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

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Abstract

Using the Tick Size Pilot experiment as an exogenous shock to algorithmic trading (AT), we establish a causal positive relation between AT and innovation measured by the quantity and quality of patents. This result reflects that AT increases the efficiency with which prices capture the benefits of innovation, which prompts managers to devote more resources to innovation as the stock price performance influences managers' compensation and career prospects. Consistently, the relation we document is stronger (i) for firms in which managerial compensation is more closely linked to the share price performance and (ii) for more opaque firms, in which managerial effort is more difficult to infer from accounting information and stock prices play an important monitoring role. The conclusions generalize to other measures of innovation such as R&D spending.

JEL: D53; G12; G14; M41

Keywords: algorithmic trading; patents; citations; innovation

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We thank Stefan Nagel, James Ohlson, Jay Jung, Art Kraft, Tim Martens, Zhongwei Huang, Shiva Shivakumar and seminar participants Bayes Business School and participants at meetings of the 2022 American Accounting Association, 2022 European Accounting Association, 2022 Canadian Academic Accounting Association, 11th International Conference of the Financial Engineering and Banking Society, 2022 Financial Management and Accounting Research Conference, 2022 European Financial Management Association meeting and for helpful comments. All errors and omissions are our own.

1. Introduction

Innovation is a lengthy and costly process of developing and testing new ideas that associates with a high project failure rate (Holmstrom 1989).[1] Managerial incentives to spend resources on projects with uncertain outcomes and delayed payoffs will decrease when the stock price efficiency is low — the extent and the speed with which prices impound information entering the public domain — because prices do not fully capture the benefits of innovation, such as its impact on earnings growth and future returns.[2] Importantly, 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. Lev and Sougiannis (1996) and Chan, Lakonishok and Sougiannis (2001) document that R&D spending predicts future stock returns.

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 will promote more innovation. AT increases price discovery by quickly incorporating new public information into stocks prices through their trades and through supplying liquidity 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

[1] Though risky, 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.'

[2] Managers care about prices capturing the outcomes of innovation because share price targets are a frequent performance measure 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' compensation, e.g., through stock options, and 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 these actions will be reflected in the stock price.'

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. ATs have a material impact on capital markets as in recent years their trades have 'accounted for more than 50 per cent of the reported trading volume in U.S. stock markets', Lee and Watts (2021, p.375).[3] By promoting fast and more complete impounding of innovation news into stock prices, we expect that ATs will incentivize managers to engage in more innovation.

It is not obvious that higher price efficiency promoted by AT would increase innovation rates. First, the higher speed with which new information incorporates into prices means news about disappointing innovation projects also impounds more quickly, and as a result, the pool of investment projects a firm would purse may narrow (Hirshleifer and Suh 1992).[4] Second, ATs can identify and trade in advance of information-driven trades, which discourages fundamental investors from devoting resources to costly information acquisition (Weller 2018, Lee and Watts 2021). Less *informative* stock prices reduce managerial ability to learn from stock prices, e.g., about the direction and scope of the firm's projects, which can reduce innovation (Chen, Goldstein and Jian 2007). Third, 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 […] 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). Liquidity also promotes formation of blockholdings, which can negatively affect innovation (Yafeh and Yosha 2003, Jones and Danbolt 2003, Tribo, Berrone and Surroca 2005, Kang, Chung and Kim 2019). Higher stock liquid can also lower

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[3] 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.

[4] Managers may *overinvest* in innovation when investors misinterpret higher innovation levels as a signal of better outlook (Bebchuk and Stole 1993), higher 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 innovation outcomes impound quicker into stock prices disciplining managers, e.g., from analyst reports and media questioning the benefits of high innovation spending.

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 an open question that we tackle empirically.

To establish causality between AT and corporate innovation, we 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 need to be updated, eroding the speed advantage of algorithmic trades over staled quotation (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 a significant reduction in AT in treated, but not in control stocks, after the start of TSP. 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 since 2012 and to align the length of the pre-treatment period with the treatment period, we limit the analysis 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 trading 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 instantaneous 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, we measure innovation by the number of patents (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). Compared to other measures of innovation, patents 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 a successful outcome of innovation resulting in a patent.[5]

Our analysis proceeds as follows. First, we confirm a statistically and economically significant reduction in AT after the start of TSP for treated compared to controls stocks in our sample of firms that engaged in innovation activity at any point over the sample period. 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%. 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.

[5] 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).

Next, we present our main result of 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 termination of 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.[6] To distinguish the effect of AT on innovation from the effect of changes in tick size in treated firms, we show that our conclusion is present only for treated stocks that experienced a reduction in AT, but not for treated firms that despite a decrease in the tick size, did not experience a reduction in AT. Thus, our conclusion is not confounded by changes in stock liquidity mediated through changes in TSP-induced trading costs.[7]

Exploring the speed with which treated firms change their innovation activity, we find that the effect we document becomes significant in the latter half of the TSP period. This evidence is consistent with managers needing time to observe changes in AT and understand the implications lower AT has on price efficiency and to adjust firm innovation levels accordingly. To support our argument that the AT effect on innovation is channeled through more efficient prices, we examine price reactions to the U.S. Patent and Trademark Office patent grant disclosures announcements around the TSP event. Relative to control stocks and after the start of TSP, treated firms have (i) relatively slower price discovery of patent disclosures around patent grant announcements, and (ii) lower abnormal returns for 60 days following the patent grant announcement and a subsequent reversal that corrects the initial underreaction.

[6] Because our sample period is short — two years before and two years of the TSP period, firm-fixed effects largely control for (i) managerial characteristics and (ii) the characteristics of managerial contracts that could affect innovation.
[7] Studies document that TSP treated firms experienced 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). Relevant for us, Fang et al. (2014) document that lower stock liquidity, measured by higher spreads, increases innovation through changes in investor composition and takeover pressure, thus lower liquidity of treated firms should have a positive effect on innovation, which further excludes the liquidity channel affecting our conclusions.

Our conclusion on a positive effect ATs have on innovation is unchanged when we examine 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 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 application and disclosure.

Cross-sectional tests show that the positive effect of AT on innovation is stronger for stocks with a higher proportion of the CEO's stock-based compensation in total compensation. 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 weaker for less opaque firms, as measured by lower accruals and higher financial reporting quality proxies. High quality accounting numbers increase the relative usefulness of accounting information compared to stock prices for assessing managerial effort (Kang and Liu 2008, Garvey and Swan 2002). Finally, we document a reduction in the sensitivity of CEO's forced turnover to poor stock price performance in treated firms, a result consistent with less efficient prices having a lesser impact on managerial career outcomes.

Next, we examine the novelty and economic significance of 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 material: treated firms have on average 50.4% fewer citations compared to control stocks after the start of TSP. We reach a similar conclusion that the quality of patents for treated firms reduces when we measure their originality (Hall, Jaffe and

Trajtenberg 2001), and use the Kogan et al. (2017) measures of economic value of patents.[8] 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.

We perform several tests to exclude alternative explanations and confounding effects. First, a decrease in AT could be associated with changes in institutional ownership, which in turn can affect firm innovation. We examine changes in total institutional ownership and in ownership by transient and by dedicated investors. We do not find significant evidence of changes in total institutional holdings or in the share of transient investors' holdings. Dedicated ownership tends to increase for the treated firms, which should have a positive effect on innovation (Bushee 1998, Aghion, Van Reenen and Zingales 2013).[9] 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. We find no evidence of (i) changes in analyst coverage between the two groups nor (ii) changes in analyst forecast dispersion measured before earnings announcements that would suggest changes in the quality of the firm's information environment. Third, we find no evidence that our result captures managerial myopic underinvestment to boost short-term profits. Such an explanation requires that AT reduction in treated firms 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

[8] The measure of economic value of patents in Kogan et al. (2017) 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.

[9] Borochin and Yang (2017) document that dedicated investors have informational advantage and their trades decrease future firm misvaluation relative to fundamentals, while transient investors have the opposite effect.

investment spending, but find no such evidence. We conclude that the alternative explanations are unlikely to be behind our evidence.

Our study contributes to the emerging literature on the real effects AT 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 stock price efficiency. This result has important policy implications as regulators debate how to regulate AT and the risk it poses to capital markets.[10] The study complements the research focused on the impact ATs have on liquidity, price discovery and informativeness (Hendershott, Jones and Menkveld 2011; Hasbrouck and Saar 2013; Chordia and Miao 2020; Chakrabarty, Moulton and Wang 2020; Bhattacharya, et al 2020; Hu, Pan and Wang 2017; Weller 2018; Lee and Watts 2020; Boehmer, Fong and Wu 2021).

Our finding on a positive relation between price discovery and patents complements the research stream that suggests a positive relation between price *informativeness* (the acquisition and incorporation of private information into prices) and innovation. 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 returns, 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

[10] Securities and Exchange Commission's report 'Staff Report on Algorithmic Trading in U.S. Capital Markets' highlights that ATs pose significant capital market risks as they 'exacerbate periods of unusual market stress or volatility.', SEC (2020). The report also acknowledges the need for 'continued vigilance in monitoring these advances in technology and trading, and updating of systems and expertise will be necessary in order to help ensure that our capital markets remain fair, deep, and liquid.'

Wruck 1987, Kaplan and Minton 2006, Jenter and Kanaan 2006). Weller (2018) and Lee and Watts (2021) document that AT order screening to avoid adverse selection and 'back-running' reduce the incentives for fundamental investors and analysts to acquire private information, with detrimental effects on price informativeness.[11] Our sensitivity tests suggest that any *negative* effect ATs have on innovation mediated through lower price informativeness is likely to be low. For example, we do not find that the increased information discovery by dedicated investors for TSP stocks (our proxy for fundamental investors following Borochin and Yang (2017)) has a significant effect on 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). Several 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

[11] 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, which reduces stock price informativeness.

the concern that noise in the stock price reduces the usefulness of contracts linked to the stock price performance.[12]

2 Literature review

2.1 Algorithmic trading and innovation

In the last decade, algorithmic trading has attracted significant attention from academics, regulators, market operators (e.g., the listing exchanges), practitioners, and the public.[13] The literature documents that the automation and the 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 EDGAR filings and Michigan Index of Consumer Sentiment announcements for high AT stocks.

[12] 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), institutional monitoring (Coffee, 1991; Gillan and Starks, 2000) 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.

[13] 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.

We expect that the positive effect AT has on price efficiency facilitates quick and more complete impounding of public innovation signals into stock prices, which increases managers' incentives to invest in innovation. Managers care about quick and efficient impounding of innovation news into stock prices, e.g., about the impact innovation will have on future earnings, because their contracts are typically tied to the stock price performance to reduce agency risk (Ittner et al. 1997; Indjejikian and Nanda 2002; Core et al. 2003; Aghion et al. 2013) and the stock price performance affects managers' career prospects (Chang, Dasgupta and Hilary 2010). Therefore, a more efficient pricing, through AT, provides the necessary incentive for corporate managers to exert costly effort to improve the firm's fundamental value through innovation, since 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).

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) argue that AT discourages fundamental investors from acquiring costly private information before 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 and 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 returns, consistent with managers learning from stock prices. Other research identifies 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.

In our setting, it is unclear how ATs will affect innovation as their positive effect on innovation mediated through higher price efficiency can be offset by their negative effect on informativeness, which in turn has a negative impact on innovation. This tension motivates our empirical analysis of this research question.

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 market making and price discovery of small capitalization securities[14]. 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 from \$0.01 to \$0.05 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

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

completion in October 2018, all treated stocks returned to their original trading tick size. We exploit 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 stock 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 the SEC Market Information Data Analytics System (MIDAS) data to construct 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 post-TSP period.[15]

[15] Lee and Watts (2021, p.383) highlight that '[I]n contrast to TAQ data, which only provide information on the national best bid offer (NBBO), MIDAS incorporates quote and cancellation information from the entire order book' and '[A]s discussed in Weller (2018), MIDAS data allow researchers to construct vastly improved AT proxies. For instance, some earlier AT studies used the NASDAQ AT proprietary dataset (e.g., Brogaard, Hendershott, and Riordan 2017; O'Hara, Yao, and Ye 2014; Carrion 2013), which covers a short sample period, 2008–2009, and includes only around 120 stocks. Other studies used TAQ data, which only include the NBBO, thus omitting the rest of the order book where AT activity may be taking place. Further, TAQ data traditionally ignored odd lot trades, where a large amount of AT activity is known to occur (O'Hara et al. 2014).'

3.3 Innovation variables

Following extant literature (Hall et al. 2001; Hirshleifer, Low, and Teoh 2012; Atanassov 2013; Seru 2014; Sunder, Sunder and Zhang 2017), we construct several 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*, to capture 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 the 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 innovation by a firm compared to the industry average.

To capture the patents' quality and their technological and economic importance, we count the total number of 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, forward citations capture the scientific value of the patent (Trajtenberg, Jaffe and Henderson 1997; Hall, Jaffe, and Trajtenberg 2001). Following Hall et al. (2001), we also measure patents' originality, *Originality*, which captures how many previous patents an invention draws on to produce a novel idea. More backward citations indicates lower originality as the patent is more closely related to previous innovations.

To speak 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 [patent-promoted] 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 concern that patent citations may not adequately measure knowledge flows (Agrawal and Henderson 2002; Jaffe et al. 2002).

The number and quality of patents measure innovation output conditional on the firm's decision to protect the innovation through a patent and on the successful outcome of the patent application. 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 more broadly than 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.

3.4 Regression model

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:

$$\begin{aligned} innovation_{i,t} = \gamma_0 + \gamma_1\, Post_t + \gamma_2\, Treatment_i + \gamma_3 Post_t \times Treatment_i \\ + Controls_{i,t} + \varepsilon_{i,t} \end{aligned} \tag{1}$$

where $innovation_{i,t}$ is the logarithm of one plus the measures of firm's patents in quarter *t*, $Post_t$ is an indicator variable that takes the value of one for each quarter of the TSP period from October 2016 to September 2018, and zero for the period from October 2014 to September 2016. $Treatment_{i,t}$ equals one if a 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, $\gamma_3 Post_t \times Treatment_i$. To the extent that AT activity enhances the efficiency of stock prices in capturing innovation investments, the decrease in AT in treated firms during the TSP period should reduce corporate innovation, and thus γ_3 should be negative. On the other hand, lower AT activity in treated firms may increase information acquisition by fundamental investors increasing stock price informativeness. Higher stock price informativeness means managers are better able to learn from stock prices, which will lead to positive γ_3.

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.[16] These concerns motivated Lee and Watts (2021) to present results without controls, and as robustness, results with four controls variable (firm size, book-to-market, ROA and asset growth). However, to build confidence in our results, we include a number of firm-level control variables in all models. 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 use internally generated cash to capture cash liquidity (*Cash/Assets*). We control for institutional ownership using the percentage of institutional holdings (*Institutional ownership*). Variables definitions are in Appendix A and measured for each quarter. In robustness tests, we also include firm-fixed effects to control for time-invariant firm characteristics. To minimize the effect of extreme observations, we winsorize

[16] 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.

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.

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 MIDAS. We obtain patent-level data from the U.S. Patent and Trademark Office (USPTO) database, which we match to the TSP sample.[17] Similar to Kogan et al. (2017), for our main tests, we only keep firms with at least one 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.[18] However, robustness tests show our conclusions are unchanged when we assign zero to firms with no patent information (Fang et al. 2014). We use CRSP and 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 (1,980 treated firm-quarter-years and 1,974 control firm-quarter-years).

5. Results

Panel A of Table 1 presents descriptive statistics for the six AT measures. The AT measures exhibit comparable values to those in previous research, alleviating the concern that the

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[17] See Graham, Hancock, Marco and Myers (2020) for a description of the U.S. Patent and Trademark Office data.
[18] Assuming zero for a firm that would never engage in a patent development can produce spurious associations between patent counts and predictors of innovation (Hausman et al., 1984).

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). The correlations between the six AT proxies presented in Panel B are significant and comparable to earlier research (e.g., Lee and Watts 2021). Finally, Panel C reports pre-TSP means for the AT measures split between treatment and control stocks and their difference. Consistent with earlier studies (e.g., Chakrabarty et al. 2021), there are no significant differences in the pre-treatment intensity of AT between the two groups.

[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 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. 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, the number of forward citations, originality of patents, the real and nominal measures of private economic value of patents, and R&D spending. During our period sample firms obtained on average 3.552 patents per year-quarter (−0.014 industry-adjusted patents) with an average of 5.076 citations. The mean nominal (real) value of patents is $8.177 million ($3.364 million), and the average patent draws on fewer previous patents compared to the patent with the highest past citation rate. The mean R&D spending is 5.9% of sales. 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. Consistent with the TSP firms being earlier in the firm's life cycle, they tend to have low profitability, cash holdings and leverage, but high growth potential. The average institutional ownership in our sample is 70.8%.

[Table 3]

In Panel A (Panel B) of Table 4, we examine whether there are significant differences in the mean values of the dependent (control) variables between our treatment and control samples in the pre-TSP period. We do not find any significant differences in the pre-treatment 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 C evaluates the presence of pre-existing trends following the approach from Donelson, McInnis and Mergenthaler (2016) and Ahmed, Li and Xu (2020). Specifically, we include pre-TSP period indicators in Eq. (1) and their interactions with the treatment firm indicator. This approach allows control and treated firms to have different pre-treatment trends in innovation. 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. The dependent variable is log of 1+number of patents, which is our main measure of innovation. The regression result shows that none of the interaction terms between pre-TSP period indicators and the treatment dummy are significant, which suggests no significant differential trend for treated firms before TSP.[19] This result is consistent with the parallel trend assumption holding in the data and further supports the supposition that the random assignment of the TSP program did not result in selectivity bias on firm innovation activities.[20] In untabulated results, we reach a similar conclusion when using the other measures of innovation as dependent variables.

[Table 4]

5.3 Regression results for the relation between AT and innovation

Panel A of Table 5 examines the effect of TSP on the log 1+number of patents as described in Eq.(1). The regression 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 the number of patents. This evidence suggests that AT activity is positively associated with corporate innovation.[21] The economic magnitude of the

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[19] Including pre-treatment period indicators changes the interpretation of the coefficient on the interaction Post×Treatment in Table 4, which now captures the differential effect relative to the 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.

[20] The evidence that innovation levels 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 but not control firms. This case would require non-random assignment between treated and control firms on innovation, which the TSP natural experiment avoids.

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

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%).[22]

[Table 5]

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 effects in Eq. (1). Column 'Firm-fixed effects' documents that our conclusions remain unchanged for this analysis. Further, 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 (unlogged) industry-adjusted patent count as the dependent variable in Eq. (1). We continue to find a significant negative coefficient on the interaction term *Post*×*Treatment*.

The last columns of Panel A use the log 1+R&D spending, measured as the ratio of research and development expenditures for the most recent fiscal quarter scaled by sales, 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 (e.g., 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. 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.

[22] 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 $\frac{5.3\%}{37.9\%}$.

Intensity of AT

The SEC split stocks in the treatment group into subcategories based on the intensity of trading restrictions. Stocks with the smallest trading restrictions are quoted in \$0.05 increments but can trade at the \$0.01 tick size. Stocks with the strictest trading restrictions are quoted and traded at \$0.05 increments and subject to a 'trade-at' requirement. The trade-at requirement prohibits a trading venue from meeting an incoming order without displaying the National Best Bid and Offer. This discourages trades channelled to dark and alternative venues. Rindi and Werner (2017) and Comerton-Forde, Gregoire and Zhong (2019) highlight that liquidity providers, such as ATs, are disincentivized to trade in stocks where orders have to be executed at 'trade-at' rule. We expect higher trading restrictions to lead to a larger reduction in AT and consequently a more significant impact on innovation.

We first use principal component analysis to create an index measure, *AT factor*, from the six AT proxies and use it as the dependent variable in Eq. (1). The weights are −0.208 for ln *odd_lot*, −0.286 for ln *cancel_ord*, −0.280 for ln *cancel_ord2*, 0.197 for ln *trade_vol*, 0.203 for ln *trade_vol2* and 0.171 for ln *trade_size*. We multiply the *AT factor* by −1 so that higher values of the measure reflect higher intensity of AT. We then create an indicator variable *TR_Intensity*, which captures treatment stocks with the strictest trading restrictions, that we interact with *Treatment* and *Post*×*Treatment* indicators. We expect these stocks to experience an incremental reduction in AT and in innovation.[23] The first columns of Panel B confirm an incremental reduction in AT for treated stocks subject to the strictest trading restrictions and the latter columns of Panel B show an incremental negative effect on innovation. This evidence is consistent with innovation reducing in the intensity of AT in treated firms.

[23] We perform this test because it is not obvious that treated stocks with most strict trading restrictions will experience an incremental reduction in AT. For example, Lee and Watts (2021, p.373) highlight that '[P]rior studies (e.g., Rindi and Werner 2019; Chung, Lee, and Roosch 2020), as well as our own analyses, find that the effect on liquidity is economically similar across the three groups of treated firms', which is why they 'combine these three subgroups and refer to them collectively as the "treated firms"'. Lee and Watts (2021) do not test if TSP had a differential effect on the intensity of AT across stocks subject to varying level of trading restrictions.

Placebo test

Next, we 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, *Placebo_post*. We then run equation (1) with the AT factor as the dependent variable and then with the number of patents as the dependent variable. Panel C regression results show no significant differences in the intensity of AT for the treated firms compared to controls for the placebo period nor significant differences in innovation between the two groups. These results suggest that there are no changes in the intensity of AT and in innovation between treated and control firms absent the TSP program.

The overinvestment explanation

Our results could capture treated firms reducing overinvestment incrementally to control firms after the start of TSP when both treated and control where overinvesting in innovation before the start of TSP. Reducing the pool of underperforming projects should then associate with a relatively higher future financial performance of treated firms as measured by the return on assets or cash flows. In contrast, if treated firms reduce overinvestment in innovation with positive outcomes, future financial performance should be poorer compared to control firms. Panel D reports results for Eq. (1) where we use future quarterly mean ROA and cash/assets measured over six quarters relative to the current year-quarter.[24] Treated firms have lower future ROA and generate less cash, a result consistent with treated firms rejecting positive NPV innovation projects that would have associated with better operating performance in the future. The evidence that treated firms have poor financial performance in the future also helps rule out that the innovation decline for treated firms that we observe reflects myopic underinvestment in innovation by treated firms' managers to temporarily boost corporate performance (we do not

[24] We look six quarters ahead because (i) we do not expect benefits of innovation to have instantaneous impact on financial performance and (ii) the period is short enough to give us confidence that changes in financial performance are linked to changes in firm's innovation.

see a reason why a reduction in AT should associate with amplified capital market pressures to boost reported earnings leading to myopic underinvestment).

Assuming zero for missing patent information

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. We also expect to see a significant proportion of missing patent data in our sample that includes small firms. Appendix B repeats the main regression and the model with firm-fixed effects assuming zero for missing patent data. For this analysis, we augment Eq. (1) with an indicator for missing patent observations, *Missing_Patent_D*. The sample size increases to 23,035 observations and the coefficient on the interaction term *Post*×*Treatment* remains significantly negative. Thus, assuming missing values reflect no innovation activity produces similar conclusion to our main tests. The conclusions are the same when we use R&D as the dependent variable and assume zero for missing R&D data.

5.3.1 The effect of changes in AT vs. in tick size on innovation

The result that a reduction in AT affects innovation could be confounded by the effect of a higher tick size, which increases trading cost in treated firms. Thus, it is plausible that it is treated firms' lower liquidity that reduces innovation. Fang et al. (2014) document that higher stock liquidity reduces innovation by increasing the risk of hostile takeovers and increasing ownership by non-dedicated investors. Thus, if our results captured the liquidity channel, we should observe an increase in innovation in treated firms. However, we recognize that stock liquidity may affect innovation through other channels than identified in Fang et al. (2014), for example, lower liquidity in treated firms may discourage investment by monitoring institutional investors and it is lower monitoring that reduces incentives to innovate. To distinguish between the liquidity and AT channels, we identify the direction of the change in AT for treated firms after the start of the pilot program, which we then interact with the interaction term *Post×Treatment*. This approach splits the interaction term into two variables: *Post×Treatment×decrease in AT* and *Post×Treatment×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. We use the odd lot ratio and the trade to volume measures that have a positive association with AT to capture directional changes in AT. Further, we use the *AT factor* based on the principal component analysis of the six AT measures to identify an average increase or reduction in AT in the treatment compared to the pre-treatment period (as in Table 5, higher values of the AT factor indicate higher intensity of AT).

Table 6 confirms that the reduction in innovation comes from treated firms that also experience a reduction in AT. The coefficients on *Post×Treatment×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 of Table 5. This evidence suggests that the *Post×Treatment* interaction in our main regression captures the effect of AT with some 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.[25] Thus, the liquidity channel cannot explain our results.

[Table 6]

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 to 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 the early and later subperiods. *Post_Sept2017* is an indicator variable for the early part of the

[25] Fang et al. (2014) argue their result on a negative effect liquidity has on innovation captures 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 [higher risk of hostile takeovers] is debatable due to the greatly decreased frequency of hostile takeovers since the late 1980s.', further, they find that by using a price impact measure to capture institutional trading costs, they find no relation between liquidity and innovation. Dass et al. (2017), using more recent patent data, find that liquidity has no impact on innovation. As a result, the negative link between liquidity and innovation in Fang et al. (2014) is unclear.

TSP period that is between October 2016 and September 2017. *Post_ Sept2018* captures the period between October 2017 and the end of the TSP program in September 2018. 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.

Table 7 reports regression results when we include the subperiod indicators and their interactions in Eq. (1). The coefficient on the interaction term between the treatment dummy and the indicator for the early months of the TSP period, *Post_Sept2017×Treatm*ent, is negative but insignificant. The coefficient on *Post_Sept2018×Treatment* is –0.097 and significant, which suggests that the effect we document becomes significant in the later period of the TSP (the F-test reported in the bottom rows of Table 7 confirms that the coefficient on *Post_Sept2017×Treatment* is significantly different from the coefficient on *Post_Sept2017×Treatment*).

[Table 7]

5.3.3 Quality and economic value of innovation

Next, we turn to the measure of the scientific significance of patents captured by the number of citations (Harhoff et al. 1999) or originality (Hall et al 2001). This test helps us differentiate whether managers trade-off a lower number of patent applications for a relatively higher quality of patents or whether both the count and quality of innovation are reduced by lower AT activity. The first columns of Table 8 document that the number of citations decreases for treated firms following the start of TSP.[26] The economic effect is significant with citations reducing by 50.4%, $\left(i.e., \frac{11.8\%}{1/_2(8.9\%+37.9\%)}\right)$. We reach a similar conclusion when we examine the average originality of patents. After the start of TSP, new patents draw more strongly on previous

[26] We collect 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). There is also a concern 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 control firms would suffer from a similar bias.

inventions, which suggests lower uniqueness of newly developed patents. Jointly, the evidence suggests the scientific significance of patents decreases for treated firms.

[Table 8]

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 8 results suggest that the scientific and economic value of patents reduce as AT activity decreases.[27]

5.3.4 Cross-sectional tests

CEO stock compensation

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. Table 9 reports a negative coefficient on the triple interaction term *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.[28]

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[27] In untabulated results, we find that our conclusions from Table 8 are unchanged when we scale raw citation counts and the real and nominal measures of private economic value by the number of patents.
[28] In untabulated results, we find no significant differences in the mean percentage CEO stock compensation between control and treated stocks before and during TSP.

[Table 9]

Probability of forced CEO turnover

A corollary of less efficient prices is their lower disciplining role, i.e., stock price performance plays a lesser role in determining managerial career outcomes. In other words, the market penalty for lack of innovation – poorer stock price performance – has a lesser effect on managerial career outcomes. To test this prediction, we estimate the sensitivity of forced managerial turnover next quarter to past stock return performance (Hayes, Lemmon and Qiu 2012) using the data on forced turnover from Peters and Wagner (2014). To capture the return performance, we calculate cumulative abnormal returns measured over 180 days before quarter-end and use the S&P500 index as the normal return benchmark. To make the interpretation easier, we multiple cumulative abnormal returns by -1 so that higher values capture more negative return performance. The results in Table 9 confirm that CEO turnover in treated firms is less sensitive to poor return performance after the start of TSP, consistent with a lower impact stock price performance has on managerial career outcomes.[29]

Earnings quality

If stock prices do not capture managerial effort in creating shareholder value, e.g., through innovation, the firm's compensation committee and investors will put more weight on accounting information, such as earnings performance, to judge managerial performance (Holmstrom 1979; Banker and Datar 1989; Milgrom and Roberts 1992; Feltham and Xie 1994; Yermack 1995). Thus, lower monitoring usefulness of stock prices should have a lesser effect on innovation for firms with higher earnings quality as monitoring through financial statements can substitute reduced stock price monitoring. Following the literature, we use total accruals as a measure of earnings quality (Dechow, Ge and Schrand 2010), which is defined as the assets-

[29] The fraction of the sample with forced CEO turnover is only 2%, which is why we also used Execucomp to calculate instances of managerial turnover, which identified 9.8% of observations with CEO changes. Our conclusions are the same for this sample though it also includes voluntary CEO departures.

scaled difference between net income before extraordinary items and cash flow from operating activities, *Accruals*. We multiply accruals by −1 so that higher values capture higher earnings quality, *Low accruals*. We then interact *Low accruals* with the indicators for *Post*, *Treatment* and their interaction. Table 9 documents that the coefficient on the triple interaction term *Post*×*Treatment*×*Low accruals* is positive, consistent with a weaker link between AT and innovation when accounting numbers provide more precise 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 high reporting quality, *Composite high EQ measure*, based on a principal component analysis of audit fees (weight 0.596), a dummy variable for restatement (weight −0.14), accruals (weight −0.595) and an indicator for whether the auditor is PCAOB registrant (weight 0.026). The last columns of Table 9 confirm incrementally weaker effect on innovation for treated firms relative to control stocks after the start of TSP when a firm has high reporting quality. This result is consistent with financial information substituting less efficient prices in monitoring managerial effort related to innovation.

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). 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 '[T]he 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.'[30] To speak about the speed of price

[30] Kogan et al. (2017) do not find evidence of significant price reactions around patent application dates and argue this reflects that the USPTO does not publish applications at the time they are filed.

discovery, in the spirit of Weller (2018), we create a ratio of the price reaction on the patent grant announcement day to the total signal content measured in a three-day window centered on the patent grant disclosure day, $\frac{AR(0)}{CAR(-1,1)}$. Higher values of the ratio suggest that most of the signal content impounds on the announcement day and the ratio is not dependent on the economic value of the patent. The normal return benchmark we use to calculate abnormal returns around the patent grant announcement is the Carhart (1997) four-factor model estimated over 100 days before the patent grant date. Table 10 documents that price discovery happens outside the announcement day for treated firms relative to controls after TSP.[31] This result is consistent with lower AT associating with less efficient impounding of public innovation signals into stock prices.

[Table 10]

To understand if price inefficiency (i.e., underreaction) persists after the patent grant announcement, we also examine post-grant date abnormal returns over various windows. As Table 10 shows, we 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.[32] 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 is reversed over this window. Starting from day 101 after the announcement, we find no evidence of abnormal return performance. Jointly, Table 10 results are consistent with lower efficiency with which prices impound patent information for treated firms after TSP.

[31] The conclusions are similar when we measure the total signal content over the period from the announcement day to five days after the announcement.

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

6. Alternative explanations

This section presents tests that help rule out alternative explanations. First, we show that 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 report that financial constraints and changes in treated firms' reporting quality do not explain our findings.

6.1 Changes in institutional ownership and stock price informativeness

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. We believe this channel is unlikely to explain our findings as we control for institutional holdings in all regressions. Nevertheless, to ensure that our results are not confounded by changes in institutional ownership, Table 11 examines whether treated firms exhibit relatively lower levels of institutional ownership in the TSP period.

The evidence presented in the first column of Panel A, Table 11 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 institutional investors on innovation should be mostly related to increases in ownership by institutions with long investment horizons, as innovation benefits take long time to materialize, we classify institutional investors based on their investment type using the classification from 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.

[Table 11]

Weller (2018) and Lee and Watts (2021) document that ATs reduce fundamental investors' information acquisition, which in turn lowers stock price *informativeness*. Next, we attempt to gauge the extent the positive effect ATs have on innovation mediated through price discovery is moderated by the potential negative effect on innovation mediated through lower price informativeness. Following Borochin and Yang (2017), we focus on dedicated investors to capture investors exerting effort to uncover private information. We then interact the measure of dedicated institutional ownership with the indicators for *Post*, *Treatment* and *Post*×*Treatmetn* from Eq. (1). Panel B of Table 11 shows no significant coefficient on the triple interaction term on *Post*×*Treatment*×*Dedicated*, though the positive sign is consistent with higher informativeness promoted by dedicated investors having a positive effect on innovation. Thus, it seems any effect lower AT in TSP stocks have on innovation, mediated through higher fundamental information acquisition, is likely to be small.

Several studies use EDGAR web traffic to capture information acquisition intensity (Lee, Ma and Wang 2015, Dehaan, Shevlin and Thornock 2015, Drake, Roulstone, and Thornock 2015). An increase in EDGAR searches suggests more fundamental information acquisition which should lead to more informative prices and consequently more innovation. To validate that the informativeness effect on innovation is likely to be small in our treated firms, we also examine changes in EDGAR searches (for all filings) for our sample of innovation firms. We follow Ryans (2017) and use the log 1+the total number of human downloads from EDGAR per quarter-firm-year as the dependent variable in Eq. (1). Appendix C shows an insignificant coefficient on the interaction *Post*×*Treatment*, which supports our prediction that changes in fundamental information acquisition for treated firms, and their effect on innovation, are likely to be small.[33]

6.2 Changes in information environment

Ahmed et al. (2020, p.869) argue that a larger tick size 'increases the scrutiny of managers' financial reporting choices and reduces their incentives to engage in misreporting. They 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. Wang, Zhai, Sun, and Colombage (2020) documents a positive relation between earnings quality and earnings persistence and R&D activity. Thus, the increase in earnings quality for treated stocks should work against our result. We believe our findings 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.

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, using a more recent patent data, Dass et al. (2017) show no association between analyst coverage and patent counts. Table 12 reports Eq.(1) results where the dependent variable is the number of analysts covering the stock. We find no

[33] Lee and Watts (2021) in their Table 7 find an increase in EDGAR searches in very short windows around earnings announcements (−1,1) and (−10,1). However, their figure 7 shows that over longer horizons, this effect becomes insignificant, which is similar to our finding.

evidence of changes in analyst coverage for treated firms compared to controls stocks in our sample. 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 the 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 12 show no evidence of change in forecast dispersion. Jointly, the test results make it unlikely that changes in the firm's information environment explain our results.

[Table 12]

6.3 Voluntary disclosure and additional controls

Hope and Liu (2021, p. 6) report that lower liquidity of TSP stocks reduces treated firms' frequency of management earnings guidance, but they 'do not find evidence that algorithmic trading or fundamental information acquisition explain [their] results.' When we control for earnings guidance in our regressions (result untabulated), the magnitude of the coefficient on *Post*×*Treatment* is actually slightly higher (coefficient = −0.057, p-value = 0.015). Coupled with the evidence that lower voluntary disclosure promotes more innovation (Chen, Huang, Huang and Wang 2021), we believe it is unlikely that our evidence on lower innovation is because of reduced voluntary disclosures in treated firms.[34]

Finally, we also run Eq. (1) when we control for earnings quality, voluntary disclosure, analyst coverage, analyst forecast dispersion, CEO total compensation and CEO gender and find that our main result remains significant despite the sample size reducing by half (results untabulated). However, we are careful to draw conclusions from the regression with too many controls as Lee and Watts (2020, p.379) caution that '[A] key advantage of the Tick Size Pilot setting is that it allows us to estimate treatment effects with relatively few concerns for selection

[34] Hope and Liu (2021) argue that lower liquidity in treated firms reduces incentives to trade in a stock thus managerial incentives to provide voluntary disclosure. Their evidence suggests that increasing innovation disclosure may not counter lower AT and its impact on innovation when investors' incentives to trade are low. Thus, managers may not have viable innovation disclosure strategies to mitigate the negative effect that a reduction in AT has on innovation.

issues or omitted variable bias that would otherwise exist. In our randomized setting, over-usage of control variables may in fact introduce a "bad controls" problem, resulting less efficient estimators and potential bias in estimates (e.g., Angrist and Pischke 2009).'

6.4 Innovation and financial constraints

TSP can affect treated firms' cost of capital, e.g., because stock liquidity of treated firms becomes lower, and as a result, financially constrained firms reduce investment in innovation. Though we control for firm's financial constraints through the Cash/Assets ratio, to speak more directly to this alternative explanation, we interact Cash/Assets with the indicators *Post*, *Treatment* and *Post*×*Treatmetn* from Eq. (1). In untabulated results, we find that the interaction terms are insignificant and our main conclusions remain unchanged. Thus, it is unlikely that our results reflect a shock to financially constrained firms that, in response, reduce their investments in innovation.

7. Conclusions

We use the Tick Size Pilot natural experiment to examine the causal impact of algorithmic trading on innovation. We document an economically significant relation between a reduction in AT in treated firms and the number of patents and their economic and scientific significance. We argue that the result reflects that lower AT in treated firms reduces the efficiency with which prices reflect patent information, which reduces managerial incentives to spend resources on innovation.

Our study identifies an important channel through which market mechanisms, here the stock price efficiency promoted by AT, affect corporate innovation. Previous research that established a positive relation between stock returns and innovation builds on the efficient market hypothesis to assume prices efficiently capture the expected benefits of innovation and more innovative firms are rewarded with higher returns (e.g., Pakes 1985; Griliches, Hall and

Pakes 1991; Hall, et al., 2005). We showcase that a reduction in price efficiency, due to a reduction in AT, has a negative impact on innovation. Our evidence is consistent with managers rationally reducing innovation if they believe not all benefits will be quickly and fully reflected in the stock price. In this way, the findings also add to our understanding of the factors affecting managers' innovation decision. Finally, the study responds to the regulatory call for more research on capital market consequences of AT. The evidence suggests that regulators should consider the impact regulatory constraints on AT can affect firm innovative behavior.

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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.
Originality	Originality of patents. The measure is defined as the sum of backward citations a patent makes scaled by the maximum sum of backward citations made for all patents in the firm's two-digit SIC industry in the previous year. We then take $1 -$ the average value of the measure across all patents a firm applied in quarter t.
Treatment	An indicator variable for a firm in the treatment group that experienced an increase in tick size.
TR_Intensity	An indicator variable for treatment firms that experienced most strict trading restrictions: stocks quoted and traded at $0.05 increments and subject to a 'trade-at' requirement.
Post	An indicator variable for the post-treatment period that is between October 2016 and September 2018.
decrease in AT	An indicator variable for a decrease in algorithmic trading.
zero or increase in AT	An indicator variable for a no change or an increase in algorithmic trading.
Post_Sept2017	An indicator variable for the early part of the post-treatment period that is between October 2016 and September 2017.
Post_ Sept2018	An indicator variable for the middle part of the post-treatment period that is between October 2017 and September 2018.
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.
Placebo_post	A pseudo-treatment period from October 2014 to December 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

Continued on next page

Appendix A, *continued*

Variable name	Variable description
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
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.
Abnormal returns	Cumulative abnormal returns measured over 180 days before quarter-end where the normal return benchmark is the S&P500 index. We multiple the cumulative abnormal returns by -1 so that higher values capture more negative return performance.
Low accruals	Low accruals defined as −1 times accruals where accruals are calculated as net income before extraordinary activities less net cash flow from operating activities and then scaled by total assets.
Composite high EQ measure	An index measure of high reporting quality based on a principal component analysis of audit fees (weight 0.596), a dummy variable for restatement (weight −0.14), accruals (weight −0.595) and an indicator for whether the auditor is PCAOB registrant (weight 0.026).
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 effects
Industry effect	Industry effects based on Fama-French industry definitions.

Appendix B. Assuming zero for missing patent data

	Main regression		Firm-fixed effects		R&D	
	Coeff	p	Coeff	p	Coeff	p
Intercept	1.171	0.000			−0.033	0.487
Post	−0.016	0.002	−0.021	0.000	0.026	0.265
Treatment	0.017	0.004			0.027	0.692
Post×Treatment	−0.014	0.006	−0.008	0.069	−0.037	0.095
Missing_Patent_D	−1.196	0.000	−0.916	0.000	−0.438	0.091
Controls	Yes		Yes		Yes	
Quarter effects	Yes		Yes		Yes	
Industry effects	Yes		No		Yes	
Firm effects	No		Yes		No	
N	23035		23035		23035	
R^2	74.75%		90.24%		9.51%	

The table reports results for Eq. (1) when we use a sample of all TSP stocks and assume zero if the firm did not report any patents in either pre- or the TSP period. *Missing_Patent_D* is an indicator variable for missing patent data. In column 'R&D', we assume zero for missing R&D data.

Appendix C. EDGAR searches

	Coefficient	p-value
Intercept	6.990	0.000
Post	-3.847	0.000
Treatment	-0.081	0.239
Post×Treatment	-0.142	0.402
Controls	Yes	
Quarter effects	Yes	
Industry effects	Yes	
N	3343	
R2	44.49%	

The table reports results for Eq.(1) where the dependent variable is log 1+the number of human downloads from EDGAR (of all filings) per firm-year-quarter counted using the method from Ryans (2017). Data is from www.jamesryans.com and available through June 30, 2017.

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

	Treatment	Control	Difference	p-value
Panel C: Pre-treatments means for the AT measures				
odd lot	0.150	0.146	0.004	0.476
trade_vol	0.028	0.029	−0.001	0.410
trade_vol2	0.034	0.036	−0.002	0.209
cancel_ord	45.421	44.035	1.386	0.709
cancel_ord2	33.214	31.291	1.923	0.316
trade size	100.879	103.693	−2.814	0.320

The table reports descriptive statistics for the algorithmic trading measures (Panel A), their Pearson correlations (Panel B) and pre-TSP means split between treatment and control stocks (Panel C). The sample includes 3,954 firm-quarters for firms with at least one patent at any point over the period October 2014 to September 2018, which covers the pre- and TSP period. *odd_lot* is the quarterly average odd lot volume ratio defined as total odd lot volume to total trade volume. *trade_vol* is the quarterly average total trading volume ratio calculated as the total displayed trading volume to the order volume. *trade_vol2* is the quarterly average total trading volume ratio per displayed order defined as the total trading volume divided by total number of trades. *cancel_ord* is the quarterly average cancelled to trades ratio, defined as the ratio of total cancel orders to the total number of displayed orders. *cancel_ord2* is the quarterly average cancelled order to the total number of trades defined as the total number of cancelled orders to total number of trades. *trade_size* is the quarterly average trade size defined as total trade volume times 1000 and scaled by total trades.

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

	odd_lot		cancel_ord		cancel_ord2		trade_vol		trade_vol2		trade size	
	Coeff	p-value	Coeff	p-value	Coeff	p-value	Coeff	p-value	Coeff	p-value	Coeff	p-value
Predicted sign on Post×Treatment	−		−		−		+		+		+	
Intercept	−2.651	0.000	4.218	0.000	3.790	0.000	−3.384	0.000	−3.019	0.000	5.158	0.000
Post	0.277	0.000	−0.303	0.000	−0.302	0.000	0.272	0.000	0.256	0.000	−0.127	0.000
Treatment	−0.004	0.866	0.023	0.235	0.024	0.162	0.000	0.997	−0.008	0.699	−0.003	0.809
Post×Treatment	−0.137	0.000	−0.379	0.000	−0.311	0.000	0.270	0.000	0.212	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	
R^2	33.03%		30.09%		30.37%		23.26%		23.89%		45.01%	

The table reports the difference-in-differences regressions results where the dependent variables are the measures of algorithmic trading. *Treatment* is an indicator variable for a firm in the treatment group that experienced an increase in tick size. *Post* is an indicator variable for the post-treatment period that is between October 2016 and September 2018. p-values are based on standard errors clustered at the industry and quarter level.

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
Industry-adjusted number of patents	−0.014	−1.244	4.703	−2.231	0.425
$nValue	8.177	5.939	8.671	2.939	10.827
$rValue	3.364	2.441	3.525	1.217	4.485
Originality	0.822	0.979	0.555	0.911	0.994
R&D	0.059	0.020	0.075	0.000	0.103
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. Panel A reports the measures of innovation as captured by patents. *Number of patents* is the total number of patents a company applied for in a quarter that were ultimately granted. *Number of citations* is the total number of citations the granted patent made counted up till December 31st, 2019. *Industry-adjusted number of patents* is the number of firm patents less the mean patent count for the Fama-French industry the firm belongs to calculated for each year-quarter. *$nValue* is the Kogan et al. (2017) value of innovation in millions of nominal dollars calculated for each firm-year-quarter. *$rValue* is the Kogan et al. (2017) value of innovation in millions of dollars deflated to 1982 using the CPI calculated for each firm-year-quarter. *Originality* captures how many previous patents an invention draws on to produce a novel idea. *R&D* is the ratio of research and development expenditures for the previous quarter scaled by total sales calculated for each firm-year-quarter. Panel B reports descriptives statistics for control variables that we define in Appendix A.

Table 4. Pre-treatment means for the variables and test of parallel trend

	Treatment	Controls	Difference	t-test	p-value
Panel A: Pre-treatment means for patent counts and citations					
Number of patents	4.025	3.497	0.528	0.900	0.391
Number of citations	6.683	6.766	−0.083	−0.580	0.575
Industry-adjusted number of patents	0.096	−0.123	0.219	−1.060	0.288
$nValue	6.228	6.512	−0.284	1.140	0.255
$rValue	2.617	2.702	−0.085	0.470	0.638
Originality	0.815	0.830	−0.016	−0.380	0.704
R&D	0.054	0.063	−0.009	0.260	0.795
Panel B: Pre-treatment means for control variables					
MV	951.500	961.200	−9.700	−0.140	0.893
ROA	−0.016	−0.031	0.015	0.300	0.772
Leverage	0.437	0.406	0.031	1.420	0.189
Cash	−0.006	−0.022	0.016	0.310	0.764
B/M	0.457	0.421	0.036	0.880	0.402
IO	0.706	0.676	0.030	0.910	0.384

	Coefficient	p-value
Panel C: Test of parallel trends for the number of patents		
Intercept	0.943	0.000
Pre_Sept2015×Treatment	−0.004	0.940
Pre_March2016×Treatment	−0.044	0.552
Pre_Sept2016×Treatment	−0.045	0.275
Pre_Sept2015	0.023	0.728
Pre_March2016	0.024	0.720
Pre_Sept2016	0.069	0.238
Post×Treatment	−0.084	0.094
Post	−0.086	0.128
Treatment	0.086	0.162
Quarter effects	Yes	
Industry effects	Yes	
N	3954	
R^2	12.01%	

Panel A presents pre-treatment means for the dependent variables separately for the treatment and control firms. We also report the difference in means and the corresponding t-test and p-value. Panel B reports means for the control variables and their difference between treated and control stocks. Panel C tests the parallel trend assumption that there is no difference in innovation levels between treated and control firms before TSP. *Pre_Sept2015* is an indicator variable for the pre-treatment period that is between March 2015 and September 2015. *Pre_March2016* is an indicator variable for the pre-treatment period that is between October 2015 and March 2016. *Pre_Sept2016* is an indicator variable for the pre-treatment period that is between April 2016 and September 2016. p-values are based on standard errors clustered at the industry and quarter level.

Table 5. The relation between AT and corporate innovation

	log 1+number of patents		Firm-fixed effects		industry-adjusted number of patents		log 1+R&D	
	Coeff	p	Coeff	p	Coeff	p	Coeff	p
Panel A: Main analysis								
Intercept	0.966	0.000			−0.558	0.028	0.598	0.058
Post	−0.132	0.001	−0.153	0.011	−0.163	0.206	0.018	0.130
Treatment	0.060	0.142			0.241	0.178	0.015	0.632
Post×Treatment	−0.053	0.004	−0.037	0.017	−0.439	0.022	-0.041	0.004
Controls	Yes		Yes		Yes		Yes	
Quarter effects	Yes		Yes		Yes		Yes	
Industry effects	Yes		No		Yes		Yes	
Firm effects	No		Yes		No		No	
N	3954		3954		3954		2787	
R^2	12.34%		74.47%		4.18%		41.13%	

	AT factor		log 1+number of patents	
	Coeff	p	Coeff	p
Panel B: Intensity of AT				
Intercept	8.314	0.006	−0.156	0.033
Post	−2.149	0.000	−0.062	0.001
Treatment	0.066	0.933	0.034	0.129
Post×Treatment	−3.773	0.000	−0.035	0.099
Treatment×TR_Intensity	1.975	0.126	−0.022	0.502
Post×Treatment×TR_Intensity	−3.342	0.010	−0.054	0.057
Controls	Yes		Yes	
Quarter effects	Yes		Yes	
Industry effects	Yes		Yes	
Firm effects	Yes		Yes	
N	3954		3954	
R^2	11.06%		10.26%	

	AT factor		log 1+number of patents	
	Coeff	p	Coeff	p
Panel C: Placebo test				
Intercept	−0.230	0.928	0.768	0.000
Placebo_Post	0.228	0.840	−0.048	0.234
Treatment	2.055	0.342	0.063	0.146
Placebo_Post×Treatment	−2.183	0.296	0.009	0.635
Controls	Yes		Yes	
Quarter effects	Yes		Yes	
Industry effects	Yes		Yes	
N	4768		4768	
R2	6.71%		15.81%	

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	Future ROA		Future Cash/Assets	
	Coeff	p	Coeff	p
Panel D: Future financial performance				
Intercept	−0.044	0.000	−0.030	0.000
Post	0.004	0.108	0.006	0.137
Treatment	0.001	0.411	0.005	0.115
Post×Treatment	−0.005	0.099	−0.009	0.033
Controls	Yes		Yes	
Quarter effects	Yes		Yes	
Industry effects	Yes		Yes	
N	3499		3499	
R2	61.4%		63.0%	

Panel A reports Eq. (1) difference-in-differences regression results on the patent counts. 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 is the R&D intensity. Panel B reports regression results for Eq. (1) where we include a dummy variable for the intensity of trading restrictions among treated firms, *TR_Intensity*. *AT factor* measures the intensity of AT and is an index measure based on the six AT measures. We use the *AT factor* as the dependent variable in Eq. (1) and report regression results in column 'AT factor'. Panel C reports regression results for Eq. (1) when we define the pre-treatment period from January 2012 to September 2014 and the pseudo-treatment period from October 2014 to December 2016, *Placebo_post*. Panel D uses future quarterly mean ROA, *Future ROA*, and mean cash/assets, *Future Cash/Assets*, measured over six quarters relative to the current year-quarter as the dependent variables in Eq. (1). p-values are based on standard errors clustered at the industry and quarter level.

Table 6. The direction of the change in AT for treated firms

	AT = odd_lot		AT = trade_vol		AT = AT factor	
	Coeff	p	Coeff	p	Coeff	p
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×Treatment×decrease in AT	−0.122	0.000	−0.199	0.000	−0.272	0.000
Post×Treatment×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	
R^2	12.44%		12.46%		12.55%	

The table reports regression results for Eq.(1) where we identify the direction of change in AT for treated firms. *decrease in AT* is an indicator variable for a reduction in AT. *zero or increase in AT* is an indicator variable for a zero or increase in AT. To capture AT, we use the odd lot, trade to volume. and the AT factor measures. p-values are based on standard errors clustered at the industry and quarter level.

Table 7. The speed with which firms react to TSP

	Coeff	p
Intercept	−0.200	0.416
Post_Sept2017×Treatment	−0.039	0.161
Post_Sept2018×Treatment	−0.097	0.034
Post_Sept2017	−0.040	0.108
Post_Sept2018	−0.107	0.118
Treatment	0.033	0.550
Controls	Yes	
Quarter effects	Yes	
Industry effects	Yes	
N	3954	
R^2	10.20%	
Testing the hypothesis: Post_Sept2017×Treatment = Post_Sept2018×Treatment		
F-test	2.750	
p-value	0.097	

The table reports Eq. (1) results where we split the TSP period into subperiods. *Post_Sept2017* is an indicator variable for the early part of the post-treatment period that is between October 2016 and end of September 2017. *Post_ Sept2018* captures the period between October 2017 and the end of the TSP program at the end of September 2018. p-values are based on standard errors clustered at the industry and quarter level.

Table 8. Number of citations and KSPP innovation value measure

	Number of citations		Originality		KSPP real		KSPP nominal	
	Coeff	p	Coeff	p	Coeff	p	Coeff	p
Intercept	1.098	0.000	0.721	0.000	−0.872	0.084	−2.431	0.058
Post	−0.488	0.000	−0.001	0.831	0.068	0.747	0.443	0.381
Treatment	0.113	0.041	−0.001	0.874	−0.044	0.594	−0.099	0.622
Post×Treatment	−0.118	0.034	−0.013	0.088	−0.196	0.028	−0.494	0.028
Controls	Yes		Yes		Yes		Yes	
Quarter effects	Yes		Yes		Yes		Yes	
Industry effects	Yes		Yes		Yes		Yes	
N	3954		3954		2737		10291	
R^2	9.36%		12.32%		11.59%		2.16%	

The table reports regression results for Eq.(1) where the dependent variable is the log 1+number of citations, the measure of patents' average originality, and the Kogan et al. (2017) measures of the private economic value of patents calculated in real and nominal terms. p-values are based on standard errors clustered at the industry and quarter level.

Table 9. Cross-sectional analysis

	X=% stock compensation		Probability of forced turnover X=−1×abnormal returns		X=Low accruals		X= Composite high EQ measure	
	Coefficient	p-value	Std coefficient	p-value	Coefficient	p-value	Coefficient	p-value
Intercept	1.296	0.000			1.013	0.000	1.037	0.000
Post×X	0.018	0.618	0.117	0.337	0.056	0.681	−0.275	0.483
Treatment×X	0.084	0.041	0.165	0.138	−0.422	0.091	−0.952	0.028
Post×Treatment×X	−0.096	0.054	−0.235	0.023	0.204	0.033	0.760	0.018
X	−0.024	0.477	−0.218	0.106	−0.014	0.944	0.324	0.449
Uninteracted dummies	Yes		Yes		Yes		Yes	
Controls	Yes		Yes		Yes		Yes	
Quarter effects	Yes		Yes		Yes		Yes	
Industry effects	Yes		Yes		Yes		Yes	
N	2166		3942		3954		3836	
R^2	11.69%		14.44%		17.15%		16.98%	

The table presents abbreviated results for Eq. (1) augmented with interaction terms capturing the intensity of managerial stock compensation, high earnings quality measured by accruals and by a composite earnings quality measure. *% stock compensation* is the ratio of stock-based to total compensation. Column 'Probability of forced turnover' reports regression results for a model predicting the likelihood of a forced CEO turnover next quarter as a function of cumulative abnormal returns measured over 180 days before the quarter-end where the normal return benchmark is the S&P500 index. We multiple the cumulative abnormal returns by −1 so that higher values capture more negative return performance. We report standardized coefficients in column 'Std coefficient' where all variables are standardized to a mean of zero and unit standard deviation. Low accruals are firm total accruals multiplied by −1 so that higher values indicate higher earnings quality. Composite high EQ measure is an index measure of high earnings quality based on a principal component analysis of audit fees (weight 0.596), a dummy variable for restatement (weight −0.14), accruals (weight −0.595) and an indicator for whether the auditor is PCAOB registrant (weight 0.026). p-values are based on standard errors clustered at the industry and quarter level.

Table 10. Price discovery at the patent grant date

	$\frac{AR(0)}{CAR(-1,1)}$		CAR(1,60)		CAR(61,100)		CAR(101,140)	
	Coeff	p	Coeff	p	Coeff	p	Coeff	p
Intercept	0.295	0.000	−0.004	0.647	0.051	0.211	0.023	0.557
Post	0.024	0.051	0.005	0.146	−0.011	0.333	−0.011	0.294
Treatment	0.022	0.017	−0.001	0.677	−0.011	0.041	−0.011	0.220
Post×Treatment	−0.035	0.038	−0.009	0.063	0.020	0.075	0.017	0.304
Controls	Yes		Yes		Yes		Yes	
Quarter effects	Yes		Yes		Yes		Yes	
Industry effects	Yes		Yes		Yes		Yes	
N	9565		9565		9565		9565	
R^2	0.15%		0.27%		2.06%		1.26%	

Column $\frac{AR(0)}{CAR(-1,1)}$ reports results for Eq. (1) where the dependent variable is the ratio of the patent grant 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) four-factor model as the normal return benchmark. p-values are based on standard errors clustered at the industry and quarter level.

Table 11. Tick Size Pilot and institutional ownership

	Y=Institutional ownership		Y=Transient		Y=Dedicated	
	Coeff	p	Coeff	p	Coeff	p
Panel A: The effect TSP has on institutional ownership						
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	
R^2	6.98%		6.32%		7.87%	

	Coefficient	p-value
Panel B: Interactions with dedicated ownership		
Intercept	0.939	0.000
Post×Dedicated	0.251	0.241
Treatment×Dedicated	−0.293	0.115
Post×Treatment×Dedicated	0.126	0.643
Dedicated	0.102	0.487
Post	−0.149	0.000
Treatment	0.072	0.007
Post×Treatment	−0.058	0.036
Controls	Yes	
Quarter effects	Yes	
Industry effects	Yes	
N	3815	
R^2	12%	

Panel A 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. Panel B reports results for Eq. (1) when we include interactions with ownership by dedicated investors. p-values are based on standard errors clustered at the industry and quarter level.

Table 12. 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	
R^2	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. p-values are based on standard errors clustered at the industry and quarter level.