# Algorithmic trading and corporate innovation: Evidence from the Tick Size Pilot

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## Abstract

We propose that stock price efficiency, i.e., the speed and the extent with which prices reflect information in the public domain, reduces underinvestment in innovation. Managers are willing to devote more resources to innovation because more efficient prices better reflect outcomes of managerial effort, such as the choice of investment projects, and the stock price performance frequently features in managerial compensation contracts. Using the intensity of algorithmic trading (AT) to capture price efficiency and the Tick Size Pilot experiment as an exogenous shock to AT, we establish a causal positive relation between price efficiency and innovation measured by patents. The relation is stronger for firms where managerial compensation is more closely linked to the share price performance and for more opaque firms where managerial effort is more difficult to infer from accounting information. Our results generalize to other measures of innovation such as R&D spending and citations.

JEL: D53; G12; G14; M41

Keywords: algorithmic trading; patents; citations; innovation

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## 1. Introduction

Innovation is a key driver of corporate growth and is estimated to account for 50% of U.S. GDP growth (He and Tian, 2018). Porter (1992, p. 65) argues that '[T]o compete effectively in international markets, a nation's businesses must continuously innovate and upgrade their competitive advantages.' However, innovation is a lengthy and costly process of developing and testing new ideas that associates with a high project failure rate (Holmstrom 1989). Risk-averse managers dislike spending resources on projects with uncertain outcomes and delayed payoffs leading to underinvestment in innovation (Aghion, Van Reenen, and Zingales, 2013). We propose that underinvestment is exacerbated when price efficiency—the speed and the extent with which prices impound information entering the public domain—is low because prices do not reflect outcomes of managerial effort, such as the choice of investment projects and their impact on earnings growth and future returns. Managers care about prices capturing their decisions because share price targets are a frequent performance measures in compensation contracts (Ittner, Larcker, and Rajan 1997; Indjejikian and Nanda 2002; Core, Guay, and Verrecchia 2003), and the share price performance bears on managers' career outcomes (Chang, Dasgupta and Hilary 2010). Bond, Edmans and Goldstein (2012, p.5) argue that decision makers 'care about market prices because they are party to contracts that are contingent on market prices. This is most relevant for firm managers, whose compensation is tied to the firm's share price. Then, the manager's incentives to take real actions will depend on the extent to which they will be reflected in the stock price.'

We propose that algorithmic traders (ATs), defined as investors who use automated systems to execute low-latency trading strategies, increase price efficiency, which in turn reduces underinvestment in innovation.[1] Algorithmic trading (AT) has material effect on price discovery as in recent years has 'accounted for more than 50 per cent of the reported trading volume in

[1] ATs trade frequently during the day and act strategically with respect to trading information from other investors, public news and order flow, profiting by either providing or taking liquidity and by taking advantage of even the smallest trading opportunities. ATs end up the trading day with zero or very low stock inventory.

U.S. stock markets', Lee and Watts (2021, p.375). AT increases price efficiency by quickly incorporating new public information into stocks prices through their trades and through liquidity supply to non-ATs, such as hedge funds, that trade on public signals (Chordia and Miao, 2020, Chakrabarty, Moulton, and Wang 2021, Rindi and Werner, 2019, Albuquerque, Song, and Yao, 2020, Zhang 2010). Innovation signals that ATs and other investors can trade-on come from several sources, including patent grants from the U.S. Patent and Trademark Office, thus can be easily picked up by ATs' computer algorithms.

Importantly for our setting, previous research documents that stock prices do not fully incorporate public information on firm innovation. Gu (2005, p.385) reports that analysts and investors 'do not fully incorporate the implication of enhanced innovation capabilities for future earnings into stock prices and earnings forecasts. This bias is significantly associated with future abnormal stock returns.' Deng, Lev and Narin (1999) find that current period patent count and citations, readily available public metrics of corporate innovation, predict future abnormal returns. Higher price efficiency promoted through AT should associate with fast and more complete impounding into stock prices of signals about the impact today's innovation will have on future earnings incentivizing managers to engage in more innovation.[2]

It is not obvious that higher price efficiency promoted by AT would increase investment rates. AT increases stock liquidity, which in turn increases the incentive for non-dedicated investors to hold stocks. Fang, Tian, and Tice (2014) document that higher ownership by non-dedicated investors reduces managerial incentives to innovate. They report that 'higher liquidity introduced through decimalization promotes ownership by non-dedicated institutions who increase pressure on managers to boost current profits and cut long-term investment in innovation or risk the exit of these investors.', Fang et al. (2014, p. 208). Higher stock liquid can

[2] Managers may *overinvest* when investors misinterpret higher investment levels as a signal of better outlook (Bebchuk and Stole 1993), when firms generate free cash flow (Jensen 1986, Stulz 1990) or when managers prioritize private benefits, such as larger firms (Malmendier and Tate, 2005). Higher price efficiency should contribute to a reduction in overinvestment as signals of disappointing investment outcomes, e.g., poor M&A performance, impound quicker into stock prices disciplining managers.

also lower acquisition costs, in turn motivating managers to reduce innovation spending to improve firm short-term performance and reduce takeover pressure (Fang et al. 2014; Stein 1988; Shleifer and Summers 1988). Thus, whether ATs promote or impede innovation is a question we tackle empirically.

To establish causality between AT and corporate innovation, we follow Lee and Watts (2021) and Chakrabarty, Cox and Upson (2021) and take advantage of the exogenous shock to AT related to the regulatory Tick Size Pilot (TSP) program. In October 2016, the SEC started a two-year experimental program to examine the impact an increase in tick size will have on market quality and liquidity provision of small-capitalization stocks (market capitalization of $3 billion or less). SEC randomly selected 1,200 treatment firms where the tick size increased from $0.01 to $0.05, and a sample of 1,400 securities that continued trading with a tick size of $0.01. The pilot ran for two years after which treatment stocks reverted to the original $0.01 tick size. A consequence of a larger tick size was (i) lower frequency with which quotes were updated, eroding the speed advantage of algorithmic trades (Foucault, Roell, and Sandas 2003), and (ii) higher cost ATs faced when stepping in front of other limit orders, which reduced their incentives to trade in affected stocks. Lee and Watts (2021) show significant reductions in AT in treated, but not in control stocks, after the start of TSP.[3] Consistent with lower AT reducing price discovery, Chakrabarty, Cox and Upson (2021, p. 3) report 'that the relative price discovery of tick-constrained [treated] firms decreases significantly' compared to control stocks. The TSP program has the classic characteristics of a laboratory-style randomized natural experiment that allows us to causally link changes in AT, as a result of the TSP program, to corporate innovation.

We use the Securities and Exchange Commission's Market Information Data Analytics System (MIDAS) to identify AT trades. The data are available over the period 2012–2018 and to align the length of the pre-treatment period with the post-treatment period, we limit the analysis

[3] We are not the first to use TSP to study consequences of algorithmic trading. Lee and Watts (2021) use the TSP to study the relation between algorithmic trading and fundamental information acquisition and Bilinski, Karamanou, Kopita and Panayides (2021) to study the effect AT has on analyst research production.

to October 2014 to September 2016 as the pre-treatment period and October 2016 to September 2018 as the treatment period. We use six proxies for the trading activity of ATs: the odd lot ratio, which captures the fraction of volume associated with abnormally small trades that are more likely AT driven (O'Hara, Yao, and Ye, 2014), two trade-to-order ratios that are inversely related to the significant number of electronic order submissions ATs place as part of their 'slice and dice' algorithms (Hendershott, Jones, and Menkveld 2011), two cancel-to-trade ratios that are associated with the increased number of order cancellations by ATs stemming from their nearly instantaneously update of quotes (Hasbrouck and Saar, 2013), and the average trade size that is inversely related to AT activity as ATs split larger orders into smaller ones (Conrad, Wahal, and Xiang, 2015; O'Hara et al. 2014).

Following a well-established literature (Schmookler 1962, 1966; Sokoloff 1988; Jaffe and Trajtenberg 2002; Hall, Jaffe and Trajtenberg 2005; Moser and Voena 2012; Kogan, Papanikolaou, Seru and Stoffman 2017; Moser 2016), we measure innovation by the number of patents. Using patents compared to other measures of innovation has two main advantages. First, Autor, Dorn, Hanson, Pisano and Shu (2020, p.361) argue that 'the year in which a patent application is filed provides a reasonable proxy for the year in which an invention occurs', which allows us to capture the timing of the innovation effort. Second, patent citations provide an ex-post indication of the quality and impact of the innovation (Trajtenberg 1990; De Rassenfosse and Jaffe, 2018) that helps with cross-sectional identification. In robustness tests, we also examine the private economic value of patents using the Kogan et al. (2017) measure, and look at R&D spending as a broader measure of corporate innovation that is not dependent on successful outcomes of innovation resulting in a patent.[4]

Our analysis proceeds as follows. First, we confirm a statistically and economically significant reduction in AT for treated compared to controls stocks in our TSP sample. For

[4] The U.S. Patent and Trademark Office does not disclose information on unsuccessful patent applications. The rate of granted to applied patents is estimated to be between 97% (Quillen and Webster 2001) and 75% in Lemley and Sampat (2008).

example, treated firms exhibit a reduction in the two cancel-to-trade ratios of 31.1% and 37.9% in the post-TSP period, and an increase in trade size of 8.9%. These results validate that the TSP resulted in a significant decrease in AT activity for treated firms relative to control firms after the start of the TSP program and the economic magnitudes of the effect are consistent with Lee and Watts (2021).

Next, we present our main result on a positive causal relation between AT and innovation. This effect is economically significant as treated firms have on average 5.3% less patents relative to control stocks after the start of TSP (this effect is material considering that the intensity of AT for treated firms reduces by between 37.9% and 8.9%, depending on a measure, thus a complete ceases in AT activity would reduce treated firms' innovation by between 13.9% and 59.5%). The effect persists when we control for firm-fixed effects to account for time-invariant firm characteristics. The effect is stronger for treated firms with significant reductions in AT, but not for treated firms that despite a decrease in tick size, did not experience a reduction in AT.[5] This result helps us distinguish between the effect ATs have on innovation from changes in TSP-induced trading costs further strengthening our conclusions.[6]

We recognize that managers need time to understand the implications lower AT has on price efficiency and adjust firm innovation levels accordingly. Exploring the speed with which treated firms change their innovation activity, we find that the effect we document becomes significant approximately a year after the start of TSP.

Our conclusion on a positive effect ATs have on innovation is unchanged when we use R&D spending, a broader measure of corporate innovation (Baysinger, Kosnik and Turk 1991; Hill and Snell 1989; Scherer 1984; Barker and Mueller 2002). We document a significant

[5] Because we look at two years before and during the TSP, firm-fixed effects control for (i) managerial characteristics and (ii) the characteristics of managerial contracts that could affect innovation.
[6] Several studies document that treated firms experience an increase in quoted and effective spreads and a reduction in trading volume (Rindi and Werner 2019; Albuquerque, Song and Yao 2017; Chung, Lee and Rosch 2018; Lee and Watts 2021). Fang et al. (2014) document that lower stock liquidity, measured by higher spreads, increases innovation through changes in investor composition and takeover pressure. Lower liquidity of treated firms should then have a positive effect on innovation, which further excludes the liquidity channel affecting our conclusions.

reduction in R&D spending of 20.5% for treated firms relative to control firms after the start of TSP. Because R&D spending reflects the cost a firm incurred during the fiscal year, it helps us to confirm that ATs affect firm's innovative behavior rather than strategic timing of patent applications and disclosure.

Cross-sectional tests show that the positive effect of AT and innovation is stronger for stocks where the proportion of the CEO's stock-based compensation in total compensation is higher. This result is consistent with price efficiency being more important when a larger share of managerial compensation depends on the stock price performance (see also Fishman and Hagerty 1989). Further, the relation between AT and innovation is stronger for more opaque firms, as measured by higher accruals and lower financial reporting quality proxies. Low quality accounting numbers increase the relative usefulness of stocks prices compared to accounting information for assessing managerial effort (Kang and Liu 2008, Garvey and Swan 2002).

Next, we examine the novelty and economic significance of the patents as proxied by the number of citations (Harhoff, Narin, Scherer and Vopel 1999). This test helps us to address if our main result captures firms trading-off higher quality of innovation for less frequent patent applications. We document that treated firms experience a significant decrease in their citations and the effect is economically significant: treated firms have on average 50.4% fewer citations compared to control stocks after the start of TSP. We reach a similar conclusion when we use the Kogan et al. (2017) measures of economic value of patents.[7] In nominal terms, the average dollar value of a patent reduces by $0.494m for treated firms relative to control firms after the start of TSP.

To speak more directly to the prediction that ATs promote more innovation by reducing underinvestment, we also examine industry-adjusted innovation level. Consistently, we find a

[7] Kogan et al. (2017) measure looks at the stock market reactions to patent grants and is based on the intuition that stock prices are forward-looking and provide an estimate of the private value to the patent holder that is based on ex ante information. Kogan et al. (2017) report the measure is positively related to the scientific value of patents, growth, reallocation, and creative destruction.

significant reduction in the industry-adjusted number of patents. Finally, we follow Kogan et al. (2017) and examine price reactions to the U.S. Patent and Trademark Office patent grant disclosures announcements around the TSP event. We find (i) lower magnitudes of price reactions to patent grant announcements, (ii) relatively slower price discovery of patent disclosures, and (iii) lower abnormal returns for 60 days after patent disclosures and reversal afterwards for treated stocks relative to control firms after the start of TSP. These results support our argument that the AT effect on innovation is channeled through less efficient price discovery.

We perform several tests to exclude alternative explanations and confounding effects. First previous research documents that ATs can crowd out fundamental investors' information searches as fundamental traders cannot profitably trade on their private information (Weller 2018; Lee and Watts 2021; Bilinski et al. 2021). Thus, a decrease in AT could be associated with a higher ownership by fundamental traders, which in turn can affect firm innovation. To capture fundamental trading, we examine changes in total institutional ownership and in ownership by transient and in dedicated investors, but do not find significant evidence of changes in any of the ownership measures. Thus, changes in ownership composition do not explain our results. Second, He and Tian (2013) report that higher analyst coverage associates with lower innovation as it increases pressure on managers to meet short-term earnings expectations. To test whether our results capture the analyst coverage channel, we examine changes in analyst research activities for treated and control stocks, but find no evidence of changes in analyst coverage between the two groups nor changes in analyst forecast dispersion before earnings announcements that would suggest changes in the quality of the firm's information environment. Third, we argue that the results do not capture managerial myopic underinvestment to boost short-term profits. Such an explanation requires that AT reduction associates with amplified capital market pressures to boost reported earnings, which seems unlikely. Further, following Kraft, Vashishtha and Venkatachalam (2017), we also examine future return on assets to see if treated firms experience comparative increases in profitability that could be attributed to a myopic reduction in investment spending, but find no such evidence. Jointly, we find the alternative explanations are unlikely to be behind our evidence.

Our study contributes to the emerging literature on the real effects algorithmic trading has on capital markets. Stiglitz (2014, p. 9) asks that 'assuming that flash trading improved 'price discovery,' does the information produced lead to better resource allocations …?' He argues that '…real decisions, e.g., about how much to invest in a steel mill, are clearly unlikely to be affected by these variations in prices within a nanosecond. In that sense, they are fundamentally irrelevant for real resource allocations.' Our evidence suggests ATs have real impact on corporate innovation through their effect on price discovery.

Our finding on a positive relation between price discovery and patents complements the research stream that documents a positive relation between price informativeness and innovation (Fishman and Hagerty, 1989, Paul, 1992, Dow and Gorton, 1997, Luo, 2005, Chen et al., 2007, Dow et al., 2017, Singh and Yerramilli, 2014). Fishman and Hagerty (1989), Paul (1992), Dow and Gorton (1997), Luo (2005), Dow et al. (2011) and Singh and Yerramilli (2014) develop theoretical models linking price informativeness with managerial learning and innovation. Chen, Goldstein and Jian (2007) argue that higher price informativeness increases the sensitivity of investment to stock prices, consistent with managers learning from stock prices. Higher informativeness can also promote innovation through the discipling effect of prices (Amershi and Sunder 1987), such as through increased managerial turnover (Warner, Watts and Wruck 1987, Kaplan and Minton 2006, Jenter and Kanaan 2006). Our setting allows us to identify how price efficiency as opposed to price informativeness — the acquisition and incorporation of private information into prices— affects innovation. Weller (2018), Lee and Watts (2021) and Bilinski et al. (2021) document that AT order screening to avoid adverse selection and 'back-running' reduces the incentives for investors and analysts to acquire private information reducing price informativeness.[8] A positive correlation between AT and corporate investment identifies how price efficiency, as opposed to price informativeness and managerial learning, affect innovation.

Our research contributes novel evidence to the literature on the links between the key actors in financial markets and corporate innovation. He and Tian (2013) show that financial analysts exert pressure on managers to meet short-term goals and as a result, managers spend less on research and development for longer-term innovative outcomes. Institutional investors (Aghion, et al. 2013), foreign institutions (Luong, Moshirian, Nguyen, Tian and Zhang 2017), and hedge funds (Brav, Jiang, Ma, and Tian 2018) have a positive effect on innovation due to their expertise in improving innovation efficiency and their monitoring role. He and Tian (2019) document that short-sellers play a disciplinary role affecting the quality, efficiency and value of patents. Our study shows that ATs, who account for a significant portion of daily trading volume, significantly contribute to corporate innovation.

Finally, the study adds insights to the contracting literature that examines the structure and efficiency of managerial contracts (Narayanan 1985; Trueman 1986; Stein 1989; Bebchuk and Stole 1993; Bizjak, Brickley and Coles 1993). Severe studies document that noise in managerial performance measures reduces managers' incentive to exert effort (Murphy 2002; Core et al. 2003, Gerakos, Ittner, and Larcker 2007). Our evidence suggests that AT can alleviate the concern that noise in the stock price reduces the usefulness of contracts linked to the stock price performance.[9]

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[8] In other words, algorithmic investors trade quickly and fully on information that becomes a public domain (higher price efficiency), but their speed advantage discourages non-ATs from acquiring costly new information they cannot profitably trade on (see Weller 2018).

[9] Linking managerial compensation to share price performance assumes that managers cannot take actions leading to persistent overpricing, thus higher compensation. The evidence of active arbitrage (Shleifer and Vishny, 1997), disappearing anomalies (MacLean and Pontiff 2016), and improved asset pricing and research methods (Pástor–Stambaugh 2003; Novy-Marx 2013; Fama and French 2015; Ball, Gerakos, Linnainmaa and Nikolaev 2015; Hou, Xue and Zhang 2015; Harvey, Liu and Zhu 2016) provide little support for persistent overvaluation. The literature on managerial compensation assumes an average manager is risk averse (e.g., Amihud and Lev 1981; Guay 1999 and Core and Guay 2002), but risk neutral and risk prone managers are also likely to underinvest if their decision outcomes are not reflected in stock prices.

## 2 Literature review and hypotheses development

### 2.1 Algorithmic trading and innovation

Algorithmic trading has attracted significant attention from academics, regulators, market operators (e.g., the listing exchanges), practitioners, and the public in the last decade.[10] The literature has identified that the automation and speed advantage of ATs trading strategies improves stock liquidity, and reduces short-term volatility (Hendershott et al. 2011; Chordia, Roll and Subrahmanyam 2011; Hasbrouck and Saar 2013; Hagstromer and Norden 2013). ATs also improve price discovery through liquidity demand and liquidity supply functions (Brogaard, Hendershott, and Riordan 2019). As a result, we see reductions in return autocorrelations (Chaboud, Benjamin, Hjalmarsson and Vega 2014) and fewer arbitrage opportunities for non-AT investors to trade on (Conrad et al. 2015). Important for our setting, the literature documents that AT facilitates faster and more complete impounding into stock prices of information that is in the public domain. Bhattacharya, Chakrabarty and Wang (2020) and Chordia and Miao (2020) document stronger market reactions to earnings announcements for high AT firms, and Chakrabarty et al. (2021) report that AT facilitates price efficiency during low attention periods. Rogers, Skinner and Zechman (2017) and Hu, Pan and Wang (2017) report significant improvement in price efficiency to a range of public news announcements for high AT stocks.

We expect that the positive effect AT has on price efficiency facilitates quick and more complete impounding of public innovation signals into stock prices reducing underinvestment in innovation (Aghion et al. 2013). Managers care about quick and efficient impounding of information because their contracts are typically tied to stock prices in order for shareholders to solve agency problems (Ittner et al. 1997; Indjejikian and Nanda 2002; Core et al. 2003). This can

[10] The book Flash Boys by Michael Lewis (2014) became the #1 best seller by arguing that algorithmic trading firms use their speed advantage to make a profit at the expense of ordinary investors. AT advocates responded arguing that the book is a 'work of fiction'. This controversy resulted in significant publicity and numerous studies by academics, in addition to political and investment-side pressure on regulators.

be achieved—and managers would care about stock prices—only if prices are informative (Murphy 2001; Core et al. 2003; Gerakos et al. 2007). Therefore, a more efficient pricing through AT provides the necessary incentives for corporate managers to exert costly effort to improve the firm's fundamental value, since this will be reflected in the stock price, and innovation is one of the key drivers of corporate growth (Caballero and Jaffe 1993; Klette and Kortum 2004; Lentz and Mortensen 2008; Garcia-Macia, Hsieh and Klenow 2015). Our prediction also extends to the case of overinvestment, e.g., empire building (Jensen, 1986), as signals of poor investment outcomes, such as lower project return revealed at earnings announcements, quicker and fully impound into stock prices. In this case, AT discipline managers to select only projects that benefit shareholders reducing overinvestment.

### 2.2 Price informativeness and innovation

AT promoted price efficiency comes at the cost of lower price informativeness, which captures the amount of discoverable (private) information reflected into stocks prices. Korajczyk and Murphy (2019) document that ATs can identify and almost concurrently trade in the same direction—and at the expense—of informed institutions, reducing the latter's incentive to acquire costly private information. Weller (2018) reports that ATs reduce the amount of information reflected in the stock price before earnings announcements, and Lee and Watts (2021) report that AT discourages fundamental investors from acquiring costly private information, but improve price discovery at and after earnings announcements.

Several studies link price informativeness to innovation through the feedback they provide to managers on the projects investors considered value-increasing. The idea that prices are a useful source of information is not new. Hayek (1945) argues that information generation is decentralized. The stock market is an important source of information as prices aggregate diverse pieces of information revealed by trading. Consistent with the theoretical models (Fishman and Hagerty 1989; Paul 1992; Dow and Gorton 1997; Luo 2005; Dow et al. 2017 and Singh and

Yerramilli 2014), Chen et al. (2007) document that higher price informativeness increases the sensitivity of investments to stock prices, consistent with managers learning from stock prices. Other research linked managers learning from private information contained in stock prices at mergers and acquisitions (Betton, Eckbo, Thompson, and Thorburn 2014) and when deciding on corporate cash savings (Fresard 2010). In an international setting, Hsu, Tian and Xu (2014) show that better developed equity markets promote innovation not only by offering financing to firms, but also through information production. Li, Moshirian, Tian and Zhang (2016) identify that International Financial Reporting Standards (IFRS) adopters have higher innovation output, which they link to more informative IFRS disclosures. If ATs negative effect on price informativeness dominates their positive effect on price efficiency, we should observe a negative relation between AT and innovation. This leads to our main hypothesis:

*Hypothesis:* ATs have a positive effect on corporate innovation.

## 3. Research methods: The Tick Size Pilot program

To examine the causal effect AT has on corporate innovation, we use the Tick Size Pilot Program, a randomized controlled experiment that intended to examine the effect of the tick size increase on the market making and price discovery of small capitalization securities[11]. All eligible stocks included in the program have a market capitalization of less than $3 billion, an average closing price of at least $2, and an average trading volume of 1 million shares or less. The program introduced a widening of quoting and trading increments for 1,200 randomly selected securities, while 1,400 control securities continued to be traded in the normal quote of $0.01. The pilot was phased in during October 2016, lasted two years, and with its completion in October 2018, all treated stocks returned to their original trading tick size. Lee and Watts (2021) provide evidence that widening tick size increments significantly deters AT activities. We exploit

[11] Previous studies confirm that the randomized sampling resulted in similar pre-treatment covariates distributions between treated and controls stocks (see Lee and Watts, 2021 and Bilinski et al., 2021)

the increase in the tick size within the pilot program and use a difference-in-differences research design to understand how an exogenous reduction in AT, thus lower price efficiency, affects corporate innovation.

### 3.1 Measures of AT activity in a stock

ATs are characterized by a high daily trading volume and low latency of order submissions and cancellations. As in Weller (2018) and Lee and Watts (2021), we use six daily proxies that capture these characteristics. The odd lot volume ratio, *odd_lot,* is calculated as the total odd lot trade volume divided by total trade volume. The cancel-to-trade ratio, *cancel_ord* (*cancel_ord2*), is the count of all cancelled orders divided by the count of all trades based on displayed orders (total number of trades). A higher odd lot and cancel-to-trade ratio is associated with greater algorithmic trading activity. The trade-to-order ratio, *trade_vol* (*trade_vol2*) is calculated as the total trade volume based on displayed orders (total trade volume) divided by the total order volume. *Trade_size* is the average trade size defined as total trade volume times 1000 and scaled by total trades. A higher trade-to-order ratio and trade size is associated with less algorithmic trading activity. All six proxies are calculated as averages for each quarter of the two-year pre-TSP and the TSP period.

### 3.3 Innovation variables

Following extant literature (Hall, Jaffe, and Trajtenberg 2001; Hirshleifer, Low, and Teoh 2012; Atanassov 2013; Seru 2014; Sunder, Sunder and Zhang 2017), we construct three measures to capture the amount and quality of innovation. First, we use the total number of patent applications filed in a quarter that are eventually granted, *#patents*, as a proxy for a firm's innovation quantity. As in Griliches, Pakes, and Hall (1987) and Sunder et al. (2017), we use the patent application date to capture the timing of innovation as it more closely aligns with the time of actual innovation than patent grant date. In further tests, we also create an industry-adjusted measure of innovation similar to Ciftci, Lev and Radhakrishnan (2011), *adj #patents*, to capture relative underinvestment (or overinvestment) by a firm compared to the industry average.

To capture the patents' quality and their technological and economic importance, we count the total number citations and their economic value. *#citations* is the number of citations made to the granted patent as of December 31st 2019. Roach and Cohen (2013, p.504) argue that 'patent citations are the most widely employed measure of knowledge flows in the economics, management, and policy literatures.' A patent that receives more citations after the grant date is more likely to include technology that is valuable for subsequent innovation advances. Thus, citations capture originality and economic and scientific value of the patent (Trajtenberg, Jaffe and Henderson 1997; Hall, Jaffe, and Trajtenberg 2001). To speak directly to the economic value of patents, we use the Kogan et al. (2017) measure of the average stock market response to news about patents granted to a firm in a quarter-year. Kogan et al. (2017, p.669) argue the measure 'contains considerable information about firm growth in addition to what is contained in patent citations.' We measure the dollar value of granted patents both in inflation-adjusted values, *$rValue*, and in nominal terms, *$nValue*. Our use of these alternative measures of patent value also addresses the critique of patent citations as a measure of knowledge flows (Agrawal and Henderson 2002; Jaffe et al. 2002).

The number of patents and of citations measure innovation output for successful patent applications and when the firm decides to protect innovation through a patent. Griliches (1990) and Sunder et al. (2017) highlight that despite this limitation, there is no other widely available measure to better capture firms' technological advances, which explains the popularity of the patent measure in research. However, in additional tests, we also use research and development intensity, calculated as research and development expenditures scaled by sales, *R&D*. This measure captures a firm's investment in innovative activities and is broader to the number of patents and citations, as (i) not all R&D investments lead to patent granting, and (ii) only successful or significant innovation is patentable.

To speak to the causality of the relation between AT activity and corporate innovation, we employ a difference-in-differences research design using the randomized experiment of the Tick Size Pilot, and estimate the average treatment effect on corporate innovation in treated firms using the following model:

$$\begin{aligned} innovation_{i,q,t} \\ &= \gamma_0 + \gamma_1 Post_{q,t} + \gamma_2 Treatment_{i,q,t} \\ &+ \gamma_3 Post_{q,t} \times Treatment_{i,q,t} + Controls_{i,q,t} + \varepsilon_{i,q,t} \end{aligned} \tag{1}$$

where $innovation_{i,q,t}$ is the logarithm of one plus the measures of firm's patents described above, $Post_{q,t}$ is an indicator variable that takes the value one for all quarters *q* in year *t* in the TSP period from October 2016 to September 2018, and zero for all quarters *q* in the period from October 2014 to September 2016. $Treatment_{i,q,t}$ equals one if firm *i* belongs in the treatment group that experienced an increase in tick size, and zero otherwise. The main variable of interest that captures the incremental effect of the exogenous reduction in AT activity on a treatment firm's innovation activities is captured by the interaction term, $\gamma_3 Post_{q,t} \times Treatment_{i,q,t}$. To the extent that AT activity enhances the efficiency of stock prices in communicating innovation investments, we expect managers to underinvest in corporate innovation, in the absence of AT activity in the post-TSP period, and thus *γ3* should be negative. On the other hand, lower AT activity in treated firms may (i) reduce holdings by non-monitoring investors due to lower stock liquidity (Fang et al. 2014) and (ii) increase information acquisition by fundamental investors increasing stock informativeness (Weller 2018 and Lee and Watts 2021) and both effect will lead to positive *γ3*.

Lee and Watts (2021, p.379) highlight that a key advantage of the TSP is that it allows a researcher 'to estimate treatment effects with relatively few concerns for selection issues that would otherwise exist absent a randomized control sample' and that including controls can lead to a 'bad controls' problem (e.g., Angrist and Pischke 2009), if, for example, the controls are correlated with the tick size treatment.[12] These concerns motivated Lee and Watts (2021) to present results without controls. However, to build confidence in our results, we include a number of firm-level control variables that might have an impact on innovation and should be uncorrelated with the TSP. We control for firm size, growth opportunities and profitability using the natural logarithm of total assets (*Firm size*), the book-to-market ratio (*B/M*), and return on assets (*ROA*). To account for the effect of capital structure, we also include the leverage ratio (*Leverage*) and internally generated cash (*Cash/Assets*). We control for the institutional ownership using the percentage of institutional holding (*Institutional ownership*). To minimize the effect of extreme observations, we winsorize all continuous variables at the top and bottom 1% of each variable's distribution. All models include industry and quarter fixed effects, while standard errors are clustered at industry and quarter. In robustness tests, we also include firm-fixed effects to control for time-invariant firm characteristics.

## 4. Data

The list of securities included in the TSP is obtained from the FINRA website. Following Weller (2018) and Rindi and Werner (2019), we exclude preferred stocks, stocks dropped due to mergers, delistings or with prices below $1, or stocks that changed TSP group during our sample period, which leaves 1,970 firms (987 treated and 983 control firms). We construct our AT activity proxies using daily order book information across all major U.S. stock exchanges from the SEC Market Information Data Analytics System (MIDAS). We obtain patent-level data from the U.S. Patent and Trademark Office (USPTO) database, which we match to the TSP sample.[13] Similar to Kogan et al. (2017), for our main tests, we only keep firms with at least one

[12] Lee and Watts (2021, p.379) highlight that 'while controlling for liquidity or institutional ownership might seem sensible, these variables themselves can be affected by the tick size treatment (e.g., Rindi and Werner 2019; Albuquerque et al. 2020)' and that 'the securities in this pilot study are smaller firms by design, and data availability can be an issue when a large set of control variables is added.' They show that differences between key firm characteristics, such as market capitalization of treatment and control firms before the start of the program are not statistically significant. Albuquerque et al. (2020) in their Table 2 find no significant differences in returns, size, market-to-book ratio and various liquidity measures between treatment and control stocks before TSP.

[13] See Graham, Hancock, Marco and Myers (2020) for a description of the U.S. Patent and Trademark Office data.

patent at any point over the period October 2014 to September 2018, which covers our pre- and TSP period. We focus on firms with patents as Hausman, Hall, and Griliches (1984) caution against using samples with excessive firm-years with zero patent counts. However, robustness tests show our conclusions are unchanged when we assign zero to firms with no patent information (Fang et al. 2014). We use Compustat to calculate fundamental ratios for control variables and collect institutional ownership data from 13F filings. The final sample includes 3,954 firm-quarter-years.

## 5. Results

Table 1 presents descriptive statistics (Panel A) for the six AT measures and the Pearson correlations (Panel B). The AT measures exhibit comparable values to those in previous research, alleviating the concern that the distribution of AT measures may be affected by a non-random sample selection process. Specifically, the mean (median) value of the average trade size in Lee and Watts (2021) is 95.09 (85.51) and similar to our sample mean (median) of 97.986 (89.191). Similarly, the mean (median) value of the odd lot ratio in Lee and Watts (2021) is 0.192 (0.163) that is close to the respective value of 0.166 (0.159) for our sample. Lee and Watts (2021) report a mean value of 0.0359 (28.33) for the trade to order (cancel to trade) ratio that falls between our two measures of trade to order (cancel to trade) 0.033 and 0.040 (26.358 and 35.589). Finally, the correlations between the six AT proxies as presented in Panel B are significant, suggesting that the measures capture a similar, but not identical, underlying economic construct.

[Table 1]

### 5.1 Changes in AT for treated stocks after the start of TSP

Because our sample does not include all firms in the original TSP, we first examine whether the documented reduction in AT activity following the TSP is present for the treatment relative to control firms in our sample (see Cox, Van Ness, and Van Ness 2019; Chung, Lee and Rösch

2020 and Lee and Watts 2021 for similar tests). For this analysis, we use the difference-in-differences panel regression framework similar to that depicted in Eq. (1) by regressing each of the six AT measures on *Post*, *Treatment* and their interaction. Table 2 results indicate a significant reduction in AT activity for treated relative to control firms after the introduction of the program as evidenced by significant coefficients on the interaction term *Post* × *Treatment*. The reduction in AT activity is economically significant as well. For example, treated firms exhibit a reduction in the two cancel to trade ratios of 31.1% and 37.9% in the post-TSP period and an increase in trade size of 8.9% consistent with a significant decrease in AT activity relative to control firms after the start of the TSP program.

[Table 2]

**5.2 Descriptive statistics for firm's patents**

In Panel A of Table 3, we present descriptive statistics for our dependent variables: the number of patents filed and the number of citations, and in panel B for the control variables used in the regression analyses. Panel A suggests that during our period sample firms obtained on average 3.552 patents per year-quarter with an average of 5.076 citations. Given that our sample comprises of smaller firms, it is not surprising that our proxies for corporate innovation are smaller, yet comparable, to those reported in related research. For example, Kim, Park and Song (2019) report an average number of patents of 5.447 per year for their sample of firms with non-missing patent information over 1980–2004.

In panel B of Table 3, we present descriptive statistics for the control variables used in the analyses. Our sample firms have an average market value of $1,155 million, which reflects that the SEC only considered firms with market capitalization of less than $3 billion for the TSP experiment.

[Table 3]

In Panel A of Table 4, we examine whether there are significant differences in the mean values of the dependent variables between our treatment and control samples in the pre-TSP period. We do not find any significant differences in the means of the two groups, a result that is consistent with the random allocation of stocks to treated and control groups of the pilot program.

Panel B evaluates the presence of pre-existing trends following the approach from Donelson, McInnis and Mergenthaler (2016) and Ahmed et al. (2020). Specifically, we include pre-TSP period indicators in Eq. (1) and their interactions with the treatment firm indicator to allow innovation to vary between treatment and control groups. Specifically, *Pre_Sept2015* is an indicator variable for the pre-treatment period between March 2015 and September 2015. *Pre_March2016* is an indicator for the pre-treatment period between October 2015 and March 2016, and *Pre_Sept2016* for the pre-treatment period between April 2016 and September 2016. The intercept captures the pre-TSP period between October 2014 (i.e., the start of our sample period) and February 2015. Panel B of Table 4 shows that none of the interaction terms between pre-TSP period indicators and the treatment dummy is significant, which suggests no significant differential trend for treated firms before TSP.[14] This result is consistent with the parallel trend assumption and further supports the supposition that the random assignment of the TSP program did not result in selectivity bias on firm innovation activities.[15]

[Table 4]

### 5.3 Regression results for the relation between AT and innovation

Table 5 examines the effect of TSP on the number of patents as described in Eq.(1). The results provide consistent and strong evidence that the decreased AT activity in treated compared to control firms following the introduction of the TSP program resulted in a significant decrease in

[14] Including pre-treatment period indicators changes the interpretation of the coefficient on the interaction Post×Treatment, which now captures the differential effect relative to pre-TSP period between October 2014 and February 2015 captured by the intercept. The true 'difference-in-differences' comparison as specified in Eq. (1) is presented in the next section.
[15] The evidence that investments are similar between treated and control firms also reduces the likelihood that our results capture a correction in previous excess investments of managers among treated firms. Such a correlation would require non-random assignment between treated and control firms, which the TSP natural experiment avoids.

the number of patents, suggesting that AT activity is positively associated with corporate innovation.[16] The economic magnitude of the effect is around 5.3% (considering that the intensity of AT for treated firms reported in Table 2 reduces by between 37.9% and 8.9%, depending on a measure, the associated reduction in treated firms' innovation is between 13.9% and 59.5%).[17]

[Table 5]

Column 'Zero for missing patent data' in Table 5 shows that our conclusions are unchanged when we assign zero to firms without patent data. For this model, we augment Eq. (1) with an indicator for missing patent observations, *Missing_PatentD*. The sample size increases to 23,035 observations and the coefficient on the interaction term *Post*×*Treatment* remains significantly negative.[18] Thus, assuming missing values reflect no innovation activity produces similar conclusion to our main tests.

The random assignment into treated and control firms alleviates the concern the pilot program is correlated with firm characteristics leading to omitted correlated variable problem (Lee and Watts, 2021). However, we also repeat the regression after including firm-fixed effect in Eq. (1). Columns 'Firm-fixed effects' document that our conclusions remain unchanged for this analysis both for the sample with non-missing patent data and for the sample assuming zero for missing patent information.

Our hypothesis is centered on AT alleviating underinvestment in innovation by increasing price efficiency. To speak more directly to the *underinvestment* explanation, we estimate Eq. (1) where the dependent variable is the industry-adjusted level of innovation. Specifically, each year-quarter we calculated the mean patent count for the Fama-French industry the firm belongs to, which we then subtract from the firm-year patent count. We then use the industry-

[16] The results are the same when we use unlogged patent counts as the dependent variable.

[17] We calculate this value by dividing the coefficient on *Post*×*Treated* by the average range reduction in AT activity in treated stocks from Table 2, i.e., $\frac{5.3\%}{8.9\%}$ and $\frac{5.3\%}{37.9\%}$.

[18] Large-sample studies document that between 84% (Atanassov, Nanda, and Seru 2007) and 73% (Tian and Wang 2014) of Compustat firms have missing patent data between 1974-2006.

adjusted patent count as the dependent variable in Eq. (1). We continue to find a significant negative coefficient on the interaction term *Post×Treatment*, consistent with AT alleviating underinvestment in innovation relative to the firm's industry peers.

The last columns of Table 5 use the R&D spending, measured as the ratio of research and development expenditures for the most recent fiscal quarter scaled by total sales, for our sample firms as the dependent variable in Eq. (1). Although R&D does not capture the quality of innovation or the success of the innovation process, it does reflect the intensity with which firms pursue innovation and is often used as an innovation measure (Hausman et al. 1984; Becker-Blease 2011). Regression results show a significant reduction in R&D spending for treated firms relative to controls after the start of TSP in line with our main results.[19] The economic effect is comparable with our main results showing a 4.9% reduction in R&D spending. Overall, we find consistent evidence that a reduction in AT leads to a reduction in the level of corporate innovation.

In untabulated results, we also run a placebo test where we select the same treatment and control stocks and define the pre-treatment period from January 2012 to September 2014 and the pseudo-treatment period from October 2014 to December 2016. We then run equation (1) for this sample and find insignificant coefficients on the interaction term. Thus, the AT effect is not present absent the TSP program.

#### 5.3.1 Distinguishing between the liquidity and AT effect on innovation

Lee and Watts (2021) report a reduction in effective spreads and trading volume for treated firms and the liquidity reduction may affect stock innovation. To distinguish between the liquidity and AT channels, we follow Ahmed et al. (2020) and identify the direction of the change in AT for treated firms after the start of the pilot program, which we then interact with the treatment

[19] In untabulated results, we continue to find a negative coefficient on the interaction term *Post×Treatment* for the full TSP sample assuming zero for missing R&D values.

period indicator. This approach splits the interaction term *Post×Treatment* into two variables: *Post×decrease in AT* and *Post×zero or increase in AT*, where variables *decrease in AT* and *zero or increase in AT* are indicator variables for a directional change in AT. As in Ahmed et al. (2020), we use the odd_lot ratio and the trade volume AT measures that have a positive association with AT to capture directional changes in AT. Further, we use principal component analysis to create an index, *PCA AT*, based on the six AT measures to identify an average increase or reduction in AT in the treatment compared to the pre-treatment period.[20]

Panel B of Table 5 confirms that the reduction in innovation comes from treated firms that also experience a reduction in AT. The coefficients on *Post×decrease in AT* are significantly negative for all measures of AT and over two times larger in magnitude than the coefficient on *Post×Treatment* in Panel A, which suggests that the *Post×Treatment* interaction captures the effect of AT with noise. The insignificant coefficient on *Post×zero or increase in AT* is consistent with Eaton, Irvine and Liu (2021) and Dass, Nanda, and Xiao (2017) that liquidity has no association with innovation as measured by patents.[21] Thus, the liquidity channel cannot explain our results.

#### 5.3.2 The speed with which managers adjust innovation in response to TSP

We recognize that managers need time to understand the implications lower AT has on price efficiency and adjust firm innovation levels accordingly. This section examines the speed with which treatment firms adjust their innovation activities. For this test, we split the TSP period into three subperiods. *Post_May2017* is an indicator variable for the early part of the post-

[20] The weights in the index are −0.664 for *odd lot*, 0.914 for *trade_vol*, 0.896 for *trade_vol2*, −0.630 for *cancel_ord*, −0.650 for *cancel_ord2* and 0.546 for *trade size*.

[21] We recognize that Fang et al. (2014) document that higher liquidity reduces innovation, thus if our results captured the liquidity channel, we should observe an increase in innovation in treated firms. Fang et al. (2014) argue their results capture higher risk of hostile takeovers and of exit by institutional investors dissatisfied with poor firm performance. However, Eaton et al. (2021, p.836) argue that '[T]he importance of the former reason is debatable due to the greatly decreased frequency of hostile takeovers since the late 1980s.', further, they find that using a price impact measure to capture institutional trading costs. Jointly with the evidence in Dass et al. (2017) that liquidity has no impact on innovation when using more recent patent data and in Eaton et al (2021) that using price impact measure of liquidity shows no association with innovation, the negative link between liquidity and innovation is unclear.

treatment period that is between October 2016 and May 2017. *Post_ Dec2017* captures the period between June 2017 and December 2017 and *Post_Sept2018* the period between January 2018 and the end of the TSP program. We then interact the three subperiod indicators with the treatment dummy, which compares the innovation activities of treated firms with that of control firms in each subperiod.

Panel C of Table 5 reports regression results when we include the subperiod indicators and their interactions in Eq. (1). We also report results for Eq. (1) that includes firm-fixed effects. The coefficient on the interaction term between the treatment dummy and the indicator for the early months of the TSP period, *Post_May2017×Treatm*ent, is insignificant in both regressions. Moving to the later sub-periods, the coefficient on *Post_Dec2017×Treatment* is –0.053 and on *Post_Sept2018×Treatment* is –0.091, which suggests that the effect we document becomes significant in the later periods of the TSP (the F-test reported in the bottom rows of Panel C confirms that the coefficient on *Post_Sept2018×Treatment* is significantly different from the coefficient on *Post_June2017×Treatment*).

### 5.3.3 Quality of innovation

Next, we turn to the measures of economic significance of the patents captured by the number of citations (Harhoff et al. 1999). This test helps us differentiate whether managers trade-off lower number of patent applications for a relatively higher quality of patents or whether both the count and quality of innovation reduces. The first columns of Table 6 document that the number of citations reduce.[22] The economic effect is significant with the citations reducing by

[22] We collect the patent data in 2021, which alleviates the concern that the average two-year lag between a patent's application date and the grant date leads to patents data under review missing from the dataset (Hall et al. 2001). The second truncation problem is that citations accumulate over long period of time and more recent patents will by construction have fewer citations. Our difference in differences design adjusts for this effect as controls firm would suffer from a similar bias.

50.4% $\left(\frac{11.8\%}{1/2(8.9\%+37.9\%)}\right)$. Jointly with Table 5 results, this evidence suggests the scientific significance of patents reduces as prices become less efficient.

[Table 6]

Next, we use the real and nominal measures of private economic value of patents from Kogan et al. (2017) as dependent variables in Eq. (1). We document a significant reduction in the economic value of patents for treated firms relative to controls stocks after the start of TSP. In nominal terms, the average dollar value of a patent reduces by $0.494m for treated firms relative to control firms after the start of TSP. Jointly, Table 6 results suggest that the scientific and economic value of patents reduces as price efficiency decreases. In untabulated results, we find that our conclusions from Table 6 are unchanged when we scale raw citation counts and the real and nominal measures of private economic value by the number of citations.

### 5.3.4 Cross-sectional tests

Managers will care more about stock prices reflecting their effort related to innovation if their compensation is more closely tied to the stock price performance (Lewellen, Loderer, and Martin 1987; Smith and Watts 1992; Gaver and Gaver 1993; and Bushman, Indjejikian, and Smith 1996). Thus, the effect of AT on innovation should be more pronounced when a larger portion of managerial compensation is stock based. Following previous studies, e.g. Cheng (2004), we measure CEO's fraction of share-price dependent compensation as the ratio of the sum of stock awards and stock options and restricted stock holdings and grants to total compensation, *% stock compensation*, which we then interact with the indicators for treatment, the TSP period, and their interaction.[23] Table 7 reports a negative coefficient on the triple interaction term

[23] In untabulated results, we find no significant differences in mean CEO equity compensation between control and treated stocks before and during TSP.

*Post×Treatment×% stock compensation*, which is consistent with the effect of ATs on innovation being incrementally more important when a larger share of CEO's compensation is stock based.

[Table 7]

The compensation committee and investors will put more weight on the stock price performance to assess managerial effort when effort in affecting firm value cannot be measured clearly based on noisy accounting information (Holmstrom 1979; Banker and Datar 1989; Milgrom and Roberts 1992; Feltham and Xie 1994; Yermack 1995). Following the literature, we use total accruals as a measure of low earnings quality (Dechow, Ge and Schrand 2010), which is defined as the difference between cash flow from operating activities and net income before extraordinary items, *Accruals*. We then interact *Accruals* with the indicators for *Post*, *Treatment* and their interaction. The coefficient on the triple interaction term is negative consistent with a stronger link between AT and innovation when accounting numbers provide noisy signals of managerial effort in creating shareholder value.

Accruals remain a contentious measure of earnings quality (DeFond 2010), which is why we also use a composite measure of low reporting quality, *PCA EA factor*, based on a principal component analysis of audit fees, a dummy variable for restatement, accruals, Big4 auditor indicator and an indicator for whether the auditor is PCAOB registrant. The last columns of Table 7 confirm that AT have incrementally stronger effect on innovation for treated firms relative to control stocks after the start of TSP and with high values of *PCA EA* factor.

#### 5.3.5 Price discovery around patent grants announcements

Our argument on the positive relation between AT and innovation is based on the premise that AT increases the efficiency of price reactions to public signals about corporate innovation eliminating underreaction to patent news (Deng et al. 1999 and Gu 2005). This prediction is based on previous evidence that ATs trade faster and impound public information more completely around earnings announcements (Bhattacharya et al. 2020, Chordia and Miao 2020, Frino, Prodromou, Wang, Westerholm and Zheng, 2017 and Lee and Watts, 2021), EDGAR filings (Rogers et al. 2017) and analyst forecast announcements (Bilinski et al. 2021). Our next test validates this proposition for public announcements of patent grants by the U.S. Patent Office. Kogan et al. (2017, p.673) describe that 'The USPTO issues patents on Tuesdays, unless there is a federal holiday. The USPTO's publication, Official Gazette, also published every Tuesday, lists patents that are issued that day along with the details of the patent.'[24] We measure the abnormal return around the patent grant announcement, $AR(0)$, which we then use as the dependent variable in Eq. (1). The normal return benchmark is the Carhart (1997) model estimated over 100 days before the patent grant date. Table 8 documents lower price reactions to patent grant announcements dates for treated firms relative to control stocks after the start of TSP, consistent with a lower efficiency with which public information about patents impounds into stock prices.[25]

To speak about the speed of price discovery, in the spirit of Weller (2018), we create a ratio of the price reaction on the report announcement day to the total signal content measured in a three-day window centered on the patent grant disclosure day. Higher values of the ratio suggest that most of the signal content impounds on the announcement day, while lower value suggest more of the signal content is impounded outside the announcement day. We find that price discovery happens outside the announcement day for treated firms relative to controls after TSP.[26] This result is consistent with lower AT associating with less efficient impounding of public innovation signals into stock prices.

To understand if price inefficiency (i.e., underreaction) persists after the patent disclosure, we also examine post-grant date abnormal returns over various windows. We

[24] Kogan et al. (2017) do not find evidence of significant price reactions around patent application filing dates and argue this reflects that the USPTO does not publish applications at the time they are filed.
[25] Our conclusions from Table 8 are unchanged when we control for the economic significance of the patent using citation counts.
[26] The conclusions are similar when we measure the total signal content over the period from the announcement day to five days after the announcement.

continue to find a negative coefficient on the interaction term *Post×Treatment* from one to 60 days after the grant date. This result is consistent with comparatively lower return of treated firms relative to controls for about two months after the grant date.[27] The positive coefficient on *Post×Treatment* in the window from 61 to 100 days after the patent grant date suggests the initial underreaction for treated stocks corrects and the evidence of abnormal performance in window 101 to 140 days after the patent grant date. Jointly, Table 8 results are consistent with lower efficiency with which prices impound patent information for treated firms after TSP.

### 5.4 Alternative explanations

This section presents tests that help rule out alternative explanations. First, we show our results are not driven by changes in institutional ownership and institutional ownership composition in treated firms. Second, we show the results are not due to changes in the quality of the firm's information environment as captured by changes in analyst coverage and forecast dispersion. Third, we address whether managerial myopia and underinvestment to boost short-term profitability can explain our findings.

#### 5.4.1 Changes in institutional ownership

Previous research has documented a positive relation between institutional ownership and corporate innovation (Kochhar and David 1996; Aghion et al. 2013). Thus, it is possible that the decreased levels of innovation we document for the treated firms in the TSP period may in fact be the result of a confounding decrease in their institutional ownership and not the result of AT reduction. This is an unlikely scenario as (i) AT reduces the profitability of institutional

[27] Our results are consistent with Chordia and Miao (2020) who report that more intensive AT reduces the post-earnings announcement drift.

investors' trades and as a result, institutional ownership in stocks with low AT activity tends to be higher (Bilinski et al. 2021) and (ii) we control for institutional holdings, which should capture this channel. Nevertheless, to ensure that our results are not confounded by changes in institutional ownership, Table 9 examines whether treated firms exhibit relatively lower levels of institutional ownership in the TSP period.

The evidence presented in the first column of Table 9 suggests that treated firms do not exhibit changes in their overall level of institutional ownership between the pre- and TSP periods and relative to control stocks. Under the premise that the positive effect of institutions on innovation should be mostly related to increases in ownership by institutions with long horizons, we classify institutional investors based on their investment type (Bushee 1998). We find no evidence of changes in transient ownership and evidence of an increase in dedicated ownership for treated firms relative to controls after TSP. As dedicated ownership associates with an increase in innovation (Aghion et al. 2013), it cannot explain why innovation reduces for treated firms after the start of TSP. We conclude that the documented decrease in innovation for treated firms is not consistent with changes in institutional ownership providing further support to our conclusion that decreased AT activity in treated firms has a negative impact on firm innovation.

[Table 9]

### 5.4.3 Changes in information environment

Ahmed et al. (2020, p.869) argue using the TSP sample, that the tick size 'increases the scrutiny of managers' financial reporting choices and reduces their incentives to engage in misreporting' and report 'a significant decrease in the magnitude of discretionary accruals, a significant reduction in the likelihood of just meeting or beating analysts' forecasts, and a marginally significant decrease in restatements for the treated firms in the pilot program.' Biddle and Hilary (2006, p.963) report that higher accounting quality promotes more innovation 'by reducing information asymmetry between managers and outside suppliers of capital.' Park (2018, p.874) also reports a positive relation between financial reporting quality and corporate innovation as it 'helps investment decision makers identify value-enhancing opportunities with fewer errors' and promotes internal collaboration. Thus, our results are unlikely to capture the documented improvements in reporting quality of treated firms after the start of the pilot program since those would result in increased innovation. In fact, our results show that AT and increased price efficiency have a stronger effect on innovation since it prevails in environments of firm opaqueness.

To further examine the link between the quality of the firm's information environment and innovation, we also look at potential changes in analyst coverage for treated firms. He and Tian (2013, p.856) report that 'firms covered by a larger number of analysts generate fewer patents and patents with lower impact', however, Dass et al. (2017) show that using a more recent patent data shows no association between analyst coverage and patent counts. Table 10 reports Eq.(1) results where the dependent variable is the number of analysts covering the stock. We find no evidence of changes in analyst coverage for treated firms. Further, we look at analyst forecast dispersion, which is a common measure of information environment quality (Lang and Lundholm 1996; Barron, Byard and Kim 2002). We calculate forecast dispersion based on analyst's last EPS forecast issued before quarterly earnings announcements, which we then use as a dependent variable in Eq.(1). The last columns of Table 10 show no evidence of change in forecast dispersion. Jointly, the tests make it very unlikely that changes in the firm's information environment explain our results.

[Table 10]

**5.4.4 Managerial myopia**

The investment decline for treated firms that we observe could reflect myopic underinvestment by managers to temporarily boost corporate performance. We believe this explanation is unlikely as we see no reason why a reduction in AT should associate with amplified capital market pressures to boost reported earnings. Further, Table 9 evidence on an increase in dedicated ownership and Table 10 evidence on no changes in analyst coverage are also inconsistent with increased market discipline. However, to further rule out the possibility that declines in AT associate with an increase in myopic underinvestment, we follow Kraft et al. (2017) and examine changes in one-year-ahead return on assets for treated relative to control stocks. We find no evidence that treated firms have better future ROA compared to controls after TSP. The result is the same looking at two-year-ahead ROA. Thus, we do not find any evidence the results could capture managers reducing investments to boost short-term profits.

## 6. Conclusions

We use the Tick Size Pilot natural experiment to examine the causal impact of algorithmic trading on innovation. We document a positive and economically significant relation between AT and the number of patents and their economic and scientific significance. We argue that these results reflect that AT improves the efficiency with which prices reflect patent information, which we validate by showing lower price reactions and less efficient price discovery around patent grant disclosures. The study highlights an important real effect AT has on corporate decision making.

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## Appendix A. Definitions of variables used in the study

| Variable name | Variable description |
|---|---|
| **Panel A: Dependent and main independent variables** | |
| Number of patents | The total number of patents a company applied for in a quarter that were ultimately granted |
| Number of citations | Total number of citations the granted patent made counted up till December 31st, 2019. |
| Industry-adjusted number of patents | The number of firm patents less the mean patent count for the Fama-French industry the firm belongs to calculated for each year-quarter. |
| R&D | the ratio of research and development expenditures for the previous quarter scaled by total sales calculated for each firm-year-quarter. |
| *$nValue* | Kogan et al. (2017) value of innovation in millions of nominal dollars calculated for each firm-year-quarter. |
| *$rValue* | Kogan et al. (2017) value of innovation in millions of dollars deflated to 1982 using the CPI calculated for each firm-year-quarter. |
| Treatment | An indicator variable for a firm in the treatment group that experienced an increase in tick size. |
| Post | An indicator variable for the post-treatment period that is between October 2016 and September 2018. |
| Post_May2017 | An indicator variable for the early part of the post-treatment period that is between October 2016 and May 2017. |
| Post_ Dec2017 | An indicator variable for the middle part of the post-treatment period that is between June 2017 and December 2017. |
| Post_Sept2018 | An indicator variable for the late part of the post-treatment period that is between January 2018 and September 2018. |
| Pre_Sept2015 | An indicator variable for the pre-treatment period that is between March 2015 and September 2015. |
| Pre_March2016 | An indicator variable for the pre-treatment period that is between April 2015 and March 2016. |
| Pre_Sept2016 | An indicator variable for the pre-treatment period that is between April 2016 and September 2016. |
| **Panel B: AT measures** | |
| odd_lot | Quarterly average odd lo to volume ratio defined as total odd lot volume to total trade volume, calculated per firm |
| cancel_ord | Quarterly average cancelled to trades ratio, defined as the ratio of total cancel orders to the total number of displayed orders, calculated per firm |
| cancel_ord2 | Quarterly average cancelled order to the total number of trades defined as the total number of cancelled orders to total number of trades, calculated per firm |
| trade_vol | Quarterly average total trading volume ratio calculated as the total displayed trading volume to the order volume, calculated per firm |
| trade_vol2 | Quarterly average total trading volume ratio per displayed order defined as the total trading volume divided by total number of trades, calculated per firm |
| trade_size | Quarterly average trade size defined as total trade volume times 1000 and scaled by total trades, calculated per firm |

*Continued on next page*

Appendix A, *continued*

| Variable name | Variable description |
|---|---|
| **Panel C: Controls and other measures** | |
| Firm size | Firm size calculated as the log of total assets for the most recent fiscal quarter. |
| ROA | Return on assets calculated as the ratio of net income over total assets for the most recent fiscal quarter. |
| Leverage | Leverage calculated as the ratio of long-term debt over total assets for the most recent fiscal year. |
| Cash/Assets | Firm liquidity calculated as the sum of income before extraordinary items and depreciation and amortization scaled by total assets calculated for the most recent fiscal year. |
| B/M | The book-to-market ratio calculated as the ratio of common equity scaled by total market capitalization for the most recent fiscal quarter. |
| Missing_Patent_D | An indicator variable equal to 1 if the patent data is missing for a stock and zero otherwise. |
| Institutional ownership | Percentage institutional ownership in a stock. |
| Transient | Transient institutional ownership using the classification from Bushee (1998). |
| Dedicated | Dedicated institutional ownership using the classification from Bushee (1998). |
| % stock compensation | The ratio of stock-based to total compensation. Stock-based compensation is the sum of value of stock awards, restricted stock holdings, grant date fair value of options granted, and restricted stock grant. total compensation equals to salary + bonus + other annual + restricted stock grants + LTIP payouts + all other + value of option grants. |
| Accruals | The ratio of total accruals to average assets. Accruals are calculated as net cash flow from operating activities less net income before extraordinary activities. |
| PCA EA factor | Principal component analysis of audit fees, a dummy variable for restatement, accruals, Big4 auditor indicator and an indicator for whether the auditor is PCAOB registrant. |
| Number of analysts | The number of analysts who issued at least one EPS forecasts for the firms in the previous quarter. |
| Dispersion | The dispersion in the analyst EPS forecasts issued before firm's quarterly earnings announcements. We keep only the latest EPS forecast issued for a firm. |
| AR(0) | The market-adjusted abnormal return on the patent grant disclosure day. |
| AR(0)/CAR(-1,1) | The ratio of the patent grant announcement date price reaction to the cumulative abnormal return measured from one day before to one day after the announcement. |
| Quarter effect | Quarter-fixed effects |
| Industry effect | Industry fixed-effects based on Fama-French industry definitions. |

## Table 1. Descriptive statistics for AT measures

| | Mean | Median | Std Dev | Lower Quartile | Upper Quartile |
|---|---|---|---|---|---|
| **Panel A. Descriptive statistics for AT measures** | | | | | |
| odd lot | 0.166 | 0.159 | 0.080 | 0.110 | 0.210 |
| trade_vol | 0.033 | 0.031 | 0.017 | 0.020 | 0.043 |
| trade_vol2 | 0.040 | 0.038 | 0.020 | 0.026 | 0.052 |
| cancel_ord | 35.589 | 24.219 | 45.625 | 16.908 | 37.371 |
| cancel_ord2 | 26.358 | 20.353 | 25.430 | 14.494 | 29.618 |
| trade size | 97.986 | 89.191 | 35.814 | 77.435 | 108.150 |

| | odd lot | trade_vol | trade_vol2 | cancel_ord | cancel_ord2 |
|---|---|---|---|---|---|
| **Panel B. Pearson correlations between AT measures** | | | | | |
| trade_vol | −0.492 | | | | |
| | 0.000 | | | | |
| trade_vol2 | −0.472 | 0.976 | | | |
| | 0.000 | 0.000 | | | |
| cancel_ord | 0.165 | −0.416 | −0.365 | | |
| | 0.000 | 0.000 | 0.000 | | |
| cancel_ord2 | 0.124 | −0.468 | −0.429 | 0.927 | |
| | 0.000 | 0.000 | 0.000 | 0.000 | |
| trade size | −0.734 | 0.438 | 0.464 | 0.070 | 0.093 |
| | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |

The table reports descriptive statistics for the algorithmic trading measures (Panel A) and their Pearson correlations (Panel B). Variables definitions are in Appendix A.

**Table 2. Changes in AT measures for the sample of treated and control stocks**

| | odd_lot | | trade_vol | | trade_vol2 | | cancel_ord | | cancel_ord2 | | trade size | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Coeff | p-value | Coeff | p-value | Coeff | p-value | Coeff | p-value | Coeff | p-value | Coeff | p-value |
| Predicted sign | − | | − | | − | | + | | + | | + | |
| Intercept | −2.651 | 0.000 | −3.384 | 0.000 | −3.019 | 0.000 | 4.218 | 0.000 | 3.790 | 0.000 | 5.158 | 0.000 |
| Post | 0.277 | 0.000 | 0.272 | 0.000 | 0.256 | 0.000 | −0.303 | 0.000 | −0.302 | 0.000 | −0.127 | 0.000 |
| Treatment | −0.004 | 0.866 | 0.000 | 0.997 | −0.008 | 0.699 | 0.023 | 0.235 | 0.024 | 0.162 | −0.003 | 0.809 |
| Post×Treatment | −0.137 | 0.000 | 0.270 | 0.000 | 0.212 | 0.000 | −0.379 | 0.000 | −0.311 | 0.000 | 0.089 | 0.000 |
| Controls | Yes | | Yes | | Yes | | Yes | | Yes | | Yes | |
| Quarter effects | Yes | | Yes | | Yes | | Yes | | Yes | | Yes | |
| Industry effects | Yes | | Yes | | Yes | | Yes | | Yes | | Yes | |
| N | 3954 | | 3954 | | 3954 | | 3954 | | 3954 | | 3954 | |
| R2 | 33.03% | | 23.26% | | 23.89% | | 30.09% | | 30.37% | | 45.01% | |

The table reports Eq.(1) results where the dependent variable is the measure of algorithmic trading.

## Table 3. Descriptive statistics for patent regression variables

| | Mean | Median | Std Dev | Lower Quartile | Upper Quartile |
|---|---|---|---|---|---|
| **Panel A. Dependent variables** | | | | | |
| Number of patents | 3.552 | 2.000 | 4.964 | 1.000 | 4.000 |
| Number of citations | 5.076 | 1.000 | 13.242 | 0.000 | 3.000 |
| **Panel B. Controls** | | | | | |
| MV | 1155.8 | 835.8 | 1160.7 | 292.3 | 1679.8 |
| ROA | −0.024 | 0.003 | 0.076 | −0.042 | 0.017 |
| Leverage | 0.433 | 0.412 | 0.272 | 0.208 | 0.580 |
| Cash | −0.015 | 0.012 | 0.076 | −0.032 | 0.026 |
| B/M | 0.416 | 0.358 | 0.364 | 0.206 | 0.570 |
| IO | 0.708 | 0.782 | 0.286 | 0.559 | 0.921 |

The table reports descriptive statistics for the main variables used in the study.

**Table 4. Pre-treatment means for the number of patents and the number of citations and test of parallel trend**

| | N | Treatment | Controls | Difference | t-test | p-value |
|---|---|---|---|---|---|---|
| **Panel A: Pre-treatment means** | | | | | | |
| Number of patents | 2261 | 4.025 | 3.497 | 0.528 | 0.900 | 0.391 |
| Number of citations | 2261 | 6.683 | 6.766 | -0.083 | -0.580 | 0.575 |

| | Coefficient | p-value |
|---|---|---|
| **Panel B: Test of parallel trends** | | |
| Intercept | 0.930 | 0.000 |
| Pre_Sept2015×Treatment | −0.003 | 0.962 |
| Pre_March2016×Treatment | −0.067 | 0.215 |
| Pre_Sept2016×Treatment | −0.045 | 0.427 |
| Pre_Sept2015 | 0.114 | 0.043 |
| Pre_March2016 | 0.032 | 0.510 |
| Pre_Sept2016 | 0.064 | 0.129 |
| Post×Treatment | −0.083 | 0.029 |
| Post | −0.081 | 0.032 |
| Treatment | 0.090 | 0.023 |
| Quarter effects | Yes | |
| Industry effects | Yes | |
| N | 3954 | |
| R2 | 12.53% | |

Panel A presents pre-treatment means for the patent counts and patent citations for the treatment and control firms. Panel B tests the parallel trend assumption that there is no difference in innovation levels between treated and control firms before TSP.

**Table 5. The relation between AT and corporate innovation**

| | Main regression | | | | Firm-fixed effects | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Firms with patents | | Zero for missing patent data | | Firms with patents | | Zero for missing patent data | | Industry-adjusted patent counts | | R&D | |
| | Coeff | p | Coeff | p | Coeff | p | Coeff | p | Coeff | p | Coeff | p |
| **Panel A: Main analysis** | | | | | | | | | | | | |
| Intercept | 0.966 | 0.000 | 1.171 | 0.000 | | | | | −0.558 | 0.028 | 0.649 | 0.036 |
| Post | −0.132 | 0.001 | −0.016 | 0.002 | −0.153 | 0.011 | −0.021 | 0.000 | −0.163 | 0.206 | 0.026 | 0.063 |
| Treatment | 0.060 | 0.142 | 0.017 | 0.004 | | | | | 0.241 | 0.178 | 0.020 | 0.651 |
| Post×Treatment | −0.053 | 0.004 | −0.014 | 0.006 | −0.037 | 0.017 | −0.008 | 0.069 | −0.439 | 0.022 | −0.049 | 0.020 |
| Missing_Patent_D | | | −1.196 | 0.000 | | | −0.916 | 0.000 | | | | |
| Controls | Yes | | Yes | | Yes | | Yes | | Yes | | Yes | |
| Quarter effects | Yes | | Yes | | Yes | | Yes | | Yes | | Yes | |
| Industry effects | Yes | | Yes | | No | | No | | Yes | | Yes | |
| Firm effects | No | | No | | Yes | | Yes | | No | | No | |
| N | 3954 | | 23035 | | 3954 | | 23035 | | 3954 | | 2787 | |
| R2 | 12.34% | | 74.75% | | 74.47% | | 90.24% | | 4.18% | | 39.93% | |

*continued on next page*

| | AT=odd_lot | | AT=trade_vol | | AT=PCA AT | |
|---|---|---|---|---|---|---|
| | Coeff | p | Coeff | p | Coeff | p |
| **Panel B: Direction of the change in AT for treated firms** | | | | | | |
| Intercept | 0.961 | 0.000 | 0.972 | 0.000 | 0.978 | 0.000 |
| Post | −0.131 | 0.000 | −0.132 | 0.000 | −0.131 | 0.000 |
| Treatment | 0.061 | 0.010 | 0.061 | 0.010 | 0.060 | 0.010 |
| Post×decrease in AT | −0.122 | 0.000 | −0.199 | 0.000 | −0.272 | 0.000 |
| Post×zero or increase in AT | −0.025 | 0.364 | −0.037 | 0.160 | −0.034 | 0.175 |
| Controls | Yes | | Yes | | Yes | |
| Quarter effects | Yes | | Yes | | Yes | |
| Industry effects | Yes | | Yes | | Yes | |
| N | 3954 | | 3954 | | 3954 | |
| R2 | 12.44% | | 12.46% | | 12.55% | |

| | Firms with patents | | Firm-fixed effects | |
|---|---|---|---|---|
| | Coefficient | p-value | Coefficient | p-value |
| **Panel C: The speed with which firms react to TSP** | | | | |
| Intercept | 0.945 | 0.000 | | |
| Post_May2017 | -0.071 | 0.009 | -0.088 | 0.001 |
| Post_May2017×Treatment | -0.040 | 0.305 | -0.008 | 0.814 |
| Post_Dec2017 | -0.128 | 0.000 | -0.120 | 0.000 |
| Post_Dec2017×Treatment | -0.053 | 0.076 | -0.030 | 0.407 |
| Post_Sept2018 | -0.240 | 0.000 | -0.300 | 0.000 |
| Post_Sept2018×Treatment | -0.091 | 0.070 | -0.114 | 0.011 |
| Treatment | 0.067 | 0.007 | -0.088 | 0.001 |
| Controls | Yes | | Yes | |
| Quarter effects | Yes | | Yes | |
| Industry effects | Yes | | No | |
| Firm effects | No | | Yes | |
| N | 3954 | | 3954 | |
| R2 | 12.75% | | 70.29% | |
| Testing the hypothesis: Post_June2017×Treatment= Post_Sept2018×Treatment | | | | |
| F-test | 3.25 | | 4.53 | |
| p-value | 0.072 | | 0.033 | |

Panel A reports regression results for Eq.(1) for a sample of firms with patent information and when we assume zero patents for firms without patent data. Column 'Firm-fixed effects' reports Eq.(1) results augmented with firm-fixed effects. Column 'Industry-adjusted patent counts' reports results where the dependent variable in Eq.(1) is the industry-adjusted number of firm patens. Column 'R&D' reports Eq.(1) results where the dependent variable are R&D expenses. Panel B reports results Eq.(1) results where we identify the direction of change in AT for treated firms. Panel C reports Eq. (1) results where we split the TSP period into three subperiods.

## Table 6. Number of citations and KSPP innovation value measure

| | Number of citations | | KSPP real | | KSPP nominal | |
|---|---|---|---|---|---|---|
| | Coeff | p | Coeff | p | Coeff | p |
| Intercept | 1.098 | 0.000 | −0.872 | 0.084 | −2.431 | 0.058 |
| Post | −0.488 | 0.000 | 0.068 | 0.747 | 0.443 | 0.381 |
| Treatment | 0.113 | 0.041 | −0.044 | 0.594 | −0.099 | 0.622 |
| Post×Treatment | −0.118 | 0.034 | −0.196 | 0.028 | −0.494 | 0.028 |
| Controls | Yes | | Yes | | Yes | |
| Quarter effects | Yes | | Yes | | Yes | |
| Industry effects | Yes | | Yes | | Yes | |
| N | 3954 | | 2737 | | 10291 | |
| R2 | 9.36% | | 11.59% | | 2.16% | |

The table reports regression results for Eq.(1) where the dependent variable is the number of citations, and the Kogan et al. (2017) measures of the private economic value of patents calculated in real and nominal terms.

**Table 7. Cross-sectional analysis**

| | X=% stock compensation | | X=Accruals | | X=PCA EA factor | |
|---|---|---|---|---|---|---|
| | Coeff | p | Coeff | p | Coeff | p |
| Intercept | 1.296 | 0.000 | 1.022 | 0.000 | 1.058 | 0.000 |
| Post×X | 0.018 | 0.618 | 0.169 | 0.500 | 0.028 | 0.561 |
| Treatment×X | 0.084 | 0.041 | 0.235 | 0.228 | 0.151 | 0.025 |
| Post×Treatment×X | −0.096 | 0.054 | −0.439 | 0.005 | −0.122 | 0.015 |
| X | −0.024 | 0.477 | −0.035 | 0.830 | −0.064 | 0.143 |
| Post | −0.206 | 0.001 | −0.140 | 0.001 | −0.118 | 0.008 |
| Treatment | −0.032 | 0.626 | 0.035 | 0.546 | 0.030 | 0.566 |
| Post×Treatment | 0.058 | 0.293 | −0.013 | 0.756 | −0.049 | 0.240 |
| Controls | Yes | | Yes | | Yes | |
| Quarter effects | Yes | | Yes | | Yes | |
| Industry effects | Yes | | Yes | | Yes | |
| N | 2166 | | 3954 | | 3836 | |
| R2 | 11.69% | | 16.72% | | 17.30% | |

The table presents results for Eq.(1) augmented with an interaction term with the intensity of managerial stock compensation, firm's earnings quality measured by accruals, and an index measure from a principal component analysis of audit fees, a dummy variable for restatement, accruals, Big4 auditor indicator and an indicator for whether the auditor is PCAOB registrant.

**Table 8. Price discovery at the patent grant date**

| | AR(0) | | $\frac{AR(0)}{CAR(-1,1)}$ | | CAR(1,60) | | CAR(61,100) | | CAR(101,140) | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Coeff | p | Coeff | p | Coeff | p | Coeff | p | Coeff | p |
| Intercept | -0.019 | 0.005 | 0.295 | 0.000 | -0.004 | 0.647 | 0.051 | 0.211 | 0.023 | 0.557 |
| Post | 0.007 | 0.020 | 0.024 | 0.051 | 0.005 | 0.146 | -0.011 | 0.333 | -0.011 | 0.294 |
| Treatment | 0.007 | 0.002 | 0.022 | 0.017 | -0.001 | 0.677 | -0.011 | 0.041 | -0.011 | 0.220 |
| Post×Treatment | -0.007 | 0.023 | -0.035 | 0.038 | -0.009 | 0.063 | 0.020 | 0.075 | 0.017 | 0.304 |
| Controls | Yes | | Yes | | Yes | | Yes | | Yes | |
| Quarter effects | Yes | | Yes | | Yes | | Yes | | Yes | |
| Industry effects | Yes | | Yes | | Yes | | Yes | | Yes | |
| N | 9565 | | 9565 | | 9565 | | 9565 | | 9565 | |
| R2 | 0.42% | | 0.15% | | 0.27% | | 2.06% | | 1.26% | |

The table reports Eq. (1) results where the dependent variable is the abnormal return on the patent grant date, AR(0). Column $\frac{AR(0)}{CAR(-1,1)}$ reports results for Eq. (1) where the dependent variable is the ratio of the announcement date price reaction standardized by the total signal value measured in a three-day window around the patent grant announcement. Column CAR(1,60) reports results for Eq. (1) where the dependent variable is the cumulative abnormal return (CAR) from day 1 to day 60 after the patent grant date. Column CAR(61,100) reports results for Eq. (1) where the dependent variable is CAR measured over 61 to 100 days after the patent grant. Column CAR(101,140) reports results for Eq. (1) where the dependent variable is CAR measured over 101 to 140 days after the patent grant. We use the Carhart (1997) model as the normal return benchmark.

**Table 9. Tick Size Pilot and institutional ownership**

| | Y=Institutional ownership | | Y=Transient | | Y=Dedicated | |
|---|---|---|---|---|---|---|
| | Coeff | p | Coeff | p | Coeff | p |
| Intercept | 0.526 | 0.000 | 0.270 | 0.000 | 0.057 | 0.000 |
| Post | 0.022 | 0.041 | 0.006 | 0.681 | −0.022 | 0.000 |
| Treatment | 0.026 | 0.028 | −0.008 | 0.269 | −0.027 | 0.005 |
| Post×Treatment | 0.000 | 0.993 | −0.011 | 0.204 | 0.021 | 0.015 |
| Controls | Yes | | Yes | | Yes | |
| Quarter effects | Yes | | Yes | | Yes | |
| Industry effects | Yes | | Yes | | Yes | |
| N | 3815 | | 3815 | | 3815 | |
| R2 | 6.98% | | 6.32% | | 7.87% | |

The table reports regression results for Eq.(1) where the dependent variable is the percentage institutional ownership and the ownership by transient and dedicated investors as a fraction of total institutional ownership.

**Table 10. Analyst coverage and forecast dispersion**

| | Y=Number of analysts | | Y=Dispersion | |
|---|---|---|---|---|
| | Coeff | p | Coeff | p |
| Intercept | 2.326 | 0.000 | 0.250 | 0.156 |
| Post | −0.126 | 0.537 | 0.164 | 0.454 |
| Treatment | −0.277 | 0.178 | −0.044 | 0.362 |
| Post×Treatment | −0.273 | 0.245 | −0.184 | 0.408 |
| Controls | Yes | | Yes | |
| Quarter effects | Yes | | Yes | |
| Industry effects | Yes | | Yes | |
| N | 3746 | | 3556 | |
| R2 | 26.00% | | 3.63% | |

The table reports Eq.(1) regression results where the dependent variable is either the number of analysts covering a stock or analyst forecast dispersion measured before quarterly earnings announcements.