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Keywords: Market feedback; Directors' learning from stock prices; Algorithmic trading; High-frequency trading; CEO turnover

JEL classifications: G30, J63, L20, E44, G19

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1. Introduction

In this paper, we explore how algorithmic trading (AT), a method of executing orders using automated pre-programmed instructions, affects the extent to which directors on corporate boards learn information from stock prices when making CEO turnover decisions—directors' learning from stock prices. Our research question is motivated by two growing strands in the literature. First, it is argued that stock prices aggregate decision-relevant information that is otherwise dispersed among market participants, and that decision-makers learn such investor information contained in stock prices to guide their decisions (see Bond, Edmans, and Goldstein [2012] and Goldstein [2022] for a review). Existing evidence supporting this proposition is mainly based on *managers* learning investor information from prices in making investment and earnings forecasting decisions.[1] However, decision-makers other than managers also wish to glean information from stock prices in making economic decisions (e.g., Bond et al. 2012; Goldstein 2022). Second, AT is one of the most notable financial innovations in several decades (e.g., Stiglitz 2014; Menkveld 2016), and it constitutes a substantial portion of recent trading in stock markets (Kaya 2016). There is growing evidence of the financial market consequences of AT, and we build on this literature by studying how AT affects corporate directors, an important group of real decision-makers for the firm.[2]

Directors, particularly outside directors, require high-quality information in conducting a monitoring role, including CEO turnover decisions that significantly affect shareholder value (Jensen and Ruback 1983; Holmström 2004; Armstrong, Guay, and Weber 2010). However, directors are often informationally disadvantaged because managers do not always transfer

[1] See Luo (2005); Chen, Goldstein, and Jiang (2007); Zuo (2016); Edmans, Jayaraman, and Schneemeier (2017); Jayaraman and Wu (2020); Ye, Zheng, and Zhu (2022); and Pinto (2022).

[2] See Hendershot, Jones, and Menkveld (2011); Brogaard, Hendershott, and Riordan (2014); Chakrabarty, Moulton, and Wang (2017); Weller (2018); Chordia and Miao (2020); and Lee and Watts (2021) for evidence on the effects of AT on capital market consequences. Ye et al. (2022) is an exception in that they examine the effect of AT on managerial learning from stock prices in making capital expenditure decisions. In our paper, we focus on directors' learning from stock prices.

information that may adversely affect their employment (Jensen 1993; Verrecchia 2001; Chen, Guay, and Lambert 2020). At the same time, outside directors are unlikely to become as informed as the managers that directors are monitoring even after expending much time and effort (Armstrong et al. 2010). On this point, stock prices provide a useful source of information to directors in making CEO turnover decisions. Investors exert costly efforts to acquire non-public information that affects firm value and trade on it for profit motives (Grossman and Stiglitz 1980), and CEO performance and the quality of CEO-firm match should be part of this information (Pan, Wang, and Weisbach 2015). If such information was unknown to directors but is impounded into prices by informed traders, directors would have incentives to use such information to decide whether to retain or replace poorly performing CEOs.

To assess the effect of AT on directors' learning from stock prices, we rely on the sensitivity of CEO turnovers to stock returns (i.e., turnover-return sensitivity) in a similar spirit to prior studies using investment-price sensitivity to test for managerial learning from stock prices.[3] The intuition is that CEO turnover will be more negatively associated with stock returns when movements in prices are more likely to originate from information that directors do not know (and thus have not yet used it in their decisions) than from information that they already know (and thus have already used it in their decisions).

Under the director learning channel, the sensitivity of CEO turnover to stock returns (i.e., the negative relation) is expected to decrease with AT. AT interferes with the production of information from investors by free-riding on their order flows or front-running their informed trades (Weller 2018; Lee and Watts 2021). As a result, stock prices are less likely to incorporate information from investors, some of which may concern CEO performance and

[3] Prior studies document a robust negative relation between the likelihood of CEO turnover and stock returns, suggesting that poorly performing CEOs are more likely to be fired (e.g., Weisbach 1988; Huson, Malatesta, and Parrino 2004). Throughout the paper, we use the term *turnover-return sensitivity* to refer to the magnitude of the negative relation between the likelihood of CEO turnover and stock returns; thus, a decreased turnover-return sensitivity means a reduction in the magnitude of the negative relation.

CEO-firm match and be unknown to directors. As AT increases, therefore, directors are expected to rely less on price signals in making CEO turnover decisions, leading to a decrease in turnover-return sensitivity.

Absent the director learning channel, however, AT can also affect turnover-return sensitivity. On the one hand, AT discourages investors' fundamental information acquisition activities (Weller 2018; Lee and Watts 2021) and also lowers investors' incentives to conduct fundamental analysis, resulting in poor financial reporting quality (Ahmed, Li, and Xu 2020). This leads to a decrease in turnover-return sensitivity because poor performance that may result in CEO turnover will not be reflected in stock prices. On the other hand, AT may increase the extent to which stock prices reflect existing information, such as firm disclosures, by improving liquidity (Chakrabarty et al. 2017; Chordia and Miao 2020). This argument leads to an increase in turnover-return sensitivity.

Using 11,857 firm-year observations between 2012 and 2019, we examine how AT affects the sensitivity of forced CEO turnover (hereafter simply CEO turnover) to stock returns. We find that CEO turnover is negatively associated with stock returns, confirming the findings of prior studies (e.g., Guo and Masulis 2015; Guay, Taylor, and Xiao 2015; Jenter and Lewellen 2021). The coefficient estimate of CEO turnover-return sensitivity suggests that, on average, a one standard deviation decrease in stock returns is associated with a 1.91% increase in the likelihood of CEO turnover, which represents approximately 53.17% relative to its unconditional mean. Importantly, we find that this adverse effect of stock returns on CEO turnover decreases with AT. When AT moves from the bottom to the top decile, the inverse relationship between CEO turnover and stock returns is significantly reduced by approximately 79.17%. The reduction is robust to the inclusion of a set of control variables including stock liquidity and firm-year fixed effects. These results are consistent with AT impeding directors' ability to learn investor information from stock prices in CEO turnover decisions.

To address endogeneity concerns, we exploit a randomized controlled experiment conducted by the SEC's Tick Size Pilot (TSP) program, which widened tick size for a subset of firms (i.e., treatment firms). Lee and Watts (2021) show that treatment firms experienced a significant decrease in AT during the TSP period, compared to control firms. We build on their findings and use the TSP experiment as an exogenous shock to AT and conduct a generalized difference-in-differences estimation. We find that the sensitivity of CEO turnover to stock returns becomes more negative for treatment firms during the TSP period compared to control firms, mitigating endogeneity concerns associated with the effects of AT on turnover-return sensitivity.

Although the negative effect of AT on turnover-return sensitivity is consistent with directors' learning from stock prices, it is also consistent with AT hindering stock prices' ability to reflect directors' own information about the implications of CEOs' actions for future cash flows, as AT discourages investors' fundamental information acquisition and fundamental analysis (Weller 2018; Lee and Watts 2021). Given that a direct proxy for a source of information that directors learn from stock prices is absent, we conduct two sets of cross-sectional tests and investigate whether the effect of AT on the turnover-return sensitivity is greater in firms where the price-based director learning is predicted to be stronger. The first set of tests exploits firm characteristics, and the second set of tests exploits director characteristics.

First, we associate the effect of AT on turnover-return sensitivity with firm characteristics that are correlated with the types of information that directors likely wish to glean from prices. Learning models commonly assume that investors collectively have information advantages in assessing growth opportunities vis-à-vis assets-in-place and macroeconomic factors vis-à-vis firm-specific factors (e.g., Gao and Liang 2013; Goldstein and Yang 2019; Jayaraman and Wu 2019; Goldstein, Yang, and Zuo 2022). Thus, the effect of AT on turnover-return sensitivity is expected to be more pronounced in growth firms and firms with greater exposure to macroeconomic factors. Next, research suggests that investors' information advantage also stems from their geographic presence (Gao and Xiao 2022). As opposed to corporate insiders such as directors, investors are geographically dispersed and thus can impound information into prices that is otherwise unavailable to corporate insiders. Thus, the effect of AT on turnover-return sensitivity is expected to be more marked for firms with a geographically dispersed investor base, compared to firms with a geographically concentrated investor base. Consistent with these cross-sectional predictions, we find that AT moderates turnover-return sensitivity to a greater extent for growth firms, firms with greater exposure to macroeconomic factors, and firms with a geographically dispersed investor base.

Second, we investigate whether the effect of AT on turnover-return sensitivity varies with directors' expertise and directors' information set. Gleaning decision-relevant information from stock prices can be a challenging task because while directors need to distinguish between price movements due to decision-relevant information and those due to noise trading, they lack expertise or are often time-constrained. This implies that directors with greater expertise are more likely to learn investor information from stock prices in CEO turnover decisions. Accordingly, the effect of AT on turnover-return sensitivity is expected to be more marked when firms' boards consist of directors with greater expertise.

Further, learning models posit that managers rely more on investor information in prices when their own information set is poor (e.g., Bai, Philippon, and Savoy 2016), and empirical evidence is consistent with this prediction (e.g., Chen et al. 2007). A similar intuition can apply to directors. When making CEO turnover decisions, directors are more likely to rely on information from stock prices when their own information set is poor. Accordingly, the effect of AT on the turnover-return sensitivity is expected to be more marked when firms' boards consist of less informed directors. We find results consistent with these cross-sectional predictions—AT moderates the sensitivity of CEO turnover to stock returns to a greater extent when directors have industry expertise as CEOs and when directors are less privately informed (measured by directors' insider trading activities and trading profitability). Taken together, our cross-sectional results substantiate our inference and indicate that the effect of AT on turnover-return sensitivity is due to a decrease in directors' learning from stock prices.

We conduct a battery of sensitivity and robustness analyses. While our main analysis uses a composite AT measure based on four proxies (Odd Lot Ratio, Cancel-to-Trade Ratio, Trade-to-Order Ratio, and Average Trade Size), we find that our results also hold when we consider each proxy separately. Next, we assess whether our results are robust to alternative definitions of CEO turnover. We repeat our analysis using all CEO turnovers (both forced and voluntary) and find that our results generally hold. Further, we follow Jenter and Lewellen (2021) and identify performance-induced CEO turnovers, i.e., CEO turnover that would not have occurred had performance been good. Our findings are robust to this measure. Finally, prior research suggests that accounting earnings and stock returns could be substitutive performance measures in CEO turnover decisions (e.g., Engel, Hayes, and Wang 2003).[4] This implies that directors are more likely to rely on price signals when the firm's earnings are noisy signals of CEO performance. Accordingly, we expect the relation between AT and turnover-return sensitivity to be more marked among firms with low earnings quality. Using earnings timeliness and persistence as proxies for earnings quality (Engel et al. 2003; Suk, Lee, and Kross 2021), we find results consistent with these predictions.

This study makes contributions to three strands of literature. First, it contributes to an emerging literature on how decision-makers learn investor information from stock prices to guide their real decisions (see Bond et al. [2012] and Goldstein [2022] for a review). Studies provide growing evidence in support of this informational feedback from the market, yet they

[4] An extensive body of literature also examines the relative importance of stock prices and earnings as performance measures for CEO compensation (Holmström 1979; Lambert and Larcker 1987; Baber, Kang, and Kumar 1998; Core, Guay, and Verrecchia 2003; Jayaraman and Milbourn 2012; Li and Wang 2016; Bettis, Bizjak, Coles, and Kalpathy 2018; Jayaraman, Ling, Wu, and Zhang 2021).

mostly focus on corporate *managers* making corporate investment decisions (e.g., Luo 2005; Chen et al. 2007; Edmans et al. 2017; Jayaraman and Wu 2020; Pinto 2022; Ye et al. 2022) and revising earnings forecasts (Zuo 2016). We extend this strand of research by exploring corporate *directors* as decision-makers who glean investor information from stock prices when making CEO turnover decisions. We present evidence consistent with directors gleaning information from stock prices in deciding whether to replace poorly performing CEOs. Future research may explore price-based learning by other decision-makers (e.g., creditors, customers, auditors, employees, etc.).

Second, we contribute to the literature on algorithmic trading. Prior research primarily focuses on AT's influence in capital market settings, such as liquidity and price discovery (e.g., Brogaard et al. 2014; Weller 2018; Chordia and Miao 2020; Lee and Watts 2021). We extend this line of research by exploring the effect of AT on real decision-makers. Our evidence suggests that the effect of AT goes beyond financial market participants and extends to directors' decisions on whether to replace poorly performing CEOs. On this point, our study is related to Ye et al. (2022), who document the adverse effect of AT on managerial learning from stock prices in making investment decisions. The two studies are of relevance to policymakers because any conclusions solely based on financial market consequences may be incomplete in assessing the overall effect of algorithmic trading on the economy.[5]

Finally, we contribute to the CEO turnover literature. Prior research extensively examines the role of corporate governance and agency problems in explaining CEO turnovers (e.g., Weisbach 1988; Denis, Denis, and Sarin 1997; Mikkelson and Partch 1997; Huson, Parrino, and Starks 2001; Kaplan and Minton 2012; Peters and Wagner 2014; Jenter and Kanaan 2015). Several recent studies find that corporate boards incorporate the competitive

[5] Congress directed the SEC to assess the benefits and risks of algorithmic trading as part of the Economic Growth, Regulatory Relief, and Consumer Protection Act of 2018. The report by SEC staff primarily analyzes the benefits and risks associated with financial markets (https://www.sec.gov/tm/reports-and-publications/special-studies/algo_trading_report_2020).

costs that decrease firm value upon the CEO's departure into turnover decisions (Kini, Williams, and Yin 2021; Lin, Peters, and Seo 2022). We document a new determinant of CEO turnover by showing that a trading innovation in financial markets, algorithmic trading, affects CEO turnover.[6]

2. Related Literature and Hypothesis Development

2.1. Related literature

Our study is related to three strands of the literature: (1) managerial learning from stock prices; (2) algorithmic trading; and (3) CEO turnover. First, research presents evidence that managers learn information from stock prices in decisions of M&A, capital expenditure, and management earnings forecasts. Luo (2005) examines M&A and finds that the likelihood of withdrawing the announced M&A is negatively associated with market reactions to the M&A announcement, and the effect of market reactions is stronger when managers are expected to benefit from learning from prices (i.e., when uncertainty lies in technology). Jayaraman and Wu (2020) provide consistent results using management capital expenditure forecasts. They find that managers tend to increase (decrease) capital expenditures relative to their forecasts when the market reaction to forecast announcements is positive (negative). They also find that the effect of market reactions is greater when more informed trading arises during the announcement window. Pinto (2022) exploits the JOBS Act, which reduced mandatory disclosure requirements for emerging growth companies (EGCs) to facilitate access to equity markets. He finds that the investment-to-price sensitivity for EGCs are higher than non-EGCs, suggesting that reduced disclosure requirements lead to increased managerial learning from stock prices. Chen et al. (2007) analyze the sensitivity of corporate investment to stock prices

[6] We discuss several related studies in the CEO turnover literature in Section 2.1. (e.g., Bushman, Dai, and Wang 2010; Ferreira, Ferreira, and Raposo 2011; Gorton, Huang, and Kang 2017; Bennett, Stulz, and Wang 2020; Hayes, Tian, and Wang 2022).

and provide evidence that this sensitivity increases with the extent to which prices are expected to contain more information that is new to managers.[7] Outside of investment decisions, Zuo (2016) examines whether managers look to price signals to glean information in revising their own earnings forecasts. He finds a positive association between forecast revisions and stock returns that have accrued since the original forecasts, and the association is stronger when prices are expected to contain information that is new to managers. A common feature of these studies is that they study managers as decision-makers who learn from stock prices. We extend this line of research by studying whether directors on corporate boards rely on stock prices to make CEO turnover decisions.

Second, our study is also related to the literature on algorithmic trading. Several studies show that AT expedites the incorporation of public information into stock prices by reducing information processing costs (Brogaard et al. 2014; Blankespoor, deHaan, and Marinovic 2020). AT around earnings announcements is associated with faster incorporation of earnings surprises (i.e., higher earnings response coefficients) and a smaller magnitude of post-earnings announcements drift (Bhattacharya, Chakrabarty, and Wang 2020; Chordia and Miao 2020). Also, AT is associated with faster analyst forecast revisions and lower forecast dispersions, suggesting that analysts benefit from the speedy assimilation of public information into stock prices (Bhattacharya et al. 2020). Another strand of research examines the effect of AT on private information acquisition. For example, Weller (2018) documents reduced price informativeness when AT increases, and Lee and Watts (2021) find elevated informed trading prior to earnings announcements when AT decreases during the TSP program. More recent research studies the real effect of AT. Ye et al. (2022) use the TSP program as a negative shock to AT and show that investment-price sensitivity increases during the program. These findings suggest that decreased AT during the TSP program improves the extent to which informed

[7] See Goldstein (2022) for a detailed review on more studies.

traders acquire and trade on private information, which managers glean from prices to make better investment decisions. Our study extends the findings of Ye et al. (2022) by exploring whether AT affects important decisions made by boards of directors, i.e., CEO turnover decisions.

Third, our study is related to the literature on CEO turnover, specifically the line of research that examines the link between the informativeness of performance signals, particularly stock prices, and CEO turnover. Bushman et al. (2010) find that both the likelihood of CEO turnover and its sensitivity to stock return performance are increasing in the idiosyncratic component of stock returns, suggesting that directors' ability to learn about CEO ability or CEO-firm match depends on the composition of stock returns (i.e., idiosyncratic versus systematic). Hayes et al. (2022) use deregulation in the banking industry as a setting and show that CEO turnovers in banks become more sensitive to stock prices when more growth opportunities arise in the deregulated environment. Ferreira et al. (2011) theoretically model the relation between price informativeness and board structure and argue that informative prices can either *complement* board monitoring (by allowing boards to use stock prices as an input to their monitoring tasks) or *substitute* board monitoring (by making external monitoring mechanisms more effective). They find that stock price informativeness is negatively associated with board independence, suggesting a substitutive relation. However, research also documents evidence suggesting a complementary relation. For instance, Gorton et al. (2017) find a positive relation between forced CEO turnover and the level of private information acquisition, as measured by the probability of informed trading (PIN), implying that boards may use stock prices as an input in their monitoring decisions when they contain more private information. More recently, Bennett et al. (2020) find that CEO turnover is more sensitive to Tobin's q when prices are more informative.[8]

Overall, these studies underscore the role of stock prices as an important performance signal in evaluating CEO performance and the quality of CEO-firm match.[9] Unlike these studies, which examine how stock prices reflect *total* information about CEO performance regardless of whether it is known to directors, our emphasis is on the information in stock prices that is *unknown* to directors, which may facilitate directors' learning from prices.

2.2. Hypothesis Development

The role of information in stock prices in informing directors' decisions to replace poorly performing CEOs arises because there is information asymmetry between directors and managers (Armstrong et al. 2010). This asymmetry exists because, with regard to information hierarchy, managers are viewed as more informed than outside directors. What's more, opportunistic managers are likely to withhold information (e.g., annual budgets and a wide set of other internal reports) that may be detrimental to their own private benefits (Jensen 1993; Verrecchia 2001; Chen et al. 2020).

To overcome this information asymmetry, directors have incentives to pay attention to stock prices to glean investor information that could guide their CEO turnover decisions, just as managers do in making investment decisions as reviewed in Section 2.1. Investors exert costly efforts to acquire non-public information that affects firm value for profit motives and incorporate it into stock prices via the trading process (Grossman and Stiglitz 1980). Investors actively acquire and trade on information about CEO performance and the quality of CEO-firm

[8] As discussed in Goldstein (2022), one challenge in this line of research is that it is hard to construct empirical measures that truly capture price informativeness. To address this issue, several prior studies rely on market characteristics and exogenous shocks that likely alter price informativeness and examine managerial learning from prices (e.g., Foucault and Fresard 2012; Edmans et al. 2017; Ye et al. 2022; Pinto 2022). We build on these studies by also identifying a setting (the TSP Program) that reduced the amount of acquirable investor information in prices.

[9] Another strand of research links the informativeness of earnings to CEO turnover. Studies find that the sensitivity of forced CEO turnover to earnings performance increases with earnings timeliness (Engel et al. 2003) or earnings persistence (Suk et al. 2021).

match because CEOs have far-reaching implications for various corporate decisions (Pan et al. 2015). Such investor information regarding CEO performance and the CEO-firm match in prices, which might otherwise be unknown to directors, can guide their CEO turnover decisions. For directors' learning from prices to arise, we note that one need not assume that investors are more informed about CEO performance and the CEO-firm match than directors. Rather, we argue that directors can learn from price signals as long as they are not fully aware of all information relevant to assessing CEO performance and the CEO-firm match and that investors collectively provide some information that may be unknown to directors absent price signals (Bond et al. 2012; Goldstein 2022).

AT can impair the role of stock prices in guiding directors' CEO turnover decisions because AT discourages investors' costly information acquisition activities by free-riding on order flows or front-running informed trades (Weller 2018; Lee and Watts 2021). Facing diminished expected returns on information acquisition efforts, investors reduce the production of information, and as a result, stock prices incorporate less information that could otherwise inform directors about CEO performance and CEO-firm match. Accordingly, as AT increases, directors are less likely to learn investor information from prices when making CEO turnover decisions, and thus the turnover-return sensitivity is expected to decrease with AT.

However, we also note that even absent the director learning channel, the turnover-return sensitivity could be associated with AT. On the one hand, research documents that AT lowers investors' incentives to conduct fundamental analysis, which in turn adversely influences investors' efforts to search for fundamental information (Lee and Watts 2021). Lack of fundamental monitoring by investors via fundamental analysis results in poor financial reporting quality (Ahmed et al. 2020). Therefore, to the extent that directors rely on public information in monitoring CEOs as suggested by Armstrong et al. (2010), turnover-return sensitivity is expected to decrease with AT. On the other hand, AT provides liquidity to stock markets, which in turn may improve the extent to which stock prices reflect existing information such as firm disclosures and analyst reports (Chakrabarty et al. 2017; Chordia and Miao 2020). Under this scenario, turnover-return sensitivity is expected to increase with AT. Given these conflicting predictions, we present our hypothesis in the null form.

***H1**: Algorithmic trading is unrelated to the sensitivity of CEO forced turnover to stock returns.*

We argue that the relation between AT and turnover-return sensitivity is more marked when price-based director learning is most likely (Bond et al. 2012). First, we hypothesize that directors will most likely learn from prices for firms whose investors are most likely to possess informational advantages relative to directors. We argue that investors are more likely to possess informational advantages relative to directors regarding three aspects of the firm: 1) growth opportunities, 2) macroeconomic exposures, and 3) investors' geographic dispersion.

Learning models commonly assume that investors' informational advantage lies in assessing growth opportunities, which requires aggregating various sources of information regarding market trends, consumer demand, and industry competition, rather than information on the firm's assets-in-place (Gao and Liang 2013; Goldstein and Yang 2019; Jayaraman and Wu 2019; Goldstein et al. 2022). Moreover, investors are more likely to possess informational advantages regarding macroeconomic factors compared to firm-specific factors, which directors may have better access to (through their regular meetings with management). Lastly, investors are more likely to possess informational advantages when they have a diverse geographic presence since they can impound a variety of local information relevant to the firm, which directors do not have easy access to, into prices.

***H2a**: The relation between algorithmic trading and the sensitivity of CEO forced turnover to stock returns will be more pronounced for firms with more growth opportunities, more macroeconomic exposures, and a more geographically dispersed investor base.*

Second, we hypothesize that directors' price-based learning is most likely to occur 1) when they possess the expertise to do so and 2) when they have more incentives to do so. Gleaning decision-relevant information from stock prices is challenging as it requires directors to distinguish between price movements due to decision-relevant information and those due to noise trading. Thus, directors with expertise are more likely to engage in price-based learning. Also, learning models posit that managers will rely more on investor information when their own information set is poor (Bai et al. 2016). We similarly hypothesize that directors will also turn to investor information via prices when their own information set is poor. Put simply, these directors with poor information sets will have the most incentives to engage in price-based learning.

H2b: *The relation between algorithmic trading and the sensitivity of CEO forced turnover to stock returns will be more pronounced when corporate boards consist of directors with greater expertise and poor information sets.*

3. Research Design

3.1. Empirical Specification

To examine the effects of AT on CEO turnover, we estimate the following OLS regression model with firm and year fixed effects.

$$
\begin{aligned}
Forced \quad = \quad & \beta_1\, RET + \beta_2\, AT + \boldsymbol{\beta_3\, RET \times AT} + \beta_4\, Size + \beta_5\, BTM + \beta_6\, RETVOL \\
& + \beta_7\, EARNVOL + \beta_8\, AIM + \beta_9\, ROA + \beta_{10}\, \#\, of\, Analysts + \beta_{11}\, IOR \\
& + \beta_{12}\, DIV + \beta_{13}\, Duality + \beta_{14}\, Ownership + \beta_{15}\, Age + \beta_{16}\, Tenure + \phi_i \\
& + \eta_t + \varepsilon,
\end{aligned}
\tag{1}
$$

where *Forced* is an indicator variable that equals one if forced CEO turnover occurs in period *t*, else zero.[10] *RET* is based on the *Return* variable, which is industry-adjusted stock returns measured over the periods *t* and *t-1*. Jenter and Lewellen (2021) find that corporate boards put

[10] Forced turnover data is from https://www.florianpeters.org/ and https://doi.org/10.5281/zenodo.4543893.

a greater weight on stock price performance in tenure years 0 and -1 than in prior years. The industry adjustments are based on the equal-weighted Fama-French 48 industry returns. We rank the *Return* variable into deciles ranging between 1 and 10 and divide it by 10 to create *RET*. We use a decile-ranked measure of stock returns since stock returns are skewed.[11]

The primary variable of interest is *AT*, which is the decile-ranked variable using a composite measure for algorithmic trading (*Algorithmic Trading*) based on four AT proxies: Odd Lot Ratio, Cancel-to-Trade Ratio, Trade-to-Order Ratio, and Average Trade Size (Weller 2018; Lee and Watts 2021).[12] *Odd Lot Ratio* is the natural logarithm of the equal-weighted average of the daily odd lot ratio, and *Cancel-to-Trade Ratio* is the natural logarithm of the equal-weighted average of the daily cancel-to-trade ratio. Higher values of *Odd Lot Ratio* and *Cancel-to-Trade Ratio* are associated with higher levels of algorithmic trading. *Trade-to-Order Ratio* is the natural logarithm of the equal-weighted average of the daily trade-to-order ratio, and *Average Trade Size* is the equal-weighted average of the daily average trade size. Lower values of *Trade-to-Order Ratio* and *Average Trade Size* are associated with higher levels of algorithmic trading. All four proxies are averaged over the same measurement period of the industry-adjusted stock returns (i.e., periods *t* and *t-1*). *Algorithmic Trading* is the first principal component of the four algorithmic trading proxies.[13] We then rank the *Algorithmic Trading* variable into deciles ranging between 1 and 10 and divide it by 10 to create *AT*. Therefore, in equation (1), the coefficient on *RET* reflects the turnover-return sensitivity when the firm's level of algorithmic trading is in the bottom decile. The coefficient on the interaction between *RET* and the *AT* represents the differential turnover-return sensitivity when the firm's algorithmic trading activities move from the bottom to the top decile.

[11] Our inferences are similar when we use raw returns instead of the decile-ranked returns.
[12] Refer to Lee and Watts (2021) for a brief motivation behind each proxy and the details of the calculation.
[13] Principal component analysis explains the variance structure of data by linear combinations of variables and thus reduces the data to a few principal components but retains a maximum of information contained in the original variables with less noise. We find that the first principal component of the four AT proxies explains about 51.1% of their common variation (untabulated).

We follow prior studies on CEO turnover and AT and include the following set of control variables in the regression model (e.g., Guo and Masulis 2015; Weller 2018): Firm size (*Size*), book-to-market ratio (*BTM*), stock return volatility (*RETVOL*), earnings volatility (*EARNVOL*), Amihud's (2002) stock illiquidity measure (*AIM*), return on assets (*ROA*), number of financial analysts following the firm (*# of Analysts*), institutional ownership (*IOR*), and an indicator variable equal to one if the firm is a dividend-payer, else zero (*DIV*), an indicator variable equal to one if the CEO is the chairman of the board, else zero (*Duality*), the percentage of shares owned by the CEO (*Ownership*), the natural logarithm of CEO Age (*Age*), and the natural logarithm of CEO tenure (*Tenure*). ϕ_i and η_t represent firm fixed effects and year fixed effects, respectively. We cluster standard errors by firm. Appendix A provides more details on variable construction.

Following prior studies (e.g., Cornelli, Kominek, and Ljungqvist 2013; Guo and Masulis 2015; Dasgupta, Li, and Wang 2018), we employ a linear probability model for two primary reasons. First, a linear probability model allows us to include firm and year fixed effects to control for unobservable firm-specific characteristics that are endogenously determined with CEO turnover decisions (e.g., Hermalin and Weisbach 1998).[14] Second, the marginal effects of two interacted variables associated with our cross-sectional tests and the TSP experiment, which we will describe later, differ from the marginal effects of changing just the interaction terms when the model is nonlinear (Ai and Norton 2003).

3.2. Data and Descriptive Statistics

We combine several data sources. We use CRSP to calculate stock returns, stock return volatility, and stock liquidity measures. We retrieve firms' financial statement data from Compustat. We obtain data on proxies for algorithmic trading from the Market Information

[14] Including high-dimensional fixed effects in the nonlinear specification is inappropriate since it makes it difficult to conduct local maximum likelihood estimation due to the incidental parameter problem (Neyman and Scott 1948).

Data Analytics System (MIDAS) database. MIDAS significantly improves the identification of AT and has collected order data across all major U.S. stock exchanges since 2012. We construct the CEO turnover sample with all firms in ExecuComp, which also provides data on CEO titles (i.e., whether the CEO is the chairman), tenure, age, and stock ownership. We obtain the number of analysts following the firm from the Thomson Reuters I/B/E/S database and institutional holdings information from the 13F database. We also collect outside directors' identities and employment histories from the BoardEx employment file. We require non-missing values for AT proxies, stock returns, and other variables used in our regressions. The above data requirements yield a sample of 11,857 firm-year observations that correspond to 1,755 unique firms over the sample period between 2012 and 2019. We winsorize all continuous variables at the 1st and 99th percentiles to reduce the influence of outliers.

Panel A of Table 1 reports the descriptive statistics of the CEO turnover variables, AT proxies, and other control variables. The mean value of the likelihood of all CEO turnover (*Turnover*) and forced CEO turnover (*Forced*) is approximately 10.1% and 3.6%, respectively, which is similar to that in prior studies (e.g., Guo and Masulis 2015; Jenter and Lewellen 2021).

Panel B of Table 1 provides unconditional Pearson correlations among AT proxies. We find that all proxies are correlated with each other in the expected direction with some individual variation (Weller 2018). As expected, *Algorithmic Trading*, the composite measure, is highly correlated with the four input variables: it is significantly positively correlated with *Odd Lot Ratio* (0.852) and *Cancel-to-Trade Ratio* (0.698) and significantly negatively correlated with *Trade-to-Order Ratio* (-0.685) and *Average Trade Size* (-0.738). These high correlations suggest that the extent to which the composite measure, *Algorithmic Trading*, captures the common variation driven by AT appears to be balanced.

4. Empirical Results

4.1. Main Results

Table 2 provides estimation results using equation (1). In column 1, we use the raw industry-adjusted stock return measured over the periods *t* and *t-1* (*Return*) without interaction with *AT*. Consistent with prior research, we find a significant negative coefficient on *Return* at the 1% level, indicating that forced CEO turnover is more likely when firm performance decreases. The coefficient estimate suggests that a one standard deviation decrease in *Return* is associated with a 1.91% increase in the likelihood of forced CEO turnover, which represents approximately 53.17% relative to its unconditional mean (-0.5317 = -0.044×0.435/0.036).

In column 2, we interact *Return* with *AT*. We find a negative coefficient on *Return* (Coeff.=-0.072) and a positive coefficient on the interaction term *Return* × *AT* (Coeff.=0.057). These coefficients are both statistically significant at the 1% level. This implies that, when algorithmic trading is in its bottom decile, a one standard deviation decrease in *Return* is associated with a 3.13% increase in the likelihood of forced CEO turnover, but this inverse relationship between forced CEO turnover and stock returns significantly decreases by approximately 79.17% when AT moves from the bottom to the top decile. In column 3, we replace *Return* with the decile-ranked return variable, *RET*, and find similar results. We find a significantly negative coefficient on *RET* at the 1% level (Coeff.=-0.122) while the coefficient on *RET* × *AT* is significantly positive at the 1% level (Coeff.=0.111). The result indicates that when algorithmic trading is in its bottom decile, a one standard deviation decrease in *RET* is associated with a 3.5% increase in the likelihood of forced CEO turnover (-0.035 = -0.122 × 0.287). However, this inverse relationship between forced CEO turnover and stock price performance is significantly reduced by approximately 90.98% when AT moves from the bottom to the top decile, consistent with the result in column 2. Overall, these findings are consistent with AT impeding directors' ability to learn investor information from stock prices when making CEO turnover decisions.

4.2. Tick Size Pilot Program

A critical limitation of the findings based on OLS estimation lies in the endogenous nature of CEO turnover decisions: AT and CEO turnovers can be jointly determined by unobservable and unmodelled firm-specific characteristics, hindering causal inferences due to correlated omitted variable biases. To address this issue, we exploit a randomized field experiment conducted by the SEC, the Tick Size Pilot (TSP) program, which widened the tick size from one cent to five cents for a set of randomly selected stocks. Research finds that treatment firms experience a significant decrease in AT and a significant increase in informed trading during the TSP period compared to control firms (Lee and Watts 2021; Ye et al. 2022). Using this experiment as an exogenous source of variation in AT, we explore whether decreased AT causes treatment firms to exhibit an increase in CEO turnover-return sensitivity during the TSP period.

We first perform a validity check by exploring whether treatment firms experience increased AT in our sample. Consistent with findings in Lee and Watts (2021), we document a significant decrease in AT across the four individual AT proxies. We report the validation results in Appendix B.

Next, we turn to the effect of the TSP experiment on forced CEO turnover-return sensitivity. We use the following generalized difference-in-differences estimation model with firm and year fixed effects.

$$\begin{aligned} Forced \;=\; & \beta_1\, Treat \times Post + \beta_2\, RET + \beta_3\, Treat \times RET + \beta_4\, Post \times RET \\ & + \boldsymbol{\beta_5\, Treat \times Post \times RET} + \beta_6\, Size + \beta_7\, BTM + \beta_8\, RETVOL \\ & + \beta_9\, EARNVOL + \beta_{10}\, AIM + \beta_{11}\, ROA + \beta_{12}\, \#\, of\, Analysts + \beta_{13}\, IOR \\ & + \beta_{14}\, DIV + \beta_{15}\, Duality + \beta_{16}\, Ownership + \beta_{17}\, Age + \beta_{18}\, Tenure + \phi_i \\ & + \eta_t + \varepsilon, \end{aligned} \tag{2}$$

where *Forced* is an indicator variable equal to 1 if the forced CEO turnover occurs in period *t*, else zero. *Treat* is an indicator variable equal to 1 for the treatment firms, and 0 otherwise. The TSP experiment officially began on October 3, 2016 and was gradually phased in over the month of October. All treatment firms were under the program by the end of October 2016, and the program ended in October 2018. Hence, *Post* is an indicator variable that equals 1 for fiscal years in the Pilot period (i.e., 2017 and 2018) and 0 for fiscal years in the pre-Pilot period (i.e., 2015 and 2016).[15] *Treat* and *Post* are subsumed by firm and year fixed effects, respectively. All other variables are previously defined in equation (1).

As noted by Lee and Watts (2021), a key advantage of the TSP setting is that it is a randomized experiment, which enables researchers to estimate causal treatment effects with less concern about endogeneity. In a randomized experiment, including control variables is redundant and may even cause a "bad control" problem and less efficient estimators (Angrist and Pischke 2009; Lee and Watts 2021). However, to ensure that our estimates are robust, we estimate equation (2) both with and without control variables used in equation (1) and fixed effects.

Panel A of Table 3 provides the estimation results. In column 1, we do not include control variables and fixed effects and find a significantly negative coefficient on *RET* × *Treat* × *Post* at the 1% level (Coeff. = -0.177), indicating that treatment firms experience a significantly increased turnover-return sensitivity in the TSP period. In column 2, we additionally include control variables and find a similar result significant at the 1% level (Coeff. = -0.160). In column 3, we further include firm and year fixed effects and find that the coefficient estimate is slightly increased relative to that of column 2 and statistically significant

[15] We measure stock price performance over the periods t and t-1 (i.e., a 2-year window). Thus, stock price performance in 2017 is measured under the two tick-size regimes and thus includes the phase-in period. Note that this measurement choice and the inclusion of 2017 in our analysis work against finding significant results. In untabulated tests, we check the sensitivity of our results by either dropping 2017 (1-year post period vs 2-year pre-period) or dropping 2017 and 2015 (1-year post-period vs 1-year pre-period) and find similar inferences.

at the 1% level (Coeff. = -0.171). Overall, these findings provide causal evidence of the effects of AT on turnover-return sensitivity.

To further substantiate our inference, we conduct a falsification test by creating a variable that captures fiscal years 2013 and 2014 as the pseudo-TSP period. *Pseudo Post* is an indicator variable that equals one (zero) for fiscal years 2013 and 2014 (2011 and 2012). We re-estimate equation (2) using this variable and report the results in Panel B. We find that the coefficient estimates in columns 1-3 are close to zero and statistically insignificant, indicating that our findings are not driven by a differential time trend between the treatment and control firms during the pre-TSP program period.

4.3. Cross-Sectional Tests

4.3.1 Firm Characteristics

In this section, we perform cross-sectional tests to corroborate our argument underlying boards' learning from prices when making CEO turnover decisions. Learning models in prior research posit that investors' information advantage over insiders (e.g., managers and directors) lies in assessing certain types of uncertainties, such as growth opportunities (Gao and Liang 2013; Bai et al. 2016; Goldstein and Yang 2015, 2019) and firms' exposure to macroeconomic factors (Ye et al. 2022). Empirical evidence suggests that managers rely more on stock prices in making investment decisions under these circumstances (Kim, Park, and Wilson 2021; Goldstein et al. 2022; Ye et al. 2022). Moreover, investors' information advantage over insiders may also stem from their geographic diversity (Gao and Xiao 2022). Thus, we expect the negative effect of algorithmic trading on turnover-return sensitivity to be more pronounced when firms have greater growth opportunities, when firm performance is more exposed to macroeconomic factors, and when firms have a geographically dispersed investor base. We argue that boards are more likely to learn about CEO performance or CEO-firm match from prices under these circumstances.

First, we investigate the cross-sectional variation of the effect of algorithmic trading with respect to the firm's growth option. To test this prediction, we follow Peters and Taylor (2017) and employ a measure of the firm's intangible capital.[16] This measure is based on the replacement cost of intangible capital and estimated to be the sum of the firm's externally purchased intangible capital (i.e., goodwill) and internally created intangible capital. The replacement cost of internally created intangible capital is computed as the sum of knowledge capital (based on R&D spending) and organizational capital (based on SG&A expenses). We create an indicator variable (*High INTCAP*) that equals one if a firm's intangible capital as of the beginning of period *t* is above the sample median, else zero.

We present the estimation results in column 1 of Table 4. Consistent with our expectation, we find a statistically significant and positive coefficient on the interaction term, *High INTCAP* × *AT* × *RET* at the 5% level. This finding supports our inference of boards' price-based learning as informed traders' information advantages lie in assessing growth opportunities rather than assets-in-place (Goldstein et al. 2022).

Second, we explore whether our results are more pronounced when firm performance is more exposed to macroeconomic factors. We proxy for each firm's macroeconomic exposure using the R-squared from a regression of the firm's quarterly earnings on the quarterly Gross Domestic Product (GDP) and energy price index over the past four years (Hutton, Lee, and Shu 2012). A higher R-squared value would indicate that the firm's earnings are more sensitive to macroeconomic factors, and thus stock prices are more likely to impound such macroeconomic information. We create an indicator variable (*High MACRO*) that equals one if a firm's macroeconomic exposure is above the sample median, else zero.

We present the estimation results in column 2 of Table 4. Consistent with our

[16] We do not use a market-based measure of growth options, such as Tobin's q or market-to-book ratio, in this analysis because algorithmic trading can affect the stock price informativeness and thus the informativeness of the market-based measures of growth options.

expectation, we find a statistically significant and positive coefficient on the interaction term, *High MACRO* × *AT* × *RET* at the 10% level. This finding implies that the negative relation between algorithmic trading and the turnover-return sensitivity is more pronounced among firms with more macroeconomic exposure, consistent with directors' price-based learning being impeded for firms whose prices most likely contain information unknown to directors.

Third, we examine whether our results are more pronounced for firms with a more geographically dispersed investor base. To proxy for the firm's investor base, we use the variation in the locations of requests for firms' filings on the SEC EDGAR system. Prior studies find that such requests largely correspond to the firm's investor base given their significant explanatory power in explaining the firm's market reaction to earnings news (Drake, Roulstone, and Thornock 2015; Drake, Johnson, Roulstone, and Thornock 2020; Chen 2021). Specifically, we estimate a search-weighted Herfindahl-Hirschman Index (HHI) as the sum of the squares of each firm's number of non-robotic EDGAR searches by IP addresses from each state during the year, scaled by the number of all non-robotic EDGAR searches during the year. We then create *High Investor Diversity*, an indicator variable that equals one if the search-weighted HHI of the firms' geographic investor base is below the median during the year, else zero. We then interact this variable with *AT*.

Column 3 of Table 4 presents the estimation results. Consistent with our expectation, we find a statistically significant and positive coefficient on the interaction term *High Investor Diversity* × *RET* × *AT* at the 5% level. This finding is consistent with our expectation and indicates that the negative relation between algorithmic trading and turnover-return sensitivity is more pronounced among firms with a more geographically dispersed investor base.[17]

[17] In untabulated analysis, we also find that the negative relation between AT and turnover-return sensitivity is more pronounced for firms with internationally dispersed operations, consistent with investors having an informational advantage over directors when firms' operations are subject to heterogeneous factors, such as regulatory, economic, and geopolitical uncertainties and different cultural norms (Bushman, Chen, Engel, and Smith 2004; Jennings, Seo, and Tanlu 2013; Goldstein and Yang 2015).

Overall, the cross-sectional results documented in Table 4 are consistent with our hypothesis H2a and directors' learning from stock prices: the negative effect of algorithmic trading on turnover-return sensitivity is concentrated in circumstances where firms' prices are most likely to contain investor information unknown to directors.

4.3.2 Director Characteristics

We turn to directors' characteristics and investigate whether the effect of AT on the turnover-return sensitivity varies by directors' expertise and directors' information set. An important assumption underlying our hypothesis is that directors can glean decision-relevant information from stock prices when making CEO turnover decisions. Directors' ability and expertise to learn from prices, however, may exhibit considerable heterogeneity. Moreover, directors may also exhibit heterogeneity in their incentives to rely on investor information from stock prices when engaging in their monitoring efforts.

First, we examine directors' expertise. We argue that directors with industry experience as a CEO are more likely to possess the expertise to glean decision-relevant information from stock prices. Consistent with heterogeneous director expertise, prior studies suggest that outside directors' industry experience facilitates better information flow to the firm and board monitoring. Dass, Kini, Nanda, Onal, and Wang (2014) find that directors from related industries bring valuable knowledge and help firms overcome information challenges, such as demand and supply shocks. In addition, extant research suggests that managers (i.e., CEOs) are able to learn from stock prices. Since decision-makers should remove the effect of noise trading on stock prices to extract useful information for their decisions (Jayaraman and Wu 2020), directors with prior industry experience as a CEO are better able to glean information from stock prices and hence rely more on stock prices to assess CEO performance and CEO-firm match. Thus, these directors are more severely affected by AT, leading to a more pronounced negative relation between algorithmic trading and turnover-return sensitivity.[18]

To test this prediction, we proxy for directors' prior industry experience as a CEO with *High INDEXP,* an indicator variable that equals one if the number of outside directors who have worked in the same industry as a CEO prior to joining the current firm divided by the total number of directors in period *t* is greater than the sample median, else zero. We then interact this indicator variable with *AT* and *RET*. The results are reported in column 1 of Table 5. We find a statistically significant and positive coefficient on the interaction term (*High INDEXP* × *RET* × *AT*) at the 5% level. This finding is consistent with a learning channel, where boards are more likely to glean information from stock prices when they consist of more outside directors with industry experience as a CEO, and AT deters such learning by reducing the informativeness of stock prices.

Next, we examine directors' incentives to learn from stock prices. Specifically, we expect that directors look more to stock prices to glean information to make CEO turnover decisions when their own information set is poor. For directors to effectively fulfill their monitoring duties, they rely on inside information that outsiders likely do not have access to (Bebchuk and Weisbach 2010). A large body of literature examines the open market stock trading by outside directors to estimate the amount of inside information these directors possess. For instance, Ravina and Sapeinza (2010) find that outside directors earn substantial abnormal returns from trading the firm's stock. Cao, Dhaliwal, Li, and Yang (2015) find that outside directors with social ties to the CEO earn higher insider profits. Masulis and Zhang (2019) further find that directors are less likely to trade on the firm's stock when they are distracted, suggesting lower monitoring intensity during periods of distraction. Motivated by this line of research, we argue

[18] Outside directors' industry experience can substitute for less informative stock prices due to algorithmic trading. In this case, the adverse effect of algorithmic trading on turnover-return sensitivity is moderated by the presence of directors with industry experience.

that when boards consist of more directors that i) are less likely to trade the stock of the firm and ii) have lower insider trading profits, they are less likely to possess inside information and thus will have more incentives to glean important information from stock prices in making CEO turnover decisions.

We test this prediction by directly linking directors' names under BoardEx to the Thomson Reuters' Insider Filing Data Feed, which records all trading activities of all corporate insiders as reported on SEC Forms 3, 4, and 5 during our sample period. We restrict our analyses to open market sales and purchases since these trades are most likely to be information-driven, unlike option grants (Ravina and Sapienza 2010). Following our argument above, we create two variables that capture the level of boards' information disadvantage that increases the likelihood of relying on stock prices.

First, we estimate the percentage of the corporate board that has traded the firm's stock at least once during the year. We create a board-level variable, *Low Insider Trading*, an indicator variable that takes a value of one for boards with a below-median percentage of outside directors trading in the firm's stock during the year, else zero. Second, we estimate the average insider trading profits of the outside directors during the year by following the method of Jagolinzer, Larcker, and Taylor (2011). Specifically, for every trade, we measure the trade profitability as the intercept (i.e., alpha) from the four-factor Fama and French (1993) and Carhart (1997) models estimated over 180 days following the trade. For sales transactions, we multiply by -1 to the alpha value. We take the mean alpha value if the director has multiple trades during the period. We then create a board-level variable *Low Insider Profit*, an indicator variable that takes a value of one for boards with a below-median average trading profit during the year, else zero. Our main variable of interest for these tests is the triple interaction term that interacts with the board-level indicator variable (*Low Insider Trading* or *Low Insider Profit*) with *AT* and *RET*.

The results for these cross-sectional tests are reported in columns 2 and 3 of Table 5. We find a statistically significant and positive coefficient on the interaction term at the 1% and 10% levels for columns 2 and 3, respectively. These findings further corroborate the learning channel, in which boards are more likely to glean information from stock prices when they consist of more outside directors with informational disadvantages, and AT deters such learning by reducing the informativeness of stock prices.

5. Additional Tests

5.1. Individual AT Proxy

In our baseline estimation, we compute a composite measure of algorithmic trading based on principal component analysis to isolate the underlying construct common to all algorithmic trading proxies. In this section, we employ the individual proxy for algorithmic trading and examine whether the results are robust. Similar to *AT*, we rank each individual algorithmic trading proxy into deciles ranging from 1 to 10 and divide it by 10. *OLR, CTR, TOR,* and *ATS* are decile-ranked variables of Odd Lot Ratio, Cancel-to-Trade Ratio, Trade-to-Order Ratio, and Average Trade Size, respectively. We interact each individual algorithmic trading proxy with *RET* and re-estimate equation (1).

Table 6 reports these results. Note that *OLR* and *CTR* increase with algorithmic trading, while *TOR* and *ATS* decrease with algorithmic trading. Consistent with the results in Table 2, we find that the interaction terms in columns 1 and 2 are statistically significant and positive at the 1% level while the interaction terms in columns 3 and 4 are statistically significant and negative at the 1% level. These findings indicate that our primary finding is robust to individual algorithmic trading proxies.

5.2. CEO Turnover Classification

We use forced CEO turnover in our baseline specification since such turnover decisions clearly reflect corporate boards' firing decisions. Most studies follow the classification algorithm developed by Parrino (1997) to classify CEO turnovers as forced vs. voluntary (Huson et al. 2004; Hazarika, Karpoff, and Nahata 2012; Peters and Wagner 2014; Guay et al. 2015; Guo and Masulis 2015; Jenter and Kanaan 2015; Lin et al. 2022). This algorithm is based on the interpretation of press reports and departure announcements, which may lead to measurement errors in the classification. For example, it is possible that the media covers certain types of firms more frequently than others, and the CEO turnover incidences identified by the interpretation of press releases could be driven by an increase in media coverage. As such, the classification is inevitably somewhat subjective and might suffer from reporting biases (Parrino 1997; Peters and Wagner 2014; Lin et al. 2022). In addition, even though prior research finds that forced CEO turnover is significantly associated with poor firm performance, the press-based classification algorithm does not specifically factor firm performance into account, understating the extent of true forced CEO turnover that is driven by poor firm performance. Indeed, Jenter and Lewellen (2021) argue that the literature significantly underestimates the likelihood of turnovers caused by poor performance by solely focusing on forced turnovers.

To address this measurement issue, we take two approaches. First, we extend the number of turnovers by examining all CEO turnover as the dependent variable. While this approach does not classify turnovers, it may include CEO turnovers that are not driven by boards and thus reduce the empirical power. Given this trade-off, we re-estimate equations (1) and (2) replacing the forced CEO turnover with all CEO turnovers.

Table 7 demonstrates the results of estimating equation (1). In Panel A, *Turnover* is an indicator variable that equals 1 if there is CEO turnover event in period *t*, else zero. We record a CEO turnover whenever the CEO identified in ExecuComp changes, after correcting for mistakes and errors (Gentry, Harrison, Quigley, and Boivie 2021).[19] In column 1, we find that the coefficient on the interaction term *RET* × *AT* is statistically significant and positive at the 5% level. The statistical significance is slightly weaker than that of column 2 in Table 2, possibly due to the aforementioned measurement issue in all CEO turnover. In columns 2-5, we use individual proxies for algorithmic trading and find consistent results. In Table 8, we also examine turnover-return sensitivity using all CEO turnover in the TSP setting. Panel A presents the results estimating equation (2) replacing *Forced* with *Turnover.* We find that our inference holds using all CEO turnovers as the dependent variable in the TSP setting.

Second, we follow Jenter and Lewellen (2021) and use performance-induced CEO turnover as the dependent variable. Conceptually, performance-induced turnover is defined as turnover that would not have occurred had performance been good. Jenter and Lewellen (2021) find that CEO turnovers that are typically classified as voluntary based on the Parrino algorithm are significantly more likely at lower levels of firm performance, indicating that many of those so-called voluntary turnovers were prompted by poor performance. They estimate that between 38% and 55% of all CEO turnovers are performance-induced, which is approximately twice the percentage of forced turnovers found in prior research. Unlike the classification of forced turnovers, the classification of performance-induced turnovers does not require any a priori determination (i.e., forced or voluntary) and only depends on firm performance for identification.[20]

We construct two performance-induced turnover variables. First, *Perf-Ind (Median)* is an indicator variable equal to 1 if any type of CEO turnover occurs in period *t* and firm *i*'s

[19] We do not include CEO turnover events that are clearly not driven by corporate boards, i.e., CEO departures due to sudden death or illness (Gentry et al. 2021). In untabulated tests, we find that the results are qualitatively similar when we include them.
[20] However, as Jenter and Lewellen (2021) point out, both performance-induced turnover and forced turnover are imperfect proxies for true forced turnover. For instance, performance-induced turnover does not capture incidences of forced turnover that are due to reasons other than observable performance, such as disagreements between the board and the CEO about long-term strategy.

performance falls below the sample median of firm performance, else zero (e.g., Jenter and Lewellen 2021; Lin et al. 2022). Following Jenter and Lewellen (2021), we measure firm performance as the industry-adjusted stock returns scaled by the stock return volatility measured over the periods *t* period *t-1*. Second, *Perf-Ind (Two-Probit)* is an indicator variable equal to 1 if any type of CEO turnover occurs in period *t* and the implied probability of performance-induced CEO turnover is greater than 50%, else zero. The implied probability of performance-induced CEO turnover is estimated using the two-stage Probit model following Jenter and Lewellen (2021) (see Table 4 therein).[21] Consistent with the return measurement in our paper and the estimation method in Jenter and Lewellen (2021), we use industry-adjusted stock returns scaled by stock return volatility measured over periods *t* and *t-1* as the primary performance measure in the estimation of the two-stage Probit model.

In Panel B of Table 7, we present the results estimating equation (1), replacing *Forced* with the performance-induced CEO turnover variable. In columns 1-5, we use *Perf-Ind (Median)* as a dependent variable and in columns 6-10, we use *Perf-Ind (Two-Probit)* as a dependent variable. Consistent with results in Tables 2 and 6, we find evidence that algorithmic trading reduces turnover-return sensitivity when we use performance-induced CEO turnover as a dependent variable. Panel B of Table 8 demonstrates the results estimating equation (2) using the TSP setting and replacing *Forced* with the performance-induced CEO turnover variable. Again, our inferences are unchanged. Overall, the findings in Tables 7 and 8 suggest that our inference is unaffected by potential measurement errors or biases in the CEO turnover classification and thus robust to various research design choices.

5.3. Earnings Properties and Algorithmic Trading

Research suggests that boards rely on market-based and accounting-based performance signals when assessing CEOs (Bushman and Smith 2001). Prior research suggests that market-

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[21] We thank Dirk Jenter for sharing the STATA code to estimate the two-stage Probit model.

based performance measures can substitute for accounting-based performance measures: stock returns would receive relatively greater weight in CEO turnover decisions when accounting-based performance measures are less informative (e.g., Engel et al. 2003). Therefore, we expect that the negative effect of algorithmic trading on turnover-return sensitivity is more pronounced when earnings are less informative.

We measure earnings informativeness as earnings timeliness or earnings persistence (Engel et al. 2003; Suk et al. 2021). First, we measure firm-level earnings timeliness as the R-squared from the regression of *EARN* on *NEG*, *Ret*, and the interaction between *NEG* and *Ret*, estimated from firm-specific rolling regressions over the prior 10-year period (e.g., Engel et al. 2003; Cho 2015). *EARN* is measured as firm *i*'s earnings before extraordinary items and discontinued operations in period *t* deflated by the beginning of year market value of equity. *Ret* is the 15-month stock return ending three months after the end of period *t*. *NEG* is a dummy variable equal to one if *Ret* is negative, else zero. We create *Low Earn Timeliness*, an indicator variable that equals one if the earnings timeliness for firm *i* in period *t* is below the sample median, else zero, and we interact this variable with *AT* and *RET*.

We report the estimation results in Appendix C. In column 1, we find a statistically significant and negative coefficient on the triple interaction term *Low Earn Timeliness* × *RET* × *AT* at the 10% level, consistent with AT decreasing turnover-return sensitivity particularly when earnings is less timely.

Second, we estimate firm-level earnings persistence by estimating a rolling regression of the change in return on assets in period *t* (ΔROA_t) on the change in return on assets in period *t-1* (ΔROA_{t-1}) over the prior 10-year period (Suk et al. 2021). The slope coefficient on the ΔROA_{t-1} variable captures the persistence of abnormal earnings. We create *Low Earn Persistence*, an indicator variable that equals one if the persistence of abnormal earnings is below the sample median, else zero, and interact this variable with *AT* and *RET*.

Consistent with the result based on earnings timeliness, in column 2 we also find a statistically significant and negative coefficient on the triple interaction term *Low Earn Persistence* × *AT* × *RET* at the 10% level. Overall, findings in these additional cross-sectional tests support our prediction that the effect of algorithmic trading on the sensitivity of forced CEO turnover to stock returns is more pronounced when firms' earnings are less informative.

6. Conclusion

We examine the effects of algorithmic trading on the extent to which corporate boards rely on stock returns in CEO turnover decisions. We find that the negative relation between the likelihood of forced CEO turnover and stock returns is reduced when algorithmic trading increases. We find consistent evidence using the 2016 Tick Size Pilot Program as an exogenous reduction of algorithmic trading, providing causal inference on the effects of AT on CEO turnover decisions. This adverse effect of algorithmic trading is more marked for growth firms, firms with greater exposure to macroeconomic factors, and firms with a geographically dispersed investor base. These findings are consistent with the notion that directors are more likely to learn from prices where the information that AT crowds out is more likely to be new to directors. Furthermore, the negative effect of algorithmic trading on turnover-return sensitivity is stronger when directors' expertise to extract decision-relevant information from prices is greater and when directors' own information set is poorer. In sum, our paper provides evidence suggesting that directors learn from stock prices in the secondary financial markets, which aggregate information about CEO performance and CEO-firm match, and that they incorporate this information into their CEO turnover decisions.

Appendix A: Variable Definitions

Variable	Description
Forced	*Forced* is an indicator equal to 1 if there is a forced CEO turnover in period *t*, and 0 otherwise. Forced CEO turnover is classified using the Parrino (1997) algorithm.
Turnover	*Turnover* is an indicator equal to 1 if there is a CEO turnover in period *t*, and 0 otherwise.
Perf-Ind (Median)	*Perf-Ind (Median)* is an indicator equal to 1 if any type of CEO turnover occurs in period *t* and firm *i*'s performance falls below the sample median of firm performance, and 0 otherwise. Firm performance is measured as the industry-adjusted stock returns scaled by the stock return volatility measured over periods *t* and *t-1*.
Perf-Ind (Two-Probit)	*Perf-Ind (Two-Probit)* is an indicator equal to 1 if any type of CEO turnover occurs in period t and the implied probability of performance-induced CEO turnover is greater than 50%, and 0 otherwise. The implied probability of performance-induced CEO turnover is estimated using the two-stage Probit model following Jenter and Lewellen (Table 4 therein). Consistent with the return measurement in our paper and following Jenter and Lewellen, we use industry-adjusted stock returns scaled by stock return volatility measured over periods *t* and *t-1* in the estimation of the two-stage Probit model.
Return	*Return* is the industry-adjusted stock returns for firm *i* measured over periods *t* and *t-1*.
Algorithmic Trading	*Algorithmic Trading* is a composite measure for AT activity obtained from the Principal Component Analysis using the four proxies for AT activity: *Odd Lot Ratio, Cancel-to-Trade Ratio, Trade-to-Order Ratio,* and *Average Trade Size.*
Odd Lot Ratio	*Odd Lot Ratio* is the natural logarithm of the equal-weighted average of the daily odd lot ratio, which is measured over periods *t* and *t-1.* The odd lot ratio is computed as the sum of all odd lot trade volume (*oddlotvol*) divided by the sum of all trade volume (*litvol*).
Cancel-to-Trade Ratio	*Cancel-to-Trade Ratio* is the natural logarithm of the equal-weighted average of the daily cancel-to-trade ratio, which is measured over periods *t* and *t-1.* The cancel-to-trade ratio is computed as the count of all canceled orders (*cancels*) divided by the count of all trades (*littrades*).
Trade-to-Order Ratio	*Trade-to-Order Ratio* is the natural logarithm of the equal-weighted average of the daily trade-to-order ratio, which is measured over periods *t* and *t-1.* The trade-to-order ratio is calculated as the sum of all trade volume (*litvol*) divided by the sum of all order volume (*ordervol*).
Average Trade Size	*Average Trade Size* is the equal-weighted average of the daily average trade size, which is measured over periods *t* and *t-1.* The average trade size is computed as the sum of all trade volume (*litvol*) divided by the count of all trades (*littrades*).
Size	*Size* is measured as the natural logarithm of market value of equity for firm *i* at the beginning of period *t*.
BTM	*BTM* is the book-to-market of equity for firm *i* at the beginning of period *t*.
RETVOL	*Return Volatility* is measured as the standard deviation of daily market-adjusted abnormal returns for firm *i* in period *t-1.*
EARNVOL	*EARNVOL* is measured as the standard deviation of return on assets over the prior 10-year period.
AIM	*AIM* is the natural logarithm of one plus the average of the daily AIM for firm *i*, which is measured over periods *t* and *t-1.* Daily AIM is measured as the ratio of absolute stock return to dollar volume [10,000,000 × absolute *ret* ÷ (*prc* × *vol*)].
ROA	*ROA* is measured as return on assets for firm *i* in period *t-1.*

# of Analysts	*# of Analysts* is the natural logarithm of one plus the number of financial analysts following firm *i* at the beginning of period *t*.
IOR	*IOR* is the level of institutional ownership for firm *i* as of the beginning of period *t*.
DIV	*DIV* is an indicator equal to 1 if the firm pays dividends in period *t-1*, and 0 otherwise.
Duality	*Duality* is an indicator equal to one if the CEO is the chairman of the board at the beginning of period *t*, and 0 otherwise.
Ownership	*Ownership* is measured as the percentage of shares owned by the CEO at the beginning of period *t*.
Age	*Age* is the natural logarithm of CEO age in years.
Tenure	*Tenure* is the natural logarithm of CEO tenure in years.
INTCAP	*INTCAP* is estimated as the replacement cost of intangible capital (Peters and Taylor 2017). The replacement cost of intangible capital is estimated as the sum of the firm's externally purchased intangible capital, i.e., goodwill, and internally created intangible capital. The replacement cost of internally created intangible capital is computed as the sum of knowledge capital (based on R&D spending) and organizational capital (based on SG&A expenses).
MACRO	*MACRO* is estimated as the R-squared from a regression of the firm's quarterly earnings on the quarterly Gross Domestic Product (GDP) and energy price index over the past four years.
Investor Diversity	*Investor Diversity* is measured as the sum of the squares of the number of non-robotic EDGAR searches by IP addresses from each state during the year, scaled by the number of all non-robotic EDGAR searches during the year (i.e., a search-weighted HHI). A larger value implies a more geographically concentrated investor base.
INDEXP	*INDEXP* is measured as the number of outside directors who have worked in the same industry as a CEO prior to joining the current firm, divided by the total number of directors.
Insider Trading	*Insider Trading* is measured as the percentage of outside directors that have traded the firm's stock at least once during the year.
Insider Profit	*Insider Profit* is measured as the average insider trading profit during the year by outside directors. For every trade, we first measure the trade profitability as the intercept (i.e., alpha) from the four factor Fama and French (1993) and Carhart (1997) model estimated over 180 days following the trade. For sales transactions, we multiply -1 to the alpha value. We take the mean alpha value if the director has multiple trades during the period.
Earn Timeliness	*Earn Timeliness* is the firm-level earnings timeliness measure derived by estimating the following equation over the prior 10-year period: $EARN = \alpha_0 + \alpha_1 NEG + \alpha_2 Ret + \alpha_3 NEG \times Ret + \varepsilon$. *EARN* is measured as firm *i*'s earnings before extraordinary items and discontinued operations in period *t* deflated by the beginning of year market value of equity. *Ret* is the 15-month stock return ending three months after the end of period *t*. *NEG* is a dummy variable equal to 1 if *Return* is negative, and 0 otherwise. The R^2 from this model captures the earnings timeliness.
Earn Persistence	*Earn Persistence* is the firm-level abnormal earnings persistence derived by estimating the following equation over the prior 10-year period: $\Delta ROA_t = \beta_0 + \beta_1 \Delta ROA_{t-1} + \varepsilon_t$, where ΔROA_t is the industry-adjusted return on assets for firm *i* in period *t*. The slope coefficient β_1 captures the persistence of abnormal earnings.

Appendix B: Validity Checks – Tick Size Pilot Program

	Odd Lot Ratio		*Cancel-to-Trade Ratio*		*Trade-to-Order Ratio*		*Average Trade Size*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat	0.014		0.040		-0.014		-0.001	
	(0.039)		(0.037)		(0.040)		(0.021)	
Post	0.356***		-0.180***		0.130***		-0.205***	
	(0.013)		(0.015)		(0.014)		(0.008)	
Treat* × *Post	**-0.167*** **	**-0.157*** **	**-0.268*** **	**-0.266*** **	**0.251*** **	**0.238*** **	**0.059*** **	**0.054*** **
	(0.019)	**(0.016)**	**(0.021)**	**(0.019)**	**(0.021)**	**(0.019)**	**(0.013)**	**(0.012)**
Size		0.041		-0.044		-0.052*		-0.031*
		(0.028)		(0.030)		(0.027)		(0.016)
BTM		-0.188***		-0.135***		0.102**		0.067***
		(0.039)		(0.041)		(0.041)		(0.023)
RETVOL		-5.727***		-5.554***		5.516***		3.265***
		(1.481)		(1.624)		(1.576)		(0.942)
EARNVOL		-0.442		-0.338		0.534		0.083
		(0.326)		(0.368)		(0.357)		(0.214)
AIM		0.134		0.599***		-0.132*		0.125
		(0.088)		(0.129)		(0.079)		(0.080)
ROA		0.297***		0.127		-0.076		-0.064
		(0.086)		(0.110)		(0.088)		(0.056)
# of Analysts		-0.030		-0.015		0.033		0.008
		(0.019)		(0.021)		(0.021)		(0.012)
IOR		0.123		0.061		-0.161***		-0.083
		(0.079)		(0.090)		(0.056)		(0.069)
DIV		-0.052		-0.072**		0.061*		0.017
		(0.033)		(0.033)		(0.034)		(0.020)
Fixed Effects (Firm, Year)	No	Yes	No	Yes	No	Yes	No	Yes
Observations	2,507	2,507	2,507	2,507	2,507	2,507	2,507	2,507
Adjusted R2	0.086	0.912	0.142	0.867	0.098	0.882	0.105	0.854

The dependent variable used in each column is denoted in the top row of the table. *Treat* is a dummy variable equal to 1 for treatment firms, and 0 for control firms. *Post* is a dummy variable equal to 1 for the fiscal years of 2018 and 2017, and 0 for the fiscal years of 2015 and 2016. All variables are defined in Appendix A. Standard errors are clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Robust standard errors are in parentheses.

Appendix C: Additional Cross-Sectional Tests – Earnings Informativeness

	Forced	
	(1)	
RET	-0.097***	-0.085***
	(0.026)	(0.026)
AT	-0.061*	-0.075**
	(0.033)	(0.033)
RET × AT	0.072*	0.059
	(0.038)	(0.038)
Low Earn Timeliness	0.059**	
	(0.028)	
Low Earn Timeliness × RET	-0.071*	
	(0.038)	
Low Earn Timeliness × AT	-0.093**	
	(0.042)	
Low Earn Timeliness × RET × AT	**0.101***	
	(0.056)	
Low Earn Persistence		0.053*
		(0.028)
Low Earn Persistence × RET		-0.081**
		(0.037)
Low Earn Persistence × AT		-0.066
		(0.043)
Low Earn Persistence × RET × AT		**0.094***
		(0.057)
Control Variables	Yes	Yes
Fixed Effects (Firm, Year)	Yes	Yes
Observations	9,773	10,312
Adjusted R2	0.079	0.083

This table presents additional cross-sectional tests based on earnings properties. *Forced* is an indicator equal to 1 if forced CEO turnover occurs in period t and 0 otherwise. *AT* is a decile-ranked variable and is based on a composite measure of algorithmic trading obtained from principal component analysis using four algorithmic trading proxies measured over periods *t* and *t-1*. To create the *AT* variable, we rank the composite measure into deciles ranging from 1 to 10 and divide it by 10. *RET* is a decile-ranked variable of industry-adjusted stock returns measured over periods *t* and *t-1*. To create *RET*, we rank the industry-adjusted stock returns into deciles ranging from 1 to 10 and divide it by 10. *Earn Timeliness* is measured as R^2 from estimating the following equation on a rolling horizon basis using the prior 10-year period: $EARN = \alpha_0 + \alpha_1 NEG + \alpha_2 Ret + \alpha_3 NEG \times Ret + \varepsilon$. *EARN* is measured as firm *i*'s earnings before extraordinary items and discontinued operations in period *t* deflated by the beginning of year market value of equity. *Ret* is the 15-month stock return ending three months after the end of period *t*. *NEG* is a dummy variable equal to 1 if *Ret* is negative, and 0 otherwise. *Low Earn Timeliness* is an indicator equal to 1 if *Earn Timeliness* is below the sample median, and 0 otherwise. *Earn Persistence* is a firm-level abnormal earnings persistence on a rolling horizon basis using the prior 10-year period: $\Delta ROA_t = \beta_0 + \beta_1 \Delta ROA_{t-1} + \varepsilon_t$, where ΔROA_t is the change in industry-adjusted return on assets for firm *i* in period *t*. The slope coefficient β_2 captures the persistence of abnormal earnings. *Low Earn Persistence* is an indicator equal to 1 if *Earn Persistence* is below the sample median, and 0 otherwise. All variables are defined in Appendix A. Standard errors are clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Robust standard errors are in parentheses.

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Table 1 Descriptive Statistics

Panel A Descriptive Statistics

	N	Mean	STD	P25	Median	P75
Forced	11,857	0.036	0.186	0.000	0.000	0.000
Turnover	11,857	0.101	0.301	0.000	0.000	0.000
Perf-Ind (Median)	11,857	0.061	0.239	0.000	0.000	0.000
Perf-Ind (Two-Probit)	11,857	0.052	0.222	0.000	0.000	0.000
Return	11,857	0.062	0.435	-0.201	-0.004	0.236
Algorithmic Trading	11,857	0.000	1.430	-0.875	-0.052	0.841
Odd Lot Ratio	11,857	2.650	0.714	2.190	2.705	3.176
Cancel-to-Trade Ratio	11,857	3.269	0.446	2.966	3.208	3.487
Trade-to-Order Ratio	11,857	0.969	0.428	0.704	1.017	1.281
Average Trade Size	11,857	2.360	0.367	2.098	2.338	2.602
Size	11,857	7.927	1.630	6.802	7.782	8.994
BTM	11,857	0.530	0.407	0.251	0.449	0.737
RETVOL	11,857	0.018	0.008	0.012	0.016	0.022
EARNVOL	11,857	0.049	0.057	0.014	0.030	0.061
AIM	11,857	0.056	0.194	0.002	0.007	0.027
ROA	11,857	0.049	0.093	0.012	0.045	0.090
# of Analysts	11,857	2.197	0.784	1.609	2.303	2.833
IOR	11,857	0.772	0.232	0.694	0.837	0.930
DIV	11,857	0.609	0.488	0.000	1.000	1.000
Duality	11,857	0.447	0.497	0.000	0.000	1.000
Ownership	11,857	0.021	0.068	0.001	0.003	0.011
Age	11,857	4.040	0.124	3.970	4.043	4.111
Tenure	11,857	1.919	0.820	1.338	1.946	2.527

Panel B Pairwise Correlations Among AT Proxies

		(1)	(2)	(3)	(4)
(1)	*Algorithmic Trading*				
(2)	*Odd Lot Ratio*	**0.852**			
(3)	*Cancel-to-Trade Ratio*	**0.698**	**0.303**		
(4)	*Trade-to-Order Ratio*	**-0.685**	**-0.278**	**-0.812**	
(5)	*Average Trade Size*	**-0.738**	**-0.931**	**-0.101**	**0.116**

This table presents descriptive statistics for our sample during the sample period between 2012 and 2019. Panel A presents summary statistics for the variables used in our analyses. To avoid undue outlier influence, all continuous variables are winsorized at the 1st and 99th percentiles. Panel B presents pairwise correlations among the AT proxies. The significance level at 1% is bolded. All variables are defined in Appendix A.

Table 2 Algorithmic Trading and Forced CEO Turnover

	Forced		
	(1)	(2)	(3)
Return	**-0.044***** **(0.005)**	**-0.072***** **(0.011)**	
AT	-0.052*** (0.018)	-0.056*** (0.018)	-0.111*** (0.026)
Return × AT		**0.057***** **(0.016)**	
RET			-0.122*** (0.017)
RET × AT			**0.111***** **(0.026)**
Size	0.001 (0.008)	0.001 (0.008)	0.001 (0.008)
BTM	0.044*** (0.014)	0.042*** (0.014)	0.042*** (0.014)
RETVOL	-0.682 (0.610)	-0.618 (0.613)	-0.732 (0.610)
EARNVOL	0.216* (0.123)	0.203* (0.123)	0.192 (0.122)
AIM	-0.010 (0.017)	-0.007 (0.018)	-0.007 (0.018)
ROA	-0.035 (0.037)	-0.032 (0.037)	-0.037 (0.036)
# of Analysts	-0.008 (0.007)	-0.008 (0.007)	-0.007 (0.007)
IOR	0.003 (0.018)	0.004 (0.018)	0.004 (0.018)
DIV	-0.000 (0.010)	-0.001 (0.010)	-0.001 (0.010)
Duality	-0.043*** (0.010)	-0.043*** (0.010)	-0.042*** (0.010)
Ownership	-0.076 (0.052)	-0.080 (0.052)	-0.084 (0.052)
Age	-0.132*** (0.047)	-0.131*** (0.047)	-0.130*** (0.047)
Tenure	0.069*** (0.006)	0.069*** (0.006)	0.069*** (0.006)
Firm Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Observations	11,857	11,857	11,857
Adjusted R2	0.078	0.079	0.079

This table presents the estimation results from the regression of an indicator for forced CEO turnover on algorithmic trading and control variables. *Forced* is an indicator equal to 1 if forced CEO turnover occurs in period t, and 0 otherwise. *Return* is the industry-adjusted stock returns measured over periods *t* and *t-1*. *AT* is a decile-ranked variable and based on a composite measure of algorithmic trading obtained from principal component analysis using four algorithmic trading proxies: Odd Lot Ratio, Cancel-to-Trade Ratio, Trade-to-Order Ratio, and Average Trade Size. The algorithmic trading proxies are measured over the same performance measurement window as *Return*. To create the *AT* variable, we rank the composite measure into deciles ranging from 1 to 10 and divide it by 10. *RET* is a decile-ranked variable of industry-adjusted stock returns (*Return*). To create *RET*, we rank the industry-adjusted stock returns into deciles ranging from 1 to 10 and divide it by 10. All other variables are defined in Appendix A. Standard errors are clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Robust standard errors are in parentheses.

Table 3 Tick Size Pilot Program and CEO Turnover

Panel A Difference-in-Differences Estimation

	Forced		
	(1)	(2)	(3)
Treat	-0.074**	-0.066**	
	(0.030)	(0.029)	
Post	-0.070**	-0.060*	
	(0.031)	(0.031)	
Treat × Post	0.128***	0.120***	0.119***
	(0.041)	(0.040)	(0.041)
RET	-0.137***	-0.126***	-0.091***
	(0.034)	(0.033)	(0.033)
RET × Treat	0.102**	0.090**	0.077*
	(0.040)	(0.040)	(0.044)
RET × Post	0.098**	0.085**	0.073*
	(0.044)	(0.043)	(0.041)
RET × Treat × Post	**-0.177*** **	**-0.160*** **	**-0.171*** **
	(0.056)	**(0.056)**	**(0.060)**
Control Variables	No	Yes	Yes
Firm Fixed Effects	No	No	Yes
Year Fixed Effects	No	No	Yes
Observations	2,507	2,507	2,507
Adjusted R2	0.022	0.032	0.111

Panel B Falsification Test

	Forced		
	(1)	(2)	(3)
Treat	-0.009	-0.006	
	(0.028)	(0.028)	
Pseudo Post	-0.042	-0.042	
	(0.028)	(0.027)	
Treat × Pseudo Post	0.014	0.014	0.044
	(0.039)	(0.038)	(0.039)
RET	-0.082***	-0.077***	-0.034
	(0.027)	(0.026)	(0.030)
RET × Treat	0.010	0.006	0.015
	(0.039)	(0.038)	(0.041)
RET × Pseudo Post	0.055	0.057	0.053
	(0.042)	(0.041)	(0.043)
RET × Treat × Pseudo Post	**-0.017**	**-0.017**	**-0.068**
	(0.056)	**(0.055)**	**(0.057)**
Control Variables	No	Yes	Yes
Firm Fixed Effects	No	No	Yes
Year Fixed Effects	No	No	Yes
Observations	2,272	2,272	2,272
Adjusted R2	0.008	0.020	0.097

This table demonstrates the results from the difference-in-differences estimation of forced CEO turnover. *Forced* is an indicator equal to 1 if forced CEO turnover occurs in period t and 0 otherwise. *RET* is a decile-ranked industry-adjusted stock return variable measured over periods *t* and *t-1*. *Treat* is a dummy variable equal to 1 for treatment firms, and 0 for control firms in the Tick Size Pilot Program. In Panel A, *Post* is a dummy variable equal to 1 for the fiscal years of 2017 and 2018, and 0 for the fiscal years of 2015 and 2016. In Panel B, *Pseudo Post* is a dummy variable equal to 1 for the fiscal years of 2013 and 2014, and 0 for the fiscal years of 2011 and 2012. All variables are defined in Appendix A. Standard errors are clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Robust standard errors are in parentheses.

Table 4 Cross-Sectional Tests: Firm Characteristics and Market Feedback

	Forced		
	(1)	(2)	(3)
RET	-0.093***	-0.090***	-0.065**
	(0.022)	(0.024)	(0.027)
AT	-0.055*	-0.084***	-0.079**
	(0.032)	(0.032)	(0.036)
RET × AT	0.058*	0.061	0.036
	(0.032)	(0.037)	(0.040)
High INTCAP	0.094***		
	(0.031)		
High INTCAP × RET	-0.068**		
	(0.034)		
High INTCAP × AT	-0.137***		
	(0.047)		
High INTCAP × RET × AT	**0.137****		
	(0.053)		
High MACRO		0.050**	
		(0.024)	
High MACRO × RET		-0.066**	
		(0.033)	
High MACRO × AT		-0.059	
		(0.038)	
High MACRO × RET × AT		**0.096***	
		(0.051)	
High Investor Diversity			0.042
			(0.028)
High Investor Diversity × RET			-0.065*
			(0.039)
High Investor Diversity × AT			-0.098**
			(0.045)
High Investor Diversity × RET × AT			**0.117****
			(0.059)
Control Variables	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Observations	11,857	11,857	8,809
Adjusted R2	0.081	0.080	0.091

This table demonstrates the cross-sectional tests in settings where boards actively learn from stock prices. *Forced* is an indicator equal to 1 if forced CEO turnover occurs in period t and 0 otherwise. *AT* is a decile-ranked variable for the composite measure of algorithmic trading measured over periods *t* and *t-1*. *RET* is a decile-ranked variable of industry-adjusted stock returns measured over periods *t* and *t-1*. *High INTCAP* is an indicator equal to 1 if a firm's replacement costs of intangible capital (*INTCAP*) are above the sample median, and 0 otherwise. *High MACRO* is an indicator equal to 1 if the extent of a firm's exposure to macroeconomic factors (*MACRO*) is above the sample median, and 0 otherwise. *High Investor Diversity* is equal to 1 if the extent of a firm's investors' geographic diversity (*Investor Diversity*) is below the sample median, and 0 otherwise. Investors' geographic diversity is measured as the sum of the squares of the number of non-robotic EDGAR searches by IP addresses from each state during the year, scaled by the number of all non-robotic EDGAR searches during the year. A lower value indicates more geographic dispersion of the firm's investor base. All variables are defined in Appendix A. Standard errors are clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Robust standard errors are in parentheses.

Table 5 Cross-Sectional Tests: Boards' Characteristics and Market Feedback

	Forced		
	(1)	(2)	(3)
RET	-0.105***	-0.098***	-0.108***
	(0.022)	(0.021)	(0.020)
AT	-0.082***	-0.087***	-0.091***
	(0.031)	(0.029)	(0.029)
RET × AT	0.073**	0.064**	0.084***
	(0.033)	(0.032)	(0.031)
High INDEXP	0.034		
	(0.031)		
High INDEXP × RET	-0.047		
	(0.036)		
High INDEXP × AT	-0.081*		
	(0.046)		
High INDEXP × RET × AT	**0.105****		
	(0.053)		
Low Insider Trading		0.031	
		(0.025)	
Low Insider Trading × RET		-0.072**	
		(0.035)	
Low Insider Trading × AT		-0.071*	
		(0.037)	
Low Insider Trading × RET × AT		**0.136*****	
		(0.052)	
Low Insider Profit			0.038
			(0.026)
Low Insider Profit × RET			-0.047
			(0.036)
Low Insider Profit × AT			-0.070*
			(0.039)
Low Insider Profit × RET × AT			**0.087***
			(0.053)
Control Variables	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Observations	11,857	11,857	11,857
Adjusted R2	0.079	0.080	0.079

This table demonstrates the cross-sectional tests in settings where boards have greater ability or incentive to learn from stock prices. *Forced* is an indicator equal to 1 if forced CEO turnover occurs in period t and 0 otherwise. *AT* is a decile-ranked variable for the composite measure of algorithmic trading measured over periods *t* and *t-1*. *RET* is a decile-ranked variable of industry-adjusted stock returns measured over periods *t* and *t-1*. *High INDEXP* is an indicator equal to 1 if the extent of outside directors' industry experience (*INDEXP*) is greater than the sample median, and 0 otherwise. *Low Insider Trading* is an indicator equal to 1 if the percentage of the board engaging in insider trading (*Insider Trading*) is less than the sample median, and 0 otherwise. *Low Insider Profit* is an indicator equal to 1 if the average insider trading profit by directors (*Insider Profit*) is less than the sample median, and 0 otherwise. All variables are defined in Appendix A. Standard errors are clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Robust standard errors are in parentheses.

Table 6 Individual Algorithmic Trading Proxy and CEO Turnover

	Forced			
	(1)	(2)	(3)	(4)
RET	-0.116***	-0.112***	-0.003	-0.012
	(0.018)	(0.017)	(0.015)	(0.015)
OLR	-0.086***			
	(0.026)			
RET* × *OLR	**0.093*** **			
	(0.026)			
CTR		-0.071***		
		(0.022)		
RET* × *CTR		**0.089*** **		
		(0.027)		
TOR			0.091***	
			(0.023)	
RET* × *TOR			**-0.104*** **	
			(0.026)	
ATS				0.087***
				(0.023)
RET* × *ATS				**-0.096*** **
				(0.026)
Control Variables	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	11,857	11,857	11,857	11,857
Adjusted R2	0.078	0.078	0.079	0.078

This table demonstrates the estimation results using individual algorithmic trading proxies. *Forced* is an indicator equal to 1 if forced CEO turnover occurs in period t, and 0 otherwise. *OLR* is a decile-ranked variable for the odd-lot-ratio. *CTR* is a decile-ranked variable for the cancel-to-trade ratio. *TOR* is a decile-ranked variable for the trade-to-order ratio. *ATS* is a decile-ranked variable for the average trade size. Algorithmic trading proxies are measured over periods *t* and *t-1*. *RET* is a decile-ranked variable of industry-adjusted stock returns measured over periods *t* and *t-1*. All variables are defined in Appendix A. Standard errors are clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Robust standard errors are in parentheses.

Table 7 Alternative CEO Turnover Classifications

Panel A All CEO Turnover

	Turnover				
	(1)	(2)	(3)	(4)	(5)
RET	-0.121***	-0.117***	-0.114***	-0.014	-0.025
	(0.024)	(0.025)	(0.024)	(0.025)	(0.023)
AT	-0.117***				
	(0.037)				
RET* × *AT	**0.092****				
	(0.039)				
OLR		-0.090**			
		(0.038)			
RET* × *OLR		**0.080****			
		(0.039)			
CTR			-0.073**		
			(0.031)		
RET* × *CTR			**0.077***		
			(0.040)		
TOR				0.098***	
				(0.032)	
RET* × *TOR				**-0.101*****	
				(0.039)	
ATS					0.084**
					(0.036)
RET* × *ATS					**-0.090****
					(0.038)
Size	-0.006	-0.007	-0.010	-0.009	-0.008
	(0.011)	(0.011)	(0.010)	(0.010)	(0.011)
BTM	0.030*	0.032*	0.033*	0.031*	0.033*
	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)
RETVOL	-0.089	0.077	0.066	0.018	0.131
	(0.874)	(0.877)	(0.876)	(0.873)	(0.881)
EARNVOL	0.332**	0.337**	0.335**	0.338**	0.343**
	(0.165)	(0.165)	(0.166)	(0.166)	(0.165)
AIM	-0.026	-0.031	-0.027	-0.034	-0.040
	(0.029)	(0.030)	(0.030)	(0.029)	(0.030)
ROA	-0.069	-0.072	-0.076	-0.071	-0.073
	(0.049)	(0.049)	(0.049)	(0.049)	(0.049)
# of Analysts	-0.013	-0.013	-0.013	-0.014	-0.013
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
IOR	-0.010	-0.012	-0.009	-0.010	-0.012
	(0.027)	(0.028)	(0.028)	(0.028)	(0.028)
DIV	-0.002	-0.001	-0.001	-0.002	-0.001
	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
Duality	0.006	0.006	0.006	0.005	0.006
	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
Ownership	-0.123	-0.127	-0.134	-0.139	-0.130
	(0.105)	(0.106)	(0.105)	(0.105)	(0.105)
Age	0.529***	0.529***	0.530***	0.531***	0.529***
	(0.068)	(0.068)	(0.067)	(0.068)	(0.068)
Tenure	0.136***	0.135***	0.135***	0.135***	0.135***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	11,857	11,857	11,857	11,857	11,857
Adjusted R2	0.088	0.088	0.088	0.088	0.088

Panel B Performance-Induced CEO Turnover

	Perf Ind (Median)					*Perf Ind (Two-Probit)*				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
RET	-0.233***	-0.238***	-0.226***	-0.152***	-0.153***	-0.173***	-0.164***	-0.180***	-0.084***	-0.106***
	(0.018)	(0.019)	(0.018)	(0.019)	(0.018)	(0.020)	(0.021)	(0.019)	(0.020)	(0.019)
AT	-0.115***					-0.119***				
	(0.031)					(0.031)				
RET* × *AT	**0.074****					**0.077****				
	(0.030)					**(0.032)**				
OLR		-0.089***					-0.087***			
		(0.031)					(0.032)			
RET* × *OLR		**0.078*****					**0.055***			
		(0.030)					**(0.033)**			
CTR			-0.072***					-0.084***		
			(0.026)					(0.026)		
RET* × *CTR			**0.059****					**0.089*****		
			(0.030)					**(0.032)**		
TOR				0.095***					0.093***	
				(0.028)					(0.028)	
RET* × *TOR				**-0.072****					**-0.084*****	
				(0.029)					**(0.032)**	
ATS					0.087***					0.086***
					(0.030)					(0.029)
RET* × *ATS					**-0.080*****					**-0.051**
					(0.029)					**(0.032)**
Size	-0.005	-0.006	-0.011	-0.009	-0.007	-0.017*	-0.017**	-0.022**	-0.021**	-0.018**
	(0.009)	(0.009)	(0.009)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
BTM	0.012	0.015	0.016	0.014	0.015	0.006	0.009	0.009	0.008	0.009
	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)
RETVOL	-1.064	-0.850	-0.909	-0.968	-0.828	-0.046	0.121	0.154	0.099	0.129
	(0.664)	(0.662)	(0.657)	(0.655)	(0.661)	(0.665)	(0.661)	(0.656)	(0.658)	(0.656)
EARNVOL	0.285**	0.290**	0.286**	0.289**	0.299**	-0.081	-0.071	-0.080	-0.075	-0.059
	(0.131)	(0.130)	(0.132)	(0.132)	(0.130)	(0.154)	(0.154)	(0.155)	(0.155)	(0.154)
AIM	-0.011	-0.016	-0.011	-0.020	-0.027	-0.020	-0.025	-0.021	-0.030	-0.039*
	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)
ROA	-0.085**	-0.089**	-0.090**	-0.087**	-0.089**	-0.019	-0.021	-0.025	-0.021	-0.023
	(0.039)	(0.039)	(0.039)	(0.039)	(0.039)	(0.045)	(0.045)	(0.046)	(0.045)	(0.045)
# of Analysts	-0.006	-0.006	-0.006	-0.007	-0.006	-0.003	-0.003	-0.002	-0.003	-0.002

	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
IOR	0.014	0.011	0.015	0.015	0.011	0.044**	0.041*	0.044**	0.044**	0.040*
	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)
DIV	0.001	0.002	0.001	0.000	0.001	-0.030**	-0.029**	-0.030**	-0.030**	-0.029**
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
Duality	-0.008	-0.007	-0.008	-0.008	-0.007	-0.006	-0.005	-0.006	-0.006	-0.005
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Ownership	-0.054	-0.061	-0.065	-0.069	-0.063	-0.060	-0.065	-0.075	-0.078	-0.065
	(0.088)	(0.088)	(0.088)	(0.088)	(0.088)	(0.082)	(0.082)	(0.082)	(0.082)	(0.082)
Age	0.249***	0.250***	0.251***	0.251***	0.249***	-0.183***	-0.183***	-0.181***	-0.180***	-0.185***
	(0.055)	(0.055)	(0.055)	(0.055)	(0.055)	(0.051)	(0.051)	(0.051)	(0.051)	(0.051)
Tenure	0.083***	0.082***	0.082***	0.082***	0.082***	0.062***	0.061***	0.061***	0.061***	0.061***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11,857	11,857	11,857	11,857	11,857	11,857	11,857	11,857	11,857	11,857
Adjusted R2	0.103	0.102	0.102	0.102	0.102	0.125	0.124	0.124	0.124	0.124

This table demonstrates the results estimating equation (1) using alternative CEO turnover classifications. In Panel A, the dependent variable is *Turnover*, which is an indicator equal to 1 if a CEO turnover occurs in period *t* and 0 otherwise. *AT* is a decile-ranked variable for the composite measure of algorithmic trading. *OLR* is a decile-ranked variable for the odd-lot-ratio. *CTR* is a decile-ranked variable for the cancel-to-trade ratio. *TOR* is a decile-ranked variable for the trade-to-order ratio. *ATS* is a decile-ranked variable for the average trade size. Algorithmic trading proxies are measured over periods *t* and *t-1*. *RET* is a decile-ranked variable of industry-adjusted stock returns measured over periods *t* and *t-1*. In Panel B, the dependent variable is measured as the performance-induced CEO turnover. In columns 1-5, *Perf-Ind (Median)* is an indicator equal to 1 if any type of CEO turnover occurs in period t and firm i's performance falls below the sample median of firm performance, and 0 otherwise. Firm performance is measured as the industry-adjusted stock returns scaled by the stock return volatility measured over periods *t* and *t-1*. In columns 6-10. *Perf-Ind (Two-Probit)* is an indicator equal to 1 if any type of CEO turnover occurs in period *t* and the implied probability of performance-induced CEO turnover is greater than 50%, and 0 otherwise. All variables are defined in Appendix A. Standard errors are clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Robust standard errors are in parentheses. All variables are defined in Appendix A. Standard errors are clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Robust standard errors are in parentheses.

Table 8 Alternative CEO Turnover Classifications and TSP Experiment

Panel A All CEO Turnover

	Turnover		
	(1)	(2)	(3)
Treat	-0.065*	-0.066*	
	(0.035)	(0.035)	
Post	-0.045	-0.046	
	(0.037)	(0.038)	
Treat × Post	0.137***	0.136***	0.155***
	(0.051)	(0.051)	(0.052)
RET	-0.108***	-0.104**	-0.056
	(0.041)	(0.041)	(0.046)
RET × Treat	0.067	0.066	0.072
	(0.052)	(0.052)	(0.062)
RET × Post	0.041	0.039	0.035
	(0.056)	(0.056)	(0.058)
RET × Treat × Post	**-0.134***	**-0.133***	**-0.210*** **
	(0.075)	**(0.075)**	**(0.079)**
Control Variables	No	Yes	Yes
Firm Fixed Effects	No	No	Yes
Year Fixed Effects	No	No	Yes
Observations	2,507	2,507	2,507
Adjusted R2	0.011	0.030	0.204

Panel B Performance-Induced CEO Turnover

	Perf-Ind (Median)			*Perf-Ind (Two-Probit)*		
	(1)	(2)	(3)	(1)	(2)	(3)
Treat	-0.069**	-0.068**		-0.061**	-0.057**	
	(0.033)	(0.033)		(0.029)	(0.029)	
Post	-0.042	-0.041		0.035	0.045	
	(0.035)	(0.036)		(0.034)	(0.035)	
Treat × Post	**0.143*** **	**0.138*** **	**0.142*** **	**0.117** **	**0.108** **	**0.104** **
	(0.049)	**(0.049)**	**(0.049)**	**(0.048)**	**(0.048)**	**(0.052)**
RET	-0.206***	-0.201***	-0.193***	-0.136***	-0.125***	-0.094**
	(0.033)	(0.033)	(0.036)	(0.030)	(0.031)	(0.043)
RET × Treat	0.083**	0.080*	0.083*	0.089**	0.085**	0.064
	(0.042)	(0.042)	(0.049)	(0.040)	(0.040)	(0.053)
RET × Post	0.043	0.038	0.056	-0.019	-0.038	-0.031
	(0.045)	(0.046)	(0.048)	(0.046)	(0.046)	(0.054)
RET × Treat × Post	**-0.152** **	**-0.143** **	**-0.182*** **	**-0.170*** **	**-0.152** **	**-0.172** **
	(0.061)	**(0.061)**	**(0.067)**	**(0.064)**	**(0.064)**	**(0.074)**
Controls	No	Yes	Yes	No	Yes	Yes
Firm Fixed Effects	No	No	Yes	No	No	Yes
Year Fixed Effects	No	No	Yes	No	No	Yes
Observations	2,507	2,507	2,507	2,507	2,507	2,507
Adjusted R2	0.060	0.065	0.156	0.049	0.062	0.139

This table demonstrates the results estimating equation (2) using alternative CEO turnover classifications. In Panel A, the dependent variable is *Turnover*, which is an indicator equal to 1 if a CEO turnover occurs in period *t* and 0 otherwise. *Treat* is a dummy variable equal to 1 for treatment firms, and 0 for control firms in the Tick Size Pilot Program. *Post* is a dummy variable equal to 1 for fiscal years 2017 and 2018, and 0 for fiscal years 2015 and 2016. *RET* is a decile-ranked industry-adjusted stock return variable measured over periods *t* and *t-1*. In Panel B, the dependent variable is measured as the performance-induced CEO turnover. In columns 1-3, *Perf-Ind (Median)* is an indicator equal to 1 if any type of CEO turnover occurs in period *t* and firm *i*'s performance falls below the sample median of firm performance, and 0 otherwise. In columns 4-6. *Perf-Ind (Two-Probit)* is an indicator equal to 1 if any type of CEO turnover occurs in period *t* and the implied probability of performance-induced CEO turnover is greater than 50%, and 0 otherwise. All variables are defined in Appendix A. Standard errors are clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. Robust standard errors are in parentheses.