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Within-industry timing of earnings warnings: do managers herd?

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Abstract An earnings surprise can be caused by a combination of firm-specific factors and market or industry factors. We hypothesize that managers have an incentive to time their warnings to occur soon after their industry peers' warnings to minimize their apparent responsibility for earnings shortfalls. Using duration analysis, we find that firms accelerate their warnings in response to peer firms' warnings. We conduct several tests to control for alternative explanations for warning clustering (for example, common shocks and information transfer) and conclude that the observed clustering is primarily due to herding. Our study is one of the first to empirically examine *managers*' herding behavior and the first to document clustering of bad news. Moreover, we provide a multi-firm perspective on managers' disclosure decisions that alerts researchers to consider or control for herding when they examine other determinants of managers' disclosure decisions.

Keywords Herding · Voluntary disclosure · Timing · Earnings warnings · Bad news

JEL Classification M41 · G14

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1 Introduction

> Two years ago, when the whole technology sector was booming, companies wanted to make sure their earnings reports kept up with the pack. This led to exaggeration in many cases and, apparently, to outright fraud in some instances.
> Last spring, when the economic downturn was taking its toll, the same herding instincts were at work, and many companies used this as an opportunity to take big write-downs on bad investments. The market grades on a curve, to some extent, and getting a C+ when everyone else was getting C's wasn't so bad. (Hal R. Varian. *The New York Times*. 3/14/2002. p. 2, C1.)

Most accounting studies examine managers' decisions without considering peer firms' actions. In reality, accounting reporting and disclosure decisions are often made in a sequential multi-firm setting. That is, managers observe and consider other firms' actions when making their own decisions. For example, managers' decisions about whether to expense stock options, hold conference calls, or provide pro forma earnings are likely affected by their peers' decisions. This phenomenon is called "herding," also referred to as "social learning" (Bikhchandani et al. 1998). Even though herding in the capital markets "is often presumed to be pervasive" and several theories have been developed to identify the mechanisms that could cause herding, empirical evidence of herding is "surprisingly sparse" (Welch 2000). So far, the finance and accounting literatures have documented herding behaviors of financial analysts, mutual fund managers, and other institutional investors. Our study provides new evidence of herding by corporate managers. Moreover, our study expands herding theories by suggesting a mechanism of bad-news herding related to managers' attempts to influence their evaluators.

We use earnings warning events to examine managerial herding for three reasons. First, earnings warnings are significant corporate events that investors and analysts closely monitor in the "warning season" preceding quarterly earnings announcements. For example, these events are closely watched by Earnings Whispers.com and MorningStar.com. The average market reaction to warnings is a decline of about 10% in stock price in the 3 days surrounding a warning (Tucker 2007). Second, managers exercise substantial discretion over both the incidence and timing of forward-looking earnings disclosures. Numerous studies examine the determinants of disclosure decisions, but with the exception of Baginski et al. (1995) study of intra-day timing, no study examines the *timing* of these disclosures. Finally, the data on earnings warnings are extensive and readily available, unlike other corporate events mentioned earlier that may also be subject to managerial herding. This makes earnings warnings a promising area for identifying managers' herding behavior.

We hypothesize that managers time their warnings to cluster with their peers to reduce their apparent responsibility for bad news. Earnings news typically has two components: (1) a market/industry component that is outside the manager's control and (2) a firm-specific component for which the manager is directly responsible. Managers arguably have an incentive to influence company directors' and other market participants' assessments of the extent to which bad news is due to external factors. The covariation theory in psychology predicts that evaluators attribute an agent's behavior to external factors to a larger extent when other agents exhibit similar behavior than when his action is unique (Koonce and Mercer 2005). As long as managers believe that their evaluators would be less likely to hold them responsible when other firms also issue bad news, they would be motivated to time earnings warnings to appear in an industry cluster, thereby minimizing their blame for the bad news.

To test this hypothesis empirically, we select earnings warnings issued in a window of about 60 days beginning in the third month of the fiscal quarter.[1] We require at least three warnings in an industry in each quarter for within-industry analysis and obtain a sample of 3,525 warning events from 509 industry quarters between 1996 and 2003 that meet this criterion. Within each industry-quarter group, we identify a lead warning firm (the first firm to issue a warning) and follower firms (the remaining firms). Our analysis focuses on the 3,016 warning followers. About 60% of the followers issue their warnings within 5 days of a peer's warning.[2] For 72.5% of the followers, we observe a peer firm's warning in the 5 days preceding or following the firm's own warning.

We obtain our main herding (or strategic clustering) result from duration analysis with time-varying covariates. We find that the probability that a follower warns on a specific day is *positively* associated with the number of peer firms' warnings in the preceding 5 days. This result suggests that warnings cluster and that the followers time their warnings to occur soon after their peers' warnings, consistent with our hypothesis of herding.

We conduct several tests to eliminate or control for the possibility that warnings cluster for reasons other than herding, specifically common shocks and information transfer. We conduct three tests to address the possibility that clustering is due to common shocks. First, we test clustering using only warnings issued *after* the fiscal quarter ends, a period in which common shocks should not affect quarterly earnings. We still find clustering. Second, we test clustering of good-news alerts because common shocks should be as likely to cause clustering of good news as they are of bad news. More important, our argument for herding applies only to bad news, not to good news, so a finding of good-news clustering would be inconsistent with our hypothesis but consistent with clustering caused by common shocks. We find no evidence of clustering of good news. Last, we control for earnings or return synchronicity, as measured by the tendency of firms' earnings or returns to move with the market or industry. We expect that clustering caused by common shocks is more likely to occur to firms with high synchronicity than for those with low synchronicity. We still find evidence of warning clustering after this control.

[1] Our sample selection is consistent with the warning studies by Kasznik and Lev (1995), Soffer et al. (2000), and Atiase et al. (2006). These studies identify warnings as unfavorable earnings disclosures issued in the 60-day window before earnings announcement. Because earnings are typically announced 25–30 days after the fiscal quarter end, the starting point from when these studies collect warnings is close to the beginning of the third fiscal month.

[2] On average, seven warnings are issued for an industry-quarter. If a uniform distribution is assumed, the average interval between successive warnings should be about 9 days.

As Acharya et al. (2008) demonstrate, information transfer may induce managers to cluster warnings. For example, an external news event such as peers' warnings may reduce investors' estimate of a firm's earnings. This lowers the equilibrium disclosure threshold, and some previously withheld bad news is thus disclosed, resulting in apparent clustering. We perform two tests to address this alternative explanation of clustering. In the first test, we estimate the effect of peers' warnings on a sample firm's stock price and find that the average magnitude of returns due to information transfer is only 10% of the market reaction to a firm's own warning. In the second test, we control for the cumulative stock returns in the 5 days before a firm's warning in the duration analysis. We still find a positive association between warning timeliness and the number of recent peers' warnings. Therefore, we conclude that the observed warning clustering is primarily due to herding and not to information transfer.

Our study makes several contributions to the herding and accounting literatures. First, our study is one of the first studies to examine herding by corporate managers. Prior studies document herding by securities traders or their advisers. The key question in this context is whether trading or advice on trading reveals valuable private information for *equity valuation*. In contrast, in our context, corporate managers are concerned about *performance evaluation* and have an incentive to influence the blame their evaluators assign for poor performance. We therefore focus on the timing of managers' bad news disclosures rather than on the information managers reveal to the market (that is, the content of warning). Indeed, we expect the valuation effect of a dollar of internally and externally caused shortfalls to be similar.[3]

Second, we are the first to document herding of bad news. It is well known that disclosing or reporting bad news is different from disclosing or reporting good news because of asymmetric litigation risks (Skinner 1994) and asymmetric value functions (Kahneman and Tversky 1979). The attribution process in which individuals explain an outcome using internal or external factors provides a mechanism for bad-news herding. The mechanism introduced in this paper could be applied to herding of other types of bad news, such as goodwill impairment and asset write-offs.

Third, our study provides a multi-firm perspective on managers' disclosure decisions, expanding on the pervasive single-firm view of these decisions in the literature.[4] Our findings suggest that researchers should consider or control for managers' herding in future work when examining determinants of managers' decisions. Finally, our finding may help investors better predict warnings. Ceteris

[3] The persistence of an earnings shortfall is likely to be unrelated to whether it is caused by internal or external factors. For example, an economic recession and a change in the management could both have long-lasting effects.

[4] The accounting literature has a stream of information transfer studies. A firm's earnings forecast conveys information that affects nonforecasting industry peers' returns (Baginski 1987; Han et al. 1989). Early earnings announcements affect the stock returns of late announcers in the industry (Foster 1981; Clinch and Sinclair 1987; Han and Wild 1990; Freeman and Tse 1992; Lang and Lundholm 1996b). The major difference between managerial herding and information transfer is that the former focuses on *managers*' decisions in a multi-firm setting, whereas the latter focuses on *investors'* inferences in a multi-firm setting.

paribus, investors who know that bad news clusters may better anticipate its arrival from observing the timing of other firms' bad news.

The rest of the paper is organized as follows. Section 2 reviews prior research. Section 3 develops our hypothesis, Section 4 introduces our empirical model, and Section 5 describes the data and provides descriptive statistics. We present the primary duration analysis as well as three robustness tests in Section 6, address alternative explanations in Section 7, and conclude in Section 8.

2 Prior research

Our research focuses on whether a firm's disclosure timing is affected by its peer firms' disclosures and is related to the broader herding literature examining decision-making in a multi-agent context. Our study is also related to the bad-news disclosure literature because it examines the timing of earnings warnings.

2.1 Herding theories

Herding is a rational "social learning" behavior—decision-making in a multi-agent setting in which an agent can observe others' actions and use these observations in his own decision making. As a result, agents' actions cluster. For this reason, we use "herding" and "strategic clustering" interchangeably in the paper.

Herding was originally identified by Keynes (1936, p. 158), who observed: "It is better for reputation to fail conventionally than to succeed unconventionally." So far three types of economic theories have been developed to explain herding: (i) information cascade, (ii) reputation herding, and (iii) other strategic clustering.

The information cascade theory was pioneered by Banerjee (1992) and Bikhchandani et al. (1992). In these models, each agent receives a private signal and makes a decision in sequence about whether to adopt a risky technology. An agent late in the sequence observes the decisions of those earlier in the sequence, infers the early decision makers' signals, and assigns equal weight to such information and his own private signal. After just a few rounds, an agent's private information could be so overwhelmed by the information inferred from others' actions that he takes the same actions as his predecessors *as if* he has discarded his own information. Because his action does not reflect his private signal, the agents after him learn nothing from his action and so make their decisions in the same way as he does. An information cascade is thus formed. Information cascades are fragile and can be easily shattered by the arrival of new information or a change in the preference or payoff of late agents. To our knowledge, only two theoretical studies have modeled information-cascade herding in the capital market setting (Avery and Zemsky 1998; Arya and Mittendorf 2005).

Reputation herding was first modeled by Scharfstein and Stein (1990) and extended by Trueman (1994) and Graham (1999). The key aspect of this theory is that an agent's payoff depends on his reputation—other people's assessment of his ability. Assuming "smart" agents receive positively correlated signals and thus act alike and "dumb" agents act independently, the theory shows that while the leader acts on his private information, others herd and largely ignore their privation information.

The information-cascade and reputation-herding theories take the sequence in which agents act as given and only examine the actions.[5] These scenarios differ from our setting because we consider the possibility that agents decide on the timing of their actions to occur at a specific point in sequence. Dye and Sridhar (1995) and Gul and Lundholm (1995) expand the herding literature by modeling the sequence of agents' actions and showing that strategic clustering can arise even in the absence of information cascades and reputation herding. Neither model, however, is directly applicable to our setting. Dye and Sridhar assume that firms receive *independent* signals but that their uncertain information endowments are correlated, whereas we assume a positive correlation of firms' private signals owing to common market/industry factors. Gul and Lundholm assume all firms forecast a common item, whereas in our setting firms warn about their own earnings, which have both common and idiosyncratic components.

2.2 Empirical tests of herding

Empirical tests of herding are challenging because private information is unobservable. As Graham (1999) points out, existing empirical herding studies typically investigate empirical clustering without directly testing the implications of herding models. For example, Welch (2000) examines herding among security analysts by testing whether the information transition matrix of analysts' recommendations moves to the consensus as the latter changes over time. Graham (1999) tests herding by examining the propensity of an investment newsletter's asset-allocation advice to change in the same direction as that of the market leader (Value Line) in the previous month. Grinblatt et al. (1995) and Wermers (1999) investigate herding of mutual fund managers by testing whether funds tend to buy or sell the same stocks at the same time. Nofsinger and Sias (1999) examine the correlation between the change in institutional ownership and contemporaneous returns to infer herding. Clement and Tse (2005) examine differences between analyst forecasts that converge to the consensus (herding forecasts) and those that diverge from it (bold forecasts). They find that bold forecasts are more accurate than herding forecasts, suggesting that herding may often reflect analysts' attempt to express agreement with their peers rather than their incorporating new information in their forecast.

None of the above studies examines firm *managers*' herding and none uses duration analysis—our research question and approach in this study. A concurrent study by Brown et al. (2006) is closely related to our study. They examine annual capital expenditure (CAPEX) forecasts using duration analysis and find that a firm's propensity to issue CAPEX forecasts is positively associated with the proportion of industry peers that have provided such forecasts. In addition, they find that this positive association is stronger for less reputable firms and firms in more competitive

[5] The exceptions are Chamley and Gale (1994) and Zhang (1997).

industries. They conclude that herding for capital expenditure disclosures is driven by both information and reputation factors.

Our study complements Brown et al. by examining another important corporate disclosure event—earnings warnings—that sheds further light on incentives for managers to herd. We argue that managers' herding for earnings warnings is likely to be driven by concerns about their evaluation in the managerial labor market, specifically by managers' incentive to reduce blame for poor performance. More important, we predict that this motivation for herding leads to clustering of bad news but not of good news. We find results consistent with this prediction.

2.3 Bad-news disclosure

Prior research suggests that a firm issues earnings warnings to (1) reduce litigation costs, (2) enhance managers' reputation for transparency, and (3) improve the company's competitive advantage in product markets.

Skinner (1994, 1997) argues that early disclosure of negative earnings news reduces expected litigation costs by shortening the class action lawsuit period and finds that timely disclosures are indeed associated with lower legal settlement costs. Kasznik and Lev (1995) find that large firms and firms with more severe earnings news are more likely to warn, consistent with the litigation-cost argument. Soffer et al. (2000), Baginski et al. (2002), and Field et al. (2005) also provide supportive evidence for the litigation argument, but Francis et al. (1994) and Johnson et al. (2001) do not.

The second motivation for earnings warnings is reputation. Skinner (1994, 1997) argues that firms may disclose bad news early to maintain their reputation for transparency with analysts and money managers. Tucker (2009) finds that analyst following indeed decreases for firms that withhold bad news, supporting this motivation.

The third motivation is related to competition in product markets. Dontoh (1989) demonstrates that dual oligopolists may disclose bad news to reduce competition by inducing their competitor to reduce production. Darrough and Stoughton (1990) propose a model in which a product-market leader promptly discloses negative earnings news to deter entry.

The above studies all examine factors that distinguish warning firms from nonwarning firms. Because of lack of empirical studies on disclosure timing, we resort to the existing warning literature to control for other determinants of warning timeliness than herding.

3 Hypothesis development

In our setting, each of N firms in an industry anticipates an earnings surprise of X_i, where $i \in [1, N]$. The earnings surprise has a component, $\alpha_i X_i$, that is affected by market or industry factors outside the manager's control and the other component, $(1 - \alpha_i) X_i$, that is due to internal factors under the manager's control. Although X_i is eventually announced by the firm, α_i is not fully observed.

We assume that the manager is concerned with others' perceptions of his ability and therefore has an incentive to minimize his apparent responsibility for an earnings disappointment by influencing his evaluators to overstate α_i. This assumption is consistent with managers' concerns about the labor market and with their egotism-driven bias elaborated by psychological studies. For example, Baginski et al. (2000) find that managers provide explanations for 65.4% of their earnings forecasts and that these explanations consist exclusively of *external* factors for 63.5% of the unambiguous bad-news forecasts but for only 40.2% of the good-news forecasts.

To confirm this insight, we collect press releases accompanying 100 warnings and 100 good-news alerts randomly selected from our samples (described in detail later). Out of 95 bad-news releases and 93 good-news releases that we can locate, managers use external factors for 49.5% of the bad news but only 15.1% of the good news. For example, in a June 15, 2001 press release, General Semiconductor Inc. attributes a second-quarter earnings disappointment to weak demand: "The semiconductor business is a cyclical business, and, like our peers, our revenues have been affected by the current market environment."

The above explanation is obviously self-serving because external factors should be as likely to cause favorable earnings news as to cause unfavorable earnings news. Alternatively, managers could time the warning to occur soon after peers' warnings (that is, to herd), hoping that company directors and other market participants would blame external factors for the earnings shortfall to a greater extent than is warranted. According to the covariation theory on individuals' attribution processes in psychology, evaluators are more likely to use external factors to explain an agent's behavior when other agents exhibit similar behavior than when the agent's action is unique.[6] The covariation theory was established by Kelley (1967, 1973) and has been tested empirically. It is one of the two main attribution theories discussed in contemporary psychology textbooks (Eysenck 2004). While the herding strategy we hypothesize could coexist with the explanation strategy, it exploits an implicit cognitive process of evaluators.[7]

Consistent with the covariation theory, we observe that the financial press often provides market or industry explanations when *several* firms in an industry issue earnings warnings. For example, an October 4, 2005 *Forbes* article reports that La-Z-Boy blamed its 2005 earnings warning on customers taking advantage of auto incentives instead of buying furniture and on weather-related raw material shortages. The article notes that another furniture retailer also blamed auto incentives in its earnings warning and lists two other furniture retailers that recently issued sales warnings. This example suggests that the press is more likely to attribute clustered warnings to external factors than it is with a standalone warning.

[6] Evaluators use the "covariation" of behaviors from several individuals and sources to identify possible causes.

[7] Our conjecture about managers' behavior does not necessarily imply that boards of directors are misled. Managers and their evaluators may be linked by interlocking directorships (Brick et al. 2006) and social ties (Hwang and Kim 2008). Issuing a warning in a cluster would enable the evaluators to justify attributing an unwarranted proportion of the shortfall to external factors even though the evaluators can see through the manager's timing strategy.

As long as managers believe that their evaluators are less likely to hold them responsible for bad news when other firms also issue bad news, they would be motivated to strategically time the warning to be in an industry cluster and thereby minimize their blame for the earnings shortfall. Therefore, we predict:

H: Managers time their warnings to occur soon after their industry peers' warnings.

4 Empirical model

4.1 Duration analysis

We use duration analysis to test whether managers time their warnings to occur soon after peer firms' warnings. We measure the actual warning duration as the number of days from the beginning of the third fiscal month of the quarter to a firm's warning date. In other words, we use the beginning of the third fiscal month as the reference point to measure warning timeliness (also see Sect. 5.1). The use of this reference point for a cross section of firms implicitly assumes that managers privately observe signals about forthcoming earnings at about the same point in their fiscal quarters and therefore relative to the beginning of the third fiscal month.

Duration analysis estimates the parameters of the distribution of duration if the distribution form is known, or estimates the effect of covariates on duration if the distribution form is unknown.[8] The former is parametric and the latter is nonparametric. Intuitively, one could directly estimate the determinants of duration by using the logarithm of duration as the dependent variable in an ordinary least squares (OLS) regression. This OLS model, however, requires the duration distribution to be log-normal, the covariates to be time-invariant, and the duration not to be right-censored.[9]

Duration analysis is commonly implemented by estimating a hazard model. Hazard models allow for time-varying covariates, right-censoring, other distribution assumptions, or no knowledge of the distribution. A hazard function is used to describe the distribution of duration, which can be characterized in various equivalent ways (for example, moment generating function, c.d.f, p.d.f, survival function, hazard function), In the case of warnings, warning hazard is the probability that a follower warns on a particular day, given that it has not previously warned in that quarter. Because warning duration is extended each day when warning does not occur, warning hazard is *inversely* related to warning duration.

[8] The term "duration model" is used in engineering for models that examine the timing of mechanical failures (Greene 2003, 790–791). Biomedical research has a longer history of using this method and refers to it as "survival analysis." Duration models have been applied in economics to examine the length of unemployment spells and in finance and accounting to study the staging of venture capital (Gompers 1995), earnings management (Beatty et al. 2002), and the influence of investment banking ties on the speed of changes in analysts' recommendations (O'Brien et al. 2005).

[9] Right-censoring means that the measurement for duration does not end naturally but ends early because of either researchers' constraints or an outside force. In our setting, warning duration is right-censored at the earnings announcement date (strictly speaking, 3 days before the earnings announcement date).

4.2 Cox proportional hazard model

We use the Cox proportional hazard model for the duration analysis. Proportional hazard models specify a common baseline hazard for all firms and allow individual firms' hazard functions to differ proportionally with observed covariates. The Cox model is semi-parametric: it is parametric to the extent that the effects of covariates are specified in a functional format, but it is nonparametric to the extent that the baseline hazard is unspecified. We use the Cox model because our interest is in investigating whether a firm's warning likelihood on each day is shifted upward by peer firms' recent warnings, even though the baseline hazard is unknown.

Our Cox model is specified in Eq. (1) and is estimated using warning followers.[10] Function $h_0(t)$ is the unspecified baseline hazard rate. Variable t is the number of days from the beginning of the third fiscal month of the quarter to the warning date. For example, t is 1 if a warning is issued on the 30th day before the quarter-end. The model uses daily observations for each firm from $t = 1$ to the warning date and is estimated using partial likelihood.[11]

$$\begin{aligned} h(t_i) = h_0(t_i)\exp\Big[& b_0 PeerWarn(t)_i + b_1 Size_i + b_2 BadNews_i + b_3 Analyst_i \\ & + b_4 MarketShare_i + b_5 Past_i + b_6 PastTime_i + b_7 LeaderTime_i \\ & + b_8 IndTime_i + \sum_{k=1}^{6} b_{k+8} DumSIC_k \Big] \end{aligned} \tag{1}$$

PeerWarn(t) is our primary variable of interest and is measured as the number of warnings issued by industry peers in the 5 days preceding day t (not including day t).[12,13] For each firm this variable may take a new value each day in the moving window (Fig. 1). This is the only time-varying covariate in the model. Here we do not count same-day peers' warning because of the concern that a sample firm may be unable to observe peers' warnings *and* publish its own warning on the same day. Our results are robust to the treatment of same-day peer warnings.

If, as we predict, managers time their warnings to occur soon after their peers' warnings, the coefficient on *PeerWarn*(t) should be positive. In other words, a positive coefficient would suggest that a follower's warning is more likely and thus more timely than it would be in the absence of peers' warnings. On the other hand, a

[10] We assume that at the beginning of the third fiscal month, a firm has decided whether to warn and the only remaining decision is when to warn.

[11] The model is $h(t_i) = h_0(t_i)\ \exp\,(X_i\beta)$. Suppose a set of sample firms in one industry issues warnings on K different dates $(T_1, T_2, T_3, \ldots, T_k)$ for a quarter and that there is at most one warning each day. Let R_i be the set of firms that have not yet issued a warning by day T_i, where $i = 1, 2, \ldots, k$. The probability that a firm warns on day T_i is $\exp(X_i\beta)\big/\sum_{j\in R_i}\exp(X_j\beta)$. Then, $LnL(\beta|data) = \sum_{i=1}^{k}\left[X_i\beta - \ln\left(\sum_{j\in R_i}\exp(X_j\beta)\right)\right]$ is the partial log-likelihood of observing the data, and the Cox estimator for β maximizes this function (Cox 1975).

[12] We obtain similar results when *PeerWarn* (t) is measured in a 3-, 7-, or 10-day interval.

[13] Brown et al. (2006) use the cumulative proportion of industry peers that have disclosed by day t as the primary variable of interest. We use the number of recent warnings by peer firms in the preceding 5 days as our primary explanatory variable because the measure best reflects our expectation that managers seek to reduce the blame for poor performance by issuing warnings at about the same time as other firms with earnings disappointments.

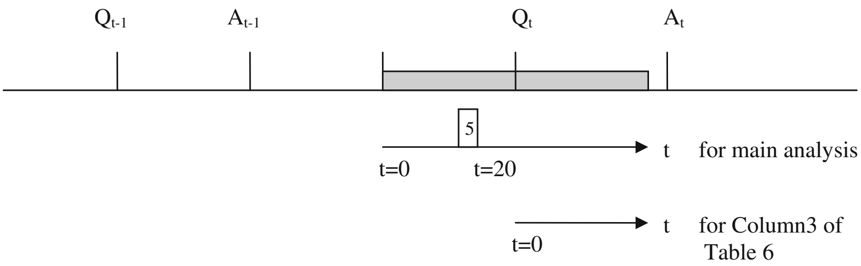

Fig. 1 Timeline of duration analysis. The timeline shows the fiscal quarter-end of the current quarter (Q_t) and the earnings announcement of the current quarter (A_t) as well as the fiscal quarter end and earnings announcement date for the previous quarter (Q_{t-1} and A_{t-1}). Warnings are collected in a period that begins on the third month of the fiscal quarter and ends 3 days prior to the earnings announcement. For each day in the warning period, duration, t, is the number of days from the beginning of the third month of the fiscal quarter to that date and takes the value $t = 1$ on the first day of this period. The duration analysis tests whether the likelihood of warning on date t is positively associated with the number of industry peers' warnings in the 5 days before t. The figure shows this moving 5-day window. In a robustness test in Sect. 6.2, where we examine warnings issued after the fiscal quarter ends, the duration analysis starts at the end of the fiscal quarter

negative coefficient is consistent with anti-herding: a follower delays its warning if other firms have warned recently. In the absence of any effect from peers' actions, a firm's warning duration (or, equivalently, the warning hazard) is determined by Eq. (1) when *PeerWarn*(t) is excluded.

We use two sets of control variables in the duration analysis. The first set is the factors that are identified in prior studies as being associated with firms' decisions to warn. We assume that the factors affecting a firm's decision to warn also affect the timeliness of the warning. In particular, we expect that firms are more likely to warn promptly to shorten the class action lawsuit period in the event of a lawsuit. We control for the litigation concern by including a firm's market capitalization (*Size*) and the magnitude of its bad news (*BadNews*). *Size* is a firm's average market value of equity in the four fiscal quarters before the event. *BadNews* is the magnitude of a firm's negative earnings surprise, measured as the difference between the forthcoming earnings and the most recent analyst consensus compiled before the warning, deflated by the split-adjusted beginning-of-event-quarter stock price.

Next, we control for analyst coverage (*Analyst*) because firms may warn promptly to maintain their reputation with analysts. *Analyst* is the average number of analyst forecasts included in the most recent consensus before a firm announces earnings for the four quarters before the event quarter. Furthermore, we use a firm's product market share (*MarketShare*) to investigate whether the product-market-related incentives explain warning timeliness. Firms whose products command a high market share are more likely than other firms to be a bellwether. Those firms could benefit from issuing an earnings warning that discourages new entrants to the industry or induces peer firms to cut production to ease product supply. *Marketshare* is the ratio of a firm's total sales in the most recent fiscal year before the event quarter over the industry's total sales in that year.

We convert *Size* and *Analyst* into within-industry-quarter rankings because we examine these factors relative to industry peers in the within-industry timing decision. To ensure fine relative measures, we rank a firm's *Size* and *Analyst* values among *all* firms in the industry quarter that are covered by Compustat, CRSP, and I/B/E/S rather than just among the warning sample firms.[14] We also rank *BadNews* because its distribution is skewed. Note that *BadNews* is the magnitude of negative earnings surprise, so a firm with a more negative earnings surprise receives a higher ranking.[15] For interpretation convenience, we normalize the rankings of *Size, BadNews,* and *Analyst* to be between 0 and 10 and *MarketShare* to be between 0 and 100.

The second set of control variables is related to a follower's warning history, the leader's timeliness, and the industry norm of warning timing. Firms that have warned before would be familiar with the procedures and consequences of issuing warnings, and thus they may be less hesitant to release the current warning. To capture this possibility, we include a dummy variable *Past*, which is 1 if the firm warns in any previous quarter in the sample period and 0 otherwise. We expect a positive coefficient for *Past*. Moreover, firms' disclosure practices tend to be sticky (Lang and Lundholm 1996a; Anilowski et al. 2007), so we control for the possibility that firms may invariably warn at about the same time relative to the fiscal quarter-end. We include *PastTime*—a firm's average actual warning duration in a maximum of four previous quarters in which it warned. The variable is set to 0 for the firms that did not warn in prior quarters. *PastTime* is equivalent to the interaction between *Past* and a variable measuring past warning duration. Thus inferences from the *PastTime* coefficient are applicable to firms with a warning history. We predict a negative coefficient on *PastTime*.

The timeliness of followers' warnings may increase when the leader warns early. We include *LeaderTime* to control for this possibility. The variable is measured as the difference between the leader's warning date and the beginning of a follower's third fiscal month (that is, the reference point at which $t = 1$). We predict a negative coefficient for *LeaderTime*.

We also control for warning timing due to industry practices. For example, firms in an industry may issue warnings 30 days before their earnings announcement because that is when firms in the industry typically have sufficiently reliable information about the upcoming earnings. To control for this possibility, we calculate the median number of days from warnings to earnings announcements for each industry during the sample period. The dummy variable *IndTime* is 1 if a follower's warning is within 2 days before or after the industry median point and 0 otherwise. We predict a positive coefficient for this variable.

Finally, we include dummies for the six industries with the most frequent warnings to control for other unknown industry factors that may affect the timing of warnings.

[14] Within-sample rankings would be coarse for industry quarters that have only a few warnings.

[15] We rank *BadNews* in the full warning sample, not among warning firms in the industry-quarter, because the rankings under the latter approach would be coarse for industry quarters that have only a few warnings.

5 Data and descriptive statistics

5.1 Data

We use the First Call CIG database to identify warnings about quarterly earnings. The database includes 56,674 guidelines with CUSIP identifiers from 1993 to late 2004, of which 30,554 are guidelines about quarterly earnings issued by U.S. firms (hereafter referred to as "guidance"). From these events, we obtain 10,866 events that are clearly coded as negative guidance by First Call.[16]

To measure the sequence of warnings in an industry, we label a firm's fiscal quarter by the calendar quarter with which it overlaps most and refer to this procedure as "calendarization." We do so because only about two-thirds of firms in an industry end their fiscal quarters in the same month. Calendarization allows us to better control for industry/market trends. After calendarization, fiscal quarters that end in the last month of the calendar quarter and in the two adjacent months are classified in that calendar quarter. For example, the second calendar quarter includes the fiscal quarters that end in May, June, or July of that year.

We choose 32 quarters between 1996 and 2003 as the sample period.[17] The number of negative earnings guidelines in the sample period is 9,223. Of these events we retain 5,605 negative guidelines that are issued after the beginning of the third fiscal month of the quarter. Figure 2 shows that negative guidance arrives in two waves. The first wave ("earnings forecasts") peaks around the end of the first fiscal month; the second wave ("earnings warnings") is centered on the fiscal quarter end. For the former, managers' forecasting ability is crucial; for the latter, it is reasonable to assume that managers know whether their quarter will be disappointing because most of the quarter has elapsed.[18] Because we focus on the disclosure decision once managers accumulate sufficient private information, we restrict the sample to earnings warnings—negative guidance issued between 31 days before the fiscal quarter end and 3 days before the earnings announcement date. We choose 31 days because it is the ebb of the two waves.

Next, we retain the first warning that a firm issues for a quarter and therefore delete 89 duplicate warnings. We delete another 813 events because of missing

[16] First Call uses analysts' earnings expectations as the benchmark to code managers' earnings guidance as positive, negative, or in-line guidance. Although the data manual does not specify how guidance with a range estimate is coded, we infer based on the comparison of the estimates and analysts' consensus that CIG codes guidance as "negative" if the upper estimate is lower than analyst consensus, "positive" if the lower estimate is higher than analyst consensus, and "in-line" if analyst consensus is between the lower and upper estimates.

[17] The period begins in 1996 because there are only a few observations (328 events) before 1996 and because the Private Securities Litigation Reform Act of 1995 substantially expanded the safe-harbor protection to firms for disclosing forward-looking information.

[18] To demonstrate managers' differential forecasting ability in the two windows (waves), we compare the incidence of point vs. range forecasts across the windows. While only 29.1% of the earnings forecasts are point estimates, the percentage is substantially larger at 39.3% for earnings warnings. If we treat a point estimate as having zero range, the median range of earnings warning estimates as a percentage of the absolute value of the mean forecast is 6.1%, whereas that of earnings forecasts is 9.3%. The difference is both economically and statistically significant (Wilcoxon $Z = 8.07$).

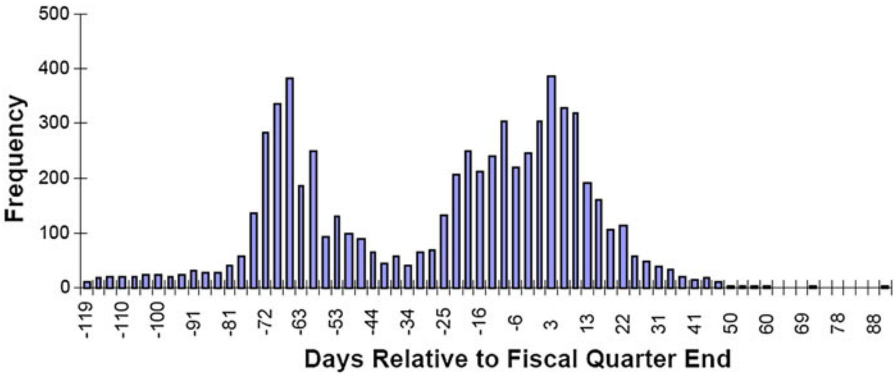

Fig. 2 Timing of negative quarterly earnings per share guidance. The data source is First Call's CIG database. First Call collects company guidelines from press releases and interviews and codes them as negative, positive, and in-line guidance by comparing the disclosure with existing market expectations. Positive and in-line guidance also have bimodal distributions, similar to negative guidance

identifiers in Compustat (Gvkey), CRSP (Permno), and I/B/E/S (Ticker). We then collect the most recent analyst consensus forecast for the event quarter compiled after the previous-quarter earnings announcement date, but before the warning date, and delete 86 events that do not have such data. We delete another 75 events because the recorded guidance date is after the earnings announcement date, suggesting possible data errors. To mitigate potential CIG classification errors for negative guidance, we delete 268 events where the realized earnings per share ("EPS") recorded by I/B/E/S is in fact higher than the most recent I/B/E/S consensus before the warning. After applying these criteria, we have 4,274 warning events. We use industry classifications defined by Fama and French (1997) to conduct industry-level analysis, requiring that each industry quarter have at least three warning events, and accordingly we delete 722 events that fail this requirement. Finally, we delete 27 events because of missing data for sales, market value, or earnings. We are then left with 3,525 warning events (see Table 1).[19]

The warning events span 509 industry quarters, with mean and median warnings per industry-quarter of 7 and 5, respectively. Table 2 lists the industries in the order of warning frequency. The first six industries appear in at least 24 of the 32 sample quarters. On average, business services, chips, retail, and computers each has at least 10 warnings in a typical quarter. Appendix A illustrates two such industry quarters. We identify one warning leader for each industry-quarter, and our final sample is therefore comprised of 509 warning leaders and 3,016 followers.

[19] The warnings are issued by 1,817 unique firms. Among these firms, 919 (50.6%), 465 (25.6%), 225 (12.4%), 107 (5.9%), and 101 (5.5%) issued warnings once, twice, three, four, and five or more times, respectively.

Table 1 Sample collection

Data cleaning procedure	Excluded	Remaining events
Guidance in the First Call CIG data file with “Cusip” available		56,674
Exclude non-EPS guidance	1,832	54,842
Exclude guidance issued by non-U.S. firms	2,853	51,989
Exclude guidance initiated by M&A, accounting change, etc.	238	51,751
Exclude guidance about annual earnings	21,197	30,554
Retain negative guidance	19,688	10,866
Retain negative guidance for 1996Q1-2003Q4 (calendarized)	1,643	9,223
Retain negative guidance issued between 31 days before the fiscal quarter end and 3 days before earnings announcement	3,618	5,605
Exclude duplicate warnings for the same fiscal quarter	89	5,516
Retain events with available identifiers in Compustat, CRSP, and I/B/E/S	813	4,703
Retain events with available I/B/E/S analyst consensus compiled after prior-quarter earnings announcement and before the warning	86	4,617
Exclude guidance whose issuance date is after the earnings announcement date (data error)	75	4,542
Exclude events for which the actual EPS is higher than the recent analyst consensus compiled before the warning	268	4,274
Exclude industry-year-quarters that have fewer than three warnings	722	3,552
Exclude observations that have missing data for firm size, analysts, bad news, and market share	27	3,525
Warnings about quarterly earnings issued by U.S. firms		3,525

5.2 Descriptive statistics

We summarize the characteristics of both warning leaders and followers in Table 3, though we are primarily interested in the 3,016 warning followers. In Appendix B we identify the firm characteristics that distinguish a warning leader from its followers. In Panel B of Table 1, *PeerWarn*(*Actual*) is the number of peer warnings in the 5 days before the follower’s own warning. Its average is above 1, meaning that, on average, followers’ warnings *immediately* follow peer firms’ warnings (that is, within 5 days).

Table 4 presents descriptive statistics to provide an intuitive picture of warning clustering. Panel A groups warning followers by the interval in days between the sample firm’s warning and the most recent peer’s warning. In the first row, 519 firms (17.2%) warn on the same day as a peer’s warning. The number of firms warning on the first day after the most recent peer’s warning is 491 (16.3%). Within 5 days after the most recent peer’s warning, 1,737 firms (57.6%) have warned. The number of warnings issued in the five subsequent days (from days 6 to 10) is only

Table 2 Industry distribution of warning cross sections

Industry name	# of cross sections	# of warning firms in a cross-section					
		Mean	Min	Q1	Median	Q3	Max
Business services	31	19	4	15	19	22	33
Microchips	30	12	3	8	11	15	38
Retail	30	13	3	8	10	16	30
Machinery	27	8	3	4	6	11	18
Computers	26	11	3	6	10	15	23
Wholesale	24	6	3	5	6	8	12
Construction materials	21	4	3	3	4	5	8
Consumer goods	20	5	3	4	5	6	9
Chemicals	20	6	3	4	6	7	11
Medical equipment	19	4	3	3	4	5	9
Measuring & control equip.	19	5	3	3	4	6	12
Transportation	18	6	3	4	6	8	10
Insurance	18	5	3	3	4	5	16
Banking	17	5	3	3	6	6	11
Steel works	16	7	3	5	7	9	10
Restaurants & hotels	16	4	3	3	4	5	8
Pharmaceutical products	15	3	3	3	3	4	4
Apparel	14	4	3	3	3	4	5
Petroleum and natural gas	14	4	3	3	4	5	7
Automobiles	13	6	3	4	5	6	11
Utilities	12	4	3	3	3	6	6
Food products	11	4	3	3	4	4	5
Printing & publishing	10	5	3	3	4	6	9
Electrical equipment	10	4	3	3	4	4	6
Textiles	9	4	3	3	3	5	7
Recreational products	8	3	3	3	3	3	6
Healthcare	8	3	3	3	3	4	4
Business supplies	7	5	3	3	5	6	9
Entertainment	6	4	3	3	3	4	6
Rubber and plastic products	4	4	3	3	4	4	4
Miscellaneous	3	3	3	3	3	3	3
Telecommunications	3	4	3	3	4	4	4
Personal services	3	4	3	3	3	7	7
Shipping containers	3	3	3	3	3	3	3
Trading	2	4	3	3	4	4	4
Agriculture	1	3	3	3	3	3	3
Construction	1	3	3	3	3	3	3
Total	509						

The industries are classified according to the 48-groupings of Fama and French (1997). The second column lists the number of year-quarter cross sections in each industry. The last few columns provide summary statistics for the number of warnings in a cross section

Table 3 Firm characteristics of leaders and followers

Variable	Obs.	Mean	Q1	Median	Q3	P95
Panel A: Warning leaders						
Warning duration	509	16	7	14	23	38
Size	509	4,813	256	699	2,013	15,942
Surprise	509	−0.008	−0.011	−0.005	−0.002	−0.001
Analyst	509	6.95	3.00	5.33	8.75	18.00
MarketShare	509	0.022	0.001	0.005	0.018	0.089
Price	509	25.24	13.63	21.01	32.06	59.06
Panel B: Warning followers						
Warning duration	3,016	30	19	31	39	57
PeerWarn(Actual)	3,016	1.1	0	1	1	4
Size	3,016	3,112	167	480	1,450	11,284
Surprise	3,016	−0.012	−0.014	−0.007	−0.003	−0.001
Analyst	3,016	6.52	2.75	5.00	8.50	18.25
MarketShare	3,016	0.011	0.001	0.002	0.009	0.049
Price	3,016	21.45	10.50	17.44	28.19	52.38

Variable definitions: *Warning duration* is the number of days after the beginning of the third fiscal month until the warning date. *PeerWarn*(*Actual*) is the number of warnings issued by industry peer firms in the 5 days (excluding the sample firm's warning date) before a sample firm's own warning. *Size* is the average market capitalization (in millions) in the four fiscal quarters before the warning. *Surprise* is the difference between the forthcoming earnings per share and the most recent analyst consensus compiled before the warning, deflated by the split-adjusted beginning-of-event-quarter stock price, where "event quarter" is the quarter that the warning addresses. *Analyst* is the average number of analyst forecasts included in the most recent consensus before earnings announcements for the four quarters before the event quarter. *Marketshare* is the ratio of a firm's total sales for the most recent fiscal year before the event quarter divided by the industry's total sales for that year. *Price* is the stock price at the beginning of the event quarter

576 (19.1%). The percentage of firms decreases substantially as the interval exceeds 10 days. These patterns are consistent with herding.

In addition to the interval from a peer's warning, warning clustering can be described by the location of a warning in a cluster. We use two different counts to identify location. First, we count the number of peers' warnings in the *preceding* 5 days, including peers' warnings issued earlier on the same day as the sample firm's. Then, we count the number of peers' warnings in the *subsequent* 5 days, including peers' warnings issued later on the same day as the sample firm's. A warning leads a cluster if there is at least one peer's warning in the subsequent 5 days but not in the preceding 5 days. A warning is in the middle of a cluster if there is at least one peer's warning in both the preceding and subsequent 5 days. A warning is at the tail of a cluster if there is at least one peer's warning in the preceding 5 days but not in the subsequent 5 days.

Panel B shows warning leaders and followers separately. Among the leaders, 345 (67.8%) are standalone and 164 (32.2%) lead a cluster. The percentage of standalone leaders is high perhaps because some leaders' fiscal quarters end a month earlier than the other firms in the industry, as confirmed in Appendix B. Among the

Table 4 Descriptive statistics for warning clustering

Days	Incremental number of firms	Percentage (%)	Cumulative number of firms	Cumulative percentage (%)
Panel A: How soon do firms issue warnings after the most recent peer's warning?				
Same-day[a]	519	17.2	519	17.2
1	491	16.3	1,010	33.5
2	202	6.7	1,212	40.2
3	194	6.4	1,406	46.6
4	162	5.4	1,568	52.0
5	169	5.6	1,737	57.6
[6, 10]	576	19.1	2,313	76.7
[11, 15]	302	10.0	2,615	86.7
[16, 20]	130	4.3	2,745	91.0
[21, 25]	89	3.0	2,834	94.0
[26, 30]	87	2.9	2,921	96.9
[31, 94]	95	3.1	3,016	100.0
Total	3,016	100		

	Leader		Follower			
	Alone	Lead a cluster	Head of a cluster	Middle of a cluster	Tail of a cluster	Alone
Panel B: Location in warning clusters						
Number of warnings in the past 5 days[a]	N/A	N/A	0	≥1	≥1	0
Number of warnings in the subsequent 5 days[b]	0	≥1	≥1	≥1	0	0
Sample Observations (%)	345 (67.8%)	164 (32.2%)	466 (15.5%)	1,091 (36.2%)	630 (20.9%)	829 (27.5%)
Subtotal	509 (100%)		3,016 (100%)			

[a] Same-day peers' warnings are included

[b] Among these 519 observations, 355 also have the second most recent peer warning occurring in the 5 days before the firm's warning

followers, our primary focus, 466 (15.5%) lead a cluster; 1,091 (36.2%) are in the middle of a cluster; and 630 (20.9%) end a cluster. The remaining 829 (27.5%) stand alone. These statistics indicate that 72.5% of the followers are in a warning cluster and that our sample of 3,025 warnings has 630 clusters, 26.0% of which are led by a leader and 74.0% of which are led by a follower, suggesting that warning clustering is more around followers themselves than around the leader.

6 Test results of duration analysis

6.1 Primary test

The last two columns of Table 5 report coefficient estimates and the hazard ratios of the full model. The estimation uses 95,146 daily observations from 3,016 followers and is robust to model misspecification in the linear form of covariates (Lin and Wei 1989) and to the inclusion of multiple events from the same firm in different quarters. The coefficient on *PeerWarn*(t) is significantly positive ($b_0 = 0.071$, z-statistic $= 5.98$), indicating that the probability that a follower that has not previously warned will issue a warning on a specific day is significantly positively associated with the number of peer firms' warnings in the preceding 5 days. The hazard ratio is the proportional change in hazard when the covariate increases by 1, that is, $\exp(\beta_i)$. The hazard ratio of *PeerWarn*(t) is 1.074, meaning that the probability that a follower will warn is 7.4% higher if the number of peers' recent warnings increases by 1. This result suggests that a follower accelerates the release of warning after observing peer firms' recent warnings, supporting our hypothesis.[20]

The estimation results for the first set of control factors are as follows. Warning timeliness increases with firm size ($b_1 = 0.043$, z-statistic $= 3.95$). The coefficient on *BadNews* is insignificant ($b_2 = -0.010$, z-statistic $= -1.39$).[21] *Analyst* and *MarketShare* are not significantly associated with warning timeliness perhaps because of their high correlations with *Size* (see Columns 1 and 2).

The estimation results for the second set of control variables are as follows. The coefficient on *Past* is significantly positive ($b_5 = 0.585$, z-statistic $= 8.11$), indicating that the current warning is more timely for firms that issued warnings in prior periods. The coefficient on *PastTime* is significantly negative ($b_6 = -0.017$, z-statistic $= -8.56$), indicating that the more timely the warnings in prior periods, the more timely the current warning. The coefficient on *LeaderTime* is significantly negative ($b_7 = -0.011$, z-statistic $= -9.25$), indicating that the

[20] We also estimate an OLS model with the logarithm of the actual duration as the dependent variable and *PeerWarn*(*Actual*) instead of *PeerWarn*(t) as the explanatory variable. The coefficient on *PeerWarn*(*Actual*) is -0.039 ($t = -6.22$), consistent with the Cox model results, and the model R^2 is 21.2 % (recall that duration is inversely related to hazard rate).

[21] We also measure bad news as the absolute price-deflated difference between the firm's EPS estimate (or the midpoint for a range estimate) and the most recent consensus before the warning. We substitute the realized earnings for company estimate when the latter is not in point or range form. The coefficient on *BadNews* is significantly negative (coefficient $= -0.016$, z-statistic $= -2.18$). Of our sample firms, 28.6, 64.6, and 6.8% provide point, range, and other-form warnings, respectively.

Table 5 Cox duration analysis of warning followers

$$h(t_i) = h_0(t_i)\ \exp\left[b_0 PeerWarn(t)_i + b_1 Size_i + b_2 BadNews_i + b_3 Analyst_i + b_4 MarketShare_i + b_5 Past_i + b_6 PastTime_i + b_7 LeaderTime_i + b_8 IndTime_i + \sum_{k=1}^{6} b_{k+8} DumSIC_k\right]$$

	Coefficient (z)	Coefficient (z)	Coefficient (z)	Hazard ratio
PeerWarn(t)	0.072*** (6.08)	0.071*** (5.99)	0.071*** (5.98)	1.074
Size			0.043*** (3.95)	1.044
BadNews	−0.030*** (−4.44)	−0.021*** (−2.98)	−0.010 (−1.39)	0.990
Analyst		0.038*** (4.86)	0.010 (0.98)	1.010
MarketShare	0.021** (2.56)	0.013 (1.61)	0.007 (0.86)	1.007
Past	0.606*** (8.33)	0.582*** (8.07)	0.585*** (8.11)	1.795
PastTime	−0.018*** (−8.68)	−0.017*** (−8.49)	−0.017*** (−8.56)	0.983
LeaderTime	−0.011*** (−9.37)	−0.010*** (−9.37)	−0.011*** (−9.25)	0.990
IndTime	0.346*** (6.80)	0.343*** (6.73)	0.348*** (6.87)	1.417
Business services	−0.259*** (−4.68)	−0.271*** (−4.84)	−0.280*** (−4.94)	0.756
Microchips	0.183** (2.52)	0.190*** (2.66)	0.178** (2.47)	1.195
Retail	0.034 (0.43)	0.043 (0.53)	0.040 (0.48)	1.041
Machinery	0.033 (0.39)	0.027 (0.31)	0.023 (0.26)	1.023
Computers	0.160** (2.32)	0.141** (2.03)	0.125* (1.78)	1.133
Wholesale	−0.204** (−2.48)	−0.213*** (−2.61)	−0.236*** (−2.90)	0.790
Wald-test (χ^2)	436.14	463.56	465.39	

The tests use 95,146 daily observations from 3,016 warning followers. There is one observation for each firm on each day from the beginning of the third fiscal month ($t = 1$) to the actual warning date. t is the number of days since the beginning of the third fiscal month ($t = 1$). The hazard rate ($h(t)$) is the probability density of a follower's issuing the warning on day t, given that it has not issued the warning in the preceding $t - 1$ days. $h_0(t)$ is the unspecified baseline hazard rate, being the same for all firms. Hazard ratio is the proportional change in hazard when the covariate increases by 1, and mathematically it is exp (β_i). Industry classifications are based on the 48 Fama and French (1997) groupings. ***, **, and * indicate statistical significance at the 1, 5, and 10% levels, respectively, in a two-tailed test. The estimation is robust to heteroskedasticity, misspecification in the linear form of covariates, and the inclusion of multiple events from a firm in different quarters

Variable definitions: *PeerWarn*(t) is the number of warnings issued by peer firms in the 5 days before day t (not including day t). *Size* is the average market capitalization (in millions) in the four fiscal quarters before the warning. *BadNews* is the magnitude of negative earnings surprise, where *Surprise* is the difference between the forthcoming earnings per share and the most recent analyst consensus compiled before the warning, deflated by the split-adjusted beginning-of-event-quarter price. *Analyst* is the average number of analyst forecasts included in the most recent consensus before earnings announcements for the four quarters before the event quarter. *Marketshare* is the ratio of a firm's total sales for the most recent fiscal year before the warning divided by the industry's total sales for that year. *Past* is 1 if a firm issued at least one warning in the previous sample quarters and 0 otherwise. *PastTime* is the average days it took a firm to issue warnings in the up-to-four prior quarters in which the firm warned and is zero for those with no prior warnings (that is, *Past* = 0). *LeaderTime* is the difference between the leader's warning date and the beginning of the follower's third fiscal month. *IndTime* is 1 if a follower's warning is within 2 days of the median time point, with respect to the earnings announcement date, at which industry firms give warnings and 0 otherwise. For the model estimation, we rank *Size* and *Analyst* among all firms in the industry quarter covered by Compustat, CRSP, and I/B/E/S and rank *BadNews* in the full sample. For convenient interpretation, we normalize *BadNews, Size*, and *Analyst* to be between 0 and 10 and *MarketShare* to be between 0 and 100 timeliness of a follower's warning increases with the timeliness of the leader's warning. The coefficient on *IndTime* is significantly positive ($b_8 = 0.348$, z-statistic $= 6.87$), suggesting that warning timeliness is partly driven by industry norms.

6.2 Robustness tests

We provide three robustness checks for the primary test: (1) alternative industry classifications, (2) estimation on a "calendar quarter" subsample, and (3) alternative reference point for warning duration.[22,23]

6.2.1 Alternative industry classifications

Our industry classifications are based on the Fama and French (1997) industry groups. Bhojraj et al. (2003) compare three production-technology-based industry classifications (two-digit SIC codes, the North American Industry Classification System "NAICS," and Fama–French) and a valuation-based classification (the Global Industry Classification Standard "GICS," developed by Morgan Stanley). They find a high level of correspondence among the Fama-French, SIC, and NAICS classifications and also find that the GICS classification is least correlated with the other three. Because of the high correspondence between the SIC, NAICS, and Fama-French classifications, we expect similar test results under these industry classifications. For robustness, we re-estimate the duration model using the GICS classification.

Column 1 of Table 6 reports the results with the industry dummies suppressed for conciseness.[24] The coefficient on *PeerWarn*(t) is 0.115 with a z-statistic of 7.10, consistent with our finding in the primary test. The results for firm size, the incidence and timeliness of past warnings, the influence of the warning leader's timeliness, and the industry norm of warning timing are all similar to those in the primary test. Under GICS, market share is positively associated with warning timeliness (coefficient $= 0.012$, z-statistic $= 2.30$). Overall, our primary findings are robust to this alternative industry classification system.

6.2.2 "Calendar-quarter" subsample

In our primary analysis, we classify a firm's fiscal quarter by the calendar quarter with which it overlaps most. This design has the advantage of preserving sample

[22] In an unreported robustness test, we separate the sample into pre- and post-FD subsamples and find no difference in warning clustering, though warnings are weakly more timely in the post-FD in a one-tailed test ($z = 1.61$).

[23] We construct a variable that counts the number of warnings in the 5-day window before the sample's warning issued by firms in industries *other than* the sample firm's. We find no association between this variable and warning timeliness, whereas *PeerWarn*(t) remains significantly positively associated with warning timeliness. This result is consistent with our message of *within-industry* herding.

[24] Some industry-dummy coefficients have different signs than those in Table 5. As is typical for panel data, our interest is in controlling for the fixed industry effects, not in making inferences from their coefficients.

Table 6 Robustness tests of the duration analysis

	GICS industry	Calendar quarter	Warnings after FQE
PeerWarn(t)	0.115*** (7.10)	0.089*** (5.61)	0.073*** (2.73)
Size	0.047*** (3.72)	0.047*** (3.64)	0.018 (0.86)
BadNews	−0.012 (−1.52)	−0.012 (−1.44)	−0.021 (−1.36)
Analyst	0.003 (0.27)	0.003 (0.21)	0.022 (1.16)
MarketShare	0.012** (2.30)	0.005 (0.67)	−0.006 (−0.62)
Past	0.542*** (6.68)	0.548*** (6.06)	0.568*** (3.72)
PastTime	−0.015*** (−7.01)	−0.017*** (−6.63)	−0.014*** (−3.54)
LeaderTime	−0.011*** (−9.92)	−0.023*** (−11.01)	−0.081*** (−9.44)
IndTime	0.301*** (5.16)	0.373*** (6.07)	0.466*** (3.54)
Six major industry dummies are included but not reported.			
Wald-test (χ^2)	444.64	391.85	193.83

Column 1 uses the first six digits of the Global Industry Classification Standard (GICS) code for industry. For example, business services is (202010, 253020, 451010-30), microchips (453010, 452050), retail (255020-40), machinery (201060), computers (452020), and wholesale (255010). The estimation uses 83,956 daily observations of 2,622 warning followers from 558 industry-quarters. Column 2 uses the 48 groupings in Fama and French (1997) as in the primary analysis, but excludes the firms whose fiscal quarters do *not* end in March, June, September, or December. We refer this sample as the "calendar quarter" sample. The test uses 76,067 daily observations of 2,320 warning followers. Column 3 uses a subset of the "calendar quarter" sample and includes only the warnings issued *after* the fiscal quarter-end (FQE). We use a 3-day window for *PeerWarn*(t) instead of the original 5-day window because the duration measurement window here is 30 days shorter than that in the primary test. The test uses 11,683 daily observations of 884 warning followers. The table reports the coefficients and z statistics (in parentheses) of the Cox model. See Table 5 for variable definitions and other notes

and information (especially on whether there is a recent peer's warning). The disadvantage is that we may classify early quarter-end firms as warning leaders. Although the problem does not affect our definition of timeliness (which is measured relative to a firm's own fiscal quarter-end or equivalently the third fiscal month) or clustering (which is estimated by the association of timeliness with recent peers' warnings in real time), we use a calendar-quarter subsample to provide a cleaner test.

We delete warnings issued by firms whose fiscal quarters do not end in March, June, September, or December and then retain industry-quarters that have at least three warnings. Column 2 of Table 6 reports the Cox model results, which are similar to our finding in the primary test (coefficient on *PeerWarn*(t) = 0.089, z statistic = 5.61).

6.2.3 Alternative reference point to measure warning duration

In the primary test, the beginning of the third fiscal month is the reference point and is the starting point for the "horse race" to determine warning timeliness. We now move this reference point forward to the fiscal quarter-end for two reasons. First, at the end of the fiscal period, managers should know how the firm has performed and whether its earnings will meet the market expectations. The new test assesses the robustness of our findings to the assumption in the primary test that managers have private information at the beginning of the third fiscal month. Second, after the fiscal period ends, a market or industry shock should not change the earnings number and therefore should not contribute to warning clustering. The new test helps address the concern that the clustering we observe is merely due to common shocks.

Column 3 of Table 6 presents this robustness test. We use only warnings issued after the fiscal quarter ends and recalculate warning duration, *t*, with respect to the quarter-end. To ensure a clean test (possibly at the expense of test power), we require the firms to be from the "calendar-quarter" subsample. We drop industry quarters with fewer than three warnings. We use a 3-day window for *PeerWarn*(*t*) in this test, in contrast to the 5-day window in our primary test, because the new duration measurement window is 30 days shorter than that in the primary test. The Cox model estimation uses 11,683 daily observations of 884 warning followers, and our primary result holds in this subsample.

7 Alternative explanations for warning clustering

7.1 Clustering caused by a common shock

Firms may be affected by an unfavorable market- or industry-wide shock so that they decide to warn at about the same time, leading to clustering of warnings. Although our third robustness test in Sect. 6.2 addresses this concern, we provide two new tests below.

7.1.1 Test clustering of good-news alerts

Common shocks should be as likely to induce good-news clustering as they are to induce bad-news clustering. Moreover, our herding hypothesis hinges on the blame game for bad news and does not predict herding of good news. Therefore, by testing whether good-news alerts also cluster, we can simultaneously mitigate the common-shock explanation for warning clustering and strengthen our herding explanation for warning clustering.

We collect a good-news alert sample following the same procedures that we use for the warning sample. In comparison to the 4,274 available warning events, the number of available good-news alerts is 1,161. This number is less than one-third of the number of warnings, indicating that good-news alerts are less common than warnings. Among these good-news alerts, 562 are from 115 industry quarters that have at least three alerts. From this set we use 447 good-news *followers* for the Cox model estimation, where *PeerAlert*(*t*) is the counterpart of *PeerWarnt*(*t*) and other variables are measured as in our primary test except for replacing bad news with good news.

The first column of Table 7 reports the Cox model estimation results. The coefficient on *PeerAlert*(*t*) is 0.055, insignificantly different from zero with a z statistic of 1.26, suggesting that no good-news clustering occurs. To check whether this insignificance is due to using a good-news sample that is smaller than the bad-news sample in our primary test, we collect a comparable-size bad-news sample and

Table 7 Duration analysis for good-news alerts

	Good-news alerts	Bad-news warnings, with sample size restricted to be similar to the good-news sample
PeerAlert(t)	0.055 (1.26)	
PeerWarn(t)		0.114** (2.23)
Size	0.017 (0.57)	−0.012 (−0.45)
BadNews	−0.031 (−1.63)	−0.031* (−1.88)
Analyst	0.054* (1.93)	0.029 (1.16)
MarketShare	0.022 (1.10)	0.024* (1.68)
Past	1.147*** (5.84)	0.414** (2.56)
PastTime	−0.024*** (−5.05)	−0.013*** (−2.90)
LeaderTime	−0.005** (−2.20)	−0.015*** (−5.43)
IndTime	0.106 (0.92)	0.409*** (3.18)
Six major industry dummies are included but not reported.		
Wald-test (χ^2)	111.16	74.64

We follow the same procedures we use for the warning sample in the primary analysis and collect a sample of good-news alerts. The Cox model estimation uses 14,697 daily observations of 447 good-news-alert followers from 115 industry-quarters. Column 2 uses a bad-news sample of similar sample size as the good-news sample. The procedures are as follows. First, from our primary bad-news sample, we exclude the industry quarters with 12 or more warnings so that the distribution of the number of warnings in an industry-quarter is similar to that of good-news alerts. This procedure leaves us with 449 industry-quarters. Then we randomly select 115 industry-quarters from these 449 cross-sections. This procedure results in 500 warning followers. Finally, we estimate the Cox model using 16,265 daily observations of the 500 warning followers. The table reports the coefficients and z statistics (in parentheses) of the Cox model. *PeerAlert*(t) is defined similarly as *PeerWarn*(t) except for using good news. See Table 5 for variable definitions and other notes

re-estimate the Cox model.[25] Using 500 warning followers of this downsized warning sample, we find the coefficient on *PeerWarn*(t) to be 0.114, significantly positive with a z statistic of 2.23.[26] Therefore, we conclude that managers cluster warnings but not good-news alerts.[27]

7.1.2 Control for synchronicity

Firms with earnings that are highly synchronous with their industry peers are more likely to be affected by common shocks and thus have a higher propensity to cluster

[25] The 25th, 50th, 75th, and 95th percentiles of the distribution of events per industry quarter for the bad-news sample are 3, 5, 8, and 20, whereas those for the good-news sample are 3, 4, 5, and 12. To match with the good-news sample distribution, we exclude from the bad-news sample the industry quarters with 12 or more warnings. The reduced bad-news sample contains 1,881 events from 449 industry quarters and the 25th, 50th, and 75th percentiles of the distribution of the number of events per industry quarter are comparable with those of the good-news sample. We then randomly choose 115 industry quarters to mimic the sample size of good news, leading to 500 warning followers.

[26] We speculate that the reduced z statistic is due to less variation in *PeerWarn*(t) after the industry quarters with more than 12 events are excluded.

[27] We find few good-news alerts in warning clusters or within 5 days after the last warning in a cluster (unreported), consistent with bad-news herding.

warnings. On the other hand, a necessary condition for managers to credibly assign blame for their firm's earnings shortfall to market or industry factors is the existence of some earnings synchronicity. Otherwise, investors would have no reason to believe that any of the earnings shortfall is outside managers' control. Despite a possibility of removing a precondition for strategic clustering, we control for synchronicity and re-examine the association of warning timeliness with the number of recent peers' earnings.

For each firm, we measure earnings synchronicity (*SynEarnFirm*) by the R^2 of the regression of the firm's return on assets (ROA) on the industry ROA (calculated as the total industry earnings divided by the total industry assets) in the 20 quarters before the event quarter. This measurement follows Eq. (12) of Morck et al. (2000) except that they deal with countries, not industries, and they value-weight firms' ROAs to calculate the country-level ROA.[28] We also calculate stock synchronicity (*SynRetFirm*) as the R^2 of the regression of the firm's weekly stock returns on the value-weighted market returns and industry returns in the calendar year before the event quarter. This measurement follows Eq. (1) of Piotroski and Roulstone (2004) except that we do not include lagged market and industry returns. Moreover, we follow Morck et al. to calculate industry-level co-movement indexes, *SynEarnInd* and *SynRetInd*, as the weighted average of firm-specific R^2, where the weight is the regression sum of squared total variations (SST) as a percentage of total SST for firms in the industry. Among these measures, our primary interest is in firm-level earnings synchronicity because what is at issue is the timing of firm-specific earnings warnings.

We present descriptive statistics for the synchronicity measures in Fig. 3. Warning followers are stratified into seven groups by the actual number of peers' warnings in the preceding 5 days (that is, *PeerWarn*(*Actual*), or loosely speaking, "the degree of clustering"). Using group means, the chart shows that the *industry-level* co-movement indexes are flat across these groups, indicating that the degree of clustering is unrelated to the level of industry synchronicity. The *firm-level* synchronicity measures appear to be higher for the groups with at least one peer's warning than for the group with no peers' warnings; however, synchronicity does not increase monotonically with the degree of clustering.

Column 1 of Table 8 reports the Cox model estimations with the synchronicity level and its interaction with recent peers' warnings included as explanatory variables. Here the synchronicity measure is *SynEarnFirm*, which is ranked for this test between 0 and 10 among all firms in the same industry quarter covered by Compustat, CRSP, and I/B/E/S. *SynEarnFirm* is positively associated with warning timeliness (coefficient = 0.017, $z = 2.15$). The interaction of synchronicity with *PeerWarn*(*t*), however, is insignificant. More important, with these controls the coefficient on *PeerWarn*(*t*) remains both economically and statistically significant (coefficient = 0.063, $z = 2.80$). The results are similar when *SynRetFirm* is used instead, whereas industry-level synchronicity has no effect in the Cox model (unreported). These results alleviate the concern that responses to a common shock, rather than herding, explain the observed warning clustering.

[28] Our approach treats each industry as one entity and prevents firm-level ROA outliers from driving the industry-level ROA.

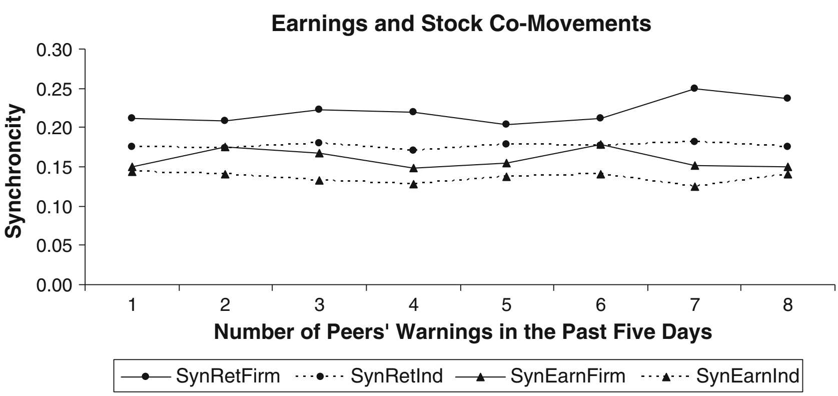

Fig. 3 Earnings and stock synchronicity. The figure plots earnings and stock synchronicity at both the firm and industry levels for seven subgroups of warning followers. (Group means are used.) The subgroups are partitioned by the number of industry peers' warnings issued in the past 5 days before a firm's own warning (*PeerWarn*(*Actual*)). The numbers (percentages) of observations are 1,443 (47.8%), 846 (28.1%), 353 (11.7%), 180 (6.0%), 77 (2.6%), 47 (1.6%), and 70 (2.3%) for the no-peer-warning, 1, 2, 3, 4, 5, and larger than 5 peer warning groups, respectively. Variable definitions: Earnings synchronicity at the firm level (*SynEarnFirm*) is the R^2 of the regression of a firm's return on assets (*ROA*) on its industry ROA (calculated as total industry earnings divided by total industry assets) in the 20 quarters before the event quarter. Return synchronicity at the firm level (*SynRetFirm*) is the R^2 of the regression of a firm's weekly buy-and-hold returns on the value-weighted market returns and value-weighted industry returns in the calendar year before the warning. Synchronicity at the industry level, *SynEarnInd* and *SynRetInd*, is the weighted average of firm-level synchronicity, where the weight is a firm's sum of squared total variations of the regression (*SST*) over the total SST for firms in the industry. Industry classifications use the 48-groupings in Fama-French (1997)

7.2 Clustering induced by information transfer

Warnings may cluster because of information transfer from recent peers' warnings. In a single-firm setting with proprietary disclosure costs or uncertainty regarding managers' endowment of private information or both, the equilibrium disclosure policy follows a threshold rule: those with better private signals than the threshold disclose and others do not (Verrecchia 1983; Dye 1985; Jung and Kwon 1988). The key here is that managers disclose as long as the valuation with the private signal is higher than the blanket value assessed on all firms that do not disclose. In a multi-firm setting, peers' warnings serve as an external news event. A positive correlation between a firm's earnings and those of its peers would result in a decline of its stock price even if it does not warn. The reduced mean of the posterior distribution of firm value lowers the disclosure threshold and consequently some bad news that was previously withheld is disclosed. Thus, theoretically, peers' warnings could cause information transfer, which in turn induces the sample firm to warn, resulting in warning clustering. This process has been modeled by Acharya et al. (2008).

We take two steps to address the information-transfer explanation for warning clustering. First, we estimate returns from information transfer. For each industry quarter, we identify the leader, the first follower, the second follower, and other followers. To separate the effect of information transfer from the effect of a firm's own warning, we exclude a firm if its warning is on the same day as those of the

Table 8 Duration analysis: control for alternative explanations for warning clustering

Control for	Common shock	Inf. transfer	Both
PeerWarn(*t*)	0.063*** (2.80)	0.065*** (5.16)	0.058*** (2.55)
Size	0.045*** (3.85)	0.043*** (3.94)	0.044*** (3.79)
BadNews	−0.005 (−0.60)	−0.010 (−1.28)	−0.004 (−0.46)
Analyst	0.013 (1.22)	0.010 (0.93)	0.013 (1.22)
MarketShare	0.003 (0.29)	0.007 (0.87)	0.002 (0.28)
Past	0.572*** (7.74)	0.582*** (8.03)	0.570*** (7.67)
PastTime	−0.017*** (−8.14)	−0.017*** (−8.47)	−0.017*** (−8.06)
LeaderTime	−0.011*** (−8.78)	−0.011*** (−9.20)	−0.011*** (−8.65)
IndTime	0.375*** (6.84)	0.341*** (6.71)	0.367*** (6.67)
SynEarnFirm	0.017** (2.15)		0.020** (2.49)
SynEarnFrim*PeerWarn(t)	0.001 (0.34)		0.001 (0.24)
RecentRet		0.001 (0.21)	−0.001 (−0.34)
RecentRet*PeerWarn(t)		−0.002** (−2.13)	−0.002** (−2.02)
Six major industry dummies are included but not reported.			
Wald-test (χ^2)	431.68	465.35	436.46

SynEarnFirm is the R^2 of the regression of a firm's return on assets (ROA) on its industry ROA (calculated as total industry earnings divided by total industry assets) in the 20 quarters before the event quarter. We rank synchronicity (between 0 and 10) among all firms in the same industry quarter covered by Compustat, CRSP, and I/B/E/S. *RecentReT* is the buy-and-hold return (expressed in a percentage) in the 5 days before a firm's warning. Column 1 uses 83,412 daily observations of 2,669 warning followers. Column 2 uses 94,124 daily observations of 2,984 warning followers. Column 3 uses 82,470 daily observations of 2,640 warning followers. The table reports the coefficients and *z* statistics. See Table 5 for other variable definitions and notes

leader, first follower, or second follower. For each firm we calculate the market-adjusted stock returns on the day of its own warning (R_{own}) and on the day of the leader's (R_{leader}), first follower's ($R_{\text{follower1}}$), and second follower's warnings ($R_{\text{follower2}}$) that precede the sample firm's own. The last three returns are designed to capture information transfer.

Panel A of Table 9 tabulates these returns. R_{own} is about −5% across the four groups. R_{leader}, $R_{\text{follower1}}$, and $R_{\text{follower2}}$ are statistically significantly negative at between −0.3 and −0.5%, confirming the existence of information transfer. The magnitude, however, is small—only about 10% of the magnitude of the market reaction to a firm's own warning.[29]

In the second step, we control for information transfer in the Cox model. We use the stock returns in the preceding 5 days (*RecentRet*) to control for information transfer, because in Acharya et al. (2008) any external unfavorable news event, including peers' warnings, could induce a manager to disclose previously withheld bad news. Figure 4 displays this measure for the groups with varying degrees of clustering. Even

[29] On the other hand, later warnings have larger earnings shortfalls than early ones (Row 2), so the total market reaction to later firms should be larger. To examine the aggregate return effect, we collect market-adjusted returns from the day after the analyst consensus for the leader to the sample firm's own warning date (included). After controlling for earnings surprise, we find no difference in stock returns across firms of different warning sequence.

Table 9 Market-adjusted returns associated with information transfer

Median	Leader (L)	*Wilcoxon (F1–L)*	First follower (F1)	*Wilcoxon (F2–F1)*	Second follower (F2)	*Wilcoxon (O–F2)*	Other followers (*O*)
Warning duration	13		23		30		33
Surprise	−0.005	*−1.71**	−0.006	*−0.69*	−0.006	*−1.74**	−0.007
R_{own}	−5.0%***	*−0.66*	−5.6%***	*1.30*	−4.7%***	*0.33*	−4.5%***
R_{leader}			−0.4%***	*1.16*	−0.3%**	*−1.16*	−0.4%***
$R_{\text{follower1}}$					−0.5%**	*0.00*	−0.4%***
$R_{\text{follower2}}$							−0.4%***

To separate the effect of a firm's own warning from the effect of information transfer from others' warnings, we require a firm to be the sole warning firm in its industry on that day for it to be included in the "leader," "first follower," or "second follower" sample. For a firm to be included in "other followers," its warning cannot be on the same day as the warnings by the leader, first follower, and second follower. As a result, the leader, first follower, second follower, and other followers have 448, 406, 443, and 1,937 observations, respectively. ***, **, and * indicate statistical significance at the 1, 5, and 10% levels, respectively, in a two-tailed test

Variable definitions: *Warning duration* is the number of days after the beginning of a firm's third fiscal month until its warning date. *Surprise* is the difference between the forthcoming earnings per share and the most recent analyst consensus compiled before the warning, deflated by the split-adjusted beginning-of-event-quarter stock price. R_{own} is a firm's market-adjusted return on its own warning day. R_{leader} is a firm's market-adjusted return on the warning day of the industry leader. $R_{\text{follower1}}$ is a firm's market-adjusted return on the warning day of the first warning follower in the industry. $R_{\text{follower2}}$ is a firm's market-adjusted return on the warning day of the second warning follower in the industry

those with no recent peers' warnings experience an average return of −2% before the warning (the market-adjusted return is similar), suggesting information leakage. *RecentRet* generally decreases with the number of peers' warnings, consistent with the conjecture that information transfer induces warning clustering.

The Cox estimation with controls for information transfer is reported in Column 2 of Table 8. The interaction of *RecentRet* (expressed as a percentage) and *PeerWarn*(t) has a negative coefficient (coefficient = −0.002, $z = -2.13$), indicating that lower recent stock returns increase the likelihood of warning clustering. More important, after controlling for clustering induced by information transfer, the coefficient on *PeerWarn*(t) remains both economically and statistically positive (coefficient = 0.065, $z = 5.16$). Recognizing that both common shocks and information transfer may cause warning clustering, we control for both effects in Column 3. The coefficient on *Peerwarn*(t) remains significantly positive (coefficient = 0.058, $z = 2.55$). Taken together, our tests in this section support our primary finding of warning herding.

8 Conclusion

An earnings surprise can be caused by a combination of firm-specific factors and market or industry factors, but managers' performance evaluation is likely to be

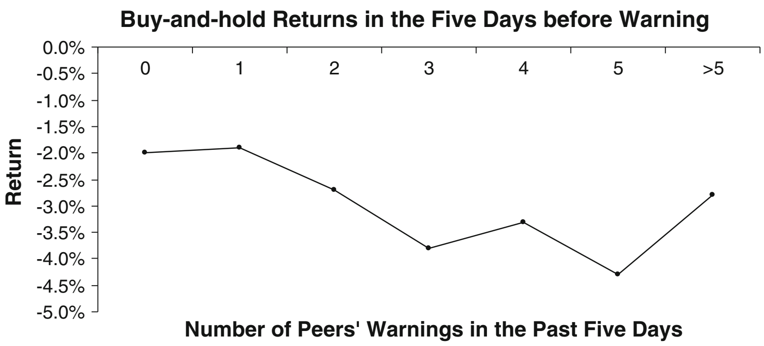

Fig. 4 The relation of stock returns in the 5 days preceding earnings warnings to the number of recent peer-firm warnings. The figure plots the buy-and-hold stock returns in the 5 days before a firm's warning for seven subgroups of warning followers. (Group means are used.) The subgroups are partitioned by the number of industry peers' warnings issued in the preceding 5 days before a firm's own warning (*PeerWarn*(*Actual*)). The numbers (percentages) of observations are 1,443 (47.8%), 846 (28.1%), 353 (11.7%), 180 (6.0%), 77 (2.6%), 47 (1.6%), and 70 (2.3%) for the no-peer-warning, 1, 2, 3, 4, 5, and larger than 5 peer warning groups, respectively

more adversely affected by firm-specific problems than by external factors. Managers thus have an incentive to choose disclosure strategies that overstate the apparent role of external factors in earnings shortfalls. Evaluators are more likely to use external factors to explain an agent's behavior when other agents behave similarly than when the agent's behavior is unique. Thus, we predict that managers who expect an earnings shortfall strategically time their warnings to occur soon after peer firms' warnings to minimize their blame for the earnings shortfall. We find evidence consistent with managerial herding.[30]

As in prior empirical herding studies, it is challenging to document warning herding because managers' private information and incentives are unobservable. We perform a battery of tests to examine alternative explanations for warning clustering. The additional analyses notwithstanding, we cannot rule out the possibility that the observed clustering is due to clustering in the timing of information arrival to firms or to other causes unrelated to the exercise of managerial discretion.

Our finding that managers herd in issuing warnings has immediate implications for market participants who closely monitor earnings warning. Market participants should be aware that managers exercise discretion over the timing of their warnings,

[30] Although our primary goal is to document the existence of warning herding, we conduct preliminary tests to determine whether herding managers have stronger career concerns than nonherding managers. We use the CEO's age as a proxy for career concerns because the younger a manager, the longer his future earnings stream, and thus the greater his concern about others' perceptions of his ability (Gibbons and Murphy 1992; Garen 1994; Bryan et al. 2000). We obtain CEO age data for 2,977 of the 3,016 warning followers (98.7%). We retrieve data for 1,430 observations from Compustat's executive compensation database and hand-collect data for the remaining 1,547 observations from DEF 14A proxy statements. This sample is comprised of 1,699 herders (those in the middle or at the tail of a warning cluster) and 1,278 nonherders. We find that the average CEO age is 53.6 for the herding group and 54.6 for the non herding group. The CEOs are younger in herding firms than in nonherding firms in both a t-test ($t = 3.78$) and a nonparametric Wilcoxon test ($z = 3.70$). This result is consistent with career concerns motivating warning herding.

so the appearance of warnings in a cluster need not imply that the bad news is from a common cause. Furthermore, our finding may help investors better predict the arrival of warnings because the likelihood of warning increases when peers start to warn and decreases with the time elapsed since the last warning by a peer firm.

Our study offers two promising avenues for future research of earnings forecasts or warnings. First, future research might examine the conditions under which managers are most likely to exercise their discretion in disclosure timing, along with the effects of managers' reporting strategies on their careers. Second, future research might examine the information on earnings performance that investors can glean from managers' disclosure-timing decisions.

In a broader sense, our study provides an important new perspective on managers' decisions when discretion is allowed. Managers make financial reporting and disclosure decisions in a multi-firm environment and use their private information as well as considering peer firms' actions in making these decisions. For example, SFAS No. 123, effective for fiscal years beginning after December 15, 1995, allows firms to either recognize their stock option expenses on the income statement or disclose them in the footnotes. On July 14, 2002, when Coca-Cola announced its plan to recognize option expenses, almost all U.S. companies opted for disclosure, with the prominent exceptions of Boeing, Winn-Dixie Stores, and AMB Property Corp. (Robinson and Burton 2004). A day after Coca-Cola broke the ice, Washington Post Co. and Bank One followed suit. By March 2003, 179 companies had decided to recognize stock option expenses and by July 2004, the number increased to 753 (SFAS 123R, para.B5).

Both Aboody et al. (2004) and Robinson and Burton (2004) observe clustering of corporate announcements for the decision to recognize stock option expense, and both find that investors react to early announcers but not to late announcers. Aboody et al. conclude (p. 148) that early announcers' returns are positive because investors favorably reassess those firms' prospects, but late announcers' returns are insignificant because investors believe mandatory expense recognition is imminent and therefore infer no signal from those announcements. Reputation herding offers an alternative explanation of the differential market reactions. Reputation herding models predict that early movers act on private information (for example, information about future prospects, as Aboody et al. conclude), but those making the same decisions later merely herd by mimicking the early announcers, ignoring their private information. Consequently, early announcers reveal new information, but later announcers do not.

Corporate managers have extensive discretion in many types of operating and disclosure decisions. The manner in which corporate managers exercise this discretion is a fascinating yet still poorly understood issue. Herding, in the form of informational cascade, reputational herding, or other strategic clustering as documented in our study, may appear in several contexts such as acquisitions, divestitures, the use of restricted stock, and asset write-offs. By using a novel duration analysis to document managers' herding, our study provides new insight into how managers use their discretion and opens the door for related future research.

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Appendix A

See Table 10.

Table 10 Example of earnings warnings issued in an industry quarter

Event	Warning date	Time of day	Fiscal quarter end	Earnings ann. date	Company name
Example 1: The chips industry in 1998 Q2					
1	1-Jun-98	10:36:01	30-Jun-98	20-Jul-98	Kemet Corp.
2	3-Jun-98	7:56:01	30-Jun-98	16-Jul-98	Artesyn Technologie Inc.
3	4-Jun-98	8:46:01	30-Jun-98	7-Jul-98	Motorola Inc.
4	5-Jun-98	8:25:01	30-Jun-98	22-Jul-98	DII Group Inc.
5	11-Jun-98	8:14:01	30-Jun-98	17-Aug-98	Ariel Corp.
6	11-Jun-98	9:37:01	30-Jun-98	21-Jul-98	Praegitzer Industries Inc.
7	11-Jun-98	16:33:01	30-Jun-98	27-Jul-98	MEMC Electronic Matrials Inc.
8	12-Jun-98	8:21:01	30-Jun-98	15-Jul-98	Celeritek Inc.
9	15-Jun-98	9:48:01	30-Jun-98	23-Jul-98	Benchmarq Microelectronics
10	15-Jun-98	16:46:01	30-Jun-98	23-Jul-98	Telcom Semiconductor Inc.
11	17-Jun-98	8:54:01	30-Jun-98	15-Jul-98	Stratex Networks Inc.
12	22-Jun-98	9:34:01	30-Jun-98	22-Jul-98	Cidco Inc.
13	23-Jun-98	16:18:01	30-Jun-98	22-Jul-98	Quality Semiconductor Inc.
14	23-Jun-98	17:41:01	30-Jun-98	23-Jul-98	Microsemi Corp.
15	25-Jun-98	9:35:01	30-Jun-98	29-Jul-98	Spectrian Corp.
16	30-Jun-98	16:33:01	30-Jun-98	14-Jul-98	Advanced Fibre Comm. Inc.
17	30-Jun-98	16:56:01	30-Jun-98	16-Jul-98	ATMEL Corp.
18	30-Jun-98	17:15:01	30-Jun-98	9-Jul-98	NMS Communications Corp.
19	30-Jun-98	18:12:01	30-Jun-98	22-Jul-98	Checkpoint Systems Inc.
20	1-Jul-98	16:49:01	30-Jun-98	21-Jul-98	Mosaix Inc.
21	2-Jul-98	8:56:01	30-Jun-98	23-Jul-98	EIS International Inc.
22	6-Jul-98	17:27:01	30-Jun-98	23-Jul-98	Zoran Corp.
23	7-Jul-98	18:08:01	31-Jul-98	25-Aug-98	Semtech Corp.
24	8-Jul-98	17:16:01	31-Jul-98	19-Aug-98	Hadco Corp.
25	9-Jul-98	8:44:01	31-Jul-98	19-Aug-98	Photronics Inc.
26	10-Jul-98	13:51:01	30-Jun-98	11-Aug-98	Teraforce Technology Corp.
27	20-Jul-98	9:16:01	31-Jul-98	19-Aug-98	Analog Devices

Table 10 continued

Event	Warning date	Time of day	Fiscal quarter end	Earnings ann. date	Company name
28	31-Jul-98	20:01:01	31-Jul-98	31-Aug-98	REMEC Inc.
29	14-Aug-98	6:46:01	31-Jul-98	14-Sep-98	CIENA Corp.
Example 2: The retail industry in 2001 Q2					
1	31-May-01	7:32:01	30-Jun-01	25-Jul-01	Footstar Inc.
2	15-Jun-01	8:17:01	30-Jun-01	11-Jul-01	Fasternal Co.
3	21-Jun-01	8:50:01	30-Jun-01	25-Jul-01	Sherwin-Williams Co.
4	27-Jun-01	8:35:01	30-Jun-01	31-Jul-01	CVS Corp.
5	2-Jul-01	19:17:01	30-Jun-01	26-Jul-01	Insight Enterprises Inc.
6	3-Jul-01	8:32:01	31-Jul-01	15-Aug-01	Payless Shoesource Inc.
7	5-Jul-01	8:10:01	30-Jun-01	31-Jul-01	Guess Inc.
8	5-Jul-01	8:39:01	31-Jul-01	15-Aug-01	Federated Dept. Stores
9	9-Jul-01	16:17:01	31-Jul-01	21-Aug-01	Gadzooks Inc.
10	9-Jul-01	16:18:01	30-Jun-01	31-Jul-01	Guitar Center Inc.
11	9-Jul-01	19:50:01	30-Jun-01	18-Jul-01	Linens N Things Inc.
12	10-Jul-01	8:05:01	30-Jun-01	25-Jul-01	Haverty Furniture
13	10-Jul-01	16:19:01	31-Jul-01	16-Aug-01	Tiffany & Co.
14	12-Jul-01	6:01:01	31-Jul-01	23-Aug-01	Claires Stores Inc.
15	12-Jul-01	7:02:01	31-Jul-01	21-Aug-01	Wilsons Leather Experts Inc.
16	12-Jul-01	7:51:01	31-Jul-01	15-Aug-01	AnnTaylor Stores Corp.
17	12-Jul-01	8:03:01	31-Jul-01	23-Aug-01	Mens Wearhouse Inc.
18	12-Jul-01	8:38:01	31-Jul-01	16-Aug-01	Sharper Image Corp.
19	12-Jul-01	8:42:01	31-Jul-01	14-Aug-01	TJX Companies Inc.
20	13-Jul-01	8:24:01	31-Jul-01	20-Aug-01	Toys R Us Inc.
21	25-Jul-01	17:06:01	31-Jul-01	23-Aug-01	Hibbett Sporting Goods Inc.
22	31-Jul-01	7:34:01	31-Jul-01	31-Aug-01	Finlay Enterprises Inc.
23	2-Aug-01	8:54:01	31-Jul-01	11-Sep-01	Neiman Marcus Group Inc.
24	6-Aug-01	1:30:01	31-Jul-01	28-Aug-01	Genesco Inc.
25	7-Aug-01	7:01:01	31-Jul-01	30-Aug-01	Zale Corp.
26	9-Aug-01	6:31:01	31-Jul-01	21-Aug-01	Saks Inc.
27	9-Aug-01	6:58:01	31-Jul-01	23-Aug-01	Ultimate Electronics Inc.
28	9-Aug-01	7:30:01	31-Jul-01	13-Aug-01	Pacific Sunwear Calif. Inc.
29	9-Aug-01	8:15:01	31-Jul-01	23-Aug-01	Charming Shoppes
30	15-Aug-01	8:23:01	31-Jul-01	6-Sep-01	CSK Auto Corp.

Appendix B

Warning leader vs. followers

We test the factors that distinguish a warning leader from its followers in a logit model:

$$\Pr(Leader_i) = F(a_0 + a_1 Size_i + a_2 BadNews_i + a_3 Analyst_i + a_4 MarketShare_i + a_5 PastLeader_i + a_6 FQE_i + a_7 Group_i + \varepsilon_i).$$

Leader is 1 if a firm issues the first warning in the industry for the event quarter and 0 otherwise. *Size, BadNews*, *Analyst*, and *MarketShare* are as defined in the duration analysis in the main text of the paper. We additionally include *PastLeader*, *FQE,* and *Group*.

Firms' disclosure practices tend to be sticky (Lang and Lundholm 1996a). We expect firms that were warning leaders in prior quarters to be more likely to warn first in the current quarter than other firms. *PastLeader* is 1 if a firm is the warning leader in its industry in any of the five previous quarters in which it warned and 0 otherwise.

Firms that end their fiscal quarters earlier are likely to issue warnings sooner than their peers. We define *FQE* as 1, 0, and −1 if a firm ends its fiscal quarter in the first, second, and third month of the calendar quarter, respectively and expect a positive coefficient.

Finally, we include the number of firms in each industry quarter cross section, *Group*. The larger the number of firms in an industry quarter, the less likely it is that a particular firm is the leader.

Table 11

	Coefficient	Coefficient	Coefficient	Odds Ratio
Intercept	−0.631*** (−2.97)	−0.611*** (−2.88)	−0.810*** (−3.56)	N/A
Size			0.091*** (2.78)	1.095
BadNews	−0.070*** (−3.62)	−0.069*** (−3.58)	−0.053*** (−2.60)	0.948
Analyst	0.039* (1.79)	0.025 (1.11)	−0.037 (−1.15)	0.964
MarketShare		0.023** (2.25)	0.014 (1.31)	1.014
PastLeader	0.199 (1.59)	0.198 (1.58)	0.204 (1.63)	1.227
FQE	2.638*** (12.07)	2.640*** (12.05)	2.664*** (12.06)	14.354
Group	−0.130*** (−10.15)	−0.127*** (−9.91)	−0.128*** (−10.02)	0.879
McFadden pseudo R^2	0.197	0.198	0.201	

A total of 3,525 observations are used, among which 509 are leaders. The odds ratio is the proportional change in the odds of being the leader when the independent variable increases by 1 (mathematically, the odds ratio is the exponential transformation of the coefficient.). ***, **, and * indicate statistical significance at the 1, 5, and 10% levels, respectively, in a two-tailed test. The estimation is robust to heteroskasticity and firm-level error correlations

Variable definitions: *Leader*, *PastLeader*, *FQE*, and *Group* are defined in the text of this appendix. *Size* is the average market capitalization (in millions) in the four fiscal quarters before the warning. *BadNews* is the full-sample ranking of the magnitude of negative earnings surprise, where *Surprise* is the difference between the forthcoming earnings per share and the most recent analyst consensus compiled before the warning, deflated by the split-adjusted beginning-of-event-quarter stock price. *Analyst* is the average number of analyst forecasts included in the most recent consensus before earnings announcements for the four quarters before the event quarter. *Marketshare* is the ratio of a firm's total sales for the most recent fiscal year before the event quarter over the industry's total sales for that year. We rank *Size* and *Analyst* among all firms in the industry-year-quarter that are covered by Compustat, CRSP, and I/B/E/S and rank *BadNews* in the full sample. For convenient interpretation, we normalize the rankings of *BadNews, Size*, and *Analyst* to be between 0 and 10 and *MarketShare* to be between 0 and 100

Table 11 presents the estimation results. The last two columns report the full-model results. As predicted, firm size is significantly positively associated with being the warning leader ($a_1 = 0.091$, z-statistic $= 2.78$). In the last column of the table we report the odds ratio. The odds ratio for *Size* is 1.095, indicating that when a firm's size rank increases by 10%, the odds of being the warning leader are 9.5% higher. The magnitude of bad news is negatively associated with being the leader ($a_2 = -0.053$, z-statistic $= -2.60$). The coefficients on *Analyst* and *MarketShare* are insignificant, probably due to their high correlations with firm size, (See Columns 1 and 2 of Table 11 when *Size* is excluded.) Finally, the coefficient on *PastLeader* is statistically insignificant ($a_5 = 0.204$, z-statistic $= 1.63$). As expected, the coefficient on *FQE* is significantly positive, and the coefficient on *Group* is significantly negative, both merely reflecting a mechanical relationship induced by our research design.

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