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Does Stock Liquidity Shape Voluntary Disclosure?

Evidence from the SEC Tick Size Pilot Program

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Abstract: Employing the SEC Tick Size Pilot Program that increases the minimum trading unit of a set of randomly selected small-capitalization stocks, we examine whether and how an exogenous change in stock liquidity affects corporate voluntary disclosure. Using difference-in-differences analyses with firm fixed effects, we find that treatment firms respond to the liquidity decline by issuing fewer management earnings forecasts, while in contrast, control firms do not exhibit a significant change. Next, we show that the effect is more pronounced when firms experience more severe liquidity decreases during the TSPP and rule out a set of alternative explanations. Further strengthening the identification, we find a consistent reversal effect after the end of the pilot program. To generalize our findings, we use voluntary 8-K filings and conference calls as alternative voluntary disclosure proxies and find similar effects. Overall, these findings show how an exogenous change in stock liquidity shapes the corporate information environment.

Keywords: Stock liquidity; voluntary disclosure; management guidance; management earnings forecasts; SEC Tick Size Pilot Program

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1. Introduction

Managers employ voluntary disclosure as a strategy to shape stock liquidity, especially when stock liquidity is low (Graham, Harvey, and Rajgopal 2005; Balakrishnan, Billings, Kelly, and Ljungqvist 2014; Guay, Samuels, and Taylor 2016; Schoenfeld 2017). The literature suggests that voluntary disclosure is useful in shaping liquidity in that it alleviates information asymmetry among investors, decreases the trading costs arising from adverse selection, and thus improves liquidity (Diamond and Verrecchia 1991; Welker, 1995; Leuz and Wysocki 2008). These studies show that voluntary disclosure positively affects liquidity and imply that liquidity is *endogenously* affected by the information environment. However, it is not clear whether and how an *exogenous* liquidity change affects corporate voluntary disclosure decisions. In this study, we examine the effect of an exogenous liquidity change on voluntary disclosure by employing the 2016 SEC Tick Size Pilot Program (henceforth, the **TSPP**).

In order to boost liquidity provision and analyst coverage for small-capitalization firms, the SEC started the tick size pilot program by switching the minimum trading unit (i.e., tick size) of a set of randomly selected small-capitalization stocks from one cent to five cents during the period of October 2016 to September 2018 (WSJ 2016). Despite the intention to improve liquidity of small-cap firms, studies on this pilot program suggest that, on average, pilot firms experience a reduction in stock liquidity (Albuquerque, Song, and Yao 2020; Li, Ye, and Zheng 2020).[1] Meanwhile, the TSPP did not impose direct changes to the information environment,

[1] Our focus is on the overall decline in stock liquidity for pilot firms in the TSPP, which is supported by the evidence from Albuquerque et al. (2020) and Li et al. (2020). Chung, Lee, and Rösch (2020) show that the TSPP has different effects on stock liquidity for orders of different sizes. Specifically, the TSPP improves the liquidity for large-size orders but impairs the liquidity for small-size orders because the widened tick size imposes a larger spread while increasing the market depth at each price. However, order-level liquidity effect may or may not translate into an overall change in stock liquidity, as it depends on the proportion of large-size (small-size) orders

as the sample selection process and experiment treatment are both unrelated to disclosure practices. Hence, we are able to use the TSPP to isolate the portion of exogenous liquidity variation resulting from the change in tick size but not directly related to the corporate information environment.

Studies on the relation between voluntary disclosure and liquidity primarily explore how voluntary disclosure affects liquidity (Balakrishnan et al. 2014). In contrast, we study the *opposite direction* of the disclosure-liquidity relation by focusing on how liquidity, especially liquidity variations that are exogenous with respect to the information environment, affects voluntary disclosure. Our research question and findings complement those of prior studies by indicating that voluntary disclosure and liquidity are mutually influenced. Our study exploits a unique setting, in which firms experience shocks to liquidity due to a market microstructure change *without* experiencing shocks to information asymmetry. In this setting, we are able to provide causal inference on whether and how liquidity shapes firms' disclosure incentives. This setting helps us to control for the findings from extant research that voluntary disclosure positively influences liquidity.

We argue that the liquidity decline induced by the TSPP may lead to reduced voluntary disclosure. The liquidity reduction may have an attenuation effect on the market reaction to voluntary disclosure, which is likely to weaken both managers' incentive to disclose and investors' demand for voluntary disclosure. Specifically, limited liquidity may hinder the market from impounding the information into stock prices in a timely and sufficient manner (Hirshleifer and Teoh 2003). As a result, managers' ability to influence stock prices using disclosure is weakened, thus reducing the expected benefits they can gain from voluntary disclosure (Trueman 1986; Cotter, Tuna, and Wysocki 2006; Versano and Trueman 2017).

for each stock. Managers are likely more concerned with the overall liquidity of their stocks than the liquidity of a single order and develop our hypotheses based on an overall reduction in stock liquidity.

Meanwhile, investors may trade less and thus have lower demand for disclosure because low liquidity imposes additional trading costs. As a result, less trading leads to lower propensity for investors to require information from managers and thus make managers refrain from disclosure (Abramova, Core, and Sutherland 2020).

We employ the TSPP to identify a change in liquidity that is relatively independent from the information environment.[2] Using the sample of pilot and control stocks designated by the SEC, we perform difference-in-differences analyses with *firm fixed effects* around the implementation of the TSPP to examine whether and how firms respond to the TSPP by adjusting their voluntary disclosure. Specifically, we use the eight quarters before and after the TSPP starting quarter *t* (2016Q4) to show the effect for the complete two-year pilot program period (i.e., quarter *t-8* to *t+7*).[3] Using management earnings forecasts as the primary measure for voluntary disclosure, we find that the treatment firms significantly reduce the frequency of earnings forecasts after the start of the TSPP, while control firms do not exhibit a significant change.[4] Specifically, relative to that of control firms, the frequency of management earnings forecasts reduces by 15% relative to the mean of our sample. The effect is both statistically and economically significant and robust to a broad set of control variables as well as firm and year-quarter fixed effects. In addition, parallel trend tests, as well as placebo analyses, indicate that the findings are not likely to be driven by other confounding factors. We also find that the effect is more pronounced for short-term forecasts and forecasts bundled with earnings announcements.

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[2] Consistent with prior studies (e.g., Albuquerque, Song, and Yao 2020; Li, Ye, and Xiong 2020), we show in untabulated analyses that, relative to control firms, pilot firms experience a significant decrease in quarterly average liquidity, including bid-ask spreads, turnover ratios, dollar trading volume, and the Amihud's (2002) illiquidity measure.

[3] Our inferences are robust to: (1) exclusion of the starting quarter; and (2) shorter sample periods (e.g., two, four, or six quarters before and after the starting quarter).

[4] Management guidance tends to be a sticky corporate decision because the reduction or cessation of management forecasts is costly (Houston, Lev, and Tucker 2010). We control for firm fixed effects in all regression models. We additionally include a lagged term for the dependent variable and the inferences are not affected.

To further support our causal inferences, we further exploit the *termination* of the TSPP in September 2018. If the quantity of voluntary disclosure reverses after the termination, it is more likely that our findings are due to the pilot program. We show that the frequency of management earnings forecasts reverses for treatment firms after the termination and that the post-termination disclosure level is not significantly different from the pre-implementation level. This finding further supports the idea that the change in management guidance is driven by the TSPP instead of other confounding factors.

As we argue that the liquidity decline during the TSPP leads to less voluntary disclosure, we expect that managers will reduce their voluntary disclosure to a larger extent when the liquidity decrease is severe. Therefore, we perform cross-sectional analyses on the strength of liquidity reduction amid the TSPP. To gauge the strength of the liquidity reduction, we use both *ex-ante* and *ex-post* measures. From an *ex-ante* perspective, we expect firms with a binding increase in tick size (firms with pre-TSPP quarterly bid-ask spreads below \$0.05) to experience a stronger impact on stock liquidity.[5] Consistent with our expectations, we find that the effect is stronger for firms with a binding increase in bid-ask spreads.

We also use changes in bid-ask spreads, dollar trading volume, stock turnover, and Amihud's (2002) illiquidity ratio surrounding the start of the TSPP as *ex-post* proxies for the intensity of the TSPP's impact on liquidity. The change in liquidity around the start of the pilot program measures the actual impact of the TSPP on stock liquidity. We show that the decline in management guidance is more pronounced for firms with an actual decrease in dollar trading volume or stock turnover and firms with an actual increase in bid-ask spreads or Amihud's

[5] The binding increase means that a stock has a bid-ask spread less than \$0.05 before the TSPP. After the TSPP switches the tick size to \$0.05, the stock's tick size will be mechanically increased to \$0.05. Research shows that stocks with binding tick size lose more liquidity after the tick size increases (Harris 1994; Harris 1997; Albuquerque et al. 2020).

(2002) illiquidity ratio. These findings lend support to the idea that the liquidity effect of the TSPP explains the decrease in voluntary disclosure.

Our main hypothesis is built on the notion that liquidity decreases reduce managers' capital-market benefits of voluntary disclosure. In line with this argument, we expect that firms with lower expected marginal benefits of voluntary disclosure before the TSPP are more likely than others to refrain from disclosure after the TSPP. The intuition is that, if we hold the costs of disclosure constant, for firms with low expected marginal benefits, it is more likely that the negative liquidity shock shifts their marginal benefits of disclosure below the marginal costs. Thus, these firms may reduce their voluntary disclosure to a point such that their marginal costs are no larger than the marginal benefits.

To test this prediction, we assume that firms that are more transparent in the pre-TSPP period have lower marginal benefits of voluntary disclosure than opaque firms because the demand for voluntary disclosure of transparent firms tends to be lower than that for opaque firms. Specifically, we use pre-TSPP level of analyst coverage, institutional holdings, and analyst forecast dispersion as proxies for transparency and expected marginal benefits of disclosure. The results support our expectation. We find that the decline in the frequency of management earnings forecasts for treatment firms is mainly driven by firms with broader analyst coverage, more institutional holdings, and lower forecast dispersion before the TSPP. Meanwhile, firms with lower analyst coverage and institutional holdings and higher forecast dispersion before the TSPP do not exhibit a significant change. We also consider pre-TSPP levels of stock liquidity as another proxy for transparency before the TSPP, and our inferences are robust.

Some recent studies on the TSPP show that the change in tick size impedes algorithmic trading while enhancing earnings quality and information acquisition around earnings announcements (Ahmed, Li, and Xu 2020; Lee and Watts 2021). It is possible that these effects of the TSPP also play a role in corporate disclosure decisions and the relation between liquidity decline and voluntary disclosure. We consider and test these effects.

First, Lee and Watts (2021) show that, relative to control firms, pilot firms experience a reduction in algorithmic trading and an increase in fundamental information acquisition through EDGAR after the TSPP. It is possible that the change in algorithmic trading and investors' information acquisition play a role in corporate disclosure decisions. In cross-sectional analyses, we partition firms based on the four algorithmic trading measures used by Lee and Watts (2021) and the number of non-robot EDGAR search volume. We do not find evidence that algorithmic trading or fundamental information acquisition explain our results.

Second, as earnings quality for pilot firms improves after the TSPP as per Ahmed et al. (2020), it is possible that managers also increase voluntary disclosure because mandated financial reporting and voluntary disclosure can be complements (Ball, Jayaraman, and Shivakumar 2012). It is also possible that enhanced earnings quality substitutes the need for voluntarily disclosed information, as more reliable earnings information can enhance investors' ability to correctly value a firm. To test this possible explanation for our findings, we partition firms based on the sign of the change in discretionary accruals around the TSPP. We do not find evidence that the two subsamples are different in terms of the change in voluntary disclosure. To further address potential alternative explanations, we conduct additional cross-sectional analyses and find no evidence that changes in analyst coverage or institutional holdings explain our findings.[6]

To generalize our findings beyond those provided by management guidance, we employ conference calls and voluntary 8-Ks as alternative voluntary disclosure proxies. We show that

[6] These tests are different from those using pre-event levels of analyst coverage and institutional holdings. Pre-event levels capture firms' expected marginal benefits of voluntary disclosure, and we expect that firms with lower marginal benefits would be more likely to refrain from voluntary disclosure than others when facing the liquidity decline. In contrast, we use the changes of analyst coverage and institutional holdings to check if the decrease in voluntary disclosure is simply driven by the change in the information demand from institutions and analysts.

treatment firms also reduce the frequency of conference calls and voluntary 8-K filings after the TSPP relative to control firms.

Our study contributes in several ways. First, our paper contributes to research on the relation between stock liquidity and voluntary disclosure (e.g., Diamond and Verrecchia 1991; Welker 1995; Balakrishnan et al. 2014). Using a natural experiment conducted by the SEC, we show the role of liquidity in shaping disclosure practices. Studies suggest that voluntary disclosure is useful in shaping stock liquidity and that managers employ voluntary disclosure as a strategy to improve liquidity (Graham, Harvey, and Rajgopal 2005; Balakrishnan et al. 2014; Guay et al., 2016; Schoenfeld 2017). We contribute by showing that stock liquidity also affects voluntary disclosure. Our study also highlights a different angle of understanding the relation between disclosure and liquidity. In addition to the idea that liquidity is an outcome of the information environment, our findings indicate that the portion of liquidity associated with the market microstructure can also affect disclosure. Further, our findings may also help to understand why some opaque firms do not disclose or stop disclosing even if their liquidity is low.

Second, we provide timely evidence on the impact of the TSPP from an accounting perspective. Extant studies investigate market microstructure changes and asset pricing implications (e.g., Albuquerque et al. 2020; Chung et al. 2020). In contrast, our study shows indirect and *unintended* impacts of the TSPP by focusing on corporate disclosure decisions. Related to our study, Lee and Watts (2021) document an increase in fundamental information acquisition from the SEC EDGAR subsequent to the TSPP. Also, Ahmed et al. (2020) show a decrease in discretionary accruals.[7] Our study complements these studies and advances our

[7] Note that we include discretionary accruals as a control variable in all of our specifications. Our inferences are not affected by including or removing this variable.

understanding of how the TSPP affects the corporate information environment by showing that the TSPP has an adverse impact on voluntary disclosure.

Third, we add to research that examines the effect of stock liquidity on corporate decisions such as payout policies and compensation plans (e.g., Banerjee, Gatchev, and Spindt 2007; Jayaraman and Milbourn 2012; Edmans, Fang, and Zur 2013; Fang, Tian, and Tice 2014; Li, Ye, and Zheng 2020). We show that liquidity also shapes voluntary disclosure. Noticeably, voluntary disclosure, unlike other corporate decisions, has a simultaneous relation with stock liquidity. On one hand, as we show in this study, stock liquidity affects the decision environment of voluntary disclosure. On the other hand, as suggested by the literature, voluntary disclosure also influences liquidity through resolving information asymmetry.

2. Background and Hypotheses Development

2.1 The SEC Tick Size Pilot Program

The recent decades have seen an evolution in tick sizes in the U.S. stock market – from one-eighth to one-sixteenth, and then to the decimalization in 2001. The tick size is the minimal price variation for quoting and trading a stock and can have a substantial impact on stock liquidity by reshaping the market structure (Grossman and Miller 1988; Harris 1994; Harris 1997; Goldstein and Kavajecz 2000). Specifically, a small tick size benefits liquidity demanders by reducing the bid-ask spreads, while a large tick size increases the margin of liquidity provision and induces a wealth transfer from liquidity providers to demanders (Grossman and Miller 1988; Harris 1997).

Although smaller tick size is associated with smaller spreads (Bessembinder 2003), decimalization may not be good to small-capitalization firms (Harris 1997; Furfine 2003). Due to the severe adverse selection problem in small-capitalization stocks, market makers are more vulnerable to trading from better-informed traders in small-capitalization stocks than in large-capitalization stocks. Thus, they are more likely to step away from small-capitalization stocks when the margin from liquidity provision is reduced by decimalization (Glosten and Milgrom 1985; Bhattacharya and Spiegel 1991; WSJ 2016). Consequently, small-capitalization stocks in the U.S. equity market have a difficult time in obtaining equity financing due to their illiquidity (Grant Thornton 2013).

In order to improve the liquidity of small-capitalization firms, in 2012, the JOBS Act assigned the SEC the task of studying how decimalization (trade at $0.01) affects IPOs and the stock liquidity of small-capitalization emerging growth stocks. Supporters of the TSPP posit that a wider tick size incentivizes market makers' liquidity provision through increasing their expected payoffs (Grant Thornton 2013; WSJ 2016).

The Financial Industry Regulatory Authority (FINRA) and the National Securities Exchanges formally proposed the TSPP on August 25, 2014. On May 6, 2015, the SEC approved the plan and decided to implement it on October 3, 2016 for two years. The SEC started the data collection and stock selection process on March 7, 2016. The sample selection process involves a stratified sampling based on market capitalization, share price, and trading volume. The process ensures that stocks in the test and control groups are comparable in terms of the three dimensions. On September 3, 2016, the SEC released the full list of pilot stocks.[8] The program includes approximately 1,200 treatment stocks and 1,400 control stocks. Figure 1 shows the timeline of the TSPP.

[8] The list of firms involved in the TSPP was available to the public only one month before the start of the pilot program. Hence, it is not likely that firms and investors have already responded to the pilot program before its implementation.

On October 3, 2016, the TSPP commenced, and the tick size changes went into effect in a staggered fashion. All the test stocks had switched to a \$0.05 tick size before November 2016.[9] The TSPP finally ended on September 28, 2018 and the tick size returned to \$0.01 for all stocks.

Studies on the TSPP primarily focus on the market microstructure and asset pricing implications of the widened tick size. However, contrary to the intention of the TSPP, most studies find a decrease in many aspects of stock liquidity for pilot firms. For example, Albuquerque et al. (2020) show that, relative to control firms, pilot firms experience an increase in quoted spreads and effective spreads as well as a decrease in trading volume after the TSPP. Li et al. (2020) also provide evidence that widened tick size reduces firm-level stock liquidity.

In addition to the findings regarding firm-level liquidity, Rindi and Werner (2019) and Chung et al. (2020) study the effects of the TSPP on order-level liquidity. Consistent with Harris (1997), their results indicate that the effects are heterogeneous for orders of different sizes – large-size orders benefit from the TSPP due to improved market depth, while small-size orders are more costly because of increased spreads.

Our study is premised on the finding that increased tick size reduces firm-level stock liquidity. We believe it is intuitive that managers tend to focus on the overall liquidity of their stocks instead of the liquidity of a single order. In addition, it is not clear how the liquidity of individual orders is associated with firm-level overall liquidity, as it depends on the composition of orders and the trading strategies of investors (e.g., splitting orders or aggregating orders).

Beyond the liquidity effect, other studies on the TSPP examine the effects on dark pool trading (Li et al. 2020; Thomas, Zhang, and Zhu 2020), algorithmic trading (Lee and Watts

[9] For the following two-year period, the pilot stocks stayed with the \$0.05 tick size and were not able to opt-in a particular group or opt-out of the program, unless (1) the stocks are delisted, (2) the stock prices drop below \$1, or (3) the firms engage in merger and acquisition.

2021), discretionary accruals (Ahmed et al. 2020), and corporate decisions such as payout policies and investments (Li et al. 2019; Ye, Zheng, and Zhu 2021). Among these studies, Ahmed et al. (2020) and Lee and Watts (2021) are most relevant to our study because they show the effects of the TSPP on other aspects of the corporate information environment. Specifically, Lee and Watts (2021) argue and show that, due to the decline in algorithmic trading, the information acquisition through EDGAR for pilot firms increases following the TSPP, especially around earnings announcements. In line with the information acquisition effect, Ahmed et al. (2020) conclude that earnings quality improves for treatment firms with more scrutiny from investors.

These findings may *not* directly speak to how voluntary disclosure would change after the tick size change. First, it is not clear whether the liquidity effects or the information environment effects of the TSPP dominate managers' voluntary disclosure decisions after the TSPP. Second, improved earnings quality does not guarantee more voluntary disclosure. On one hand, Ball et al. (2012) show that mandatory disclosure and voluntary disclosure can be complements, indicating that improved earnings quality may lead to more voluntary disclosure. On the other hand, Einhorn (2005) suggests that the propensity of voluntary disclosure is non-monotonically associated with the information quality of mandatory disclosure. It is thus possible that pilot firms reduce voluntary disclosure despite the enhanced earnings quality. Third, increased information acquisition through EDGAR may result from insufficient information supply from managers. Meanwhile, more information acquisition may push managers' to disclose more information. As a result, it is unclear how information acquisition is associated with voluntary disclosure because information acquisition activities can be both the trigger for more disclosure and the consequence of insufficient disclosure.

2.2 Hypotheses

Firm-provided voluntary disclosure is one of the most important information sources for the market. Beyer, Cohen, Lys, and Walther (2010) report that management guidance is one of the most important channels for voluntary disclosure and accounts for 55% of stock return variations driven by accounting-based information (see also De Franco and Hope 2011). Firms actively employ voluntary disclosure to influence the capital market, with the aim to increase liquidity and stock prices and to reduce litigation risks (e.g., Skinner 1997; Graham, Harvey, and Rajgopal 2005).

Based on the theoretical framework developed by Diamond and Verrecchia (1991), one of the primary capital market goals of issuing voluntary disclosure to improve stock liquidity (Leuz and Wysoski 2008). Under the umbrella of this theory, the literature provides considerable evidence that voluntary disclosure affects liquidity (e.g., Welker 1995; Coller and Yohn 1997; Schoenfeld 2017). In line with the theoretical arguments, Graham et al. (2005) find in their survey that managers voluntarily communicate with investors to increase the overall liquidity of their stocks, especially when liquidity is low. Balakrishnan et al. (2014) report that increased voluntary disclosure improves liquidity. They find that firms voluntarily disclose more information after a negative impact to the information environment to improve their stock liquidity.

In contrast to studies that explore how disclosure affects liquidity through its effects on the information environment, our study examines the *opposite* direction – whether and how stock liquidity affects managers' incentive to issue voluntary disclosure. More importantly, our study differs from prior research in that we exploit a setting where firms experience an exogenous shock to liquidity without experiencing a shock to information asymmetry. As managers learn from the market and adjust their decision making based on market conditions (Bakke and

Whited 2010; Foucault and Fresard 2014; Zuo 2016), we argue that firm-specific stock liquidity may affect the benefits managers can gain from voluntary disclosure.

Related to but different from the liquidity motive, another major benefit of voluntary disclosure is that managers can use voluntary disclosure to shape the market's expectations and thus affect stock prices (Trueman 1986; Cotter, Tuna, and Wysocki 2007; Versano and Trueman 2017). By influencing short-term stock prices through voluntary disclosure, managers can obtain favorable outcomes for seasoned equity offerings (Lang and Lundholm 2000), execution of stock options (Aboody and Kasznik 2000), and insider trading (Noe 1999). However, managers' ability to successfully influence short-term stock prices through disclosure relies on the extent to which the market responds to their disclosure. Liquidity reflects the intensity of trading and is negatively associated with trading costs for investors. When stock liquidity decreases with the widened tick size, the market may exhibit an attenuated response to new information. The reason is two-fold. First, with insufficient liquidity, investors face higher costs and frictions when they would like to trade on the new information and thus may refrain from trading. As a result, investors become inattentive and underreact to new information when stock liquidity is insufficient (Hirshleifer and Teoh 2003; Hirshleifer, Lim, and Teoh 2011; Li and Xu 2012).[10] Due to attenuated market response, managers may expect lower benefits from issuing voluntary disclosure and thus refrain from disclosure in the first place.[11] Second, investors may have lower information demand because of reduced stock trading and pay insufficient attention

[10] Low stock liquidity due to the TSPP adds additional costs and frictions for stock trading. The TSPP increases the bid-ask spreads and reduces the trading volume of pilot stocks. As a result, investors may incur additional trading costs or even cannot find a counterparty to trade with. If these costs exceed investors' benefits of trading on new information, they may refrain from trading, thus leading to insufficient market response to corporate disclosure.

[11] In untabulated analyses, we show that, relative to control firms, pilot firms experience a significant decrease in the magnitude of three-day cumulative abnormal returns and dollar trading volume around management forecast dates after the TSPP. We also find that pilot firms have larger post-management-guidance drifts than control firms after the TSPP. These results are consistent with our argument that reduced liquidity attenuates market response and leads to insufficient reaction to voluntary disclosure.

to information disclosure.[12] As a result, managers may respond to decreased information demand and limited attention by disclosing less (Abramova et al. 2020). Accordingly, we state the primary hypothesis in the alternative form as follows:

H1: Firms reduce voluntary disclosure when their stock liquidity decreases due to the increased tick size.

Figure 2 provides an intuitive illustration of H1. Consistent with theory work on voluntary disclosure and stock liquidity, we assume that the marginal benefits (costs) of voluntary disclosure are diminishing (increasing) with respect to disclosure levels. Holding other factors constant, managers will issue voluntary disclosure when their marginal benefits exceed marginal costs and choose non-disclosure otherwise.

In Figure 2(a), we show the situation before the exogenous liquidity decrease. "Benefits (0)" correspond to the marginal benefits of disclosure before the liquidity decrease. "Costs" denote marginal costs. Under this situation, firms' first best disclosure level is D*, where their marginal benefits of disclosure equal to marginal costs.

As we argue that a negative liquidity shock would reduce managers' marginal benefits of disclosure, the shock will shift the marginal benefit curve to the left. In Figure 2(b), we plot the case after the exogenous liquidity decrease, in which "Benefits (0)" and "Benefits (1) correspond to the marginal benefits of disclosure before and after the liquidity decrease, respectively. We can see that a negative liquidity shock shifts marginal benefits of disclosure and firms' first best disclosure level to the left. This is consistent with our prediction from our first hypothesis.

[12] If investors generally pay less attention to voluntary disclosure when liquidity is low or when they are not trade actively, it is also possible that the negative liquidity shock attenuates the marginal benefits of disclosure in a broader sense through effects beyond immediate market reactions.

The tick size change likely will not impact every firm identically but may have heterogeneous impacts on stock liquidity of different firms. We expect that the more severe the negative liquidity shock, the larger the extent to which marginal benefits of disclosure are reduced. In other words, the negative liquidity shock would shift the marginal benefit curve to the left to a larger extent in Figure 2(b) when liquidity loss is stronger. Therefore, we expect that firms experiencing a larger drop in liquidity will reduce their voluntary disclosure to a larger extent. We state the hypothesis in the alternative form as below:

H2: Firms reduce voluntary disclosure to a larger extent when their stock liquidity is more negatively impacted by the increased tick size.

However, in line with the view that managers improve liquidity via voluntary disclosure (e.g., Diamond and Verrecchia 1991; Balakrishnan et al. 2014), it is also possible that managers issue more voluntary disclosure to counteract the negative liquidity impact from the TSPP. This case is possible when managers believe their marginal benefits exceed the marginal costs of disclosure despite the liquidity decrease.

We argue that firms that are relatively opaque before the negative liquidity shock have weaker incentives to reduce voluntary disclosure. These relatively opaque firms may not disclose enough before the shock, and their marginal benefits of disclosure exceed marginal costs to a larger extent than those transparent firms. When both transparent and opaque firms are hit by the negative liquidity shock, it is more likely that the liquidity shock does not shift opaque firms' marginal benefits of disclosure below marginal costs. In contrast, it is more likely for firms with low marginal benefits (i.e., the transparent firm) that the liquidity impact shifts their marginal benefits of disclosure below marginal costs.

We illustrate this idea in Figure 2(b). If a firm maintains a pre-shock disclosure level between D** and D*, they are likely to reduce their disclosure because their marginal costs of disclosure are higher than the marginal benefits. In contrast, firms with a pre-shock disclosure level below D** may issue more voluntary disclosure because their marginal benefits are still higher than marginal costs despite the negative liquidity shock. In our empirical analysis, we assume that opaque firms are more likely to be firms with pre-shock disclosure level below D**.[13] We expect that, with an exogenous liquidity shock, firms that are relatively opaque may have less incentives to reduce disclosure. It is even possible that they may have the incentive to increase disclosure to counteract the effect of the liquidity shock. Meanwhile, firms that are relatively transparent may choose not to disclose. We state the hypothesis in the alternative form as follows:

H3: Transparent firms reduce voluntary disclosure to a larger extent than opaque firms when their stock liquidity is negatively impacted by the increased tick size.

Ahmed et al. (2020) and Lee and Watts (2021) find that the tick size change affects algorithmic trading, fundamental information acquisition through EDGAR, and discretionary accruals. These findings may be associated with voluntary disclosure decisions and explain our findings. For example, improved information acquisition through EDGAR and investors' scrutiny on financial reports may increase investors' demand for more corporate information and thus induce managers to issue more voluntary disclosure. Also, improved information acquisition from EDGAR around earnings announcements may explain why the tick size change mutes the market reaction to voluntary disclosure. This view implies a competing explanation on attenuated market reaction to disclosure beyond the effects of decreased

[13] It is difficult to quantify D* and D** in Figure 2(b) for empirical studies. Therefore, we cannot tell exactly which firms maintain disclosure levels between D** and D* or which firms are below D** before the TSPP. However, firms with disclosure levels below D** are relatively opaque compared to other firms, and thus we use opaque (transparent) firms to represent firms with disclosure levels below D** (between D** and D*) in our empirical implementation.

liquidity as we argued in our hypothesis.[14] It is also possible that improved earnings quality shown in Ahmed et al. (2020) is related to changes in voluntary disclosure practices. In addition, reduced algorithmic trading may also be associated with firms' information environment and the changes in disclosure practices (Hendershott, Jones, and Menkveld 2011; Weller 2018).

We do not propose directional hypotheses for these possible alternative explanations on why the tick size change affects voluntary disclosure but test the above potential explanations in our empirical analyses. If any of these alternative explanations holds, we would expect that the treatment effect of the TSPP on voluntary disclosure would vary with the corresponding variable, such as discretionary accruals, measures for EDGAR search activities, and algorithmic trading proxies.

3. Research Design

3.1 Model

Our objective is to examine whether and how the liquidity change from the TSPP shapes firms' voluntary disclosure. We adopt a difference-in-differences (DID) design outlined in equation (1) on a firm-quarter sample. Specifically, the outcome variables are proxies for voluntary disclosure. *TREAT×START* is our test variable. *TREAT* is an indicator variable that equals one if a firm is in the pilot group of the TSPP; START is an indicator variable that equals one for all the quarters after the start of the TSPP (2016Q4). We include several control variables as well as firm and year-quarter fixed effects. Firm fixed effects control for time-

[14] Accordingly, we want to ensure that the attenuated market reaction to voluntary disclosure is not driven by enhanced information acquisition from EDGAR. In addition to showing that the tick size change mutes the market reaction to subsequent voluntary disclosure, we also examine if the increase in information acquisition activities further enhances the attenuation effect (untabulated). However, we find no evidence supporting this view.

invariant firm characteristics within the short sample period such as corporate governance and auditing.[15] We cluster standard errors at the firm level.

$$VolDisc_{i,t} = \beta_0 + \beta_1 TREAT_i \times POST_t + CONTROLS_{i,t} + \alpha_i + \gamma_t + \varepsilon_{i,t} \tag{1}$$

For the main analyses, we include eight quarters before and after the TSPP starting quarter in the regression sample (i.e., quarter *t*-8 to *t*+7 relative to the starting quarter *t*). In robustness checks, we implement different sample period specifications.

3.2 Data and Variables

We obtain data on the TSPP from the SEC's (and FINRA's) website, which contains a complete list of the treatment and control stocks in the pilot program. We collect management earnings forecasts from I/B/E/S Guidance and use Compustat Fundamental Quarterly, CRSP, I/B/E/S, and Thomson Reuters 13F to construct control variables.

We use management earnings forecasts as the primary measure of voluntary disclosure (e.g., Penman 1980; Beyer et al. 2010; Balakrishnan et al. 2014). Specifically, we use the natural logarithm of the number of management earnings forecasts in a quarter (*NUM MEF*) as our main dependent variable and also consider an indicator variable of the incidence of management guidance in a quarter (*ISSUE MEF*).

We also examine the characteristics of management guidance. Specifically, we consider the horizon of management earnings forecasts and define long-term forecasts (*NUM LTF*) as those with a forecast period at least six months after the announcement date and short-term forecasts (*NUM STF*) as those with a forecast period within six months after the announcement

[15] In untabulated results, we nevertheless include such characteristics as control variables. Our inferences are not affected.

date.[16] We also examine management guidance issued with and without earnings announcements and we define a management earnings forecast as bundled forecasts (*NUM BUNDLED*) if the announcement date overlaps with the quarterly earnings announcement dates obtained from Compustat and stand-alone forecasts (*NUM ALONE*) otherwise.

To extend our analyses, we also employ other voluntary disclosure proxies such as the number of voluntary 8-Ks, and conference calls (*NUM VOL 8K*, and *NUM CALL*). We extract 8-K filings from SEC EDGAR and follow He and Plumlee (2020) to classify 8-Ks into mandatory 8-Ks and voluntary 8-Ks.[17] For conference calls, we use the data provided by Capital IQ and define *NUM CALL* as the number of conference calls in a quarter.

Consistent with the literature, we control for firm characteristics including firm size (*SIZE*), cash holding (*CASH*), return on assets (*ROA*), market-to-book ratio (*MB*), sales growth rate (*SALEGROWTH*), tangibility (*TANGIBILITY*), leverage ratio (*LEV*), discretionary accruals estimated using the modified Jones model (*ACCR*), quarterly stock returns (*RET*), and quarterly stock return volatility (*STDRET*). We include the number of analysts covering the firm (*AF*) and the percentage of institutional ownership (*INST*) to control for the information environment. We also include liquidity measures (bid-ask spreads (*SPREAD*), dollar trading volume (*DOLVOL*), turnover rate (*TURNOVER*), and Amihud's (2002) illiquidity measure (*ILLIQ*)), algorithmic trading measures (odd lot ratio (*OLR*), trade-to-order ratio (*TOR*), cancel-to-trade ratio (*CTR*), and average trade size (*ATS*)), and an information acquisition measure (EDGAR search volume (*ESV*)) as additional control variables.[18] All variables are winsorized at the 1st and 99th percentiles, and the Appendix provides detailed variable descriptions.[19]

[16] Our inferences are unaffected by defining long-term forecasts using alternative horizons such as three months and one year.
[17] We follow He and Plumlee (2020) and define 8-Ks with item codes 2.02 (Results of Operations and Financial Conditions), 7.01 (Regulation FD Disclosure), or 8.01 (Other Events) as voluntary 8-Ks.
[18] We obtain data from SEC MIDAS to calculate algorithmic trading measures and use the EDGAR search volume data provided by http://www.jamesryans.com/. See Ryans (2017) for details on the measure.
[19] The inferences are unaffected by winsorization.

3.3 Data and Sample

The sample period ranges from quarter *t*-8 to *t*+7 relative to the starting quarter *t* (2016Q4). We exclude financial industry firms (SIC 6000 to 6999) from the sample as well as observations that have missing values. We use the treatment and control stocks as defined by the TSPP in line with Lee and Watts (2021). The full sample comprises 15,465 firm-quarter observations, with 536 unique treatment firms and 540 unique control firms in the sample. Table 1 shows the summary statistics. The mean of *ISSUE MEF* is 0.257, indicating that 25.7% of firm-quarters in the full sample have at least one management earnings forecast.[20] For firms issuing guidance, the maximum *NUM MEF* is 1.609, corresponding to four management guidance events in a quarter. The sample mainly consists of small-capitalization firms, as the mean of SIZE is 19.847, corresponding to a market capitalization of $416 million.

Table 2 tabulates the univariate comparison of each variable between the treatment group and the control group for both pre-TSPP and post-TSPP periods. The first three columns display the differences between the two groups before the TSPP. The two groups are not significantly different in firm size, profitability, and analyst coverage before the TSPP, but they do differ in other characteristics and management guidance practices – treatment firms are more likely than control firms to have management earnings forecasts.[21] However, pre-treatment differences do not indicate the violation of the parallel trend assumption, and we validate the assumption in Table 4. To further ensure the robustness, we use an alternative control group defined from the Compustat universe as well as a sample constructed using propensity score matching (see Section 5.3). All inferences are robust to alternative control samples. The last

[20] The percentage of firms with management guidance is relatively low because our sample mainly consists of small-cap firms. Our inferences are robust to the removal of firms with no management guidance before the start of the TSPP.

[21] As the sample selection process of the TSPP accounts for market capitalization, trading volume, and share price, the treatment group and the control group may still be different in other firm characteristics.

three columns show the differences between the two groups after the TSPP. The differences in the frequency of management guidance, voluntary 8-Ks, and conferences calls between two groups decrease after the program, and the decline is mainly driven by the decrease in the treatment group.

4. Empirical Results

4.1 Primary Results

Panel A of Table 3 reports the estimates of equation (1) and tests H1. The dependent variable is *NUM MEF* for columns (1) to (6) and *ISSUE MEF* for column (7). Column (1) shows the result of a DID regression without control variables or fixed effects, which yields a coefficient of -0.035 (t = -2.38) for *TREAT×START*, indicating that treatment firms significantly decrease the frequency of management earnings forecasts after the start of the TSPP relative to control firms. In terms of economic magnitude, the coefficient of *TREAT×START* in column (1) translates into a 15% decrease relative to the mean of the number of forecasts. In addition, *START* has a coefficient of 0.013 (t = 1.22), which implies that control firms do not experience a significant change in management guidance practices. This result indicates that the TSPP induces treatment firms to issue fewer management earnings forecasts.

For columns (2) to (6), we include control variables as well as both firm and year-quarter fixed effects.[22] In column (2), we include control variables for firm characteristics but no fixed effects. In column (3), we only include *TREAT×START* and fixed effects without other controls. In column (4), we add firm characteristics and stock performance variables to control for factors that may be associated with voluntary disclosure. We further include the information

[22] The firm fixed effects subsume the effect of *TREAT*, and the year-quarter fixed effects subsume the effect of *START*. In untabulated analyses, we find that all inferences are unaffected if we replace the firm fixed effects with industry effects or if we remove all fixed effects.

acquisition proxy and algorithmic trading measures in columns (5).[23,24] Across all these specifications, the inference from column (1) remains.[25]

Noticeably, management guidance practices tend to be sticky because the sudden cessation of guidance incurs significant costs for the firm (e.g., Houston, Lev, and Tucker 2010). Although we control for firm fixed effects, we consider an even more conservative specification in column (6) by incorporating the one-period lagged term of *NUM MEF*. The magnitude of the coefficient for *TREAT×START* decreases by roughly one third compared to that in column (1), but the coefficient is still significant at the 1% level, implying that guidance stickiness does not drive the findings.

In column (7), we use an indicator for the incidence of management earnings forecasts as the dependent variable and estimate an OLS model with both firm and year-quarter fixed effects.[26] The coefficient for *TREAT×POST* is negative and significant, indicating that treatment firms are less likely to issue guidance relative to control firms after the TSPP.[27]

As our sample mainly consists of small firms that do not issue management guidance throughout the sample period, in Panel B of Table 3, we restrict our analyses to firms that issue at least one management earnings forecast in the period before the TSPP (i.e., quarter *t*-8 *to t*-1). The sample contains 5,692 firm-quarter observations with 212 treatment firms and 186

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[23] Weller (2018) points out that algorithmic trading measures are highly correlated. Thus, we also include these four algorithmic trading measures into regressions one-by-one in untabulated analyses. Our estimates for treatment effects are similar, and our inferences are not affected.
[24] The sample size is smaller for columns (5) and (6) because some firms cannot be matched with the SEC MIDAS data to calculate algorithmic trading proxies.
[25] Our conclusions (here and below) are robust to alternative econometric models for count variables, including Poisson and negative binomial models (untabulated). In addition, our inferences are not affected if we use the tick size pilot program as an instrument variable for stock liquidity measures and implement the two-stage least square approach. The results are not tabulated here for brevity.
[26] We employ LPM due to the extensive fixed effect structure. However, our inferences are robust to employing a Logistic regression model. In untabulated results, we show that the marginal effect from the Logistic model (fixing all other variables at their mean value) is comparable to the estimates from the linear model.
[27] Most of the control variables do not load in the regressions with fixed effects in Table 3. A likely reason is that firm fixed effects absorb most of the variation in the control variables.

control firms. Column (1) shows that our primary result also holds, with the coefficient estimate of -0.087 (t = -2.76) for *TREAT×POST*.

We argue that decreased liquidity impairs the market's ability to incorporate disclosure information into stock prices in the short term. Accordingly, we expect that managers are inclined to cut disclosure with short horizons because short-horizon disclosure is more impacted by attenuated reactions in the short run (Bergman and Roychowdhury 2008). Consistently, in columns (2) and (3) of Panel B, we classify management guidance into short-term and long-term ones using six months as the cut-off horizon and find that most of the reduction in *NUM MEF* is attributed to short-term forecasts. The results suggest that treatment firms are more likely to stop providing short-term forecasts than long-term forecasts after the tick size change. We also find that the decrease in management guidance is more pronounced for management guidance bundled with earnings announcements in columns (4) and (5).[28]

4.2 Identifying Assumptions for Difference-in-Differences Analyses

To validate the parallel trend assumption of using the DID design, we estimate the DID model by interacting *TREAT* with an indicator variable for each quarter to show the dynamics of the treatment effect. Panel A of Table 4 displays the results. The benchmark period for these regressions is quarter t-8 to quarter t-5 relative to the starting quarter t. Compared to the benchmark period, we do not observe significant treatment effect for quarters t-4 to t-1. The coefficients become negative and significant for the quarters after t and are quite persistent.[29] These results help to justify the use of the DID design. One noticeable observation is that the

[28] As the TSPP focuses on small-cap firms, our sample contains a sizable number of firms that never issue management guidance. Therefore, we restrict the comparison between the effects on short-horizon and long-horizon forecasts and bundled and unbundled forecasts to be within issuing firms because issuing firms may be systematically different from non-issuing firms. Also, we are not able to compare short-horizon with long-horizon forecasts if a firm never issues management forecasts.

[29] The estimate is not significant for quarter t+1 in column (1) (t-statistics = -1.22) and quarter t in column (2).

treatment effect does not occur immediately, which is consistent with the view that management guidance tends to be a sticky decision. Figure 3 provides a visualization of these results.

To further ensure that the change in voluntary disclosure is not driven by confounding factors, we conduct two sets of placebo analyses. Specifically, we create pseudo-implementation quarters by shifting the actual starting quarter of the TSPP either four quarters ahead or after and include four quarters before and after the starting quarter in the sample.[30] In Panel B of Table 4, we re-estimate the treatment effects using the pseudo-events but do not find significant results in any of the columns. The placebo analyses, together with the parallel trend tests, support a causal relation between the liquidity decrease and the reduction in voluntary disclosure.

Furthermore, the pilot program ended on September 28, 2018, and the tick size went back to $0.01 for pilot stocks after that date. If the change in voluntary disclosure is driven by the pilot program, we would expect a reversal effect after the end of the pilot program. Consequently, we assess whether voluntary disclosure activities recover for treatment firms after the termination of the pilot program. We define 2018Q3 as the termination quarter and use a sample that ranges from four quarters before the implementation quarter to 2019Q4 to estimate both the implementation and termination effects of the *TSPP* in a single regression. Specifically, we define *START* to equal one for all quarters between 2016Q4 and 2018Q3, and END to equal one for all quarters after 2018Q3. We include two interaction terms, *TREAT*×*START* and *TREAT*×*END*, in the regression model and control for firm and year-quarter fixed effects.[31]

[30] We do not use eight quarters before and after the pseudo-implementation quarter for the placebo tests because the post-termination period (quarters after 2018Q3) would be included. The estimates would then be contaminated by the reversal effects after the end of the pilot program.

[31] As a result, the benchmark period for this model is 2015Q4 to 2016Q3.

We show the results in Panel C of Table 4. The negative and significant coefficients of *TREAT×START* confirm the baseline findings that treatment firms reduce their management guidance during the TSPP period relative to control firms. The coefficients of *TREAT×END* are negative but not significant, indicating that the treatment effect of the TSPP after the termination is not significantly different from that of the pre-TSPP period. To show the termination effect more directly, we test the difference in coefficients between *TREAT×START* and *TREAT×END*, which captures the reversal effect. For both columns, the reversal effect is significant at the 10% level (using two-sided tests). The reversal after the termination further supports the finding that it is the TSPP, instead of other factors, that causes the reduction of voluntary disclosure of treatment firms.

4.3 The Effect of Liquidity Decreases

H1 argues that the liquidity decrease leads to the reduction in voluntary disclosure activities. If liquidity changes during the TSPP indeed explain our findings, we expect that the observed effects will be more pronounced for firms with larger decreases in liquidity due to the TSPP (i.e., H2). In this section, we test H2 using a set of cross-sectional analyses based on the severity of liquidity decrease during the TSPP.

To measure the severity of the liquidity impact of the TSPP, we consider both *ex-ante* and *ex-post* proxies. Specifically, we use pre-TSPP spreads as an *ex-ante* proxy for the intensity of the impact on stock liquidity from the TSPP. We partition the sample using the pre-TSPP spreads defined as the quarterly bid-ask spreads of quarter *t*-1 and use \$0.05 as the cut-off.[32] For stocks with spreads below \$0.05 before the TSPP (i.e., stocks with pre-TSPP binding bid-

[32] The sum of the number of observations differs slightly from the sample size in our primary results. It is because a few firms may have missing values for the corresponding partitioning variable of quarter *t*-1 and thus do not have observations for quarter *t*-1 after we remove missing values in our sample construction process.

ask spreads), their spreads mechanically increase to at least \$0.05 after the TSPP. Thus, these stocks experience a more severe liquidity drop than other stocks (Albuquerque et al. 2020).

In columns (1) and (2) of Table 5, we tabulate the results for the cross-sectional analyses based on *ex-ante* severity of liquidity decreases during the TSPP. As shown in columns (1) and (2), firms with a binding increase in the tick size reduce their frequency of guidance to an extent (coefficient = -0.038, t = -2.61) larger than other firms (coefficient = -0.023, t = -1.41). The difference is statistically significant. The results support H2 and suggest that firms with a larger expected liquidity decline during the TSPP reduce management guidance activities to a larger extent.

Next, we employ realized changes in the dollar trading volume, stock turnover, bid-ask spreads, and Amihud's (2002) illiquidity measure as *ex-post* proxies for the intensity of the TSPP's impact on liquidity. To measure the change in liquidity, we use the differences in the three liquidity measures between quarter t+1 and quarter t-1 and partition the sample based on whether it is an increase or a decrease.[33]

We report the results in columns (3) to (10) of Table 5. From columns (3) and (4), we find that, if there is an actual decrease in dollar trading volume, the coefficient for *TREAT×START* is -0.060 (t = -3.06); otherwise, the coefficient for *TREAT×START* is -0.023 (t = -1.55). The difference is significant at the 1% level. The results are consistent with our expectation that firms experiencing larger liquidity decreases tend to reduce management guidance to a larger extent. In columns (5) to (10), the inferences are consistent if we use the changes in stock

[33] We use one quarter before and after the start of the TSPP to calculate the changes in liquidity because a longer period may involve noises and confounding factors. As we show in the timeline of TSPP (Figure 1), the list of treatment and control firms of the pilot program is available to the public only one month before its start; therefore, using a short window is helpful to capture the immediate effects of the TSPP on liquidity without introducing too many confounding factors in our analyses. Nonetheless, we also use longer periods (the whole pre-TSPP period and the whole post-TSPP period) to define changes in untabulated analyses. Our inferences here and below based on Tables 5, 6, and 7 are not affected if we use the whole pre-TSPP and post-TSPP periods to define changes and levels.

turnover, quarterly mean of bid-ask spreads, and Amihud's (2002) illiquidity ratio to partition the sample.

4.4 The Effect of Pre-TSPP Marginal Benefits of Disclosure

Given we argue that the liquidity decrease amid the TSPP reduces the marginal benefits of disclosure and thus leads to less disclosure, we expect the effects to be more evident for firms already with lower marginal benefits of disclosure before the TSPP. H3 suggests that, after the exogenous liquidity decrease, firms may reduce their voluntary disclosure to a point such that their marginal costs are no larger than the reduced marginal benefits. As a result, relative to firms with high marginal benefits of disclosure, firms with low marginal benefits of disclosure may need to reduce voluntary disclosure to a larger extent so that the marginal benefits are no less than the marginal costs of disclosure. As marginal benefits of disclosure are diminishing with respect to transparency levels (Verrecchia 2001), we expect that firms that are more transparent before the TSPP have lower marginal benefits and are more susceptible to the potential impact of the TSPP.

To operationalize this idea, we use pre-TSPP levels of analyst coverage, institutional holdings, and analyst forecast dispersion measured at quarter *t*-1 relative to the start quarter *t* as the proxies for pre-TSPP transparency and marginal benefits of disclosure (Lang and Maffett 2011; Lang, Lins, Maffett 2012; Samuels, Taylor, and Verrecchia 2021). We find evidence that supports our arguments. In Panel A of Table 6, for those firms with pre-TSPP analyst coverage below the median, the coefficient for *TREAT×START* is negative and significant. However, we find a much stronger effect for firms with pre-TSPP analyst coverage above the median. The difference in the magnitude of coefficients between the two subsamples is statistically significant at the 5% level. In columns (3) and (4), we find similar results when partitioning the sample using pre-TSPP institutional holdings. We also use analyst forecast dispersion as a proxy for firm transparency. In columns (5) and (6), the results indicate that firms with smaller analyst forecast dispersion are more likely to reduce their management guidance after the tick size pilot program.[34]

In addition, we consider pre-TSPP levels of stock liquidity as alternative proxies for marginal benefits of disclosure. We assume that firms with a higher level of liquidity before the TSPP are more transparent and thus have a lower level of marginal benefits of disclosure. We report the results in Panel B of Table 6. We find consistent results that the treatment effects on voluntary disclosure are more pronounced among firms with higher liquidity before the TSPP.

In sum, these findings support our prediction from H3 that the liquidity effects on voluntary disclosure are associated with their impact on the marginal benefits of disclosure. In addition, these results indicate that the findings in Table 3 are mainly driven by firms that are more transparent (with lower marginal benefits of disclosure) before the TSPP.

4.5 Potential Alternative Explanations

Ahmed et al. (2020) find that the TSPP also influences treatment firms' discretionary accruals. It is possible that the TSPP affects voluntary disclosure through improved earnings quality instead of the liquidity shock. However, studies provide mixed evidence on the relation between mandatory disclosure quality and voluntary disclosure. In particular, Ball et al. (2012) suggest that mandatory disclosure quality and voluntary disclosure are complements, while Einhorn (2005) implies that mandatory disclosure quality may have a non-linear relation with voluntary disclosure. In addition, Lee and Watts (2021) show that the TSPP leads to a reduction

[34] The sample size for columns (5) and (6) is smaller compared to other columns. It is because we require firms to have at least five analyst forecasts to reliably calculate analyst forecast dispersion. As our sample consists of small-cap firms that do not have much analyst coverage, our sample size is reduced by roughly a half by doing so.

in algorithmic trading and an improvement in information acquisition for treatment firms. These findings also have the potential to drive the change in voluntary disclosure activities after the pilot program.

In this section, we evaluate these alternative explanations. Specifically, we perform cross-sectional analyses based on discretionary accruals, fundamental information acquisition, and algorithmic trading. We partition firms based on absolute-valued discretionary accruals (*ACCR*), EDGAR search volume (*ESV*), and a set of algorithmic trading proxies (*OLR*, *TOR*, *CTR*, and *ATS*). If any of these alternatives explains the change in voluntary disclosure, we expect that the treatment effect of the TSPP on voluntary disclosure varies with the corresponding variable. In other words, the differences in the estimated treatment effects between two subsamples partitioned by the corresponding variable would be significantly different from zero.

In Panel A of Table 7, we report the results for cross-sectional analyses based on the changes in algorithmic trading around the TSPP. We partition the sample based on whether there is an actual increase or decrease in algorithmic trading proxies around the TSPP. The change is defined as the difference in each variable between quarter t+1 and quarter t-1. In contrast to what we find in Tables 5 and 6, we do not find significant difference in the TSPP's effect on voluntary disclosure for all partitioning variables (*OLR*, *CTR*, *TOR*, and *ATS*). The results imply that it is unlikely that reduced management guidance activities are driven by the change in algorithmic trading.

Similarly, we consider the changes in unsigned discretionary accruals (*ACCR*) and EDGAR search volume (*ESV*) around the TSPP in columns (1) to (4) of Panel B of Table 7. We still find no evidence that the change in voluntary disclosure during the TSPP is associated with the changes in either discretionary accruals or information acquisition.

In addition, analyst coverage and institutional ownership are strongly associated with corporate information environment. Although the TSPP fails to improve analyst coverage and institutional ownership for pilot firms (Chen et al. 2020), we still test if the changes in analyst coverage and institutional ownership explain our findings. Columns (5) to (8) of Panel B of Table 7 tabulates the results for these cross-sectional analyses. We find no evidence that the changes in analyst coverage or institutional holdings explain the reduction in management guidance.

5. Additional Analyses

5.1 Alternative Voluntary Disclosure Proxies

To increase the generalizability of the results, we repeat the analyses with alternative proxies for voluntary disclosure – voluntary 8-Ks and conference calls (Frankel, Johnson, and Skinner 1999; Bowen, Davis, and Matsumoto 2002; Brown, Hillegeist, and Lo 2004; Balakrishnan et al. 2014; Guay et al. 2016; He and Plumlee 2020).

We extract corporate 8-K filings from the SEC EDGAR and follow He and Plumlee's (2020) approach to classify 8-K filings into mandatory and voluntary ones based on the item codes. We repeat our analyses by replacing *NUM MEF* with the natural logarithm of the number of voluntary 8-K filings (*NUM VOL 8K*) in columns (1) and (2) of Table 8. Column (1) shows the regression results using *NUM VOL 8K* as the dependent variable. We find a significant result that, relative to control firms, treatment firms issue fewer voluntary 8-K filings after the TSPP.

We next examine conference calls in column (2) of Table 8. In column (2), we use the natural logarithm of the number of conference calls (*NUM CALL*) as the dependent variable. The coefficient of *TREAT×START* is -0.033 and is significant at the 5% level (t=-2.54), indicating that the number of conference calls for treatment firms decreases after the TSP program compared to control firms.

In conclusion, we observe negative and significant effects of the TSPP when employing alternative voluntary disclosure proxies, which is consistent with our findings using management guidance as the disclosure proxy.

5.2 Robustness Checks

In the baseline regressions, we employ the control group stocks selected by the SEC and use eight-quarter data before and after the implementation quarter. In this section, we assess whether the findings are sensitive to different choices of sample period or driven by systematic differences between treatment and control stocks.

Panel A of Table 9 presents the results using samples with two, four, and six quarters before and after the starting quarter. Our inferences hold across all these specifications. Next, in columns (1) and (2) of Panel B of Table 9, we change the control sample to all the stocks in the Compustat universe that are not included in the treatment group but have prices no less than \$2 and market capitalization no more than \$3 billion, which is consistent with the selection criteria of the TSPP. The results are still consistent with our primary findings.

To further address the possibility of systematic differences in firm characteristics between treatment and control firms driving the findings, we re-estimate the baseline model for a sample constructed using propensity score matching (PSM). Accordingly, we do a one-to-one matching without replacement based on all the control variables using the data of the quarter before the starting quarter of the TSPP (i.e. quarter *t*-1) to match treatment firms with control firms.[35] Columns (3) and (4) in Panel B of Table 9 present results with this smaller sample.

[35] In untabulated results, we show that the treatment group and the PSM control group are not significantly different for most covariates.

The treatment effect obtained using the PSM sample is consistent with that in Table 3, implying that differences in firm characteristics are not likely to drive the findings.

Our empirical tests employ control variables that are motivated by prior research. Rather importantly, we also include firm fixed effects that account for time-invariant firm characteristics. Nevertheless, in untabulated tests, we incorporate additional control variables in our model, including multiple stock liquidity measures, an indicator for Big-4 audit firm, and total executive compensation. Despite the loss of observations, our inferences are not affected by the inclusion of these additional control variables.

6. Conclusion

Although the TSPP starts with the intention with boosting liquidity provision for small-capitalization firms, studies find that the TSPP leads to a drop in firm-level liquidity (Albuquerque et al. 2020; Li et al. 2020). Built upon this notion, our study examines whether and how an exogenous liquidity shock affects corporate voluntary disclosure decisions.

Using the SEC Tick Size Pilot Program and management guidance as the primary proxy for voluntary disclosure, we find robust results that treatment firms respond to an exogenous liquidity decrease by reducing their voluntary disclosure activities while control firms do not exhibit significant changes. To further support that the liquidity decrease rather than other changes during the pilot program explains the change in voluntary disclosure, we conduct a set of analyses to rule out possible explanations, such as the change in earnings quality, fundamental information acquisition, and algorithmic trading. To generalize our findings, we observe similar effects when using voluntary 8-K filings and conference calls as alternative voluntary disclosure proxies. Our conclusions are also robust to different choices of model specifications, sample periods, as well as various matching approaches. Overall, our findings suggest that an *exogenous* liquidity decrease leads to less voluntary disclosure.

Most existing studies suggest that voluntary disclosure is useful in improving stock liquidity. What makes this study different is that our findings allude to the opposite direction that stock liquidity also affect voluntary disclosure. To some degree, extant research views stock liquidity as an "output" *endogenously* affected by voluntary disclosure, implying that managers may increase "input" (i.e., voluntary disclosure) when "output" is insufficient. In contrast, our study employs an *exogenous* liquidity change that is not directly associated with disclosure. Our results imply that managers view stock liquidity as a part of their disclosure decision environment and that reduced liquidity may restrict managers' expected benefits from disclosure and disincentivize them from voluntarily disclosing information. As stock liquidity is not simply determined by the corporate information environment but also affected by other factors such as market microstructure characteristics, our findings add to our understanding of managers' voluntary disclosure decisions when they face variations in stock liquidity that are exogenous to the information environment.

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Appendix: Variable Definitions

Variable	Definition
ISSUE MEF	Indicator of the incidence of management earnings forecasts in a quarter, which equals to one if a firm has at least one management earnings forecast in a quarter and equals to zero otherwise.
NUM MEF	The natural logarithm of the number of management earnings forecasts in given quarter.
NUM LTF	The natural logarithm of the number of long-term management earnings forecasts in given quarter. Long-term forecasts are defined as the forecast with forecasted period at least six months after the announcement dates.
NUM STF	The natural logarithm of the number of short-term management earnings forecasts in given quarter. Short-term forecasts are defined as the forecast with forecasted period within six months after the announcement dates.
NUM BUNDLED	The natural logarithm of the number of management earnings forecasts bundled with earnings announcements in given quarter.
NUM ALONE	The natural logarithm of the number of stand-alone management earnings forecasts in given quarter.
NUM VOL 8K	The natural logarithm of the number of voluntary 8-K filings in given quarter. Voluntary 8-Ks are defined following the approach proposed by He and Plumlee (2020). 8-K filings with items of code 2.02, 7.01, and 8.01 are classified as voluntary 8-Ks.
NUM CALL	The natural logarithm of the number of conference calls in given quarter.
SIZE	Firm size, defined as the natural logarithm of market capitalization.
CASH	Cash holding, defined as cash and cash equivalents scaled by total assets.
ROA	Return-on-assets, defined as operating income scaled by total assets.
MB	Market-to-book ratio, defined as the market capitalization scaled by the book value of assets.
SALEGROWTH	Sales growth rate, defined as the percentage change in sales relative to that in the same quarter of the previous fiscal year.
TANGIBILITY	Tangibility, defined as net property, plant and equipment scaled by total assets.
LEV	Leverage ratio, defined as the book value of debt scaled by total assets.
ACCR	Absolute-valued discretionary accruals computed using the modified Jones (1991) model. Specifically, discretionary accruals are estimated within two-digit SIC industries and a minimum of 10 observations is required for each group.

RET	Quarterly stock return.
STDRET	Stock return volatility, measured by the standard deviation of daily stock return in a quarter.
AF	Analyst following, defined as the natural logarithm of the number of analysts who issue at least one analyst forecast for the firm in a quarter.
INST	Institutional holdings, defined as the number of shares held by institutional investors at the end of quarter scaled by total number of shares outstanding at the end of quarter.
DISP	Analyst forecast dispersion, defined as the standard deviation of analyst quarterly EPS forecasts for a quarter scaled by end-of-quarter stock prices. We require firms to have at least five analyst forecasts to calculate forecast dispersion.
DOLVOL	Dollar trading volume, defined as the natural logarithm of quarterly dollar trading volume scaled by market capitalization. Quarterly dollar trading volume is the sum of daily close prices times the daily trading volume.
TURNOVER	Stock turnover ratio, defined as total trading volume scaled by share outstanding.
SPREAD	Quarterly mean of daily bid-ask spreads. We use the closing bid and ask data (*bid* and *ask*) from CRSP and calculate daily bid-ask spreads as $100 \times (ask - bid)/[(ask + bid)/2]$.
ILLIQ	Amihud's (2002) illiquidity ratio, defined as the natural logarithm of one plus the quarterly mean of daily Amihud's (2002) ratio. We use *ret*, *prc*, and *vol* from CRSP to compute daily Amihud's (2002) ratio as $10000000 \times \lvert ret \rvert / (prc \times vol)$.
ESV	EDGAR search volume, defined as the frequency of non-robot access to SEC EDGAR. The data are obtained from James Ryans's website, http://www.jamesryans.com/
OLR	Odd lot ratio, defined as odd lot trade volume scaled by total trade volume.
TOR	Trade-to-order ratio, defined as total trade volume divided by total order volume.
CTR	Cancel-to-trade ratio, defined as the count of cancels scaled by the count of trades.
ATS	Average trade size, defined as total trade volume scaled by the count of trades.

Figure 1: Timeline of the SEC Tick Size Pilot Program

The figure shows the timeline of SEC tick size pilot program, which started on October 3, 2016 and ended on September 28, 2018.

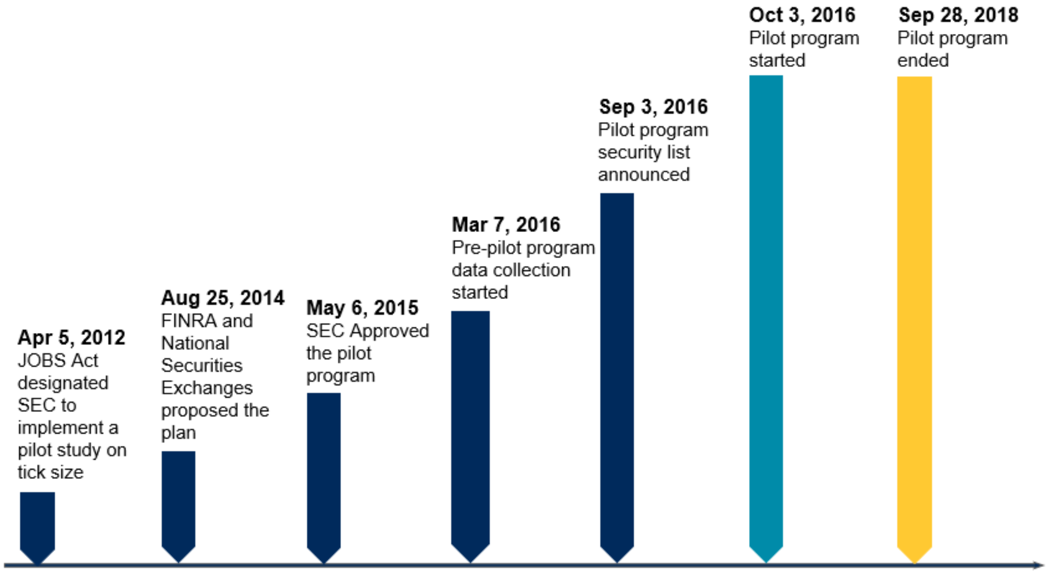
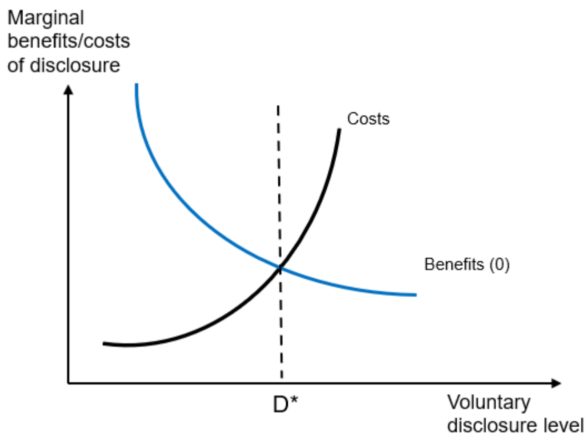


Figure 2: Illustration of H3

This set of the figures illustrate H3. Figure 2(a) plots the marginal benefits and costs of voluntary disclosure before an exogenous change in stock liquidity and marginal benefits of disclosure. Figure 2(b) plots the case after a negative exogenous change in stock liquidity and marginal benefits of disclosure.

(a) Before an exogenous change in stock liquidity



(b) After a negative exogenous change in stock liquidity

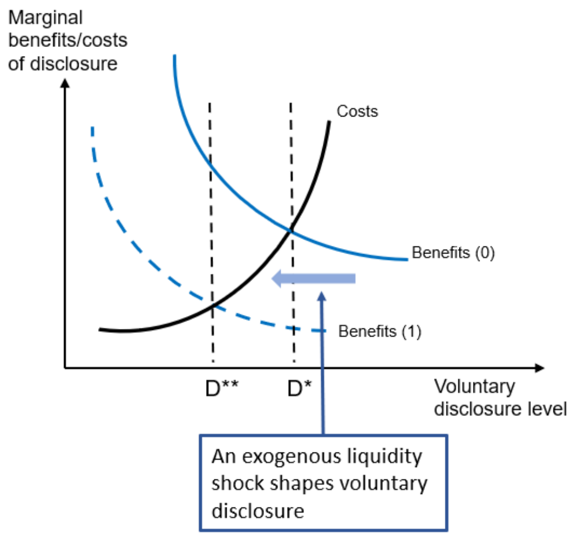
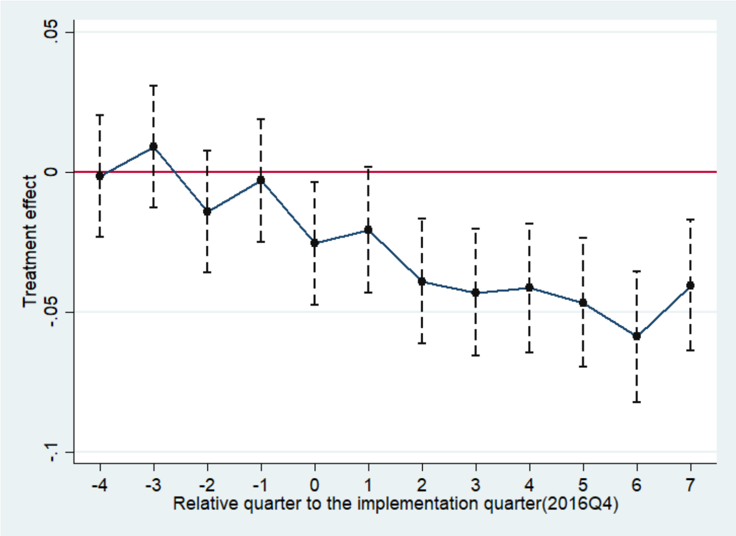


Figure 3: Management Earnings Forecasts around the Implementation of TSPP

The figures show the treatment effect of the implementation of the TSPP on management guidance activities. The benchmark period is quarter *t*-8 to *t*-5, relative to the starting quarter *t*. Panel A displays the treatment effect on the number of management earnings forecasts (*NUM MEF*). Panel B displays the treatment effect on the propensity of issuing management earnings forecasts (*ISSUE MEF*). The dashed line plots the 95% confidence intervals of the treatment effect. Variable definitions are available in Appendix.

Panel A: The number of management earnings forecasts.



Panel B: The propensity of issuing management earnings forecasts.

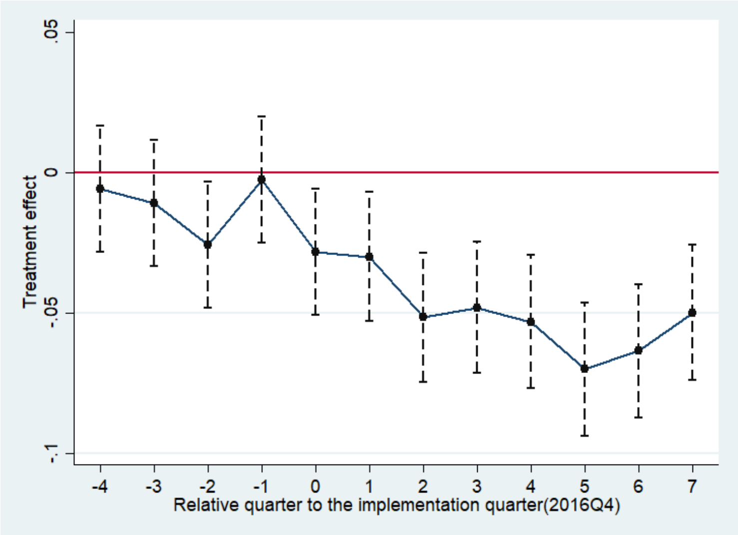


Table 1: Descriptive Statistics

The table displays the descriptive statistics for the sample from quarter *t*-8 to *t*+7, relative to the starting quarter *t* of the TSPP. The sample contains 15,465 firm-quarter observations for the TSPP treatment firms and control firms after excluding financial industry firms and firm-quarters with missing values in key variables. All the variables are winsorized at the 1st and 99th percentiles. Variable definitions are available in Appendix.

	N	Mean	Std	Min	25th	Median	75th	Max
A: Voluntary Disclosure Variables								
NUM MEF	15,465	0.221	0.395	0.000	0.000	0.000	0.693	1.609
ISSUE MEF	15,465	0.257	0.437	0.000	0.000	0.000	1.000	1.000
NUM LTF	15,465	0.003	0.053	0.000	0.000	0.000	0.000	1.609
NUM STF	15,465	0.219	0.393	0.000	0.000	0.000	0.693	1.609
NUM BUNDLED	15,465	0.197	0.359	0.000	0.000	0.000	0.000	1.609
NUM ALONE	15,465	0.035	0.174	0.000	0.000	0.000	0.000	1.609
NUM 8K	15,465	0.981	0.606	0.000	0.693	1.099	1.386	2.944
NUM VOL 8K	15,465	0.057	0.207	0.000	0.000	0.000	0.000	1.946
NUM CALL	15,465	0.640	0.379	0.000	0.693	0.693	0.693	2.197
B: Major Control Variables								
SIZE	15,465	19.847	1.355	15.790	18.942	20.020	20.925	23.571
CASH	15,465	0.231	0.248	0.000	0.040	0.136	0.338	0.950
ROA	15,465	-0.002	0.058	-0.341	-0.008	0.013	0.026	0.111
MB	15,465	1.666	1.684	0.062	0.642	1.100	2.007	10.352
SALEGROWTH	15,465	0.162	0.674	-0.976	-0.045	0.055	0.193	5.479
TANGIBILITY	15,465	0.228	0.235	0.001	0.057	0.136	0.317	0.928
ACCR	15,465	0.415	0.832	0.001	0.024	0.081	0.343	4.764
LEV	15,465	0.224	0.232	0.000	0.002	0.172	0.362	1.107
RET	15,465	0.032	0.223	-0.586	-0.100	0.018	0.147	0.888
STDRET	15,465	0.028	0.013	0.008	0.018	0.025	0.033	0.106
AF	15,465	1.449	0.808	0.000	1.099	1.609	2.079	3.219
INST	15,465	0.372	0.254	0.000	0.095	0.449	0.601	0.693
DISP	7,058	0.004	0.015	0.000	0.000	0.001	0.001	0.123
C: Liquidity Measures								
SPREAD	15,465	0.363	0.439	0.013	0.075	0.157	0.482	3.537
DOLVOL	15,465	2.971	0.104	2.415	2.910	2.996	3.050	3.205
TURNOVER	15,465	0.470	0.717	0.000	0.182	0.345	0.580	37.226
ILLIQ	15,465	1.344	1.785	0.009	0.181	0.563	1.801	11.359
D: Algorithmic Trading and Information Acquisition Measures from Lee and Watts (2021)								
ESV	15,465	5.145	3.378	0.000	0.000	7.047	7.535	9.178
OLR	13,661	1.345	0.727	0.658	1.106	1.190	1.347	5.262
TOR	13,661	0.031	0.016	0.006	0.019	0.029	0.042	0.087
CTR	13,661	46.044	55.417	8.502	17.848	26.913	47.465	343.393
ATS	13,661	0.119	0.071	0.045	0.079	0.099	0.133	0.800

Table 2: Descriptive Statistics for the Treatment and Control Groups before and after the TSPP Implementation

This table presents the descriptive statistics for the treatment and control group before and after the TSPP implementation. The implementation quarter *t* is 2016Q4, with pre-implementation period from *t*-8 to *t*-1 and post-implementation period from *t* to *t*+7. For each period, the table reports the mean for the control group, the mean for the treatment group, the difference and the significance level of *t*-statistics. The sample consists of the TSPP pilot firms and control firms after excluding financial industry firms and firm-quarters with missing values in key variables. All the variables are winsorized at the 1st and 99th percentiles. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

	Pre-implementation period [*t*-8, *t*-1]			Post-implementation period [*t*, *t*+7]		
Variables	Treatment	Control	Difference	Treatment	Control	Difference
NUM MEF	0.257	0.189	0.068***	0.235	0.202	0.034***
ISSUE MEF	0.292	0.220	0.072***	0.276	0.241	0.035***
NUM LTF	0.004	0.004	-0.001	0.003	0.003	-0.000
NUM STF	0.256	0.187	0.068***	0.234	0.200	0.034***
NUM BUNDLED	0.224	0.168	0.057***	0.213	0.184	0.029***
NUM ALONE	0.048	0.033	0.016***	0.033	0.025	0.008**
NUM 8K	1.116	1.112	0.004	0.839	0.842	-0.003
NUM VOL 8K	0.064	0.065	-0.001	0.042	0.055	-0.013***
NUM CALLS	0.618	0.588	0.030***	0.674	0.682	-0.009
SIZE	19.752	19.781	-0.029	19.895	19.947	-0.052*
CASH	0.250	0.221	0.030***	0.256	0.209	0.046***
ROA	-0.000	0.000	-0.000	-0.006	-0.003	-0.003***
MB	1.640	1.514	0.126***	1.767	1.657	0.110***
SALEGROWTH	0.175	0.124	0.051***	0.169	0.165	0.004
TANGIBILITY	0.224	0.231	-0.007	0.219	0.232	-0.013***
LEV	0.206	0.220	-0.014**	0.217	0.246	-0.030***
ACCR	0.330	0.303	0.027*	0.283	0.281	0.002
RET	0.020	0.006	0.014***	0.017	0.023	-0.006
STDRET	0.028	0.028	-0.000	0.027	0.028	-0.000
AF	1.477	1.470	0.007	1.434	1.419	0.015
INST	0.366	0.382	-0.016***	0.362	0.383	-0.021***

Table 3: Management Earnings Forecasts and the Impact of the TSPP

Panel A: Regression results using the whole sample.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				NUM MEF			*ISSUE MEF*
TREAT×START	-0.035**	-0.031**	-0.038***	-0.037***	-0.035***	-0.025***	-0.041***
	(-2.38)	(-2.20)	(-3.13)	(-3.10)	(-2.73)	(-2.92)	(-3.30)
TREAT	0.068***	0.063***					
	(2.97)	(3.06)					
START	0.013	0.014					
	(1.22)	(1.34)					
LAG(NUM MEF)						0.353***	
						(12.56)	
SIZE		0.029***		0.040***	0.044***	0.031***	0.043***
		(3.17)		(3.65)	(3.08)	(3.20)	(3.68)
CASH		-0.234***		-0.017	-0.004	0.011	-0.018
		(-5.53)		(-0.42)	(-0.08)	(0.35)	(-0.44)
ROA		0.345**		0.149**	0.167***	0.136***	0.149**
		(2.43)		(2.51)	(2.59)	(2.63)	(2.41)
MB		-0.005		-0.002	-0.002	-0.001	-0.004
		(-0.81)		(-0.36)	(-0.36)	(-0.26)	(-0.77)
SALEGROWTH		-0.017***		-0.001	-0.001	-0.001	-0.000
		(-4.11)		(-0.46)	(-0.58)	(-0.35)	(-0.09)
TANGIBILITY		-0.293***		-0.005	0.004	0.016	0.017
		(-7.25)		(-0.10)	(0.06)	(0.37)	(0.30)
ACCR		-0.006		0.001	0.001	0.001	0.000
		(-1.15)		(0.33)	(0.31)	(0.52)	(0.14)
LEV		-0.206***		0.019	0.035	0.018	0.003
		(-5.31)		(0.58)	(0.93)	(0.68)	(0.07)
RET		0.010		-0.017**	-0.021**	-0.012	-0.019**
		(0.86)		(-2.14)	(-2.24)	(-1.48)	(-2.28)
STDRET		-1.638***		-0.048	-0.111	-0.026	-0.220
		(-3.98)		(-0.28)	(-0.48)	(-0.12)	(-1.25)
AF		0.127***		0.039***	0.042***	0.034***	0.042***
		(9.56)		(3.77)	(3.77)	(4.21)	(3.57)
INST		0.055		-0.010	-0.023	-0.018	-0.024
		(1.31)		(-0.19)	(-0.33)	(-0.39)	(-0.44)
ESV					0.000	-0.000	
					(0.01)	(-0.02)	
OLR					0.001	0.000	
					(0.27)	(0.26)	
TOR					-0.396	-0.079	
					(-1.27)	(-0.34)	
CTR					-0.000	-0.000	
					(-1.11)	(-0.89)	
ATS					0.059	0.034	
					(1.27)	(0.93)	
Observations	15,465	15,465	15,465	15,465	13,660	13,660	15,465
Adjusted R^2	0.005	0.185	0.772	0.774	0.776	0.803	0.806
Firm FE	No	No	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	No	No	Yes	Yes	Yes	Yes	Yes
Cluster Level	Firm	Firm	Firm	Firm	Firm	Firm	Firm

Panel B: Regression results using only firms issue management guidance before the TSPP.

	(1)	(2)	(3)	(4)	(5)
	NUM MEF	*NUM STF*	*NUM LTF*	*NUM ALONE*	*NUM BUNDLED*
TREAT×START	-0.087***	-0.087***	0.003	-0.009	-0.074**
	(-2.76)	(-2.77)	(0.44)	(-0.60)	(-2.54)
SIZE	0.112***	0.114***	-0.007	0.014	0.099***
	(3.43)	(3.49)	(-1.02)	(0.94)	(3.37)
CASH	0.012	0.013	0.018	0.119**	-0.080
	(0.10)	(0.11)	(0.78)	(2.06)	(-0.72)
ROA	0.851***	0.839***	0.002	0.182	0.749***
	(2.85)	(2.79)	(0.04)	(0.87)	(2.87)
MB	-0.012	-0.013	0.005	-0.003	-0.009
	(-0.73)	(-0.80)	(1.47)	(-0.41)	(-0.63)
SALEGROWTH	-0.010	-0.012	0.006	0.028	-0.039*
	(-0.35)	(-0.42)	(0.80)	(1.40)	(-1.83)
TANGIBILITY	0.124	0.187	-0.074	-0.099	0.222
	(0.62)	(0.88)	(-1.31)	(-0.83)	(1.31)
ACCR	0.004	0.002	0.004	0.005	0.000
	(0.52)	(0.37)	(1.46)	(0.97)	(0.02)
LEV	0.021	0.019	0.035	-0.007	0.035
	(0.18)	(0.16)	(1.15)	(-0.11)	(0.34)
RET	-0.047*	-0.052**	0.005	-0.021	-0.029
	(-1.90)	(-2.14)	(1.10)	(-1.05)	(-1.38)
STDRET	-0.123	-0.215	-0.017	1.065**	-0.817
	(-0.18)	(-0.31)	(-0.07)	(2.25)	(-1.34)
AF	0.118***	0.121***	-0.004	0.013	0.112***
	(3.55)	(3.68)	(-0.59)	(0.75)	(3.95)
INST	0.052	0.059	-0.005	-0.070	0.124
	(0.40)	(0.46)	(-0.21)	(-0.92)	(1.22)
Observations	5,692	5,692	5,692	5,692	5,692
Adjusted R^2	0.524	0.525	0.144	0.160	0.579
Firm FE	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes
Cluster Level	Firm	Firm	Firm	Firm	Firm

The table presents the difference-in-differences regression results on the number of management earnings forecasts issued and the propensity to issue management earnings forecasts. Panel A tabulates the regression results using the whole sample. For columns (1) to (7), the dependent variable is the natural logarithm of the number of management earnings forecast (*NUM MEF*). For column (8), the dependent variable is the indicator of the incidence of management earnings forecasts (*ISSUE MEF*). Panel B tabulates the regression results using a sample of firms that issue at least one management earnings forecast in the period before the TSPP. The dependent variable for column (1) is the natural logarithm of the number of management earnings forecast (*NUM MEF*). For columns (2) and (3), the dependent variables are the natural logarithm of the number of short-term and long-term forecasts (*NUM STF* and *NUM LTF*), respectively. For columns (4) and (5), the dependent variables are the natural logarithm of the number of stand-alone forecasts and forecasts bundled with earnings announcements (*NUM ALONE* and *NUM BUNDLED*), respectively. The control variables are as defined in Appendix and winsorized at the 1st and 99th percentiles. *T*-statistics based on standard errors that are clustered at the firm level are reported in parentheses. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 4: Identifying Assumptions for Difference-in-Differences Analyses

The table presents the results for dynamics of the treatment effect of the TSPP. Panel A displays the results for parallel trend tests, with *NUM MEF* and *ISSUE MEF* as the dependent variable for columns (1) and (2), respectively. The independent variables include all the control variables as well as the interaction terms between treatment group indicator and quarter indicators for *t*-4 to *t*+7, with the quarter *t*-8 to *t*-5 as the benchmark period. Panel B shows the results for placebo tests using quarter *t*-4 and *t*+4 as pseudo implementation quarter, with *NUM MEF* and *ISSUE MEF* as the dependent variable for columns (1) and (2) and columns (3) and (4), respectively. Panel C employs the end of the TSPP as a treatment and shows the reversal of the effect. The tests use the sample ranging from quarter *t*-8 to *t*+12, relative to the starting quarter *t*. The dependent variable is *NUM MEF* and *ISSUE MEF* for columns (1) and (2), respectively. *START* equals to 1 for all quarters after 2016Q4 and *END* equals to 1 for all quarters after 2018Q3. The control variables are as defined in Appendix and winsorized at the 1st and 99th percentiles. *t*-statistics based on standard errors that are clustered at the firm level are reported in parentheses. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Parallel trend and the dynamics of treatment effects.

	(1) *NUM MEF*	(2) *ISSUE MEF*
TREAT×(t-4)	-0.001	-0.004
	(-0.06)	(-0.39)
TREAT×(t-3)	0.012	-0.007
	(0.95)	(-0.59)
TREAT×(t-2)	-0.011	-0.022
	(-0.80)	(-1.57)
TREAT×(t-1)	0.001	0.002
	(0.10)	(0.15)
TREAT×(t)	-0.022*	-0.024*
	(-1.71)	(-1.66)
TREAT×(t+1)	-0.018	-0.026*
	(-1.22)	(-1.71)
TREAT×(t+2)	-0.035**	-0.047***
	(-2.17)	(-2.77)
TREAT×(t+3)	-0.039**	-0.043**
	(-2.15)	(-2.25)
TREAT×(t+4)	-0.038**	-0.048**
	(-2.07)	(-2.54)
TREAT×(t+5)	-0.045**	-0.067***
	(-2.34)	(-3.38)
TREAT×(t+6)	-0.059***	-0.062***
	(-3.20)	(-3.09)
TREAT×(t+7)	-0.040**	-0.048**
	(-2.19)	(-2.51)
Control Variables	Yes	Yes
Observations	15,465	15,465
Adjusted R^2	0.774	0.806
Firm FE	Yes	Yes
Year-Quarter FE	Yes	Yes
Cluster level	Firm	Firm

Panel B: Placebo tests.

	(1)	(2)	(3)	(4)
	t-4 as pseudo implementation quarter		*t*+4 as pseudo implementation quarter	
	NUM MEF	*ISSUE MEF*	*NUM MEF*	*ISSUE MEF*
TREAT×START	-0.001	-0.009	-0.012	-0.014
	(-0.15)	(-0.91)	(-1.11)	(-1.18)
Observations	9,279	9,279	8,533	8,533
Adjusted R^2	0.837	0.859	0.819	0.844
Firm FE	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes
Cluster level	Firm	Firm	Firm	Firm

Panel C: Management Earnings Forecasts and the Termination of the TSPP.

	(1)	(2)
	NUM MEF	*ISSUE MEF*
TREAT×START	-0.038***	-0.041***
	(-3.24)	(-3.37)
TREAT×END	-0.012	-0.012
	(-1.27)	(-1.10)
TREAT×END – TREAT×START	-0.026*	-0.029**
F-statistics	[2.96]	[3.64]
Control Variables	Yes	Yes
Observations	19,993	19,993
Adjusted R^2	0.773	0.803
Firm FE	Yes	Yes
Year-Quarter FE	Yes	Yes
Cluster Level	Firm	Firm

Table 5: Liquidity Effect and the Impact of the TSPP

The table presents the difference-in-differences regression results for subsamples partitioned using the proxies for the extent of liquidity reduction. The dependent variable for all regressions is the natural logarithm of the number of management earnings forecasts (*NUM MEF*). Columns (1) and (2) are partitioned by the pre-TSPP bid-ask spreads (in cents) using $0.05 as the cutoff. Columns (3) and (4), columns (5) and (6), columns (7) and (8), and columns (9) and (10) are partitioned by the changes in dollar trading volume (*DOLVOL*), stock turnover ratio (*TURNOVER*), bid-ask spread (*SPREAD*), and Amihud's (2002) illiquidity measure (*ILLIQ*) around the TSPP, respectively. The changes are measured as the difference in values between quarter t+1 and quarter t-1. The control variables are included and as defined in Appendix and winsorized at the 1st and 99th percentiles. t-statistics based on standard errors that are clustered at the firm level are reported in parentheses. Chi-squared statistics that test whether the difference between coefficients of *TREAT×START* is different from zero are reported in brackets. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Pre-TSPP *Spreads (in cents)*		The change in *DOLVOL*		The change in *TURNOVER*		The change in *SPREAD*		The change in *ILLIQ*	
	<0.05	>0.05	< 0	> 0	< 0	> 0	< 0	> 0	< 0	> 0
TREAT×START	-0.038***	-0.023	-0.060***	-0.023	-0.057***	-0.024	-0.018	-0.044***	-0.021	-0.057***
	(-2.61)	(-1.41)	(-3.06)	(-1.55)	(-3.05)	(-1.53)	(-1.01)	(-2.94)	(-1.48)	(-2.79)
Difference	0.015*		0.037***		0.033***		0.026**		0.036***	
Chi^2 *statistics*	[2.77]		[9.27]		[7.58]		[3.93]		[8.68]	
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11,762	3,703	5,792	9,673	6,619	8,846	4,939	10,526	8,672	6,793
Adjusted R^2	0.775	0.709	0.750	0.783	0.761	0.783	0.766	0.777	0.784	0.762
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster Level	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm

Table 6: Pre-TSPP Transparency, Marginal Benefits of Disclosure, and the Impact of the TSPP

The table presents the difference-in-differences regression results for subsamples partitioned by the measure for the transparency of the information environment before the TSPP. The dependent variable for each column is the natural logarithm of the number of management earnings forecasts (*NUM MEF*). In Panel A, Columns (1) and (2) are partitioned by the median of the pre-TSPP analyst coverage, which is defined as analyst following (*AF*) in quarter *t*-1. Columns (3) and (4) are partitioned by the median of pre-TSPP institutional ownership, which is defined as the percentage owned by institutional investors (*INST*) in quarter *t*-1. Columns (5) and (6) are partitioned by analyst forecast dispersion in quarter *t*-1, and the analyses are restricted to firms with at least five analyst forecasts so that the measurement of dispersion is reliable. In Panel B, we partition firms based on pre-TSPP liquidity levels in quarter *t*-1 measured using dollar trading volume (*DOLVOL*), turnover ratios (*TURNOVER*), bid-ask spreads (*SPREAD*), and Amihud's (2002) illiquidity ratios (*ILLIQ*). The control variables are as defined in Appendix and winsorized at the 1st and 99th percentiles. *t*-statistics based on standard errors that are clustered at the firm level are reported in parentheses. Chi-squared statistics that test whether the difference between coefficients of *TREAT×START* is different from zero are reported in brackets. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: The Effects of Pre-TSPP Firm Transparency

	(1)	(2)	(3)	(4)	(5)	(6)
	Pre-TSPP *AF*		Pre-TSPP *INST*		Pre-TSPP *DISP*	
	<Median	>Median	<Median	>Median	<Median	>Median
TREAT×START	-0.021**	-0.050**	-0.019	-0.054***	-0.069**	-0.032
	(-2.14)	(-2.32)	(-1.48)	(-2.72)	(-2.12)	(-1.03)
Difference	0.029***		0.035***		0.037*	
Chi² statistics	[6.01]		[8.75]		[3.08]	
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,512	7,513	7,510	7,518	3,484	3,493
Adjusted R^2	0.784	0.748	0.776	0.753	0.745	0.721
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster Level	Firm	Firm	Firm	Firm	Firm	Firm

Panel B: The Effects of Pre-TSPP Firm Liquidity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Pre-TSPP *DOLVOL*		Pre-TSPP *TURNOVER*		Pre-TSPP *SPREAD*		Pre-TSPP *ILLIQ*	
	< Median	> Median	< Median	> Median	< Median	> Median	< Median	> Median
TREAT×START	-0.023*	-0.049**	0.000	-0.079***	-0.042**	-0.030**	-0.051**	-0.022*
	(-1.87)	(-2.41)	(0.04)	(-3.84)	(-2.06)	(-2.54)	(-2.54)	(-1.73)
Difference	0.026**		0.079***		0.012		0.028**	
Chi^2 statistics	[4.79]		[43.68]		[1.04]		[5.97]	
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,463	7,469	7,460	7,470	7,460	7,471	7,461	7,471
Adjusted R^2	0.753	0.730	0.769	0.753	0.808	0.752	0.754	0.746
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster Level	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm

Table 7: Tests for Alternative Explanations

The table presents the difference-in-differences regression results for subsamples partitioned by proxies implied by alternative explanations. The dependent variable for each column is the natural logarithm of the number of management earnings forecasts (*NUM MEF*). In Panel A, we partition the sample based on four algorithmic trading proxies (*OLR*, *TOR*, *CTR*, and *ATS*). In Panel B, we use the change in absolute-valued discretionary accruals (*ACCR*), EDGAR search volume (*ESV*), analyst coverage (*AF*) and institutional holdings (*INST*) to partition the sample. The changes are measured as the difference in values between quarter *t*+1 and quarter *t*-1. *t*-statistics based on standard errors that are clustered at the firm level are reported in parentheses. Chi-squared statistics that test whether the difference between coefficients of *TREAT×START* is different from zero are reported in brackets. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: The effects of the changes in algorithmic trading.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	The change in *OLR*		The change in *TOR*		The change in *CTR*		The change in *ATS*	
	<0	>0	<0	>0	<0	>0	<0	>0
TREAT×START	-0.024	-0.046**	-0.036	-0.035***	-0.033**	-0.025	-0.041**	-0.036**
	(-1.57)	(-2.57)	(-1.50)	(-2.63)	(-2.19)	(-1.24)	(-2.46)	(-2.27)
Difference	0.022		0.001		0.008		0.005	
Chi² statistics	[2.63]		[0.00]		[0.51]		[0.67]	
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,886	7,579	2,831	12,634	10,013	5,452	8,024	7,441
Adjusted R^2	0.803	0.739	0.770	0.775	0.781	0.753	0.771	0.775
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster Level	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm

Panel B: The effects of the changes in discretionary accruals, EDGAR search, and other aspects of the information environment.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	The change in *ACCR*		The change in *ESV*		The change in *AF*		The change in *INST*	
	<0	>0	<0	>0	<0	>0	<0	>0
TREAT×START	-0.037**	-0.035**	-0.045*	-0.034**	-0.044*	-0.034**	-0.041*	-0.035**
	(-2.10)	(-2.23)	(-1.93)	(-2.46)	(-1.71)	(-2.53)	(-1.91)	(-2.50)
Difference	0.002		0.013		0.010		0.006	
Chi² statistics	[0.03]		[0.73]		[0.53]		[0.65]	
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,180	7,184	3,499	11,966	3,726	11,739	4,569	10,896
Adjusted R^2	0.779	0.770	0.714	0.784	0.792	0.767	0.775	0.774
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster Level	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm

Table 8: Alternative Voluntary Disclosure Proxies

The table presents the difference-in-differences regression results using alternative voluntary disclosure proxies. For column (1), the dependent variable is the natural logarithm of the number of voluntary 8-K filings defined based on He and Plumlee (2020) (*NUM VOL 8K*). Column (2) uses the natural logarithm of the number of conference calls (*NUM CALL*). The control variables for both panels are as defined in Appendix and winsorized at the 1st and 99th percentiles. *t*-statistics based on standard errors that are clustered at the firm level are reported in parentheses. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)
	NUM VOL 8K	*NUM CALL*
TREAT×START	-0.012*	-0.033**
	(-1.66)	(-2.54)
Control Variables	Yes	Yes
Observations	15,465	15,465
Adjusted R^2	0.185	0.661
Firm FE	Yes	Yes
Year-Quarter FE	Yes	Yes
Cluster Level	Firm	Firm

Table 9: Alternative Sample Periods and Control Groups

The table presents the robustness check results using alternative sample periods and control groups. Panel A reports the results for robustness checks using different sample periods. Columns (1) and (2) use two quarters before and after the implementation and include the implementation quarter. Columns (3) and (4) use four quarters before and after the implementation. Columns (5) and (6) use six quarters before and after the implementation. Panel B reports the results for robustness checks using different control groups. Columns (1) and (2) use a one-to-one propensity-score-matched sample by matching control firms to treatment firms using all the control variables four quarters before the implementation. Columns (3) and (4) use the Compustat as the universe but restrict to firms with pre-event stock prices no less than \$2 and market capitalization no more than \$3 billion. The dependent variables are *NUM MEF* and *ISSUE MEF*. The control variables are as defined in Appendix and winsorized at the 1st and 99th percentiles. *t*-statistics based on standard errors that are clustered at the firm level are reported in parentheses. *, **, and *** represent statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Different sample periods.

	(1)	(2)	(3)	(4)	(5)	(6)
	[*t*-2, *t*+2]		[*t*-4, *t*+4]		[*t*-6, *t*+6]	
	NUM MEF	*ISSUE MEF*	*NUM MEF*	*ISSUE MEF*	*NUM MEF*	*ISSUE MEF*
TREAT×START	-0.017*	-0.018*	-0.027***	-0.026**	-0.032***	-0.036***
	(-1.80)	(-1.88)	(-2.61)	(-2.33)	(-2.80)	(-2.95)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,096	5,096	9,118	9,118	12,972	12,972
Adjusted R^2	0.848	0.875	0.795	0.831	0.782	0.812
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster Level	Firm	Firm	Firm	Firm	Firm	Firm

Panel B: Different control groups.

	(1)	(2)	(3)	(4)
	PSM sample		Compustat Universe Sample	
	NUM MEF	*ISSUE MEF*	*NUM MEF*	*ISSUE MEF*
TREAT×START	-0.027**	-0.028**	-0.028**	-0.030**
	(-2.55)	(-2.48)	(-2.29)	(-2.34)
Control variables	Yes	Yes	Yes	Yes
Observations	14,596	14,596	18,986	18,986
Adjusted R^2	0.776	0.804	0.776	0.800
Firm FE	Yes	Yes	Yes	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes
Cluster Level	Firm	Firm	Firm	Firm