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Algorithmic trading and changes in firms' equity capital

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Abstract

We use a large sample from 2001 – 2009 that incorporates intraday transactions data from 39 exchanges and an average of 12,800 different common stocks to assess the effect of algorithmic trading (AT) on firms' capital raising activities. Greater AT intensity reduces net equity issues over the next year, but this is only partly driven by AT's effect on proceeds from new securities issues. Our findings suggest that the main driver of this relationship is AT's effect on share repurchases.

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1. Introduction

Algorithmic trading (AT) and high frequency trading (HFT) account for most of today's trading activity and are of current interest to market participants and regulators. Unfortunately, empirical evidence on these traders' strategies and the attendant effects on markets is still limited. Yet, regulators around the world debate whether AT/HFT should be regulated and place increasing scrutiny on automated and high frequency order submission strategies and their effects on markets. Existing research agrees broadly that AT/HFT supplies liquidity to the market most of the time, but not always and not for all stocks; it disagrees on the effect of AT/HFT on volatility; and it is moot on longer-term effects of AT/HFT, such as a potential impact on firm's ability to raise new capital. This is the question we seek to address in this paper.

Greater algorithmic trading (AT) intensity appears to increase intraday price volatility (Boehmer, Fong, and Wu, 2012, "BFW"). Several traders, especially liquidity providers, dislike volatility because it increases the adverse selection risk associated with limit order strategies, the typical way of supplying liquidity. This can discourage liquidity suppliers and arbitrageurs. If the elevated volatility and the potentially associated reduction in liquidity persist over time they can adversely affect a firm's ability to raise new capital. For example, if greater AT intensity crowds out lower-frequency liquidity suppliers, the remaining liquidity supply may become highly correlated with the presence of HFT. This increases the risk that liquidity will dry up during periods of market stress, and BFW (2012) find indicative evidence to support this assertion. Lower or more volatile liquidity, in turn, could then discourage long-term investors who would otherwise invest in the stock market, and it would become more expensive or otherwise more difficult for firms to raise new capital. Moreover, greater AT can also affect non-equity sources of capital. For example, greater volatility in equities could make firms' debt more risky and hence increase their cost of debt. On the other hand, greater AT intensity could attract other investors, whether high or low frequency, who were not in the market previously. Overall, it seems quite likely that high levels of AT could change the nature of liquidity supply, and with it the willingness of long-term investors to provide capital to firms. But the net effect of AT on stock markets' role in capital formation is an empirical question that we seek to address in this project. We use a 9-year international panel and an AT metric based on the intensity of message traffic that we relate econometrically to subsequent changes in firm capital.

Financial economists distinguish between agency and proprietary trading algorithms. Agency algorithms typically manage the timing, target execution venue, and the size of order submissions associated with investment decisions. For example, an index fund can use an algorithm that minimizes execution costs for a portfolio transition that is necessary because of a change in the composition of its reference index. Proprietary algorithms are not necessarily associated with investment decision and instead seek to profit from the trading environment itself. One example would be a market-making strategy that supplies liquidity to buy-side traders and possibly to agency algorithms. Another example includes arbitrage strategies that seek to exploit historical patterns and eliminate temporary mispricing in the market. Yet another example would be order anticipation strategies that seek to predict buy-side order flow and profit from initially trading in the same direction, and then reversing trade direction once the buy side trader begins executing his order and the associated price impact begins to move price. Finally, HFT refers to a subset of proprietary algos that react to market updates or other events within milliseconds and typically exhibit extremely short holding periods (Hasbrouck and Saar, 2011). Mainly because of their overall importance, but also because HFT strategies are neither transparent nor well understood, there is substantial public policy interest in HFT.

Different algorithmic trading strategies can have quite different impacts on market quality. Agency algos typically take liquidity; market making algos provide liquidity; arbitrage algos take liquidity but make prices more accurate; and order anticipation algos increase the costs of trading for the order flow they are able to predict. The current interest in the consequences of AT and HFT arises largely from the lack of data on how frequently the different strategies are used. Unfortunately, available data sources do not generally permit researchers to differentiate between the different types of AT. While we have some evidence on the effect of AT on market quality, we do not yet understand whether orders generated by algos are correlated across stocks or over time and, as a result, pose the possibility of elevated systemic risk; or whether there are longer-term consequences, the question we address in this paper.

In this paper, we take a very basic but comprehensive approach that contributes new evidence to this debate. We follow Hendershott, Jones, and Menkveld (2010) and BFW (2012) and infer proxies for algorithmic trading (AT), a superset of HFT, from measures that are derived from the intensity of intraday message traffic. We use nine years of intraday security-level quote and trade data for 39 markets around the world. This new and comprehensive sample covers an average of 12,800 firms, excluding the U.S. in this draft, and allows us to exploit variation in algorithmic trading intensity in the cross-section of stocks and markets.

We have several objectives. First, we describe the relationship between algorithmic trading and firms' ability to raise new capital, measured in two ways. We use McLean, Pontiff, and Watanabe's (2009) measure of net new capital raised, which takes into account both new issues and equity repurchased by the firm. Our second set of measures is derived from actual proceeds from security sales, including both equity and debt securities. These measures rely on transactions reported in event time, and we aggregate both variables at annual horizon.

Second, in addition to analyzing the main effects of AT on long-term capital changes, we also assess the cross-sectional determinants of AT's effect. We believe that it is important to understand the cross-sectional determinants of the benefits and costs of greater AT intensity. Specifically, stocks that are larger in terms of market capitalization or have low volatility are typically also easier to trade. It is easier to provide liquidity in these stocks, and the algorithms that traders employ will differ substantially across stocks. In particular, high-frequency market making strategies are probably easier to implement in large stocks than in small, high volatility stocks. To address these issues, we divide stocks into terciles based on market cap and volatility within each market and allow the effects of AT to differ according to these characteristics.

Third, we contribute international evidence on a question where researchers have mainly relied on U.S. samples. Some empirical studies of HFT have looked outside the U.S. (see BFW, 2012; Hendershott and Riordan, 2009; and Menkveld, 2010), but all except BFW (2012) are based on relatively small samples. None of these studies attempts to disentangle the longer-term consequences of AT.

We find that greater AT intensity is, on average, associated with declines in equity capital in the next year. This result is only partly driven by a decline in new securities issues; rather, greater AT intensity is associated with an increase in repurchase activity. These results control for market capitalization, book-to-market, volatility, liquidity, and information asymmetry at the firm level, and for secular trends at the market level. We also show that these effects are concentrated in firms that are among the smallest third or the most volatile third of firms in a given market.

Our paper is organized as follows. We review the theoretical and empirical literature in the next section. In section 3, we discuss our data and define the key variables we use. We discuss our empirical design in section 4 and present our results in section 5. The final section concludes.

2. Literature on algorithmic and high frequency trading

Only in the 2000s have information technology and market structure developed into an arena that facilitates fast, automated trading. In the U.S., this is mainly a consequence of limit order display rules that were implemented beginning in the early the 1990s and, in particular, the implementation of Reg NMS in 2005. Other factors also play roles, including the NYSE's 2003 change to autoquote (mandatory automatic quote updates, as opposed to manual updates initiated by specialists), the development of fast markets that compete with the traditional venues, and the increase in capital available for proprietary trading. Other markets, including Europe, have also adapted trading protocols to facilitate HFT, mostly in the second half of the decade. Here, regulation also played a key role – MIFID, for example, provides a framework for off-exchange trading that set the stage for more AT (see Menkveld, 2010).

Despite being a young literature, analyses of HFT and algorithmic trading reveal an interesting dichotomy. Several theoretical and empirical models analyze HFT's effects on market quality measures, including execution costs, volatility, and informational efficiency. While theoretical models mostly predict negative (or mixed) consequences of having fast traders in the market, the average effects estimated in empirical results are often positive. We discuss this literature in the next two subsections.[1]

2.1. Theory

Hoffman (2010) extends Foucault's (1999) limit order market and allows algorithmic and human traders to compete. Their ability to react faster to new events allows algorithmic traders to evade the adverse selection that is associated with stale limit orders. In this model, the effect of introducing algorithmic traders has ambiguous effects on trading volume and the price impact of human traders, but it decreases the profits of human traders. Considering the overall effect, Hoffman shows that in most cases human traders are strictly worse off when algorithmic trading is widespread. Cartea and Penalva (2011) design a model with liquidity traders, market makers, and HFT. They find that HFT increase overall trading volume, but also volatility and the price impact of liquidity traders. Market makers come out even–they lose market share (and thus revenues) for liquidity provision to the HFT, but are compensated with higher rewards for their remaining liquidity supply. The cost for the higher rewards to market making, and for the greater revenues to HFT, are all born by the liquidity traders. McInish and Upson (2011) arrive at similar conclusions using a different mechanism. In their model, strategic fast traders are the first to learn about quote updates and use this privileged information to trade at stale prices with slow traders. Here, too, does HFT activity increase trading costs for (slow) liquidity traders. In Jarrow and Protter's (2011) model, HFT also observe order-flow information faster than other traders. They show that when demand curves are downward sloping, HFT's activity affects price and creates a temporary mispricing that HFT can profitably exploit. In this case,

[1] The remainder of this section borrows heavily from BFW (2012).

the detrimental effect lies in less efficient pricing in addition to a transfer from slow to fast traders.

A similar wealth transfer arises in an earlier model by Brunnermeier and Pederson (2005). They allow traders to follow order anticipation strategies ("predatory trading" in their model), a strategy that requires the ability to predict order flow in real time at high frequency and is easily implemented as a trading algorithm. Order anticipators attempt to predict large uninformed orders and then trade ahead of these orders, in the same direction. This increases the costs for the large liquidity trader, who will end up trading at relatively inferior prices, perhaps even with the order anticipator. Brunnermeier and Pederson show that this leads to price overshooting and that it withdraws liquidity from the market when it is most needed (by the large trader). As a result, a wealth transfer occurs from the large liquidity trader to the order anticipator. Moreover, they show that the low-liquidity event can trigger systemic liquidity shocks for other traders and markets, thereby multiplying the negative consequences the order anticipator imposes on the market.

The models discussed so far generally predict higher costs to uninformed and/or slow liquidity traders in the form of a greater price impact and pre-trade information leakage. Greater execution costs essentially involve a wealth transfer from slow to fast traders, but this does not necessarily have welfare implications. Biais, Foucault, and Moinas (2010) make an elegant argument in this regard. They show that HFT can generate gains either from trade or from adverse selection, which would arise from their faster access to information. But a social planner would only consider gains from trade, not from adverse selection. As a result, HFT overinvest in technology, which leads to socially undesirable outcomes. Overall, existing theoretical models agree that HFT has undesirable consequences for liquidity traders, informational efficiency, and volatility, and these effects may well result in lower social welfare. Jovanovic and Menkveld (2011) also study welfare implications of high frequency trading. In their model, middlemen intermediate between fast limit order and slow market order traders. Depending on parameter values, their entry may increase or decrease trading volume, and also has a mixed effect on welfare.

2.2. Empirical studies

The recent spread of HFT has spurred a number of empirical studies that examine its consequences. Their inferences are easiest to synthesize by first categorizing the type of data that each study uses. To date, there is no academic study of equity trading that uses data where the researcher can directly identify trader-level order submission strategies and their consequences for algorithmic or HF traders, either over time or across stocks. Researchers follow one of two approaches: infer the portion of algo/HF trading from intraday data; or use data where HF traders are identified as a group. We discuss advantages and disadvantage of both approaches below.

The most basic approach uses standard intraday transactions data and either develops proxies for HFT, or infers their actions from the speed with which traders react to market events. On the downside, these approaches do not exactly measure HFT or AT– instead, they infer it from the data with relatively unknown consequences for the quality of inference. But the advantage to these approaches is that they permit construction of broad and long panels that allow fairly general inferences. We adhere to this approach in this article, and closely follow Hendershott, Jones, and Menkveld (2010) and BFW (2012) in using message counts as a proxy for AT activity. Hasbrouck and Saar (2011) and Egginton, VanNess, and VanNess (2010) instead infer HFT activity from periods of apparent high-frequency activity. The former identify episodes of orders that react within milliseconds to market updates. The latter examine high-activity intervals, defined as one-minute periods where the quotes-per-minute count exceeds a historical average by 20 standard deviations (and the trading day as a whole is not too different, defined as being less than two standard deviations away from the mean). These samples lose breadth relative to the message-count sample, but are able to study periods where HF activity takes actually place.

The second category of data provides summary information about the type of trader. For example, Brogaard (2010, 2011a) and Hendershott and Riordan (2011) use a 2008–2009 Nasdaq sample that reveals the aggregate order flow generated by 26 HFT firms that account for about three quarters of sample trading volume. Here, the advantage is that actual HFT can be observed for a random sample of 120 stocks. Potential drawbacks include the selection of HFT firms, which have been picked by the exchange that provided the data and, presumably, have been willing to have their order flows disclosed to academics. Because HF strategies are typically considered sensitive both from a legal and competitive perspective, this selection process could conceivably result in orders that are more benign than a random sample of HFT orders. There are also other potential issues that complicate inference from this dataset. First, the sample of 26 HFT firms does not include any of the large proprietary trading desks that, allegedly, are responsible for a substantial portion of HFT. Second, we do observe orders that the sample traders have submitted to other markets. Third, the high percentage of trading volume of those 26 firms is of some concern. High trading volume is not necessarily representative of HF traders – instead, they are typically characterized as traders with relatively low volume, but a very high ratio of order messages to trades (see Kirilenko et al., 2011; Hasbrouck and Saar, 2011; and Hendershott, Jones, and Menkveld, 2011).[2] Overall, while these data are currently the most informative about HFT strategies in equities, they also have significant shortcomings that complicate inference.

In summary, the broadest data, which in principle would allow the strongest inferences, makes the least clear distinction between HF, algorithmic, and slow trading. In the other extreme, data sets that identify actual HF activity tend to be either small or not necessarily representative for other reasons. Moreover, some of these available data sets are subject to endogeneity concerns, because it is generally not easy to identify whether causality goes from market quality to HFT activity, of from HFT activity to market quality.

Against these basic concerns about the data, most but not all results document positive effects of HFT. Hendershott, Jones, and Menkveld (2010) show that algorithmic trading is associated with better liquidity and faster price discovery. They use the 2003 introduction of autoquote at the NYSE as an instrument to argue that algorithmic trading causes these market quality improvements. Brogaard (2010, 2011a, 2011b) uses the 2008–2009 Nasdaq-selected data on

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[2] The sample used by Hendershott and Riordan (2009) also falls into this category but it is not subject to a selection concern. They use a short sample of exchange-classified algorithmic trades at Deutsche Boerse. Also similar is Menkveld's (2010) sample, which uses brokerage identities to infer the trades by a single HFT in the European market. These samples allow inferences about algos and HFT, respectively, but are relatively narrow.

We also note that the Nasdaq sample does not capture all volume in Nasdaq-listed stocks, which trade on multiple venues. Therefore, the high percentage of (Nasdaq) trading volume that this sample represents is not inconsistent with proprietary trading desks being excluded from the sample. Proprietary desks do not necessarily report trades to Nasdaq and may instead execute on venues that report, for example, to the National Stock Exchange.

26 HFTs and shows ambiguous effects on volatility, but improvements in liquidity. Based on HFT activity inferred from millisecond-level responses, Hasbrouck and Saar (2011) find improvements in volatility, spreads, and depth when these fast traders are active. Using the same data as Brogaard does, Hendershott and Riordan (2011) document that HFT play an important role in price discovery. Additionally, for a much smaller Deutsche Boerse sample that is not subject to selection concerns, Hendershott and Riordan (2009) find that algorithmic trading makes prices more informative.

BFW (2012) also find that AT improves liquidity and efficiency, but show that the positive effect on liquidity is limited to AT in stocks that are large, high-priced, or volatile. They also show that AT is less likely to include liquidity providing strategies when market making is particularly costly and document that AT results in significantly higher volatility, especially for small stocks and stocks whose returns are already volatile.

Several other studies also find predominantly negative consequences of AT or HFT. Kirilenko et al. (2011) argue that HFT worsened (but did not cause) the May 6, 2010 Flash Crash. Because this is the only study that can see exactly what HFT do, it carries significant weight among the empirical work we have so far. Dichev, Huang, and Zhou (2011) find that trading per se generates excess volatility, suggesting that HFT can lead to undesirable levels of volatility. Hasbrouck and Saar (2009) are the first to document the "fleeting" nature of many limit orders in electronic markets, and question the traditional view that limit orders provide liquidity to the market. This argument raises questions about the quality or usefulness of often short-lived liquidity that HFT supply. Consistent with this concern, Egginton, VanNess, and VanNess (2011) show that periods of extremely active quoting behavior are associated with degraded liquidity and elevated volatility. Importantly, they show that such episodes are surprisingly frequent. While there are good economic reasons for such quote-bunching to occur as a benign by-product of HF liquidity provision, as Hasbrouck and Saar (2011) argue, it is also possible that it arises as a consequence of intentional "quote stuffing." McInish and Upson (2011) examine trading around quote changes and compare fast and slow responses. They find that fast traders strategically leave stale orders on the book and that slow traders often interact with these at prices that are inferior to those available elsewhere. Finally, Chaboud et al. (2009) look at HFT in the foreign exchange market and document that the correlation among algorithmic "machine" orders is much higher than the correlation among "human" orders. This raises questions regarding the contribution of algorithms to the transmission of systemic risk.

Overall, AT appears to have strong effects on other market participants and the markets they trade in. Some studies document that AT provide liquidity, but large-sample studies show that this may not apply to all firms and not every day. Therefore, the increase in volatility that is also associated with AT could have real additional effects on markets, especially if it were to drive away potential liquidity suppliers. Liquidity is one important reason why firms access the market—secondary market trading allows some firms to sell public securities at a better price than they would receive for a private placement without a liquid secondary market. If liquidity declines as a result of elevated volatility, these firms may more frequently prefer to raise capital elsewhere—either in other countries or other markets, such as the private-placement market. Our study is designed to detect such potential effects on firms' ability to raise capital.

3. Data

We obtain intraday quotes and trades from the Thomson Reuters Tick History (TRTH) database. Our initial sample includes all non-U.S. common stocks covered in the database. We identify the primary listing market for each of these stocks and drop trading in stock that takes place on all other markets. This filter produces stocks trading on 40 primary equity exchanges in 37 countries.[3] We obtain data on daily returns, daily high and low prices, trading volume, security details and financial statement data from Datastream and WorldScope. We extract proceeds on securities sales by all sample companies from Thomson Reuters SDC Platinum New Issues database. We obtain details on markets and trading protocols from Reuter's Speedguide and the Handbook of World Stock, Derivative and Commodity Exchanges. Details on the sources and the construction of the data set are in BFW (2012).

For inclusion in our final sample we impose a few additional data requirements. We drop Ireland, where fewer than 30 stocks are listed prior to 2008. We exclude stocks that have data for fewer than 21 trading days during the sample period. For the average year, our sample includes about 12,800 firms and we have substantial variation in the number of firms across markets. Over the sample period, the number of listed firms increases by 140% and, in 2009, represents an aggregate market capitalization of USD 16.7 trillion.

3.1. Variables

Our objective in this analysis is to measure the effect of algorithmic trading on long-term capital raising activities. We use two approaches – one approach that incorporates both new issues and repurchases, and another that uses the actual proceeds from new issues. We describe these variables in this section, along with our proxies for algorithmic trading activity.

Net new equity (NNE)

Following McLean, Pontiff and Watanabe (2009), we construct net new equity (NNE), a measure that captures the change in the number of shares outstanding adjusted for stock splits and distribution events such as stock dividends and share repurchases. We use the capital adjustment index from Datastream, recorded at the end of year t (CAI_t), to calculate the number of actual shares outstanding for that year (Shs_t). The CAI is the cumulative product of the inverse of the individual-period capital adjustment factors. NNE is then computed as $NNE_t = Ln(AdjShs_t) – Ln(AdjShs_{t-1})$, where $AdjShs_t = SharesOutstanding_t / CAI_t$.

Proceeds from the sale of securities

We obtain the currency value of all non-IPO securities issues covered in SDC Platinum. We separate the value into equity and non-equity ("other") issues and divide each by total assets. In contrast to NNE, which is a measure of net issuance, the SDC variables represent actual amounts raised in the capital market.

Algorithmic / high frequency trading

High frequency algorithmic activity is generally associated with fast order submissions and cancellations (see Hasbrouck and Saar, 2011). The proxy used by Hendershott, Jones, and Menkveld (2010) and BFW (2012) reflects this concept and we follow these studies and use

[3] China has three exchanges (Hong Kong, Shenzhen, and Shanghai), Japan has two (Osaka and Tokyo), all other countries have exactly one exchange included in the sample. Due to data restrictions on SDC new issues, we have no data on Tokyo, Osaka, and Toronto and exclude these markets from all SDC analysis.

AT, the negative of trading volume in USD100 divided by the number of messages, as our proxy for algorithmic trading activity. It represents the negative of the dollar volume associated with each message (defined as either a trade or a quote update). An increase in this measure reflects an increase in algorithmic activity.[4]

3.2. Descriptive statistics

BFW (2012) document a steep increase in both the number of sample firms and the frequency of message traffic over the sample period. Table 1 presents descriptive statistics for the variables that are of main interest in the current analysis. We first compute time-series means and standard deviations for each firm and then average across firms within each market. We report the mean and median of these market-specific means, and the average standard deviation in Table 1.

The average annual NNE is 0.057, which means that the average increase in net equity is about $e^{0.057}$–1=5.9% per sample year. This implies that the typical sample firm issues more equity than it repurchases. The mean AT is –5.45, implying an average trade size of $545 per trade or quote change message. During a typical year, the average firm issues new securities that represent 5% of the firm's assets. Equity issues alone account for about two thirds of this amount, or 3.5% of assets per year. While the coverage of the financial statement data that underlies NNE is not identical to the coverage of SDC that underlies issuance proceeds, comparing proceeds from equity in Panel B to the net new issues in Panel A suggests that the sample firms, on average, reduce their holdings of treasury shares (i.e., have negative repurchases). This is consistent with anecdotal evidence from the post-crisis periods in our sample (following the 2000 market crash and following the 2007–2008 crises).

4. Methodology

Our objective is to identify whether algorithmic trading intensity is related to the amount of capital firms raise in the subsequent year. We have no theoretical guidance on when a given level of AT should affect issuance activity. Here, we assume that the impact, if any, takes place during the next year. This approach minimizes the noisiness that would enter the estimation with longer intervals, and provides a conservative lower bound for the potential effects of AT. Results based on quarterly estimation, where possible, are qualitatively similar to the annual results.

We control for variables that are likely to affect a firm's propensity to issue new securities, including firm size (measured as market capitalization of equity), book-to-market, past returns, volatility (measured as the intraday high price minus low price, divided by the closing price), and liquidity (relative effective spreads). We further separate RES into a temporary component, the relative realized spread (RRS) and a permanent component, the price impact (RPI). Price changes tend to be permanent if they are based on new information, so we can interpret RPI

[4] Our measure of AT differs in an important way from the one used by HJM, who have access to order-level messages. For our world-wide sample, we only have access to a subset of these messages. We only observe each exchange's best quotes and trades, rather than all order-related messages. This means that we cannot directly capture one important dimension of HFT activity, the high ratio of order (submission and cancel) messages to trades that is so characteristic of many popular HFT strategies. But the HFT strategies that are mentioned, for example, in the SEC 2010 concept release involve activity at the BBO, rather than behind it. Therefore, we believe that HFT activity in our BBO-trade data set is highly correlated with HFT activity in an order-trade data set.

as a measure of information asymmetry. RRS can be interpreted as the return to liquidity providers. All variables are averaged during a quarter or year, depending on the model specification. To address autocorrelation in the errors we also include the lagged dependent variable. In all regression, we standardize each period's cross-section of firms to make coefficients comparable across countries.

We use two alternative estimation approaches. One approach uses panel estimation that relies on only eight time-series observations. We estimate this model with market and time fixed effects. The time fixed effects are incorporated implicitly as a by-product of the standardization that we perform within each market each period. Together, the fixed effects prevent us from interpreting systematic patterns in market quality across firms or secular patterns over time as the result of algorithmic trading. The second approach uses market-specific panel regressions with time fixed effects that we aggregate into one overall coefficient across the 39 markets. For the overall panel, we use dynamic Arellano and Bond (1991) standard errors for inference. The aggregated coefficients should be less susceptible to autocorrelated residuals (unless they lead to autocorrelated coefficients), and we compute simple cross-sectional t-statistics across the 39 markets in our sample. This approach should be conservative, because all inference is based only on the 39 market-specific observations. Specifically, we estimate a panel regression of the form

$$NC_{it} = \alpha_i + \gamma_t + \beta AT_{it} + \delta\_X_{it} + \varepsilon_{it,} \qquad (1)$$

where the α_i are market fixed effects, the γ_t are year fixed effects, AT is our proxy for algorithmic trading, and X is a vector of control variables. This vector includes the log of market value of equity, log book-to-market, average daily return, volatility, liquidity, and the lagged dependent variable. All explanatory variables are lagged by one year to ensure that they are predetermined.[5]

In part of our analysis, we explicitly allow the effect of AT to depend on cross-sectional firm characteristics such as market cap and 20-day return volatility. Unless stated otherwise, we determine each year, separately for each market, the lowest and highest tercile of firms based on the most recent 20 trading days. We assign "LOW" and "HIGH" dummies to firms in these terciles, respectively. We augment our regression model (1) with the two interactions between AT and each dummy. The interaction coefficients capture the potentially differential effect of AT on market quality in the LOW or HIGH terciles relative to the middle tercile. The total effect of AT for firms in the LOW tercile is given by the sum of the coefficient on AT and the coefficient on AT*LOW. We interpret results for the HIGH dummy analogously.[6]

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[5] We include the lagged dependent variable as a regressor to mitigate the effects of potential autocorrelation. But we do not specify an explicit dynamic model, which could affect the estimator's consistency and make the resulting estimates hard to interpret. As a robustness check, we re-estimate all reported regressions without lagged dependent variables. These estimates tend to have somewhat larger standard errors but they are qualitatively identical to those reported in this paper.

[6] An alternative approach to these cross-sectional tests would be to use the same breakpoints across all markets and periods. This would better capture the effect of algo strategies if strategies tend to be similar for firms of similar size. However, constant breakpoints would essentially classify firms by market, which would be undesirable.

5. Regression results

5.1. Net new equity (NNE)

We begin by investigating the relationship between McLean, Pontiff, and Watanabe's (2009) net new equity (NNE) and our proxy for AT. Panel A in Table 2 reports the results of an annual panel regression that pools firms, markets, and years. We estimate a significantly negative coefficient for AT of –0.033. This indicates that a one-standard-deviation increase in AT is associated with a 0.033 standard deviation decrease in NNE. Using the third column of Table 1, this amounts to a change in equity of –0.033*[exp(0.261)–1]=–0.98%, which implies an economically substantial decline in net equity. NNE aggregates new issues and repurchases, and we will estimate their individual coefficients below to see which component is affected most by AT.

Panel A also presents coefficients for the control variables. NNE is greater for firms that are small, perform poorly and have high volatility during the previous year. Higher transaction costs are not significantly related to NNE, but it is possible that AT is associated with the degree of asymmetric information about a firm. Depending on the strategy, traders may prefer or dislike firms with high asymmetry. To control for information flow, we decompose the effective spread into an information component (RPI) and a liquidity component (RRS). This regression is the second regression in Panel A. We find that firms with more information asymmetry tend to have lower NNE, perhaps because their cost of capital is higher. Naturally the effect of AT is virtually unchanged, because separating the liquidity variable into two components does not change the correlation between AT and liquidity. But now the interpretation is different: AT is associated with lower NNE, even when we control for the degree of information flow across stocks and years.

A caveat to the panel regression in Panel A is that we do not control for year-to-year autocorrelation, which may inflate standard errors. As an alternative, we present the average coefficient from market-specific panel regressions with time fixed effect regressions in Panel B. The results are quite similar to those in Panel A but have larger standard errors. The coefficients on AT have the same sign as those in Panel A and are still significant at the 5% level. We believe that this test has relatively low power, because the time-series consists of only eight years. Therefore, we find it reassuring that this method confirms the results from the tests in Panel A.

BFW (2012) find that the effect of AT on market quality differs depending on firm size and volatility. Thus, potential longer-term consequences can also differ along those dimensions. In Table 3, we estimate a model similar to (1) with the interactions between LOW/HIGH dummies and AT as additional independent variables. We construct the dummies within each market by sorting firms, each period, according to firm size (market cap of equity) and volatility (the standard deviation of daily returns). LOW represents the lowest tercile of firms; HIGH represents the highest tercile of firms according to the sorting variable.

The coefficients on the interactive variables measure how the effect of AT differs for firms in the low/high tercile from the effect of AT for firms in the middle tercile. Panel A, for example, reports pooled market-year fixed effect regressions as in Panel A of Table 2. The AT coefficient is –0.068 and means that an increase in AT significantly decreases NNE in middle-tercile firms. The LOW*AT coefficient is not significant, implying that the effect of AT is not different in small firms than in middle-tercile firms. But the HIGH*AT coefficient is positive and significant at the 5% level. The total effect of AT in large firms is –0.068+0.045=–0.023 – this is still negative, but the effect of AT is significantly less negative for net equity issues in large firms than in middle-tercile firms. Panel C tabulates the corresponding model using market-specific panel regressions, and the results are qualitatively identical.

We obtain a similarly convex relationship when we separately estimate the effect of AT across volatility terciles. As Panels B and D show, the effect is significantly negative in the middle tercile, but is much less pronounced for low-volatility firms and somewhat less pronounced also for high-volatility firms. The former are significant, but the marginal effects for HIGH volatility firms are much noisier. Moreover, none of the interactions is significant using the market-specific regressions in Panel D.

These results show that more AT is associated with lower NNE for small and mid-cap firms, and for firms in the middle volatility tercile. The likely reason for a smaller effect in the low and high volatility firms lies probably in the role of repurchases. When prices become too volatile, firms refrain from share repurchases because liquidity is more expensive, or trading too risky. When volatility is low, trading volumes are probably low as well so that firms cannot purchase larger quantities of shares without adversely moving prices.

5.2. Proceeds from securities issues

One limitation of NNE is that it is an aggregate measure of changes in equity. It does not differentiate between actual new securities issues and open-market repurchases (or sales of previously repurchased shares). In this section, we use a different measure that ignores net repurchases and is based exclusively on the proceeds from actual issues of new securities. In this analysis, we are interested in how AT affects the incidence of new securities issues. We include both primary and secondary issues, because it is not important whether the proceeds go to existing shareholders or to the firm—we would like to evaluate total new capital issues. We divide proceeds into those from equity and other securities. Other securities would include private equity placements, publicly traded debt, certificates, preferred stock, units, and convertible debt. As reported in Table 1, nearly two thirds of all issues in the SDC database are equity. For our estimation, we aggregate security issues at the calendar year level. This makes the results comparable to those in Table 2 and 3 and it is unlikely that securities issues move at higher frequencies.[7]

Our main results for these variables are in Table 4. Quite similar to our analysis of NNE, we find that more AT is associated with lower proceeds from new securities sales. The main effect of a one-standard-deviation increase in AT is a 0.021 standard deviation reduction in proceeds. From Table 1, this implies a decline in new issues by roughly –0.021*0.719=–1.51% of total assets; again, an economically significant reduction in new issues. When we decompose proceeds into equity and other, we see that the main impact is due to the change in equity proceeds. A one-standard-deviation increase in AT is associated with a 0.025*0.562=1.41% decline in new equity issues. The coefficient on "other" proceeds is also significant, but the economic effect is much smaller. The market-specific panel results in Panel B are qualitatively identical but a bit noisier. In Table 5, we separate the effects of AT along the market cap and volatility distributions. These estimates tend to be quite noisy and do not add significantly to the linear estimates from Table 4.

[7] In robustness tests we have used quarterly estimates for the tests that use the SDC variables. We obtain qualitatively identical results in these tests.

5.3. Estimated share repurchases

Repurchases are a sizeable component of net equity issuance. Firms repurchase their own shares when they consider prices to be below their fundamental value, when they perceive repurchases to have tax or other advantages over dividends, or when they have excess cash. Unfortunately, no precise measure of repurchase activity exists because under most jurisdictions repurchases neither need to be reported as they happen, nor are annual aggregates reported in financial statements. Instead, firms tend to report aggregate changes to shares outstanding, which could arise from repurchases, but also from sales, stock dividends, share compensation for employees, and similar activities. In this section, we approximately decompose NNE into an equity proceeds portion and a repurchase portion. We construct a proxy for repurchases and test its sensitivity to changes in AT. We compute repurchases, REP, as

$$REP_t = NNE_t - NewEquityShares_t / SharesOutstanding_{t-1}. \qquad (2)$$

Intuitively, we adjust the net change in adjusted shares by the number of new shares issued during the year over which NNE is measured. The result should be highly correlated with the fraction of shares outstanding that was repurchased during year t. REP is not a perfect measure for several reasons: the source of NewEquityShares (SDC) and NNE (Worldscope) may have different coverage of changes in shares outstanding, or come from different consolidation levels. Clearly NNE captures the aggregate change of all related activity, but SDC may not cover all events that are associated with changes in equity (such as shares issued as compensation, conversions of bonds or preferred stock to equity, or certain control transactions). Moreover, SDC does not carry data on Toronto, Tokyo, and Osaka. For these reasons, REP can overstate or understate true repurchases, and the coefficients of the components (new issues and repurchases) will not necessarily add up to the coefficient of NNE. Yet, there is no reason to believe that the mean change in REP differs systematically from the mean change in true repurchases at annual horizons.

Subject to these caveats, we regress REP on AT with controls as before and report the results in Table 6. We show that more intense AT increases repurchases significantly. A one-standard-deviation increase in AT is associated with a 0.018 standard deviation increase in repurchases. This suggests that the negative effect of AT on NNE (see Table 2) results only partly from a decline in proceeds from new securities issues—instead, AT increases repurchases, which in turn implies a decline in NNE. Moreover, the repurchase result also explains why proceeds from equity issues (Table 4) are less strongly related to AT than NNE (Table 2)—one interpretation is that the main channel through which AT affects NNE are repurchases rather than changes in new equity issues.

Panels B and C of Table 6 show that AT has the strongest effect on small-cap and mid-cap stocks and on stocks in the middle or high volatility tercile. In contrast, the aggregate effect of AT in large and low-volatility firms (the sum of the coefficients on AT and AT*dummy) is relatively weak. Combined with the findings that AT in large and low-volatility firms also has the weakest effect on NNE (Table 3), this is consistent with the view that repurchases are the main channel through which AT affects NNE.

One reason why repurchases increase could be the higher liquidity that is associated with AT. BFW (2012) show that AT increases liquidity in large and mid-cap stocks, but reduces liquidity in small-cap stocks. But because AT increases repurchase activity the most in small caps, higher liquidity cannot explain the increased repurchases for small-cap stocks. But it is possible that managers of mid-cap firms increase repurchases because their shares are more liquid as a result of AT activity.

6. Conclusions

We use intraday message traffic in 12,800 firms trading in 39 major stock markets to analyze the effects of algorithmic trading on these markets' ability to provide capital to firms. We cover a broad cross-section of stocks in these trading centers and create measures of algorithmic trading (AT) intensity and firm-level new capital issuance. We estimate the relationship between algorithmic trading and annual changes in capital. We also explore whether cross-sectional factors that are known to affect trading are related to the effect that AT has on new capital. All of our tests control for market capitalization, book-to-market, information asymmetry, liquidity, volatility, and persistence in the variable that measures changes in capital.

Taken together, our results show that more intense AT is associated with a decline in net new equity in the following year. This is partly driven by a negative effect that AT has on proceeds from new equity issues, but our results suggest that the main driver is an increase in share repurchases.

Our results are subject to two caveats. First, we use a time series of only nine years. Capital changes slowly, and nine years may not provide a full picture of the connections between AT and firms' desire and ability to raise capital in the stock market. We intend to update the analysis in a future revision, but the time series will remain relatively short. Second, and related to the first point, we cannot directly establish causality. This would be possible with either a suitable instrument that affects AT but not capital changes, or at least a longer time series that allows Granger causality tests. We currently have neither. BFW (2012) use co-location as an instrument for their daily analysis of AT's effect on market quality. Most co-location events take place in 2008 or later, however, so for our annual analysis here we do not have a sufficient number of observations after these events. Therefore, co-location is not a feasible instrument for this analysis.

Despite these caveats, however, we are confident that our results are not generated by reverse causality. Most importantly, it is not obvious how and why algorithmic traders would prefer trading (or even be able to identify) stocks that a year later become subject to more intense share repurchases and declines in net equity. Moreover, if a variable existed that links current AT to future equity changes, it would need to be unrelated to our current controls to make the reverse causality explanation work. We believe that this puts the bar quite high and consider the scenario where changes in AT cause changes in equity capital the more likely explanation.

Overall, BFW (2012) find that AT increases volatility at the firm level. In this analysis, we argue that an increase in volatility can discourage liquidity suppliers, which in turn could make it more costly for firms to raise new capital. Our results in this paper bear out this thought, in that we find a negative association between AT and changes in equity capital. Our findings suggest that the activity of algorithmic traders can have impact beyond the immediate trading environment and potentially affect the more fundamental functions of capital markets, such as the allocation of capital to firms.

Appendix: Results across geographic regions

In this appendix, we provide some descriptive evidence on the effects of AT on long-term capital raising in different geographical regions and particularly across several European markets. In Panel B of Table A1, we present the raw estimates for individual European countries. All models are annual market-specific year fixed effects panel models as in Table 2 Panel B. Significance levels in Panel B are Arellano and Bond (1991) standard errors. The aggregated coefficients in Panel A, representing Europe, Asia, and Other countries, are averages across markets as before. In Panel A, t-statistics are computed from the cross-section of market-specific coefficients for markets in that region. This is a low-power test as it uses only 16 observations for Europe, 11 for Asia, and 9 for other markets, and none of the region-wide averages differs significantly from zero. Statistical significance aside, more intense AT is associated with a decline in NNE, in aggregate proceeds, in equity proceeds, and an increase in repurchase activity in each region in Panel A of Table A1.

Across the European countries, Xetra and LSE, the most active markets in the region, show significant evidence that AT reduces net new equity (NNE). In Germany, part of this effect is explained by a significant positive relation between AT and REP. The Warsaw stock exchange, however, exhibits the opposite effect: more AT increases NNE, and this is partly driven by a decline in REP when AT becomes more intense.

Overall, this discussion reveals important differences across countries. We believe that the addition of more data will reduce standard errors to make the country level results more precise, especially because virtually all markets have experienced further increases in message traffic after 2009, when our sample period ends.

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Table 1: Descriptive statistics

This table reports aggregate summary statistics of net new securities issues. Net new equity issuance (NNE) is computed as in McLean, Pontiff and Watanabe (2009) at the annual frequency. All proceeds, and its components equity proceeds and other proceeds, are the annual aggregate value of securities issues (normalized by firm-level total assets at the time of issue, reported on SDC Platinum). AT, algo trading activity, is defined as the negative of dollar volume over message traffic. the remaining variables are the natural logarithm of the previous period's year-end market cap, Ln(ME), the natural logarithm of the previous period's Book-to-Market, Ln(B/M), the previous period's daily average return (Ret), the average daily price range (estimated as the difference between the highest and lowest transaction prices scaled by closing price), relative effective spread (RES), relative realized spreads (RRS), and price impact (RPI), all scaled by the closing price and measured in bp. The sample includes 39 markets from 2001 to 2009. The first two columns report the mean (median) of market-specific time series averages. The third column shows the average of market-specific time-series standard deviations.

	Mean	Median	Average standard deviation
NNE	0.057	0.048	0.261
All Proceeds	0.050	0.015	0.719
Equity proceeds	0.035	0.009	0.562
Other proceeds	0.016	0.004	0.240
AT	–5.448	–3.581	28.973
Ln(ME)	4.923	5.004	1.827
Ln(B/M)	–0.411	–0.562	0.938
Avg. daily Ret	–0.010%	–0.023%	0.648%
Price range	0.029	0.028	0.018
RES (in bp)	302	245	398
RRS (in bp)	194	140	339
RPI (in bp)	109	87	155

Table 2. Algo trading and net new equity issuance

This table reports annual regressions of net equity issuance (NNE) on algo trading and control variables. NNE is annual equity issuance computed as in McLean, Pontiff and Watanabe (2009). The independent variables are all measured in the prior year and include algo trading (AT), the natural logarithm of the year-end market cap (lnME), the natural logarithm of year-end Book-to-Market (Ln(B/M)), the daily average return (Ret), liquidity measured by the relative effective spread (RES), the temporary price impact measured by relative realized spreads (RRS), and the permanent price impact (RPI), all scaled by the closing price and measured in bp, volatility measured by daily price range, and the lagged dependent variable. Panel A reports market and year fixed effects regression results. Panel B reports mean coefficients of cross-sectional regressions within each market. All continuous variables are standardized to have a mean of zero and standard deviation of one every year within each market.

	Coef	t	%pos	Coef	t	%pos
Panel A: Market and year fixed effects panel regression						
AT	–0.033	–9.05	n/a	–0.033	–9.22	n/a
Ln(ME)	–0.042	–9.91	n/a	–0.046	–10.69	n/a
Ln(B/M)	0.002	0.53	n/a	0.002	0.50	n/a
Ret	–0.026	–7.46	n/a	–0.026	–7.39	n/a
DV_Lag1	0.041	13.63	n/a	0.041	13.61	n/a
Price Range	0.149	48.13	n/a	0.151	48.16	n/a
RES	–0.0003	–0.08	n/a			
RPI				–0.013	–3.51	n/a
RRS				0.006	1.80	n/a
Panel B. Aggregate of market-specific regressions						
AT	–0.025	–2.47	31%	–0.027	–2.68	36%
Ln(ME)	–0.040	–2.37	38%	–0.037	–2.28	41%
Ln(B/M)	0.035	3.28	72%	0.035	3.22	72%

	Coef	t	%pos	Coef	t	%pos
Ret	–0.026	–2.58	33%	–0.027	–2.61	36%
DV_Lag1	0.029	2.36	64%	0.028	2.32	62%
Price range	0.140	9.88	97%	0.140	10.01	95%
RES	–0.015	–1.31	38%			
RPI				0.003	0.36	54%
RRS				–0.013	–1.10	44%

Table 3. Algo trading, net new equity issuance and the cross-section of firms

This table reports annual regressions of net equity issuance (NNE) on algo trading and control variables. NNE is annual equity issuance computed as in McLean, Pontiff and Watanabe (2009). The independent variables are all measured in the prior year and include algo trading (AT), the natural logarithm of the year-end market cap (lnME), the natural logarithm of year-end Book-to-Market (Ln(B/M)), liquidity measured by the relative effective spread (RES), the temporary price impact measured by relative realized spreads (RRS), and the permanent price impact (RPI), all scaled by the closing price and measured in bp, the daily average return (Ret), volatility measured by daily average price range, and the lagged dependent variable. The High and Low dummies represent firms that are in the top and bottom market cap (return volatility) terciles within each market each year in Panels A and C (B and D). Panels A and B report market and year fixed effects regression results. Panels C and D report mean coefficients of cross-sectional regressions within each market. All continuous variables are standardized to have a mean of zero and standard deviation of one every year within each market.

	Coef	t	%pos	Coef	t	%pos
Panel A: Market and year fixed effects panel regression, interacting AT with market cap dummies						
AT	–0.068	–7.61	n/a	–0.068	–7.63	n/a
AT*Low	–0.016	–1.16	n/a	–0.015	–1.11	n/a
AT*High	0.045	4.62	n/a	0.045	4.56	n/a
Ln(ME)	–0.044	–10.06	n/a	–0.048	–10.78	n/a
Ln(B/M)	0.002	0.69	n/a	0.002	0.65	n/a
Ret	–0.027	–7.88	n/a	–0.027	–7.80	n/a
DV_Lag1	0.0410	13.52	n/a	0.041	13.49	n/a
PriceRange	0.151	48.48	n/a	0.152	48.50	n/a
RES	0.002	0.55	n/a			
RPI				–0.012	–3.14	n/a
RRS				0.008	2.23	n/a
Panel B: Market and year fixed effects panel regression, interacting AT with return volatility dummies						
AT	–0.050	–7.97	n/a	–0.051	–8.07	n/a

	Coef	t	%pos	Coef	t	%pos
AT*Low	0.025	3.49	n/a	0.025	3.46	n/a
AT*High	0.017	1.89	n/a	0.018	1.99	n/a
Ln(ME)	–0.043	–9.95	n/a	–0.046	–10.73	n/a
Ln(B/M)	0.002	0.68	n/a	0.002	0.66	n/a
Ret	–0.028	–7.97	n/a	–0.028	–7.89	n/a
DV_Lag1	0.041	13.56	n/a	0.041	13.53	n/a
PriceRange	0.149	47.81	n/a	0.150	47.86	n/a
RES	0.000	–0.11	n/a			
RPI				–0.013	–3.51	n/a
RRS				0.006	1.77	n/a
Panel C: Aggregate of market-specific regressions, interacting AT with market cap dummies						
AT	–0.108	–3.64	13%	–0.116	–3.62	13%
AT*Low	–0.063	–1.66	51%	–0.065	–1.70	51%
AT*High	0.087	3.35	72%	0.092	3.29	74%
Ln(ME)	–0.054	–3.39	26%	–0.052	–3.31	26%
Ln(B/M)	0.037	3.47	72%	0.037	3.40	72%
Ret	–0.029	–2.84	28%	–0.029	–2.87	33%
DV_Lag1	0.027	2.19	62%	0.026	2.15	62%
PriceRange	0.140	9.46	92%	0.140	9.45	92%
RES	–0.011	–0.95	41%			
RPI				0.005	0.63	56%
RRS				–0.011	–0.86	44%
Panel D: Aggregate of market-specific regressions, interacting AT with volatility dummies						

	Coef	t	%pos	Coef	t	%pos
AT	–0.070	–2.34	28%	–0.073	–2.29	28%
AT*Low	0.049	1.65	69%	0.050	1.64	67%
AT*High	0.031	0.91	46%	0.024	0.78	46%
Ln(ME)	–0.040	–2.45	36%	–0.037	–2.35	38%
Ln(B/M)	0.035	3.26	72%	0.034	3.19	72%
Ret	–0.027	–2.72	38%	–0.028	–2.74	38%
DV_Lag1	0.028	2.28	62%	0.027	2.25	59%
RES	–0.014	–1.18	38%			
PriceRange	0.140	9.64	97%	0.141	9.73	92%
RPI				0.004	0.47	59%
RRS				–0.012	–1.02	46%

Table 4. Algo trading and proceeds from securities issues

This table reports annual regression of capital raising on algo trading and control variables. The dependent variables are total annual proceeds from securities issues, proceeds from public equity issues, and proceeds from other securities issues, each scaled by total assets. The independent variables are all measured in the prior year and include algo trading (AT), the natural logarithm of the year-end market cap (lnME), the natural logarithm of year-end Book-to-Market (Ln(B/M)), the temporary price impact measured by relative realized spreads (RRS) and the permanent price impact (RPI), both scaled by the closing price and measured in bp, the daily average return (Ret), volatility measured by price range , and the lagged dependent variable. Panel A reports market and year fixed effects regression results. Panel B reports mean coefficients of cross-sectional regressions within each market. All continuous variables are standardized to have a mean of zero and standard deviation of one every year within each market.

	All Proceeds			Equity proceeds			Other proceeds		
	Coef	t	%pos	Coef	t	%pos	Coef	t	%pos
Panel A: Market and time fixed effects panel regression									
AT	–0.021	–4.92	n/a	–0.025	–5.65	n/a	–0.012	–2.58	n/a
Ln(ME)	–0.013	–2.55	n/a	–0.030	–5.66	n/a	0.013	2.22	n/a
Ln(B/M)	–0.056	–15.25	n/a	–0.050	–13.11	n/a	–0.042	–10.25	n/a
Ret	0.004	1.00	n/a	–0.012	–2.64	n/a	0.018	3.83	n/a
DV_Lag1	0.076	21.30	n/a	0.049	13.63	n/a	0.080	20.16	n/a
Price range	0.071	18.85	n/a	0.070	17.88	n/a	0.043	10.31	n/a
RPI	–0.009	–2.02	n/a	–0.012	–2.56	n/a	0.003	0.68	n/a
RRS	–0.004	–1.05	n/a	–0.010	–2.26	n/a	–0.004	–0.92	n/a
Panel B. Aggregate of market-specific regressions									
AT	–0.023	–2.18	33%	–0.022	–2.17	28%	–0.006	–0.91	44%

	All Proceeds			Equity proceeds			Other proceeds		
Ln(ME)	–0.014	–0.92	47%	–0.032	–2.07	39%	0.001	0.04	50%
Ln(B/M)	–0.051	–6.21	8%	–0.043	–5.16	19%	–0.027	–3.57	25%
Ret	–0.004	–0.49	50%	–0.023	–1.79	36%	0.032	3.67	75%
DV_Lag1	0.061	3.89	83%	0.039	3.01	69%	0.075	3.37	66%
Price range	0.074	6.03	92%	0.079	6.48	89%	0.042	5.04	84%
RPI	–0.009	–0.77	39%	–0.016	–1.19	39%	0.002	0.14	44%
RRS	–0.027	–2.29	22%	–0.035	–2.91	28%	–0.023	–2.38	25%

Table 5. Algo trading, proceeds from securities issues and the cross-section of firms

This table reports annual regressions of capital raising on algo trading and control variables. The dependent variables are total annual proceeds from securities issues, proceeds from public equity issues, and proceeds from other securities issues, each scaled by total assets. The independent variables are all measured in the prior year and include algo trading (AT), the natural logarithm of the year-end market cap (lnME), the natural logarithm of year-end Book-to-Market (Ln(B/M)), the temporary price impact measured by relative realized spreads (RRS) and the permanent price impact (RPI), both scaled by the closing price and measured in bp, the daily average return (Ret), volatility measured by price range, , and the lagged dependent variable. The High and Low dummies represent firms that are in the top and bottom market cap (return volatility) terciles within each market in Panels A and C (B and D). Panels A and B report market and year fixed effects regression results. Panels C and D report mean coefficients of cross-sectional regressions within each market. All continuous variables are standardized to have a mean of zero and standard deviation of one every year within each market.

	All Proceeds			Equity proceeds			Other proceeds		
	Coef	t	%pos	Coef	t	%pos	Coef	t	%pos
Panel A: Market and year fixed effects panel regression, interacting AT with market cap dummies									
AT	–0.017	–1.51	n/a	–0.018	–1.53	n/a	–0.014	–1.12	n/a
AT*Low	–0.018	–1.13	n/a	–0.015	–0.86	n/a	–0.014	–0.80	n/a
AT*High	–0.004	–0.34	n/a	–0.008	–0.60	n/a	0.003	0.22	n/a
Ln(ME)	–0.014	–2.74	n/a	–0.031	–5.70	n/a	0.012	1.95	n/a
Ln(B/M)	–0.055	–15.24	n/a	–0.049	–13.11	n/a	–0.042	–10.23	n/a
Ret	0.004	0.94	n/a	–0.012	–2.64	n/a	0.018	3.75	n/a
DV_Lag1	0.076	21.29	n/a	0.049	13.63	n/a	0.080	20.15	n/a
Price range	0.071	18.85	n/a	0.070	17.85	n/a	0.044	10.35	n/a
RPI	–0.009	–1.94	n/a	–0.012	–2.52	n/a	0.004	0.76	n/a

	All Proceeds			Equity proceeds			Other proceeds		
RRS	–0.004	–0.93	n/a	–0.010	–2.18	n/a	–0.004	–0.79	n/a
Panel B: Market and year fixed effects panel regression, interacting AT with return volatility dummies									
AT	–0.034	–4.192	n/a	–0.027	–3.244	n/a	–0.028	–3.121	n/a
AT*Low	0.017	1.888	n/a	0.002	0.240	n/a	0.022	2.217	n/a
AT*High	0.013	1.095	n/a	0.003	0.279	n/a	0.012	0.905	n/a
Ln(ME)	–0.013	–2.622	n/a	–0.030	–5.653	n/a	0.012	2.087	n/a
Ln(B/M)	–0.056	–15.251	n/a	–0.050	–13.103	n/a	–0.042	–10.249	n/a
Ret	0.004	0.976	n/a	–0.012	–2.697	n/a	0.019	3.888	n/a
DV_Lag1	0.076	21.285	n/a	0.049	13.616	n/a	0.080	20.158	n/a
Price range	0.071	18.739	n/a	0.070	17.855	n/a	0.043	10.216	n/a
RPI	–0.009	–1.968	n/a	–0.012	–2.542	n/a	0.004	0.766	n/a
RRS	–0.005	–1.099	n/a	–0.010	–2.303	n/a	–0.004	–0.916	n/a
Panel C. Aggregate of market-specific, quarterly Fama MacBeth regressions, interacting AT with market cap dummies									
AT	–0.036	–1.21	36%	–0.001	–0.02	42%	–0.040	–2.15	44%
AT*Low	–0.029	–0.96	39%	–0.077	–1.68	42%	–0.032	–0.88	28%
AT*High	0.007	0.23	50%	–0.019	–0.43	47%	0.022	0.96	56%
Ln(ME)	–0.022	–1.32	39%	–0.042	–2.29	31%	–0.010	–0.51	47%
Ln(B/M)	–0.050	–6.37	8%	–0.043	–5.19	19%	–0.027	–3.68	22%

	All Proceeds			Equity proceeds			Other proceeds		
Ret	–0.004	–0.51	53%	–0.024	–1.75	36%	0.033	3.66	75%
DV_Lag1	0.060	3.87	83%	0.039	2.98	69%	0.075	3.33	63%
Price range	0.074	5.93	92%	0.080	6.21	89%	0.042	4.46	81%
RPI	–0.013	–0.97	39%	–0.018	–1.23	33%	–0.001	–0.12	47%
RRS	–0.022	–1.96	25%	–0.030	–2.58	36%	–0.019	–2.02	28%
Panel D. Aggregate of market-specific, quarterly Fama MacBeth regressions, interacting AT with volatility dummies									
AT	–0.020	–1.88	36%	–0.020	–1.64	42%	–0.012	–0.83	47%
AT*Low	–0.003	–0.19	64%	0.018	0.58	56%	0.013	0.70	50%
AT*High	0.000	–0.01	44%	0.002	0.09	50%	–0.044	–1.16	41%
Ln(ME)	–0.014	–0.97	47%	–0.035	–2.10	33%	–0.005	–0.28	47%
Ln(B/M)	–0.051	–6.21	8%	–0.044	–5.19	17%	–0.029	–3.77	22%
Ret	–0.004	–0.51	53%	–0.023	–1.77	39%	0.030	3.46	75%
DV_Lag1	0.059	3.79	81%	0.038	2.91	69%	0.075	3.33	72%
Price range	0.075	5.79	92%	0.080	6.26	89%	0.047	5.02	88%
RPI	–0.012	–1.04	39%	–0.020	–1.44	33%	–0.003	–0.25	47%
RRS	–0.022	–1.83	25%	–0.032	–2.60	31%	–0.016	–1.77	28%

Table 6. Algo trading and equity repurchases

This table reports an annual regression of equity repurchases on algo trading. The dependent variable is a proxy for share repurchases, computed as the difference between NNE and annual aggregate new equity issues, all scaled by shares outstanding. NNE is annual equity issuance computed as in McLean, Pontiff and Watanabe (2009). The independent variables are all measured in the prior year and include algo trading (AT), the natural logarithm of the year-end market cap (lnME), the natural logarithm of year-end Book-to-Market (Ln(B/M)), the temporary price impact measured by relative realized spreads (RRS) and the permanent price impact (RPI), both scaled by the closing price and measured in bp, the daily average return (Ret), volatility measured by price range, and the lagged dependent variable. The High and Low dummies represent firms that are in the top and bottom market cap (return volatility) terciles within each market in Panel B (Panel C). All regressions are market and year fixed effects models. All continuous variables are standardized to have a mean of zero and standard deviation of one every year within each market.

	Coef	t
Panel A: Market and year fixed effects panel regression		
AT	0.018	3.96
Ln(ME)	–0.010	–1.82
Ln(B/M)	–0.016	–4.22
Ret	–0.009	–2.09
DV_lag1	0.061	16.77
Price range	–0.010	–2.63
RPI	–0.004	–0.76
RRS	–0.004	–0.82
Panel B: Market fixed effects panel regression, interacting AT with market cap dummies		
AT	0.046	4.21
AT*Low	–0.007	–0.45
AT*High	–0.036	–2.97
Ln(ME)	–0.010	–1.86
Ln(B/M)	–0.016	–4.29

	Coef	t
Ret	–0.008	–1.89
Price range	–0.011	–2.77
DV_lag1	0.061	16.74
RPI	–0.004	–0.95
RRS	–0.004	–0.98
Panel C: Market fixed effects panel regression, interacting AT with return volatility dummies		
AT	0.023	2.85
AT*Low	–0.014	–1.50
AT*High	0.018	1.55
Ln(ME)	–0.009	–1.74
Ln(B/M)	–0.017	–4.38
Ret	–0.007	–1.65
Price range	–0.010	–2.61
DV_lag1	0.061	16.72
RPI	–0.004	–0.94
RRS	–0.005	–1.07

Table A1. The effect of AT on new capital raising across regions

This table reports a subset of coefficients from annual regressions of NNE, the proceeds from all securities issues, the proceeds from equity issues, and equity repurchases on algo trading. The dependent variables are NNE, the annual equity issuance computed as in McLean, Pontiff and Watanabe (2009), issuance proceeds from SDC, and a proxy for share repurchases, computed as the difference between NNE and annual aggregate new equity issues, all scaled by shares outstanding. The independent variables are all measured in the prior year and include algo trading (AT) and several controls. Control coefficients are estimated but not tabulated for the natural logarithm of the year-end market cap (lnME), the natural logarithm of year-end Book-to-Market (Ln(B/M)), liquidity measured by the relative effective spread, the daily average return (Ret), volatility proxied by price range, and the lagged dependent variable. We present annual market-specific year fixed effect models. All continuous variables are standardized to have a mean of zero and standard deviation of one every year within each market. The asterisks ***, **, and * represent statistical significance at the 1%, 5%, and 10% level, respectively.

	NNE		All proceeds		Equity proceeds		Shares repurchased as % of shrout	
	Coef		Coef		Coef		Coef	
Panel A: Global regions								
Europe	–0.0260		–0.021		–0.028		0.021	
Asia	–0.0258		–0.016		–0.002		0.021	
Rest	–0.0208		–0.035		–0.032		0.007	
Panel B: European markets in detail								
Euronext Amsterdam	–0.062		0.006		–0.038		0.053	
Athens	–0.019		–0.083		–0.083		0.066	
Brussels	0.006		–0.061		–0.076		0.008	
Copenhagen	0.026		0.002		0.020		0.006	
Dt Boerse Xetra	–0.071	***	–0.022		–0.023		0.069	***

	NNE		All proceeds		Equity proceeds		Shares repurchased as % of shrout	
Helsinki	–0.070		–0.052		–0.044		0.057	
London	–0.070	***	–0.009		–0.009		0.000	
Euronext Lisbon	0.008		0.043		–0.004		–0.014	
Madrid	0.023		–0.004		0.002		0.027	
Milan	–0.049		–0.025		–0.026		0.011	
Oslo	–0.016		–0.015		–0.023		–0.022	
Euronext Paris	–0.031	*	–0.011		–0.012		0.007	
Swiss Exchange	–0.001		–0.005		–0.024		0.001	
Stockholm	–0.039		0.011		0.010		0.016	
Wiener Boerse	–0.240	**	–0.192	**	–0.181	*	0.156	*
Warsaw	0.189	***	0.084		0.069		–0.103	**

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