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ORIGINAL RESEARCH

How do insider trading incentives shape nonfinancial disclosures? Evidence from product and business expansion disclosures

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Abstract
Nonfinancial disclosures of product and business expansion planning occur frequently in practice and are an important vehicle by which managers convey corporate information to outsiders. However, little is known about how the opportunistic incentives of managers affect the choice of such nonfinancial disclosures. This study examines whether managers make their nonfinancial disclosures strategically for self-serving trading incentives. I find strong and robust evidence to suggest that managers manipulate the timing and selectivity of their nonfinancial disclosures in an attempt to maximize trading profits. Specifically, managers tend to disclose bad (good) news on products or business expansion before purchasing (selling) shares. I also find that such strategic behavior is more evident when the expected price impact of the disclosures is greater and when the CEOs are more powerful. However, I do not find evidence that the strategic behavior is weaker for firms with high institutional stock ownership. Overall, my results contribute to understanding managers' strategic use of nonfinancial disclosures in fulfilling personal trading incentives, and should be of interest to boards of directors charged with the responsibility of monitoring and restricting opportunistic managerial disclosures and insider trades.

Keywords Managerial incentives · Product and business expansion disclosures · Insider trades

JEL Classification M41 · G14

1 Introduction

This study investigates whether, and how, insider trading opportunities provide managers with incentives to make strategic disclosures of product and business expansion (hereafter, PBE) plans to the public. Product-related disclosures are defined as disclosures of plans that relate to the introduction, change, improvement, or discontinuation of a company's

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products or services. Business expansion disclosures relate to an increase in current operations through internal growth, such as entering into new markets with existing products, opening a new branch, establishing a new division, increasing production capacity, or investing additional capital in current operations, but excluding growth by virtue of mergers and acquisitions.[1] Such nonfinancial disclosures are voluntarily made by firms through press releases or news outlets. Apart from good news, a PBE plan could imply bad news on future payoffs to investors when, for example, PBE activities are at odds with a firm's financial capacity and operational status, or are subject to external or internal threats such as (1) compliance threats associated with relevant polities, laws, regulations, or corporate governance, (2) financial threats arising from volatility in the financial market and real economy, (3) strategic threats related to customers, suppliers, competitors, and investors, (4) operational threats that concern the processes, systems, people, and overall value chain of a business, and (5) uncertainty as to managerial ability to execute the firm's PBE strategies.

The motivations of this study are three-fold. First, firm management wishes for a lower share price before buying shares, and for a higher stock price before selling stocks. To this end, managers can manipulate PBE disclosures. Given PBE news may occur within a firm at any point in time during a year, when and how to disclose PBE news are at the discretion of managers. They can be selective in releasing good (bad) news, and withhold bad (good) news, on PBE information to inflate (deflate) stock prices at the points when self-serving trading opportunities arise.[2] Whether managers would do so, however, depends on managers' trade-off between the expected benefits and costs of such a strategic behavior. The costs to the managers may be associated with litigation and reputational losses, since regulations require firm management to disclose value-relevant news on a timely basis. Hence, whether managers tend to manipulate the timing and selectivity of PBE disclosures is an open empirical question to examine in this study.

Second, news hoarding and selective disclosures are hard to detect for outsiders, because it is often difficult to discern whether, at a particular point in time, insiders are not informed of any news or are deliberately withholding news (Dye 1985; Jung and Kwon 1988). Theory and evidence (e.g., Dye 1985; Jung and Kwon 1988; Verrecchia 2001; Kothari et al. 2009; Kim et al. 2011) suggest that, compared to disclosing misleading or biased information, withholding information entails relatively lower detection risk and lower litigation risk. Thus, it is used more prevalently among listed companies to fulfil various opportunistic incentives (e.g., Kothari et al. 2009; He et al. 2021). This underscores the importance of investigating this issue empirically. Earnings forecasted or announced by firms pertains to an aggregate number that bears good news, or bad news, only. In contrast, PBE plans may contain rich, heterogenous information involving both good news and bad news. Therefore, nonfinancial disclosures of PBE plans provide a powerful setting to examine directly the managerial selective-disclosure and news-hoarding behavior.

[1] The definitions of the product and business expansion disclosures follow Capital IQ, a division of Standard and Poor's. "Appendix 2" gives examples of firms' product and business expansion plans.

[2] Managers can opt not to disclose corporate news fully to the public. In disclosing corporate news, managers might withhold some bad (good) news they know. In such a selective-disclosure case, stock prices are likely to be inflated (deflated) as a result of investors' overreactions to the partially disclosed news, because, as to be mentioned in the main body text, it is often too hard for outside investors to see through the news hoarding, which is more so for the hoarding of PBE news that is qualitative in nature.

Third, existing literature on the role of managerial incentives in corporate disclosures focuses predominantly on management earnings forecasts (e.g., Bushman and Indjejikian 1995; Frankel et al. 1995; Noe 1999; Aboody and Kasznik 2000; Lang and Lundholm 2000; Nagar et al. 2003; Cheng and Lo 2006; Brockman et al. 2008; Rogers 2008; Cheng et al. 2013; Baginski et al. 2018) with little regard to nonfinancial disclosures. Disclosures of PBE plans pertain to the key value-relevant nonfinancial disclosures that occur frequently in practice (e.g., Nichols 2010; He 2018; He et al. 2019), but little is known about how the opportunistic incentives of managers shape the choice of such nonfinancial disclosures. To fill this gap in the literature, I set out to examine whether, and how, insider trades create incentives for managers to take advantage of PBE disclosures to manipulate information flows.

Insider trading regulations strictly prohibit any insider trade made strategically before disclosures of material nonpublic information (e.g., Garfinkel 1997; Noe 1999). Hence, following prior research (e.g., Cheng and Lo 2006; Rogers 2008; Cheng et al. 2013), I use insider trading after corporate disclosures to proxy for managers' *ex ante* incentives to seek trading profits. To increase trading gain, managers can selectively provide good (bad) news disclosures to inflate (deflate) stock prices before selling (purchasing) shares. But managers might incur trading costs for doing so, and the trading costs vary. In the case of insider purchases, a stock price increase as a consequence of the stock purchases would only result in opportunity costs, which are not regarded as damaging to an investor (Niehaus and Roth 1999). Hence, presumably, insider purchases would not lead to litigation to insiders; accordingly, trading risk would be low.[3]

I define disclosure risk broadly as including litigation risk and reputation risk that are associated with strategic disclosures.[4] While selectively releasing bad news and hiding good news before stock purchases, managers may defend themselves against potential litigation and/or reputation losses by claiming that they are ignorant of good news at the time of the bad news disclosures. In such a case, managers would bear not only low trading risk from insider purchases but also low disclosure risk from nonfinancial disclosures. Therefore, managers might be inclined to make bad-news PBE disclosures selectively before purchasing shares. On the other hand, I also allow for the possibility that managers do not intend to do so, if they are averse to the trading risk and disclosure risk or feel themselves not able to lower such risks.

[3] Prior research (e.g., Cheng and Lo, 2006) defines trading risk as the litigation risk arising from insider trades that are alleged to have occurred in contravention of insider trading regulations. I define trading risk in a broad way as including both litigation risk and reputation risk that arise from insider trades. The motivation and story of my study differ from those of the prior literature by concerning managers' trade-off between their expected benefits versus costs of making the opportunistic disclosures for fulfilling personal trading incentives. Because reputational costs and litigation costs are both the main costs associated with the strategic behavior, I account for both in the paper. The insider trading rules that prohibit trading on material nonpublic information apply equally to insider sales and insider purchases, but as discussed in the main body text, insider sales are generally associated with higher expected legal costs than are insider purchases. On the other hand, I assume that reputation risk involves insider sales *vis-à-vis* insider purchases in a relatively equal manner. Reputational costs associated with insider sales (purchases) would be low, to the extent that insiders can convince outside investors of the sales (purchases) being intended for liquidity needs or portfolio diversifications (signaling of good firm prospect).

[4] Disclosure risk refers to the litigation risk arising from disclosures that are proven to be opportunistic *ex post* (e.g., Cheng and Lo, 2006; Choi et al., 2010). In a broad sense, disclosure risk may also encompass reputation risk, i.e., the risk of reputational losses.

On the other hand, to the extent that bad news hoarding is more subject to queries and criticisms than good news hoarding, it would be harder for insiders to defend themselves from litigation and reputational losses by claiming that they were unaware of bad news at the point of a good news disclosure made before stock sales. Furthermore, unlike insider purchases, insider sales could lead to high litigation costs for insiders. To be specific, if insiders sell shares after a good news disclosure, a stock price decline resulting from the stock sales would damage the wealth of incumbent shareholders who fail to trade duly. As a result, shareholders who suffer losses could sue insiders, alleging that the insiders traded on foreknowledge of price-relevant corporate disclosures (e.g., Huddart et al. 2007). Hence, insiders would still bear litigation risk for selling shares after disclosures. Insiders would (or would not) selectively disclose good news on PBE prior to selling shares, if the costs associated with this self-serving strategy are perceived by insiders to be lower (higher) than the expected trading gain.

My empirical tests are conducted based on a sample of 27,016 PBE disclosures made by U.S. listed firms. Following prior research (e.g., Cheng and Lo 2006; Brockman et al. 2008; Ge and Lennox 2011; He 2018), I use abnormal stock returns to classify whether a PBE disclosure pertains to good or bad news to the market. Using an ordinary logit regression, I find that the likelihood of a bad-news PBE disclosure, relative to that of a good-news PBE disclosure, is significantly higher before insider purchases than in the absence of insider purchases. This is consistent with the view that a bad-news nonfinancial disclosure being made before insider purchases entails low trading risk and low disclosure risk for insiders. I also find that managers tend to disclose good PBE news before insider sales, which suggests that the trading benefits are perceived by insiders to outstrip the expected costs associated with the strategic behavior of managers. This is likely because the litigation risk for insider sales does not manifest itself in a good-news nonfinancial disclosure that is of low disclosure risk to insiders. My findings are robust to using various approaches to mitigate potential endogeneity.

To enrich my analyses and further ensure the robustness of the main results, I conduct three supplemental tests. First, I analyze my baseline regression results cross-sectionally. They are more evident when the expected price impact of PBE disclosures is greater and when CEOs are more powerful, but hold independently of institutional stock ownership. Second, I find evidence that my baseline results are not driven by self-selection of managerial decisions to disclose PBE plans voluntarily. Third, I find that the baseline results are robust to addressing potential confounding effects that arise from some PBE disclosures being bundled contemporaneously with management earnings forecasts or earnings announcements.

The key contribution of this study is as follows. Firstly, prior disclosure literature investigates the impact of managerial incentives on voluntary financial disclosures in the setting of equity offerings (e.g., Frankel et al. 1995; Marquardt and Wiedman 1998; Lang and Lundholm 2000; Kim 2016), stock repurchases (e.g., Brockman et al. 2008), stock and stock option grants (e.g., Aboody and Kasznik 2000; Nagar et al. 2003), leveraged buyout offers (e.g., Hafzalla 2009), stock-for-stock mergers (e.g., Ge and Lennox 2011), and insider trades (e.g., Noe 1999; Bushman and Indjejikian 1995; Rogers and Stocken 2005; Cheng and Lo 2006; Rogers 2008; Cheng et al. 2013). However, nonfinancial disclosures are neglected in this research area. This study is the first to provide evidence on how PBE disclosures are shaped by the opportunistic incentives of managers.

There are three differences between management earnings forecasts and PBE disclosures in terms of the role they play as an instrument for managers to fulfil opportunistic incentives. First, unlike management earnings forecasts that could be verified by

subsequent audited earnings reports, qualitative PBE disclosures, especially in terms of their disclosure completeness and timeliness, are relatively hard to verify, at least in a short run. Therefore, managers can manipulate the timing and selectivity of PBE disclosures to exploit self-serving opportunities with relatively low detection risk. Second, compared to management earnings forecasts that imply mainly the short-term prospects of a firm's performance, PBE disclosures have implications for long-term streams of a firm's future earnings (Nichols 2010; He 2018; He et al. 2019). Third, PBE disclosures are relatively more discretionary in their timing and selectivity than are management earnings forecasts. Prior studies (e.g., Graham et al. 2005; Field et al. 2005) document that firms tend to commit to a long-standing policy of providing continual earnings forecasts, which are often scheduled to take place shortly before earnings announcements. A discontinuity of earnings forecasts would subject firms to reputational losses and increased cost of capital (Chen et al. 2011; Marshall and Skinner 2022). By contrast, disclosures of PBE news are often not scheduled and may occur sporadically throughout the year, hence facilitating the timing of disclosures to a larger extent for fulfilling opportunistic incentives. More importantly, a management earnings forecast pertains to an aggregate number reflecting a firm's projected earnings performance. By contrast, PBE plans involve richer and more specific heterogenous information, including varied nature of news, from which managers can select to affect stock prices. Given the foregoing differences between PBE disclosures and management earnings forecasts, it is beneficial to advance the strategic disclosure literature by exploring whether, and how, managers opportunistically disclose PBE news before stock trades to attain their trading incentives.

Secondly, this paper also contributes to the insider trading literature. A vast body of literature (e.g., Johnson et al. 2009; Badertscher et al. 2011; Thevenot 2012; Skaife et al. 2013; Billings and Cedergren 2015; Dechow et al. 2016) finds evidence that insiders actively trade in advance of various price-relevant corporate events. But less research attention has been paid to managers manipulating disclosures before insider trades. This strategy enables managers to fulfil their personal trading incentives but does not violate insider trading laws directly, and thus should warrant more attention and scrutiny by regulators and boards of directors. To this end, a few studies (e.g., Cheng and Lo 2006; Rogers 2008) provide evidence that managers tend to issue bad-news earnings forecasts to deflate stock prices before purchasing shares. However, they find no evidence that managers make good-news earnings forecasts before selling shares. Unlike this prior research, I focus on nonfinancial disclosures, and find strong evidence not only on bad news disclosures made before insider purchases but also on good news disclosures made before insider sales. Thus, this study, in conjunction with the related literature, provides a fuller portrait of managers' use of disclosure strategies to fulfil personal incentives. Given that informed insider sales would cause real damages to uninformed investors, my findings have important incremental implications for market participants. For example, institutional investors should alert to my finding that the managers' strategic behavior prevails even among firms with high institutional stock ownership. In addition, this study should also be of interest to boards of directors who are monitoring and restricting insider trades on behalf of shareholders. My findings suggest that boards of directors should closely monitor the schedules and content of PBE disclosures and the insider trades *post* the disclosures.

The remainder of the paper proceeds as follows. Section 2 reviews the related literature and develops hypotheses. Section 3 describes the data collection and variable measurements. Section 4 explains the research methodology and discusses the results. Section 5 concludes.

2 Related literature and hypothesis development

2.1 Related literature on corporate disclosures and insider trades

Prior literature investigates how managers exploit their discretion over corporate disclosures for opportunistic purposes. For instance, Lang and Lundholm (2000) provide evidence that firms release more good news for a higher stock price before equity offerings. Brockman et al. (2008) show that managers tend to release pessimistic earnings forecasts to deflate stock prices before stock repurchases. Ge and Lennox (2011) find that managers withhold bad earnings news before stock-for- stock mergers. Yermack (1997) and Aboody and Kasznik (2000) document that managers strategically disclose bad news to deflate stock prices before stock option grants to maximize option values. Several studies explore strategic disclosures by managers in the insider trading settings that exclude option grants. Cheng and Lo (2006) find that managers release more bad earnings news prior to purchasing shares of their firms. In parallel, Rogers (2008) find some weak evidence that managers provide lower quality disclosures prior to purchasing shares than they would in the absence of insider trades. Cheng et al. (2013) find that managers tend to release more precise earnings forecasts for good (bad) news than for bad (good) news before selling (buying) shares. Overall, the evidence in this literature indicates that corporate disclosures, which occur shortly before price-relevant events, are subject to the opportunistic incentives of managers. Put differently, it is the opportunistic incentives for equity offerings, stock repurchases, stock-for-stock mergers, option grants, and insider trades, that drive disclosure behaviors, rather than the disclosures causing those events to take place *ex post*. As such, hypothetically, reverse causality is less concerned in this strategic-disclosure literature; so too is this study which looks at PBE disclosures made before insider trades.

This paper is also related to a growing body of works on opportunistic insider trades. This literature provides evidence that managers tend to trade stocks before disclosures of a wide range of price-relevant corporate news; these include earnings announcements (e.g., Ke et al. 2003; Piotroski and Roulstone 2005), management earnings forecasts (Penman 1982, 1985; Billings and Cedergren 2015), earnings misstatements (e.g., Badertscher et al. 2011; Agrawal and Cooper 2015), equity offerings (e.g., Karpoff and Lee 1991), stock repurchases (e.g., Lee et al. 1992), dividend announcements (John and Lang 1991), bankruptcies (Gosnell et al. 1992; Seyhun and Bradley 1997), disclosures of internal control weaknesses (Skaife et al. 2013), as well as other corporate news releases (Elliott et al. 1984; Givoly and Palmon 1985; Summers and Sweeney 1998; Beneish 1999; Dechow et al. 2016).

My study differs from the foregoing related literature by examining insider trades *after* corporate disclosures, which are less subject to legal constraints by insider trading laws, and by focusing on product and business expansion disclosures, a distinctive type of value-relevant nonfinancial disclosures. The follow-up sections explicate how PBE disclosures can be employed by managers in a way that fulfils their personal trading incentives.

2.2 The role of managerial incentives in shaping PBE disclosures

While this study makes the first attempt to highlight the impact of managerial incentives on product and business expansion disclosures, it is important to note how such

nonfinancial disclosures help managers fulfil self-serving incentives. Firstly, managers have discretion on when and how to make disclosures to the public (e.g., Richardson 2002). Nonfinancial disclosures of PBE plans, especially in respect of disclosure completeness and timeliness, are relatively hard to verify *ex post*, or at least in a short run, by outside investors who generally do not have access to a firm's private information. Thus, managers can manipulate the timing and selectivity of PBE disclosures to fulfil personal trading incentives without bearing high disclosure risk. Specifically, managers can selectively release (withhold) good (bad) PBE news to boost stock prices, or selectively disclose (withhold) bad (good) PBE news to reduce stock prices. Given such incomplete disclosures of news, investors are likely to overreact to the good (bad) PBE news, leading to an inflation (deflation) of stock prices and an increase in trading gain by insiders. On the other hand, while withholding PBE news at a specific point in time, managers can defend themselves from litigation and reputational losses by arguing that they were unaware of, or knew with uncertainty, about the news at that point. As such, the potential reputational and legal costs for withholding PBE news would be relatively low for managers, even if outsiders discern the incomplete or untimely PBE disclosures. This line of reasoning is in line with the prevailing literature (e.g., Kothari et al. 2009; Baginski et al. 2018; Bao et al. 2019; He et al. 2021) which shows that managers tend to withhold various material value- relevant news. Secondly, in pricing firm equity, investors, if rational and sophisticated, should rely on nonfinancial information such as product or business expansion to forecast a firm's long-term streams of future earnings. Consistent with this notion, Nichols (2010) finds evidence of strong market reactions to PBE disclosures; He et al. (2019) find that analysts actively utilize PBE disclosures for their earnings forecasts for firms, and that investors react more strongly to the analysts' earnings forecasts in response to PBE disclosures. Thirdly, PBE disclosures may occur sporadically and are discretionary in their timing and selectivity, thus making the timing of disclosures relatively easy for managers to fulfil personal trading incentives. In addition, PBE plans involve rich and specific heterogenous information, including varied nature of news, from which managers can select to affect stock prices. In sum, nonfinancial disclosure of PBE plans is a powerful, flexible instrument that managers can use to fulfil their personal incentives. In the next section, I discuss how insider trading incentives impact disclosures of PBE plans.

2.3 Hypothesis development—insider trading incentives and PBE disclosures

The value of insider trades is associated with stock prices. So, to increase trading gain, managers can exploit their private information and manipulate corporate disclosures to influence stock prices. But insider trading regulations (particularly, the "disclose or abstain" doctrine) require that insiders who possess material private information should disclose it to the public, or abstain from trading *ex ante* based on the private information. Any insider purchase (sale) preceding price-relevant good (bad) news disclosures is regarded as illegal.[5] The enactment of the Insider Trading Sanctions Act (ITSA) of 1984

[5] Due to the direct legal constraints, managers usually dare not deliberately delay good (bad) news until after stock purchases (sales). Noe (1999) finds evidence of insider trades after management earnings forecasts but no evidence of strategic insider trades before the forecasts are released. Garfinkel (1997) and Huddart et al. (2007) find that insiders tend to strategically trade shares after earnings announcements but not before earnings announcements. Thus, consistent with the disclosure literature (e.g., Cheng and Lo, 2006; Rogers, 2008; Cheng et al., 2013), I focus on corporate disclosures ahead of insider trades.

and the Insider Trading and Securities Fraud Enforcement Act (ITSFEA) of 1988 in U.S. substantially increased penalties for illegal insider trades.[6] Therefore, when managers plan for trading, they can instead opt to selectively disclose good (bad) news to inflate (deflate) stock prices before selling (buying) shares. However, insiders who trade after corporate disclosures may still be suspected of having exploited foreknowledge of price-relevant public disclosures (Huddart et al. 2007). This, if confirmed to be true, would contravene the U.S. securities laws governing the release of forward-looking statements that surround insider trades (Arshadi 1998; Rogers and Stocken 2005). Although insiders still bear some degree of litigation risk for trading after corporate disclosures, this risk is lower than that associated with trading before disclosures (e.g., Garfinkel 1997; Noe 1999; Huddart et al. 2007). Such litigation risk is mainly manifested in the insider selling case for two reasons.

First, when insider sales are followed by a price decline, investors who fail to trade duly would suffer losses and can thereby file a lawsuit against insiders, alleging that the insiders traded on foreknowledge of public disclosures (e.g., Huddart et al. 2007). In contrast, a stock price increase following insider purchases only leads to opportunity losses for investors. Because the opportunity costs of not purchasing shares duly are not considered as damages to investors (Niehaus and Roth 1999), presumably insider purchases after disclosures would not result in litigation to insiders. Second, most private enforcers of insider trading rules focus exclusively on insider selling cases, and the courts often recognize insider sales as an action with scienter (Johnson et al. 2007; Rogers 2008).[7] In contrast, insider purchases, especially after bad news disclosures, could be construed alternatively as insiders signaling their optimistic beliefs in a firm's future prospect. Thus, they are not usually recognized as a mechanism to establish evidence of scienter in courts. Insiders could defend themselves against legal and/or reputational risk associated with insider purchases by claiming that the purchases are aimed at signaling good future prospect of firms.

The Stock Exchange's listing agreement in U.S. seeks to "*ensure timely disclosure of information that may affect security values or influence investment decisions* (NYSE Listed Company Manual 2019)". Failure to disclose all PBE news on a timely basis transgresses the listing agreement, and could bring about litigation and associated reputational losses for firm management after the strategic disclosures are discovered. Nevertheless, prior research (e.g., Bradbury et al. 2009; Kim et al. 2011; Baginski et al. 2018; Bao et al. 2019; He et al. 2020) finds evidence that managers withhold bad news for extended periods. The completeness and timeliness of nonfinancial disclosures as to PBE plans are not easily discernable by outside investors. Hence, insiders may bear relatively low disclosure risk for manipulating the timing and selectivity of such nonfinancial disclosures. Such disclosure risk is even lower for selectively releasing bad news and hiding good news, than for selectively disclosing good news and concealing bad news, at a specific point in time. Given low trading risk from insider purchases and low disclosure risk from bad-news PBE disclosures, the perceived costs to insiders of purchasing shares after a bad-news PBE disclosure should be lower compared to the perceived benefits of trading gain. Therefore, managerial incentives to buy shares are expected to induce a higher incidence of a bad-news PBE disclosure in advance of the

[6] ITSA increased civil penalties by 300% and increased criminal penalties by 1000% relative to pre-existing penalties. ITSFEA increased criminal penalties to a maximum of $1 million and increased the length of jail sentence to a maximum of 10 years (Jagolinzer and Roulstone, 2009).

[7] Scienter is defined by the U.S. Supreme Court as "*a mental state embracing intent to deceive, manipulate, or defraud*.".

share purchases. That said, I also allow for the possibility that managers might still scruple about potential reputation losses and litigation that are associated with the opportunistic strategy and thus refrain from adopting such self- serving maneuver. Accordingly, I make my first hypothesis in both the null and alternative forms as follows.

$H1_0$ *The likelihood of a bad news disclosure (relative to that of a good news disclosure) of product or business expansion information before insider purchases does not differ from the relative likelihood of the bad news disclosure in the absence of insider purchases.*

$H1_a$ *The likelihood of a bad news disclosure (relative to that of a good news disclosure) of product or business expansion information before insider purchases is higher than the relative likelihood of the bad news disclosure in the absence of insider purchases.*

As discussed previously, insiders may still bear high trading risk for selling shares after good news disclosures. In this scenario, if the perceived benefits of a trading gain are higher (lower) than the perceived costs associated with the trading and disclosures, insiders would (would not) selectively disclose good news on PBE before selling shares. Thus, my second hypothesis, stated respectively in the null and alternative forms, follows.

$H2_0$ *The likelihood of a good news disclosure (relative to that of a bad news disclosure) of product or business expansion information prior to insider sales does not differ from the relative likelihood of the good news disclosure in the absence of insider sales.*

$H2_a$ *The likelihood of a good news disclosure (relative to that of a bad news disclosure) of product or business expansion information prior to insider sales is higher than the relative likelihood of the good news disclosure in the absence of insider sales.*

3 Data and variable measurements

3.1 Data sources and sample

The empirical analysis is conducted based on data gathered primarily from six sources: Compustat, Center for Research in Security Prices (CRSP), Institutional Brokers Estimate System (I/B/E/S), Capital IQ, Factset, and Thomson Financial. I obtain insider trading data from the Thomson Financial Insider Research Services Historical Files. Consistent with Huddart and Ke (2007), insider trading transactions used in the empirical tests are limited to open market purchases and open market sales. Non-open-market transactions, such as dividend reinvestments, stock transfers among family members, and pension transactions, are excluded. I further restrict the insider trading transactions to those by officers and directors only, excluding those by non-officer employees who are unlikely to have an influence on corporate disclosure decisions.[8]

[8] My results all hold when I use CEOs' insider trades only or when I use the aggregate insider trades made by CEOs, CFOs, and chairmen of boards.

To focus on the aggregate influence of firm management team, I sum the purchases and sales by all managers of the same firm in the periods of interest.[9]

I draw the PBE disclosure data from Capital IQ. It maintains a team of over 600 analysts who collect and code key corporate developments from press releases and news outlets for all U.S. listed firms from the year 2002. My sample period starts in that year, and ends in 2012.[10] Since the Regulation Fair Disclosure was enforced in 2001 to prohibit firms from communicating private information to institutional investors, it is unlikely for these investors to be informed enough to front-run insider trades during my sample period. It thus provides a good setting in which to examine the opportunistic insider trades and strategic PBE disclosures.

Capital IQ has data on a variety of corporate development items, including earnings guidance, product announcements, and business expansion announcements. Product and business expansion announcements pertain to stand-alone public disclosures, which, in content, are exclusive of other types of corporate reporting and disclosures; this makes my empirical analysis relatively clean and not subject to systematic confounding effects induced by other concurrent disclosures. The PBE news announcements were all initiated by firms, with each announcement corresponding to a unique news content. I collected 85,535 disclosure observations that have ticker symbols. A firm might make multiple, distinct PBE news announcements at the same date, giving rise to duplicate disclosure observations. I remove these duplicate data, reducing the sample size to 78,106 observations. I then use ticker symbols to merge these disclosure data with the I/B/E/S-CRSP link table, and the sample reduces to 64,869 observations. Further, I require that observations have the necessary data available in the databases to construct the variables of interest for my empirical tests. This procedure yields the final sample that comprises 27,016 disclosure observations for 873 unique firms.[11] Panels A, B, and C of Table 1 tabulate the frequency of PBE disclosures by year, quarter, and industry, respectively. Firms in the chemical products, electronic equipment, computer equipment and services industries have the highest frequency of PBE disclosures. Panel A of Table 2 gives descriptive statistics of the variables used in the main tests.

3.2 Measures of the news content of PBE disclosures

Following Noe (1999), Cheng and Lo (2006), Brockman et al. (2008), Nichols (2010), Ge and Lennox (2011), and He (2018), among others, I use the stock market reaction to identify whether a disclosure conveys good or bad news to the market. Specifically, a PBE disclosure is classified as a good (bad) news disclosure if the cumulative abnormal stock returns over the 3-day window centered on the disclosure date are positive (negative).[12]

[9] For a given firm in a period, some insiders may be selling while others may be buying. In this case, insider sales (purchases) will be subtracted from insider purchases (sales) to reflect the net direction of insider purchases (sales) in that period.

[10] I obtain qualitatively the same results for all the related empirical tests, if the financial crisis period of 2007–2009 is excluded from my sample period. It ends in 2012 as I was limited from accessing the data on PBE disclosures.

[11] I obtain qualitatively identical results if I use firm-quarter observations, in place of disclosure-quarter observations, for the hypothesis tests. For the firm-quarter observations, only the last (or alternatively, the first) announcement of PBE plans is selected if an observation has multiple PBE plans announced on different dates during a fiscal quarter.

[12] Inferences from the results for the hypothesis tests remain unchanged if I use the top (bottom) third, or top (bottom) quartile, of the abnormal stock returns as the cutoff for the good (bad) news classification.

The cumulative abnormal returns are calculated based on the market model with an estimation period of [−181, −2] relative to the PBE disclosure date. In addition, I use an alternative estimation window, [−181, −2] plus [2, 52], to construct the measure for disclosure news, and obtain qualitatively identical results; this specification for the news measure, which accounts for a post-disclosure period for the estimation window, also assuages the reverse causality concern (discussed in Sect. 4.3). The dependent variable in my main empirical analyses is *Gbnews*, which equals 1 if a firm delivers a good news disclosure of PBE information, and equals 0 if a firm makes a bad-news PBE disclosure.[13] The mean value of *Gbnews*, reported in Table 2, indicates that 50.7% of the announcements of PBE plans pertain to good news disclosures. Suijs (2007, *p.*391) contends that "a firm may prefer not to disclose its private information if the firm is uncertain of investor response". Therefore, consistent with prior research (e.g., Cheng et al. 2013), my sample observations in the main tests are restricted to those that have disclosures of PBE plans.[14]

3.3 Measures of insider trading incentives

Insider trading opportunities motivate managers to be strategic to change their nonfinancial disclosure policies. As discussed in Sect. 2.3, if insiders wish for a high trading gain, they should trade shortly after disclosures. In such a scenario, the *ex post* trading intensity reflects the degree of managers' *ex ante* incentives to grab trading gain. Consistent with prior studies (e.g., Sivakumar and Waymire 1994; Noe 1999; Ke et al. 2003; Cheng et al. 2013), I focus on insider trades in the 30-day period after a disclosure, not only because delayed trading after a disclosure would reduce trading profits for insiders, but also because PBE news might not be withheld for long by managers. I obtain qualitatively the same results if I expand the window to be the 90-day period *post* PBE disclosures. Because the variables for insider trades are highly skewed, I use the logarithm transformation of insider trades for empirical tests.

To separate sale incentives from purchase incentives for a given firm in the periods of interest, I follow the related literature (e.g., Cheng et al. 2013) and define the insider trading variables as follows. *Insiderbuy* equals the natural logarithm of one plus the net insider purchases (i.e., the number of shares purchased minus the number of shares sold) over a 30-day window after a PBE disclosure, if a firm has a positive amount of net insider purchases over the 30-day window, and equals 0 otherwise. *Insidersell* equals the natural logarithm of one plus the net insider sales (i.e., the number of shares sold minus the number of shares purchased) over a 30-day period after a PBE disclosure, should a firm have a positive amount of net insider sales over the 30-day window, and equals 0 otherwise. As shown in Table 2, the mean value of *Insidersell* is significantly higher than the mean *Insiderbuy*, indicating a higher intensity of insider sales than that of insider purchases after PBE disclosures. Figure 1a plots the frequency of insider trades (*TInsiderbuy* and *TInsidersell*) for the whole population of

[13] The regression results still hold when the dependent variable is broken into the product-disclosure-only case and the business-expansion-disclosure-only case, respectively.

[14] There are two reasons why the main tests are conditioned on firms making a PBE disclosure over a fiscal quarter. First, by doing so, I alleviate the concern (covered in Sect. 4.3) that managers may not foresee exactly the price responses to a PBE disclosure, given that managers do not intend to disclose PBE news when they are uncertain about investor response to the news (Suijs, 2007). Second, the announcement returns used to capture the news content of PBE disclosures also encompass the "risk-reducing" effect of a disclosure (i.e., a decrease in perceived information asymmetry due to the incidence of a disclosure). Such "risk-reducing" effect would have been counterbalanced and dis-functioned in the regression analyses, if the regressions are run based on the disclosure sample only (He, 2018).

U.S. listed firms during my sample period. From the chart, we can see that, in general, insider sales occur far more frequently than insider purchases. Panel B of Table 2 provides additional summary statistics of insider trades for the whole population. *TInsiderbuy* (*TInsidersell*) has the mean value of 0.5245 (2.8644), which is significantly lower than the mean of *Insiderbuy* (*Insidersell*) for the disclosure sample. This suggests that the post-PBE-disclosure insider trades are more intense than the insider trades in general.

To reduce legal and reputational risks for informed insider trades, insiders might opt to make routine trades in place of non-routine trades to pretend to be non-opportunistic traders. In such a case, routine insider trades are still opportunistic (He et al. 2021). Therefore, I do not separate routine insider trades from non-routine insider trades in my main analysis. My results still hold for following Cohen et al. (2012) to restrict the measures of insider trades to non-routine trades only.

In addition, following Cheng et al. (2013), I use indicator variables to capture the existence of insider trades in the 30-day period after a PBE disclosure. The indicator variable for insider sales (*DInsidersell*) equals 1 if the amount of net insider sales is positive (i.e., insider sales are larger than insider purchases) and 0 otherwise. The indicator variable for insider purchases (*DInsiderbuy*) equals 1 if the amount of net insider purchases is positive and 0 otherwise. The use of this alternative specification of insider trading does not alter any inference drawn in the main empirical tests.[15]

Lastly, in the cases when corporate insiders trade their stocks in advance of releases of value-relevant news, stock returns can be used to measure the profitability of the insider trading, on the assumption that investors do not possess private information about the news and thus would not react to it prior to the disclosure of it. Nevertheless, my study concerns the insider trades taking place *after* strategic managerial disclosures. Specifically, insiders capitalize on investors' overreaction to the strategic PBE disclosures to profit from their stock trading. As both corporate insiders and outside investors trade on the PBE disclosures, stock returns after the disclosures would not capture the profits of the insider trades. Hence, as with the related literature (e.g., Cheng and Lo 2006; Cheng et al. 2013), I do not examine the insiders' trading profits in this study either.

4 Research design and empirical results

4.1 Univariate analysis of the hypotheses H1 and H2

Table 3 reports correlation matrix among the variables used in the baseline regression. The correlation between *Insiderbuy* (*Insidersell*) and *GBnews* is − 0.0287 (0.0471) and statistically significant at the 1% level, thus providing initial support for the hypothesis $H1_a$ ($H2_a$). Panel A (Panel B) of Table 4 reports the results of the two-sample tests of mean for *GBnews*; the two subsamples are partitioned by whether a given observation's amount of net insider purchases (net insider sales) is greater than 0. The mean value of *GBnews* for the subsample observations that have a positive amount of net insider purchases is 0.4512, significantly lower (t-stat. = 4.71) than the mean *GBnews* for the other subsample observations

[15] Using the indicator variables imposes no restriction on the specific form of the relationship between insider trades and PBE disclosures, thereby increasing the power of the tests. However, the use of the insider trading indicators ignores the effect of the magnitude of insider trades which is presumably proportional to the amount of trading gain and to the strength of insider trading incentives. Hence, I use the continuous variables in the main tests.

Table 1 The frequency of PBE disclosures

Panel A: *The frequency of PBE disclosures across years*

Years	Frequency	Percentage (%)
2002	1322	4.89
2003	1395	5.16
2004	1823	6.75
2005	2215	8.20
2006	2081	7.70
2007	2494	9.23
2008	3231	11.96
2009	3267	12.09
2010	3130	11.59
2011	3274	12.12
2012	2784	10.31

Panel B: *The frequency of PBE disclosures across quarters*

Quarters	Frequency	Percentage (%)
Spring (months 1–3)	7016	25.97
Summer (months 4–6)	6471	23.95
Autumn (months 7–9)	6926	25.64
Winter (months 10–12)	6603	24.44

Panel C: *The frequency of PBE disclosures across industries*

Industries (SIC codes)	Frequency	Percentage (%)
Oil and gas (13, 29)	240	0.89
Food products (20)	315	1.17
Paper and paper products (24–27)	62	0.23
Chemical products (28)	3413	12.63
Manufacturing (30–34)	243	0.90
Computer equipment and services (35, 73)	9144	33.85
Electronic equipment (36)	7424	27.48
Transportation (37, 39, 40–42, 44, 45)	883	3.27
Scientific instruments (38)	1687	6.24
Communications (48)	529	1.96
Electric, gas, and sanitary services (49)	197	0.73
Durable goods (50)	197	0.73
Retail (53, 54, 56, 57, 59)	560	2.07
Eating and drinking establishments (58)	236	0.87
Entertainment services (70, 78, 79)	95	0.35
Health (80)	59	0.22
Others	1732	6.41

Panel A (Panel B) reports the frequency of PBE disclosures across different years (quarters). Panel C presents the frequency of PBE disclosures across different industries; the industry classification is based on the first two digits of Standard Industrial Classification (SIC) codes. The observations are at the disclosure-quarter level and total 27,016 for the period 2002–2012.

Table 2 Descriptive statistics

Panel A: *Summary statistics of variables for the PBE-disclosures sample*								
Variables	Mean	SD	N	5%	25%	Median	75%	95%
Industrial Classification (SIC) codesG-Bnews	0.5070	0.5000	27,016	0	0	1	1	1
Insiderbuy	0.5676	2.2887	27,016	0	0	0	0	7.1253
Insidersell	3.4028	5.6889	27,016	0	0	5.3033	11.0360	13.5494
LagInsiderbuy	0.5227	2.1984	27,016	0	0	0	0	6.6821
LagInsidersell	5.5325	5.6720	27,016	0	0	5.9927	11.0096	13.5345
ChangInsider-buy	0.0448	2.9394	27,016	−0.6931	0	0	0	4.2195
ChangInsid-ersell	−0.0044	6.2912	27,016	−11.6333	−1.1270	0	1.0564	11.6845
DInsiderbuy	0.0619	0.2409	27,016	0	0	0	0	1
DInsidersell	0.5020	0.5000	27,016	0	0	1	1	1
Asue	0.5627	0.4961	27,016	0	0	1	1	1
Size	8.1301	2.2510	27,016	4.8379	6.4196	7.8342	9.7085	11.9351
Insti	0.7149	0.2158	27,016	0.3195	0.5878	0.7224	0.8704	0.9711
Abtradvol	4.95mil	194mil	27,016	−111mil	−7.10mil	−84,200	5.14mil	115mil
Qtrret	28.8454	1.23E+3	27,016	−1.1928	−0.3208	−0.0060	0.4902	9.4519
BM	0.4115	0.3734	27,016	0.0719	0.1972	0.3228	0.5489	1.0556
Roa	0.0037	0.0779	27,016	−0.0971	0.0017	0.0151	0.0283	0.0499
Litig	0.6460	0.4782	27,016	0	0	1	1	1
CapitalEx	0.0246	0.0343	27,016	0.0015	0.0066	0.0143	0.0293	0.0806
Debt	0.1890	0.6487	27,016	0	0	0.0241	0.1489	0.8089
Panel B: *Summary statistics of insider trading variables for the full sample*								
TInsiderbuy	0.5245	2.0802	91,228	0	0	0	0	6.6859
TInsidersell	2.8644	4.6381	91,228	0	0	0	7.9780	11.6918

Panel A tabulates descriptive statistics of the variables used in the regression analyses; the sample contains observations that have a product or business expansion disclosure during the period 2002–2012, and consists of 27,016 disclosure-quarter observations. *GBnews* equals 1 if the cumulative abnormal stock returns in the 3-day window centered on the PBE announcement date are positive, and 0 if the cumulative abnormal stock returns are negative. The cumulative abnormal returns are calculated using the market model with an estimation period of $[-181, -2]$ relative to the announcement date for a firm. *Insiderbuy* equals the natural logarithm of 1 plus the net insider purchases over the 30 days after a PBE disclosure if a firm has a positive amount of net insider purchases over the 30-day window; *Insiderbuy* equals 0 if the firm's net insider purchases over the 30-day window are negative or zero. *Insidersell* equals the natural logarithm of 1 plus the net insider sales over the 30 days after a PBE disclosure if a firm has a positive amount of net insider sales over the 30-day window; *Insidersell* equals 0 if the firm's net insider sales over the 30-day window are negative or zero. Other variables are defined in "Appendix 1". Panel B reports descriptive statistics of the insider trading variables; the sample comprises 91,228 firm-quarter observations, including observations that have a product or business expansion disclosure during the period 2002–2012, and those without the disclosure during the period. *TInsiderbuy* equals the natural logarithm of 1 plus the net insider purchases over the month ending at the end of a fiscal quarter if a firm has a positive amount of net insider purchases during the month; *TInsiderbuy* equals 0 if the firm's net insider purchases over the month are negative or zero. *TInsidersell* equals the natural logarithm of 1 plus the net insider sales over the month ending at the end of a fiscal quarter if a firm has a positive amount of net insider sales during the month; *TInsidersell* equals 0 if the firm's net insider sales over the month are negative or zero.

that have a zero or negative amount of net insider purchases, which is 0.5107. The mean of *GBnews* is significantly higher (t-stat. = 6.91) for observations that have a positive amount of net insider sales than for observations with a non-positive amount of net insider sales. Panel C compares *Insiderbuy* and *Insidersell* between the good-news disclosure sample and the bad-news disclosure sample. The mean value of *Insiderbuy* (*Insidersell*) is significantly lower (higher) for the good news sample than for the bad news sample [t-stat. = −4.59 (7.51)]. The mean difference in *Insiderbuy* (*Insidersell*) is −0.1281 (0.5194), which is equivalent to 22.57% (15.26%) of its overall sample mean and is economically significant. Thus, these univariate results are all consistent with the hypotheses $H1_a$ and $H2_a$.

Panel D reports a result for the mapping between PBE news and future earnings. The mean values of earnings for the future three fiscal years *post* the announcements of good PBE news are all significantly higher than those after the bad news announcements. This result lends support not only to the implications of PBE disclosures for long-term streams of a firm's future earnings, but also to the return-based classification of good *vis-à-vis* bad news for PBE disclosures.

4.2 Multivariate tests of the hypotheses H1 and H2

The baseline regression model used to test the hypotheses H1 and H2 is as follows:

$$\begin{aligned} GBnews_q = {} & \alpha_0 + \alpha_1 Insiderbuy_q (\text{or } Insidersell)_q + \alpha_2 Asue_{q-1} + \alpha_3 BM_{q-1} + \alpha_4 Size_{q-1} \\ & + \alpha_5 ROA_{q-1} + \alpha_6 Insti_{q-1} + \alpha_7 CapitalEx_{q-1} + \alpha_8 Debt_{q-1} + \alpha_9 Litigation_{q-1} \\ & + \alpha_3 industry{-}fixed{-}effects + \alpha_4 year{-}fixed{-}effects + \alpha_5 quarter{-}fixed{-}effects + \varepsilon \end{aligned} \tag{1}$$

The dependent variable is *GBnews*, an indicator variable equaling 1 (0) if a product or business expansion disclosure pertains to a good (bad) news disclosure, as defined previously. The key independent variable is *Insiderbuy* (*Insidersell*), which proxies for insiders' purchase (sale) incentives, as defined earlier. I put *Insiderbuy* and *Insidersell* in separate regressions because, by the variable construction, they are opposite in nature.[16] A logit regression is run for Model (1). If the hypothesis $H1_a$ ($H2_a$) holds, the coefficient on *Insiderbuy* (*Insidersell*) should be negative (positive) and statistically significant at a conventional level.

Following Nichols (2010) and He (2018), I control for earnings surprise (*Asue*), book-to-market ratio (*BM*), firm size (*Size*), return on assets (*ROA*), institutional stock ownership (*Insti*), capital expenditures (*CapitalEx*), financial leverage (*Debt*), and industry-level litigation risk (*Litigation*). *Asue* is an indicator variable for whether a firm's earnings surprise, measured by the actual earnings per share (henceforth, EPS) less the median consensus analyst forecast of EPS, is positive for the fiscal quarter. A positive earnings surprise (*Asue*) is expected to be associated with a higher incidence of a good news disclosure (*Gbnews*). Prior research (e.g., Ball and Shivakumar 2005; Francis and Martin 2010; Jayaraman and Shivakumar 2013) documents that conservative corporate reporting and disclosures curb value-destroying investment and financing activities. Therefore, firms with high institutional stock ownership (high financial leverage), subject to higher monitoring from institutional investors (creditors), are likely to be conservative in their disclosures. In

[16] I obtain qualitatively the same multivariate results if I put *insidebuy* and *insidersell* in the same regressions.

a similar vein, larger firms are more mature in operating their business and hence are likely to be more conservative in their corporate disclosures. Hence, *Insti*, *Debt*, and *Size* should be related negatively to *GBnews*. Firms with good performance are likely to have more good news. Thus, *ROA* is expected to be positively associated with *GBnews*. Higher capital expenditures (*CapitalEx*) imply more promising investment opportunities for a firm and are thus expected to be related to a higher incidence of a good news disclosure (*GBnews*).

I further control for abnormal trading volume (*Abtradvol*) and abnormal stock returns (*Qtrret*) to account for the impact of potential fundamental-related events on corporate disclosures.[17] The controls of *Abtradvol* and *Qtrret* also mitigate the potential correlated-omitted-variables bias induced by events that affect firm fundamentals. All the control variables are constructed for the fiscal quarter that precedes the PBE announcement quarter, and are defined in "Appendix 1." Finally, I include industry-, year-, and quarter-fixed effects in the regression, and cluster the standard errors of the coefficients by firm to control for possible time-series correlations of residuals within firms (Petersen 2009).[18]

Table 5 reports the logit regression results for the effect of insider trading incentives on PBE disclosures. The coefficient for *Insiderbuy* is statistically significant at the 1% level with the expected negative sign. This result indicates that the incidence of a bad news disclosure on PBE information, relative to that of a good-news PBE disclosure, is positively correlated with insider purchases made shortly after the disclosure. Such evidence suggests that, when insiders plan to purchase shares from the stock market, they are more likely to disclose bad news on PBE information beforehand, which is consistent with the hypothesis $H1_a$.[19]

The coefficient on *Insidersell* is positive and statistically significant, consistent with the hypothesis $H2_a$. Thus, insiders tend to disclose good news on PBE information before selling shares. This result is in contrast with the prior research (e.g., Cheng and Lo 2006) which finds no evidence that insiders provide good news earnings forecasts before selling shares. Indeed, insider sales after a good news disclosure are subject to high legal jeopardy, because the litigation risk associated with insider sales is amplified in good-news earnings forecasts that, in themselves, entail high disclosure risk. However, the trading risk for insider sales might not be manifested in nonfinancial disclosures which entail relatively low disclosure risk. Thus, the costs associated with a good-news PBE disclosure being made before insider selling are likely to be perceived by insiders as being lower than the expected trading gain. This explains why I find a high incidence of good-news PBE disclosures made before insider sales.

[17] Alternatively, I exclude observations that have an announcement of equity issuance, merger, acquisition, or stock repurchase over the PBE announcement quarter, and still obtain qualitatively the same results for the hypothesis tests.

[18] My results remain qualitatively identical if I cluster the standard errors by disclosure. I also do a robustness check by winsorizing all the continuous variables at the 1st and 99th percentiles for the regression analyses, and obtain qualitatively the same results. That done, one drawback of the winsorization is that it might undermine the economic implications of variables. Accounting for this, my main (robustness) tests are based on the samples that are not (are) winsorized.

[19] Insider purchases can be alternatively attributed to managers' signaling of good future prospect for their firm. However, if managers *genuinely* aim at such signaling, they should avoid making stock purchases right after a PBE disclosure, since such stock purchases can be suspected as opportunistic rather than for the signaling purpose. Therefore, the results for the hypothesis $H1_a$ are less likely to be alternatively driven by the managers' signaling via stock purchases.

Baghai et al. (2014) argue that it is more straightforward to interpret the economic significance of results based on OLS models.[20] Thus, I also apply OLS regression to Model (1) using a continuous variable, *car*, as the dependent variable. *car* is the cumulative abnormal stock returns surrounding a PBE disclosure and is as defined in Sect. 3.2. The regression results (not tabulated) are consistent with the hypothesis $H1_a$ ($H2_a$): The coefficient on *Insiderbuy* (*Insidersell*) is negative (positive) and statistically significant (t-stat. = −4.08 (4.72)). A one-standard-deviation increase in *Insiderbuy* (*Insidersell*) is associated with an increase in *car* by 0.00071 (0.00262), which accounts for around 23.28% (85.90%) of the sample mean of *car* and is thus economically significant.

Overall, my baseline regression results are consistent with the notion that managers manipulate the timing and selectivity of PBE disclosures to fulfil self-serving trading incentives. Nonetheless, the baseline results are inconsistent with, and thereby rule out, the possibility that managers trade in response to plausible market under-reactions to PBE disclosures, because under this under-reaction possibility, managers should buy (sell) shares, rather than sell (buy) shares, after the good (bad) news disclosures.

4.3 Control for endogeneity

The idea behind the hypothesis tests is to investigate how insider trading incentives shape nonfinancial disclosure strategies. The causality flow runs from trading motives to disclosures, where the former is proxied empirically by insider trades after PBE disclosures. But there might be two main sources of endogeneity in my research context. The first is that both PBE disclosures and insider trades are simultaneously driven by some unobserved firm characteristic(s). The second type of potential endogeneity is reverse causality. In particular, more insider purchases (sales) occurring after a bad (good) news PBE disclosure can indicate either one or both of the following: (1) Managers' incentives to buy (sell) shares motivate a bad (good) news disclosure, as hypothesized in $H1_a$ ($H2_a$); (2) When stock price decreases (increases) after a bad (good) news disclosure, managers buy (sell) shares in response to the decreased (increased) stock price. In the latter case, the insider trading can be regarded as a passive response to disclosure choices. As such, reverse causality arises in a way that disclosures induce insider trades.

Because strategic PBE disclosures and insider trading are made in conjunction within a short window, identification of exogenous shocks (especially enforcements of insider trading regulations that apply simultaneously and equally to insider purchases and insider sales) to conduct a natural experiment is unlikely to address the endogeneity issues in my setting, and thus I seek other approaches. To address the first type of endogeneity, I use a firm-fixed-effects model and a reduced- form difference-in-differences specification. To cope with both the first and second types of endogeneity, I follow Cheng and Lo (2006) and Cheng et al. (2013) to employ a two-stage instrumental-variables regression technique, and importantly, also conduct a falsification test. The remainder of this section discusses each of the foregoing approaches and the associated results.

[20] Of the whole population of U.S. listed firms, those that have insider trades account for a small proportion. It is thus difficult to meaningfully interpret the economic significance of the effect of insider trades, especially in a logit or probit regression analysis. For this reason, some prior studies (e.g., Cheng et al., 2013) on insider trading do not report the economic significance of their results.

Fig. 1 **a** Plots the frequency of insider trades (*TInsiderbuy* and *TInsidersell*) for the whole population of listed firms that consists of 91,228 firm-quarter observations. The x axis presents the values of *TInsiderbuy* and *TInsidersell*, respectively. Of the 91,228 observations, 85,333 (65,181) observations have *TInsiderbuy* (*TInsidersell*) equal to 0. *TInsiderbuy* and *TInsidersell* are defined in "Appendix 1". **b**, **c** plots the frequency of insider purchases (insider sales) that surround bad (good) news PBE disclosures for the disclosure sample that comprises 27,016 observations; the x axis presents the values of *Insiderbuy* and *LagInsiderbuy* (*Insidersell* and *LagInsidersell*), respectively. There are 25,345 (25,465) observations with *Insiderbuy* (*LagInsiderbuy*) equal to 0, and 13,453 (13,375) observations with *Insidersell* (*LagInsidersell*) equal to 0. *Insiderbuy*, *LagInsiderbuy*, *Insidersell*, and *LagInsidersell* are defined in "Appendix 1". Value of variables represent the amount of insider trades ▶

4.3.1 Firm-fixed-effects logit regression and additional controls

The firm-fixed-effects model is widely used in empirical research to control for cross-sectional heterogeneity and to mitigate the problem of endogeneity (Wooldridge 2000; Amir et al. 2016; Swanquist and Whited 2018). An effective firm-fixed-effects model requires that (1) unobservable firm characteristics, which affect both PBE disclosures and insider trades, are time-invariant, and that (2) both the dependent variable (*GBnews*) and the key independent variables (*Insidersell* and *Insiderbuy*) display sufficient time-series variation. Columns (1) and (2) of Table 6 presents the firm-fixed-effects logit regression results, which are qualitatively the same as those reported in Table 5. In an additional analysis, I also control for board independence (*Indpboard*) and audit committee independence (*Indpaudit*). Both variables are constructed based on the data from the Institutional

(c) *Net insider sales around good-news PBE disclosures*

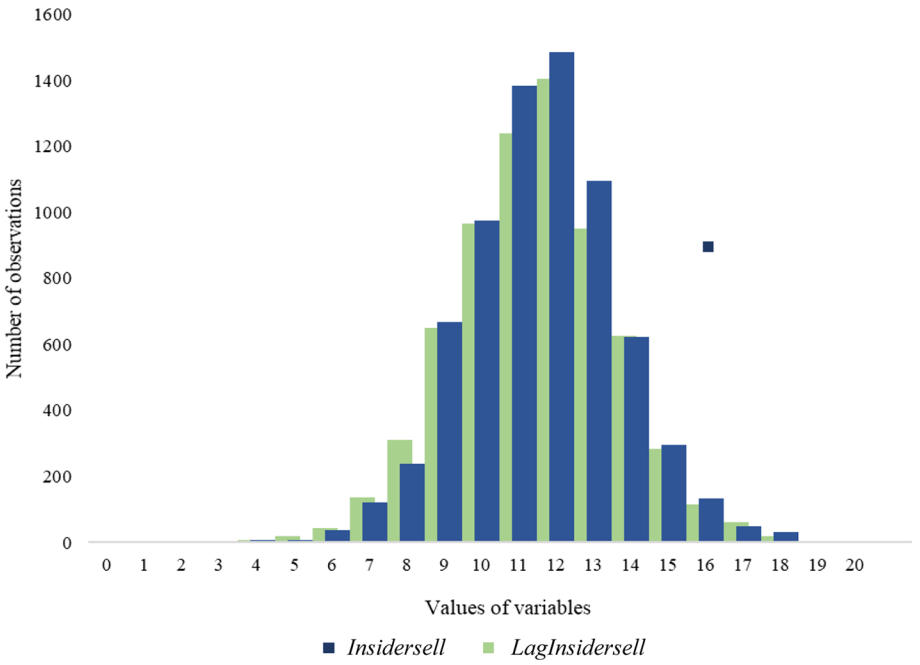
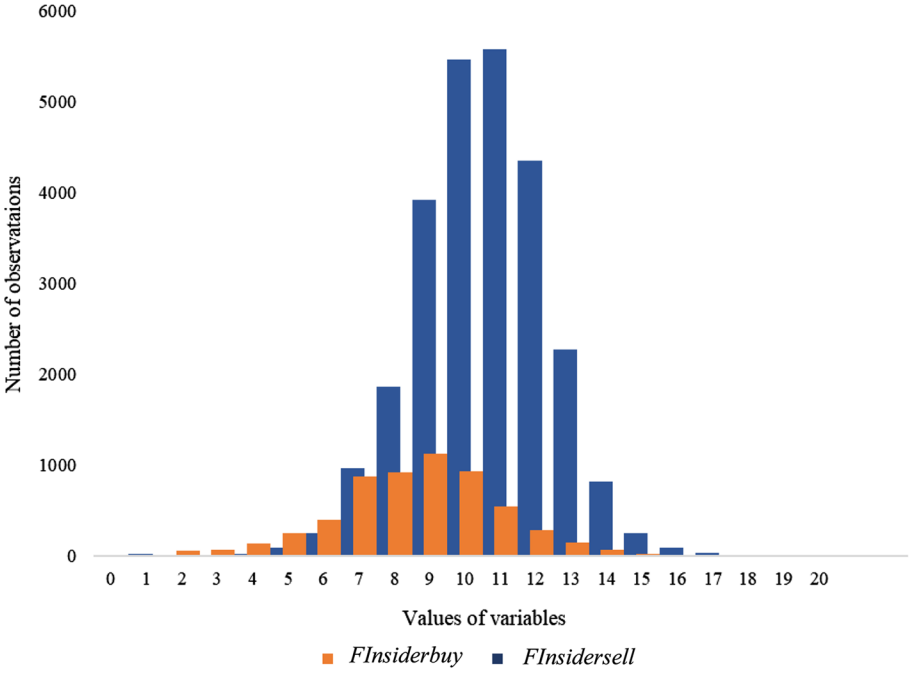
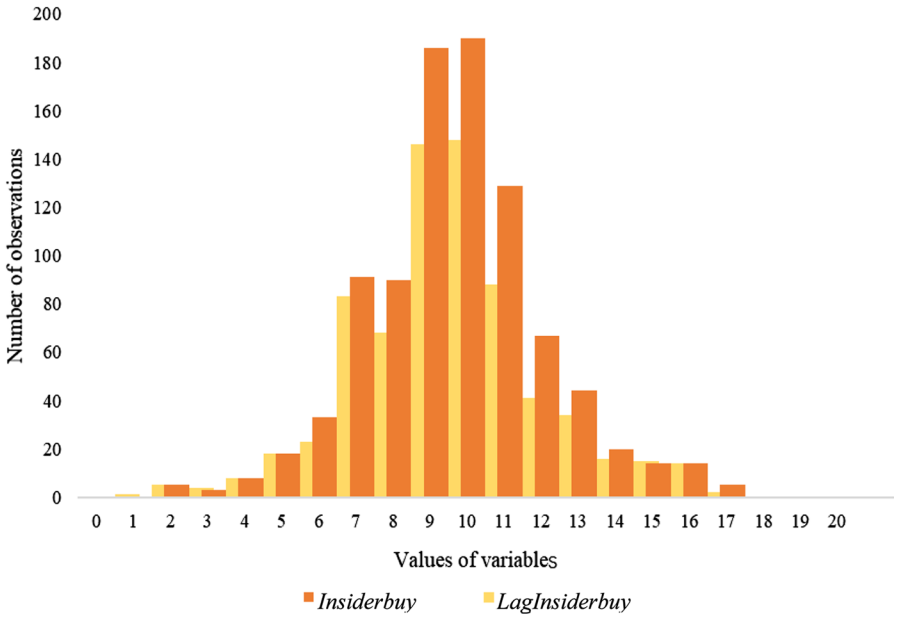


Fig. 1 (continued)

(a) *Insider trades for the population of U.S. listed firms*



(b) *Net insider purchases around bad-news PBE disclosures*



Shareholders Services (ISS) database which covers only S&P 1500 firms after the year

2007. These additional controls thus reduce our sample to 10,038 observations, but still lead to qualitatively the same results. They are reported in Column (3) ((4)) of Table 6: the coefficient on *Insidersell* (*Insiderbuy*) is 0.0180 (−0.0303), with t-stat. equal to 4.23 (−3.21).

4.3.2 Reduced-form difference-in-differences approach

I perform a reduced-form difference-in-differences specification where the key independent variables in Model (1) are replaced with the variables for change in insider trades around a PBE disclosure (namely, *ΔInsidersell* and *ΔInsiderbuy*, defined in "Appendix 1"). The underlying control sample for the change specification comprises the observations that do not have any insider trade surrounding PBE disclosures; *ΔInsidersell* and *ΔInsiderbuy* would equal zero for these observations. The reduced-form difference-in-differences approach controls for firm-fixed effects, executive-fixed effects, and macro- events that drive both insider trades and PBE disclosures, thereby alleviating potential correlated-omitted-variable(s) bias.

Table 7 shows the results for the reduced-form difference-in-differences specification. *ΔInsiderbuy* has highly significant coefficients in the predicted negative sign. This indicates that the incidence of a bad news disclosure of PBE information is associated with an increase in insider purchases, consistent with the hypothesis $H1_a$. The coefficients for *ΔInsidersell* are positive and statistically significant, so insiders' incentives to increase stock sales induce a higher likelihood of a good-news PBE disclosure, which lends support to the hypothesis $H2_a$. In line with these regression results, Fig. 1b, c reveals that the frequency of net insider purchases (net insider sales) after bad (good) news PBE disclosures is higher than that before the disclosures. Collectively, these results elicit the same inferences as do the results in Table 5, substantiating that the main results are insensitive to correcting for potential correlated-omitted-variable(s) bias.

4.3.3 Two-stage instrumental-variables regression

Managers' anticipation of stock market reactions to PBE disclosures might deviate from the actual observed market reactions. In such a case, insiders would have to adjust their stock trades in a way that deviates from the trades that had been intended before PBE disclosures. This gives rise to another endogeneity concern: measurement errors, and specifically, that the post-PBE-disclosure insider trades do not accurately capture the *ex ante* insider-trading incentives. Such type of endogeneity also potentially plagues prior studies (e.g., Cheng and Lo 2006; Rogers 2008; Cheng et al. 2013) that explore post-disclosure insider trades, but had been addressed in their two-stage instrumental-variables regression analysis. In line with prior studies, I also adopt such a regression technique, and thereby address the endogeneity attributed to correlated-omitted-variable(s) bias, measurement errors, and reverse causality. The regression model's effectiveness in addressing the endogeneity problems depends on the validity of instrumental variables (Larcker and Rusticus 2010). A valid instrumental variable should be directly related to the endogenous key independent variables (*Insiderbuy* and *Insidersell*) but not affect the dependent variable (*GBnews*), except indirectly through the endogenous independent variables (*Insiderbuy* and *Insidersell*).

I use two instrumental variables for the two-stage regression. The first is the number of stock options granted over a three-year period that ends at the beginning of the PBE

announcement quarter (*OptionG*).[21] The more stock options that have been granted in the previous three years, the more likely it is that managers would trade during the PBE announcement quarter. So *OptionG* is expected to be positively correlated with *Insiderbuy* and *Insidersell*. However, *OptionG* is unlikely to have a direct impact on the subsequent disclosure news (Edmans et al. 2018), making it a valid instrumental variable. The second instrumental variable is stock market liquidity (*LIQ*), measured by the daily relative effective spreads averaged over the PBE announcement quarter and multiplied by −1, as per Fang et al. (2009). The more liquid the stocks, the easier it is for insiders to trade their shares. So, stock liquidity is expected to be positively associated with insider trades. Although it may have an influence on the incidence of a PBE disclosure, it should not have a direct impact on the likelihood of a good-news PBE disclosure relative to that of a bad-news PBE disclosure (*Gbnews*), and hence is used as the instrument as well.

In the first-stage regression, the variable for actual insider trades is regressed on the instrumental variables and on a set of control variables as included in Model (1). The predicted insider trading obtained from the first-stage regression is estimated based on the past public information (as reflected in the control variables) for the fiscal quarter preceding the PBE announcement quarter, and thus, is unlikely to contain private information known only to firm management (Cheng and Lo 2006). As such, the predicted insider trading, which replaces the actual insider trading in the second-stage regression, will not be affected by PBE disclosures, mitigating the potential endogeneity bias caused by reverse causality. Table 8 presents the two-stage instrumental regression results. The fitted *Insiderbuy* in the second-step probit regression takes on a significantly negative coefficient; the coefficient for the fitted *Insidersell* is positive and statistically significant at the 1% level. These results suggest that insiders are inclined to disclose bad (good) news on PBE before buying (selling) shares, and thus support the hypothesis $H1_a$ ($H2_a$). Overall, the results in Table 8 corroborate that the results reported in Table 5 are not driven by the potential reverse causality or omitted variable(s).

4.3.4 A falsification test

I further conduct a falsification test to rule out the alternative explanation ascribed to the reverse causality or correlated-omitted variable(s). Specifically, I randomly fake 27,016 "event" dates for my disclosure sample in the non-PBE disclosure period, which is defined as the period outside of [−30, 30] relative to the PBE disclosure date. I then code the good *vis-à-vis* bad news events based on the cumulative abnormal stock returns over the three-day window centered on the fake event date. As shown in Table 9, I find no association between my coded good *vis-à-vis* bad news and subsequent insider trades, thereby refuting the passive trading explanation and reverse causality possibility. Such a non-association also suggests that my results reported in Table 5 are not attributed to correlated- omitted variable(s).

In all, my main results are reasonably robust to controlling for potential endogeneity. Studies on insider trades over narrow windows surrounding corporate disclosures, as compared to long windows, are less subject to the endogeneity ascribed to correlated omitted

[21] As a robustness check, I measure the option grant variable in a shorter window, i.e., over a two-year (or one-year) period ending at the beginning of the PBE announcement quarter, and obtain qualitatively the same results for the two-stage regression estimations.

Table 3 Spearman correlation matrix

	GBnew	*Insiderbuy*	*Insidersell*	*Asue*	*Size*	*Insti*	*Abtradvol*	*Qtrret*	*BM*	*Roa*	*Litig*	*CapitalEx*	*Debt*
GBnews	1												
Insiderbuy	−0.0287 (0.021)***	1											
Insidersell	0.0471 (0.001)***	−0.2383 (0.000)***	1										
Asue	0.0250 (0.001)***	−0.0976 (0.000)***	0.1797 (0.000)***	1									
Size	0.0015 (0.808)	−0.0990 (0.000)***	0.3411 (0.000)***	0.2360 (0.000)***	1								
Insti	−0.0065 (0.282)	−0.0566 (0.000)***	0.0689 (0.000)***	0.0980 (0.000)***	0.0392 (0.000)***	1							
Abtradvol	−0.0157 (0.010)***	−0.0142 (0.019)**	0.0305 (0.003)***	0.0075 (0.221)	−0.0065 (0.285)	0.0119 (0.051)*	1						
Qtrret	−0.0056 (0.358)	−0.0563 (0.000)***	0.1006 (0.000)***	0.0934 (0.000)***	0.0952 (0.000)***	0.0166 (0.006)***	0.1897 (0.000)***	1					
BM	0.0027 (0.655)	0.0558 (0.000)***	−0.2486 (0.000)***	−0.0697 (0.000)***	−0.3580 (0.000)***	0.1398 (0.000)***	-−0.0999 (0.000)***	−0.1717 (0.000)***	1				
Roa	0.0107 (0.079)*	−0.0962 (0.000)***	0.2726 (0.000)***	0.2919 (0.000)***	0.5128 (0.000)***	0.0496 (0.000)***	0.0291 (0.000)***	0.0781 (0.000)***	−0.3147 (0.000)***	1			
Litig	−0.0013 (0.827)	−0.0158 (0.009)***	0.0984 (0.046)**	0.0717 (0.000)***	0.0371 (0.000)***	0.0487 (0.000)***	−−0.0145 (0.017)**	0.0222 (0.000)***	−0.0987 (0.000)***	−0.0546 (0.000)***	1		
CapitalEx	0.0002 (0.969)	0.0053 (0.381)	0.0386 (0.000)***	−0.0564 (0.000)***	0.0934 (0.000)***	0.0471 (0.000)***	−−0.0438 (0.000)***	−0.0064 (0.295)	−0.0472 (0.000)***	0.1070 (0.000)***	−0.1139 (0.000)***	1	
Debt	0.0027 (0.663)	0.0545 (0.000)***	−0.0869 (0.000)***	−0.0080 (0.191)	0.1917 (0.000)***	0.0548 (0.000)***	−−0.0463 (0.000)***	−0.0281 (0.000)***	0.1067 (0.000)***	−0.1130 (0.001)***	−0.2782 (0.000)***	0.0882 (0.000)***	1

This table presents the Spearman correlations among the variables used in the baseline regression. 27,016 observations are involved in the correlation tests. All the variables are defined in "Appendix 1". The figures in parentheses are the *p* values for the Spearman correlations. ***, **, *Denote the two-tailed statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 4 Univariate analysis

Panel A: *Comparison of PBE news by whether net insider purchases are greater than 0*					
Variable	*Insiderbuy* > 0		*Insiderbuy* = 0		Mean difference (t-stat.)
	Mean	N_1	Mean	N_0	
GBnews	0.4512	1671	0.5107	25,345	−0.0595 (−4.71)***
Panel B: *Comparison of PBE news by whether net insider sales are greater than 0*					
Variable	*Insidersell* > 0		*Insidersell* = 0		Mean difference (t-stat.)
	Mean	N_1	Mean	N_0	
GBnews	0.5279	13,563	0.4859	13,453	0.0420 (6.91)***
Panel C: *Comparison of net insider sales and net insider purchases by PBE news*					
Variables	*GBnews* = 1 (good news)		*GBnews* = 0 (bad news)		Mean difference (t-stat.)
	Mean	N_1	Mean	N_0	
Insiderbuy	0.5044	13,697	0.6325	13,319	−0.1281 (4.59)***
Insidersell	5.7842	13,697	5.2648	13,319	0.5194 (7.51)***
Panel D: *Comparison of future earnings by PBE news*					
Variables	*GBnews* = 1 (good news)		*GBnews* = 0 (bad news)		Mean difference (t-stat.)
	Mean	N_1	Mean	N_0	
$AnnualEarnings_{t+1}$	2192.9	9494	1940.4	9001	252.5 (3.40)***
$AnnualEarnings_{t+2}$	2430.0	10,161	2180.5	9667	249.5 (3.11)***
$AnnualEarnings_{t+3}$	2612.3	10,590	2366.9	10,173	245.4 (2.91)***

This table reports the results of the two-sample tests of mean. Panel A (Panel B) compares the mean value of *GBnews* for the two subsamples that are partitioned by whether the observations' net insider purchases (net insider sales) are greater than 0. N_1 (N_0) in Panel A refers to the number of disclosure observations that have net insider purchases greater than (equal to or lower than) 0. N_1 (N_0) in Panel B refers to the number of disclosure observations that have net insider sales greater than (equal to or lower than) 0. Panel C compares the mean values of *Insiderbuy* (and *Insidersell*) for the two subsamples that are partitioned by whether the observations' PBE disclosures pertain to a good-news disclosure or a bad-news disclosure. Panel D compares the mean values of income before extraordinary items for the future three fiscal years (i.e., $AnnualEarnings_{t+1}$, $AnnualEarnings_{t+2}$, and $AnnualEarnings_{t+3}$, respectively) beyond the PBE announcement year. N_1 (N_0) in Panel C and Panel D refers to the number of observations that have a good (bad) news PBE disclosures (i.e., *GBnews* = 1 (*GBnews* = 0)). *GBnews, Insiderbuy*, and *Insidersell* are defined in "Appendix 1".

variable(s) (e.g., Huddart et al. 2007). Furthermore, if disclosures followed by insider trades are driven by the omitted variable(s), we should have expected good (bad) news disclosure accompanied by insider purchases (sales). This, however, is opposite to the prediction in the hypothesis $H1_a$ ($H2_a$). Hence, the omitted-variable(s) problem, even if it existed, would create bias only in favor of the hypotheses $H1_a$ and $H2_a$. Regarding reverse causality, as argued by Cheng et al. (2013), it would not be serious when disclosure news is measured by abnormal stock returns. Furthermore, a large body of literature (e.g., Chen et al. 2007; Bakke and Whited 2010; Dutta and Reichelstein 2003, 2005; Foucault and Fresard 2012, 2014; Loureiro and Taboada 2015; Zuo 2016) provides evidence that managers account for information in stock prices and actively incorporate it into their investment and disclosure

Table 5 Tests of the hypotheses H1 and H2 regarding the effect of inside trading incentives on PBE disclosures

Variables	Pred.sign	*GBnews*	
		(1)	(2)
Intercept	?	0.0940 (0.42)	0.0430 (0.19)
Insiderbuy	**?**	**−0.0260 (−4.69)*** **	
Insidersell	**?**		**0.0202 (7.62)*** **
Asue	+	0.1067 (3.64)***	0.0941 (3.16)***
Size	−	0.0016 (0.21)	−0.0082 (−1.17)
Insti	−	−0.1418 (−2.31)**	−0.1641 (−2.64)***
Abtradvol	?	−0.0000 (−0.46)	−0.0000 (−0.47)
Qtrret	+	0.0000 (3.52)***	0.0000 (3.80)***
BM	?	0.0581 (1.71)*	0.0905 (2.65)***
Roa	+	−0.1235 (−0.65)	−0.1537 (−0.81)
Litig	?	−0.0223 (−0.62)	−0.0452 (−1.24)
CapitalEx	+	0.2671 (0.69)	0.1045 (0.28)
Debt	−	−0.0040 (−0.27)	0.0022 (0.14)
Industry-fixed effects	?	Included	Included
Year-fixed effects	?	Included	Included
Quarter-fixed effects	?	Included	Included
Observations		27,016	27,016
Wald χ^2		168.47	169.94

The rows highlighted in bold indicate the results for the key independent variables.

This table reports the logit regression results for the tests of the hypotheses H1 and H2. The sample period ranges from 2002 to 2012. The dependent variable is *GBnews*. The key independent variables are *Insiderbuy* and *Insidersell*, respectively. All the variables are defined in "Appendix 1". Year, quarter, and industry dummies are included in the regressions but not reported for brevity. The industry dummies are constructed from the first two digits of SIC codes. The *t* statistics in parentheses are based on the robust standard errors clustered by firm. The p values for Wald χ^2 are close to zero. ***, **, * denote the two-tailed statistical significance at the 1%, 5%, and 10% levels, respectively.

decisions. Since managers care about and keep learning from stock prices, they should have a sense of how their PBE disclosures might impact stock prices. As such, insider trades that occur shortly after disclosures are unlikely to be attributed to managers' passive

or naïve response to their own disclosure choices. In cases when insiders do not manipulate PBE disclosures and are just exploiting favorable stock-price movements for trading after the disclosures, they should be buying (selling) shares, rather than selling (buying) shares, after good (bad) news PBE disclosures. Such trading behavior is inconsistent with, and thus would not alternatively explain, our baseline regression results. All in all, both my robustness tests and conceptual arguments refute the possibility that my main results are driven by endogeneity.

4.4 Cross-sectional analysis of the relation between insider trading incentives and PBE disclosures

Prior research (e.g., Holthausen and Verrecchia 1988; Teoh and Wong 1993; Veenman 2002; Khan and Lu 2012; Lu et al. 2014) shows that value-relevant corporate news triggers greater revisions of investors' prior expectations about firm fundamentals when prior information is more uncertain and prior beliefs are more divergent. While PBE disclosures may reduce outsiders' uncertainty about corporate PBE activities (He et al. 2019), the extent to which the perceived uncertainty is resolved will be larger when investors' prior expectations are more uncertain and more diversified. In such a case, the expected price impact of PBE disclosures will be greater, and investors are likely to overreact to the strategic PBE disclosures to a larger extent. Accordingly, I predict that the strategic PBE disclosures made for opportunistic trades will be more pronounced. To test this, I use analyst forecast dispersion as the proxy for the expected price influence of PBE disclosures. The higher the analyst forecast dispersion, the greater the investors' uncertainty and/or divergence about firm fundamentals (Barron 1995; Bamber et al. 1997; Hong and Stein 2007), and thus the larger the expected price impact of PBE disclosures, as per prior studies (e.g., Veenman 2002; Khan and Lu 2012). I partition my sample into two subsamples based on the sample median of analyst forecast dispersion (namely, *Disper*, which is defined in "Appendix 1"), and estimate Model (1) separately for these two subsamples.

Table 10 reports the results. The coefficient on *Insiderbuy* is negative and statistically significant at the 1% level for the high-*Disper* subsample but is not statistically significant for the low-*Disper* subsample. The coefficient on *Insidersell* is incrementally more positive in the high-*Disper* subsample relative to the low-*Disper* subsample; the difference in the coefficients for *Insidersell* amounts to 0.0164 and is statistically significant ($p = 0.0272$). These results suggest that managers have stronger incentives to make strategic trades and PBE disclosures if *ex ante*, they feel that their disclosures would trigger stronger stock price reactions. Such evidence and inference reinforce my earlier view that managers are able to foresee the market reactions to PBE disclosures.

To explore insiders' ability to pursue the strategic disclosure and trading behavior, I also examine whether my main results vary with CEO power. As with Ke et al. (2019), I define CEO power as strong, if (1) the CEO is the chairman of the board and (2) the proportion of independent directors on the board is below the sample median. When a CEO is more powerful, s/he will be more able to make, and convince other managers of, the PBE disclosure decisions beneficial to trading incentives. Thus, such strategic behavior would likely be more pronounced. To test this conjecture, I divide my sample into two subsamples based on whether CEO power is strong, and then run Model (1) on both subsamples. The data used for constructing the CEO power variable are obtained from the Institutional Shareholders Services (ISS) database which covers S&P 1500 firms for the period starting from the year 2007.

Table 11 reports the results. The coefficient on *Insiderbuy* is negative and highly significant in the high-CEO-power subsample but is only marginally significant in the low-CEO-power subsample. *Insidersell* takes on a significantly positive coefficient in the high-CEO-power subsample, but a statistically nonsignificant coefficient in the low-CEO-power subsample. Together, these results suggest that managers' strategic PBE disclosures made for purpose of insider trades are more evident for firms with strong CEO power.

Due to the availability of PBE disclosure data, my sample period starts from the year 2002, i.e., after the enforcement of Regulation Fair Disclosure in 2001. Under this regulation, firms are prohibited from conveying private information to institutional investors. Therefore, it is interesting to see whether, in the absence of access to management's private information, institutional investors are still able to effectively monitor insiders in a way that curbs opportunistic insider trades and avoids losses from such trades. To offer insights into this issue, I test whether my main results vary across firms with high *vis-à-vis* low institutional stock holdings. To this end, I split my sample into two subsamples based on the sample median of institutional ownership (*Insti*), and then estimate Model (1) separately for the two subsamples.

Table 12 reports the results. While the coefficients for *Insiderbuy* (*Insidersell*) are negative and statistically significant at the 1% level for both the high-*Insti* subsample and the low-*Insti* subsample, the difference in the coefficients on *Insiderbuy* (*Insidersell*), which amounts to 0.0151 (0.0011), is statistically nonsignificant [$p = 0.1826$ (0.8323)]. This suggests that the strategic PBE disclosures made for opportunistic insider trades are equally likely for firms with high institutional stock ownership *vis-à-vis* those with low institutional ownership. This result is inconsistent with the proposition that institutional investors play an effective monitoring role in curbing opportunistic insider trades and strategic disclosures. However, it is consistent with the notion that insiders' exploitation of disclosure opportunities for personal trading gain is at the expense of investors that are inclusive of institutional investors. Such an inference reconciles with some prior research (Glosten and Milgrom 1985; Kyle 1985; Sias and Whidbee 2010; Indjejikian et al. 2014) which argues that opportunistic insider trades are to the detriment of investors including sophisticated, yet uninformed, investors.

4.5 Self-selection of whether to voluntarily provide a PBE disclosure

The hypothesis tests are conditioned on management's decisions to voluntarily disclose PBE information. This might give rise to sample selection bias because observations that have insider trades but do not have a PBE disclosure are omitted from the regression analyses. The intensity of insider trades might differ between the PBE-disclosure sample and non-PBE-disclosure sample. To account for this possibility, I further control for industry-level insider trades (*Industry_Insiderbuy* (*Industry_Insidersell*)), which are measured by the average of net insider purchases (sales) made by the same- industry corporate insiders during the 30-day period after a PBE disclosure. Such a control serves to mitigate the impact of the potential sample selection bias on the coefficient estimates for *Insiderbuy* and *Insidersell*. Results are reported in Panel A of Table 13 and remain qualitatively the same in support of the hypotheses $H1_a$ and $H2_a$.

I also employ a two-stage Heckman probit regression model to tackle the potential sample selectivity problem. The first-stage regression is modeled by a probit regression of management's decision to voluntarily make a PBE disclosure, where the dependent variable is the incidence of a PBE disclosure (namely, *Inci*, which is defined in "Appendix

Table 6 Firm-fixed-effects logit regression and additional controls for the tests of the hypotheses H1 and H2

Variables	Pred.sign	*GBnews*			
		(1)	(2)	(3)	(4)
Intercept	?	1.9476 (2.55)**	2.0154 (2.65)***	−0.2902 (0.89)	−0.4287 (−1.30)
Insiderbuy	**?**	**−0.0267 (−4.29)*** **		**−0.0303 (−3.21)*** **	
Insidersell	**?**		**0.0236 (7.68)*** **		**0.0180 (4.23)*** **
Asue	+	0.1021 (3.22)***	0.0874 (2.70)***	0.0524 (0.96)	0.0453 (0.82)
Size	−	−0.1021 (−2.52)**	−0.1201 (−2.90)***	0.0144 (0.86)	0.0076 (0.45)
Insti	−	−0.0820 (−0.51)	−0.1096 (−0.68)	−0.1958 (−1.32)	−0.2056 (−1.40)
Abtradvol	?	−0.0000 (−0.99)	−0.0000 (−0.87)	−0.0000 (−0.30)	−0.0000 (−0.31)
Qtrret	+	0.0000 (3.22)***	0.0000 (3.54)***	−0.0000 (−1.21)	−0.0000 (−1.07)
BM	?	0.1439 (2.61)***	0.1668 (3.01)***	0.1006 (1.21)	0.1317 (1.61)
Roa	+	0.1041 (0.41)	0.0578 (0.23)	−0.6111 (−0.95)	−0.7276 (−1.14)
CapitalEx	+	0.6597 (1.34)	0.5341 (1.09)	0.9880 (1.12)	0.8680 (0.99)
Debt	−	0.0160 (0.37)	0.0238 (0.54)	0.0489 (0.80)	0.0675 (1.11)
Litig	?			0.0483 (0.69)	0.0153 (0.21)
Indpboard	?			−0.0697 (−0.28)	−0.0027 (−0.01)
Indpaudit	?			−0.1549 (−0.70)	−0.1001 (−0.44)
Industry-fixed effects	?			Included	Included
Firm-fixed effects	?	Included	Included		
Year-fixed effects	?	Included	Included	Included	Included
Quarter-fixed effects	?	Included	Included	Included	Included
Observations		26,710	26,710	10,038	10,038
LR/Wald χ^2		686.56	745.34	12,736.8	12,657.6

The rows highlighted in bold indicate the results for the key independent variables.

Columns (1) and (2) of this table reports the firm-fixed-effects logit regression results for the tests of the hypotheses H1 and H2. Columns (3) and (4) report the logit regression results from adding additional control variables in Model (1) for the hypothesis tests. The sample period ranges from 2002 to 2012. The dependent variable is *GBnews*. The key independent variables are *Insiderbuy* and *Insidersell*, respectively. All the variables are defined in "Appendix 1". Because of no within-firm variance in the *Litig* variable and industry dummies, they are omitted by the firm-fixed-effects regression model. Year and quarter dummies are included in the regressions but not reported for brevity. The *z* statistics in parentheses are based on the standard errors clustered by firm. The *p* values for LR/Wald χ^2 are close to zero. ***, **, * denote the two-tailed statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 7 Reduced-form difference-in-differences regression analyses for the tests of the hypotheses H1 and H2

Variables	Pred.sign	*GBnews*			
		(1)	(2)	(3)	(4)
Intercept	?	−0.0325 (−0.18)	−0.0372 (−0.20)	0.0401 (0.19)	0.0287 (0.13)
ΔInsiderbuy	**?**	**−0.0243 (−5.87)*** **		**−0.0236 (−5.74)*** **	
ΔInsidersell	**?**		**0.0161 (7.43)*** **		**0.0158 (7.38)*** **
ΔAsue	+	0.0418 (1.82)*	0.0431 (1.84)*		
ΔSize	−	−0.0035 (−0.05)	−0.0013 (−0.02)		
ΔInsti	−	−0.0018 (−0.12)	−0.0011 (−0.07)		
ΔAbtradvol	?	−0.0000 (−1.54)	−0.0000 (−1.47)		
ΔQtrret	+	0.0000 (4.29)***	0.0000 (4.36)***		
ΔBM	?	0.1094 (1.22)	0.1038 (1.19)		
ΔRoa	+	−0.2138 (−1.06)	−0.2203 (−1.10)		
ΔCapitalEx	+	0.4239 (0.98)	0.4631 (1.07)		
ΔDebt	−	0.0331 (0.57)	0.0368 (0.63)		
Asue	+			0.1115 (3.78)***	0.1118 (3.73)***
Size	−			0.0032 (0.44)	0.0046 (0.63)
Insti	−			−0.1279 (−2.08)**	−0.1254 (−2.03)**
Abtradvol	?			−0.0000 (−0.44)	−0.0000 (−0.45)
Qtrret	+			0.0000 (3.44)***	0.0000 (3.38)***
BM	?			0.0569 (1.69)*	0.0586 (1.75)*
Roa	+			−0.1116 (−0.58)	−0.1171 (−0.61)
CapitalEx	+			0.2225 (0.57)	0.1844 (0.47)*
Debt	−			−0.0056 (−0.38)	−0.0052 (−0.35)
Litig	?	−0.0446 (−1.23)	−0.0461 (−1.27)	−0.0249 (−0.68)	−0.0264 (−0.72)
Industry-fixed effects	?	Included	Included	Included	Included
Year-fixed effects	?	Included	Included	Included	Included
Quarter-fixed effects	?	Included	Included	Included	Included

Table 7 (continued)

Variables	Pred.sign	*GBnews*			
		(1)	(2)	(3)	(4)
Observations		27,016	27,016	27,016	27,016
Wald χ^2		175.10	181.32	169.15	177.07

The rows highlighted in bold indicate the results for the key independent variables.

This table reports the logit regression results for the tests of H1 and H2 using reduced-form difference-in-differences specifications. The sample period ranges from 2002 to 2012. The dependent variable is *GBnews*. The key independent variables are *ΔInsiderbuy* and *ΔInsidersell*, respectively. All the variables are defined in "Appendix 1". Year, quarter, and industry dummies are included in the regressions but not reported for brevity. The first difference taken of a control variable pertains to a change in the control variable between the two adjacent quarters that precede the PBE announcement quarter. The first difference is not taken of the *Litig* variable because it has no within-firm variance. The industry dummies are constructed from the first two digits of SIC codes. The *t* statistics in parentheses are based on the robust standard errors clustered by firm. The *p* values for Wald χ^2 are close to zero. ***, **, * denote the two-tailed statistical significance at the 1%, 5%, and 10% levels, respectively.

1"). The Inverse Mills ratio estimated from the first-stage regression is then included in the second-stage regression, which is modeled by Model (1), to control for the selectivity bias. An effective Heckman (1979) model requires that exclusionary variables be validly included (excluded) in (from) the first (second) stage regression (Little 1985; Puhani 2000; Lennox et al. 2012). In my case, the valid exclusionary variables should be related to the incidence of a PBE disclosure (*Inci*), but unrelated to the disclosure news (*GBnews*) except indirectly through *Inci*.

I identify four exclusionary variables, namely, earnings volatility (*EarningsVol*) and three distinct proxies for proprietary costs of disclosures (*EntryCo*, *Mktsize*, and *Substi* per Karuna (2007)), all of which are defined in "Appendix 1". High business risk for a firm, featured by high earnings volatility, would trigger outsiders' greater demand for disclosure transparency. Therefore, *EarningsVol* is expected to be positively associated with the incidence of a PBE disclosure (*Inci*). However, *EarningsVol* should have little direct association with the likelihood of a good-news PBE disclosure relative to that of a bad-news PBE disclosure (*GBnews*), and hence could be validly excluded from the second-stage regression.

Disclosures of PBE information increase a firm's risk of leaking its relevant proprietary information to product market competitors. A firm that has lower product substitutability (lower *Substi*), lower entry costs (lower *EntryCo*), or larger market size of competing products (higher *Mktsize*) faces more intense industry-level product market competition and thus is subject to higher proprietary costs of disclosures (Karuna 2007). Accordingly, the incidence of a PBE disclosure (*Inci*) should be negatively associated with *Mktsize* and positively associated with *Substi* and *Entryco*. However, given the decision to disclose PBE information, the proprietary costs of disclosures should not have further direct impact on

the disclosure news (*GBnews*).[22] Hence, *EntryCo*, *Mktsize*, and *Substi* should also be the valid exclusionary variables for the Heckman model.

In the first-stage probit regression, *Inci* is regressed on the four exclusionary variables and the same set of independent variables as included in Model (1). Panel B of Table 13 reports the regression results. *EntryCo* and *Mktsize* are statistically significant in the expected sign. The coefficients on *Insidersell* and *Insiderbuy* are qualitatively identical to those reported in Table 5. It is possible that there is no systematic difference in firm characteristics across the disclosure sample and nondisclosure sample. In this case, selectivity will not bias the coefficient estimates. Should the exclusionary variables be valid for the Heckman estimation procedure, rho equaling 0 would indicate that there exists no sample selection bias. As reported in the table, rho is not statistically different from 0. This thus serves as another piece of evidence that my main results are immune from the potential sample selection bias. In addition, I conduct a multinomial logit regression for Model (1) using the full sample, whereby the potential selection bias would be corrected (Bourguignon et al. 2007). The inferences for the hypotheses H1 and H2 remain unchanged for applying the multinomial logit specification.

4.6 Confounding effects from bundled PBE disclosures

If some PBE disclosures are bundled contemporaneously with management earnings forecasts, the return-based measures of PBE disclosure news may introduce bias into my results. I deal with this issue as per, Ball et al. (2012) and He (2018), for example. Specifically, I regress *GBnews* on earnings forecast news, which is defined as management forecast of EPS minus the median consensus analyst forecast of EPS that is issued within 90 days prior to the management forecast date, for those "bundled" PBE disclosure observations. I then treat the residual as the market reaction to the PBE disclosure news only. Should the residual be positive (negative), the PBE disclosure is classified as a good (bad) news disclosure. For the PBE disclosures that coincide with the announcements of actual earnings, I apply a similar procedure to separate the PBE-related news from the earnings-announcement news. In particular, I regress *GBnews* on earnings surprise, which is defined as the actual EPS minus the median consensus analyst forecast of EPS that is issued within 90 days prior to the EPS announcement date, to obtain the residual for the disclosure news classification. My inferences for the hypotheses H1 and H2 remain unchanged when using these alternative news measures for PBE disclosures. The bundled PBE announcements are defined as those occurring within one day surrounding management earnings forecast dates or earnings announcement dates. There are 444 (1577) observations that are bundled with management earnings forecasts (earnings announcements), accounting for only 1.83% (5.84%) of my disclosure sample, for the period 2002–2011 (2002–2012).[23] I also obtain

[22] One may argue that, given the high proprietary costs of disclosures, firms that choose to disclose PBE plans tend to disclose good news on PBE. However, it is unclear that the proprietary costs of good news disclosures are lower than the proprietary costs of bad news disclosures. Good PBE news could also become disadvantageous once disclosed, because market competitors may learn or even imitate something good from a firm which made the good news disclosure. More importantly, regardless of whether it is good news or bad news that is released to the public, proprietary information will have been made publicly available upon the disclosure. Therefore, given the decision to voluntarily make a PBE disclosure, the proprietary costs should not be further related to the news content of the PBE disclosure.

[23] The data for management earnings forecasts were taken from the First Call Company Issued Guidance database and are available for the years till 2011, and thus the sample period for the analysis involving management earnings forecasts spans the years 2002–2011.

Table 8 Two-stage instrumental-variables regression analyses for the tests of the hypotheses H1 and H2

Variables	1st Stage *Insidersell* (1)	2nd Stage *GBnews* (2)	1st Stage *Insiderbuy* (3)	2nd Stage *GBnews* (4)
Intercept	8.7113 (18.95)***	0.0431 (0.19)	9.9426 (13.07)***	0.0911 (0.41)
Insidersell		**0.0190 (7.27)*** **		
Insiderbuy				**−0.0267 (−4.70)*** **
OptionG	0.0698 (5.26)***		0.1658 (2.86)***	
LIQ	73.2991 (6.82)***		−11.0680 (−1.61)	
Asue	−0.7447 (−1.92)*	0.0977 (3.29)***	0.2485 (2.46)**	0.1064 (3.63)***
Size	0.2399 (23.71)***	−0.0077 (−1.07)	−0.0684 (−2.17)**	0.0015 (0.21)
Insti	−1.0389 (−10.78)***	−0.1642 (−2.64)***	−1.0632 (−3.72)***	−0.1422 (−2.32)**
Abtradvol	0.0000 (1.50)	−0.0000 (−0.47)	−0.0000 (−6.58)***	−0.0000 (−0.47)
Qtrret	−0.0000 (−0.19)	0.0000 (3.78)***	0.0047 (2.63)***	0.0000 (3.52)***
BM	−0.5260 (−7.62)***	0.0881 (2.59)***	−0.0179 (−0.14)	0.0577 (1.70)*
Roa	0.7341 (2.02)**	−0.1581 (−0.83)	−1.7646 (−2.96)***	−0.1239 (−0.65)
Litig	0.7468 (14.70)***	−0.0411 (−1.15)	0.5642 (2.80)***	−0.0226 (−0.63)
CapitalEx	−0.2497 (−0.46)	0.1097 (0.29)	7.6530 (4.28)***	0.2606 (0.67)
Debt	−0.2711 (−5.70)***	0.0013 (0.09)	0.2942 (3.44)***	−0.0041 (−0.27)
Industry-fixed effects	Included	Included	Included	Included
Year-fixed effects	Included	Included	Included	Included
Quarter-fixed effects	Included	Included	Included	Included
Observations	27,016		27,016	
F-stat./Wald χ^2	46.86	143.04	6.62	146.30

The rows highlighted in bold indicate the results for the key independent variables.

This table presents the results for the tests of the hypotheses H1 and H2 using two-stage instrumental-variables regressions. The sample period ranges from 2002 to 2012. In the first stage OLS regression, the dependent variable is *Insidersell* (*Insiderbuy*), and the instrument variables are *OptionG* and *LIQ*. In the second stage regression, the dependent variable is *GBnews*, and the key independent variables are the fitted *Insidersell* (*Insiderbuy*) that is estimated from the first stage regression. The predicted signs for the coefficients in the *GBnews* regressions are the same as those shown in Table 5. All the variables are defined in "Appendix 1". Year, quarter, and industry dummies are included in the regressions but not reported for brevity. The industry dummies are constructed from the first two digits of SIC codes. The *t* statistics in parentheses are based on the robust standard errors clustered by firm. The p values for Wald χ^2 and F-statistics are close to zero. ***, **, * denote the two-tailed statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 9 Falsification tests of the effect of inside trading incentives on PBE disclosures

Variables	Pred.sign	*FakeGBnews*	
		(1)	(2)
Intercept	?	−0.4147 (−1.47)	−0.3935 (−1.38)
Insiderbuy	**?**	**0.0098** **(1.63)**	
Insidersell	**?**		**−0.0005** **(−0.19)**
Asue	+	0.0513 (1.84)*	0.0490 (1.77)*
Size	−	0.0081 (0.82)	0.0077 (0.76)
Insti	−	−0.0150 (−0.20)	−0.0200 (−0.27)
Abtradvol	?	−0.0000 (−1.29)	−0.0000 (−1.31)
Qtrret	+	−0.0000 (−5.98)***	−0.00002 (−6.00)***
BM	?	0.0564 (1.14)	0.0562 (1.12)
Roa	+	−0.1561 (−0.84)	−0.1578 (−0.85)
Litig	?	0.0527 (0.98)	0.0537 (1.00)
CapitalEx	+	0.8318 (1.91)*	0.8521 (1.93)*
Debt	−	0.0449 (1.57)	0.0454 (1.56)
Industry-fixed effects	?	Included	Included
Year-fixed effects	?	Included	Included
Quarter-fixed effects	?	Included	Included
Observations		27,016	27,016
Wald χ^2		115.42	118.49

The rows highlighted in bold indicate the results for the key independent variables.

This table reports the logit regression results for the falsification tests of the hypotheses H1 and H2. The sample period ranges from 2002 to 2012. 27,016 "event" dates are randomly faked for the disclosure sample in the non-PBE disclosure period, which is defined as the period outside of [−30, 30] relative to the PBE disclosure date. The good *vis-à-vis* bad news events are coded based on the cumulative abnormal stock returns over the three-day window centered on the fake event date. The dependent variable is *FakeGBnews*. It equals 1 if the cumulative abnormal stock returns in the 3-day window centered on the fake news date are positive, and 0 if the cumulative abnormal stock returns are negative. The cumulative abnormal returns are calculated using the market model with an estimation period of [−181, −2] relative to the fake news date for a firm. The key independent variables are *Insiderbuy* and *Insidersell*, respectively. All the other variables are defined in "Appendix 1". Year, quarter, and industry dummies are included in the regressions but not reported for brevity. The industry dummies are constructed from the first two digits of SIC codes. The *t* statistics in parentheses are based on the robust standard errors clus-

Table 9 (continued) tered by firm. The p values for Wald χ^2 are close to zero. ***, **, * denote the two-tailed statistical significance at the 1%, 5%, and 10% levels, respectively.

the same inferences for the hypotheses H1 and H2 if I tease out all the bundled observations for the hypothesis tests.

5 Conclusion

PBE disclosures have strong implications for long-term streams of a firm's future earnings and are relatively hard to verify. Therefore, such a disclosure is a potent instrument that managers can use to fulfil self-serving incentives. This study examines whether managers strategically choose nonfinancial disclosure policies to increase their personal trading gain. I focus on managers' manipulation of the timing and selectivity of PBE disclosures because such type of discretionary disclosure is not only powerful in altering information flows and influencing stock prices but also relatively hard for outsiders to see through and legally charge with.

It is posited that managers would trade off the benefits and costs of the strategic PBE disclosures made before insider trades. I find that managers tend to release bad news on PBE prior to purchasing shares. This finding is attributed to low litigation costs associated with insider purchases and with bad-news nonfinancial disclosures. Unlike insider purchases, insider sales are associated with higher legal risk (Cheng and Lo 2006; Rogers 2008), but such risk might not manifest itself in selective nonfinancial disclosures which entail relatively low disclosure risk. Or rather, insider sales, even when accompanying a good-news PBE disclosure, might not be regarded as strategic and thus not induce substantive legal and/or reputational costs, because the completeness and timeliness of a PBE disclosure are relatively hard to discern or authenticate. Consistent with this rationale, I find evidence that managers tend to make a good news disclosure on PBE before selling shares, suggesting that the costs of this strategic behavior are perceived by insiders as being lower than the expected trading gain. My main results are more pronounced when the expected price impact of PBE disclosures is higher and when the CEOs are more powerful, but do not vary on institutional stock holdings. Overall, my evidence contributes to understanding the impact of insider trading incentives on strategic nonfinancial disclosures and should be of interest to boards of directors charged with the responsibility of monitoring and restricting opportunistic managerial disclosures and insider trades. Insider trades after strategic PBE disclosures do not violate any insider trading law directly. However, this strategic behavior can be curbed indirectly by mandating managers to disclose PBE news in a complete and timely manner. Given my findings in the paper, it is imperative for not only regulators but also firm shareholders and boards of directors to require managers to commit to full and timely disclosures of PBE news to the market irrespective of the nature of the news.

Managers may bias PBE disclosures to manipulate information flows to achieve their trading incentives. But there is far less room for managers to bias PBE disclosures that relate to a firm's real business activities, compared to a quantitative financial disclosure. If, on the other hand, managers distort PBE disclosures substantively, they would risk suffering much from reputational losses and litigation, because a substantial bias in PBE disclosures, even if not verifiable in a short run, can still be easily discerned in the long run. Therefore, it is more likely that firm management resorts to manipulating the timing and selectivity of PBE

Table 10 Supplemental tests of the hypotheses H1 and H2: the moderating effect of the expected price impact of PBE disclosures

Variables	*GBnews*			
	High *Disper*	Low *Disper*	High *Disper*	Low *Disper*
	(1)	(2)	(3)	(4)
Intercept	−0.1146	−10.2927	−0.3540	−10.4106
	(−0.41)	(−10.10)	(−1.25)	(−10.22)***
Insiderbuy	**−0.0613**	**−0.0004**		
	(−3.64)* **	**(−0.03)**		
Insidersell			**0.0320**	**0.0156**
			(5.36)* **	**(3.49)*** **
Asue	0.1808	0.1499	0.1712	0.1376
	(2.30)**	(2.72)***	(2.13)**	(2.48)**
Size	−0.0272	−0.0021	−0.0364	−0.0123
	(−1.26)	(−0.13)	(−1.71)*	(−0.72)
Insti	−0.1882	−0.0083	−0.2342	−0.0379
	(−0.97)	(−0.05)	(−1.23)	(−0.22)
Abtradvol	−0.0000	−0.0000	−0.0000	−0.0000
	(−2.10)**	(−0.88)	(−2.31)**	(−0.88)
Qtrret	−0.0028	−0.0067	−0.0023	−0.0076
	(−1.27)	(−1.65)*	(−0.87)	(−1.89)*
BM	0.1741	0.0358	0.2418	0.0862
	(1.27)	(0.29)	(1.79)*	(0.70)
Roa	−0.2703	0.1916	−0.3402	0.2009
	(−0.29)	(0.20)	(−0.36)	(0.21)
Litig	−0.0202	−0.0066	−0.0502	−0.0015
	(−0.19)	(−0.08)	(−0.49)	(−0.02)
CapitalEx	0.6014	0.2544	0.6279	0.0491
	(0.51)	(0.19)	(0.55)	(0.04)
Debt	−0.0409	−0.0272	−0.0339	0.0044
	(−0.46)	(−0.23)	(−0.38)	(0.04)
Industry-fixed effects	Included	Included	Included	Included
Year-fixed effects	Included	Included	Included	Included
Quarter-fixed effects	Included	Included	Included	Included
Observations	4926	7821	4926	7821
Wald χ^2	175.40	52.92	130.09	33.95

This table reports the logit regression results for the test of whether managers' strategic PBE disclosures made for personal trading gain are more pronounced when the price impact of PBE disclosures is expected to be large. High analyst forecast dispersion (*Disper*) is used to capture the situation in which PBE disclosures are expected to have a greater impact on investors' prior expectations. The sample period ranges from 2002 to 2012. The dependent variable is *GBnews*. The key independent variables are *Insiderbuy* and *Insidersell*, respectively. The disclosure sample is split into two subsamples, with observations grouped into the high (low) *Disper* subsample if they have *Disper* higher than (equal to or less than) the sample median. *Disper* is the median consensus analyst forecast of EPS for the quarter that precedes the PBE announcement quarter. All the variables are defined in "Appendix 1". The predicted signs for the coefficients are the same as those shown in Table 5. Year, quarter, and industry dummies are included in the regressions but not reported for brevity. The industry dummies are constructed from the first two digits of SIC codes. The *t* statistics in parentheses are based on the robust standard errors clustered by firm. The *p* values for Wald χ^2 are close to zero. ***, **, * denote the two-tailed statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 11 Supplemental tests of the hypotheses H1 and H2: the moderating effect of CEO power

Variables	*GBnews*			
	High *ceopower*	Low *ceopower*	High *ceopower*	Low *ceopower*
	(1)	(2)	(3)	(4)
Intercept	−0.5007	−0.4261	−0.7855	−0.5205
	(−0.44)	(−0.55)	(−0.71)	(−0.68)
Insiderbuy	**−0.0726**	**−0.0447**		
	(−3.02)***	**(−1.95)***		
Insidersell			**0.0490**	**0.0124**
			(3.78)***	**(1.24)**
Asue	−0.1320	−0.2160	−0.1280	−0.2270
	(−0.56)	(−1.76)*	(−0.54)	(−1.81)*
Size	0.0250	0.0227	0.0084	0.0255
	(0.43)	(0.77)	(0.14)	(0.86)
Insti	0.4272	−0.2721	0.5457	−0.2319
	(0.55)	(−0.84)	(0.72)	(−0.72)
Abtradvol	−0.0000	−0.0000	−0.0000	0.0000
	(−2.36)**	(2.16)**	(−2.45)**	(2.37)**
Qtrret	0.0068	0.0001	0.0075	0.0002
	(1.66)*	(0.68)	(1.58)	(0.83)
BM	1.4367	0.1889	1.3544	0.2195
	(2.53)**	(1.01)	(2.33)**	(1.16)
Roa	18.1770	2.6780	18.3576	2.6580
	(2.10)**	(1.26)	(2.08)**	(1.24)
Litig	−0.0872	0.5805	−0.1976	0.5561
	(−0.35)	(3.69)***	(−0.80)	(3.57)***
CapitalEx	−2.9793	3.4925	−4.3142	3.3409
	(−1.20)	(2.02)**	(−1.49)	(1.94*)
Debt	0.3663	−0.0895	0.4707	−0.1020
	(0.71)	(−0.79)	(0.93)	(−0.88)
Industry-fixed effects	Included	Included	Included	Included
Year-fixed effects	Included	Included	Includedd	Included
Quarter-fixed effects	Included	Included	Included	Included
Observations	2216	741	2216	741
Wald χ^2	3890	92.49	1567.84	97.34

This table reports the logit regression results for the test of whether managers' strategic PBE disclosures made for personal trading gain are more pronounced when the CEOs are more powerful. The dependent variable is *GBnews*. The key independent variables are *Insiderbuy* and *Insidersell*, respectively. The disclosure sample is split into two subsamples, with observations grouped into the high (low) *ceopower* subsample. *ceopower* is regarded as high if (1) the CEO is the chairman of the board and (2) the proportion of independent directors on the board is lower than the sample median, and otherwise, is regarded as low. The data for constructing the *ceopower* variable are obtained from the Institutional Shareholders Services (ISS) database which covers S&P 1500 firms for the period starting in 2007. Thus, the sample period used for the regression analysis ranges from 2007 to 2012. All the variables are defined in "Appendix 1". The predicted signs for the coefficients are the same as those shown in Table 5. Year, quarter, and industry dummies are included in the regressions but not reported for brevity. The industry dummies are constructed from the first two digits of SIC codes. The *t* statistics in parentheses are based on the robust standard errors clustered by firm. The p values for Wald χ^2 are close to zero. ***, **, * denote the two-tailed statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 12 Supplemental tests of the hypotheses H1 and H2: the moderating effect of institutional stock ownership

Variables	*GBnews*			
	High *Insti*	Low *Insti*	High *Insti*	Low *Insti*
	(1)	(2)	(3)	(4)
Intercept	−0.7526	0.3258	−0.8936	0.3185
	(−2.94)***	(1.57)	(−3.62)***	(1.55)
Insiderbuy	**−0.0351**	**−0.0200**		
	(−3.85)* **	**(−2.79)*** **		
Insidersell			**0.0201**	**0.0212**
			(5.17)* **	**(5.86)*** **
Asue	0.0820	0.1190	0.0669	0.1095
	(1.80)*	(3.14)***	(1.44)	(2.88)***
Size	0.0069	−0.0012	−0.0030	−0.0110
	(0.42)	(−0.14)	(−0.19)	(−1.35)
Insti	−0.2829	−0.1497	−0.3070	−0.1981
	(−1.68)*	(−1.02)	(−1.80)*	(−1.40)
Abtradvol	−0.0000	−0.0000	−0.0000	−0.0000
	(−1.16)	(−0.09)	(−1.19)	(−0.16)
Qtrret	−0.0025	1.50E−5	−0.0025	1.60E−5
	(−2.00)**	(4.03)***	(−1.85)*	(4.12)***
BM	0.1197	−0.0116	0.1712	0.0130
	(1.73)*	(−0.27)	(2.46)**	(0.30)
Roa	0.1402	−0.1576	0.1107	−0.1760
	(0.34)	(−0.84)	(0.27)	(0.94)
Litig	−0.0321	−0.0067	−0.0520	−0.0399
	(−0.54)	(−0.13)	(−0.87)	(−0.81)
CapitalEx	0.5093	0.3188	0.2395	0.2824
	(0.93)	(0.47)	(0.43)	(0.43)
Debt	0.0323	−0.0323	0.0290	−0.0237
	(0.96)	(−1.57)	(0.81)	(−1.12)
Industry-fixed effects	included	Included	Included	Included
Year-fixed effects	included	Included	Included	Included
Quarter-fixed effects	included	Included	Included	Included
Observations	13,508	13,508	13,508	13,508
Wald χ^2	440.16	152.57	382.53	171.29

This table reports the logit regression results for the test of whether managers' strategic PBE disclosures made for personal trading gain change along the level of institutional stock holdings. The sample period ranges from 2002 to 2012. The dependent variable is *GBnews*. The key independent variables are *Insiderbuy* and *Insidersell*, respectively. The disclosure sample is split into two subsamples, with observations grouped into the high (low) *Insti* subsample if they have *Insti* higher than (equal to or less than) the sample median. *Insti* is the institutional equity ownership as a percentage of outstanding shares for a fiscal quarter preceding the PBE announcement quarter. All the variables are defined in "Appendix 1". The predicted signs for the coefficients are the same as those shown in Table 5. Year, quarter, and industry dummies are included in the regressions but not reported for brevity. The industry dummies are constructed from the first two digits of SIC codes. The *t* statistics in parentheses are based on the robust standard errors clustered by firm. The *p* values for Wald χ^2 are close to zero. ***, **, *Denote the two-tailed statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 13 Supplemental tests of the hypotheses H1 and H2: control for potential sample selection bias

Panel A: *Control for industry-level insider trades*

Variables	Pred.sign	*GBnews*	
		(1)	(2)
Intercept	?	0.0961 (0.43)	0.0409 (0.18)
Insiderbuy	**?**	**−0.0217 (−3.25)*****	
Insidersell	**?**		**0.0196 (6.82)*****
Industry_Insiderbuy	?	−0.0072 (−0.90)	
Industry_Insidersell	?		0.0013 (0.34)
Asue	+	0.1068 (3.64)***	0.0943 (3.17)***
Size	−	0.0016 (0.21)	−0.0082 (−1.15)
Insti	−	−0.1422 (−2.31)**	−0.1645 (−2.64)***
Abtradvol	?	−0.0000 (−0.46)	−0.0000 (−0.47)
Qtrret	+	0.0000 (3.54)***	0.0000 (3.82)***
BM	?	0.0581 (1.71)**	0.0903 (2.64)***
Roa	+	−0.1289 (−0.68)	−0.1545 (−0.81)
CapitalEx	+	0.2637 (0.68)	0.1037 (0.27)
Debt	−	−0.0043 (−0.29)	0.0024 (0.15)
Industry-fixed effects	?	Included	Included
Year-fixed effects	?	Included	Included
Quarter-fixed effects	?	Included	Included
Observations		27,016	27,016
LR χ^2		166.94	170.02

Panel B: *Two-stage Heckman probit regression*

Variables	1st-Stage *Inci*	2nd-Stage *GBnews*	1st-Stage *Inci*	2nd-Stage *GBnews*
	(1)	(2)	(3)	(4)
Intercept	−2.6859 (−12.64)***	0.1933 (1.00)	−2.2788 (−12.24)***	0.0361 (0.33)
Insiderbuy	0.3043 (21.39)***	**−0.0214 (−2.21)****		
Insidersell			0.2552 (29.55)***	**0.0101 (1.98)****
EarningsVol	0.0010 (2.87)***		0.0011 (3.51)***	

Table 13 (continued)

Panel B: *Two-stage Heckman probit regression*

Variables	1st-Stage *Inci*	2nd-Stage *GBnews*	1st-Stage *Inci*	2nd-Stage *GBnews*
	(1)	(2)	(3)	(4)
Entryco	5.21E−6 (1.58)		7.14E−6 (2.31)**	
Mktsize	−4.83E−7 (−1.95)*		−2.91E−7 (−1.27)	
Substi	0.4035 (3.12)***		0.2987 (2.71)***	
Asue	0.00006 (1.63)	0.0653 (3.02)***	0.00005 (1.38)	0.0580 (2.63)***
Size	0.1432 (5.85)***	−0.0044 (−0.40)	0.0701 (2.88)***	−0.0023 (−0.37)
Insti	0.2130 (1.55)	−0.1075 (−2.18)**	0.0745 (0.61)	−0.1218 (−2.50)**
Abtradvol	−0.0000 (−4.37)***	−0.0000 (−0.28)	−0.0000 (−4.08)***	−0.0000 (−0.65)
Qtrret	0.2957 (15.99)***	−0.0088 (−0.61)	0.1839 (7.51)***	−0.0017 (−0.18)
BM	−0.0051 (−0.06)	0.0373 (1.14)	0.1036 (1.41)	0.0616 (1.91)*
Roa	−1.9269 (−2.93)***	0.2688 (0.92)	−2.9347 (−4.83)***	0.1591 (0.56)
Litig	0.8715 (11.16)***	−0.0540 (−1.05)	0.6970 (9.72)***	−0.0317 (−1.03)
CapitalEx	−2.6749 (−3.13)***	0.3918 (0.95)	−2.5936 (−3.47)***	0.1471 (0.37)
Debt	−0.4061 (−3.49)***	0.0338 (0.87)	−0.2948 (−3.68)***	0.0250 (0.70)
Industry-fixed effects	Included	Included	Included	Included
Year-fixed effects	Included	Included	Included	Included
Quarter-fixed effects	Included	Included	Included	Included
χ^2 for Inverse Mills ratio (regarding whether rho = 0)		0.85 (0.358)		0.11 (0.735)
Observations	101,634	20,164	101,634	20,164
LR χ^2	21,925.33	117.82	40,633.10	155.86

The rows highlighted in bold indicate the results for the key independent variables.

Panel A of this table reports the results for Model (1) after controlling for industry-level insider trades, namely, *Industry_Insiderbuy* and *Industry_Insidersell*. *Industry_Insiderbuy* (*Industry_Insidersell*) is measured by the average of net insider purchases (sales) made by the same-industry corporate insiders during the 30-day period after a PBE disclosure. The inclusion of industry-level insider trades serves to mitigate the impact of potential sample selection bias on the coefficient estimates for *Insiderbuy* and *Insidersell*. Panel B presents the results for the tests of the hypotheses H1 and H2 using two-stage Heckman probit regression. In the first-stage probit regression, the dependent variable is *Inci*, which equals 1 if a firm delivers a PBE disclosure during a fiscal quarter and 0 otherwise. The exclusionary variables are *EarningsVol*, *EntryCo*, *Mktsize*, and *Substi*. In the second-stage probit regression, the dependent variable is *GBnews*, and the key independent variables are *Insiderbuy* and *Insidersell*, respectively. The predicted signs for the coefficients in the *GBnews* regressions are the same as those shown in Table 5. For results in both panels, the sample period ranges from 2002 to 2012, and all the variables are defined in "Appendix 1". Year, quarter, and industry dummies are included but not reported for brevity. The industry dummies are constructed from the first two digits of SIC codes. The *z* statistics in parentheses are based on the robust standard errors clustered by firm. The *p* values for LR χ^2 are close to zero. ***, **, *Denote the two-tailed statistical significance at the 1%, 5%, and 10% levels, respectively.

disclosures (which is more powerful in affecting stock prices, less likely to detect, and less subject to potential reputational losses and litigation) rather than distorting the news content of PBE disclosures, the former of which is therefore the focus of this study.

It is interesting to look further at whether and how *disclosure biasing* may complement *disclosure timing and selectivity* in managers' disclosure strategies designed to serve personal trading incentives. Nonetheless, unlike management earnings forecasts for which we have audited earnings reports as the benchmark to assess potential forecast bias, there is lack of an objective, clear-cut benchmark to appraise consistently whether, and to what extent, a PBE disclosure is biased. Researchers and practitioners may hold different perspectives and can reasonably dissent to a substantive degree on the news content of PBE disclosures. Moreover, we do not have access to firms' private information to verify PBE disclosures. Therefore, it remains an academic challenge for an archival study to probe and assess bias, if any, in the PBE disclosures. I leave this as an avenue for future research in an experimental setting. In addition, managers may manipulate the precision of PBE disclosures in a way that makes bad news disclosures more ambiguous (precise) than good news disclosures, whereby inflating (deflating) stock prices. Such a disclosure strategy might also be used by managers to attain personal trading incentives. Nonetheless, it is relatively difficult for an archival study to determine and test the degree of ambiguity in the news content of a qualitative PBE disclosure. I therefore leave this issue for future experimental research as well. Future research may also explore whether PBE disclosures are more, or less, powerful than management earnings forecasts or any other type of corporate disclosures in helping managers to fulfil personal trading incentives.

Appendix 1

See Table 14.

Table 14 Summary of variable definitions

Variables	Definitions
GBnews	1 if a firm delivers a good-news PBE disclosure, and 0 if a firm makes a bad-news PBE disclosure. The nature of the disclosure news is measured by the daily stock returns over three days centered on a PBE-disclosure date. In particular, *GBnews* equals 1 if the cumulative abnormal stock returns in the 3-day window centered on the PBE announcement date are positive, and 0 if the cumulative abnormal stock returns are negative. The cumulative abnormal returns are calculated using the market model with an estimation period of $[-181, -2]$ relative to the announcement date for a firm
Inci	1 if a firm voluntarily makes a PBE disclosure for a fiscal quarter and 0 otherwise
Insiderbuy	The natural logarithm of 1 plus the net insider purchases (the number of shares purchased less the number of shares sold) over a 30-day period after a PBE disclosure if a firm has a positive amount of net insider purchases over the 30-day window. *Insiderbuy* equals 0 if the firm's net insider purchases over the 30-day window are negative or zero
Insidersell	The natural logarithm of 1 plus the net insider sales (the number of shares sold less the number of shares purchased) over a 30-day period after a PBE disclosure if a firm has a positive amount of net insider sales over the 30-day window. *Insidersell* equals 0 if the firm's net insider sales over the 30-day window are negative or zero
LagInsiderbuy	The natural logarithm of 1 plus the net insider purchases over a 30-day period prior to a PBE disclosure if a firm has a positive amount of net insider purchases over the 30-day window. *LagInsiderbuy* equals 0 if the firm's amount of net insider purchases over the 30-day pre-disclosure window are negative or zero
LagInsidersell	The natural logarithm of 1 plus the net insider sales over a 30-day period prior to a PBE disclosure if a firm has a positive amount of net insider sales over the 30-day window. *LagInsidersell* equals 0 if the firm's amount of net insider sales over the 30-day pre-disclosure window are negative or zero
ΔInsiderbuy	*Insiderbuy* minus *LagInsiderbuy*, measuring change in net insider purchases around a PBE announcement
ΔInsidersell	*Insidersell* minus *LagInsidersell*, measuring change in net insider sales around a PBE announcement
Industry_Insiderbuy	The natural logarithm of 1 plus the average of the net insider purchases made by the same-industry corporate insiders during the 30-day period after a PBE disclosure if a firm has a positive amount of net insider purchases over the 30-day window. *Industry_Insiderbuy* equals 0 if the average of the net insider purchases over the 30-day window are negative or zero
Industry_Insidersell	The natural logarithm of 1 plus the average of the net insider sales made by the same-industry corporate insiders during the 30-day period after a PBE disclosure if a firm has a positive amount of net insider sales over the 30-day window. *Industry_Insiderbuy* equals 0 if the average of the net insider sales over the 30-day window are negative or zero
DInsiderbuy	1 if the net insider purchases (i.e., insider purchases are larger than insider sales) are positive and 0 otherwise
DInsidersell	1 if the net insider sales are positive (i.e., insider sales are larger than insider purchases) and 0 otherwise
TInsiderbuy	The natural logarithm of 1 plus the net insider purchases over a month ending at the end of a fiscal quarter if a firm has a positive amount of net insider purchases during the month; *TInsiderbuy* equals 0 if the firm's net insider purchases over the month are negative or zero
TInsidersell	The natural logarithm of 1 plus the net insider sales over a month ending at the end of a fiscal quarter if a firm has a positive amount of net insider sales during the month; *TInsidersell* equals 0 if the firm's net insider sales over the month are negative or zero

Table 14 (continued)

Variables	Definitions
OptionG	The natural logarithm of 1 plus the number of options granted over a 3-year period preceding the PBE announcement quarter
LIQ	The average of daily relative effective spread for the PBE announcement quarter, multiplied by − 1. The daily relative effective spread is calculated as the difference between the transaction price and the midpoint of the prevailing bid-ask quote, divided by the midpoint of the prevailing bid-ask quote
EarningsVol	The standard deviation of a firm's quarterly earnings for the quarters over a five-year period ending at the end of the fiscal quarter preceding the PBE announcement quarter
Entryco	The sales-weighted average of gross property, plant, and equipment in a 4-digit SIC industry for a fiscal quarter preceding the PBE announcement quarter
Mktsize	The sum of sales in a 4-digit SIC industry for a fiscal quarter preceding the PBE announcement quarter
Substi	The sum of sales in a 4-digit SIC industry, divided by the sum of operating costs in the same industry, for a fiscal quarter preceding the PBE announcement quarter
Asue	1 if the actual EPS for a quarter (that ends before the PBE announcement quarter) is greater than the median consensus analyst forecast of EPS for that quarter, and 0 otherwise
Disper	The standard deviation of analyst forecasts of EPS for the quarter that precedes the PBE announcement quarter
Size	The natural logarithm of the market value of equity at the end of a fiscal quarter that ends before the PBE announcement quarter
Insti	Institutional equity ownership as a percentage of outstanding shares for a fiscal quarter preceding the PBE announcement quarter
Abtradvol	The trading volume for a 90-day period that ends before the PBE announcement quarter, less the trading volume for a 90-day period that ends one quarter before the PBE announcement quarter. The trading volume is calculated as the sum of daily dollar trading volume (i.e., the product of the closing price at a date and the number of shares traded during the date) for a firm over a fiscal quarter
Qtrret	The size-adjusted buy-and-hold returns of a firm over a fiscal quarter, which equal the compounded raw returns minus the compounded equally-weighted returns of the same CRSP size decile and the same CRSP exchange index (NYSE/AMEX/NASDAQ) that the firm belongs to
BM	The book value of firm equity, divided by the market value of firm equity, at the end of a fiscal quarter preceding the PBE announcement quarter
Roa	Return on total assets for a fiscal quarter preceding the PBE announcement quarter
Litig	1 for firms in the biotechnology (with SIC codes 2833-2836 & 8731-8734), computers (with SIC codes 3570-3577 and 7370-7374), electronics (with SIC codes 3600-3674), and retail (with SIC codes 5200-5961) industries and 0 otherwise
CapitalEx	Capital expenditures divided by total assets for a fiscal quarter preceding the PBE announcement quarter
Debt	The ratio of long-term debt to total assets for a fiscal quarter that ends before the PBE announcement quarter
ceopower	1 if the CEO is the chairman of the board and the proportion of independent directors on the board is lower than the sample median, and 0 otherwise
Indpboard	The number of the independent outside directors on the board of a firm, divided by the number of all the directors on the board, for a fiscal quarter
Indpaudit	The number of the independent outside directors who sit on the auditing committee, divided by the number of all the directors on the board, for a fiscal quarter

Appendix 2: Examples of product and business expansion disclosures

1. An example of product-related disclosure—*American Express introduced new online and mobile payment security services*

"*New York, November 3, 2014—American Express today announced the launch of its American Express Token Service, a suite of solutions designed to enable its card-issuing partners, processors, acquirers and merchants to create a safer online and mobile payments environment for consumers.*

With American Express Token Service, traditional card account numbers are replaced with unique "tokens," which can then be used to complete payment transactions online, in a mobile app or in-store with a mobile Near Field Communication (NFC)-enabled device. By using tokens, merchants and digital wallet operators will no longer need to store consumers' sensitive payment account information in their systems. In addition, tokens can be assigned for use with a specific merchant, transaction type or payment device to provide further protection against fraud.

Based on EMVCo's Payment Tokenization Specification and Technical Framework published earlier this year, American Express Token Service offers the following features: (1) a token vault to store and map tokens to card account numbers; (2) the ability to issue tokens; (3) lifecycle management services to create, suspend, resume or delete tokens; (4) additional fraud and risk management services, such as authorization and payment data validation capabilities, for card-issuing financial institutions.

American Express Token Service is available in the U.S., and international rollout is expected to begin in 2015.

"*We believe our payments network is a tremendous asset to American Express—one that will allow us to offer our customers new features and technologies to meet their evolving spending needs," said Paul Fabara, President, Global Banking and Global Network Business, American Express. "As we move ahead, we are excited to bring these new capabilities to our customers and look forward to continuing to serve them."*

American Express also announced that it has developed network specifications for Host Card Emulation (HCE). American Express' HCE specifications provide its card-issuing partners with additional security options and solutions for payments made with mobile NFC-enabled devices that support Android iOS KitKat. With HCE, card issuers use a secure cloud server to store their customers' card account details, which can be transmitted from the cloud server to an NFC-enabled mobile device and then to a Point-of-Sale terminal in a fast, secure manner. American Express' HCE specifications are available today globally."

(Source: Press release from American Express, available at http://about.americanexpress.com/news/pr/2014/amex-intros-online-mobile-payment-security.aspx).

2. An example of business expansion disclosure—*Apple invested €1.7 billion in the New European Data centres*

"*CORK, Ireland, February 23, 2015—Apple today announced a €1.7 billion plan to build and operate two data centres in Europe, each powered by 100 percent renewable energy. The facilities, located in County Galway, Ireland, and Denmark's central Jutland, will power Apple's online services including the iTunes Store, App Store, iMessage, Maps and Siri for customers across Europe.*

"We are grateful for Apple's continued success in Europe and proud that our investment supports communities across the continent," said Tim Cook, Apple's CEO. "This significant new investment represents Apple's biggest project in Europe to date. We're thrilled to be expanding our operations, creating hundreds of local jobs and introducing some of our most advanced green building designs yet."

Apple supports nearly 672,000 European jobs, including 530,000 jobs directly related to the development of iOS apps. Since the App Store's debut in 2008, developers across Europe have earned more than €6.6 billion through the worldwide sale of apps.

Apple now directly employs 18,300 people across 19 European countries and has added over 2000 jobs in the last 12 months alone. Last year, Apple spent more than €7.8 billion with European companies and suppliers helping build Apple products and support operations around the world.

Like all Apple data centres, the new facilities will run entirely on clean, renewable energy sources from day one. Apple will also work with local partners to develop additional renewable energy projects from wind or other sources to provide power in the future. These facilities will have the lowest environmental impact yet for an Apple data centre.

"We believe that innovation is about leaving the world better than we found it, and that the time for tackling climate change is now," said Lisa Jackson, Apple's vice president of Environmental Initiatives. "We're excited to spur green industry growth in Ireland and Denmark and develop energy systems that take advantage of their strong wind resources. Our commitment to environmental responsibility is good for the planet, good for our business and good for the European economy."

The two data centres, each measuring 166,000 square metres, are expected to begin operations in 2017 and include designs with additional benefits for their communities. For the project in Athenry, Ireland, Apple will recover land previously used for growing and harvesting non-native trees and restore native trees to Derrydonnell Forest. The project will also provide an outdoor education space for local schools, as well as a walking trail for the community.

In Viborg, Denmark, Apple will eliminate the need for additional generators by locating the data centre adjacent to one of Denmark's largest electrical substations. The facility is also designed to capture excess heat from equipment inside the facility and conduct it into the district heating system to help warm homes in the neighbouring community.

Apple designs Macs, the best personal computers in the world, along with OS X, iLife, iWork and professional software. Apple leads the digital music revolution with its iPods and iTunes online store. Apple has reinvented the mobile phone with its revolutionary iPhone and App Store, and is defining the future of mobile media and computing devices with iPad."

(Source: Press release from Apple, available at http://www.apple.com/pr/library/2015/02/23Apple-to-Invest-1-7-Billion-in-New-European-Data-Centres.html).

3. An example of product-related disclosure—*Tesla Q2 2017 vehicle production and deliveries*

"PALO ALTO, Calif., July 07, 2017 (GLOBE NEWSWIRE)—In response to questions we have received about the number of customer vehicles in transit at the end of Q2, we are updating our Q2 delivery release to provide this information. This information will continue to be included in all future quarters.

In addition to Q2 deliveries, about 3,500 vehicles were in transit to customers at the end of the quarter. These will be counted as deliveries in Q3 2017.

Tesla (Nasdaq:TSLA) delivered just over 22,000 vehicles in Q2, of which just over 12,000 were Model S and just over 10,000 were Model X. This represents a 53% increase over Q2 2016. Total vehicle deliveries in the first half of 2017 were approximately 47,100.

The major factor affecting Tesla's Q2 deliveries was a severe production shortfall of 100 kWh battery packs, which are made using new technologies on new production lines. The technology challenge grows exponentially with energy density. Until early June, production averaged about 40% below demand. Once this was resolved, June orders and deliveries were strong, ranking as one of the best in Tesla history.

Provided global economic conditions do not worsen considerably, we are confident that combined deliveries of Model S and Model X in the second half of 2017 will likely exceed deliveries in the first half of 2017.

Q2 production totaled 25,708 vehicles, bringing first half 2017 production to 51,126.

*We always want our customers to experience the newest versions of Model S and X while their cars are in service, so we added fully loaded, newly built cars to our service loaner fleet. We always want the service loaner Tesla to be *better* than the customer car being serviced. The customer should never suffer for something that is our fault.*

We also finally added a sufficient number of Model X cars to our test drive and display fleet because our stores had been operating with far short of what was needed and, in some cases, none at all. There appears to be substantial untapped sales potential for Model X. It should also be noted that production quality and field reliability of the Model X, for which Tesla has been fairly criticized, have improved dramatically. It is now rare for a newly produced Model X to have initial quality problems.

The first certified production Model 3 that meets all regulatory requirements will be completed this week, with a handover of ~30customer cars at our Fremont factory on July 28. More details to follow soon.

Our delivery count should be viewed as slightly conservative, as we only count a car as delivered if it is transferred to the customer and all paperwork is correct. Final numbers could vary by up to 0.5%. Tesla vehicle deliveries represent only one measure of the company's financial performance and should not be relied on as an indicator of quarterly financial results, which depend on a variety of factors, including the cost of sales, foreign exchange movements and mix of directly leased vehicles."

(Source: Press release from Tesla, available at: http://ir.tesla.com/releasedetail.cfm?ReleaseID=1032479).

4. An example of business expansion disclosure—*Anthem statement on individual market participation in Nevada*

"Anthem, inc., August 07, 2017—*After significant dialogue with state leaders and regulators Anthem Blue Cross Blue Shield has made the difficult decision to revise our rate filing for our 2018 Individual plan offerings in Nevada.*

While we are pleased that some steps have been taken to address the long-term challenges all health plans serving the Individual market are facing, the Individual market remains volatile. A stable insurance market is dependent on products that create value for consumers through the broad spreading of risk and a known set of conditions upon which rates can be developed. Today, planning and pricing for ACA-compliant health plans has become increasingly difficult due to a shrinking and deteriorating individual market,

as well as continual changes and uncertainty in federal operations, rules and guidance, including cost sharing reduction subsidies and the restoration of taxes on fully insured coverage.

Specifically, Anthem will reduce its 2018 Individual plan offering in Nevada and will only offer an off-exchange catastrophic medical plan statewide. It's important to note, this decision does not affect those who have employer-based insurance or individuals enrolled in Medicaid, Medicare or "grandfathered" plans (plans purchased before March 2010).

Our commitment to members has always been to provide greater access to affordable, quality healthcare, and we will continue to advocate solutions that will stabilize the market and allow us to return to a more robust presence in Nevada in the future."

(Source: Press release from Anthem, available at: https://www.anthem.com/press/nevada/anthem-statement-on-individual-market-participation-in-nevada/).

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