistent with market efficiency when transaction costs are considered (Korajczyk and Sadka 2004).

[5] The total between-group return difference does not change over time, but its two components shuffle from the short- to long-term. When within-group mispricing occurs, warning or non-warning firms' coefficients on the inverse Mills ratio change from the short- to long-term, causing the self-selection effect to change over time.

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

The rest of the paper proceeds as follows. Section II reviews prior research. Sections III through VII present the econometric model, empirical predictions, variable identifications and measurements, data, and test results. Section VIII concludes.

II. PRIOR RESEARCH

The most popular reason for voluntary bad-news disclosure is to reduce expected settlement costs in class action lawsuits associated with price declines (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 this argument. For example, Soffer et al. (2000), Baginski et al. (2002), and Field et al. (2005) provide supportive evidence, whereas Francis et al. (1994) and Johnson et al. (2001) do not.

The second reason 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 for issuing warnings because the damage caused by being silent would be greater.

Several studies have examined the capital-market consequences of warnings. KL find that warning firms experience larger price declines than non-warning firms in the window that covers both the warning and the subsequent earnings announcement. This finding is confirmed by Atiase et al. (2006) in a large sample that has small earnings shortfalls. KL tentatively attribute their finding to the possibility that warnings signal a permanent earnings decline and also propose market overreaction as an alternative explanation.

Two subsequent studies examine a research question similar to mine. Shu (2003) uses 104 warning firms that have large earnings shortfalls in the 1994–1995 quarters. She finds that warning firms' short-term returns are weakly significantly lower than those of non-warning firms, but the warning effect is positive after she controls for firms' self-selection. Xu (2003) collects 151 warnings about large earnings shortfalls during 1991–1994. She finds that warning firms have larger downward analyst revisions and lower operating income than non-warning 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 non-warning firms after controlling for self-selection. Moreover, she reports that warning firms earn lower excess returns than non-warning firms over a size-M/B-momentum return benchmark in the 12 to 36 months *after* the event-quarter, concluding that investors have, in fact, under-reacted to warnings.

These studies do not provide a clear answer to my research question. In the short-term window, Shu (2003) and Xu (2003) report contradictory findings. In the long-term window, Xu (2003) does not control for self-selection and the drift she documents is intriguing. It is thus unclear whether a firm is worse off in the long run for issuing a warning, given its risks, earnings news, and non-earnings news known to the manager at the disclosure decision.

III. ECONOMETRIC MODEL

To determine whether the decision to warn is penalized by investors, I use a Heckman model (Heckman 1979, 2001) to control for the effect of firms' self-selection.

Heckman Model

I model investors' decisions and managers' warning choices in the following system:

$$R_{1i} = \alpha_1 + X_i\beta + v_{1i} \text{ (data are observed only when } Warn_i^* > 0\text{);} \tag{1}$$

$$R_{0i} = \alpha_0 + X_i\beta + v_{0i} \text{ (data are observed only when } Warn_i^* \leq 0\text{);} \tag{2}$$

$$Warn_i^* = Z_i\gamma + \varepsilon_i. \tag{3}$$

A firm has two possible states: warning or non-warning. Equations (1) and (2) model stock returns (R) in the warning state and non-warning state for the *whole population*, respectively. X is the row vector of control variables that include risk factors and earnings news (β is the column vector of coefficients), implying that v_1 and v_0 represent non-earnings news to investors but are unobservable to researchers. The warning effect is $\alpha_1 - \alpha_0$. In reality, only warning firms are observed for estimating Equation (1) and only non-warning firms are observed for estimating Equation (2), presenting a challenge in estimating the warning effect.

Equation (3) is the warning model. *Warn** is a continuous latent variable known to a manager but *not* to researchers, for example, a measure of the pressure that the manager feels about issuing a warning. If the measure is higher than a threshold (normalized to 0 in the model), then the manager issues a warning and the binary variable *Warn* is 1. Otherwise, the manager keeps silent and *Warn* is 0. What researchers observe is merely *Warn*—whether a firm warns. Assuming that Z is the row vector of all the factors that affect a manager's warning decision and are observable to researchers, such as the earnings shortfall, ε then represents the factors that affect the manager's decision but are *unobservable* to researchers.

Unlike in a standard regression, in Equation (3) after γ is estimated, the residual corresponding to ε_i is unobservable because $Warn_i^*$ is unobservable. One can make the following probabilistic statements about ε_i after observing $Warn_i$ and $Z_i\gamma$. Given that a firm warned, ε_i is likely to be high when $Z_i\gamma$ is low, thereby triggering the threshold to warn. 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 non-warning group, high ε_i is associated with low $Z_i\gamma$ and ε_i varies within each group as long as Z_i varies.

The three unobservables—v_1, v_0, and ε—are the focus of the Heckman model. The Heckman literature traditionally assumes that the choice model can be implemented by fitting data into the cumulative probability function of the standard 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. If the sets of non-earnings news represented by v_1 and v_0 do not intersect at all with the set of factors represented by ε, then $\sigma_{\varepsilon v_1}$ and $\sigma_{\varepsilon v_0}$ are 0 and the whole system collapses to stand-alone regressions. Otherwise, the three equations are intertwined as follows.

Using the properties of truncated normal distributions, one can mathematically derive the expected return of a firm based on whether the firm has warned and the observables X and Z (Greene 2003, 759). On observing a warning, investors use Equation (4) to assess stock value; for a firm that does not warn, investors use Equation (5). The last terms in Equations (4) and (5) are the "self-selection terms," where $\phi(\cdot)$ and $\Phi(\cdot)$ are the standard normal p.d.f. and c.d.f., respectively:

$$E(R_{1i}|_{warm_i=\mathbf{1}}) = \alpha_1 + X_i\beta + E(v_{1i}|_{\varepsilon_i > -Z_i\gamma}) = \alpha_1 + X_i\beta + \sigma_{\varepsilon v1}\frac{\phi(Z_i\gamma)}{\Phi(Z_i\gamma)}; \tag{4}$$

$$E(R_{0i}|_{warm_i=0}) = \alpha_0 + X_i\beta + E(v_{0i}|_{\varepsilon_i \leq -Z_i\gamma}) = \alpha_0 + X_i\beta + \sigma_{\varepsilon v0}\frac{-\phi(Z_i\gamma)}{1-\Phi(Z_i\gamma)}. \tag{5}$$

Interpretation of the Self-Selection Terms

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). In this case, ε is higher in the presence of a larger amount of other bad news. If stock prices fully impound other bad news, then v_1 and v_0 should each be lower for firms with a larger amount of bad news and thus a higher ε. Therefore, $\sigma_{\varepsilon v_1}$ and $\sigma_{\varepsilon v_0}$ are negative. The negative covariances are due to the factors (unobservable to researchers) that *both* managers and investors consider in their respective decisions; these factors are *other bad news*.[7]

The self-selection terms in Equations (4) and (5) are investors' price adjustments for a firm's other bad news. The signs of $\sigma_{\varepsilon v_1}$ and $\sigma_{\varepsilon v_0}$ determine the direction of price adjustments for each group. Because $\phi(\cdot)$ and $\Phi(\cdot)$ are positive functions, negative $\sigma_{\varepsilon v_1}$ and $\sigma_{\varepsilon v_0}$ mean that the price adjustment for a warning firm is *downward* and that for a non-warning firm *upward*. Within the warning group, the amount of price adjustment $\left(\frac{\phi(Z_i\gamma)}{\Phi(Z_i\gamma)}\right)$ decreases with $Z_i\gamma$ and thus increases with ε_i (see Appendix A). Similarly, within the non-warning group, the amount of price adjustment $\left(\frac{\phi(Z_i\gamma)}{1-\Phi(Z_i\gamma)}\right)$ increases with $Z_i\gamma$ and thus decreases with ε_i. The above relations indicate that within the warning group a firm that has a larger amount of other bad news (i.e., a higher ε_i) receives a larger price cut than a firm that has less such news. Similarly, within the non-warning group a firm with a larger amount of other good news (i.e., less other bad news—a lower ε_i) receives a larger price increase than a firm with less such news. I follow the Heckman literature and label $\frac{\phi(Z_i\gamma)}{\Phi(Z_i\gamma)}$ and $\frac{-\phi(Z_i\gamma)}{1-\Phi(Z_i\gamma)}$ as the "inverse Mills ratio" (represented by variable *Mill*) for a warning firm and a non-warning firm, respectively.

Empirical Selection Model and Its Comparison with the OLS Model

Equations (4) and (5) represent two regressions that can consistently estimate the true coefficients α_1, α_0, and β. I combine these two regressions to form Equation (6), which is my application of the Heckman selection model. Here, $\theta_{TT} = \alpha_1 - \alpha_0$, estimating the warning effect.

$$\begin{aligned} R_i = {} & \alpha_0 + \theta_{TT}\, Warn_i + X_i\beta + \sigma_{\varepsilon v_1} Mill_i{*}Warn_i \\ & + \sigma_{\varepsilon v_0} Mill_i{*}(1 - Warn_i) + w_i. \end{aligned} \tag{6}$$

[7] Other factors (unobservable to researchers) that either managers or investors but not both consider do not contribute to nonzero covariances and, thus, do not affect the self-selection terms.

In comparison, if one estimates Equations (1) and (2) in an OLS using the warning and non-warning observations, respectively, the observed coefficients—$\alpha_{1,OLS}$, $\alpha_{0,OLS}$, and β_{OLS}—are potentially inconsistent estimates of the *true* coefficients—α_1, α_0, and β—because of the traditional omitted-correlated-variables problem. That is, the expected value of v_1 for a warning observation is the last term in Equation (4) and the expected value of v_0 for a non-warning observation is the last term in Equation (5). The error terms v_1 and v_0 are expected to be correlated with X as long as $\sigma_{\varepsilon v_1}$ and $\sigma_{\varepsilon v_0}$ are not 0 and Z and X have common variables. Even though the OLS coefficients are inconsistent for the true coefficients, we can still write Equations (7) and (8). Equation (9) combines Equations (7) and (8) in one regression, where $\theta_{OLS} = \alpha_{1,OLS} - \alpha_{0,OLS}$, representing the between-group return difference after controlling for X:

$$E(R_{1i}|_{warm_i=1}) = \alpha_{1,OLS} + X_i\beta_{OLS}; \tag{7}$$

$$E(R_{0i}|_{warm_i=0}) = \alpha_{0,OLS} + X_i\beta_{OLS}; \tag{8}$$

$$R_i = \alpha_{0,OLS} + \theta_{OLS}Warn_i + X_i\beta_{OLS} + u_i. \tag{9}$$

KL estimate the between-group return difference θ_{OLS}, not the warning effect θ_{TT}. To see the relation between these two, I subtract both sides of Equations (5) from (4), subtract both sides of Equations (8) from (7), and then take the sample average. Both subtractions result in the *same* left-hand side. The right-hand side of the first subtraction is θ_{TT} plus the average difference between warning firms' price adjustments for other bad news and non-warning firms' price adjustments for other bad news—the self-selection effect (''SS''). The right-hand side of the second subtraction is θ_{OLS}. Equating the right-hand sides of both subtractions, I obtain Equation (10): θ_{OLS} can be decomposed into θ_{TT} and SS.[8] If the OLS model rather than the selection model is used, then SS is the estimation bias in determining the warning effect:

$$\theta_{OLS} = \theta_{TT} + E\left[\sigma_{\varepsilon v_1}\frac{\phi(Z_i\gamma)}{\Phi(Z_i\gamma)} - \sigma_{\varepsilon v_0}\frac{-\phi(Z_i\gamma)}{1-\Phi(Z_i\gamma)}\right]. \tag{10}$$

IV. EMPIRICAL PREDICTIONS

I predict that warnings are not penalized by investors (i.e., $\theta_{TT} = 0$) *in the long run* because warnings are voluntary.[9] Otherwise, managers who are presumably rational in making disclosure decisions would stop issuing warnings and the warning phenomenon should

[8] See Greene (2003, 788). Heckman (2001, 718) shows this decomposition relation in more general terms:

$$\theta_{OLS} = E(R_1|_{warn=1}) - E(R_0|_{warn=0}) = [E(R_1|_{warn=1}) - E(R_0|_{warn=1})] + [E(R_0|_{warn=1}) - E(R_0|_{warn=0})].$$

Adding and subtracting $E(R_0|_{warn=1})$ makes this relation clear. The first bracket on the right-hand side is θ_{TT}, generally called the ''treatment effect on the treated group''; the second bracket is SS.

[9] Investors require higher returns from firms for which public information is lacking (Klein and Bawa 1976; Barry and Brown 1985; Easley and O'Hara 2004). So, in theory 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, non-warning firms lose analyst coverage after failing to warn.

vanish. I examine four major market scenarios that are consistent with this prediction.[10] These scenarios are organized around (1) whether self-selection exists and (2) whether stock prices behave as if investors process news efficiently. In Figure 2, I summarize my predictions for the between-group return difference (θ_{OLS}), warning effect (θ_{TT}), and self-selection effect (SS) in the short- and long-term windows in each scenario.

Scenario A—Efficient Market and No Self-Selection

If managers do not consider non-earnings news when deciding whether to warn, then there is no issue of self-selection and therefore SS is 0 in both the short- and long-term. In the absence of SS, Equation (10) indicates that θ_{OLS} should be same as 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 short-term, then the short-term returns should be no different from the long-term returns, so my predictions for θ_{OLS}, θ_{TT}, and SS in the short-term are also 0 under Scenario A. Note that KL find a negative θ_{OLS}, invalidating Scenario A. My discussion of this scenario is merely for benchmarking.

Scenario B—Inefficient Market and No Self-Selection

As proposed by KL, investors may overreact to warnings. Investors are often panicked by bad news because, as Kahneman and Tversky (1979) argue, the value function of a risk-averse individual is commonly convex for losses. The market may be more alarmed by warnings than by negative earnings news released by non-warning firms in earnings announcements. For a small number of firms, a warning only foreshadows what is to 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 the act of warning.

In Scenario B, θ_{OLS} is predicted to be negative in the short-term because of the overreaction. Market overreaction is inevitably corrected in the long-term, so 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- and long-term.

Scenario C—Efficient Market and Self-Selection

Assume managers are more (less) likely to warn when they have more (less) other bad news. If stock prices fully impound such news, then the average return of warning firms should be lower than that of non-warning firms even if both groups have the same risks and earnings news—SS is negative. Because my overall prediction for θ_{TT} in the long run is 0, I expect that θ_{OLS} is equal to SS and is thus negative in the long run in Scenario C. In this efficient market scenario, my predictions for θ_{OLS}, θ_{TT}, and SS in the short-term are the same as those in the long-term.

Scenario D—Inefficient Market and Self-Selection

This scenario deviates from Scenario C in that stock prices do *not* fully impound other bad news in the short-term. All the predictions for the *long-term* in this scenario remain

[10] In an unreported post-event return test, I find no evidence that investors under-react to non-warning firms' negative earnings surprises. For brevity, this scenario is not proposed in this section.

FIGURE 2
Empirical Predictions

OLS Model: $R_i = \alpha_{0,OLS} + \theta_{OLS}\, Warn_i + X_i \beta_{OLS} + u_i$

Selection Model: $R_i = \alpha_0 + \theta_{TT}\, Warn_i + X_i \beta + \sigma_{\varepsilon v_1}\, Mill_i * Warn_i + \sigma_{\varepsilon v_0}\, Mill_i * (1 - Warn_i) + w_i$

Decomposition:

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

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

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

In the model, *R* is stock return, *X* is the row vector of variables controlling for risks and earnings news (β is the column vector of coefficients), and *Warn* is 1 for warning firms and 0 for non-warning 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 non-warning firm, where $\phi(.)$ is the p.d.f. and $\Phi(.)$ is the c.d.f. of the standard normal distribution and z is the row vector of warning factors observable to researchers. $\sigma_{\varepsilon v_1}$ ($\sigma_{\varepsilon v_0}$) is the covariance of the error terms of the warning model and the regression of *R* on *X* and the constant term using warning (non-warning) observations.
Scenarios A and C are self-explanatory. Scenario B allows for market overreaction to the act of warning. Scenario D allows for incomplete or directionally incorrect stock price adjustments for other bad news in the short-term.
See Figure 3 for the short-term window (ST) and the long-term windows (LT).

the same as those in Scenario C. In the short-term, investors' price adjustments for other bad news may be incomplete or directionally incorrect. Next, I explain why the inefficiency may occur and then explain my predictions for θ_{OLS}, θ_{TT}, and SS in the *short-term*.

Traditional economics assumes that agents are globally rational—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 is gathered and digested (Gonedes 1976; Lee 2001; O'Hara 2003).

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

In light of KL's finding, I predict θ_{OLS} to be negative in the short-term in Scenario D. Because, as shown in Equation (10), the self-selection effect is the difference between investors' average price adjustment for warning firms $\left(\sigma_{\varepsilon\upsilon_1}\dfrac{\phi(Z_i\gamma)}{\Phi(Z_i\gamma)}\right)$ and their average price adjustment for non-warning firms $\left(\sigma_{\varepsilon\upsilon_0}\dfrac{-\phi(Z_i\gamma)}{1-\Phi(Z_i\gamma)}\right)$, SS depends on the signs and magnitudes of $\sigma_{\varepsilon\upsilon_1}$ and $\sigma_{\varepsilon\upsilon_0}$. Therefore, my prediction for SS in the short-term is unclear. My prediction for θ_{TT} in the short-term is equally unclear because θ_{TT} is the residual effect in θ_{OLS} after SS is removed.

V. VARIABLES

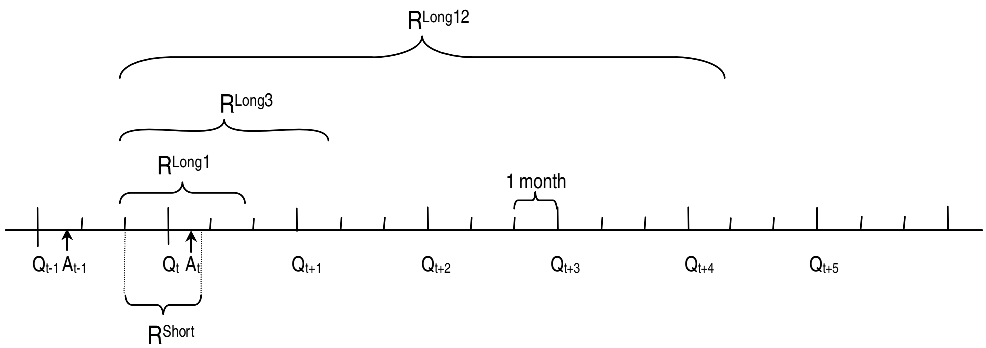
Dependent Variable

The dependent variable in my return analysis is a firm's buy-and-hold return over various windows (Figure 3). The short-term return (R^{Short}) is measured from the beginning of the third fiscal month of the event quarter (i.e., the quarter in which an earnings shortfall occurs) to five 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 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—Factor Returns

The finance literature has identified market risk, firm size, market-to-book ("M/B"), momentum, and industry as factors that affect cross-sectional stock returns. I use a portfolio approach to control for these effects. At the beginning of each month, I form beta deciles, 20 size groups, M/B deciles, momentum deciles, and 48 Fama and French (1997; hereafter Fama-French) industry groups using all U.S. common stocks covered by CRSP and Compustat. Beta is the coefficient on equal-weighted market returns in the market model that uses daily returns in the one-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

FIGURE 3
Return Windows



Q represents the end of a fiscal quarter and A represents the earnings announcement date.
R^{Short} is measured from the beginning of the third fiscal month of the event quarter to five 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 one 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 three 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.

over the book value of equity reported in the most recent quarter. Momentum is measured by the sum of past six-month returns.

To control for each factor, I match a sample observation with a portfolio to which the firm belongs at the beginning of the short-term 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, and use the equal-weighted mean as the factor return, denoted as R^{beta}, R^{size}, R^{mb}, R^{mom}, and R^{ind}, respectively.[11]

Control Variables—Accounting Characteristics

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

[11] Finance studies commonly control for these factors by subtracting the benchmark portfolio return (the same size, M/B, and momentum group as the sample firm) from the sample firm's return and then using the excess return as the dependent variable. When the number of factors is large, this approach yields a benchmark portfolio with a small number of firms. 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.

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.

Control Variables—Earnings Innovations

I use two variables to capture the change in stock prices due to news. The first variable is the event-quarter earnings surprise (*Surprise*) measured as the difference between the forthcoming earnings per share (''EPS'') and analyst consensus (both from I/B/E/S). For warning firms, the consensus is the most recent one compiled before the warning; for non-warning firms, the consensus is the last one before the beginning of the short-term window.

The second variable is future earnings change (*FutureEPS*). I control for *FutureEPS* in the short-term window because stock price leads earnings: a portion of current price change is due to information about future earnings before they are recognized in accounting books (Beaver et al. 1980). I control for *FutureEPS* in the long-term window because stock prices after the event quarter are affected by the portion of future earnings change that investors do 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 observable variables that capture managers' litigation-, reputation-, and earnings-torpedo-related motives for issuing warnings. For the litigation-related motive, I include (1) general litigation risk (*LitigRisk*), (2) expected legal settlement costs (*LogMVE* and *LogSurprise*), and (3) specific litigation risk from a firm's failure to update a previously issued projection for the event quarter (*Forecast*).

LitigRisk is the probability of being sued (see Appendix B). Because of 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 a larger magnitude of earnings shortfall. The two components arguably have a multiplicative relation (Skinner 1997), so 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, then managers may be concerned about potential lawsuits from failing to update or correct the previous projection.[12] Because 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 issues a forecast about the event quarter before the third fiscal month, and 0 otherwise.

For the reputation-related motive, I use past disclosure frequency (*PastDisclosure*) and 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, then the prior disclosure level is considered 0. *PastFollow* is the average number

[12] 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 to correct. The distinction between ''update'' and ''correct'' is ambiguous, causing controversy among legal experts.

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 firms in the event quarter that are covered by Compustat, CRSP, and I/B/E/S. This conversion controls for potential time trends in First Call, media, and analyst coverage as well as fluctuations in firms' disclosures due to market conditions or the legal environment.

To capture the motive of growth firms for softening the earnings-torpedo effect, I use M/B (Skinner and Sloan 2002; Anilowski et al. 2006). Finally, I control for earnings volatility because managers with more volatile earnings may feel less need to warn investors of an earnings fluctuation. In sum, Equation (11) implements the warning model (3). The explanatory variables in Equation (11) constitute the elements of Z in Equation (3):

$$\begin{aligned} Pr(Warn_i = 1) = \Phi(&a_0 + a_1 LitigRisk_i + a_2 LogMVE_i + a_3 LogSurprise_i \\ &+ a_4 Forecast_i + a_5 PastDisclosure_i + a_6 PastFollow_i \\ &+ a_7 M/B_i + a_8 EarnVolt_i + \varepsilon_i). \end{aligned} \tag{11}$$

After estimating Equation (11), I calculate *Mill* as $\frac{\phi(Z_i\gamma)}{\Phi(Z_i\gamma)}$ and $\frac{-\phi(Z_i\gamma)}{1 - \Phi(Z_i\gamma)}$ for a warning and a non-warning firm, respectively (see Section III). *Mill* is then included in the return regression to control for self-selection. Equation (12) implements the selection model (6):

$$\begin{aligned} R_i = c_0 &+ \theta_{TT} Warn_i + c_2 R^{beta}{}_i + c_3 R^{size}{}_i + c_4 R^{mb}{}_i + c_5 R^{mom}{}_i + c_6 R^{ind}{}_i \\ &+ c_7 Leverage_i + c_8 ROA_i + c_9 EarnVolt_i + c_{10} Surprise_i \\ &+ c_{11} FutureEPS_i + \sigma_{\varepsilon\upsilon_1} Mill_i{*}Warn_i + \sigma_{\varepsilon\upsilon_0} Mill_i{*}(1 - Warn_i) + w_i. \end{aligned} \tag{12}$$

VI. DATA

Data Collection

I collect earnings warnings from the First Call Company Issued Guidelines ("CIG") database and use 1996Q1 to 2003Q2 (calendarized) as my sample period.[13] This period starts with 1996 because the passage of the Private Securities Litigation Reform Act of 1995 expanded the safe-harbor protection to firms for issuing forward-looking information, raised the bar for class action lawsuits, and thus changed firms' legal and information environment.

Table 1 summarizes the collection of my warning events. During the sample period, U.S. companies issued 8,692 negative guidelines about quarterly earnings.[14] In addition to

[13] "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 percent of the firms covered by Compustat end their fiscal years on December 31 and the next most popular fiscal-year-end is June 30.

[14] First Call collects company disclosures from press releases and interviews, compares them with existing market expectations, and codes them as positive, negative, or in-line guidance. 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. The misclassified events are likely excluded from the sample when I delete the events with earnings surprises higher than −0.001 in the data procedures.

TABLE 1
Sample Collection

Panel A: Collection of Warning Events

Procedures	**Change**	**Remaining Events**
Negative guidance about *quarterly* earnings for 1996Q1–2003Q2		8,692
Negative guidance about *annual* earnings issued after the beginning of the last fiscal month of a fiscal year (treated as warnings for Q4)	+181	8,873
Negative guidance about quarterly earnings issued *before* the beginning of the third fiscal month of a fiscal quarter	−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 the earnings announcement date	−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)

events excluded due to missing data, I exclude 3,214 negative guidelines issued before the third fiscal month of a quarter because my study focuses on warnings, not forecasts. I also exclude 469 events of extremely small news, defined as the event-quarter earnings surprise higher than −0.001. Among these events 222 are small negative earnings surprises; they are excluded because managers may be unaware of such a small shortfall. The remaining events are positive earnings surprises according to I/B/E/S even though CIG classifies these events as negative earnings guidelines. 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 non-warning observations are the firm quarters in which the forthcoming earnings are lower than analyst 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 growth, or EBITDA, then this firm quarter is excluded from the non-warning 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 non-warning observations.

Descriptive Statistics

Table 2 describes the warning and non-warning 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 Table 2, Panel B I report the sum of daily market-adjusted returns in the warning and earnings-announcement event windows as well as in the short-term window. Warning firms experience a mean return of −13.2 percent during the three trading days around warning but the return around earnings announcement is close to 0, suggesting that warning is a timing issue rather than an issue of piecemeal disclosure. For those that have waited to release bad news until earnings announcement, the average three-trading-day return is −2.1 percent. On the other hand, warning and non-warning firms experience a mean return of −19.5 percent and −8.4 percent in the short-term window, respectively. These returns are more negative than the three-day event window returns, probably because of intra-industry information transfer (i.e., a price decline upon peers' warnings) and the arrival of other bad news.

Panel C of Table 2 describes the main variables used in the empirical tests. Warning firms have lower buy-and-hold returns, worse event-quarter earnings news (median), and larger declines in future earnings than non-warning firms. As Barber and Lyon (1997) note, returns tend to be positively skewed when the window is long (e.g., R^{Long12}). Panel D reports the pairwise 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 of Table 2 presents the correlations between firms' buy-and-hold returns and the factor returns. For brevity, I only report the three-month extension window; the correlations for other windows are similar. The correlation between realized returns and industry factor return is the highest among all correlations involving realized returns; beta factor return comes next. The correlations between the factor returns themselves range from 0.654 to 0.789, yet, according to my variance-inflation-factor analysis (unreported), multicollinearity is not a concern in the multivariate tests.

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. I then report my primary test results—the tests when self-selection *is* controlled for. I 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. Figure 4 summarizes the key results.[15]

[15] To avoid spurious relations caused by outliers, I winsorize the dependent and continuous independent variables in the return regressions (except for *Mill*) at 1 percent and 99 percent of the distributions. The test results are very similar when the dependent variables are not winsorized or when robust regressions are used (the latter is robust to outliers in both the dependent and independent variables and to violation of normality in the error term).

TABLE 2
Descriptive Statistics

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

		Warning		Non-Warning		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%

Panel B: Stock Returns[b]

Mean Return	**Warning (Three-Trading-Day)**	**Earnings Announcement (Three-Trading-Day)**	**Short-Term Window**
Warning	−0.132***	−0.004***	−0.195***
Non-Warning	NA	−0.021***	−0.084***

Panel C: Univariate Analysis of Main Variables

	Warning (3,869)		Non-Warning (23,158)		Between-Group Test (Warning − Non-Warning)	
	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***

(*continued on next page*)

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
FutureEPS	0.100	0.192	−0.012[#]	−0.234	0.185	0.152	

Panel E: Pearson Correlations between Stock Returns and Factor Returns[c]

Three-Month Extension	R^{Long3}	R^{beta}	R^{size}	R^{mb}	R^{mom}
R^{beta}	0.389				
R^{size}	0.313	0.735			
R^{mb}	0.340	0.768	0.789		
R^{mom}	0.332	0.774	0.783	0.767	
R^{ind}	0.419	0.690	0.654	0.672	0.671

*** Indicates statistical significance at 1 percent in a two-tailed test.
[#] Indicates statistical insignificance at 10 percent in a two-tailed test. The unmarked correlations in Panel D are statistically significant at 5 percent in a two-tailed test.
[a] The industries are classified according to Fama and French (1997). See Table 1 for the identification of warning observations. Non-warning 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.
[b] The returns are the sum of daily market-adjusted return for each group. The short-term window runs from the beginning of the third fiscal month of the event quarter to five days after the event-quarter earnings announcement date. The three-trading day is [–1, 1], where 0 is the event day.
[c] All the correlations are statistically significant at 1 percent in a two-tailed test. The Spearman correlations are similar and are thus not reported.

Variable Definitions for Panels C and D:

R^{Short} = a firm's buy-and-hold return from the beginning of the third fiscal month of the event quarter to five days after the event-quarter earnings announcement date;
R^{Long1}, R^{Long3}, and R^{Long12} = a firm's buy-and-hold return from the beginning of the third fiscal month of the event quarter to one, three, and 12 months after the end of the event-quarter earnings announcement month, respectively;
Beta = the coefficient on equal-weighted market returns in a market model using daily returns in the one-year period before the event quarter;
MVE = the market value of equity at the beginning of the event quarter (in millions);
M/B = the market-to-book ratio at the beginning of the event quarter;
Leverage = the average debt-to-assets ratio in the four quarters before the event quarter;
ROA = the average accounting return on assets in the four quarters before the event quarter;
EarnVolt = the standard deviation of accounting return on assets in the eight quarters before the event quarter;
FutureEPS = 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; and

For a warning observation, *Surprise* = 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 non-warning observation, *Surprise* is the difference between the forthcoming EPS and the last analyst consensus before the third fiscal month of the event quarter. Earnings surprise is deflated by the split-adjusted beginning-of-event-quarter stock price.
All variables except for *MVE* are winsorized at 1 percent and 99 percent in the full sample.

(*continued on next page*)

TABLE 2 (continued)

Variable Definitions for Panel E:

R^{Long3} =	a firm's buy-and-hold return from the beginning of the third fiscal month of the event quarter to three months after the end of the event-quarter earnings announcement month; and
R^{beta}, R^{size}, R^{mb}, R^{mom}, and R^{ind} =	the factor 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 one-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 sum of past six-month stock returns. For each warning and non-warning observation, I determine its benchmark portfolio affiliation at the beginning of the third fiscal month. I purge all warning and non-warning observations from the benchmark portfolio, randomly choose 100 members from each portfolio, calculate the buy-and-hold returns for each member over the same holding period as the dependent variable, and use the equal-weighted mean as the factor return.

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, issued a forecast for the event quarter early on, gave more disclosures, or 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 percent.[16]

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 my return regression without control for self-selection. The coefficient on *Warn*, θ_{OLS}, is not only significantly negative, but is also surprisingly close to −10 percent across all the windows (including the six-, nine-, and 12-month extensions, unreported). Specifically, θ_{OLS} is −10.1 percent for the short-term window and −9.8 percent for the three-month-extension 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 non-warning group. The negative θ_{OLS} in both the short- and long-term *invalidates Scenario B*.

Return Tests with Control for Self-Selection

Panel C of Table 4 reports my return regression when I add the control for self-selection. The t-statistics are robust to heteroscedasticity (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. Below, I draw inferences from Panel C rather than Panel D because the latter uses only 30 quarterly cross-sections and, as a result of low power, the tests may bias toward finding no warning effect in the long run—my main prediction.

[16] Aboody and Kasznik (2000) find that managers have an incentive to release pessimistic earnings forecasts before option award dates. My estimation in Table 3 does not control for option grant because the use of option data substantially reduces the sample and its coefficient is insignificant (unreported).

FIGURE 4
Summary of Key Results

		Short Term	Long Term	Post Event
Between-Group Return Difference	θ_{OLS}	–0.101***	–0.098***	0.006#
Warning Effect	θ_{TT}	–0.064***	0.007#	0.079***
Self-Selection Effect	SS	–0.037	–0.105	–0.073
Warning Firms' Coefficient on *Mill*	$\sigma_{\varepsilon v_1}$	–0.034***	–0.067***	–0.035**
Non-Warning Firms' Coefficient on *Mill*	$\sigma_{\varepsilon v_0}$	0.049***	–0.039**	–0.092***

***, ** Indicate statistical significance at 1 percent and 5 percent, respectively, in a two-tailed test.
# Indicates statistical insignificance at 10 percent in a two-tailed test.
Short Term is the short-term window running from the beginning of the third fiscal month of the event quarter to five days after the event-quarter earnings announcement date.
Long Term is the long-term window running from the beginning of the third fiscal month of the event quarter to three months after the end of the event-quarter earnings announcement month.
Post Event is the post-event window that begins on the sixth day after the event-quarter earnings announcement date and ends three months after the end of the event-quarter earnings announcement month.
The short- and long-term test results are reported in Table 4. Specifically, the results for θ_{OLS} are in Panel B; the rest are in Panel C. The post-event test results are reported in Panel A of Table 5.
The statistical significance is shown for the variables except for SS.
See Figure 2 for variable definitions.

Panel C of Table 4 shows that the coefficient on *Warn*, θ_{TT}, is significantly negative in the short-term (coefficient = −6.4 percent, t-statistic = −4.21). The negative θ_{TT} indicates that warning firms earn lower returns in the short-term than those that have similar risks, earnings news, and non-earnings news but do not warn. That is, the market penalizes the act of warning in the short-term. However, the penalty diminishes as the window is extended and it disappears for the two- and three-month extensions (also for the six-, nine-, and 12-month extensions, unreported).

Using the decomposition in Equation (10), I estimate the self-selection effect (SS) to be −3.7 percent for the short-term window and −10.5 percent for the three-month extension. The negative SS indicates that, on average, warning firms have a larger amount of other bad news than non-warning firms. The patterns of θ_{OLS}, θ_{TT}, and SS suggest that, in the short-term, the lower returns of warning firms relative to non-warning firms are due to both a negative warning effect and a negative self-selection effect. However, in the long run they are due purely to self-selection. The evidence is *consistent with Scenario D,* not with Scenario C.

To gain insight into why a warning penalty exists in the short-term, I further examine the components of the self-selection effect and their changes from the short to long-term. Panel C of Table 4 reports the coefficients on *Mill* for the warning and non-warning groups, respectively. Recall that the coefficient on warning firms' *Mill* is the estimate of $\sigma_{\varepsilon v_1}$—the covariance of the error terms of return regression (1) and warning model (3). Similarly, the coefficient on non-warning firms' *Mill* is the estimate of $\sigma_{\varepsilon v_0}$—the covariance of the error terms of return regression (2) and warning model (3). If firms are more (less) likely to warn

TABLE 3
Probit Warning Model

$$Pr(Warn_i = 1) = \Phi(a_0 + a_1 LitigRisk_i + a_2 LogMVE_i + a_3 LogSurprise_i + a_4 Forecast_i + a_5 PastDisclosure_i + a_6 PastFollow_i + a_7 M/B_i + a_8 EarnVolt_i; + \varepsilon_i)$$

	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%

*** Indicates statistical significance at 1 percent in a two-tailed test. The number of warning and non-warning observations is 3,869 and 23,158, respectively.

Variable Definitions:

Warn = 1 for the warning group and 0 for the non-warning group;
LitigRisk = 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 one-year period before the event quarter;
LogMVE = the log transformation of market value of equity at the beginning of the event quarter (in millions);
LogSurprise = 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 non-warning observation, it is the difference between the forthcoming EPS and the last analyst consensus before the third fiscal month of the event quarter. Earnings surprise is deflated by the split-adjusted beginning-of-quarter stock price;
Forecast = 1 if a firm has issued earnings forecast about the event quarter before the third fiscal month, and 0 otherwise;
PastDisclosure = the number of positive or negative guidelines about quarterly earnings issued by a firm in the 360 days before the event quarter;
PastFollow = 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 in the event quarter that are covered by Compustat, CRSP, and I/B/E/S;
M/B = the market-to-book ratio at the beginning of the event quarter; and
EarnVolt = 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 percent and 99 percent.

when they have more (less) other bad news *and* stock prices fully impound such news, then both covariances should be negative. Panel C shows that they are indeed negative in the long run, but they change from the short to long-term, suggesting within-group mispricing.

Specifically, although $\sigma_{\varepsilon\upsilon_1}$ is significantly negative in both the short-term (−0.034) and the three-month extension window (−0.067), its magnitude in the short-term is only half of that in the long-term. This finding suggests that the positions that investors initially take to adjust warning firms' prices for other bad news are correct but the adjustments are inadequate. For the non-warning group, $\sigma_{\varepsilon\upsilon_0}$ is significantly negative in the three-month

TABLE 4
Short- and Long-Term Returns

Panel A: KL Return Regression (t-statistics in parentheses, adjusted R^2 = 3.3%)[a]

$$CAR_i = b_0 + b_1Warn_i + b_2Surprise_i + b_3Warn_i * Surprise_i + b_4LogMVE_i + e_i$$

b_0	b_1	b_2	b_3	b_4
−0.043***	−0.107***	0.944***	0.078	−0.005***
(−7.15)	(−20.83)	(14.51)	(0.38)	(−5.02)

Panel B: Return Regression with *No Control* for Self-Selection

$$R_i = c_0 + \theta_{OLS}Warn_i + c_2R^{beta}{}_i + c_3R^{size}{}_i + c_4R^{mb}{}_i + c_5R^{mom}{}_i + c_6R^{ind}{}_i + c_7Leverage_i + c_8ROA_i + c_9EarnVolt_i + c_{10}Surprise_i + c_{11}FutureEPS_i + u_i$$

R =	R^{Short}	R^{Long1}	R^{Long2}	R^{Long3}
Intercept	−0.079***	−0.076***	−0.085***	−0.079***
	(−19.46)	(−15.00)	(−14.94)	(−12.42)
Warn	−0.101***	−0.101***	−0.102***	−0.098***
	(−28.04)	(−22.96)	(−20.59)	(−17.75)
R^{beta}	0.255***	0.368***	0.382***	0.370***
	(9.25)	(13.93)	(16.62)	(14.75)
R^{size}	0.040	−0.060**	−0.022	−0.084***
	(1.34)	(−2.16)	(−0.81)	(−3.04)
R^{mb}	−0.006	0.014	0.072**	0.116***
	(−0.17)	(0.47)	(2.51)	(4.03)
R^{mom}	0.186***	0.098***	0.038	−0.057**
	(6.02)	(3.49)	(1.47)	(−2.15)
R^{ind}	0.509***	0.497***	0.421***	0.535***
	(23.71)	(24.64)	(26.45)	(27.37)
Leverage	0.050***	0.047***	0.048***	0.048***
	(8.61)	(6.45)	(5.84)	(5.25)
ROA	0.018	0.163**	0.284***	0.446***
	(0.35)	(2.51)	(4.03)	(5.70)
EarnVolt	−0.477***	−0.548***	−0.539***	−0.539***
	(−8.10)	(−7.30)	(−6.43)	(−5.69)
Surprise	0.933***	0.918***	0.883***	0.918***
	(12.58)	(9.74)	(8.46)	(8.21)
FutureEPS	0.455***	0.889***	1.160***	1.470***
	(12.03)	(18.17)	(20.85)	(23.97)
R^2	22.9%	25.0%	24.9%	24.7%

Panel C: Return Regression *with Control* for Self-Selection[b]

$$R_i = c_0 + \theta_{TT}Warn_i + c_2R^{beta}{}_i + c_3R^{size}{}_i + c_4R^{mb}{}_i + c_5R^{mom}{}_i + c_6R^{ind}{}_i + c_7Leverage_i + c_8ROA_i + c_9EarnVolt_i + c_{10}Surprise_i + c_{11}FutureEPS_i + \sigma_{\varepsilon\upsilon_1}Mill_i{*}Warn_i + \sigma_{\varepsilon\upsilon_0}Mill_i{*}(1 - Warn_i) + w_i$$

Intercept	−0.068***	−0.072***	−0.086***	−0.087***
	(−14.46)	(−12.19)	(−13.01)	(−11.89)
Warn	−0.064***	−0.041**	−0.029	0.007
	(−4.21)	(−2.24)	(−1.37)	(0.30)

(*continued on next page*)

TABLE 4 (continued)

R =	R^{Short}	R^{Long1}	R^{Long2}	R^{Long3}
R^{beta}	0.254***	0.337***	0.381***	0.368***
	(9.23)	(13.92)	(16.57)	(14.68)
R^{size}	0.041	−0.059**	−0.020	−0.081***
	(1.40)	(−2.11)	(−0.74)	(−2.94)
R^{mb}	−0.004	0.013	0.072**	0.116***
	(−0.13)	(0.45)	(2.49)	(4.02)
R^{mom}	0.185***	0.097***	0.038	−0.057**
	(5.97)	(3.47)	(1.45)	(−2.15)
R^{ind}	0.508***	0.497***	0.421***	0.535***
	(23.68)	(24.64)	(26.45)	(27.39)
Leverage	0.051***	0.047***	0.047***	0.045***
	(8.81)	(6.40)	(5.67)	(4.90)
ROA	0.032	0.170***	0.287***	0.441***
	(0.63)	(2.62)	(4.06)	(5.63)
EarnVolt	−0.508***	−0.552***	−0.525***	−0.496***
	(−8.56)	(−7.30)	(−6.20)	(−5.20)
Surprise	0.876***	0.904***	0.902***	0.984***
	(11.55)	(9.36)	(8.41)	(8.57)
FutureEPS	0.452***	0.901***	1.164***	1.480***
	(11.93)	(18.18)	(20.91)	(24.11)
*Mill*Warn*	−0.034***	−0.044***	−0.050***	−0.067***
	(−3.40)	(−3.62)	(−3.60)	(−4.27)
Mill(1 − Warn)*	0.049***	0.018	−0.006	−0.039**
	(4.61)	(1.35)	(−0.38)	(−2.39)
R^2	23.0%	25.0%	25.0%	24.7%

Panel D: Robust Standard Error and Fama-MacBeth Regressions[c]

R =	R^{Short}		R^{Long3}	
	Robust Standard Error	**Fama-MacBeth**	**Robust Standard Error**	**Fama-MacBeth**
Intercept	−0.068***	−0.056***	−0.087***	−0.099***
	(−8.27)	(−5.75)	(−7.71)	(−5.27)
Warn	−0.064***	−0.087***	0.007	−0.017
	(−2.58)	(−4.87)	(0.16)	(−0.52)
R^{beta}	0.254**	0.402***	0.368***	0.311***
	(2.43)	(7.71)	(7.73)	(5.83)
R^{size}	0.041	0.087**	−0.081	0.119*
	(0.54)	(1.99)	(−1.18)	(1.81)
R^{mb}	−0.004	0.042	0.116***	0.170***
	(−0.05)	(0.57)	(2.85)	(2.73)
R^{mom}	0.185***	0.075	−0.057	−0.007
	(2.78)	(1.30)	(−1.25)	(−0.20)
R^{ind}	0.508***	0.449***	0.535***	0.485***
	(10.19)	(11.64)	(15.85)	(13.41)
Leverage	0.051***	0.041***	0.045**	0.024
	(4.31)	(3.86)	(2.11)	(1.17)
ROA	0.032	−0.012	0.441***	0.368***
	(0.132)	(−0.13)	(3.10)	(2.73)

(*continued on next page*)

TABLE 4 (continued)

***R* =**	R^{Short}		R^{Long3}	
	Robust Standard Error	**Fama-MacBeth**	**Robust Standard Error**	**Fama-MacBeth**
EarnVolt	−0.508***	−0.460***	−0.496***	−0.395***
	(−7.96)	(−7.52)	(−4.69)	(−3.49)
Surprise	0.876***	0.870***	0.984***	1.039***
	(6.73)	(6.09)	(6.20)	(5.02)
FutureEPS	0.452***	0.490***	1.480***	1.524***
	(8.19)	(9.94)	(18.47)	(20.72)
*Mill*Warn*	−0.034***	−0.022**	−0.067***	−0.052***
	(−2.69)	(−2.15)	(−2.71)	(−2.86)
*Mill**(1 − *Warn*)	0.049**	0.057***	−0.039	−0.031
	(2.23)	(2.67)	(−1.16)	(−0.85)
R^2	23.0%	21.0%	24.7%	21.9%

***, **, * Indicate statistical significance at 1 percent, 5 percent, and 10 percent in a two-tailed test, respectively.

The t-statistics are in the parentheses. In Panels B and C, the t-statistics are adjusted for heteroscedasticity (Huber/White/Sandwich standard error estimator).

[a] *CAR* is the sum of daily market-adjusted return from the beginning of the third fiscal month of the event quarter to five 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).

[b] The estimation takes two steps. In the first step I estimate the probit warning model and calculate *Mill*. In the second step I add *Mill* to the return regression and estimate the regression allowing for heteroscedasticity (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 regression, for which the statistical package makes the variance correction, with those without variance correction and find little difference.

[c] The robust standard error estimation allows for heteroscedasticity 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 quarterly cross sections and the mean coefficient estimates and R^2 are reported.

Variable Definitions:

Warn = 1 for a warning observation (3,869) and 0 for a non-warning observation (23,158);

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

R^{Long1}, R^{Long2}, and R^{Long3} = a firm's buy-and-hold return from the beginning of the third fiscal month of the event quarter to one, two, and three months after the end of the event-quarter earnings announcement month, respectively;

For each warning and non-warning observation, R^{beta}, R^{size}, R^{mb}, R^{mom}, and R^{ind} = the buy-and-hold return of a portfolio of firms in the same beta group (10), firm-size group (20), M/B group (10), return momentum 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 one-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 sum of past six-month stock returns. I purge all warning and non-warning observations from the benchmark portfolio, randomly choose 100 members from each portfolio, calculate the buy-and-hold returns for each member over the same holding period as the dependent variable, and use the equal-weighted mean as the factor return;

Leverage = the average debts-to-assets ratio in the four quarters before the event quarter;

ROA = the average accounting return on assets in the four quarters before the event quarter;

EarnVolt = the standard deviation of accounting return on assets in the eight quarters before the event quarter;

(*continued on next page*)

TABLE 4 (continued)

For a warning observation, *Surprise* = the difference between the forthcoming EPS and the most recent analyst consensus before the warning (both from I/B/E/S). For a non-warning observation, it is the difference between the forthcoming EPS and the last analyst consensus before the third fiscal month of the event quarter. *Surprise* is deflated by the split-adjusted beginning-of-event-quarter stock price;

FutureEPS = 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; and

Mill = 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 non-warning firm, where ϕ and Φ are standard normal p.d.f. and c.d.f., respectively, Z is the row vector of explanatory variables in the warning choice model, and γ is the column vector of coefficients estimated in Table 3.

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

extension window (-0.039), but is significantly positive in the short-term (0.049). The reversal suggests that investors initially make a mistake. In sum, investors are supposed to adjust prices downward for firms with more other bad news relative to those with less *both* within the warning group and within the non-warning group. However, such adjustments do not occur to the non-warning group in the short-term (and the degree of adjustments within the warning group is inadequate). Consequently, a firm with a large amount of other bad news, which therefore tends to warn, is worse off in the short-term for having warned than for being silent.

In unreported tests I partition the sample by Regulation Fair Disclosure (FD) and by the magnitude of earnings surprise. Before FD, some firms might have warned privately but are misclassified into the non-warning 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 higher pseudo R^2 (16.6 percent) than that from the full sample and that my primary results largely hold in both subperiods.[17] Alternatively, I use 1 percent price-deflated earnings surprise as the cutoff to partition the sample into small- and large-surprise subsamples. My primary results hold in both subsamples.

My use of the selection 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 other bad news. Simply put, the model uses $Z_i\gamma$ that are observable to infer other bad news that is unobservable. The more we know about $Z_i\gamma$, the better inferences we can make about other bad news. For example, if the warning model is weak, the self-selection 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.[18]

[17] Before FD, θ_{OLS}, θ_{TT}, and SS are -0.134, -0.116, and -0.018 for the short-term window and -0.125, 0.011, and -0.136 for the three-month extension window, respectively. After FD, θ_{OLS}, θ_{TT}, and SS are -0.054, -0.030, and -0.024 for the short-term window and -0.063, 0.012, and -0.075 for the three-month extension window, respectively. Both the between-group return difference and the short-term warning effect are smaller after FD than before FD perhaps because of firms' higher tendency to warn after FD than before FD (Figure 1).

[18] 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 percent and the warning effect is significantly negative even in the long run. After *Forecast* is added, the pseudo R^2 is improved to 6.1 percent and the warning effect is qualitatively similar to my primary results. The warning effect is insensitive to further adding the remaining identified warning factors. My primary results are also insensitive to (1) adding *FutureEPS* to the warning model, (2) excluding *FutureEPS*, *Leverage*, *ROA*, and *Earnvolt* from the return regression, and (3) measuring *FutureEPS* in the eight rather than four quarters after the event quarter.

Supplementary Tests—Post-Event Returns

I examine post-event returns to corroborate the primary results. The post-event windows start on the sixth day following the event-quarter earnings announcement and end at various points as the long-term windows end. Panel A of Table 5 shows that the post-event warning effect is significantly positive. 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-term window is nil. In addition, the estimate of $\sigma_{\varepsilon\upsilon_0}$ is significantly negative, consistent with its reversal from being positive in the short-term to being negative in the long-term, suggesting a correction of short-term mispricing within the non-warning group. The estimate of $\sigma_{\varepsilon\upsilon_1}$ is significantly negative in the three-month post-event window, suggesting that three months after the event quarter investors have better understanding of warning firms' other bad news, even though their initial positions are directionally correct.

In the second-to-last row, I *exclude* the self-selection terms and report the coefficient on *Warn*. This coefficient is about 0, indicating no return reversal nor drift after the event quarter. In the last row, I re-estimate the coefficient on *Warn* after further excluding *FutureEPS*. Because all the variables are publicly known at the beginning of the return window, this coefficient essentially estimates the abnormal return from a zero-investment trading strategy of longing warning stocks and shorting non-warning stocks after the event quarter. The coefficient is close to 0, indicating that the strategy based solely on the dichotomy of warning versus non-warning is not profitable, confirming that between-group mispricing does *not* exist.[19]

Further, I form trading strategies to exploit within-group mispricing. I sort warning firms and non-warning firms separately into three groups (high, medium, and low) by the predicted warning probability (P). Note that P is low when $Z_i\gamma$ is low. Recall that within the warning group and within the non-warning group, low $Z_i\gamma$ is associated with high ε_i, so low P is associated with a large amount of other bad news and vice versa. If investors initially do not understand this relation, then an arbitrage strategy of selling the warning firms with low P and buying the warning firms with high P after the event quarter is profitable. A similar strategy is applicable to the non-warning group.

Panel B of Table 5 shows, by subtracting Low P returns from High P, that this strategy earns a mean (median) abnormal return of 2.3 (2.5) percent from the warning group and 2.0 (1.6) percent from the non-warning group. The profit from the warning group is weakly significantly positive (t = 1.69; Wilcoxon Z = 1.65). The profit from the non-warning group is significantly positive (t = 3.28; Wilcoxon Z = 3.32). These results support my finding in the primary tests that mispricing exists within the warning group and within the non-warning group. When transaction costs are considered, however, these trading strategies probably yield no net profit (Korajczyk and Sadka 2004). 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 a Heckman selection model to control for price declines due to other bad news, I document

[19] 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 (without the control for self-selection) and further retain only large-surprise firms. I find that the coefficient on *Warn* is positive (coefficient = 0.026, t-statistic = 2.52 for the three-month window; coefficient = 0.043, t-statistic = 1.92 for the 12-month window), contrary to the drift documented by Xu (2003).

TABLE 5
Post-Event Returns

Panel A: Regression Estimation[a]

R =	R^{Post1}	R^{Post2}	R^{Post3}
Intercept	−0.017***	−0.025***	−0.031***
	(−4.08)	(−4.67)	(−4.80)
Warn	0.028**	0.038**	0.079***
	(2.01)	(2.21)	(3.75)
R^{beta}	0.223***	0.240***	0.198***
	(13.31)	(12.60)	(9.39)
R^{size}	0.085***	0.116***	0.064***
	(4.31)	(5.56)	(2.65)
R^{mb}	−0.034	−0.028	0.056**
	(−1.56)	(−1.21)	(2.25)
R^{mom}	−0.039**	−0.059***	−0.078***
	(−2.17)	(−3.02)	(−3.75)
R^{ind}	0.306***	0.372***	0.457***
	(21.33)	(24.00)	(26.42)
Leverage	0.000	−0.000	−0.004
	(0.02)	(−0.02)	(−0.55)
ROA	0.257***	0.364***	0.507***
	(5.54)	(6.42)	(7.56)
EarnVolt	−0.075	−0.113*	−0.088
	(−1.31)	(−1.66)	(−1.07)
Surprise	0.053	−0.030	0.004
	(0.74)	(−0.34)	(0.04)
FutureEPS	0.519***	0.819***	1.140***
	(15.13)	(18.95)	(21.66)
*Mill*Warn*	−0.013	−0.016	−0.035**
	(−1.42)	(−1.41)	(−2.49)
*Mill**(1 − *Warn*)	−0.033***	−0.053***	−0.092***
	(−3.62)	(−4.41)	(−6.43)
R^2	17.8%	19.7%	20.0%
θ_{OLS}	0.000	0.002	0.006
	(0.13)	(0.38)	(1.10)
$\theta_{OLS\ (Excluding\ FutureEPS)}$	−0.001	−0.000	0.003
	(−0.23)	(−0.05)	(0.57)

Panel B: Trading Strategies to Exploit Within-Group Mispricing[b]

Mean (Median)	Warning Firms			Non-Warning Firms		
	High *P*	Low *P*	t-test (Wilcoxon)	High *P*	Low *P*	t-test (Wilcoxon)
P	0.344	0.088		0.227	0.057	
	(0.299)	(0.092)		(0.189)	(0.058)	
Abnormal Return	0.011	−0.012	1.69*	0.005	−0.015	3.28***
	(−0.014)	(−0.039)	(1.65*)	(−0.027)	(−0.043)	(3.32***)
Observations	1,482	680		6,765	4,636	

(*continued on next page*)

TABLE 5 (continued)

***, **, * Indicate statistical significance at 1 percent, 5 percent, and 10 percent in a two-tailed test, respectively.

[a] Notes to Panel A

1. The estimation uses 3,869 warning (*Warn* = 1) and 23,158 non-warning (*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 *Mill* to the return regression, which is then estimated allowing for heteroscedasticity (Huber/White/Sandwich estimator). The t-statistics are in the parentheses.
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 date to one, two, and three months after the end of the event-quarter earnings announcement month, respectively. The three new variables are winsorized at 1 percent and 99 percent in the full sample. See 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 heteroscedasticity.
5. $\theta_{OLS\ (excluding\ FutureEPS)}$ is the least squares coefficient on *Warn* when *Mill* and *FutureEPS* are excluded. The t-statistic is robust to heteroscedasticity.

[b] Notes to Panel B:

1. *P* is the out-of-sample warning probability. Before each quarter between 1998Q1 and 2003Q2, I estimate the warning model using all available warning and non-warning observations prior to the quarter and determine the cutoffs for three warning probability subgroups (high, medium, and low) each within the warning and non-warning groups. I then calculate the out-of-sample warning probability for firms in the current quarter and use the above cutoffs to assign these firms to high, medium, or low *P* groups. To avoid a look-ahead bias, the warning model estimations use unranked variables. The uneven number of firms in ''High *P*'' and ''Low *P*'' groups is due to out-of-sample predictions in 22 quarters as well as upward trends in unranked *PastDisclosure* and *PastFollow*.
2. Abnormal Return is the residual obtained from regressing R^{Post3} on R^{beta}, R^{size}, R^{mb}, R^{mom}, R^{ind}, *Leverage*, *ROA*, *Surprise* (all publicly available information), and a constant. See Notes to Panel A and Table 4 for other variable definitions.

interesting findings. Warning firms have, on average, a larger amount of other bad news than non-warning firms. Investors initially do not fully infer and digest other bad news so that the price adjustments for such news in the short-term are incomplete within the warning group and directionally incorrect within the non-warning group. As a result, firms with a large amount of bad news, which therefore tend to warn, are worse off in the short-term for having warned than for being silent. The warning penalty, however, disappears when the short-term window is extended by three months. The evidence suggests that openness is ultimately not penalized by investors.

My study reinterprets Kasznik and Lev's (1995) finding and provides current, relevant evidence that should alleviate managers' concerns about a market penalty for openness. Moreover, my study demonstrates the importance of controlling for self-selection: Without such a control, one would conclude that openness is penalized when in fact it is not. A self-selection bias occurs when some factors (e.g., other bad news), unobservable to researchers, affect the choice decisions of one group (e.g., managers) and the subsequent decisions of another group (e.g., investors). This situation is fairly common in accounting and financial research, such as valuation of firms that choose to expense stock options, cut dividends, or issue new equity. Spurious relations may be drawn if self-selection is not controlled for.

APPENDIX A
STATEMENTS 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, $\phi(\cdot)$ is the p.d.f. and $\Phi(\cdot)$ 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:

$$\phi(x) + x\Phi(x) > 0. \tag{A1a}$$

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

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

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:

$$\phi(x) - x[1 - \Phi(x)] > 0. \tag{A2a}$$

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

$$\frac{d\left(\frac{\phi(x)}{1-\Phi(x)}\right)}{dx} = \frac{\phi'(x)[1-\Phi(x)] - \phi(x)[1-\Phi(x)]'}{[1-\Phi(x)]^2}$$
$$= \frac{-x\phi(x)[1-\Phi(x)] - \phi(x)[-\phi(x)]}{[1-\Phi(x)]^2}$$
$$= \frac{\phi(x)\{\phi(x) - x[1-\Phi(x)]\}}{[1-\Phi(x)]^2} > 0.$$

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

APPENDIX B
LITIGATION RISK ESTIMATION

LitigRisk in Equation (11) is the predicted probability of a firm being sued using a litigation model out-of-sample with the input variables measured in the one-year period before the event quarter. The litigation model that I use is similar to those used by Rogers and Stocken (2005) and Johnson et al. (2001). My model estimation uses class action filing 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 one-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 sum of 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.

I include three dummy variables for high-tech industries because prior research finds that high-tech firms are more likely to be sued. *BusinessService* is 1 if a firm is a member of Fama-French Industry Group 35, and 0 otherwise. *Computer* is 1 if a firm is a member of Fama-French Industry Group 36, and 0 otherwise. *Chip* is 1 if a firm is a member of Fama-French Industry Group 37, and 0 otherwise:

$$\begin{aligned} Pr(Lawsuit_i = 1) = F(&\mathrm{d}_0 + \mathrm{d}_1 Size_i + \mathrm{d}_2 Turnover_i + \mathrm{d}_3 Beta_i + \mathrm{d}_4 CumRet_i \\ &+ \mathrm{d}_5 StdRet_i + \mathrm{d}_6 MinRet_i + \mathrm{d}_7 BusinessService_i \\ &+ \mathrm{d}_8 Computer_i + \mathrm{d}_9 Chip_i + \varepsilon_i). \end{aligned}$$

Table B1 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 litigation risk.

TABLE B1
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***
Chip	0.041	0.22
McFadden Pseudo R^2		26.7%
426 litigated and 38,150 nonlitigated firm year observations		

*** Indicates statistical significance at 1 percent, in a two-tailed test.

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