Do Firm-Specific Stock Price Crashes Lead to a Stimulation or Distortion of Market Information Efficiency?*

JEONG-BON KIM, *City University of Hong Kong*

EDWARD LEE, *Alliance Manchester Business School, University of Manchester*

ZHENMEI (JUDY) ZHU, *School of Management, Fudan University*†

ABSTRACT

Unlike prior research that focuses on determinants of firm-specific stock price crashes (SPCs), we study the consequences of SPCs on market information efficiency. The tension underlying our research question stems from two competing explanations. As an unanticipated shock, an SPC could stimulate (distort) information efficiency by triggering investor rational attention (opinion divergence). Our identification strategy involves a difference-in-differences analysis in which SPC firms in the treatment sample are propensity score matched with non-SPC firms in the industry-peer control sample, as well as placebo tests for falsification. Consistent with the stimulation effect, we find an increase of the earnings response coefficient and a decrease in post-earnings announcement drift, from the pre- to post-SPC period, for SPC firms, but not for non-SPC firms. Further analyses reveal that SPC firms attract increased investor attention, as reflected in greater analyst coverage and more investor access to firms' online financial filings following such an event. Using mutual fund flow redemption pressure based on hypothetical sales as an exogenous shock to SPCs, we provide evidence corroborating our causal interpretation of the main findings. Collectively, the evidence suggests that SPCs can attract increased investor attention, bringing about positive externalities by stimulating market information efficiency.

Keywords: stock price crashes, earnings response coefficient, post-earnings announcement drift, market information efficiency

L'effondrement du cours des actions d'entreprises individuelles a-t-il pour effet de stimuler ou de déformer l'efficience informationnelle?

RÉSUMÉ

Contrairement aux études antérieures qui mettent l'accent sur les déterminants de l'effondrement du cours des actions (ECA) d'entreprises individuelles, nous nous attardons aux conséquences des ECA sur l'efficience informationnelle du marché. La tension sous-jacente à notre question de recherche est attribuable à l'existence de deux explications contradictoires. L'ECA, en tant que choc imprévu, pourrait stimuler (déformer) l'efficience informationnelle en attirant l'attention

* Accepted by Linda Myers. We are grateful for useful comments and suggestions that we received from Linda Myers (editor), Alan Webb (editor-in-chief), two anonymous referees, Michael Brennan, Hans Christensen, John Core, S. P. Kothari, and Martin Walker, and from attendees at the BAFA NAG Conference (Bangor, UK), CCBR Symposium (Cardiff, UK), EAA Annual Congress (Valencia, Spain), and ISAR Conference (Dalian, China), and from participants in the seminars held at City University of Hong Kong, Fudan University, Jinan University, Massachusetts Institute of Technology, Sun Yat-Sen University, University of Cyprus, University of Essex, and University of Waterloo. We acknowledge the financial support from National Natural Science Foundation of China (No. G0205-71702034).

† Corresponding author.

rationnelle des investisseurs (en suscitant chez les investisseurs une divergence d'opinions). Notre stratégie d'identification fait appel à une analyse de l'écart dans les différences dans le cadre de laquelle les entreprises touchées par un ECA dans l'échantillon expérimental font l'objet d'un appariement par coefficients de propension avec des entreprises non touchées par un ECA dans l'échantillon témoin de pairs au sein de l'industrie, ainsi qu'à des tests placebos aux fins de falsification. Conformément à l'effet de stimulation attendu, nous établissons une augmentation du coefficient de réponse aux résultats et une diminution de la dérive consécutive à l'annonce des résultats, entre la période précédant l'ECA et la période ultérieure, pour les entreprises touchées par un ECA, mais pas pour les autres entreprises. Des analyses supplémentaires révèlent que les entreprises touchées par un ECA attirent davantage l'attention des investisseurs, comme en témoignent la couverture plus importante par les analystes et l'augmentation du nombre de consultations des documents financiers en ligne des entreprises à la suite d'un tel événement. En utilisant la pression exercée sur le plan du rachat de fonds communs de placement et en nous fondant sur des ventes hypothétiques en guise de choc exogène pour les ECA, nous fournissons des données probantes qui corroborent notre interprétation causale des principales observations. Collectivement, les données probantes portent à croire que les ECA peuvent attirer l'attention des investisseurs et créer des effets positifs externes en stimulant l'efficience informationnelle du marché.

Mots-clés : effondrement du cours des actions, coefficient de réponse aux résultats, dérive consécutive à l'annonce des résultats, efficacité de l'information sur le marché

1. Introduction

A growing body of academic research focuses on identifying the causes or determinants of firm-specific stock price crash (SPC) risk. From 2001 through 2020, more than 240 published and working papers have emerged on this topic.[1] Although this literature contributes to our understanding of the antecedents of SPCs, much less is known about their consequences, beyond their expected detrimental effects on investor confidence and shareholder wealth. Unlike previous studies, our study aims to fill this gap by assessing the consequences of a firm's SPC in the context of its impact on market information efficiency with respect to a firm's stock. Specifically, we investigate whether SPCs stimulate or distort information efficiency in capital markets. Because security prices play a key role in resource allocation and corporate information influences firm valuation and governance in market-oriented economies (Kothari 2001; Beyer et al. 2010), the informational consequences of SPCs should concern investors, managers, and regulators alike. As such, the question of whether and how SPCs affect market information efficiency is important and is interesting in its own right.

The theoretical rationale and tension underlying our research question stem from two competing explanations. On the one hand, SPCs may stimulate firm-level information efficiency by invoking heightened investor attention to a crashed firm's stock price. At the general level, uncertainty motivates economic agents to gather new information and revise their beliefs, consistent with Bayesian updating (Kandel and Stambaugh 1996; Pastor and Veronesi 2009). This is because new information strengthens agents' ability to formulate decisions under conditions of uncertainty (Banker et al. 1993; Case 2012). Focusing on capital markets, evidence shows that rational attention and information gathering by investors hasten their reactions to new information such as earnings announcements (Drake et al. 2015; Ben-Rephael et al. 2017). Because a firm's SPC is a rare event that damages investor confidence and entails a huge loss of investor wealth, its occurrences should trigger or increase investor attention, thereby enhancing market information efficiency with respect to the firm's stock.

On the other hand, SPCs might also distort firm-level information efficiency by provoking a divergence of opinion among investors. Investors' expectations become more heterogeneous as

1. Appendix 1 reports the journals, topics, and year of distribution for empirical studies on firm-specific SPC risk. Panel A shows a total of 248 studies, comprised of 172 published papers (38 of which are in *Financial Times* Top 50 journals) and 76 working papers. Panel B reveals that as many as 240 of these 248 studies seek to identify SPC determinants, with information quality and corporate governance being inferred as the main drivers. Panel C shows a large increase in interest in SPCs over the latter years.

they face higher uncertainty (Miller 1977; Diamond and Verrecchia 1987). This is due, in large part, to differences in their demand for compensation for idiosyncratic risk (Merton 1987; Diether et al. 2002). As a result of opinion divergence, investors tend to place greater weight on their own subjective judgments (Holthausen and Verrecchia 1990; Kim and Verrecchia 1994), which leads them to underreact to public news (Daniel et al. 1998; Hong and Stein 2007). Empirical studies support the above analytical result by showing that divergence of investor opinions slows down price corrections subsequent to the release of firm-specific news such as earnings announcements (Garfinkel and Sokobin 2006; Anderson et al. 2007). Because SPCs bring about unexpected and severe underperformance, these unanticipated shocks could also provoke or escalate opinion divergence among investors, thereby delaying share price responses to firm-specific news and weakening market information efficiency.

Overall, the directional effect of a firm's SPC on subsequent market information efficiency is an empirical question. To address this under-explored question, we first identify SPC events and then observe subsequent changes in information efficiency with respect to a firm's earnings announcements. Specifically, an SPC event is said to occur when the firm-specific weekly returns fall more than 3.20 standard deviations below the mean for the 12-month estimation period (Kim et al. 2011a, 2011b). To observe changes in information efficiency, we adopt two distinct approaches that are well established in the literature. First, we assess the earnings response coefficient (ERC) to evaluate the responsiveness of investors to earnings news (Collins and Kothari 1989; Berkman and Truong 2009). Second, we evaluate post-earnings announcement drift (PEAD) to observe the delay in price corrections after the release of earnings news (Bernard and Thomas 1989; Chordia and Shivakumar 2006). If the stimulation (distortion) scenario holds, then we should observe an increase (decrease) in ERCs and a decrease (increase) in PEAD from the pre- to post-SPC period.

Our main identification strategy comprises an evaluation of the treatment effect by means of a difference-in-differences (DiD) analysis, as well as a falsification analysis based on placebo tests to reinforce our main results. For the DiD analysis, we take the following two steps. First, for SPC firms in our treatment sample, we determine the difference in ERCs or PEAD between two distinct announcements: (i) the *post*-SPC earnings announcement, which is made in the month subsequent to the 12-month SPC estimation window and (ii) the *pre*-SPC earnings announcement, for the equivalent fiscal quarter two years before the post-SPC earnings announcement.[2] Second, we contrast changes in ERCs or PEAD from the pre- to post-SPC announcements for SPC firms in the treatment sample versus non-SPC firms in a propensity score-matched (PSM) control sample for the same period. For the falsification analysis, we conduct placebo tests examining changes in ERCs or PEAD for either the two years after or the two years before the SPC estimation window, applied to the same set of treatment and control firms as used in the DiD analysis.

Our empirical analyses are based on a sample of 48,964 firm-quarter observations that comprises listed firms in the United States from 1984 through 2017. The DiD analysis reveals that the treatment firms experience a significant and economically meaningful increase (decrease) in ERCs (PEAD) for the post-SPC earnings announcement relative to the pre-SPC earnings announcement. In contrast, these effects are neither observed among the control firms over the same period nor among the treatment and control firms in the placebo tests. In other words, our analyses suggest that investors are more responsive to a firm's earnings news and that price discovery is less delayed subsequent to the occurrence of an SPC. In terms of economic significance, the ERC analysis shows an increase of 12.9% and the PEAD analysis shows a decrease of 58.1% for the treatment firms from the pre- to post-SPC period. Collectively, these main findings lend support to the view that the stimulation effect driven by investor rational attention dominates the distortion effect caused by investor opinion divergence.

To further substantiate this underlying explanation for our main findings, we carry out two sets of additional tests. First, we observe a significant post-SPC increase in the number of analysts

2. Therefore, the post-SPC earnings announcement window does not overlap with the 12-month SPC estimation window.

following treatment firms but no change in the diversity in analyst forecasts for treatment firms, relative to control firms. Prior literature suggests that investor attention drives an increase in analyst coverage (O'Brien and Bhushan 1990; Martineau and Zoican 2020), whereas analyst forecast diversity captures the divergence of investor opinions (Barron et al. 1998; Doukas et al. 2006). Accordingly, the observed post-SPC increase in analyst coverage, along with no change in analyst forecast diversity, provides further evidence in support of an increase in investor rational attention, but not opinion divergence among investors, following SPCs. Second, we find a significant post-SPC increase in investor access to the US SEC's Electronic Data Gathering, Analysis, and Retrieval (EDGAR) system for treatment firms relative to control firms. The existing literature shows that EDGAR access, which reflects the intensity of investors' information acquisition activities, is positively related to investor attention and demand for firm-specific information (Drake et al. 2015; Ryans 2017, 2021). As such, the findings from this set of additional tests offer more direct evidence that SPCs stimulate an increase in investor rational attention to, and information gathering activities about, SPC firms.

To strengthen our inference and rule out alternative explanations, we conduct a variety of robustness tests. First, we control for changes in financial reporting quality measured by absolute discretionary accruals (Hutton et al. 2009) and conditional conservatism (Kim and Zhang 2016). We find that the post-SPC increase (decrease) in ERCs (PEAD) remains significant even after controlling for changes in financial reporting quality potentially induced by SPCs. Second, we identify and control for the possible influence associated with various types of major corporate events, ranging from executive changes to litigations, that could also trigger SPCs. Our main inference remains robust in the presence of these controls, suggesting that our findings are unlikely to be driven by the effects of these major corporate events. Third, we provide further evidence of the causality of our main findings by drawing on mutual fund flow redemption pressure based on hypothetical sales (Edmans et al. 2012; Dessaint et al. 2019; Bennett et al. 2020) as an exogenous shock to SPCs. We first show that mutual fund flow pressure is positively related to SPCs, confirming that this pressure significantly drives the occurrence of SPCs. We then find that, across different test specifications, this pressure is positively (negatively) related to ERCs (PEAD). This finding provides corroborating evidence on the causal effect of SPCs on market information efficiency, which buttresses and enriches our main inference. Fourth, we also compare the ERC and PEAD effects between SPCs and firm-specific stock price jumps (SPJs). The ERC analysis provides some mixed evidence of an SPJ effect, which is weaker in intensity than that of the SPC effect. The PEAD analysis reveals no significant evidence of an SPJ effect. The above finding suggests that our evidence on the improved information efficiency after extreme price movements is indeed mainly specific to the extreme downside risk captured by SPCs, consistent with investors paying more attention to bad news than good news (Veronesi 1999; Leippold et al. 2008).

Our study contributes to the existing literature in the following ways. First, to our knowledge, it is among the few, if not the first, to examine the informational consequences, rather than the determinants, of firms' SPCs. Although the substantial loss of wealth for the existing shareholders is often perceived as the main consequence of SPCs (Pan 2002; Yan 2011), our analysis reveals that SPCs may also induce some positive externalities for outside investors and capital markets by improving market information efficiency. Second, our study expands the literature on the influence of investor rational attention on asset pricing (Kandel and Stambaugh 1996; Pastor and Veronesi 2009). Our evidence that SPCs accelerate price corrections following earnings announcements supports the view that investors pay selective attention toward more salient stimuli because of constraints in learning and information gathering capacity (Treisman 1960; Odean 1998). Third, we enrich the literature on accounting-based stock return anomalies by informing the long-standing debate over whether mispricing or misspecification (i.e., omitted risk factors in asset pricing models) causes PEAD (Bernard and Thomas 1990; Richardson et al. 2010). Our evidence from the PEAD analysis is consistent with this effect being driven by mispricing due to investors' insufficient attention to firm-specific information.

The paper proceeds as follows. Section 2 discusses the literature and hypotheses. Section 3 describes the research design and data. Section 4 presents the empirical findings and section 5 concludes.

2. Literature review and development of hypotheses

SPC risk literature

The investment community, security regulators, and academic researchers have paid considerable attention to the issue of firm-specific SPC risk. Given that crashes cause huge losses of investor confidence and shareholder wealth (Pan 2002; Yan 2011), prior research focuses mainly on identifying firm-specific determinants or predictors of the likelihood that an SPC will take place in the future. Earlier studies such as Jin and Myers (2006) and Hutton et al. (2009) provide evidence supporting the hypothesis that actual SPCs are triggered by the release of previously withheld bad news. Subsequent studies suggest a wide array of internal and external factors that may either exacerbate or mitigate the withholding of private information by managers or corporate insiders. Some of these within-firm factors include corporate tax avoidance (Kim et al. 2011a), equity incentives (Kim et al. 2011b), managerial overconfidence (Kim et al. 2016), financial reporting opacity (Kim and Zhang 2014), and conditional conservatism (Kim and Zhang 2016). Some factors external to firms include changes in accounting standards (DeFond et al. 2015), auditor tenure (Callen and Fang 2016), corporate tax enforcement (Bauer et al. 2020), and social norms associated with religiosity (Callen and Fang 2015). However, as indicated in our analysis of this literature in Appendix 1, prior studies pay little attention to the consequences of firm-level SPCs. We fill this research gap by examining the consequences of SPCs on information efficiency in capital markets.

Development of hypotheses

We expect a firm's SPC to have two offsetting effects on market information efficiency—that is, a stimulation effect and a distortion effect. The *stimulation* effect may arise if the SPC occurrence increases rational attention among investors. Extreme downward share price movements of a firm provide a salient signal to capital markets, drawing investor attention. These events indicate that a firm is associated with severe downside risk and a high degree of uncertainty. When faced with uncertainty shocks, investors have greater incentive to gather information and update their beliefs (Kandel and Stambaugh 1996; Pastor and Veronesi 2009) to facilitate their valuation (Banker et al. 1993; Case 2012). When investors pay more attention to a firm under conditions of higher uncertainty, they increase the demand for information, such as information supplied by financial analysts. To the extent that financial analysts can enrich the corporate information environment by serving as information intermediaries (Bradshaw 2009; Beyer et al. 2010) or by disseminating their private information (Kim and Verrecchia 1997; Barron et al. 1998), they are expected to cater to investors' information demands following uncertainty shocks such as SPCs. Based on this argument, we expect that firm-specific SPCs accelerate investors' price discovery process, causing security prices to reflect new information more quickly after such events. As a result, firm-specific SPCs would contribute to stimulating market information efficiency with respect to that firm's stock.

In contrast, the *distortion* effect can occur if an SPC provokes a divergence of investor opinions. Firms that experience SPCs are associated with extreme underperformance and uncertain future prospects. When investors perceive greater uncertainty, the heterogeneity of their expectations is likely to intensify (Miller 1977; Diamond and Verrecchia 1987). The existing literature suggests that investor opinion divergence can delay share price responses to news, and this prediction is supported by theoretical studies (Hong and Stein 1999; Allen et al. 2006; Hong and Stein 2007; Banerjee et al. 2009) and empirical evidence (Zhang 2006; Verardo 2009). For instance, Hong and Stein (2007) argue that delays in price corrections tend to be stronger when there is greater heterogeneity of beliefs among investors because they erroneously assume that the information they hold is sufficient for security valuation and it takes time for investors to correct this assumption. Allen et al. (2006) suggest that price drifts are more pronounced when there is greater divergence of investor opinions because investors need time to elicit one another's belief about firm fundamentals. Empirical studies suggest that price continuation effects such as return momentum (Zhang 2006) and PEAD (Garfinkel and Sokobin 2006) are more pronounced among firms with greater opinion divergence. Drawing on the above discussions, we would expect that firm-specific SPCs, which bring about an increase in belief heterogeneity among investors, delay systematic price corrections toward the fundamental value of the firm. This causes security prices to incorporate new information more slowly following such events, and thus contributes to distorting market information efficiency with respect to a firm's stock.

Overall, both stimulation and distortion effects on information efficiency are likely to be at play in driving changes in firm-level information efficiency following firm-specific SPCs. To evaluate changes in information efficiency, our analysis focuses upon the impact of SPCs on both ERCs and PEAD. If the stimulation (distortion) effect dominates, on average, by influencing rational attention (opinion divergence) among investors, we would expect to observe the following. First, with respect to ERCs, short-window market reactions to earnings announcements following SPCs should be more (less) responsive to earnings news, leading to higher (lower) ERCs following SPCs. Thus, we formulate and test the following hypotheses, stated in the alternative, on the influence of firm-specific SPCs on ERCs:

Hypothesis 1a (H1a). *Firm-specific SPCs stimulate market information efficiency with respect to a firm's stock and increase ERCs.*

Hypothesis 1b (H1b). *Firm-specific SPCs distort market information efficiency with respect to a firm's stock and decrease ERCs.*

Second, with respect to PEAD, price corrections over the window after earnings announcements following SPCs are likely to be quicker (slower) if SPCs enhance (deteriorate) market information efficiency. This would lead us to observe smaller (larger) PEAD following SPCs. Thus, we formulate and test the following hypotheses, stated in the alternative, on the influence of firm-specific SPCs on PEAD:

Hypothesis 2a (H2a). *Firm-specific SPCs stimulate market information efficiency with respect to a firm's stock and decrease PEAD.*

Hypothesis 2b (H2b). *Firm-specific SPCs distort market information efficiency with respect to a firm's stock and increase PEAD.*

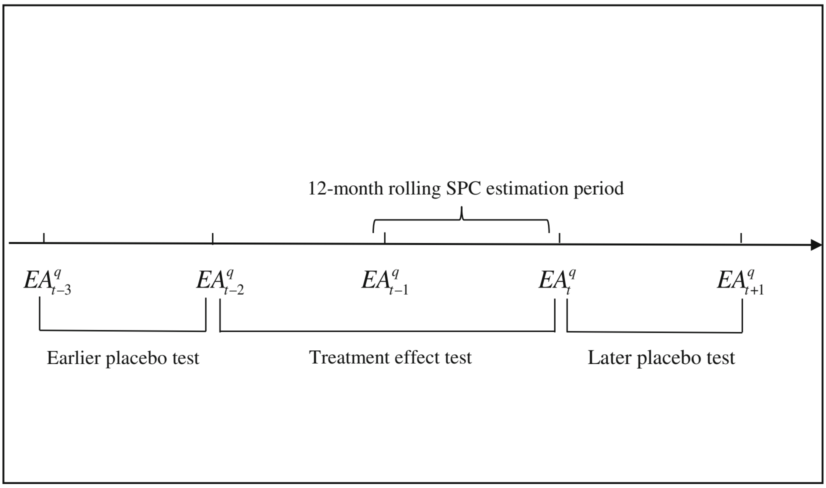
3. Research design

Identification strategy

To examine whether and how firm-specific SPCs influence ERCs and PEAD, we adopt a DiD analysis as our main identification strategy. We then reinforce our DiD results by means of a falsification analysis based on placebo tests. We define an SPC event as taking place if weekly firm-specific returns (after netting out market-wide returns), denoted by W, decline below a threshold level (Kim et al. 2011a, 2011b). Specifically, we estimate W for firm j using the following time-series regression over the 12-month period before each quarterly earnings announcement:

$$r_{j\tau} = \beta_0 + \beta_1 r_{m\tau-2} + \beta_2 r_{m\tau-1} + \beta_3 r_{m\tau} + \beta_4 r_{m\tau+1} + \beta_5 r_{m\tau+2} + \varepsilon_{j\tau}, \quad (1)$$

Figure 1 Research design



Notes: This figure illustrates our research design. For each earnings announcement EA^q_t for fiscal quarter q of year t, we determine whether or not at least one SPC event occurred in the preceding 12-month rolling estimation period that ended before the month in which the earnings announcement was made. For example, if the earnings announcement date is May 20, 2006, for earnings for the fiscal quarter ending on March 31, 2006, we determine whether or not an SPC occurred during the 12 months from May 1, 2005, through April 30, 2006. In the treatment effect test, we compare ERCs or PEAD for post-SPC earnings announcements (EA^q_t), made in the month after the 12-month SPC estimation window, with pre-SPC earnings announcements (EA^q_{t-2}), which are made for the equivalent fiscal quarter two years previously and thus not overlapping with the SPC estimation window. In the earlier placebo test, we compare ERCs or PEAD of earnings announcements (EA^q_{t-2}) with that of the equivalent fiscal quarter of the preceding year (EA^q_{t-3}). In the later placebo test, we compare ERCs or PEAD of earnings announcements (EA^q_{t+1}) with that of the equivalent fiscal quarter of the preceding year (EA^q_t). We select treatment firms as those with at least one SPC event in the 12-month period ending before EA^q_t. For the control firms, we require no SPCs in the 12-month period ending before EA^q_t.

where $r_{j\tau}$ is the return on stock j in week τ, $r_{m\tau-2}$ through $r_{m\tau+2}$ are the market returns in week $\tau-2$ through $\tau+2$, respectively; and W is the weekly firm-specific return, calculated as the natural logarithm of 1 plus the residual return ϵ. We define an SPC as taking place when W falls 3.20 standard deviations below the firm's mean over the estimation period. Appendix 2 provides more detailed variable definitions.

Our DiD analysis (i.e., the treatment effect test) compares changes in treatment firms' ERCs or PEAD from the pre- to post-SPC earnings announcements with equivalent changes for firms in a PSM-determined industry-peer control sample. Figure 1 provides an illustration of our research design. As depicted in Figure 1, the post-SPC earnings announcement (EA^q_t) is made for fiscal quarter q in year t in the month *after* the 12-month rolling SPC estimation period. The pre-SPC earnings announcement (EA^q_{t-2}) is made for the equivalent fiscal quarter q two years earlier, and therefore avoids overlapping with the estimation period.[3] Treatment firms are those that experience at least one SPC event during the 12-month rolling estimation period preceding EA^q_t. This

3. Overlapping could occur if the earnings announcement one year before the post-SPC announcement was used as the pre-SPC announcement.

DiD analysis enables us to observe, for treatment firms, changes in ERCs or PEAD after they experience the SPCs prior to their earnings announcements.

The PSM approach then matches each treatment firm with a control firm (with no SPCs prior to the earnings announcements) selected from the same industry. These control and treatment firms have a similar level of SPC propensity based on nine determinants adopted from Jang and Kang (2019).[4] In general, this set of SPC determinants provides comprehensive coverage of the widely recognized antecedents of price crash risk from the existing literature. Therefore, the treatment firms and control firms are similar in terms of the likelihood of SPCs and are exposed to similar unidentified industry and temporal influences, with the difference being that the former experience SPCs whereas the latter do not. We compare changes in ERCs and PEAD from the pre- to post-SPC period for the treatment versus control samples. This comparison enables us to test whether changes in market information efficiency (i.e., changes in ERCs and PEAD) are observed only in the treatment sample. As such, we essentially examine the causal impact of SPCs over time on subsequent changes in ERCs and PEAD.

We estimate the following regression model, separately for the treatment and control samples, in order to examine the effect of SPCs on ERCs or PEAD:

$$CAR_{jq} = \alpha_0 + \alpha_1 SUE_{jq} + \alpha_2 POST_{jq} + \alpha_3 POST_{jq} \times SUE_{jq} + Controls + \delta_{jq}, \tag{2}$$

where, for each firm j and each earnings announcement for quarter q, *CAR* is the cumulative market-adjusted stock return over two trading days (0 through +1) around an earnings announcement for ERC tests, and over 60 trading days (+2 through +61) following an earnings announcement for PEAD tests. *SUE* is standardized unexpected earnings, calculated as actual earnings per share minus analyst forecasts of earnings per share for each quarter, scaled by share price 20 days prior to the earnings announcement date; we use the decile ranked value of *SUE* by quarter ranging from −0.5 to 0.5, inclusive, in our regression analyses.[5] *POST* is equal to one for earnings announcements in year t (EA_t^q) immediately after the SPC estimation period, and zero for earnings announcements two years earlier, that is, year t−2 (EA_{t-2}^q). For control variables (*Controls*), we include a wide range of firm characteristics that could drive share price responses to earnings announcements and their interactions with *SUE*.[6] In ERC tests, if the coefficient on *POST*×*SUE*, that is, a_3, is significantly positive (negative) for the treatment sample and its absolute magnitude is significantly greater for the treatment sample than for the control sample, then the evidence would be consistent with H1a (H1b). In the PEAD tests, if the coefficient a_3 is significantly negative (positive) for treatment firms and its absolute magnitude for the treatment sample is significantly different from that for the control sample, then the evidence would be consistent with H2a (H2b).

We also perform placebo tests to examine changes in ERCs or PEAD over two different time intervals that are further away from the rolling SPC estimation period. As illustrated in Figure 1, our first set of placebo tests compares the pre-SPC periods EA_{t-3}^q and EA_{t-2}^q, and our second set of placebo tests compares the post-SPC periods EA_t^q and EA_{t+1}^q. For comparability, we perform

4. These determinants are detrended turnover (*DTURN*), negative return skewness (*NCSKEWLAG*), lagged returns (*RETLAG*), return volatility (*SIGMA*), market capitalization (*SIZEPSM*), market returns (*RM*), sales growth (*SALESG*), firm age (*AGE*), and tangible assets (*TANG*). Appendix 2 provides detailed definitions.

5. Mashruwala et al. (2006) explain the problem that arises when ranked values ranging from 0 to 1, inclusive, interact with each other. As such, we use the decile ranked values ranging from −0.5 through 0.5. These are computed as $[(n-1)/9] - 0.5$, where n is the decile rank sorted in ascending order.

6. These control variables are market beta (*BETA*), market capitalization (*SIZE*), book-to-market ratio (*BM*), return momentum (*MOM*), a subsequent SPC indicator (*SUBSPC*), whether the earnings announcement is made for the 4th fiscal quarter (*Q4*), a loss indicator (*LOSS*), earnings volatility (*EVOL*), return volatility (*IVOL*), illiquidity (*ILLIQ*), and institutional ownership (*IO*). We rank all controls except for indicator variables into deciles by quarter and assign values ranging from −0.5 through 0.5. Appendix 2 provides detailed definitions.

these placebo tests using the same sets of treatment and control firms. If the incremental changes in ERCs and PEAD from the pre- to post-SPC period observed for treatment firms but not for control firms are indeed driven by SPCs, we should not observe similar changes in ERCs and PEAD in the placebo tests. Therefore, this falsification analysis strengthens our inference by reducing the likelihood that our main findings are part of a longer time trend, rather than triggered by SPCs, or that they are driven by other unidentified confounding factors.

Sample construction

Our initial sample includes all common stocks (share code 10 or 11) listed on the NYSE, AMEX, and NASDAQ exchanges from 1984 through 2017.[7] After excluding financial (SIC codes 6000–6999) and utility firms (SIC codes 4000–4999) and those with a share price below \$5, negative book value of equity, or missing values for variables used in our main regression analyses, the sample consists of 206,253 firm-quarter observations. We further exclude observations with missing values for the determinants of SPC propensity, and this reduces the sample size to 176,718 firm-quarter observations. We also require that data for sample firms be available from year $t-3$ through year $t+1$ to maintain a consistent sample across the main treatment effect test and the earlier and later placebo tests. These criteria yield a sample of 63,016 firm-quarter observations, of which 12,441 are classified as treatment firms. In accordance with the use of the PSM approach, our final sample includes 12,241 firm-quarter observations in the treatment sample and the same number of observations in the control sample. Our DiD analysis compares changes in ERCs or PEAD from pre-SPC EA^q_{t-2} to post-SPC EA^q_t for treatment firms with those changes for control firms, resulting in a total of 48,964 firm-quarter observations used in the treatment effect test. The same number of firm-quarter observations is also used in the earlier and later placebo tests. Appendix 3 provides further details about our sample selection.

Appendix 4 reports our PSM implementation. In the first-stage model reported in panel A, we observe that up to seven out of nine determinants are statistically significant, the pseudo R^2 is 0.0306, and area under the ROC curve is 0.6223. These fit statistics are consistent with those reported in prior research on the determinants of SPCs (Kim et al. 2011a). To assess covariate balance between the treatment and control samples, we report the parametric t-test that compares the difference in means and the nonparametric Kolmogorov-Smirnov (K-S) test that compares the difference in distributions. Panel B shows that before PSM, six out of nine determinants are significantly different under the t-test and all determinants are significantly different under the K-S test. Panel C shows that after PSM, no determinants are significantly different under the t-test and only one determinant differs significantly under the K-S test. Thus, our matching process achieves balance for most covariates.

Preliminary analysis of parallel trends

To further support our DiD approach, as a preliminary analysis of parallel trends, Appendix 5 expands the time period in both directions to cover a total of 10 quarterly earnings periods ($t-5$, $t-4$, $t-3$, $t-2$, $t-1$, $t+1$, $t+2$, $t+3$, $t+4$, $t+5$) around the SPC estimation window, and evaluates the ERC or PEAD effect for the treatment and control samples separately over these periods. Panel A provides a graphical illustration of the hypothetical patterns based on the stimulation effect stated in H1a and H2a. Supporting the parallel trends assumption, the actual pattern in panel B is broadly consistent with the hypothetical pattern in panel A. It reveals that

7. We obtain stock returns, share prices, and numbers of shares from CRSP daily and monthly stock records. We collect firm-level accounting data from Compustat annual and quarterly information records. Analyst earnings forecast data and actual earnings are from the I/B/E/S detail and summary records. We collect institutional holding data from the 13F database. We calculate mutual fund flow redemption pressure using performance data from the CRSP Mutual Fund database and equity holding data from the Thomson-Reuters Mutual Fund ownership database. We identify corporate events using the Key Developments database from Capital IQ.

ERCs (PEAD) rise (drops) substantially after SPCs for the treatment sample but a similar change is not observed for the control sample, which remains relatively stable over time. Overall, we do not observe substantial variation in ERCs (PEAD) for the treatment and control samples across the pre-SPC periods, but we document an explicit increase (decrease) for the treatment sample immediately after the SPC estimation window.

Descriptive statistics

Table 1 presents the summary statistics for the variables used for our treatment effect test (pre-SPC EA^q_{t-2} to post-SPC EA^q_t), based on our treatment firms from 1984 through 2017. The post-SPC indicator variable *POST* has a mean value of 0.500 because the pre- and post-SPC earnings announcements each make up half of the total firm-quarter observations in the treatment sample. The untabulated correlation between *POST* and past returns (*MOM*) is significantly negative, consistent with our treatment firms experiencing weaker return performance during the 12-month rolling SPC estimation period. *POST* is significantly and positively correlated with analyst coverage (*ACOV*) as well as investor EDGAR access (*DRTPVACC* and *RPVACC*), suggesting higher levels of both analyst coverage and investor attention after SPC events.

TABLE 1
Summary statistics

	Obs.	Mean	SD	25%	Median	75%
$CAR_{(0,+1)}$	24,482	0.006	0.078	−0.035	0.005	0.048
$CAR_{(+2,+61)}$	24,482	0.016	0.158	−0.070	0.015	0.098
SUE	24,482	0.040	0.609	−0.015	0.041	0.153
POST	24,482	0.500	0.500	0.000	0.500	1.000
SUBSPC	24,482	0.321	0.467	0.000	0.000	1.000
BETA	24,482	1.095	0.485	0.762	1.047	1.372
SIZE	24,482	7.483	1.561	6.389	7.335	8.433
BM	24,482	0.456	0.280	0.255	0.396	0.591
MOM	24,482	0.105	0.454	−0.158	0.052	0.286
Q4	24,482	0.193	0.394	0.000	0.000	0.000
LOSS	24,482	0.138	0.345	0.000	0.000	0.000
EVOL	24,482	0.070	0.286	0.003	0.009	0.038
IVOL	24,482	0.065	0.064	0.024	0.044	0.083
ILLIQ	24,482	0.020	0.144	0.000	0.001	0.006
IO	24,482	0.590	0.342	0.400	0.682	0.854
BNEWS	24,482	0.260	0.439	0.000	0.000	1.000
ACOV	24,482	9.670	6.983	4.000	8.000	13.000
ABSUE	24,482	0.240	0.598	0.033	0.097	0.246
EXP	24,482	10.256	6.224	5.500	9.000	13.500
BSIZE	24,482	37.031	19.022	20.667	36.909	50.000
NSPE	24,482	0.359	0.480	0.000	0.000	1.000
DIVER	7,778	0.398	0.311	0.114	0.325	0.667
DRTPVACC	8,940	65.713	100.305	16.000	36.000	75.000
RPVACC	8,940	52.209	68.606	15.000	32.000	63.000
SIR	8,536	6.296	5.881	2.314	4.354	8.214
$\triangle$*ADACC*	22,828	−0.006	0.156	−0.035	−0.002	0.030
$\triangle$*CSCORE*	19,168	0.004	0.085	−0.058	−0.001	0.061

Notes: This table presents the summary statistics for variables used in the treatment effect test (EA^q_{t-2} to EA^q_t) based on the treatment sample over the period from 1984 through 2017. Appendix 2 provides detailed variable definitions. Appendix 3 describes the sample construction.

TABLE 2
Post-SPC changes in ERCs (tests of H1a and H1b)

Panel A: Regression analyses based on the treatment sample and the control sample

DV = $CAR_{(0,+1)}$	(1) Earlier placebo test (EA^q_{t-3} to EA^q_{t-2})		(2) Treatment effect test (EA^q_{t-2} to EA^q_t)		(3) Later placebo test (EA^q_t to EA^q_{t+1})	
	Treatment firms	Control firms	Treatment firms	Control firms	Treatment firms	Control firms
SUE	0.089***	0.083***	0.085***	0.083***	0.095***	0.088***
	(28.36)	(26.89)	(25.70)	(28.15)	(28.02)	(30.13)
POST	−0.000	−0.001	0.000	−0.002*	−0.003***	−0.000
	(−0.48)	(−1.36)	(0.55)	(−1.73)	(−3.23)	(−0.49)
POST×SUE	**−0.002**	**0.001**	**0.011*****	**0.001**	**0.004**	**−0.002**
	(−0.76)	**(0.48)**	**(3.65)**	**(0.48)**	**(1.24)**	**(−0.56)**
BETA	0.005**	0.002	0.003	0.004*	−0.001	0.001
	(2.19)	(0.76)	(1.07)	(1.81)	(−0.50)	(0.54)
BETA×SUE	0.020***	0.025***	0.015**	0.019***	0.015**	0.017**
	(2.80)	(3.85)	(2.16)	(3.15)	(2.05)	(2.53)
SIZE	−0.008	−0.005	−0.013*	−0.010	−0.025***	−0.017**
	(−1.07)	(−0.72)	(−1.78)	(−1.32)	(−3.40)	(−2.10)
SIZE×SUE	−0.015	−0.018	−0.001	−0.021	−0.020	−0.002
	(−0.80)	(−1.00)	(−0.05)	(−1.26)	(−1.02)	(−0.12)
BM	0.007**	0.011***	0.003	0.012***	0.004	0.007**
	(2.08)	(3.21)	(0.85)	(3.71)	(1.20)	(1.98)
BM×SUE	−0.022***	−0.037***	−0.029***	−0.034***	−0.032***	−0.026***
	(−2.71)	(−5.22)	(−3.68)	(−5.03)	(−3.94)	(−3.71)
MOM	−0.016***	−0.010***	−0.012***	−0.009***	−0.011***	−0.012***
	(−7.57)	(−4.92)	(−5.76)	(−4.33)	(−5.03)	(−5.54)
MOM×SUE	0.008	0.002	0.007	0.007	0.006	0.005
	(1.34)	(0.32)	(1.10)	(1.14)	(0.94)	(0.79)
SUBSPC	0.002*	0.004***	0.003***	0.003**	0.007***	0.002
	(1.71)	(3.08)	(2.90)	(2.42)	(5.33)	(1.45)
SUBSPC×SUE	0.011***	0.022***	0.012***	0.010**	0.015***	0.004
	(2.58)	(4.35)	(2.91)	(2.19)	(3.17)	(0.92)
Q4	0.004**	0.002	0.003**	0.003*	0.003**	0.003**
	(2.52)	(1.08)	(2.10)	(1.68)	(2.14)	(2.14)
Q4 × SUE	−0.027***	−0.020***	−0.020***	−0.027***	−0.015***	−0.027***
	(−6.08)	(−4.34)	(−4.59)	(−6.32)	(−3.02)	(−5.84)
LOSS	−0.006***	−0.006***	−0.006***	−0.009***	−0.009***	−0.009***
	(−2.69)	(−2.60)	(−2.88)	(−4.32)	(−3.86)	(−4.42)
LOSS×SUE	−0.019***	−0.031***	−0.030***	−0.021***	−0.029***	−0.023***
	(−3.24)	(−5.72)	(−5.52)	(−4.15)	(−5.33)	(−4.22)
EVOL	0.001	−0.001	0.003	−0.000	0.003	0.002
	(0.36)	(−0.38)	(1.19)	(−0.07)	(1.08)	(1.02)
EVOL×SUE	−0.010	−0.010	−0.006	−0.007	−0.008	−0.007
	(−1.40)	(−1.42)	(−0.82)	(−1.13)	(−1.10)	(−0.97)
IVOL	−0.007*	0.002	−0.002	0.003	0.002	0.004
	(−1.90)	(0.59)	(−0.69)	(1.02)	(0.71)	(1.17)
IVOL×SUE	0.016*	0.013	0.015*	0.020**	0.032***	0.023**
	(1.70)	(1.36)	(1.77)	(2.46)	(3.40)	(2.57)

(The table is continued on the next page.)

TABLE 2 (continued)

Panel A: Regression analyses based on the treatment sample and the control sample

DV = $CAR_{(0,+1)}$	(1) Earlier placebo test (EA^q_{t-3} to EA^q_{t-2}) Treatment firms	Control firms	(2) Treatment effect test (EA^q_{t-2} to EA^q_t) Treatment firms	Control firms	(3) Later placebo test (EA^q_t to EA^q_{t+1}) Treatment firms	Control firms
ILLIQ	0.016**	0.017***	0.015**	0.022***	0.012*	0.018***
	(2.49)	(2.77)	(2.48)	(3.64)	(1.91)	(2.78)
ILLIQ×*SUE*	0.007	0.017	0.026	0.009	0.008	0.034**
	(0.40)	(1.05)	(1.47)	(0.58)	(0.43)	(2.01)
IO	−0.002	0.003	−0.004	−0.001	−0.000	−0.005
	(−0.48)	(0.78)	(−0.92)	(−0.21)	(−0.05)	(−1.18)
IO×*SUE*	0.006	0.003	0.006	−0.003	0.005	−0.005
	(0.75)	(0.39)	(0.82)	(−0.49)	(0.63)	(−0.69)
Constant	0.025	−0.025	0.014	−0.015	−0.005	0.019***
	(0.87)	(−1.45)	(0.81)	(−0.31)	(−0.61)	(4.70)
Diff. in *POST*×*SUE*	F = 0.67		F = 5.35**		F = 1.51	
Firm, quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	24,482	24,482	24,482	24,482	24,482	24,482
Adjusted R^2	0.133	0.148	0.147	0.150	0.163	0.151

Panel B: Regression analyses based on the combined sample that includes treatment and control firms

DV = $CAR_{(0,+1)}$	(1) Earlier placebo test (EA^q_{t-3} to EA^q_{t-2})		(2) Treatment effect test (EA^q_{t-2} to EA^q_t)		(3) Later placebo test (EA^q_t to EA^q_{t+1})	
SUE	0.081***	(26.02)	0.084***	(27.62)	0.088***	(28.91)
POST	−0.001	(−1.14)	−0.001	(−1.37)	−0.000	(−0.37)
TREAT	−0.000	(−0.31)	−0.000	(−0.23)	0.001	(0.90)
POST×*SUE*	0.003	(1.08)	−0.000	(−0.06)	−0.003	(−0.88)
POST×*TREAT*	0.001	(0.53)	0.002	(1.15)	−0.002	(−1.56)
TREAT×*SUE*	0.006	(1.51)	−0.001	(−0.18)	0.005	(1.13)
POST*×*SUE*×*TREAT	**−0.005**	**(−1.02)**	**0.010****	**(1.99)**	**0.005**	**(0.96)**
Constant, Controls	Yes		Yes		Yes	
Quarter FE	Yes		Yes		Yes	
Matched-pair FE	Yes		Yes		Yes	
Obs.	48,964		48,964		48,964	
Adjusted R^2	0.122		0.132		0.131	

Notes: This table presents tests of H1a and H1b based on post-SPC changes in ERCs for the designated earnings announcements. Controls in panel B include the same control variables and their interactions with *SUE* as those in equation (2) and panel A. Appendix 2 provides variable definitions. Appendix 3 describes the sample construction. The sample covers the period from 1984 through 2017. All the independent variables except for dummy variables are the decile ranked values by quarter ranging from −0.5 through 0.5. The t-statistics in parentheses are adjusted for heteroskedasticity and clustered by firm. Bold indicates variables of interest. *, **, and *** represent two-tailed significance levels of 10%, 5%, and 1%, respectively.

4. Empirical findings

Hypothesis tests

Changes in the ERC (test of H1a and H1b)

To test H1a and H1b, Table 2 presents our baseline results for post-SPC changes in ERCs. Note that the dependent variable is $CAR_{(0,+1)}$. In panel A, for the treatment effect test, we conduct a DiD analysis by comparing post-SPC changes in ERCs from EA^q_{t-2} to EA^q_t for treatment firms with the same changes for control firms. The coefficient on SUE captures the magnitude of ERCs in the pre-SPC period, whereas the coefficient on $POST \times SUE$ captures the change in ERCs from the pre- to post-SPC period. As shown in column (2), we find that the coefficient on SUE is positive and significant with a similar magnitude for treatment firms (coef. = 0.085, t-stat = 25.70) and control firms (coef. = 0.083, t-stat = 28.15). This finding suggests no substantial difference in ERCs between the two samples in the pre-SPC period. In contrast, we find that the coefficient on $POST \times SUE$ is positive and significant for only the treatment firms (coef. = 0.011, t-stat = 3.65; coef. = 0.001, t-stat = 0.48 for the control firms), and the difference between the two samples is statistically significant (F-stat = 5.35). This finding shows that the magnitude of the change in ERCs from pre- to post-SPC differs significantly between the treatment and control samples. Panel B combines the treatment and control firms in the same regression and provides further supportive evidence. Specifically, the coefficient on the interaction term $POST \times SUE \times TREAT$ is positive and significant (coef. = 0.010, t-stat = 1.99) in column (2). In general, comparing the magnitude of the baseline pre-SPC ERC effect (captured by the coefficient on SUE) with the post-SPC change in the ERC effect (as reflected in the coefficient on $POST \times SUE$) for the treatment firms in panel A column (2), suggests that the increase in ERCs caused by SPCs is economically meaningful.[8]

In sum, the treatment effect test results shown in Table 2, column (2) provide baseline evidence that short-window market reactions to quarterly earnings announcements, captured by ERCs, are significantly greater post-SPC than pre-SPC. Stated another way, SPCs lead investors to react more strongly to treatment firms' earnings news (with no equivalent effect identified for the control firms), thereby enhancing market information efficiency with respect to firm-specific information contained in quarterly earnings announcements.

In addition to the treatment effect test discussed above, we also conduct two placebo tests for falsification. We use the same set of sample firms in the earlier period (EA^q_{t-3} to EA^q_{t-2}) in column (1) and in the later period (EA^q_t to EA^q_{t+1}) in column (3). The coefficients on $POST \times SUE$ in panel A and on $POST \times SUE \times TREAT$ in panel B are statistically insignificant consistently in columns (1) and (3). Together, the results of these placebo tests corroborate the view that the observed increase in ERCs in the post-SPC period observed in the main treatment effect test is unlikely to be driven by a time trend or other unidentified confounding factors.

Changes in PEAD (test of H2a and H2b)

Table 3 examines post-SPC changes in PEAD in order to test H2a and H2b. Note that the dependent variable is $CAR_{(+2,+61)}$. In panel A, for the treatment effect test, we conduct a DiD analysis by comparing post-SPC changes in PEAD from EA^q_{t-2} to EA^q_t for treatment firms with the same changes for control firms. As shown in column (2), we find that the coefficient on SUE is positive and significant with a similar magnitude for both treatment firms (coef. = 0.031, t-stat = 4.64) and control firms (coef. = 0.025, t-stat = 3.90). Given that the coefficient on SUE captures the

8. As shown in panel A, column (2), the coefficients on SUE and $POST \times SUE$ are 0.085 and 0.011 for treatment firms, respectively. This means that ERCs increase by 12.9% (= 0.011/0.085) from pre- to post-SPC.

TABLE 3
Post-SPC changes in PEAD (tests of H2a and H2b)

Panel A: Regression analyses based on the treatment sample and the control sample

	(1) Earlier placebo test (EA^q_{t-3} to EA^q_{t-2})		(2) Treatment effect test (EA^q_{t-2} to EA^q_t)		(3) Later placebo test (EA^q_t to EA^q_{t+1})	
DV = $CAR_{(+2,+61)}$	Treatment firms	Control firms	Treatment firms	Control firms	Treatment firms	Control firms
SUE	0.026***	0.024***	0.031***	0.025***	0.013**	0.029***
	(3.99)	(3.54)	(4.64)	(3.90)	(2.08)	(4.58)
POST	−0.007***	−0.010***	−0.004*	−0.004*	−0.004**	−0.000
	(−3.31)	(−4.84)	(−1.77)	(−1.92)	(−2.27)	(−0.27)
POST×SUE	**0.002**	**0.001**	**−0.018*** **	**0.006**	**0.010**	**−0.007**
	(0.32)	**(0.09)**	**(−2.63)**	**(0.80)**	**(1.47)**	**(−1.01)**
Diff. in *POST×SUE*	$F = 0.03$		$F = 5.67$**		$F = 2.93$*	
Constant, Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm and quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	24,482	24,482	24,482	24,482	24,482	24,482
Adjusted R^2	0.106	0.107	0.108	0.092	0.107	0.095

Panel B: Regression analyses based on the combined sample that includes treatment and control firms

DV = $CAR_{(+2,+61)}$	(1) Earlier placebo test (EA^q_{t-3} to EA^q_{t-2})		(2) Treatment effect test (EA^q_{t-2} to EA^q_t)		(3) Later placebo test (EA^q_t to EA^q_{t+1})	
SUE	0.034***	(4.93)	0.025***	(3.82)	0.038***	(5.68)
POST	−0.008***	(−3.75)	−0.002	(−0.83)	0.001	(0.60)
TREAT	0.004*	(1.66)	0.006**	(2.23)	0.001	(0.48)
POST×SUE	−0.006	(−0.86)	0.013*	(1.71)	−0.011	(−1.50)
POST×TREAT	0.004	(1.07)	−0.001	(−0.45)	−0.005	(−1.64)
TREAT×SUE	−0.003	(−0.32)	0.007	(0.79)	−0.021**	(−2.30)

(The table is continued on the next page.)

TABLE 3 (continued)

Panel B: Regression analyses based on the combined sample that includes treatment and control firms

DV = $CAR_{(+2,+61)}$	(1) Earlier placebo test (EA^q_{t-3} to EA^q_{t-2})		(2) Treatment effect test (EA^q_{t-2} to EA^q_t)		(3) Later placebo test (EA^q_t to EA^q_{t+1})	
POST×SUE×TREAT	**0.001**	**(0.12)**	**−0.035*****	**(−3.19)**	**0.020***	**(1.81)**
Constant, Controls	Yes		Yes		Yes	
Quarter FE	Yes		Yes		Yes	
Matched-pair FE	Yes		Yes		Yes	
Obs.	48,964		48,964		48,964	
Adjusted R^2	0.078		0.078		0.065	

Notes: This table presents tests of H2a and H2b based on post-SPC changes in PEAD for the designated earnings announcements. Controls include the same control variables and their interactions with *SUE* as those in equation (2) and Table 2. Appendix 2 provides variable definitions. Appendix 3 describes the sample construction. The sample covers the period from 1984 through 2017. All the independent variables except for dummy variables are the decile ranked values by quarter ranging from −0.5 through 0.5. The *t*-statistics in parentheses are adjusted for heteroskedasticity and clustered by firm. Bold indicates variables of interest. *, **, and *** represent two-tailed significance levels of 10%, 5%, and 1%, respectively.

magnitude of PEAD in the pre-SPC period, the above finding suggests no significant difference in PEAD between the treatment and control samples in the pre-SPC period. In contrast, the coefficient on *POST×SUE* is negative and significant for treatment firms (coef. = −0.018, *t*-stat = −2.63), but not for control firms (coef. = 0.006, *t*-stat = 0.80), and the difference in this coefficient between the two samples is statistically significant (*F*-stat = 5.67). Because the coefficient on *POST×SUE* captures the difference in PEAD from the pre- to post-SPC period, this finding shows that post-SPC changes in PEAD are significantly different between the treatment and control firms. Panel B uses the combined sample and provides additional supportive evidence because of the significantly negative coefficient on *POST×SUE×TREAT* (coef. = −0.035, *t*-stat = −3.19) in column (2). Overall, Table 3 also shows that the decrease in PEAD driven by SPCs is economically meaningful when we compare the magnitudes of the baseline pre-SPC PEAD effect (captured by the coefficient on *SUE*) with the post-SPC change in the PEAD effect (as reflected in the coefficient on *POST×SUE*) for the treatment firms in panel A column (2).[9]

Overall, the treatment effect test results reported in column (2) of Table 3 provide baseline evidence that PEAD is significantly lower in the post-SPC period than in the pre-SPC period. Therefore, SPCs lead investors to make price corrections with less delays in response to earnings news for treatment firms, to a greater degree, than for control firms. This contributes to market information efficiency with respect to firm-specific information contained in quarterly earnings announcements.

In addition to the treatment effect test discussed above, we conduct two placebo tests for falsification using the same set of sample firms in two different periods. We use the earlier period (EA^q_{t-3} to EA^q_{t-2}) in column (1) and the later period (EA^q_t to EA^q_{t+1}) in column (3). The coefficient on *POST×SUE* in panel A is consistently insignificant in columns (1) and (3), and the coefficient on *POST×SUE×TREAT* in panel B is statistically insignificant in column (1) and weakly significant (at the 10% level) with an unexpected positive sign in column (3). Together, the results of these placebo tests support the argument that the observed decrease in PEAD in the post-SPC period in our treatment effect test is less likely to be attributed to time trends or other unidentified confounding factors.

Additional analyses

Changes in analyst forecast activities

Table 4 presents results from additional tests for post-SPC changes in analyst coverage and forecast diversity. The primary objective of these additional tests is to further evaluate whether the extent of investor rational attention and opinion divergence changes around SPC events and, if so, how these changes differ for treatment versus control firms. Prior studies document that analyst coverage is positively associated with investor attention (O'Brien and Bhushan 1990), and that analyst forecast diversity is positively associated with investor opinion divergence (Barron et al. 1998; Doukas et al. 2006). For the treatment effect test, we conduct a DiD analysis using the combined sample of both treatment and control firms. Specifically, we compare post-SPC changes in analyst coverage and forecast diversity from EA^q_{t-2} to EA^q_t for treatment and control firms using the interaction term *POST×TREAT*. In addition, we conduct two placebo tests for falsification over EA^q_{t-3} to EA^q_{t-2} and EA^q_t to EA^q_{t+1}. Because analyst coverage and forecast diversity are not measured using variations in share prices, these measures of analyst activity provide a robustness check to address the concern that SPCs may affect ERCs or PEAD. This concern arises because of a broader underlying relation between these effects and stock returns.

In panel A, which reports results for analyst coverage (*ACOV*), the treatment effect test in column (2) shows that the coefficient on *POST×TREAT* is positive and statistically significant (coef. = 0.621, *t*-stat = 8.97). The earlier and later period placebo tests in columns (1) and (3) do

9. As shown in panel A, column (2), the coefficients on *SUE* and *POST×SUE* are 0.031 and −0.018 for treatment firms, respectively. This means that PEAD decreases by 58.1% (= 0.018/0.031) from pre- to post-SPC.

TABLE 4
Post-SPC changes in analyst coverage and forecast diversity (additional tests)

Panel A: Analyst coverage (analysis of investor attention)

DV = *ACOV*	(1) Earlier placebo test (EA^q_{t-3} to EA^q_{t-2})		(2) Treatment effect test (EA^q_{t-2} to EA^q_t)		(3) Later placebo test (EA^q_t to EA^q_{t+1})	
POST	0.162***	(4.73)	0.009	(0.19)	0.152***	(4.25)
TREAT	−0.208***	(−3.41)	−0.276***	(−4.32)	0.363***	(5.25)
POST×TREAT	**−0.072**	**(−1.41)**	**0.621*** **	**(8.97)**	**−0.337*** **	**(−6.14)**
SIZE	3.123***	(51.81)	3.241***	(51.58)	3.319***	(51.18)
BM	2.221***	(10.56)	2.228***	(11.23)	2.079***	(10.41)
Q4	0.580***	(6.30)	0.625***	(6.85)	0.656***	(6.80)
IO	0.882***	(4.23)	0.805***	(3.74)	0.820***	(3.68)
ABSUE	0.158***	(2.86)	0.159**	(2.51)	0.266***	(4.29)
BNEWS	−0.058	(−1.09)	−0.012	(−0.21)	−0.041	(−0.75)
EXP	−0.051***	(−7.11)	−0.068***	(−10.00)	−0.070***	(−10.28)
BSIZE	−0.033***	(−10.82)	−0.033***	(−10.47)	−0.030***	(−9.04)
NSPE	0.016	(0.21)	0.046	(0.59)	0.067	(0.89)
Constant	−11.295***	(−17.48)	−14.836***	(−15.88)	−13.787***	(−17.55)
Quarter FE	Yes		Yes		Yes	
Matched-pair FE	Yes		Yes		Yes	
Obs.	48,964		48,964		48,964	
Adjusted R^2	0.663		0.656		0.673	

Panel B: Analyst forecast diversity (analysis of opinion divergence)

DV = *DIVER*	(1) Earlier placebo test (EA^q_{t-3} to EA^q_{t-2})		(2) Treatment effect test (EA^q_{t-2} to EA^q_t)		(3) Later placebo test (EA^q_t to EA^q_{t+1})	
POST	0.002	(0.23)	0.001	(0.16)	0.002	(0.24)
TREAT	−0.016**	(−2.37)	−0.002	(−0.25)	0.011	(1.61)
POST×TREAT	**0.015**	**(1.40)**	**0.011**	**(1.09)**	**−0.006**	**(−0.57)**
SIZE	−0.007*	(−1.93)	−0.012***	(−3.22)	−0.014***	(−3.75)
BM	0.116***	(6.59)	0.130***	(7.40)	0.119***	(8.05)
Q4	0.024**	(2.54)	0.034***	(3.69)	0.039***	(3.96)
IO	−0.014	(−1.12)	−0.019	(−1.50)	−0.003	(−0.24)
ABSUE	−0.162***	(−7.44)	−0.188***	(−9.00)	−0.214***	(−14.93)
BNEWS	0.034***	(4.26)	0.032***	(4.26)	0.036***	(4.70)
EXP	−0.001*	(−1.86)	−0.001**	(−2.26)	−0.001*	(−1.74)
BSIZE	0.000	(1.02)	0.001**	(2.54)	0.001**	(2.26)
NSPE	0.020***	(2.89)	0.013**	(1.97)	0.012*	(1.82)
ACOV	0.006***	(9.91)	0.006***	(9.42)	0.006***	(10.43)
Constant	0.100	(0.97)	0.672***	(5.36)	0.656***	(3.42)
Quarter FE	Yes		Yes		Yes	
Matched-pair FE	Yes		Yes		Yes	
Obs.	15,556		15,556		15,556	
Adjusted R^2	0.142		0.151		0.173	

Notes: This table presents additional tests based on post-SPC changes in analyst coverage (panel A) and analyst forecast diversity (panel B) of the designated earnings announcements. Appendix 2 provides variable definitions. Appendix 3 describes the sample construction, with sample size reduction in panel B due to data availability of *DIVER*. The sample covers the period from 1984 through 2017. The *t*-statistics in parentheses are adjusted for heteroskedasticity and clustered by firm. Bold indicates variables of interest. *, **, and *** represent two-tailed significance levels of 10%, 5%, and 1%, respectively.

not yield similar patterns.[10] Collectively, these findings suggest that in the post-SPC period, the number of analysts following a firm significantly increases for the treatment firms relative to the control firms, and this provides supportive evidence of an increase in investor attention after SPCs. In panel B, which presents the findings for analyst forecast diversity (*DIVER*), the coefficient on *POST*×*TREAT* is statistically insignificant in the treatment effect test in column (2) as well as in the placebo tests in columns (1) and (3). Therefore, we observe no evidence of changes in investor opinion divergence after SPCs. In short, the findings in panels A and B of Table 4, taken together, lend further support to the investor attention explanation for the positive effect of SPCs on information efficiency, but do not support the opinion divergence explanation.

Changes in investor EDGAR access

Table 5 provides additional tests that evaluate whether investors' information gathering activities change around SPCs and, if so, how these changes differ between treatment and control firms. The objective of these additional tests is to provide more direct evidence in support of the investor attention explanation for our main findings. Our analyses focus on post-SPC changes in investor access to EDGAR around earnings announcement dates, based on the measures developed in Drake et al. (2015) and Ryans (2017, 2021).[11] We perform DiD analyses using the combined sample of both treatment and control firms for the treatment effect test in column (2), and placebo tests for the earlier period in column (1) and for the later period in column (3).

In panel A, the treatment effect test using Drake et al.'s (2015) measure as the dependent variable yields a positive and significant coefficient on *POST*×*TREAT* (coef. = 5.680, *t*-stat = 2.85). In sharp contrast, this coefficient is statistically insignificant in both the earlier and later period placebo tests. In panel B, the treatment effect test using Ryans' (2017, 2021) measure as the dependent variable also shows a positive and significant coefficient on *POST*×*TREAT* (coef. = 4.091, *t*-stat = 2.80). Again, this coefficient is insignificant in the earlier and later period placebo tests. These findings show a significant post-SPC increase in investor gathering of firm-specific information via online access to EDGAR.

10. In Table 4, panel A, the coefficient on *POST*×*TREAT* in the placebo tests is statistically insignificant in column (1) and significantly negative in column (3). To the extent that analyst coverage captures investor attention, the former indicates no changes in attention for treatment firms relative to control firms in the period prior to SPCs, and the latter implies significant reversal of attention for treatment firms relative to control firms over the subsequent period. However, evidence of a reversal in attention is specific to Table 4, because we do not observe a similar pattern in the later placebo tests reported in Table 5. These tests examine changes in attention using two separate measures of investor EDGAR access. In other words, evidence of a reversal of investor attention is mixed when we consider Tables 4 and 5 together. In contrast, our evidence of investor attention reversal is also broadly consistent with the existing literature, which supports the argument that attention is a selective process due to constraints in the learning and information gathering capacities of investors (Treisman 1960; Sims 2003). These studies suggest that constraints cause investors to be selective and prioritize their attention to more salient stimuli. For instance, Odean (1998) argues that selective attention among investors drives them to focus more on salient information such as extreme stock returns. Other studies suggest that selective attention causes investors to shift their focus over time due to the emergence of more salient information (Peng 2005; Peng and Xiong 2006). Regarding how long the attention triggered by SPCs would last and whether it is transitory or permanent, the aforementioned literature implies that this depends on the subsequent arrival of other salient information. We believe that these are interesting and important research questions that future studies could examine.
11. The EDGAR online system hosts all mandatory filings for US publicly listed firms and the SEC maintains a record of all users' activities on EDGAR. We measure investors' information gathering activities by their access to the firms' financial filings through EDGAR. The two measures based on Drake et al. (2015) and Ryans (2017, 2021) are calculated as the number of human page views on periodic accounting reports (10-K and 10-Q) on EDGAR during two trading days [0, +1] around the earnings announcement for each quarter. Please refer to Drake et al. (2015) and Ryans (2017, 2021) for the detailed discussion and calculation process for these two measures.

TABLE 5
Post-SPC changes in investor EDGAR access (additional tests)

Panel A: Drake et al.'s (2015) measure

DV = *DRTPVACC*	(1) Earlier placebo test (EA^q_{t-3} to EA^q_{t-2})		(2) Treatment effect test (EA^q_{t-2} to EA^q_t)		(3) Later placebo test (EA^q_t to EA^q_{t+1})	
POST	−1.114	(−1.32)	−2.300	(−1.38)	0.564	(0.43)
TREAT	0.197	(0.15)	0.338	(0.20)	5.734**	(2.49)
POST×TREAT	**1.104**	**(0.98)**	**5.680*****	**(2.85)**	**−0.904**	**(−0.48)**
SIZE	23.717***	(16.04)	31.884***	(15.39)	40.487***	(17.22)
BM	20.221***	(6.34)	33.628***	(7.48)	48.143***	(9.99)
Q4	−7.865***	(−3.60)	−16.084***	(−3.10)	−14.004***	(−3.88)
IO	−16.195***	(−5.81)	−19.190***	(−4.18)	−24.226***	(−5.60)
ABSUE	7.707***	(5.44)	10.344***	(3.52)	15.271***	(6.59)
BNEWS	4.025***	(3.13)	5.728***	(3.05)	5.862***	(3.37)
ACOV	0.706***	(2.71)	1.174***	(2.83)	0.911**	(2.54)
SIR	23.211	(1.64)	35.266	(1.13)	94.876***	(4.01)
Constant	−159.198***	(−13.94)	−227.219***	(−13.66)	−331.448***	(−15.88)
Quarter FE	Yes		Yes		Yes	
Matched-pair FE	Yes		Yes		Yes	
Obs.	17,880		17,880		17,880	
Adjusted R^2	0.539		0.456		0.553	

Panel B: Ryans' (2017, 2021) measure

DV = *RPVACC*	(1) Earlier placebo test (EA^q_{t-3} to EA^q_{t-2})		(2) Treatment effect test (EA^q_{t-2} to EA^q_t)		(3) Later placebo test (EA^q_t to EA^q_{t+1})	
POST	−0.197	(−0.31)	−1.169	(−1.01)	0.401	(0.47)
TREAT	0.075	(0.08)	−0.542	(−0.41)	2.868*	(1.91)
POST×TREAT	**0.406**	**(0.47)**	**4.091*****	**(2.80)**	**−0.091**	**(−0.08)**
Constant, Controls	Yes		Yes		Yes	
Quarter FE	Yes		Yes		Yes	
Matched-pair FE	Yes		Yes		Yes	
Obs.	17,880		17,880		17,880	
Adjusted R^2	0.566		0.469		0.596	

Notes: This table presents additional tests based on post-SPC changes in investor access of EDGAR of the designated earnings announcements. Controls in panel B are the same as those in panel A. Appendix 2 provides variable definitions. Appendix 3 describes the sample construction, with sample size reductions due to data availability of *DRTPVACC* and *RPVACC*. The sample covers the period from 2003 through 2016. The *t*-statistics in parentheses are adjusted for heteroskedasticity and clustered by firm. Bold indicates variables of interest. *, **, and *** represent two-tailed significance levels of 10%, 5%, and 1%, respectively.

Robustness tests

Controlling for changes in financial reporting quality

Table 6 reports results from our robustness tests on post-SPC changes in ERCs and PEAD after controlling for concurrent changes in firm-specific financial reporting quality that could occur after treatment firms experience SPCs. Evidence shows that future SPC risk is higher for firms with greater accounting opacity as captured by absolute discretionary accruals (Hutton

TABLE 6
Controlling for changes in financial reporting quality (robustness tests)

Panel A: Controlling for concurrent changes in absolute value of discretionary accruals

	ERC DV = $CAR_{(0,+1)}$		PEAD DV = $CAR_{(2,+61)}$	
SUE	0.082***	(19.04)	0.028***	(2.94)
POST	−0.000	(−0.26)	0.001	(0.34)
TREAT	0.001	(0.73)	0.003	(0.67)
POST×*SUE*	0.001	(0.18)	0.008	(1.09)
POST×*TREAT*	0.001	(0.39)	−0.002	(−0.78)
TREAT×*SUE*	0.008	(1.46)	0.010	(0.75)
POST*×*SUE*×*TREAT	**0.012****	**(2.27)**	**−0.034*****	**(−3.11)**
△*ADACC*	−0.000	(−0.22)	0.003	(0.80)
△*ADACC*×*SUE*	0.004	(0.75)	−0.010	(−0.75)
△*ADACC*×*TREAT*	0.001	(0.36)	0.007	(1.26)
△*ADACC*×*SUE*×*TREAT*	**−0.011**	**(−1.38)**	**0.006**	**(0.31)**
Constant, Controls, FE	Yes		Yes	
Obs.	45,656		45,656	
Adjusted R^2	0.146		0.074	

Panel B: Controlling for concurrent changes in accounting conservatism

	ERC DV = $CAR_{(0,+1)}$		PEAD DV = $CAR_{(2,+61)}$	
SUE	0.080***	(23.06)	0.020***	(2.89)
POST	−0.001	(−1.23)	−0.001	(−0.26)
TREAT	0.001	(0.57)	0.007**	(2.52)
POST×*SUE*	−0.002	(−0.44)	0.003	(0.33)
POST×*TREAT*	0.002	(1.34)	−0.003	(−0.91)
TREAT×*SUE*	−0.000	(−0.02)	0.018*	(1.79)
POST*×*SUE*×*TREAT	**0.011****	**(2.02)**	**−0.024****	**(−2.13)**
△*CSCORE*	−0.008***	(−4.10)	−0.031***	(−6.81)
△*CSCORE*×*SUE*	−0.004	(−0.65)	−0.008	(−0.60)
△*CSCORE*×*TREAT*	−0.002	(−0.66)	0.002	(0.27)
△*CSCORE*×*SUE*×*TREAT*	**0.003**	**(0.32)**	**−0.004**	**(−0.18)**
Constant, Controls, FE	Yes		Yes	
Obs.	38,336		38,336	
Adjusted R^2	0.140		0.090	

Notes: This table presents robustness tests based on post-SPC changes in ERCs and PEAD after controlling for changes in financial reporting quality (FRQ) for the treatment effect test. Panels A and B present results when FRQ is proxied by the absolute value of discretionary accruals (*ADACC*) and accounting conservatism (*CSCORE*), respectively. For each earnings announcement made for fiscal quarter q, △*ADACC* (△*CSCORE*) is measured as the difference between the *ADACC* (*CSCORE*) for the same year as the fiscal quarter q and the *ADACC* (*CSCORE*) two years earlier (skipping the year when SPCs occur). Appendix 2 provides variable definitions. Appendix 3 describes the sample construction, with sample size reductions due to availability of FRQ measures. The sample covers the period from 1984 through 2017. Controls include the same control variables and their interactions with *SUE* as those in equation (2), and the quarter and matched-pair fixed effects (FE) are included. All the independent variables except for dummy variables are the decile ranked values by quarter ranging from −0.5 through 0.5. The t-statistics in parentheses are adjusted for heteroskedasticity and clustered by firm. Bold indicates variables of interest. *, **, and *** represent two-tailed significance levels of 10%, 5%, and 1%, respectively.

et al. 2009) and for firms with less conservative accounting (Kim and Zhang 2016). We therefore use absolute discretionary accruals (*ADACC*) and accounting conservatism (*CSCORE*) to measure financial reporting quality (Armstrong et al. 2010; Dechow et al. 2010).

In panels A and B, we control for changes in financial reporting quality ($\triangle ADACC$ or $\triangle CSCORE$) from before to after the SPC estimation period. In panel A, the coefficient on $POST \times SUE \times TREAT$ is significantly positive (coef. = 0.012, *t*-stat = 2.27) for the ERC analysis and significantly negative (coef. = −0.034, *t*-stat = −3.11) for the PEAD analysis, whereas the coefficient on $\triangle ADACC \times SUE \times TREAT$ is statistically insignificant in both cases. In panel B, the coefficient on $POST \times SUE \times TREAT$ is significantly positive (coef. = 0.011, *t*-stat = 2.02) for the ERC analysis and significantly negative (coef. = −0.024, *t*-stat = −2.13) for the PEAD analysis, whereas the coefficient on $\triangle CSCORE \times SUE \times TREAT$ in both cases is statistically insignificant. Thus, our main inferences remain unaltered even after controlling for concurrent changes in financial reporting quality that are possibly induced by SPCs.

Controlling for major corporate events

Table 7 presents robustness tests that control for the occurrence of major corporate events that could also trigger SPCs. We use data from the Capital IQ Key Developments database, which covers more than 100 types of corporate events from 2002 onwards. We classify these events into 11 categories, including: corporate guidance, conferences or calls, product and business expansions, buybacks, mergers and acquisitions, executive changes, dividends, litigations, divestitures and downsizing, security offerings, and other corporate events. If any day of the specific week in which an SPC occurs falls within the two-day window [0, +1] around a corporate event date, we consider this event to be associated with the SPC.

We next construct a set of 11 dummy variables (*Events*) that represent each of the categories of corporate events. A dummy variable is coded one if at least one relevant event occurred within the 12-month period prior to the earnings announcement, and zero otherwise. Table 7, columns (1) and (4) include the set of controls for firm characteristics from equation (2) and their interactions with *SUE*. Columns (2) and (5) include the event dummy variables as controls. Columns (3) and (6) also include interactions of these dummy variables with *SUE*. In all cases, the coefficients on $POST \times SUE \times TREAT$ remain significantly positive for the ERC analyses across columns (1) to (3) and significantly negative for the PEAD analyses across columns (4) to (6). Therefore, the results from this set of robustness tests provide supportive evidence that our main findings are robust to controlling for corporate events.

Mutual fund flow redemption pressure

Table 8 provides further evidence on the causality of our main findings from robustness tests that draw on mutual fund flow redemption pressure based on hypothetical sales as an exogenous shock to SPCs. This cross-sectional test uses a larger sample of 176,718 observations. To this end, we adopt the approach from Edmans et al. (2012), Dessaint et al. (2019), and Bennett et al. (2020). The literature suggests that mutual fund redemption can generate downward pressure on the price of stocks held by these funds (Coval and Stafford 2007; Ben-Rephael et al. 2011). To identify mutual fund flow pressure that could drive stock price downward but is unrelated to firm fundamentals, Edmans et al. (2012) suggest using a stock's hypothetical sales by mutual funds that experience outflows of at least 5% of the fund's total assets.[12] The existing

12. This measure is projected from a mutual fund's previously disclosed portfolio rather than actual sales, and it captures the reduction of a fund's position that is mechanically caused by investor outflows from the fund. These outflows are exogenous to an individual firm held by the fund because investor opinions on firm fundamentals would more likely drive direct trading in that firm's stock instead of a mutual fund share. Edmans et al. (2012) argue that this measure can support causality of the impact of stock price on a firm because it satisfies the exclusion restriction (i.e., is correlated with price movements but not with firm fundamentals) and has no effect on firms other than through its influence on stock price.

TABLE 7
Controlling for major corporate events (robustness tests)

	ERC DV = $CAR_{(0,+1)}$			PEAD DV = $CAR_{(+2,+61)}$		
	(1)	(2)	(3)	(4)	(5)	(6)
SUE	0.095***	0.095***	0.067***	0.014*	0.013*	0.031**
	(23.63)	(23.57)	(9.91)	(1.74)	(1.68)	(1.99)
POST	−0.002	−0.002	−0.002	−0.003	−0.003	−0.003
	(−1.47)	(−1.45)	(−1.54)	(−1.02)	(−1.07)	(−1.04)
TREAT	0.001	0.001	0.001	0.001	0.001	0.001
	(0.75)	(0.72)	(0.68)	(0.42)	(0.44)	(0.46)
POST×*SUE*	−0.006	−0.006	−0.010**	0.003	0.003	0.004
	(−1.23)	(−1.24)	(−2.13)	(0.28)	(0.30)	(0.45)
POST×*TREAT*	0.003*	0.003*	0.004*	0.001	0.001	0.001
	(1.79)	(1.79)	(1.87)	(0.15)	(0.16)	(0.14)
TREAT×*SUE*	0.004	0.004	0.005	0.012	0.012	0.012
	(0.75)	(0.75)	(0.85)	(1.00)	(1.02)	(1.05)
POST*×*SUE*×*TREAT	**0.017****	**0.017****	**0.016****	**−0.030****	**−0.031****	**−0.030****
	(2.51)	**(2.51)**	**(2.37)**	**(−2.24)**	**(−2.28)**	**(−2.26)**
Events	No	Yes	Yes	No	Yes	Yes
Events×*SUE*	No	No	Yes	No	No	Yes
Constant, Controls, FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	25,116	25,116	25,116	25,116	25,116	25,116
Adjusted R^2	0.168	0.167	0.171	0.092	0.092	0.093

Notes: This table presents robustness tests based on post-SPC changes in ERCs and PEAD after controlling for major corporate events for the treatment effect test. These corporate events are identified from the Capital IQ Key Developments database based on their association with SPCs, and include the following categories: (i) corporate guidance, (ii) conferences or calls, (iii) product and business expansions, (iv) buybacks, (v) mergers and acquisitions, (vi) executive changes, (vii) dividends, (viii) litigations, (ix) divestitures and downsizing, (x) security offerings, and (xi) other corporate events. A corporate event is considered to be associated with an SPC if any day of the crash week falls within two-day windows [0, +1] of the corporate event date. *Events* is a set of 11 dummy variables that each represents one of the categories of corporate events, and the dummy variable is equal to one if at least one relevant event associated with SPCs occurred during the 12-month period ending prior to the month in which earnings are released, and zero otherwise. Appendix 3 describes the sample construction, with sample size reductions due to data availability of Capital IQ Key Developments. The sample covers the period from 2002 through 2017. Controls include the same control variables and their interactions with *SUE* as those in equation (2), and the quarter and matched-pair fixed effects (FE) are included. Appendix 2 provides variable definitions. All the independent variables except for dummy variables are the decile ranked values by quarter ranging from −0.5 through 0.5. The *t*-statistics in parentheses are adjusted for heteroskedasticity and clustered by firm. Bold indicates variables of interest. *, **, and *** represent two-tailed significance levels of 10%, 5%, and 1%, respectively.

studies use this measure as an exogenous shock to evaluate the impact of stock price movements on takeover probability (Edmans et al. 2012), management forecasts (Zuo 2016), corporate investment (Dessaint et al. 2019), and firm productivity (Bennett et al. 2020).[13]

In our case, we assume that although mutual fund flow redemption pressure based on hypothetical sales could trigger SPCs, it is unlikely to influence ERCs and PEAD other than through its influence on SPCs. This is because ERCs and PEAD rely on two components: (i) the investor reactions to earnings announcements and (ii) the information content of these announcements. In terms of investor reactions to earnings announcements, prior literature argues that investors are more likely to express their views on firm fundamentals directly through trading the firm's stock than indirectly through mutual fund share. Therefore, if we observe that mutual fund flow redemption pressure measured through hypothetical sales is positively related to SPCs as we expect, then any observable relation between this pressure and ERCs and PEAD is likely to be driven exogenously by SPCs.

Panel A estimates a logistic regression with *CRASH* (a dummy variable for SPCs) as the dependent variable. We observe that mutual fund flow redemption pressure (*AMFFLOW*) indeed has a significantly positive relation with SPCs. The coefficient on *AMFFLOW* is significantly positive (coef. = 4.615, z-stat = 5.70) even when all control variables are included. This confirms that exogenously arising mutual fund flow pressure drives SPCs. In panel B, we find that this pressure is positively associated with ERCs and negatively associated with PEAD. Specifically, the coefficient on *AMFFLOW*×*SUE* is significantly positive (coef. = 0.024, t-stat = 8.28) in the ERC test and significantly negative (coef. = −0.015, t-stat = −2.35) in the PEAD test when all control variables are included.

Panel C uses this measure as an instrument variable (IV) in a two-stage least squares (2SLS) regression approach. In the first-stage probit model, *CRASH* is the dependent variable. The coefficient on *AMFFLOW* is significantly positive (coef. = 0.100, z-stat = 4.05), suggesting that mutual fund flow redemption pressure indeed drives SPCs.[14] In the second stage, we find that the

13. Some studies of mutual fund flow redemption pressure suggest that it may reduce the informativeness of stock price (Zuo 2016; Bennett et al. 2020). To this extent, price movements like SPCs that are driven by this pressure might be an uncertainty shock, but may not convey information to investors. However, the investor attention literature (Banker et al. 1993; Kandel and Stambaugh 1996; Pastor and Veronesi 2009; Case 2012) suggests that uncertainty would strengthen the attention of investors, increasing their learning and information gathering. Thus, the stimulation of market information efficiency by SPCs is likely to be driven by increased investor attention following these uncertainty shocks rather than by the information conveyed through price movements. In other words, although the informativeness of stock price may deteriorate subsequent to SPCs because of mutual fund flow redemption pressure, the uncertainty shock could incentivize investors to gather more information from alternative sources, as suggested by our findings of increased analyst coverage and investor EDGAR access. Thus, it is the overall increase in investor attention, learning, and information gathering that jointly contribute to the stimulation in market information efficiency with respect to earnings news. Other studies (Ben-Rephael et al. 2017; Andrei et al. 2020; Hirshleifer and Sheng 2021) also suggest that stronger investor attention can increase ERCs and decrease PEAD so our study therefore provides complementary evidence by focusing on the impact of SPCs.

14. In Table 8, the magnitude of the coefficient on *AMFFLOW* is different between panels A and C due to two main differences in the test specification. First, panel A examines whether *AMFFLOW* determines the occurrence of SPCs and applies a stand-alone logistic regression analysis following the extant literature on the determinants of SPCs (Kim et al. 2011b). In contrast, panel C applies *AMFFLOW* as an IV in 2SLS regression and uses a probit model in the first stage following the extant literature (Li et al. 2020). Second, in panel A, *AMFFLOW* is not transformed into a decile ranked variable to be consistent with the control variables, which are determinants of SPCs based on Jang and Kang (2019). However, in panel C, it is decile ranked to be consistent with the control variables, which are required under the IV/2SLS approach to be those from the second stage regression and as such are based on equation (2). Despite these differences, panel A and the first stage regression in panel C yield consistent inferences on the relation between *AMFFLOW* and SPCs.

coefficient on the fitted value of *CRASH*×*SUE* (i.e., *Fitted CRASH*×*SUE*) is significantly positive (coef. = 0.174, *t*-stat = 8.07) in the ERC test and significantly negative (coef. = −0.093, *t*-stat = −1.99) in the PEAD test. These findings suggest that SPCs, exogenously driven by mutual fund flow pressure based on hypothetical sales, drive the subsequent

TABLE 8
Mutual fund flow redemption pressure as an exogenous shock (robustness tests)

Panel A: Mutual fund flow redemption pressure and SPCs

DV = *CRASH*				
AMFFLOW	**4.958*****	**(6.10)**	**4.615*****	**(5.70)**
DTURN			0.609***	(5.64)
NCSKEWLAG			0.061***	(5.41)
RETLAG			1.798***	(8.02)
SIGMA			11.084***	(6.53)
SIZEPSM			0.079***	(9.29)
RM			2.180	(1.15)
SALESG			0.211***	(6.03)
AGE			−0.006***	(−7.06)
TANG			−0.130***	(−3.76)
Constant	−1.004***	(−2.77)	−1.703***	(−5.01)
Quarter and industry FE	Yes		Yes	
Obs.	176,718		176,718	
Pseudo R^2	0.0256		0.0308	

Panel B: Mutual fund flow redemption pressure, ERCs, and PEAD

	ERC		PEAD	
	DV = $CAR_{(0,+1)}$		DV = $CAR_{(+2,+61)}$	
SUE	0.070***	0.076***	0.031***	0.036***
	(75.25)	(67.10)	(18.64)	(17.15)
AMFFLOW	−0.001	−0.001	−0.011***	−0.003
	(−0.68)	(−1.39)	(−4.16)	(−1.28)
AMFFLOW*×*SUE	**0.025*****	**0.024*****	**−0.021*****	**−0.015****
	(8.63)	**(8.28)**	**(−3.29)**	**(−2.35)**
Controls	No	Yes	No	Yes
Constant, FE	Yes	Yes	Yes	Yes
Obs.	176,718	176,718	176,718	176,718
Adjusted R^2	0.107	0.116	0.072	0.094

Panel C: 2SLS approach using mutual fund flow redemption pressure as an IV

	First stage		Second stage			
			ERC		PEAD	
	DV = *CRASH*		DV = $CAR_{(0,+1)}$		DV = $CAR_{(2,+61)}$	
AMFFLOW	**0.100*****	**(4.05)**				
AMFFLOW×*SUE*	−0.084*	(−1.70)				
Fitted CRASH			−0.001	(−0.16)	0.075***	(4.24)

(The table is continued on the next page.)

TABLE 8 (continued)

Panel C: 2SLS approach using mutual fund flow redemption pressure as an IV

	First stage		Second stage			
	DV = *CRASH*		ERC DV = $CAR_{(0,+1)}$		PEAD DV = $CAR_{(2,+61)}$	
Fitted CRASH×SUE			**0.174*** **	**(8.07)**	**−0.093** **	**(−1.99)**
SUE	−0.016	(−0.88)	0.076***	(67.18)	0.036***	(17.07)
Constant, Controls, FE	Yes		Yes		Yes	
Obs.	176,718		176,718		176,718	
Pseudo/Adjusted R^2	0.096		0.116		0.094	

Notes: This table presents the robustness tests that draw on mutual fund flow redemption pressure based on hypothetical sales (*AMFFLOW*) as an exogenous shock to SPCs from the cross-sectional analysis of a larger sample between 1984 and 2017. Panel A presents the logistic regression results which show the relation between *AMFFLOW* and SPCs. Panel B shows the relation between *AMFFLOW* and ERCs and PEAD, respectively. Panel C presents the results that use *AMFFLOW* as the instrument variable based on a 2SLS approach. In panel C, *Fitted CRASH* is the fitted value from the first-stage probit regression. Since the endogenous variable *CRASH* is in the interaction term, following Wooldridge (2002), we further use *AMFFLOW* and its interactions with *SUE* along with other control variables to estimate the fitted value of *CRASH×SUE* (i.e., *Fitted CRASH×SUE)*. In panels B and C, controls include the same control variables and their interactions with *SUE* as those in equation (2), and the firm and quarter fixed effects (FE) are included. Appendix 2 provides variable definitions. In panels B and C, all the independent variables except for dummy variables are the decile ranked values by quarter ranging from −0.5 through 0.5. The *z*-statistics in parentheses are clustered by firm in panel A and in the first-stage regression of panel C. The *t*-statistics in parentheses are adjusted for heteroskedasticity and clustered by firm in panel B and in the second-stage regression of panel C. Bold indicates variables of interest. *, **, and *** represent two-tailed significance levels of 10%, 5%, and 1%, respectively.

increase in ERCs and decrease in PEAD, consistent with the view that SPCs stimulate market information efficiency. To this extent, this further evidence supports the causality of our main findings.

Controlling for SPJs

Table 9 presents our further robustness tests that consider the effect of SPJs, which are extreme price movements in the opposite direction to SPCs. The objective of these tests is to evaluate whether SPJs also influence ERCs and PEAD and, if so, how this compares with the impact of SPCs. Panel A applies our DiD approach and compares changes in treatment firms' ERCs or PEAD from the pre- to post-SPJ period with equivalent changes for the control firms. Panel B uses a larger cross-sectional sample (obs. = 206,253) to compare SPCs and SPJs together in the same regression analysis.

In terms of the ERC effect, panel A shows that the coefficient on *POST×SUE* is significantly positive for the treatment sample (coef. = 0.007, *t*-stat = 2.27) and is insignificant for the control sample (0.003, *t*-stat = 1.07). However, the difference between the two samples is not statistically significant (*F*-stat = 0.77). In contrast to the DiD approach in Table 2 where we observe a significantly positive impact of SPCs on ERCs, the analysis here shows only mixed evidence that SPJs influence ERCs. In panel B, we provide evidence in column (3) that SPJs are also associated with a significantly positive effect on ERCs. However, the coefficient on *JUMP×SUE* is only 0.005 (*t*-stat = 2.91), which is about one

TABLE 9
The impact of SPCs or SPJs on subsequent ERCs and PEAD (robustness tests)

Panel A: Regression analyses based on the PSM sample, separately, for treatment firms and control firms

	ERC DV = $CAR_{(0,+1)}$		PEAD DV = $CAR_{(+2,+61)}$	
	Treatment firms	Control firms	Treatment firms	Control firms
SUE	0.087***	0.082***	0.026***	0.028***
	(26.76)	(26.29)	(4.39)	(4.32)
POST	0.001	−0.002*	0.005**	−0.001
	(1.33)	(−1.81)	(2.39)	(−0.39)
POST×SUE	**0.007****	**0.003**	**0.003**	**−0.009**
	(2.27)	**(1.07)**	**(0.47)**	**(−1.24)**
Diff. in *POST×SUE*	$F = 0.77$		$F = 1.54$	
Constant, Controls, FE	Yes	Yes	Yes	Yes
Obs.	22,194	22,194	22,194	22,194
Adjusted R^2	0.163	0.152	0.102	0.108

Panel B: Regression analyses based on a larger sample

	ERC DV = $CAR_{(0,+1)}$			PEAD DV = $CAR_{(+2,+61)}$		
	(1)	(2)	(3)	(4)	(5)	(6)
SUE	0.075***	0.079***	0.072***	0.041***	0.041***	0.043***
	(68.57)	(71.21)	(63.49)	(18.50)	(18.54)	(17.56)
CRASH	0.002***		0.002***	0.003**		0.002*
	(3.92)		(3.94)	(2.14)		(1.93)
CRASH×SUE	**0.016*****		**0.016*****	**−0.014*****		**−0.014*****
	(9.99)		**(10.16)**	**(−3.64)**		**(−3.66)**
JUMP		−0.000	−0.000		−0.003**	−0.002*
		(−0.57)	(−0.14)		(−2.12)	(−1.78)
JUMP×SUE		**0.002**	**0.005*****		**−0.002**	**−0.004**
		(1.59)	**(2.91)**		**(−0.43)**	**(−0.93)**
Constant, Controls, FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	206,253	206,253	206,253	206,253	206,253	206,253
Adjusted R^2	0.111	0.110	0.111	0.098	0.098	0.098

Notes: This table presents the robustness tests that compare the effects of SPCs on ERCs and PEAD with the effects of SPJs on ERCs and PEAD. Panel A is based on the PSM sample and panel B is based on a larger cross-sectional sample. The sample period spans from 1984 through 2017. The PSM sample follows the steps in Appendix 3 for the construction for the DiD analysis of the treatment effect test by replacing SPCs with SPJs. Appendix 2 provides variable definitions. Controls include the same control variables (replacing *SUBSPC* with *SUBSPJ*) and their interactions with *SUE* as those in equation (2), and the firm and quarter fixed effects are included. All the independent variables except for dummy variables are the decile ranked values by quarter ranging from −0.5 through 0.5. The *t*-statistics in parentheses are adjusted for heteroskedasticity and clustered by firm. Bold indicates variables of interest. *, **, and *** represent two-tailed significance levels of 10%, 5%, and 1%, respectively.

third of that on $CRASH{\times}SUE$ (coef. = 0.016, t-stat = 10.16). Regarding the PEAD effect, throughout panels A and B, we observe no significant impact of SPJs. For instance, in panel B column (6), SPJs have an insignificant effect, that is, the coefficient on $JUMP{\times}SUE$ is −0.004 (t-stat = −0.93), whereas SPCs have a significantly negative effect, that is, the coefficient on $CRASH{\times}SUE$ is −0.014 (t-stat = −3.66). Overall, the ERC analysis, but not the PEAD analysis, provides evidence that SPCs and SPJs can both stimulate market informational efficiency, although the former effect is far greater than the latter effect.[15]

5. Conclusion

Unlike prior research, our study examines the consequences of SPCs by focusing on their impact on firm-level information efficiency. We find that the stimulation effect of SPCs on information efficiency dominates the distortion effect, and that this effect can be attributed to investor rational attention following these events. Specifically, we show that incrementally higher ERCs and lower PEAD follow SPCs, consistent with the information content of earnings announcements after SPCs being incorporated more fully and quickly into security prices. Additional analyses based on analyst coverage and investor EDGAR access provide more direct and supportive evidence that SPCs trigger investor attention. Further robustness tests reveal that our main findings are not driven by concurrent changes in financial reporting quality or confounding effects associated with the occurrence of major corporate events that are likely to trigger SPCs. We also demonstrate the causal effect through the use of mutual fund flow redemption pressure based on hypothetical sales as an exogenous shock to SPCs, and show that SPC effects are much stronger than SPJ effects.

Given the scarcity of empirical evidence on the informational consequences of SPCs, we recommend further research on the consequences of this phenomenon. Some interesting research questions that stem from our study, albeit not within its scope, are as follows. First, SPCs may bring about changes in various aspects of firm behavior. The existing studies provide evidence that the information environment can induce real effects associated with firms' governance, investment, and financing activities (Roychowdhury et al. 2019; Core 2020). To this extent, whether changes in market information efficiency after SPCs can drive changes in other corporate policies is an interesting issue that warrants further research. Second, SPCs may provoke firms to cater to information demands as a result of increased investor attention. The existing literature documents various internal and external mechanisms that drive firms' disclosure incentives (Beyer et al. 2010; McVay and Szerwo 2021). As such, further research could examine whether and how disclosure incentives of firms might change following the surge of attention among investors after SPCs.

15. This is generally consistent with the existing literature, which suggests that investors tend to pay more attention to bad news than good news (Pratto and John 1991; Baumeister et al. 2001) and that managers tend to withhold bad news (Kothari et al. 2009; Bao et al. 2019). Experimental studies on loss aversion show that people care more strongly about a loss in utility than about a gain of equal magnitude (Kahneman and Tversky 1979; Tversky and Kahneman 1991). Other studies also show that asset prices tend to react more to bad news in the presence of uncertainty (Veronesi 1999; Leippold et al. 2008). Together, the implications of these studies are that, although both SPCs and SPJs are expected to trigger investor attention, the impact of SPCs on market informational efficiency is likely to be stronger than the impact of SPJs.

Appendix 1: Empirical studies on firm-specific SPC risk

Panel A: Journal distribution		**Panel B:** Topic distribution			**Panel C:** Year distribution		
		Topic	Published papers	Working papers	Year	Published papers	Working papers
Financial Times 50 journals	38	Determinants	165	75	2020	47	14
Accounting Review	6	Governance or control	43	14	2019	41	28
Contemporary Accounting Research	8	Accounting information	24	15	2018	19	13
Journal of Accounting & Economics	1	Other information	14	19	2017	19	8
Journal of Accounting Research	2	Short-selling or trading	13	2	2016	21	4
Journal of Business Ethics	5	Managerial compensation	10	1	2015	9	1
Journal of Finance	1	Institutional investor	10	1	2014	5	3
Journal of Financial and Quantative Analysis	3	Culture or religion	9	0	2013	4	1
Journal of Financial Economics	7	Corporate finance	7	8	2012	0	1
Journal of International Business Studies	1	Regulation or standards	7	3	2011	2	1
Management Science	1	Corporate social responsibility	6	0	2010	0	2
Review of Accounting Studies	2	Political influence	6	0	2009	2	0
Review of Finance	1	Investment or strategy	5	4	2008	1	0
		Auditing	5	3	2007	0	0
Other journals	134	Analyst	4	3	2006	1	0
		Competition	2	2	2005	0	0
Working papers	76				2004	0	0
		Others			2003	0	0
Total	248	Information asymmetry effect	4	0	2002	0	0
		Stock return effect	1	0	2001	1	0
		Intra-day liquidity effect	1	0			
		Literature review	1	0			
		Information disclosure effect	0	1	Total	172	76
		Total	172	76			

Notes: This table presents the journal, topic, and year distributions of empirical studies on firm-specific SPC risk. It includes studies published in accounting and finance journals and unpublished working papers on the Social Science Research Network (SSRN) over the period 2001–2020. Panel A lists journals in alphabetical order. Panel B ranks topics by number of published papers.

Appendix 2: Variable definitions

Variable	Definition
ABSUE	Absolute value of standardized unexpected earnings (*SUE*)
ACOV	Analyst coverage, measured as the number of analysts following the firm for each quarter
ADACC	Absolute value of discretionary accruals, which is derived from the modified Jones model (Dechow et al. 1995) estimated within the same Fama and French 49-industry annually
AGE	The number of years since the firm's first appearance on CRSP monthly stock file before the SPC estimation window
AMFFLOW	Mutual fund flow redemption pressure, a measure taken from Edmans et al. (2012). Appendix C of Dessaint et al. (2019) provides the calculation process for this quarterly measure. We use the absolute value of the average quarterly redemption pressure over the 12-month SPC estimation window. A higher value of *AMFFLOW* indicates higher redemption pressure
BETA	Market beta from regressing daily returns on market excess returns over the past 12 months prior to earnings announcement for each quarter
BM	Book value over market value of equity at the end of each quarter
BNEWS	Bad news indicator equal to one if standardized unexpected earnings (*SUE*) is negative, and zero otherwise
BSIZE	Median size of the brokerage houses employing analysts following the firm for each quarter. The brokerage house size is the number of analysts employed by the brokerage house
$CAR_{(0,+1)}$	Cumulative abnormal returns, adjusted for CRSP value-weighted market returns for the two-trading-day event window starting from the earnings announcement date for each quarter
$CAR_{(+2,+61)}$	Cumulative abnormal returns, adjusted for CRSP value-weighted market returns for the 60-trading-day drift window starting from day two after the earnings announcement for each quarter
CSCORE	Accounting conservatism measure calculated following Khan and Watts (2009)
DIVER	Analyst forecast diversity, a measure taken from Barron et al. (1998). They define dispersion as $V \times (1-\rho)$, where V is information uncertainty and $(1-\rho)$ is diversity. In order to keep our opinion divergence measure free from the confounding effects of uncertainty in analyst forecasts, we use the diversity to proxy for investor opinion following Doukas et al. (2006). Specifically, $\rho = h/(h+s)$ and $h = (\mathrm{SE} - (D/N))/[(\mathrm{SE} - (D/N)) + D]^2$ and $s = D/[(\mathrm{SE} - (D/N)) + D]^2$, where ρ represents consensus, h represents the precision of common information, s represents the precision of idiosyncratic information, SE is the square of the difference between mean forecast EPS and actual EPS, D is the variance in the analyst forecast EPS
DRTPVACC	Number of human page views on periodic accounting reports (10-K and 10-Q) on EDGAR during two trading days [0, +1] around the earnings announcement for each quarter, calculated according to Drake et al. (2015)
DTURN	Detrended average monthly stock turnover over the 12-month period ending before the SPC estimation window
EVOL	Earnings volatility, calculated as the variance of quarterly ROA (net income over lagged total assets) over the past eight quarters before the earnings announcement for each quarter. The raw value of *EVOL* is multiplied by 100
EXP	Median value of firm-specific experience of analysts following the firm for each quarter. Experience is measured as the number of quarters for which every analyst has followed the firm

(The table is continued on the next page.)

(continued)

Variable	Definition
ILLIQ	Amihud's (2002) illiquidity measure, calculated as the mean of the daily price impact ratio over the past one month before the earnings announcement for each quarter
IO	Institutional ownership, measured as the shares owned by institutional owners over total shares outstanding at the end of each quarter
IVOL	Idiosyncratic return volatility, calculated as the residual variance from regressing daily returns on market returns over the past 12 months prior to the earnings announcement for each quarter. The raw value of *IVOL* is multiplied by 100
LOSS	Dummy equal to one if earnings for the quarter is negative, and zero otherwise
NCSKEWLAG	Negative skewness of firm-specific weekly returns measured over the 12-month period ending prior to the SPC estimation window.
NSPE	Dummy indicating a negative special item, which is equal to one if the firm reports negative special items for the quarter, and zero otherwise
MOM	Past returns compounded over the 11-month period ending one month before the month in which earnings for each quarter are released (skipping one month)
POST	Dummy equal to one for post-SPC earnings announcement EA_t^q and zero for pre-SPC earnings announcement EA_{t-2}^q in the treatment effect test. It is equal to one for earnings announcement EA_{t-2}^q and zero for earnings announcement EA_{t-3}^q in earlier placebo tests, and equal to one for earnings announcement EA_{t+1}^q and zero for earnings announcement EA_t^q for later placebo tests
Q4	Dummy equal to one if the quarter is a fourth fiscal quarter, and zero otherwise
RETLAG	Mean of firm-specific weekly returns over 12-month period ending before the SPC estimation window
RM	Mean of the monthly CRSP value-weighted market return over the 12-month period ending before the SPC estimation window
RPVACC	Number of human page views on periodic accounting reports (10-K and 10-Q) on EDGAR during two trading days [0, +1] around the earnings announcement for each quarter, calculated according to Ryans (2017, 2021)
SALESG	Sales growth rate for the fiscal year, which is observable before the SPC estimation window
SIGMA	Standard deviation of firm-specific weekly returns over the 12-month period ending before the SPC estimation window
SIR	Short interest ratio, calculated as the number of shares shorted over the total number of shares outstanding for the month at the end of each quarter. The raw value of *SIR* is multiplied by 100
SIZE	Firm size, calculated as the natural logarithm of market value of equity at the end of each quarter
SIZEPSM	Firm size used in the PSM estimation, calculated as the natural logarithm of market value of equity at the end of fiscal year, which is observable before the SPC estimation window
CRASH	Dummy for SPCs, defined as one if firms experience at least one price crash week over the 12-month estimation window, and zero otherwise. An SPC occurs when weekly returns (*W*) fall more than 3.20 standard deviations below the mean over the 12-month period ending prior to the month in which earnings are released. *W* is estimated using the following model: $r_{j\tau} = \beta_0 + \beta_1 r_{m\tau-2} + \beta_2 r_{m\tau-1} + \beta_3 r_{m\tau} + \beta_4 r_{m\tau+1} + \beta_5 r_{m\tau+2} + \varepsilon_{j\tau}$, where $r_{j,\tau}$ is the return on stock *j* in week τ, and $r_{m\tau-2}$ to $r_{m\tau+2}$ are the returns on the CRSP value-weighted market index in week $\tau-2$ to $\tau+2$ respectively. *W* is the natural logarithm of 1 plus the residual return ϵ, that is, $W_{j,\tau} = Ln\left(1+\varepsilon_{j,\tau}\right)$

(The table is continued on the next page.)

(continued)

Variable	Definition
JUMP	Dummy for SPJs, defined as one if firms experience at least one price jump week over the 12-month estimation window, and zero otherwise. An SPJ occurs when weekly returns (*W*) rise more than 3.20 standard deviations above the mean over the 12-month period ending prior to the month in which earnings are released. *W* is defined above
SUBSPC	Dummy for SPCs after the earnings announcement, defined as one if firms experience at least one price crash week over the 12 months after the month in which earnings are released
SUBSPJ	Dummy for SPJs after the earnings announcement, defined as one if firms experience at least one price jump week over the 12 months after the month in which earnings are released
SUE	Standardized unexpected earnings for each quarter. It is calculated as actual EPS minus analyst forecasted EPS, scaled by the share price 20 days prior to the earnings announcement. Analyst forecasted earnings is the median of the analysts' latest forecasts over the past 90 days prior to the earnings announcement. The raw value of *SUE* is multiplied by 100
TANG	Asset tangibility, calculated as tangible assets over lagged total assets for the fiscal year, which is observable before the SPC estimation window
TREAT	Dummy equal to one for treatment firms, and zero for control firms

Notes: All except for the dummy variables are winsorized by quarter at the bottom and top 1% level.

Appendix 3: Sample construction

Description	Obs.
Step 1: Initial sample based on NYSE, AMEX, and NASDAQ common stocks from 1984 to 2017	
After excluding financial and utility firms, those with a share price below \$5, negative book value of equity, or missing values for variables used in our main regression analyses	206,253
After excluding firms with missing values for SPC propensity determinant variables	176,718
After excluding firms without observations for earnings announcements EA_t^q for five consecutive years from $t-3$ through $t+1$.	63,016
Step 2: Treatment and non-treatment firms before PSM (total = 63,016 obs.)	
Treatment firms are those with SPCs prior to earnings announcements EA_t^q	12,441
Non-treatment firms are those without SPCs prior to earnings announcements EA_t^q	50,575
Step 3: Treatment and control firms after PSM (total = 24,482 obs.)	
Treatment firms after excluding those without industry-peer control firms	12,241
Control firms matched with treatment firms as those in the same industry and similar SPC propensity	12,241
Step 4a: Treatment effect test (total = 48,964 obs.)	
Treatment firms' earnings announcements EA_t^q and EA_{t-2}^q	24,482
Control firms' earnings announcements EA_t^q and EA_{t-2}^q	24,482
Step 4b: Earlier placebo test (total = 48,964 obs.)	
Treatment firms' earnings announcements EA_{t-2}^q and EA_{t-3}^q	24,482
Control firms' earnings announcements EA_{t-2}^q and EA_{t-3}^q	24,482
Step 4c: Later placebo test (total = 48,964 obs.)	
Treatment firms' earnings announcements EA_{t+1}^q and EA_t^q	24,482
Control firms' earnings announcements EA_{t+1}^q and EA_t^q	24,482

Appendix 4: Propensity score estimation

Panel A: Propensity score estimation model

DV = *CRASH*	Pred. sign	Coef.	z-stat
DTURN	+	0.412**	(2.17)
NCSKEWLAG	+	0.093***	(5.20)
RETLAG	+	1.914***	(4.11)
SIGMA	+	9.930***	(3.15)
SIZEPSM	+	0.018	(1.27)
RM	+	0.670	(0.21)
SALESG	+	0.414***	(5.12)
AGE	−	−0.004***	(−3.02)
TANG	−	−0.105*	(−1.74)
Constant		−1.669***	(−7.09)
Industry and quarter FE		Yes	
Obs.		63,016	
Pseudo R^2		0.0306	
Area under ROC curve		0.6223	

Panel B: Before PSM comparison of SPC propensity determinants

			t-test		K–S test
	Treatment firms	Non-treatment firms	Mean	t-stat	p-value
DTURN	0.002	0.001	0.001	(1.01)	0.000
NCSKEWLAG	0.152	0.076	0.076***	(9.54)	0.000
RETLAG	−0.001	−0.001	0.000	(0.62)	0.000
SIGMA	0.044	0.044	0.000	(0.88)	0.000
SIZEPSM	7.559	7.495	0.064***	(4.08)	0.000
RM	0.009	0.009	−0.000**	(−1.97)	0.000
SALESG	0.127	0.109	0.018***	(9.60)	0.000
AGE	23.373	25.874	−2.501***	(−13.51)	0.000
TANG	0.519	0.601	−0.082***	(−20.96)	0.000
Obs.	12,441	50,575			

Panel C: After PSM comparison of SPC propensity determinants

			t-test		K-S test
	Treatment firms	Control firms	Mean	t-stat	p-value
DTURN	0.002	0.002	−0.000	(−0.19)	0.102
NCSKEWLAG	0.147	0.151	−0.004	(−0.35)	0.241
RETLAG	−0.001	−0.001	−0.000	(−0.32)	0.174
SIGMA	0.044	0.044	−0.000	(−0.38)	0.122
SIZEPSM	7.556	7.564	−0.008	(−0.38)	0.000
RM	0.009	0.009	−0.000	(−0.43)	0.833
SALESG	0.128	0.128	−0.000	(−0.61)	0.439
AGE	23.432	23.309	0.123	(0.53)	0.194

(The table is continued on the next page.)

(continued)

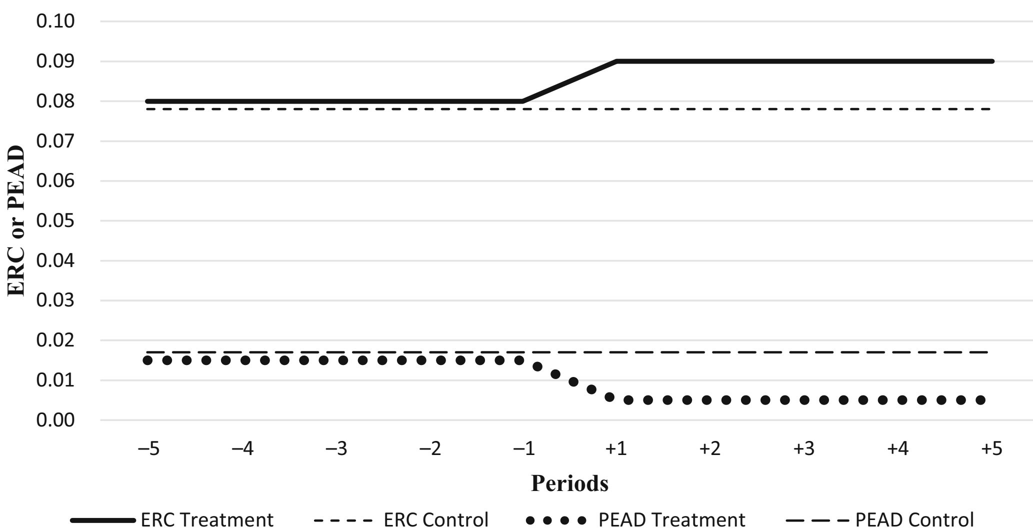
Panel C: After PSM comparison of SPC propensity determinants

	Treatment firms	Control firms	*t*-test		K-S test
			Mean	*t*-stat	*p*-value
TANG	0.520	0.521	−0.001	(−0.12)	0.498
Obs.	12,241	12,241			

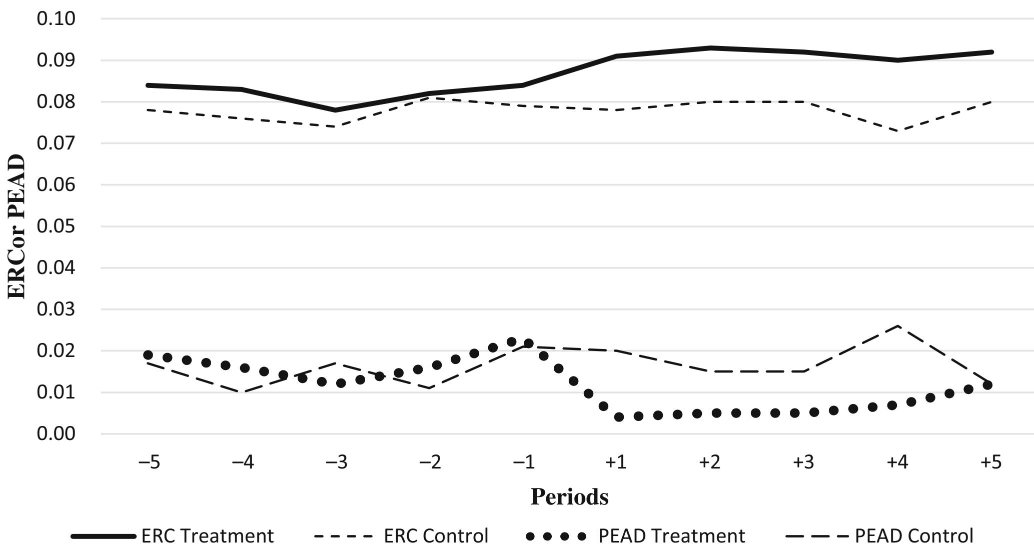
Notes: This table presents the propensity score estimation. Panel A reports the logistic regression results for estimating propensity scores. All the SPC propensity determinant variables are observable at the beginning of the SPC estimation window. Panels B and C compare SPC propensity determinants for treatment and non-treatment firms before and after PSM. Appendix 2 provides detailed variable definitions. Appendix 3 describes the sample construction. *, **, and *** represent two-tailed significance levels of 10%, 5%, and 1%, respectively.

Appendix 5: Parallel trends analysis

Panel A: Hypothetical parallel trends



Panel B: Actual parallel trends



Notes: This Appendix presents the parallel trends analysis of ERCs and PEAD over 10 earnings announcements around the SPC estimation window. Panel A depicts the hypothetical pattern and panel B presents the actual pattern based on the empirically estimated ERCs and PEAD from the following regression for each separate period: $CAR = \alpha_0 + a_1 SUE + Controls + \varepsilon$, where the dependent variables are $CAR_{(0,+1)}$ and $CAR_{(+2,+61)}$ and the coefficients on *SUE* represent ERCs and PEAD respectively; *Controls* are *BETA*, *SIZE*, *BM*, *MOM*, and firm and quarter fixed effects. Appendix 2 provides variable definitions. All the independent variables except for dummy variables are the decile ranked values by quarter ranging from −0.5 through 0.5. Period $t+1$ ($t-1$) measures ERCs and PEAD for the post-SPC EA_t^q (pre-SPC EA_{t-2}^q). Therefore, the lines from periods $t-1$ through $t+1$ illustrate the changes in ERCs and PEAD from EA_{t-2}^q to EA_t^q, which is equivalent to our treatment effect test in our main regression analysis. Periods $t+2$ through $t+5$ ($t-5$ through $t-2$) provide ERCs and PEAD for the earnings announcements for the four consecutive fiscal quarters after (before) the fiscal quarter for EA_t^q (EA_{t-2}^q).

References

Allen, F., S. Morris, and H. S. Shin. 2006. Beauty contests, bubbles and iterated expectations in asset markets. *Review of Financial Studies* 19 (3): 719–52.

Amihud, Y. 2002. Illiquidity and stock returns: Cross-section and time-series effects. *Journal of Financial Markets* 5 (1): 31–56.

Anderson, K. L., J. H. Harris, and E. C. So. 2007. Opinion divergence and post-earnings announcement drift. Working paper, https://ssrn.com/abstract=969736, accessed January 5, 2022.

Andrei, D., H. L. Friedman, and N. B. Ozel. 2020. Economic uncertainty and investor attention. Working paper, https://ssrn.com/abstract=3128673, accessed January 5, 2022.

Armstrong, C., W. Guay, and J. Weber. 2010. The role of information and financial reporting in corporate governance and debt contracting. *Journal of Accounting and Economics* 50 (2–3): 179–234.

Banerjee, S., R. Kaniel, and I. Kremer. 2009. Price drift as an outcome of differences in higher-order beliefs. *Review of Financial Studies* 22 (9): 3707–34.

Banker, R. D., S. Das, and S. M. Datar. 1993. Complementarity of prior accounting information: The case of stock dividend announcements. *The Accounting Review* 68 (1): 28–47.

Bao, D., Y. Kim, G. Mian, and L. Su. 2019. Do managers disclose or withhold bad news? Evidence from short interest. *The Accounting Review* 94 (3): 1–26.

Barron, O. E., O. Kim, S. C. Lim, and D. E. Stevens. 1998. Using analysts' forecasts to measure properties of analysts' information environment. *The Accounting Review* 73 (4): 421–33.

Bauer, A. M., X. Fang, and J. Pittman. 2020. The importance of IRS enforcement to stock price crash risk: The role of CEO power and incentives. *The Accounting Review* 96 (4): 81–109.

Baumeister, R., E. Bratslavsky, and C. Finkenauer. 2001. Bad is stronger than good. *Review of General Psychology* 5 (4): 323–70.

Bennett, B., R. Stulz, and Z. Wang. 2020. Does the stock market make firms more productive? *Journal of Financial Economics* 136 (1): 281–306.

Ben-Rephael, A., Z. Da, and R. D. Israelsen. 2017. It depends on where you search: Institutional investor attention and underreaction to news. *Review of Financial Studies* 30 (9): 3009–47.

Ben-Rephael, A., S. Kandel, and A. Wohl. 2011. The price pressure of aggregate mutual fund flows. *Journal of Financial and Quantitative Analysis* 46 (2): 585–603.

Berkman, H., and C. Truong. 2009. Event day 0? After-hours earnings announcements. *Journal of Accounting Research* 47 (1): 71–103.

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

Bernard, V. L., and J. K. Thomas. 1990. Evidence that stock prices do not fully reflect the implications of current earnings for future earnings. *Journal of Accounting and Economics* 13 (4): 305–40.

Beyer, A., D. A. Cohen, T. Z. Lys, and B. R. Walther. 2010. The financial reporting environment: Review of the recent literature. *Journal of Accounting and Economics* 50 (2–3): 296–343.

Bradshaw, M. 2009. Analyst information processing, financial regulation, and academic research. *The Accounting Review* 84 (4): 1073–83.

Callen, J. L., and X. Fang. 2015. Religion and stock price crash risk. *Journal of Financial and Quantitative Analysis* 50 (1–2): 169–95.

Callen, J. L., and X. Fang. 2016. Crash risk and the auditor-client relationship. *Contemporary Accounting Research* 34 (3): 1715–50.

Case, D. O. 2012. *Looking for Information: A Survey of Research on Information Seeking, Needs, and Behavior*, 3rd ed. Bingley: Emerald.

Chordia, T., and L. Shivakumar. 2006. Earnings and price momentum. *Journal of Financial Economics* 80 (3): 627–56.

Collins, D. W., and S. P. Kothari. 1989. An analysis of intertemporal and cross-sectional determinants of earnings response coefficients. *Journal of Accounting and Economics* 11 (1): 143–81.

Core, J. 2020. The real effects of financial reporting on pay and incentives. *Accounting and Business Research* 50 (5): 448–69.

Coval, J., and E. Stafford. 2007. Asset fire sales (and purchases) in equity markets. *Journal of Financial Economics* 86 (2): 479–512.

Daniel, K., D. Hirshleifer, and A. Subrahmanyam. 1998. Investor psychology and security market under- and overreactions. *Journal of Finance* 53 (6): 1839–85.

Dechow, P. M., W. Ge, and C. Schrand. 2010. Understanding earnings quality: A review of the proxies, their determinants, and their consequences. *Journal of Accounting and Economics* 50 (2–3): 344–401.

Dechow, P. M., R. G. Sloan, and A. P. Sweeney. 1995. Detecting earnings management. *The Accounting Review* 70 (2): 193–225.

DeFond, M. L., M. Hung, S. Li, and Y. Li. 2015. Does mandatory IFRS adoption affect crash risk? *The Accounting Review* 90 (1): 265–99.

Dessaint, O., T. Foucault, L. Fresard, and A. Matray. 2019. Noisy stock prices and corporate investment. *Review of Financial Studies* 32 (7): 2625–72.

Diamond, D. W., and R. E. Verrecchia. 1987. Constraints on short-selling and asset price adjustment to private information. *Journal of Financial Economics* 18 (2): 277–311.

Diether, K. B., C. J. Malloy, and A. Scherbina. 2002. Differences of opinion and the cross section of stock returns. *Journal of Finance* 57 (5): 2113–41.

Doukas, J. A., C. Kim, and C. Pantzalis. 2006. Divergence of opinion and equity returns. *Journal of Financial and Quantitative Analysis* 41 (3): 573–606.

Drake, M. S., D. T. Roulstone, and J. R. Thornock. 2015. The determinants and consequences of information acquisition via EDGAR. *Contemporary Accounting Research* 32 (3): 1128–61.

Edmans, A., I. Goldstein, and W. Jiang. 2012. The real effects of financial markets: The impact of prices on takeovers. *Journal of Finance* 67 (3): 933–71.

Garfinkel, J. A., and J. Sokobin. 2006. Volume, opinion divergence, and returns: A study of post-earnings announcement drift. *Journal of Accounting Research* 44 (1): 85–112.

Hirshleifer, D., and J. Sheng. 2021. Macro news and micro news: Complements or substitutes. Working paper, https://ssrn.com/abstract=2890864, accessed January 5, 2022.

Holthausen, R. W., and R. E. Verrecchia. 1990. The effect of informedness and consensus on price and volume behavior. *The Accounting Review* 65 (1): 191–208.

Hong, H., and J. C. Stein. 1999. A unified theory of underreaction, momentum trading, and overreaction in asset markets. *Journal of Finance* 54 (6): 2143–84.

Hong, H., and J. C. Stein. 2007. Disagreement and the stock market. *Journal of Economic Perspectives* 21 (2): 109–28.

Hutton, A. P., A. J. Marcus, and H. Tehranian. 2009. Opaque financial reports, R^2, and crash risk. *Journal of Financial Economics* 94 (1): 67–86.

Jang, J., and J. Kang. 2019. Probability of price crashes, rational speculative bubbles, and the cross section of stock returns. *Journal of Financial Economics* 132 (1): 222–47.

Jin, L., and S. C. Myers. 2006. R^2 around the world: New theory and new tests. *Journal of Financial Economics* 79 (2): 257–92.

Kahneman, D., and A. Tversky. 1979. Prospect theory: An analysis of decision under risk. *Econometrica* 47 (2): 263–92.

Kandel, S., and R. F. Stambaugh. 1996. On the predictability of stock returns: An asset-allocation perspective. *Journal of Finance* 51 (2): 385–424.

Khan, M., and R. L. Watts. 2009. Estimation and empirical properties of a firm-year measure of accounting conservatism. *Journal of Accounting and Economics* 48 (2–3): 132–50.

Kim, J.-B., Y. Li, and L. Zhang. 2011a. Corporate tax avoidance and stock price crash risk: Firm-level analysis. *Journal of Financial Economics* 100 (3): 639–62.

Kim, J.-B., Y. Li, and L. Zhang. 2011b. CFOs versus CEOs: Equity incentives and crashes. *Journal of Financial Economics* 101 (3): 713–30.

Kim, J.-B., Z. Wang, and L. Zhang. 2016. CEO overconfidence and stock price crash risk. *Contemporary Accounting Research* 33 (4): 1720–49.

Kim, J.-B., and L. Zhang. 2014. Financial reporting opacity and expected crash risk: Evidence from implied volatility smirks. *Contemporary Accounting Research* 31 (3): 851–75.

Kim, J.-B., and L. Zhang. 2016. Accounting conservatism and stock price crash risk: Firm-level evidence. *Contemporary Accounting Research* 33 (1): 412–41.

Kim, O., and R. E. Verrecchia. 1994. Market liquidity and volume around earnings announcements. *Journal of Accounting and Economics* 17 (1–2): 41–67.

Kim, O., and R. E. Verrecchia. 1997. Pre-announcement and event-period private information. *Journal of Accounting and Economics* 24 (3): 395–419.

Kothari, S. P. 2001. Capital markets research in accounting. *Journal of Accounting and Economics* 31 (1–3): 105–231.

Kothari, S. P., S. Shu, and P. Wysocki. 2009. Do managers withhold bad news? *Journal of Accounting Research* 47 (1): 241–76.

Leippold, M., F. Trojani, and P. Vanini. 2008. Learnings and asset prices under ambiguous information. *Review of Financial Studies* 21 (6): 2565–97.

Li, Z., L. Wang, and K. Wruck. 2020. Accounting-based compensation and debt contracts. *Contemporary Accounting Research* 37 (3): 1475–511.

Martineau, C., and M. Zoican. 2020. Investor attention and the cross-section of analyst coverage. Working paper, https://ssrn.com/abstract=3376162, accessed January 5, 2022.

Mashruwala, C., S. Rajgopal, and T. Shevlin. 2006. Why is the accrual anomaly not arbitraged away? The role of idiosyncratic risk and transaction costs. *Journal of Accounting and Economics* 42 (1–2): 3–33.

McVay, S., and B. Szerwo. 2021. Preparers and the financial reporting system. *Accounting and Business Research* 51 (5): 484–507.

Merton, R. C. 1987. A simple model of capital market equilibrium with incomplete information. *Journal of Finance* 42 (3): 483–510.

Miller, E. M. 1977. Risk, uncertainty, and divergence of opinion. *Journal of Finance* 32 (4): 1151–68.

O'Brien, P. C., and R. Bhushan. 1990. Analyst following and institutional ownership. *Journal of Accounting Research* 28: 55–76.

Odean, T. 1998. Are investors reluctant to realize their losses? *Journal of Finance* 53 (5): 1775–98.

Pan, J. 2002. The jump-risk premia implicit in options: Evidence from an integrated time-series study. *Journal of Financial Economics* 63 (1): 3–50.

Pastor, L., and P. Veronesi. 2009. Learning in financial markets. *Annual Review of Financial Economics* 1: 361–81.

Peng, L. 2005. Learning and information capacity constraints. *Journal of Financial and Quantitative Analysis* 40 (2): 307–29.

Peng, L., and W. Xiong. 2006. Investor attention, overconfidence and categorical learning. *Journal of Financial Economics* 80 (3): 563–602.

Pratto, F., and O. John. 1991. Automatic vigilance: The attention-grabbing power of negative social information. *Journal of Personality and Social Psychology* 61 (3): 380–91.

Richardson, S., I. Tuna, and P. Wysocki. 2010. Accounting anomalies and fundamental analysis: A review of recent research advances. *Journal of Accounting and Economics* 50 (2–3): 410–54.

Roychowdhury, S., N. Shroff, and R. Verdi. 2019. The effects of financial reporting and disclosure on corporate investment: A review. *Journal of Accounting and Economics* 68 (2–3): 101246.

Ryans, J. P. 2017. Using the EDGAR log file data set. Working paper, https://papers.ssrn.com/sol3/papers.cfm?abstract_id=2913612, accessed January 5, 2022.

Ryans, J. P. 2021. Textual classification of SEC comment letters. *Review of Accounting Studies* 26: 37–80.

Sims, C. 2003. Implications of rational inattention. *Journal of Monetary Economics* 50 (3): 665–90.

Treisman, A. 1960. Contextual cues in selective listening. *Quarterly Journal of Experimental Psychology* 12 (4): 242–8.

Tversky, A., and D. Kahneman. 1991. Loss aversion in riskless choice: A reference-dependent model. *Quarterly Journal of Economics* 106 (4): 1039–61.

Verardo, M. 2009. Heterogeneous beliefs and momentum profits. *Journal of Financial and Quantitative Analysis* 44 (4): 795–822.

Veronesi, P. 1999. Stock market overreaction to bad news in good times: A rational expectations equilibrium model. *Review of Financial Studies* 12 (5): 975–1007.

Wooldridge, J. 2002. *Econometric Analysis of Cross Section and Panel Data*. Cambridge, MA: MIT Press.

Yan, S. 2011. Jump risk, stock returns, and slope of implied volatility smile. *Journal of Financial Economics* 99 (1): 216–33.

Zhang, F. 2006. Information uncertainty and stock returns. *Journal of Finance* 61 (1): 105–37.

Zuo, L. 2016. The information feedback effect of stock prices on management forecasts. *Journal of Accounting and Economics* 61 (2–3): 391–413.