ESSAYS IN EMPIRICAL MARKET MICROSTRUCTURE

by

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STATEMENT OF DISSERTATION APPROVAL

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ABSTRACT

This dissertation is composed of three essays in empirical market microstructure. My first two essays study market quality issues related to High-frequency Trading (HFT) using a dataset provided by NASDAQ that identifies the activity of high-frequency traders (HFTs). My first essay studies the systematic effects of HFT on market quality. I find only small effects of HFT participation on spreads and adverse selection costs, and I find evidence that HFT trades improve price efficiency. I also examine HFT trading strategies, and show that HFTs engage in successful intraday market timing. My second essay studies HFT in extreme market conditions, focusing on whether it has a stabilizing or destabilizing effect. I find that HFTs buy during mini-flash crashes, sell during price spikes, and provide more liquidity than they consume during both types of events. These results suggest HFTs play a stabilizing role during extreme return events, but their net trading volumes are low so these effects are probably small. I also examine returns around large HFT order imbalances, and find only economically small evidence of the price momentum that these imbalances have been hypothesized to cause. Finally, I study HFT activity around sustained market order flow imbalances, termed "toxic order flow" by Easley, Lopez de Prado, and O'Hara (2011), and find that HFT participation levels decrease as order flow toxicity increases. Overall, in my first two essays, I find evidence of beneficial and neutral roles played by HFTs, both in normal and extreme market conditions, but no significant evidence for any of the detrimental impacts they are

thought to have. My third essay compares corporate bond trading costs in a market that provides pretrade transparency (the NYSE) with those in a market that is opaque (the OTC market). I find that trading costs are dramatically lower in the market with pretrade transparency, and that pretrade transparency is the most likely explanation for the difference. I also advance a likely explanation for the puzzle of why bond trading costs are lower for larger trades, and introduce a new statistical procedure for assessing trade signing errors in microstructure data with stale quotes.

I dedicate this dissertation to my family, especially my wife Traci, my children Dean and Natali, my parents Mario and Sally, and my in-laws Ronald and Dana.

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1. INTRODUCTION

This dissertation is composed of three essays in empirical market microstructure. Market microstructure is defined by Hasbrouck as "the study of the trading mechanisms used for financial securities."[1] This field deals with issues such as liquidity, pricing efficiency, trading under information asymmetry, trading strategies, and market design. Empirical market microstructure utilizes highly granular market data to reveal insights on these topics, and often involves processing large volumes of individual trades and quotes.

My first two essays study issues related to High-frequency Trading (HFT). HFT is a recent development where a small group of market participants has developed a dramatic speed advantage over other traders and participates in a large share of the trading volume. Their effects on the functioning of the markets are controversial and not well understood. I provide new evidence on this issue using a proprietary dataset provided by the NASDAQ that identifies the trades and quotes of high-frequency traders (HFTs). My first essay studies the systematic unconditional effects of HFT on market quality. I find that spreads are slightly wider in trades where HFTs provide liquidity and tighter in trades where they demand liquidity, but the differences are small and liquidity is plentiful in this sample regardless of HFT participation. I find that, contrary to

[1] See Joel Hasbrouck, *Empirical Market Microstructure*, (New York: Oxford University Press, 2007), 3.

theoretical predictions, HFTs do not impose high adverse selection costs on other traders when demanding liquidity, and they seem to improve price efficiency when they trade. I also provide evidence regarding HFT trading strategies, showing that HFTs engage in successful intraday market timing but do not seem to trade on cross-sectional return predictability. My second essay studies HFT in extreme market conditions, with a focus on whether it has a stabilizing or destabilizing effect. I find that HFTs buy during mini-flash crashes, sell during price spikes, and provide more liquidity than they consume during both types of events. These results suggest HFTs play a stabilizing role during extreme return events, but their net trading volumes are low so these effects are probably small. I also examine returns around large HFT order imbalances, and find only economically small evidence of the price momentum that these imbalances have been hypothesized to cause. Finally, I study HFT activity around sustained market order flow imbalances, termed "toxic order flow" by Easley, Lopez de Prado, and O'Hara (2011), and find that HFT participation levels decrease as order flow toxicity increases. This finding holds for both liquidity-supplying and liquidity-demanding participation, which is inconsistent with predictions that HFTs increase their liquidity demand at high toxicity levels. Overall, in my first two essays, I find evidence of beneficial and neutral roles played by HFTs, both in normal and extreme market conditions, but no significant evidence for any of the detrimental impacts they are thought to have. My only results that could arguably be considered consistent with negative impacts are findings of decreased participation around some extreme events, but these effects are not dramatic.

My third essay compares corporate bond trading costs in a market that provides pretrade transparency (the NYSE) with those in a market that is opaque (the OTC market). I find that trading costs are dramatically lower in the market with pretrade transparency, and that pretrade transparency is the most likely explanation for the difference. I also advance a likely explanation for the puzzle of why bond trading costs are lower for larger trades.

An important theme, in both this dissertation and the field of empirical market microstructure in general, is how specific trading arrangements affect market quality from an investor's perspective. This topic is relevant for investors deciding where to trade, for markets designers interested in how best to organize their trading mechanisms to attract traders, and for regulators formulating market rules. In the first two essays, the trading arrangements of interest are the market structure, technology, and regulations that have allowed high-frequency trading to exist and flourish. In the third essay, the trading arrangement of interest is pretrade transparency. Other contributions I make in this dissertation include tests of a variety of academic theories and informal hypotheses advanced by market participants and the financial press, new empirical facts that can guide future theoretical research, and a new statistical procedure for assessing trade signing errors in microstructure data with stale quotes.

2. VERY FAST MONEY: HIGH-FREQUENCY TRADING ON THE NASDAQ

2.1 Abstract

I provide large-sample evidence regarding High-frequency Trading (HFT) strategies and market quality, using a proprietary sample of NASDAQ trades and quotes that identifies HFT participation. Spreads are slightly wider for trades where HFTs provide liquidity and slightly tighter when HFTs take liquidity, suggesting that HFTs provide liquidity when it is scarce and consume liquidity when plentiful. Prices incorporate information from order flow and market-wide returns more efficiently on days when HFT participation is high. This effect is driven by HFT demand-side participation, implying that HFTs improve price efficiency when demanding liquidity. I also provide evidence regarding HFT trading strategies, showing that HFTs engage in successful intraday market timing, but do not seem to trade on cross-sectional return predictability at the horizons I study. The new evidence in this paper is relevant to the ongoing HFT-related policy debates and can potentially provide guidance to theoretical researchers seeking to model HFT behavior and market quality impacts.

2.2 Introduction

High-frequency trading has become a pervasive feature of the equity markets in a relatively short period of time. Estimates of high-frequency trading activity levels vary, but are large. For example, a 2009 article in Advanced Trading estimates that high frequency trading is responsible for 73% of the of U.S. equity trading volume. The developments in market structure (such as decimalization, REG NMS, and automated electronic limit order books) that have created the circumstances for HFT to flourish are relatively recent. Our understanding of the impact of high-frequency trading on market quality is in its infancy, partly due to its sudden emergence and also the scarcity of high quality data. There are diverse views among market participants and regulators on whether HFT is beneficial, neutral, or detrimental. Reflecting this uncertainty, proposals to both restrict and encourage high-frequency trading are simultaneously being debated. In a letter to the SEC, Senator Charles Schumer writes:

> I have come to believe that HFT provides less of the benefits to our markets than its adherents claim, and does so at a greater cost to long-term investors ... The SEC should identify market participants who frequently engage in these practices, and require exchanges and other trading venues to slow down those market participants [in times of stress] … the Commission should consider imposing a minimum quote duration, so that orders could not be sent and cancelled within a fraction of a second.[2]

Conversely, the Joint SEC-CFTC Advisory Committee recommends:

> the Commission should consider encouraging, through incentives or regulation, persons who regularly implement market maker strategies to maintain best buy and sell quotations which are ‘reasonably related to the market’ … We recognize that many High Frequency Traders are not even

[2] Sen. Schumer’s letter available at http://schumer.senate.gov/Newsroom/record_print.cfm?id=327487.

> broker-dealers and therefore their compliance with quoting requirements would have to be addressed primarily through pricing incentives.[3]

The differing views regarding the impact of HFT on market quality partly stem from the lack of consensus on the nature of their trading practices. A common view is that they have taken over the market-making function. Under this scenario, they generally benefit the market by increasing competition to provide liquidity, but there are still concerns that they lack the affirmative obligations that bound traditional market makers and could cause disruptions by exiting the market at their discretion. They are also thought to engage in high-frequency arbitrage, which may have the beneficial effect of making prices more efficient. The alternate perspective is that the liquidity they provide is unreliable, and is outweighed by disruptive practices they are alleged to employ such as order spoofing, predatory trading, herding, or overloading market infrastructure with excessive messages.

Aside from the views of market participants and regulators, there are theoretical reasons to suspect that HFT may affect market quality. In the classic market microstructure models, the major sources of trading frictions are information asymmetry, inventory risk, and order processing costs. HFTs are likely to differ from the intermediaries they have replaced in all of these dimensions. As pointed out in Jovanovic and Menkveld (2011) and Biais, Foucault, and Moinas (2011), the speed advantage of HFTs could allow them to react more quickly to public news than other traders, which would reduce the adverse selection costs they face when providing liquidity while making limit orders riskier for slower traders. Similarly, Stoll (2000) argues that speed

[3] The Joint SEC-CFTC Advisory Committee Report is available at http://www.sec.gov/spotlight/sec-cftcjointcommittee/021811-report.pdf

differentials play a role in informational frictions, and that increasing the speed parity among traders could reduce spreads under certain conditions. Inventory costs may also play a greater role than in the past. High-frequency traders generally seek to end the day flat. In models such as Garman (1976) and Ho and Stoll (1981), inventory adjustment motives affect liquidity,[4] and recent evidence is supportive (see Naik and Yadav 2003, Panayides 2007, Comerton et al. 2010). Several studies have shown evidence of market maker inventory adjustment taking place relatively slowly,[5] and if HFTs manage inventory more aggressively, we might expect the effects on liquidity to increase. Order processing costs should be reduced for HFTs because of their large trading volumes. Rebates for adding liquidity are tiered by volume, and their fixed costs will be spread over more transactions. While the classic microstructure literature has implications for HFT, there has also been a recent growth in HFT-specific theoretical literature. Jovanovic and Menkveld (2011) develop a model where the information asymmetry effects can generate either beneficial or negative impacts, and derive the conditions where each outcome is in effect. Biais, Foucault, and Moinas (2011), Cvitanic and Kirilenko (2010), and Jarrow and Protter (2011) present theoretical models where HFTs can play disruptive roles. The mechanisms are overinvestment, adverse selection, and the crowding out of slower traders in Biais, Foucault, and Moinas (2011), order sniping in Cvitanic and Kirilenko (2010), and a type of herding behavior in Jarrow and Protter (2011).

[4] To be more precise, inventory affects midquotes, effectively reducing liquidity for trades that increase inventory imbalances while improving liquidity for inventory rebalancing trades.

[5] Hasbrouck and Sofianos (1993) find cases where inventory takes long periods to revert to apparent target levels. In a more recent sample, Hendershott and Menkveld (2010) reported inventory half-lives of 0.55-2.11 days.

Despite the emerging theoretical literature and ongoing policy debates concerning HFT, there is little empirical evidence on the market quality impacts and trading behaviors of HFT. The empirical studies include Brogaard (2011, 2012a, 2012b), Menkveld (2012), Jovanovic and Menkveld (2011), Hasbrouck and Saar (2011), and Kirilenko, Kyle, Samadi, and Tuzun (2011) (KKST (2011) hereafter). Of these, KKST (2011) focus on an extreme event (the 2010 Flash Crash[6]), and Jovanovic and Menkveld (2011) and Menkveld (2012) study a single high-frequency trader, and Brogaard (2012b) focuses on trading strategies instead of market quality effects, leaving only three papers that address the collective effects of HFT in normal market conditions.[7] Brogaard (2011, 2012a) studies a sample of NASDAQ trades and quotes with HFT participation identified by the exchange, and finds that HFT is generally beneficial or benign. Brogaard (2011, 2012a) finds that they provide a large share of the liquidity in the market and play an important role in the price discovery process. Brogaard (2012) finds HFT activity dampens volatility. Hasbrouck and Saar (2011) also study recent NASDAQ data and use the intensity of order placements and cancellations, which they call strategic runs, to identify periods when HFTs are active in a stock. They find that high-frequency trading "lowers short-term volatility, reduces quoted spreads and total price impact of trades, and increases depth in the limit order book" (p. 3).

There is a related thread of empirical studies on algorithmic trading (AT). HFT is generally considered a subset of AT, but HFTs and non-HFT AT are very different.

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[6] The Flash Crash is the popular name for an event that occurred on May 6, 2010, where within a half hour period, the major U.S. equity indexes dropped more than 5% and quickly reversed most of the losses. Volatility in some ETFs and individual stocks was even greater. See KKST (2011).

[7] Arguably, Jovanovic and Menkveld (2011) could fall into this category as well. In part of their analysis, they study the introduction of an HFT-friendly trading venue on market quality, but they do not clearly claim that this measures the impact of HFT.

Hasbrouck and Saar (2011) explain the distinction clearly. They divide algorithmic traders into agency algorithms and proprietary algorithms. Agency algorithms are "employed to minimize trading costs of buy-side managers" (p. 2). These can be thought of as engaging in activities such as splitting large orders or alternating between providing and taking liquidity with the goal of meeting a longer term trading need while minimizing its price impact. Proprietary algorithmic traders encompass the subset of AT that I am referring to as HFT. They trade their own capital, turn over positions rapidly, have technology and infrastructure to trade at very high speeds (2-3 milliseconds, according to Hasbrouck and Saar 2011), and are reluctant to hold inventory overnight. The AT literature does not study HFT directly, but often touches on related issues or includes HFT in AT samples. The empirical AT studies that address market quality issues include Chaboud, Chiquoine, Hjalmarsson, and Vega (2009), Hendershott and Riordan (2009), and Hendershott, Jones, and Menkveld (2011).[8] Chaboud et al. (2009) study algorithmic trading in foreign exchange markets and find that AT trades contribute less to price discovery than human trades in two of the three currencies in their sample, AT limit orders seem to be strategically placed (face less adverse selection costs), AT reduces liquidity provision before the NFP report and increases afterwards, and there is some evidence that AT lowers volatility. Hendershott and Riordan (2009) examine AT in the DAX stocks on the Deutsche Boerse's Xetra platform. They find that ATs are more likely to demand liquidity when it is cheap and supply when it is expensive, and that ATs contribute more to price discovery than human traders. Hendershott, Jones, and Menkveld (2011) examine market quality measures on the NYSE and find that AT

[8] There is also a somewhat large AT literature that studies algorithmic trading strategies and trading costs for users of algorithms.

improves liquidity for large capitalization stocks, makes quotes more informative, and reduces the adverse selection costs of trades. Chaboud et al. (2009) and Hendershott and Riordan (2009) study data that explicitly identify algorithmic trader participation, while Hendershott, Jones, and Menkveld (2011) uses message traffic as a proxy for AT activity and utilize an infrastructure improvement to establish causality.

I contribute to this literature by examining the market quality impacts of HFT and testing several hypotheses regarding HFT behavior in a proprietary sample of NASDAQ trades and quotes that identifies HFT participation. This is the same dataset used in Brogaard (2011, 2012a), but I focus on a different set of questions and market quality dimensions. The market quality tests I conduct suggest that HFTs play a neutral or beneficial role. Trading costs are unconditionally very low, but spreads are slightly wider for trades where HFTs provide liquidity and slightly tighter when HFTs take liquidity, suggesting that HFTs provide liquidity when it is scarce and consume liquidity when plentiful. These results hold whether HFT participation is defined as being on the aggressive side, the passive side, or either, and are robust to controls for stock and trade characteristics and market conditions. To the best of my knowledge, this is the first large sample trading cost analysis performed in data that explicitly identify HFT trades.[9] Prices are more efficient on days when HFTs are more active in a given stock, in the sense that it takes less time for stock prices to incorporate information from order flow and market index returns. This result is driven by HFT liquidity-demanding trades. I also provide new evidence on the trading behavior of HFTs. Their trading performance

[9] Hasbrouck and Saar (2011) are not able to identify HFT participation in specific trades. Jovanovic and Menkveld (2011) and Menkveld (2010) study a single HFT. Other similar studies examine AT instead of HFT.

as measured in a VWAP analysis is consistent with successful intraday market timing, but I find no evidence that their trades predict the cross section of short-term expected returns.

These results should be interpreted with some caution. As discussed in more detail below, the sample does not identify the activity of all high-frequency traders, and contains only NASDAQ continuous trading activity in the sample stocks. The sample stocks are traded in multiple venues, and are presumably traded by the sample HFTs in other venues. Also, the NASDAQ exchange is organized as a virtual electronic limit order book with price and time priority, pretrade and posttrade transparency, anonymity, and a maker-taker fee model. It is not clear that any conclusions drawn in this sample will necessarily generalize to markets that are organized differently. These concerns are somewhat mitigated by the facts that the sample contains an economically large amount of trading activity, both in absolute terms and as a share of volume in the sample firms, and the identified HFT firms account for a large share of the observed volume. In addition, although I find only benign or beneficial effects of HFT in this paper, my analysis focuses on their systematic effects and does not rule out the possibility that there are certain circumstances where HFT can have negative impacts. In particular, the data do not distinguish between individual HFTs, so I can only observe their aggregated activity. Therefore, while this is useful in studying their behavior on balance and their overall impact on the market, it is possible that individual HFTs follow disruptive strategies that are hidden by the level of aggregation in the data. Nevertheless, I believe the evidence provided in this paper should advance our understanding of HFT market quality impacts and trading behavior.

The rest of this paper is organized as follows. Section 2.3 describes the data. Section 2.4 examines the level of HFT participation in the sample. Section 2.5 studies trading costs and how they vary with HFT participation. Section 2.6 presents price efficiency tests. Section 2.7 analyzes HFT trading behavior and performance. Section 2.8 concludes.

2.3 Data

2.3.1 Overview

The primary data source employed is a proprietary dataset provided by NASDAQ consisting of trades and quotes for a sample of 120 stocks. The stock sample was chosen by Terrence Hendershott and Ryan Riordan. See Table 2.1 for a list of sample stocks. It is stratified by market capitalization,[10] and is evenly split by NASDAQ and NYSE listing. The sample period covers all of 2008 and 2009 and one week in 2010.[11] The trade sample consists of all trades executed on the exchange in continuous trading, excluding crosses and NASDAQ TRF-reported trades. Trades are time stamped to the millisecond and signed to indicate whether they were initiated by a buyer or seller. The trade signs are high quality, and are based on records of rebate payments.[12] NASDAQ Inside Quotes (BBOs) are provided for subsamples of the data. These subsamples cover the first full trading week in each quarter, the week of Oct 6-10, 2008 (the week of the Lehman collapse), and the week of Feb 22-26, 2010. The BBO data are time stamped to the millisecond and does not have the problems with timestamp discrepancies that are

[10] With 40 large, 40 medium, and 40 small stocks.

[11] There is one day, October 10, 2008, missing from the dataset which may become available in the future.

[12] Rebate payments are payments made to the liquidity supplier in a maker-taker market. These are partial rebates of the fees collected by the exchange from the trade initiator.

present in alternate sources. The only filter applied to the full trade sample was the removal of trades before 9:30 am and after 4:00 pm. A subsample used for trading cost analysis also required a usable quote before and after each trade. For some analyses, additional filters were applied, and specifics are provided in the relevant sections.

A unique feature of this dataset is that high-frequency participation is identified in the data. NASDAQ has manually identified 26 high-frequency trading firms and flagged their activity. Specifically, trades contain a field with the following codes: HH, HN, NH, or NN. H identifies a high-frequency trader and N identifies a non-HFT. The first term in a pair classifies the liquidity taker, and the second term classifies the liquidity provider. For example, a trade marked HN would mean a high-frequency trader took liquidity from a non-HFT on that trade. Similarly, HFT quotes are flagged in the limit order book snapshots and a subsample of quotes.

The identities of the HFT firms are not provided. The selection process was manual and apparently somewhat subjective. The principles are described in Brogaard (2012a) as follows:

> The characteristics of firms identified as being HFTs are the following: They engage in proprietary trading… They use sponsored access providers whereby they have access to the co-location services and can obtain large-volume discounts and reduce latency. They tend to switch between long and short net positions several times throughout the day…. Orders by HFT firms are of a shorter time duration than those placed by non-HFT firms. Also, HFT firms normally have a lower ratio of trades per orders placed than non-HFT firms. (p. 7)

Brogaard (2012a) and Hasbrouck and Saar (2011) note that the selection process excludes certain types of firms that engage in HFT, such as firms whose primary business is not HFT but sometimes engage in HFT or HFT firms that route trades through a non-HFT firm. This concern is valid but is somewhat mitigated by the large percentage of trading volume that the sample firms participate in, which is described in further detail in Section 2.4. It is also worth repeating that the level of aggregation in the data does not allow individual HFTs to be studied in isolation.

I also obtain supplemental data from CRSP and TAQ. I use CRSP data for the sample stock descriptive statistics only. For several tests, I employ midpoint returns, and in some cases, I consider it preferable to use an NBBO midpoint constructed from the TAQ CQ tape instead of the NASDAQ midpoint. The NBBO includes price data from other market centers, and is available on dates when NASDAQ Inside Quotes are not provided. In addition to the larger sample size available with NBBO quotes, my main considerations in choosing a quote source for a particular application are that TAQ quotes are only time stamped to the second, while NASDAQ quotes are timestamped to the millisecond, and whether I am primarily interested in liquidity and prices across all markets or on the exchange where the sample trades occur. I also use TAQ to obtain SPY midpoints to construct a proxy for the market return, and I use trade data from the CT to assess NASDAQ's volume shares in sample stocks.[13]

2.3.2 Descriptive Statistics

Table 2.2 presents trade summary statistics. The second column reports values for the full sample. The full sample covers 509 days and contains 550,118,372 trades for approximately 106 billion shares and a total dollar volume of $3.9 trillion. The daily average share volume in the sample is 208 million shares and the dollar volume is $7.7 billion. There is substantial variation in the daily trading activity. On the 10th percentile

[13] SPY is the ticker symbol for an ETF that tracks the S&P 500.

day, there is \$4.4 billion traded, while on the 90th percentile day, \$11.9 billion is traded. The trade size is of particular interest because there is a common perception that trade sizes are much smaller than in the past. They are in fact small in this sample: the average size is 192.3 shares, the median is 100 shares, and the 90th percentile is 400 shares. The third column reports values for the subsample where matching NASDAQ pretrade and posttrade quotes are available. This subsample contains 61,272,712 trades for 11.6 billion shares and \$444 billion dollars. By comparing the two columns, we can informally assess whether the quote subsample is reasonably representative. The days with quotes have somewhat more trading activity, but in general appear similar. The subsample covers roughly 10% of the trading days in the full sample, and the aggregate trades, share volume, and dollar volume are around 11% of the full sample values. The daily mean share volume and dollar volume in the subsample are 14% and 18% higher than the full sample means. The trade size distributions are very close.

2.4 HFT Activity Levels

In this section, I examine the extent of HFT activity as a share of total dollar trading volume. I construct three measures of the HFT participation share that differ in how each trade is classified as an HFT or non-HFT trade. The first counts trades where an HFT participates on either side of a trade (All), the second only uses trades where an HFT is the liquidity demander (Demand), and the third only uses trades where an HFT is the liquidity supplier (Supply). Trades where HFT are on both sides are counted in all three measures. The denominator is all trading volume in the NASDAQ sample only, which is consistent because the numerator does not include HFT from other trading venues.

Brogaard (2012b) performs a similar analysis. This section complements the material in that paper by reporting additional pooling/weighting schemes designed to show time-series and cross-sectional variation, and by introducing a measure of stock-specific time variation in HFT participation that I use as an explanatory variable in several analyses that follow in this paper. Table 2.3 summarizes the main findings. Across the full sample, HFTs participate in 68.3% of all dollar trading volume, demand liquidity in 42.2%, and supply liquidity in 41.2%. From the daily results with trades pooled across all stocks, the mean participation shares are similar and little time variation is evident, with standard deviations ranging from 2.4% to 3.6%. These levels are strikingly high and are of a similar order of magnitude to those reported by Brogaard. The third section of Table 2.3 calculates participation shares by stock-day and reports sample statistics equally weighting across stock-days. This removes the extra weight implicitly given to stock-days with more trades in the previous sections. Here we see much lower mean participation levels (48.3%, 32.5%, and 23.2% for All, Demand, and Supply, respectively), suggesting HFTs are participating more heavily in stock-days with more trading activity. We also see more variability, with standard deviations from 15.4% to 20.5%. A natural question is whether the variability in HFT participation across stock-days is determined by temporary market conditions and or by persistent stock characteristics. To gain some insight into this question, the fourth section of Table 2.3 first takes the means of the daily participation shares for each stock, and then reports summary statistics across stocks. This analysis shows that there is substantial variation in long-run mean HFT participation across stocks. For example, the 90th percentile stock has a mean daily HFT (All) share of 72.6%, while the 10th percentile stock has a share of 25.1%. This is consistent with an analysis of HFT participation by stock-day in Brogaard (2012b), which finds that some persistent stock characteristics such as market capitalization and market-to-book are determinants of HFT activity.

For some of the tests I wish to conduct later, I will need to identify days with high HFT intensity. In light of the observations above, a stock-specific measure that controls for the normal level of HFT activity in that stock is desirable. For each of the three types of HFT participation, I construct indicator variables that take a value of 1 for each stock-day where the dollar volume participation share is in the highest tercile for that stock across all sample days and 0 otherwise. The choice of terciles is somewhat arbitrary, but seems to be a reasonable tradeoff between sample size and extremity.

Before using the HFT participation indicator variables, I address three potential concerns regarding their suitability. First, they must capture sufficient time variation in HFT activity within a given stock. Hasbrouck and Saar (2011) show that stock-specific HFT quoting intensity varies greatly over short intervals, but it is necessary to verify that this time variation is also present in trading activity and is not dampened at the daily horizon. The last section of Table 2.3 reports summary statistics on the differences between the dollar volume HFT participation levels on days when the indicator variable is 1 (high participation days) and other days (normal participation days). The mean differences are 16.6%, 16.1%, and 12.5% for All, Demand, and Supply, respectively, and their 10th percentile values are 9.4%, 10.9%, and 8.2%. Second, given the growth in HFT over time, it is possible that these variables are proxies for time trends, and any effects they capture could be attributable to time trends in market quality unrelated to HFT. To investigate this, on each day in the sample, I count the number of stocks where each of these variables show high HFT participation levels. These counts are plotted over time in Figure 2.1. Given that the indicator variables take the value of 1 in each stock's high-participation tercile, if these variables only captured a trend, we would expect to see no stocks experiencing high-HFT days in the first two-thirds of the sample and all 120 stocks with high-HFT days in the last one-third of the sample (with some noise). This is not the case. Figure 2.1 does show some signs of a trend but with strong time variability from day to day. A third concern is that HFT liquidity demand and supply may be highly correlated, and using all three variables would be redundant. Visually, there appears to be some correlation, but also periods with significant divergences. The correlation coefficients confirm that there is high correlation but also independent information. The correlation between the daily demand and supply indicator variable counts is .203, and the mean stock-specific correlation between the daily raw dollar volume shares is .147, with a 10^{th} percentile value of -.068 of and a 90^{th} percentile value of .385. Overall, it seems reasonable to use these indicators in further analyses.

2.5 Trading Costs

2.5.1 Methodology

In this section, I compare the trading costs between trades with HFT participation to those without. The primary metrics I use are effective spreads, price impacts, and realized spreads. I also report quoted spreads but, because they are conditional on a trade occurring, in this context, they are more useful in understanding when HFTs trade than as an actual measure of trading costs. All spreads are measured as percentages of the midpoint price prior to the trade, and I follow the convention of reporting half spreads to reflect one-way rather than round trip costs. For this analysis, I use the subsample of trades where both pre- and posttrade quotes are available. I also convert spreads to total dollar costs for selected cases.

Effective spreads measure the difference between a trade's execution price and the pretrade midpoint. Effective spreads compensate liquidity providers for adverse selection costs when trading with informed traders (as in Glosten and Milgrom 1985) and are expected to contain a residual component that covers inventory risk, order processing costs, and market maker rents. An established empirical decomposition method separates the effective spreads into the price impact (adverse selection component) and realized spread (residual component). See Huang and Stoll (1996) and Bessembinder and Kaufman (1997a, 1997b) for a discussion of this methodology and examples of its implementation. The following formulas are used on every trade where quotes are available:

(2.1) $\text{Effective Spread} = 100Q(P - M_0)/ M_0$

(2.2) $\text{Price Impact} = 100Q(M_T - M_0)/ M_0$

(2.3) $\text{Realized Spread} = 100Q(P - M_T)/ M_0 = \text{Effective Spread - Price Impact}$

where Q is a trade sign indicator variable equal to 1 for buys and -1 for sells, P is the trade price, M_0 is the pretrade quote midpoint, and M_T is midpoint T minutes after the trade. Reported decompositions are computed with T set to 1-minute, and untabulated robustness tests use 5-minutes and 30-minutes. The last midpoint of the regular trading hours is used when trades are within T minutes of the close. Aside from the traditional interpretations of this decomposition, there are additional reasons why it is of particular interest when combined with the HFT identification. If HFTs systematically profit from naïve market-making, we should observe high realized spreads on their liquidity-providing trades. Otherwise, if HFTs profit from these trades, it must be through some other mechanism, such as rebates or superior exit timing (i.e., beating the 1-minute benchmark used in the decomposition). If the realized spreads on these trades are much higher than on those where others provide liquidity, this suggests that HFTs have skill in choosing when to offer liquidity to the market. When taking liquidity, if HFTs are trading on information, we should observe high price impacts, while if they are simply re-balancing, we should not.

To compare trading costs in trades with HFT participation to those without, I regress these measures of trading costs on indicator variables that capture whether a HFT participated in a trade and control for stock and trade characteristics and market conditions. The regressions are variations on two models. In the first set of regressions, the effect of HFT participation is constrained to be constant across all trade types. The following specification is used:

$$\text{(2.4)} \quad SPREAD_{itn} = \alpha_{it} + \beta_1\, HFT + \beta_{2j}\, SIZE_j + \beta_3\, BUY + \beta_4\, SELL + \varepsilon$$

where i indexes stocks, t indexes day-half hour intervals, n indexes trades, and j indexes trade size groups. SPREAD is either an effective spread or price impact. HFT is an indicator variable equal to 1 if a trade had HFT participation and 0 otherwise. Different versions of the model define HFT participation by trade side (liquidity-demanding or supplying). I include fixed-effects intercepts for every stock-day-half hour to control for stock characteristics and market conditions. Trade size groups are defined as SMALL (< 500 shares), MEDIUM (>=500 shares, < 1000 shares), and LARGE (> 1000 shares). BUY and SELL indicate which side of the trade took liquidity. SMALL and SELL are dropped from the estimation, so in these cases, the fixed-effects intercepts capture the trading costs for small sells, and the coefficients on the other indicator variables must be interpreted as spread differences from small sells.

The second set of models allows the effects of HFT participation to vary with trade characteristics:

(2.5) $$SPREAD_{itn} = \alpha_{it} + \beta_{1j}\,(HFT \times SIZE_j) + \beta_2\,(HFT \times BUY) + \beta_3\,(HFT \times SELL) + \varepsilon$$

where the variable definitions are identical to the constrained version. Again, SMALL and SELL are dropped from the estimation.

I also estimate variations of the constrained model one day at a time and one stock at a time to examine how these relationships vary over time and across stocks.

2.5.2 Results

Means and medians of the spread and price impacts are reported in Table 2.4. These are tabulated for the full sample and for all counterparty type combinations in the data. For the full sample, mean effective spreads are 2.7 bps, mean price impacts are 3.9

bps, and realized spreads are -0.9 bps. These trading cost measures are strikingly low compared to historical estimates. For example, Bessembinder (2003) finds mean effective spreads of 28.9 bps and realized spreads of 17.2 bps in his postdecimalization NASDAQ sample.[14] Many other studies have noted reductions in trading costs over time (see Angel, Harris, and Spatt 2010, and Chordia, Roll, and Subrahmanyam 2008, 2011), so the low costs in this sample are not entirely unexpected. It is surprising, however, that mean realized spreads are negative for the full sample and all counterparty combinations, and medians are negative in the full sample and negative or zero for all counterparty categories. This observation holds in robustness tests using both 5-minute and 30-minute realized spreads. This means that effective spreads do not fully compensate the liquidity provider for adverse selection costs. It does not necessarily mean that liquidity providers lose money to informed traders on average, because the absolute values are small and at least partially offset by liquidity rebates. It is also possible that some liquidity providers are able to beat the 1-minute posttrade benchmarks built into these measures, which I explore in Section 2.7.1. Nevertheless, it does mean the compensation for liquidity provision is very low based on these widely-used measures. I am not aware of any prior study showing negative realized spreads in any market. I offer two possible explanations. First, it is possible that increased competition between liquidity providers has driven compensation for liquidity provision down to a level close to the liquidity rebate. Second, it is possible that disintermediation has increased to a degree where a large proportion of the trades we observe are now between traders seeking liquidity with varying degrees of patience, as opposed to trades between an impatient liquidity

[14] Originally reported as round trip spreads, converted to half spreads here to facilitate comparison with my results.

demander and a professional liquidity provider. This does not rule out the low or negative realized spreads we observe in the NH and HH categories, because if HFTs expect some small profit from a trade, they may be willing to quote aggressively to compete with patient liquidity demanders. However, these are only conjectures that I am not able to test in these data.

In all of the trading costs measures in Table 2.4, we do see some variation across the HFT participation categories, but it is generally small. Across all measures and categories, the largest difference is 1.6 bps when comparing median realized spreads between HH trades and NH or NN trades. Differences in means and medians may mask important differences that would emerge when controlling for other factors that influence trading costs, however, so I consider the regression results below to be more informative.

Table 2.5 reports the results from the regressions constraining the effect of HFT participation to be constant. In Panel A, the dependent variable is the effective spread. Models 1 and 2 define the HFT participation indicator based on the liquidity demander, and Models 3 and 4 use the liquidity provider. Models 1 and 3 include only the HFT participation indicator and stock and day-half hour fixed effects, while Models 2 and 4 include the trade size and sign controls. The controls add very little explanatory power beyond the fixed effects. The coefficient estimates show that effective spreads are 0.7 bps lower on trades where an HFT demands liquidity and 0.3 bps higher on trades where an HFT supplies liquidity. This suggests that HFTs provide liquidity when it is scarce and consume liquidity when plentiful. In Panel B, the dependent variable is 1-minute price impact, and the regression models are otherwise identical to those in Panel A. The coefficient estimates in Panel B show that 1-minute price impacts are 0.1 bps higher on trades where an HFT demands liquidity, and 0.1 bps lower on trades where an HFT supplies liquidity. All estimates on the HFT indicator variables are significant at the 1% level.

Table 2.6 reports regressions that interact the HFT participation indicators with trade characteristics to determine if the impact of HFT participation varies across trade types. In Panel A, the dependent variables are effective spreads. In Model 1, the impact of HFT demand participation is statistically smaller (less negative) for medium and large trades than for small trades, but the differences are only 0.1 bps. In Model 3, the impact of HFT supply is also smaller for medium trades and is roughly cancelled out for large trades. In Models 2 and 4, the differences in impact across buys and sells are statistically significant but are only 0.1 bps or less. In Panel B, the dependent variables are 1-minute price impacts. Here we observe more variation with characteristics, but magnitudes of the differences are still small. From Model 1, the price impacts of liquidity-demanding HFT trades of all sizes are 0.1 bps higher than similar non-HFT trades. From Model 2, the price impact of HFT liquidity-demanding sell trades is 0.1 bps higher than similar trades, and for buy trades, there is almost no effect. From Model 3, the price impacts of small trades where HFTs provide liquidity are almost indistinguishable from similar trades, while they are 0.4 bps lower for medium trades and 0.8 bps lower for large trades. From Model 4, for sell trades where HFTs provide liquidity, the price impacts are 0.2 bps lower than those of similar trades, while for buy trades, the price impacts are about the same as for similar trades. Overall, the estimated impacts of their trades on effective spread and price impacts statistically vary with characteristics, but are unconditionally small and do not become dramatically large for any particular category I have examined.

The strongest stylized fact from this analysis is that HFTs avoid more informed larger trades.

Figures 2.2 and 2.3 show how the effects of HFT on effective spreads and 1-minute price impacts vary over time and across stocks. These are plots of the coefficients on the HFT indicator variable in Equation (2.4) with all controls, estimated one day at a time or one stock at a time. From the effective spread coefficient plots in Figure 2.2 Panel A and C, we see the coefficients do vary over time, but within an economically small range. For example, in Panel A, we see the lowest value for HFT_demand is -1.5 bps, and the highest is -0.2 bps. Panels B and D show how the coefficients vary by stock, with stocks sorted by their coefficient values. These show little variation except in the tails, and by inspection, the tails tend to hold small stocks. The graphs for price impact shown in Figure 2.3 are similar, with economically small time variation in price impacts over time, and little variation across stocks except for the tails.

It is also of interest to compare the price impact regression estimates with the price discovery analysis in Brogaard (2011). Brogaard finds that when demanding liquidity, HFT trades bring information into the market, and when supplying liquidity they avoid trading with informed traders. The regression models employed in this paper provide an alternate perspective on these questions. These models test whether the price impacts of trades with HFT participation are significantly different from price impacts of other trades, after controlling for other factors described previously. The results in Table 2.5 Panel B Models 1 and 2 show that trades where HFTs demand liquidity do have very slightly higher price impacts than trades where they do not. The results in Models 3 and 4 show that trades where HFTs supply liquidity have slightly lower price impacts. The results in Table 2.6 Panel B suggest that these conclusions are somewhat trade-characteristic dependent, however. For example, from Panel B Model 3, we see that when HFTs supply liquidity in medium and large trades, the price impact is 0.4 - 0.8 bps lower than predicted by fixed effects and trade characteristics, but for small trades, the model predicts no difference between HFT and non-HFT trades. It is worth noting that these differences are all small, and small trades are more prevalent so the small trade differences probably deserve the most weight. I interpret these results as weakly supportive of the conclusion that HFT liquidity-demanding trades are more informed, but they suggest that the conclusion HFTs avoid providing liquidity to informed traders is not robust across methodologies.

It is also useful to convert some of the spread measures presented above to total dollar costs to estimate how much HFTs earned or paid in spreads on their NASDAQ trades. I report values for the 49-day subsample where sufficient data are available for this analysis. In this sample, the total dollar volume traded is $443,996 million. The total dollar effective spread paid to complete these trades amounted to $98 million.[15] The total dollar effective spread earned by HFTs is $42 million, and the total dollar effective spread paid by HFTs is $34 million, for a net dollar effective spread earned by HFTs of $8 million.[16] The total effective spreads paid by non-HFT liquidity demanders is $64 million, and the total earned by non-HFT liquidity suppliers is $56 million, for a net dollar effective spread paid by non-HFTs of $8 million. In addition to dollar effective

[15] Total dollar effective spreads are calculated as $Q(P-M_0)$ x share volume for each trade, and summed over all trades in the category of interest. Total dollar price impacts are calculated analogously.

[16] The total paid in effective spreads by HFTs is the sum over trade categories HH and HN. The total earned is the sum over trade categories HH and NH. The other total dollar spread calculations in this section follow the same pattern.

spreads, it is of particular interest to examine dollar price impacts because one of the detrimental impacts of HFTs predicted by the theoretical literature is high adverse selection costs imposed by HFTs on non-HFTs when demanding liquidity. The total dollar price impact imposed on liquidity providers over all trades is $135 million, of which $53 million is borne by HFTs, and $82 million is borne by non-HFTs. Of the $82 million borne by non-HFTs, $48 million is imposed by other non-HFTs and $34 million is imposed by HFTs. HFTs impose a total dollar price impact of $54 million on other traders when demanding liquidity (the $34 million on non-HFTs and another $20 million on HFT counterparties), only slightly more than what they bear when providing liquidity. Using Model 4 in Panel B of Table 2.4 to estimate the effect of HFT on price impacts, without HFT demand participation, non-HFT liquidity suppliers would be projected to face only $1 million less in price impact, for a total of $81 million. Taken together, these calculations suggest that HFTs earn about $9 million in realized spreads in this sample, or $47.5 million annualized across the 120 sample stocks. It is noteworthy that this is much less than the total HFT profits estimated in Brogaard (2012b), suggesting that HFTs derive a significant part of their income from sources other than the spread.[17] I interpret the total dollar price impact estimates as confirming the initial observations that the price impact effects of HFT are economically small, and as suggesting that concerns regarding excessive adverse selection costs imposed by HFTs on non-HFTs are probably overblown.

[17] Brogaard (2012b) estimates total daily HFT profits of $298,000 in this dataset, which would annualize to $75 million. Brogaard does not require quote data for the total HFT profit calculations, so this estimate is based on a larger sample than mine.

Overall, the results from the regressions confirm the initial observations from the summary statistics. HFT participation explains statistically significant differences in trading cost measures, but these differences are economically small. These results must be interpreted with some caution, however. We cannot assign causality to HFT for the small differences in trading costs I report. First, it is possible that causality runs in the opposite direction. It is likely that HFTs condition their trading behavior on expected trading costs. Second, even if HFTs do not participate in a given trade, their presence in the market could still affect the cost of that trade through competition or adverse selection. In the model of Jovanovic and Menkveld (2011), the "presence in the wings" (p. 28) of an HFT can change the behavior of other market participants. Despite this qualification, it is still informative to observe that the market does not deteriorate or improve drastically on average for trades with any combination of counterparties. In particular, the adverse selection costs imposed by HFT on slower traders in the models of Jovanovic and Menkveld (2011) and Biais, Foucault, and Moinas (2011) are empirically not much higher than those imposed by other slow traders. These results also suggest that the trading cost reductions during bursts of HFT activity found by Hasbrouck and Saar (2011) may not be simply explained by more trades being executed with HFT counterparties during these bursts.

The results of this analysis may help inform the debate about affirmative obligations for HFT liquidity provision. It appears that there is little compensation for unsophisticated liquidity provision in this market, and it may not be sufficient to induce HFTs to provide liquidity without being selective in which trades they take and in which market conditions they will operate.

2.6 Market Efficiency

Pricing efficiency is widely considered to be an important dimension of market quality. Fama (1970) describes an efficient market as one where "security prices at any time 'fully reflect' all available information" (p. 383). Chordia, Roll, and Subrahmanyam (2008) (CRS (2008) hereafter) note that the empirical literature has shown that intraday inefficiencies can exist in markets that are efficient at longer horizons, because it takes investors time to process and react to information. They further state that "the determinants of this short horizon predictability deserve a thorough investigation by finance scholars" (p. 249).

It is an open question whether high-frequency trading makes prices more efficient. Theory provides little direct guidance. There is no consensus on how to describe HFT behavior, so it is not clear whether they should be modeled as discretionary market makers, arbitrageurs, predators, or some combination. The HFT-specific models such as Jovanovic and Menkveld (2011) and Jarrow and Protter (2011) describe a variety of mechanisms that could make prices more or less efficient. Empirically, Brogaard (2011) finds that HFTs are an important part of the price discovery process, but this is not equivalent to showing that their activity makes prices more efficient and no direct efficiency tests on the time series of prices were performed. Also, Brogaard (2012a) and Hasbrouck and Saar (2011) find evidence that HFT reduces volatility, which is often informally considered an inverse measure of efficiency. However, total volatility is composed of fundamental volatility and excess volatility. While reducing excess volatility makes prices more efficient, these studies only deal with total volatility.

Finally, Hasbrouck and Saar (2011) find that HFT increases liquidity and CRS (2008) find that liquidity improves market efficiency.

In this section, I will further investigate this question by comparing the results of direct tests of price efficiency during days with high HFT activity to normal days. A common type of efficiency test measures whether prices are efficient with respect to a specific information set, and I use lagged order imbalances and market returns in this role.

2.6.1 Methodology

First, I apply tests loosely inspired by Chordia, Roll, and Subrahmanyam (2005) (CRS (2005) hereafter) and CRS (2008) to examine the incorporation of information from lagged order flows.[18] These tests exploit the concept that efficient prices will follow a random walk, and ex-ante conditioning information will not have explanatory power for future returns. CRS (2005) show that order flow imbalances in individual stocks from one period can predict returns in the next period over some short horizons. CRS (2008) show that the predictive value of lagged order flow imbalance increases on days when liquidity is low, and presents a test specification that I adapt to test the effects of HFT on efficiency. The basic form of the model I use is:

$$(2.6) \quad R_t = \alpha + \beta_1 OIB_{t-1} + \beta_2(OIB_{t-1} \text{ x } HFT) + \beta_3 MKT_t + \varepsilon$$

[18] This efficiency test is also used in Chung and Hrazdil (2010a, 2010b).

where R_t is the midpoint return calculated from TAQ midpoints, OIB_{t-1} is the lagged order imbalance, HFT is an indicator variable that identifies high-HFT participation days, and MKT is the SPY S&P 500 ETF midpoint return calculated from TAQ. I use midpoint returns instead of trade returns because predictability in transaction prices due to bid-ask bounce is not generally considered evidence of informational inefficiency. The HFT indicator in my model replaces the illiquid day indicator variable in CRS (2008), and is defined and discussed in Section 2.4. In different versions of this test, HFT participation is alternately calculated using all HFT trades, liquidity-demanding trades only, or liquidity supplying trades only. Following CRS (2008), I use 5-minute intervals to measure returns and order imbalances. I also use 1-minute intervals because CRS (2008) show that the 5-minute horizon predictability has diminished over time, and because HFT effects may be more pronounced at shorter horizons. OIB is defined as (Buyer Initiated Dollar Volume – Seller Initiated Dollar Volume) / Total Dollar Volume. OIB is measured over the same interval length as returns. MKT_t is included to reduce the correlation in the residuals across stocks. The regression is estimated one stock at a time, and the time series coefficients are averaged across stocks in a reverse of the Fama-MacBeth procedure. T-statistics are corrected for correlation in the regression residuals across stocks using the method in CRS (2008). This method adjusts the measured standard errors upwards by $[1+ (N - 1)\rho]^{1/2}$, where N is the number of individual regressions and is the mean pair-wise correlation across the residuals. If the relationship found in CRS (2005, 2008) holds in this sample, β_1 will be positive. If the market is more efficient when HFT activity is high, then the sum of β_1 and β_2 will be lower in absolute value than β_1, regardless of whether the CRS finding of a positive β_1 holds.

I employ a second set of efficiency tests using the price delay measures from Hou and Moskowitz (2005). While the CRS tests measure the incorporation of information in past order flow, price delay measures the incorporation of information from market index returns. There are at least two reasons to suspect HFT may affect the incorporation of index return information into individual stock prices. First, index returns are a plausible input variable to HFT strategies and index arbitrage is frequently mentioned in informal descriptions of suspected HFT behavior. Second, Jovanovic and Menkveld (2011) find that HFT activity is positively correlated to the explanatory power of the market index for a stock's returns. They attribute this effect to increased HFT activity when hard information has more value, but causality could run the other way as well.

Hou and Moskowitz (2005) refine procedures used earlier by Brennan, Jegadeesh, and Swaminathan (1993) and Mech (1993). While Hou and Moskowitz (2005) use price delay based on weekly data as a stock characteristic in asset pricing tests, I calculate price delay with 1-minute and 5-minute midpoint returns and employ it as an efficiency measure. To measure price delay, I first estimate the regressions:

$$R_t = \alpha + \beta_1 MKT_t + \delta_1 MKT_{t-1} + \delta_2 MKT_{t-2} \ldots + \delta_6 MKT_{t-6} + \varepsilon \quad (2.7)$$

$$R_t = \alpha + \beta_1 MKT_t + \varepsilon \quad (2.8)$$

where returns are defined as in Equation (2.6), and six lags of MKT are used. As in the order flow imbalance tests, I use both 5-minute and 1-minute intervals. These regressions are estimated one stock at a time, separately for high-HFT participation days and normal days. I refer to Equation (2.7) as the unrestricted model and Equation (2.8) as the restricted model. Then for each stock, I calculate the following price delay measures, separately on high-HFT participation days and normal days:

(2.9) $D1 = 1 - (R^2_{rest}/R^2_{unrest})$

(2.10) $D2 = (\delta_1 + 2\ \delta_2 + 3\ \delta_3 \ldots\ + 6\ \delta_6)/\ (\beta_1 + \delta_1 + 2\ \delta_2 + 3\ \delta_3 \ldots\ + 6\ \delta_6)$

(2.11) $D3 = (\ T(\delta_1) + 2\ T(\delta_2\ ) + \ \ldots\ + \ 6T(\delta_6))/\ (T(\beta_1) + T(\delta_1) + 2\ T(\delta_2) \ldots\ + \ 6T(\delta_6))$

where R^2_{rest} is the R^2 from Equation (2.8), R^2_{unrest} is R^2 the from Equation (2.7), T(.) is the t-statistic on the coefficient in Equation (2.7), and other terms are as defined in Equation (2.7). D1 is based on the procedure in Mech (1993) and can be interpreted as the additional explanatory power from the lagged returns as proportion of the total explanatory power of the unrestricted regression. Coefficient ratios similar to D2 and D3 were used in Brennan, Jegadeesh, and Swaminathan (1993), but the weightings were introduced by Hou and Moskowitz (2005). D2 gives more weight to coefficients on more distant lags of the market return. D3 is similar to D2 but gives more weight to coefficients that are estimated more precisely. Higher values of price delay reflect slower adjustment. For each stock, I calculate each price delay measure separately for high HFT days and normal days, defined as in the order imbalance tests above. Then within each stock, I subtract price delay measures on high HFT days from normal days, and average these differences across stocks.

If stock prices incorporate market-wide information more efficiently on days when HFT activity is high, then price delay should be lower on these days and the mean differences will be negative. When testing whether the mean differences are significantly different from zero, it is not clear that the differences can be considered independent observations. The inputs to the price delay measures are coefficients estimated from regressions on returns in the same sample period. For each stock, two regressions are run in separate subsamples, with different days entering the subsamples for each stock based on stock-specific HFT activity. If there are market-wide mechanisms that cause simultaneous price delays across multiple stocks, there will be some cross-sectional dependence in the differences because there is correlation between high-HFT days across stocks. To correct for this, I use the same standard error adjustment as in the order imbalance tests calculated from residuals on the unrestricted regression estimated over the full sample (not divided by HFT participation category). I believe this is a conservative approach because the sample splitting procedure should reduce the dependence relative to that in the order imbalance tests. I am not aware of any prior studies that address this issue.

2.6.2 Results

The results from the order flow imbalance tests are shown in Table 2.7. The mean coefficients on lagged order imbalance are positive and significant in all models except for the specification using 5-minute returns and liquidity-supplying HFT participation, where it is still positive and marginally significant. The number of stocks with positive and significant coefficients on lagged order imbalance in the individual regressions is higher than the number of stocks with negative significant coefficients, and often much higher. This is consistent with the findings in CRS (2008) for most of their models and subsamples. For both 5-minute and 1-minute returns, the explanatory power of lagged order imbalance is reduced on high-HFT days when HFT activity is defined using all HFT participation or liquidity-demanding HFT participation. As an illustration, consider the 5-minute returns with all HFT participation. The mean coefficient on lagged order imbalance is 0.0960. The mean coefficient on lagged order imbalance interacted with the HFT indicator is -0.0704. This means on high-HFT days, the predicted effect of lagged order imbalance is .0255 (0.0960 - .0704) compared to 0.0960 on normal HFT days, and the t-statistic of -2.69 on the coefficient on the interaction term is the test against the null that the difference in lagged OIB effect between the high and normal days is 0. There are 37 (out of 120) individual stock interaction coefficients that are significantly negative, while only are 2 significantly positive. The results for all specifications using all HFT participation or HFT liquidity-demanding participation are qualitatively similar, but are stronger for liquidity-demanding participation and with 5-minute returns. For example, the predictive power of order flow is reduced by roughly 20% on high-HFT demand days for 1-minute intervals, but it is almost completely removed for 5-minute intervals. In both specifications using HFT liquidity-supplying participation, the mean interaction terms are not significantly different from 0 and there is no strong pattern in individual coefficients.

The results from the price delay tests are shown in Table 2.8. Price delay is lower on high-all HFT participation days and high-HFT demand participation days in all specifications, and the effects using HFT supply participation days are weaker. This pattern is similar to the lagged order flow test results. For all participation and demand participation, the price delay difference point estimates are all negative and significant using raw t-statistics. Using the conservative t-statistics adjustment described above, the

5-minute differences all become insignificant. 1-minute differences are significant at the 10% level for D1 with HFT participation defined using all trades and are insignificant using HFT demand trades. For D2, the differences remain statistically significant at the 5% level in both specifications. For D3, the difference is significant at the 10% level using all HFT participation and significant at the 5% level using HFT demand participation. With participation defined using HFT liquidity supply, D2 and D3 differences are significantly negative at 5-minute horizons before the adjustment, and none are significant after. Differences are insignificant in other specifications before the adjustment and the point estimates are of mixed signs.

Overall, these results suggest prices are more efficient when HFT activity is high. Prices tend to reflect more of the information in past order flows and past market returns on high-HFT activity days, and the effect is stronger when they are demanding liquidity. Based on the evidence presented here alone, we cannot conclude that HFT activity causes market efficiency increases, only that there is a positive correlation. However, if HFTs possess comparative advantages in profitably exploiting pricing inefficiencies, it seems unlikely that HFTs choose to trade more and demand liquidity more when the market is more efficient. Also, the fact that the improvements in measured efficiency are observed primarily when HFT demand is high is relevant to a claim made in CRS (2008). They conjecture that the short-term predictive power of order imbalances is due to the limited ability of market makers to absorb the imbalances without causing price pressure. They argue that liquidity improves efficiency in this setting because arbitrage traders are more likely to trade on this predictability when liquidity is high, and they do so by submitting market orders or marketable limit orders. My results are consistent with a version of this story where HFTs play the role of arbitrageur, and inconsistent with a version where HFTs are enhancing efficiency by improving liquidity. It is also informative to interpret these results in the context of the finding in Brogaard (2012b) that HFTs tend to trade in the same direction as past order flow. The predictive relationship, when present, is that buying pressure in one interval predicts positive returns in the next and vice-versa. If HFT trading weakens this relationship, this would suggest that HFTs either trade with the contemporaneous order flow, against the lagged order flow, or both. Brogaard's results are inconsistent with the second mechanism and do not address the first. However, Brogaard studies order flow at shorter intervals so this result is not directly comparable.

Price delay reductions on high-HFT days are generally larger at 1-minute horizons than at 5-minute horizons, and this is different from what we observed in the lagged order flow tests. This could be interpreted as supporting the conjecture in Jovanovic and Menkveld (2011) that HFTs trade more aggressively on hard information, as it is easier to envision updating a stock's fair value after observing an index return than after an order flow imbalance.

2.7 Trading Behavior

2.7.1 Timing

The trading costs reported in Section 2.5 can, with some assumptions, be interpreted as trading profits earned by HFT. This is somewhat intuitive, and a related analysis is performed in Menkveld (2012). From Table 2.4, using the 5-minute decomposition, we could estimate that HFTs on average lose 0.3 bps per trade when providing liquidity to non-HFTs, and earn 1.0 bps per trade when demanding liquidity from non-HFTs (before rebates and fees).[19] Two of the required assumptions for this calculation may be incorrect in this application, however. First, we only observe a subset of HFT trades in the sample stocks. We miss trades that occur in crosses or in other trading venues. While the working assumption is that HFTs end the day flat or close to it, inspection of the data shows that the trades in the sample often add up to substantial apparent positions at the end of the day. It is not possible to tell if these positions were offset out of view or are actual overnight positions. And if they were offset, it is difficult to estimate at what price. The offsetting trades could have been done at the opening cross, the closing cross, or any price traded in sufficient size on any other trading venue. The second assumption is that positions are exited at the 5-minute (or 30-minute in the alternate decomposition) posttrade benchmark on average. These are useful benchmarks from a market quality perspective, but we do not expect that HFTs typically offset their positions mechanically after a fixed interval. If HFTs have trading skills that allow them to strategically time the reversal of their positions, then realized spreads would understate their profits.

I attempt to determine whether HFTs have trade timing skills for two main reasons. First, the estimates from the realized spreads are very small and suggest that HFTs are willing to trade for miniscule profits. If they are in fact doing this, they are providing liquidity for little compensation beyond the rebate and bringing very granular information into prices when they take liquidity. If they are instead trading based on superior price forecasts, these interpretations would be an overly optimistic description of their trading behavior and the apparent liquidity they provide could be overstated. Under

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[19] These approximations ignore the fact that price impact is not incurred on exits.

that scenario, the liquidity provided by HFT in the sample trades was only available to counterparties trading against their price forecasts, and was not offered because they perceived the measured spreads to be an adequate incentive to provide liquidity. This distinction has implications for the affirmative obligation proposals. If their profits did not really come from very small per-trade amounts scaled over very large volumes, would HFTs be economically viable if they were forced to trade less selectively? Second, little is known about the intraday predictability of stock prices. Analogous to the search for signs of longer horizon predictability in the asset manager performance literature, HFT trading performance and behavior is a natural setting to search for signs of short-term predictability.

In this section, I attempt to shed light on this question by measuring their trading performance using VWAP (Volume-Weighted Average Price) analysis.[20] By comparing the VWAPs of their buys and sells against the day's VWAP and each other, I can measure the performance of the trades in this sample against a useful benchmark without making any assumptions about the prices or times of the unobserved trades. When subtracting the VWAP of HFT buys from HFT sales, this provides an intuitive measure of their trading performance over a day and is unaffected by their impact on the market VWAP. I also calculate these separately for liquidity-demanding and liquidity-providing trades. This approach is related to the floor trader performance measure from Manaster and Mann (1996) and the traded spread from Stoll (2000).

The results of this analysis are presented in Table 2.9. All VWAP differences are signed so a positive number indicates positive trading performance (i.e., market VWAP -

[20] See Berkowitz, Logue, and Noser (1988).

HFT buy VWAP will be positive if HFTs buy below the market VWAP). The basic VWAP calculations and each difference calculation are performed daily for each stock and summarized across stock-days, sample days, and stocks. Panel A reports summary statistics averaged across all stock-days. The mean of the difference between HFT sell VWAP and HFT buy VWAP is 6.5 bps. Positive skewness is evident, as the median difference is only 2.3 bps. Buys and sells contribute about equally (3.3 bps below market VWAP and 3.2 bps above market VWAP, respectively). This performance is driven by liquidity-providing trades. For liquidity-providing HFT trades, the mean sell VWAP - buy VWAP difference is 12.8 bps, while it is only 2.3 bps for liquidity-demanding trades. The VWAP differences relate to performance on round trips, while the performance implied by realized (half) spreads mentioned above is for a one-way transaction. A comparison with the realized spreads doubled suggests that realized spreads understate HFT trading performance for liquidity-demanding trades somewhat and understate their performance when providing liquidity dramatically.[21] Note that this comparison is not entirely clean because it is based on different samples, as I compute realized spreads only on days where quotes are available and VWAP measures for all sample days, and there are also weighting differences.[22]

In Panel B, the VWAP differences for each stock are averaged over each sample day, and the resulting daily values are then averaged to produce a time series of daily measures. The standard deviation and skewness observed in Panel A decrease, which is

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[21] The difference is more dramatic when correcting for 1-way price impacts. Assuming no-timing ability, using effective spreads from Table 4 and considering price impacts on entries but not on exits, implied Sell VWAP - VWAP Buy differences are 2.5 bps for liquidity-providing round trip trades and -1.1 bps for liquidity-demanding round trip trades.

[22] A possible next step is to analyze a subsample of VWAP differences on days where there are quotes available, and use benchmarks based on volume-weighted mean spreads calculated by stock day to correct the weighting issues.

not surprising because I am essentially creating an equal-weighted portfolio of all the sample stocks every day. The consistency of the HFT liquidity-providing trade performance over time becomes apparent. On the 10th percentile day, the mean sell VWAP - buy VWAP difference across all stocks is positive 2.7 bps. In Panel C, the differences are averaged over all sample days for each stock, and then summarized across stocks. These results highlight another dimension of HFT liquidity-provision performance consistency. In the 10th percentile stock, the sell VWAP - buy VWAP difference is positive 1.9 bps. The information in Panel B and C is shown graphically in Figure 2.4. In Panel A, we see that there are some very high-performance days, and high-performance days outnumber low-performance days. From Panel C, we observe that overall performance is distributed relatively evenly across stocks, and liquidity-demanding performance is negatively correlated with liquidity-supplying performance. One final note on Table 2.9: all of the mean differences in the table indicate positive performance and all are significantly different from 0 at the 5% level or higher, with the exception of sell VWAP - market VWAP in Panel C.

At what horizon do HFTs have market timing ability? We might expect this ability to be concentrated at the shortest horizons based on their investments in very low-latency technology and various assertions in the press. This relates to questions about the nature of intraday return predictability, whether HFTs are willing to risk their capital on expected price changes that take longer to play out, and also on HFTs potential effects on price formation. I investigate this issue in two ways. First, I decompose the sell VWAP - buy VWAP differences reported in Table 2.9 into shorter term and longer term components. I replace the price on each HFT trade with the market VWAP for the 5-

minute interval in which the trade occurred, and recalculate daily HFT buy and sell VWAPS for each stock using these transformed prices. I call these HFT positioning VWAPS, and refer to the sell HFT positioning VWAP - HFT buy positioning VWAP difference as HFT positioning performance. This procedure removes the effects of HFT market timing within 5-minute intervals, and leaves only the effect of HFTs' choices of how much to buy or sell in a given 5-minute interval. If HFT market timing performance is only due to their short-term timing ability, then their positioning performance should be close to zero. I also measure the difference between the actual sell HFT VWAP – HFT buy VWAP and the HFT positioning performance on each stock-day, and call this the HFT short-term timing performance. Given the similar results across weighting schemes in Table 2.9, I only conduct this analysis with stock-day weighting. For my second test, for every stock-day, I rank each 5-minute interval by market VWAP and observe the variation in HFT activity across groups of intervals. This is designed to reveal how their trading activity differs across lower and higher price periods, and the use of 5-minute VWAPs as the measure of price focuses the test on positioning performance and away from fleeting prices. There are 78 5-minute intervals in the trading day, so I rank the intervals into 13 groups, giving each group six 5-minute intervals. The HFT activity measures I consider are net normalized HFT dollar volume and HFT order imbalance. I define net normalized HFT dollar volume as HFT buy dollar volume less HFT sell dollar volume divided by total HFT dollar volume traded. HFT order imbalance is calculated with the same formula but uses only trades where HFTs demand liquidity, and is identical to the order imbalance used in Section 2.6 with only HFT trades. If HFT market timing skill is concentrated at very high frequencies, then there should be little or no difference in their activity between low-price and high-price 5-minute intervals. This approach also reveals whether the HFT positioning performance observed above comes from trades at the extreme prices of the day or is shown more continuously across the distribution of prices, and whether this differs across liquidity-demanding and supplying trades.

The results of the decomposition analysis are presented in Table 2.10. The overall stock-day-weighted HFT sell-buy VWAP differences from Table 2.9 are repeated in Panel A for convenience. Panel B reports summary statistics on HFT positioning performance. For all trades, 5.0 bps of the 6.5 bps overall mean sell-buy VWAP difference is attributable to positioning performance. For liquidity-demanding trades, their positioning performance of 2.8 bps is actually higher than their 2.3 bps sell - buy VWAP difference. For liquidity-demanding trades, 8.1 bps of their 12.8 bps mean sell - buy VWAP difference is attributable to positioning performance. All of the positioning performance estimates are highly statistically significant. HFT positioning performance also seems to inherit much of the positive skewness in their overall sell-buy VWAP differences. Panel C reports summary statistics on HFT short-term timing performance. These are the component of HFT sell - buy VWAP difference that is not explained by their positioning performance, and can be interpreted as a measure of HFT's ability to time the market within 5-minute intervals. HFT short-term timing performance for all trades and liquidity-demanding trades is positive, while it is negative for liquidity-demanding trades. All of the short-term timing performance estimates are highly statistically significant. HFT short-term timing performance is also positively skewed, but less so than positioning performance. It is noteworthy that HFT short-term timing performance on liquidity-demanding trades is negative. This suggests that without HFT's positioning skill, their short-term timing ability in these trades is not sufficient to overcome the bid-ask spread. Overall, these results are striking. HFTs would retain most of their market timing ability if they transacted at the market VWAPs for the 5-minute intervals in which their trades occur. HFT positioning performance is greater than short-term timing performance for all trade categories. This even holds for their liquidity-providing trades, where we might expect most of their performance to come from earning the spread when it is wide. Despite the attention paid to HFT investments in the arms race to achieve the lowest possible latencies, it seems that their pricing models could be more important than their speed.

The results of the HFT activity analysis across intervals ranked by VWAP within each stock day are presented in Table 2.11. Based on the positioning performance results in Table 2.10, we can expect HFTs to buy more than they sell in 5-minute intervals when prices are low, and vice-versa. This is in fact what we observe, both for their overall trading (HFT Net Dollar Volume Ratio) and liquidity-demanding trades (HFT Order Imbalance). In addition to confirming the results in Table 2.10, this analysis also sheds light on whether this is solely driven by trading behavior at the extreme prices or is more continuous. The difference in HFT Net Dollar Volume Ratio between Group 13 (high prices) and Group 1 (low prices) is -.074 and is highly significant. Similarly, the difference in HFT Order Imbalance between Group 13 and Group 1 is -.042 and is also highly significant. This confirms that at least part of the HFT positioning performance shown previously is driven by trading in the correct direction during the extreme price intervals of the day. Further inspection shows evidence that this positioning performance is relatively continuous as well. In Groups 1-5 (the five 5-minute intervals of the day with the lowest prices), HFT net buying is significant, and in Groups 7-13 (the seven 5-minute intervals of the day with the highest prices), HFT net selling is significant. Similarly, in Groups 1-6, HFT Order Imbalance is significantly positive, and in Groups 11-13, HFT Order Imbalance is significantly negative. Across groups as prices move from low to high, HFT Net Dollar Volume Ratios are monotonically decreasing, and HFT Order Imbalances are near-monotonically decreasing. This continuous pattern reveals insights about the nature of intraday return predictability, but there is another arguably more important implication. If HFTs were systematically profiting from pushing prices to their extremes and then reversing their positions, we would expect to see very different patterns, such as selling pressure in the intervals in Group 2 or buying pressure in Group 12. While I cannot rule out that this behavior occurs sporadically or at horizons I do not study, I find no signs of it in this analysis.

I interpret these results in this section as suggesting that HFTs possess intraday market timing skills, buying when prices are temporarily low and selling when prices are temporarily high. This suggests that there is economically significant predictability in intraday prices. These timing skills are not driven by very short-term signals, and are not limited to trades made during the periods with the extreme prices of the day. Finally, HFT liquidity-providing trades outperform their liquidity-demanding trades. This raises the question why they engage in so many liquidity-demanding trades. As discussed in Section 2.4, half or more of HFT dollar trading volume is liquidity-demanding. It is possible that some of these trades are motivated by inventory rebalancing or other risk considerations instead of profits. It is also possible that HFT liquidity-demanding trades are motivated by more time-sensitive information than their liquidity-supplying trades, or that they are in the position of having employed as much capital as possible in liquidity provision and have excess capital they are willing to employ in less attractive (but still profitable) liquidity-demanding strategies.

2.7.2 Predictive Positioning

Do the positions HFTs take predict short-term cross-sectional stock returns? This is of interest for some of the same reasons that motivated the study of timing in the previous section. If the stocks that HFTs are holding or actively buying in one period outperform the stocks they are short or actively selling, that would suggest there is short-term predictability in the cross section of returns. It would also give us insights into how they allocate their capital. If HFTs trade in and out of stocks based on short-term signals, then it is unlikely that we can expect them to consistently dedicate market-making capital to specific stocks. There are several reasons to suspect HFTs may trade based on relative expected returns. First, the results from the price delay tests in Section 2.6 are consistent with relative value trading. Second, studying longer horizons, the empirical asset pricing literature has found stronger evidence of cross-sectional predictability than time-series predictability. If this is true at shorter horizons as well, it is likely that HFTs would take advantage of this. Third, by actively balancing their long and short positions, HFTs can reduce market risk, and to the extent they are able to predict relative returns, it would be natural for them to incorporate this information into their hedging choices. Finally, in various informal descriptions, HFTs are thought to engage in relative value trading, pairs trading, or "arbitrage."

In this test, I form decile portfolios based on HFTs' stock-specific trading activity and compare the 1-minute and 5-minute midpoint returns on the portfolios they have bought to those on the portfolios they have sold. I use two measures of their buying and selling activity. First, as a proxy for their overall positions at the start of each interval, I measure their cumulative net trading volume in each stock from the start of the day until the end of the prior interval. Second, as a proxy for their recent trading, I calculate the lagged position change as the cumulative net volume in each stock over the prior interval. This measure may be more informative if HFTs are not agile enough to move their entire portfolios in the direction of predicted future returns over a short period of time but do start trading in the correct direction, or if the first measure contains too much accumulated error from trades on other venues over the course of the day, as discussed in Section 2.7.1. Position changes are also measured with error but, because the errors accumulate over the measurement horizon, the position change errors should be smaller than the position errors. At the beginning of each interval, stocks are ranked into deciles based on the HFT positions and position changes, and returns are calculated for each decile and a 10-1 spread portfolio. Variations of this procedure are common in the empirical asset pricing literature to test whether a category of investor's holdings or change in holdings are related to future returns. See Yan and Zhang (2009) for an example that tests whether short-term institutional investor's equity trades are informed.

The results are shown in Table 2.12. The stocks HFTs hold or bought actively in the prior period tend to underperform the stocks they are short or sold actively. The spread portfolio returns are all negative, and are statistically significant with the exception of the position portfolios at 5-minute holding periods. They range from -0.05

bps for the 1-minute position decile spread portfolio to -0.70 bps for the 5-minute position change decile spread portfolio. As a robustness test, I ran Fama-Macbeth regressions of returns on the positions and position changes (unreported). These show qualitatively similar results: the coefficients on the positions and position changes are either significantly negative or insignificant.

Table 2.12 also reports information on the mean positions and position changes in each portfolio. This helps understand the typical magnitudes of HFT positions in each stock, how fast they change, and whether there are tendencies towards net long or short position or if positions tend to offset across stocks. The mean estimated HFT short position for a stock in the 5-minute decile 1 portfolio is -$4,576,538 and the long position in the decile 10 portfolio is $4,426,550. One-minute portfolios are similar. For the 5-minute position change portfolios, there is $347,750 of selling for the average stock in decile 1 and of $352,802 of buying for the average stock in decile 10. For the 1-minute position change portfolios, there is $129,778 of selling in decile 1 and of $130,409 of buying in decile 10. However, it is not correct to interpret this as evidence that HFTs almost completely offset their long positions with short positions, and their buying with selling, because this sample does not include the whole universe of their trades and these are unconditional mean values. A more informative approach is to divide the absolute value of their net positions (position changes) by their gross positions (position changes). Performing this analysis on 5-minute positions, the mean of this ratio is 29%, and the 90th percentile ratio is 58%. Similarly, for 5-minute position changes, the mean of this ratio is 30%, and the 90th percentile ratio is 60%. There are occasionally periods when HFT positions or position changes appear very directional, however. For example, there are

500 1-minute intervals in the sample where the net position changes are 95% or more of the gross. To the extent this sample is representative, it appears HFTs tend to offset some but not all of their long and short positions (position changes), and occasionally trade very directionally.

The magnitudes of the negative spread portfolio returns are small, and are within the mean quoted bid-ask spreads. The strongest conclusion I can draw is that I do not find evidence that their positions or trades positively predict relative returns at these horizons. This should not be interpreted as meaning they are losing money. We do not expect them to trade at the midpoints or exactly at these frequencies. It is also important to note these 120 stocks are presumably a small sample of the assets they trade. The most likely explanation for the small negative returns seems to be liquidity effects. We may be observing the effect of small reversals after the positions HFTs actively take create price pressure. The use of midpoint returns is intended to mitigate this effect, but does not necessarily do so perfectly.

2.8 Conclusion

In this paper, I analyze market quality and HFT trading behavior using a large sample of NASDAQ trades and quotes with HFT participation explicitly identified. I find that trading costs are unconditionally low in this market, but spreads are slightly wider for trades where HFTs provide liquidity and slightly tighter when HFTs take liquidity. This suggests that HFTs provide liquidity when it is scarce and consume liquidity when plentiful. Prices incorporate information from order flow and market-wide returns more efficiently on days when HFT participation is high. This effect is driven by HFT demand-side participation, implying that HFTs improve price efficiency when demanding liquidity. I also find that HFTs seem to possess intraday market timing ability, but I find no evidence that they trade to exploit predictability in cross-sectional expected returns at the horizons I study.

The new evidence in this paper is relevant to the ongoing HFT-related policy debates and can potentially provide guidance to theoretical researchers seeking to model HFT behavior and market quality impacts. Of particular interest, the trading cost and market timing analysis have implications for the proposals to impose affirmative obligations to provide liquidity on HFTs. The trading costs I document indicate that there is little compensation in the spread for naïve liquidity provision in this market. The evidence on HFT intraday market timing suggests that HFT profits are not driven by spreads alone, and it is not clear that their strategies would be economically viable if affirmative obligations prevented them from exercising discretion in when to trade.

The results in this study suggest that HFTs play a beneficial or neutral role in the market. However, it is important to note that my data are limited to NASDAQ continuous trading and my focus is on unconditional systematic effects. These issues and other limitations of this study are discussed in more detail in previous sections. Conclusions drawn in this setting may not generalize to other environments, and continued study of these issues is clearly warranted. In particular, the impact of HFT on market quality in extreme market conditions is an important topic for future research.

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Table 2.1 Sample Stocks

Sample was selected for NASDAQ by Terrence Hendershott and Ryan Riordan. Sample period is January 2008 – December 2009 and February 22, 2010 – February 26, 2010. Listing venue, price, and market capitalization are from CRSP as of February 26, 2010. Dollar trading volumes are from TAQ for trades between 9:30 am and 4:00 pm and are averaged over all days in sample for each name. Stocks are sorted in descending order by market capitalization.

					Avg. Dollar Trading Volume		
Ticker	Name	Listing	Market Cap (billions)	Price	Nasdaq	CT	NASDAQ Share
AAPL	APPLE INC	NASDAQ	185.548	204.62	1,654.244	4,229.780	38.2%
PG	PROCTER & GAMBLE CO	NYSE	183.803	63.28	186.570	889.604	20.7%
GE	GENERAL ELECTRIC	NYSE	171.357	16.06	350.167	1,710.448	19.4%
PFE	PFIZER INC	NYSE	141.635	17.55	190.253	908.159	20.4%
CSCO	CISCO SYSTEMS INC	NASDAQ	139.305	24.33	544.225	1,216.373	43.7%
GOOG	GOOGLE INC	NASDAQ	128.612	526.80	976.411	2,197.014	43.5%
HPQ	HEWLETT PACKARD	NYSE	119.564	50.79	168.456	762.414	21.6%
INTC	INTEL CORP	NASDAQ	113.408	20.53	557.855	1,285.776	42.6%
DIS	DISNEY WALT CO	NYSE	60.590	31.24	81.993	377.655	20.9%
MMM	3M CO	NYSE	57.045	80.15	65.307	329.624	19.3%
AMGN	AMGEN INC	NASDAQ	55.438	56.61	243.441	532.812	45.6%
AMZN	AMAZON COM INC	NASDAQ	52.715	118.40	306.855	727.308	42.9%
AXP	AMERICAN EXPRESS CO	NYSE	45.703	38.19	111.628	493.304	22.1%
GILD	GILEAD SCIENCES	NASDAQ	42.952	47.61	200.488	435.583	45.7%
CMCSA	COMCAST CORP	NASDAQ	33.884	16.44	192.761	417.744	45.8%
DOW	DOW CHEMICAL CO	NYSE	32.565	28.31	60.744	293.240	20.6%
HON	HONEYWELL INTERNATIONAL	NYSE	30.704	40.16	54.990	242.639	22.2%
EBAY	EBAY INC	NASDAQ	29.903	23.02	159.904	363.127	43.7%
PNC	P N C FINANCIAL SERVICES GRP INC	NYSE	27.792	53.76	51.977	303.458	17.3%
GLW	CORNING INC	NYSE	27.483	17.63	63.275	276.531	22.1%
CELG	CELGENE CORP	NASDAQ	27.363	59.52	130.188	281.530	45.9%
COST	COSTCO WHOLESALE CORP	NASDAQ	26.850	60.97	144.949	330.337	43.5%
ESRX	EXPRESS SCRIPTS	NASDAQ	26.445	96.01	89.624	193.355	46.2%
MOS	MOSAIC COMPANY	NYSE	25.993	58.39	110.995	519.619	19.5%
DELL	DELL INC	NASDAQ	25.911	13.24	200.338	464.816	41.8%
KMB	KIMBERLY CLARK	NYSE	25.286	60.74	27.274	163.386	16.4%
ADBE	ADOBE SYSTEMS	NASDAQ	18.211	34.65	115.867	245.726	46.5%
AGN	ALLERGAN INC	NYSE	17.763	58.43	23.249	124.567	18.2%
CB	CHUBB CORP	NYSE	16.753	50.46	29.610	168.466	17.5%
AMAT	APPLIED MATERIALS INC	NASDAQ	16.442	12.24	150.360	339.454	43.4%
GENZ	GENZYME CORP	NASDAQ	15.221	57.20	102.936	220.846	46.9%
BIIB	BIOGEN IDEC INC	NASDAQ	15.120	55.01	86.319	193.493	44.7%
BHI	BAKER HUGHES INC	NYSE	14.946	47.92	64.653	275.455	22.8%
GPS	GAP INC	NYSE	14.835	21.50	40.019	170.821	23.6%
SWN	SOUTHWESTERN ENERGY CO	NYSE	14.726	42.55	44.165	214.302	20.7%
KR	KROGER COMPANY	NYSE	14.363	22.10	39.537	191.461	20.4%
CTSH	COGNIZANT TECHNOLOGY SOLS	NASDAQ	14.322	48.13	63.043	145.206	43.7%

Table 2.1 Continued

Ticker	Name	Listing	Market Cap (billions)	Price	Avg. Dollar Trading Volume Nasdaq	CT	NASDAQ Share
BRCM	BROADCOM CORP	NASDAQ	13.737	31.32	128.856	295.870	43.3%
AA	ALCOA INC	NYSE	13.570	13.30	90.068	410.228	21.4%
ISRG	INTUITIVE SURGICAL INC	NASDAQ	13.516	347.14	99.863	220.226	45.3%
CSL	CARLISLE COMPANIES	NYSE	2.064	34.30	2.569	14.552	17.1%
AINV	APOLLO INVESTMENT CORP	NASDAQ	2.055	11.66	8.404	19.069	42.2%
LECO	LINCOLN ELECTRIC HOLDINGS INC	NASDAQ	2.034	47.70	9.476	17.911	52.4%
SFG	STANCORP FINANCIAL GROUP	NYSE	2.030	42.98	2.975	17.587	16.9%
FL	FOOT LOCKER INC	NYSE	2.030	12.97	5.874	30.986	19.2%
ERIE	ERIE INDEMNITY CO	NASDAQ	2.029	39.62	2.739	4.706	57.2%
LSTR	LANDSTAR SYSTEM	NASDAQ	2.004	39.89	15.164	31.165	47.6%
CNQR	CONCUR TECHNOLOGIES INC	NASDAQ	1.943	39.34	12.837	27.586	46.9%
EWBC	EAST WEST BANCORP INC	NASDAQ	1.927	17.52	8.506	19.787	42.2%
JKHY	HENRY JACK & ASSOC INC	NASDAQ	1.908	22.58	8.708	17.120	49.8%
FCN	F T I CONSULTING	NYSE	1.904	36.74	9.878	50.463	20.3%
CBT	CABOT CORP	NYSE	1.899	29.06	2.247	12.376	17.6%
PNY	PIEDMONT NATURAL GAS INC	NYSE	1.895	25.83	2.057	12.067	16.2%
GAS	NICOR INC	NYSE	1.884	41.65	4.612	23.929	18.6%
BRE	B R E PROPERTIES	NYSE	1.860	33.71	5.196	36.006	14.5%
CR	CRANE CO	NYSE	1.855	31.67	2.130	11.512	17.7%
FMER	FIRSTMERIT CORP	NASDAQ	1.838	21.13	10.510	20.940	49.8%
COO	COOPER COMPANIES INC	NYSE	1.832	40.06	3.291	19.065	16.6%
ISIL	INTERSIL CORP	NASDAQ	1.826	14.84	23.484	53.516	43.5%
MELI	MERCADOLIBRE	NASDAQ	1.815	41.14	11.878	28.770	40.0%
ROC	ROCKWOOD HOLDINGS INC	NYSE	1.782	23.99	2.556	13.784	18.5%
CSE	CAPITALSOURCE	NYSE	1.777	5.50	5.377	30.523	15.2%
CHTT	CHATTEM INC	NASDAQ	1.774	93.48	12.617	27.768	46.3%
ARCC	ARES CAPITAL	NASDAQ	1.737	13.07	4.232	9.884	42.6%
CKH	SEACOR HOLDINGS	NYSE	1.727	76.38	3.585	18.195	19.1%
NSR	NEUSTAR INC	NYSE	1.726	23.18	3.201	15.541	19.8%
PTP	PLATINUM UNDERWRITERS HLDGS LTD	NYSE	1.710	37.39	2.958	20.932	14.2%
CPWR	COMPUWARE CORP	NASDAQ	1.703	7.49	11.685	26.184	44.2%
FULT	FULTON FINANCIAL CORP PA	NASDAQ	1.697	9.62	7.652	15.745	47.2%
AYI	ACUITY BRANDS	NYSE	1.692	38.98	4.146	22.083	18.6%
SF	STIFEL FINANCIAL	NYSE	1.689	54.70	2.434	14.160	17.5%

Table 2.1 Continued

Ticker	Name	Listing	Market Cap (billions)	Price	Avg. Dollar Trading Volume Nasdaq	CT	NASDAQ Share
NUS	NU SKIN ENTERPRISES INC	NYSE	1.676	26.72	1.120	6.844	14.9%
BARE	BARE ESCENTUALS	NASDAQ	1.674	18.18	5.483	14.289	36.3%
LPNT	LIFEPOINT HOSPITALS INC	NASDAQ	1.673	30.50	11.012	25.294	44.2%
CRI	CARTERS INC	NYSE	1.664	28.66	3.179	18.992	16.9%
AMED	AMEDISYS INC	NASDAQ	1.630	57.65	16.055	37.936	44.7%
BXS	BANCORPSOUTH	NYSE	1.625	19.47	2.892	17.268	16.3%
LANC	LANCASTER COLONY CORP	NASDAQ	1.623	57.54	3.880	7.474	52.6%
CETV	CENTRAL EUROPEAN MEDIA ENT LTD	NASDAQ	1.513	26.99	10.819	23.362	46.5%
MANT	MANTECH INTERNATIONAL CORP	NASDAQ	1.106	49.38	7.760	15.189	50.8%
NXTM	NXSTAGE MEDICAL	NASDAQ	0.498	10.65	0.449	1.013	43.6%
CTRN	CITI TRENDS INC	NASDAQ	0.438	29.74	2.481	5.538	45.9%
RVI	RETAIL VENTURES	NYSE	0.438	8.94	0.210	1.410	13.4%
MAKO	MAKO SURGICAL	NASDAQ	0.436	13.21	0.283	0.703	37.4%
MOD	MODINE MANUFACTURING	NYSE	0.435	9.40	0.554	2.924	16.3%
ROG	ROGERS CORP	NYSE	0.433	27.45	0.898	4.084	18.8%
KTII	K TRON INTL INC	NASDAQ	0.424	149.46	0.684	1.369	49.8%
KNOL	KNOLOGY INC	NASDAQ	0.421	11.45	0.798	1.723	47.3%
PPD	PRE PAID LEGAL SERVICES INC	NYSE	0.418	41.64	0.633	3.928	14.7%
DCOM	DIME COMMUNITY BANCSHARES	NASDAQ	0.418	12.14	1.679	3.339	49.7%
BW	BRUSH ENGINEERED MATERIALS INC	NYSE	0.416	20.54	1.160	5.631	19.4%
SJW	S J W CORP	NYSE	0.415	22.44	0.585	2.416	17.6%
MRTN	MARTEN TRANSPORT LTD	NASDAQ	0.412	18.84	1.310	2.900	46.2%
FPO	FIRST POTOMAC REALTY TRUST	NYSE	0.411	13.68	0.395	2.884	13.2%
IPAR	INTERMEDIATE PARFUMS INC	NASDAQ	0.410	13.58	0.751	1.540	48.9%
FRED	FREDS INC	NASDAQ	0.407	10.35	2.383	5.271	44.9%
MDCO	MEDICINES COMPANY	NASDAQ	0.407	7.70	4.613	10.934	42.7%
MIG	MEADOWBROOK INSURANCE GROUP	NYSE	0.405	7.08	0.212	1.697	12.7%
ANGO	ANGIODYNAMICS	NASDAQ	0.402	16.26	1.305	2.790	44.9%
PBH	PRESTIGE BRANDS HOLDINGS INC	NYSE	0.402	8.03	0.334	2.309	14.2%
BZ	BOISE INC	NYSE	0.401	4.75	0.224	2.256	13.0%

Table 2.1 Continued

Ticker	Name	Listing	Market Cap (billions)	Price	Avg. Dollar Trading Volume: Nasdaq	CT	NASDAQ Share
CDR	CEDAR SHOPPING CENTERS INC	NYSE	0.400	6.59	0.388	2.843	12.9%
APOG	APOGEE ENTERPRISES INC	NASDAQ	0.400	14.29	2.545	5.498	46.3%
MFB	MAIDENFORM BRANDS INC	NYSE	0.399	17.22	0.276	1.982	14.0%
EBF	ENNIS INC	NYSE	0.397	15.37	0.228	1.731	13.7%
FFIC	FLUSHING FINANCIAL CORP	NASDAQ	0.395	12.69	1.000	1.975	49.8%
CPSI	COMPUTER PROGRAMS & SYSTEMS INC	NASDAQ	0.394	35.94	1.946	4.217	47.5%
RIGL	RIGEL PHARMACEUTICALS	NASDAQ	0.392	7.55	4.175	9.221	43.0%
ABD	A C C O BRANDS	NYSE	0.391	7.17	0.406	2.973	13.3%
DK	DELEK U S HOLDINGS INC	NYSE	0.390	7.27	0.426	2.433	16.8%
CRVL	CORVEL CORP	NASDAQ	0.389	32.20	0.826	1.527	52.7%
CBZ	CBIZ INC	NYSE	0.389	6.23	0.284	2.235	13.4%
AZZ	A Z Z INC	NYSE	0.388	31.41	1.118	6.523	16.9%
CCO	CLEAR CHANNEL OUTDOOR HLDGS	NYSE	0.387	9.52	0.618	4.114	15.1%
BAS	BASIC ENERGY SERVICES INC	NYSE	0.385	9.45	1.075	5.781	17.4%
IMGN	IMMUNOGEN INC	NASDAQ	0.379	6.61	0.895	2.269	41.4%
MXWL	MAXWELL TECHNOLOGIES INC	NASDAQ	0.366	13.86	1.130	2.476	45.4%
CBEY	CBEYOND INC	NASDAQ	0.360	12.41	3.403	7.301	45.2%
ROCK	GIBRALTAR INDUSTRIES INC	NASDAQ	0.353	11.68	2.079	4.423	46.2%
NC	NACCO INDUSTRIES	NYSE	0.313	46.80	0.437	2.549	17.0%

Table 2.2 Trade Summary Statistics

Trade and Inside Quote (BBO) data provided by NASDAQ. Trade sample period is January 2008 – December 2009 and February 22, 2010 – February 26, 2010. Trades are missing on October 10, 2008. Quote sample period is the first full week of each quarter during 2008 and 2009, September 15, 2008 – September 19, 2008 (the week of Lehman's failure), and February 22, 2010 – February 26, 2010. Only trades between 9:30 am and 4:00 pm are used.

Descriptive Statistics	Full Sample	Matched w/ quotes
Days in Sample	509	49
Number of Trades	550,118,372	61,272,712
Total Share Volume (millions)	105,772	11,642
Total Dollar Volume (millions)	3,919,037	443,996
Trade size		
Mean	192.3	190.0
Std Dev	449.2	447.3
10th %ile	50	58
Median	100	100
90th %ile	400	398
Num of Trades/Day		
Mean	1,080,783	1,250,464
Std Dev	393,491	570,385
10th %ile	691,279	634,906
Median	1,009,167	1,091,299
90th %ile	1,575,009	2,263,314
Daily Share Volume (millions)		
Mean	208	238
Std Dev	73	97
10th %ile	130	132
Median	197	209
90th %ile	298	396
Daily Dollar Volume (millions)		
Mean	7,699	9,061
Std Dev	3,147	4,158
10th %ile	4,439	4,537
Median	6,892	7,740
90th %ile	11,912	15,076

Table 2.3 HFT Participation Dollar Volume Shares

HFT participation shares are measured as dollar volume of sample trades with HFT participation divided by total dollar volume of sample trades. Three versions of participation shares are calculated, differing in whether HFT participation is defined as trades where an HFT participates in any side (All), the liquidity-demanding side (Demand), or the liquidity-supplying side (Supply). Trades where an HFT participates in both sides are used in all three measures. Trade data provided by NASDAQ. Trade sample period is January 2008 – December 2009 and February 22, 2010 – February 26, 2010. Trades are missing on October 10, 2008. Only trades between 9:30 am and 4:00 pm are used.

HFT Participation Definition	All	Demand	Supply
Full Sample Pooled	68.3%	42.2%	41.2%
Daily Pooling			
N	509	509	509
Mean	68.5%	42.7%	41.1%
Std Dev	2.8%	3.6%	2.4%
10th %ile	65.2%	37.9%	38.2%
Median	68.3%	42.7%	41.1%
90th %ile	72.3%	47.8%	44.1%
Stock-Day Pooling			
N	61,014	61,014	61,014
Mean	48.3%	32.5%	23.2%
Std Dev	20.5%	15.4%	16.8%
10th %ile	19.9%	10.9%	5.5%
Median	49.2%	33.2%	17.9%
90th %ile	75.0%	52.6%	50.5%
Stock Pooling			
N	120	120	120
Mean	48.3%	32.5%	23.2%
Std Dev	17.7%	11.9%	15.0%
10th %ile	25.1%	15.4%	10.2%
Median	46.4%	34.2%	15.7%
90th %ile	72.6%	47.1%	49.4%
Within-Stock Variation			
(differences between means on high participation and normal participation days for each stock)			
N	120	120	120
Mean	16.6%	16.1%	12.5%
Std Dev	5.4%	4.1%	3.6%
10th %ile	9.4%	10.9%	8.2%
Median	16.7%	16.1%	12.0%
90th %ile	23.4%	21.7%	16.9%

Table 2.4 Mean and Median Spread and Price Impact Summary

All spreads and price impacts are measured as a percent of the pretrade midpoint. Trades signs are provided by NASDAQ based on payments to liquidity providers. Uses trade subsample where both a pretrade and posttrade midpoint are available. The first letter in each trade category label refers to the liquidity taker and the second refers to the liquidity provider. H signifies that the counterparty is an HFT, N signifies a non-HFT.

				1-minute decomposition	
Category	N	Quoted Spread	Effective Spread	Price Impact	Realized Spread
All	61,272,712	0.036	0.027	0.036	-0.009
HH	11,631,186	0.032	0.023	0.035	-0.012
HN	14,837,559	0.036	0.021	0.034	-0.013
NH	19,581,587	0.033	0.028	0.032	-0.004
NN	15,222,380	0.043	0.035	0.042	-0.007
All	61,272,712	0.026	0.022	0.025	-0.002
HH	11,631,186	0.026	0.023	0.029	-0.016
HN	14,837,559	0.026	0.018	0.024	-0.010
NH	19,581,587	0.026	0.024	0.023	0.000
NN	15,222,380	0.026	0.023	0.024	0.000

Table 2.5 Regression Estimates of Effective Spread and Price Impacts on HFTs Participation Variables and Controls

The regression model is:

$$SPREAD_{itn} = \alpha_{it} + \beta_1 \text{ HFT} + \beta_{2j} \text{ SIZEj} + \beta_3 \text{ BUY} + \beta_4 \text{ SELL} + \varepsilon$$

where i indexes stocks, t indexes day-half hours, n indexes trades, and j indexes trade size groups. The regression is estimated with stock and day-half hour fixed-effects. HFT is an indicator variable that takes a value of 1 if an HFT participated in the trade and 0 otherwise. Different models define HFT based on the liquidity-demanding or liquidity-providing side of the trade. Trade size groups are defined as SMALL (< 500 shares), MEDIUM (>=500 shares, <=1,000 shares), and LARGE (> 1,000 shares). BUY and SELL are dummies indicating the aggressive side of the trade.

Panel A: Effective Spreads				
	Model			
Explanatory Variable	(1)	(2)	(3)	(4)
HFT Participation				
HFT_demand	-0.007	-0.007		
	(<.0001)	(<.0001)		
HFT_supply			0.003	0.003
			(<.0001)	(<.0001)
Trade Size				
Medium		0.000		0.001
		(<.0001)		(<.0001)
Large		0.001		0.003
		(<.0001)		(<.0001)
Trade Sign		0.000		0.000
Buy		(<.0001)		(<.0001)
R^2	27.60%	27.60%	27.41%	27.41%

Table 2.5 Continued

Panel B: 1-minute Price Impacts				
	Model			
Explanatory Variable	(1)	(2)	(3)	(4)
HFT Participation				
HFT_demand	0.001	0.001		
	(<.0001)	(<.0001)		
HFT_supply			-0.001	-0.001
			(<.0001)	(<.0001)
Trade Size				
Medium		0.005		0.004
		(<.0001)		(<.0001)
Large		0.008		0.008
		(<.0001)		(<.0001)
Trade Sign				
Buy		0.003		0.003
		(<.0001)		(<.0001)
R^2	2.44%	2.44%	2.44%	2.44%

Table 2.6 Regression Estimates of Effective Spread and Price Impacts on HFT Participation Variables, Controls, and Interactions

The regression model is:

$$SPREAD_{itn} = \alpha_{it} + \beta_{1j}\ (HFT \times SIZEj) + \beta_2\ (HFT \times BUY) + \beta_3\ (HFT \times SELL) + \varepsilon$$

where i indexes stocks, t indexes day-half hours, n indexes trades, and j indexes trade size groups. The regression is estimated with stock and day-half hour fixed-effects. HFT is an indicator variable that takes a value of 1 if an HFT participated in the trade and 0 otherwise. Different models define HFT based on the liquidity-demanding or liquidity-providing side of the trade. Trade size groups are defined as SMALL (< 500 shares), MEDIUM (>=500 shares, <=1,000 shares), and LARGE (> 1,000 shares). BUY and SELL are dummies indicating the aggressive side of the trade.

Panel A: Effective Spreads

Explanatory Variable	Model (1)	(2)	(3)	(4)
HFT Participation				
HFT_demand	-0.007	-0.007		
	(<.0001)	(<.0001)		
HFT_demand x Medium	0.001			
	(<.0001)			
HFT_demand x Large	0.001			
	(<.0001)			
HFT_demand x Buy		0.000		
		(<.0001)		
HFT_ supply			0.004	0.003
			(<.0001)	(<.0001)
HFT_supply x Medium			-0.002	
			(<.0001)	
HFT_supply x Large			-0.004	
			(<.0001)	
HFT_ supply x Buy				0.000
				(<.0001)
Trade Size				
Medium	0.000	0.000	0.002	0.001
	(<.0001)	(<.0001)	(<.0001)	(<.0001)
Large	0.001	0.001	0.004	0.003
	(<.0001)	(<.0001)	(<.0001)	(<.0001)
Trade Sign				
Buy		-0.001		-0.001
		(<.0001)		(<.0001)
R^2	27.60%	27.60%	27.41%	27.41%

Table 2.6 Continued

Panel B: 1-minute Price Impacts				
	Model			
Explanatory Variable	(1)	(2)	(3)	(4)
HFT Participation				
HFT_demand	0.001	0.001		
	(<.0001)	(<.0001)		
HFT_demand x Medium	0.000			
	(0.119)			
HFT_demand x Large	0.000			
	(0.4015)			
HFT_demand x Buy		-0.001		
		(<.0001)		
HFT_ supply			0.000	-0.002
			(<.0001)	(<.0001)
HFT_supply x Medium			-0.004	
			(<.0001)	
HFT_supply x Large			-0.008	
			(<.0001)	
HFT_ supply x Buy				0.002
				(<.0001)
Trade Size				
Medium	0.005	0.005	0.006	0.004
	(<.0001)	(<.0001)	(<.0001)	(<.0001)
Large	0.008	0.008	0.011	0.008
	(<.0001)	(<.0001)	(<.0001)	(<.0001)
Trade Sign				
Buy		0.003		0.002
		(<.0001)		(<.0001)
R^2	2.44%	2.44%	2.44%	2.44%

Table 2.7 Regressions of 5-minute and 1-minute Returns on Contemporaneous Market Returns, Lagged Order Imbalances, and Lagged Order Imbalance Interacted with a Dummy Variable for HFT Participation Regimes

Returns are from TAQ and are calculated using the last midpoint in each interval. SPY ETF returns are used as the market proxy. $OIB\$_{t-1}$ is the dollar value of buyer-initiated trades less the dollar value of seller-initiated trades divided by the total dollar volume during interval t-1. A stock is defined as having a high-HFT participation day when its participation share is in its highest tercile for that stock over the entire sample. Participation share is defined as HFT dollar volume divided by the stock's total dollar volume. Three versions of participation shares are calculated, differing in whether HFT participation is defined as trades where an HFT participates in any side (All), the liquidity-demanding side (Demand), or the liquidity-supplying side (Supply). Trades where an HFT participates in both sides are used in all three measures. The regressions are estimated separated for each stock, and cross-sectional means of coefficients across all stocks are reported. T-statistics test the null that the mean is 0. Adjusted t-statistics are corrected for cross-correlation in the residuals. The numbers of positive significant and negative significant coefficients in the individual stock regressions are reported, with significance defined as a t-statistic greater than 2 in absolute value. The sample contains 120 stocks. All coefficients are multiplied by 1000.

Panel A: 5-minute returns

HFT Participation Definition	Variable	Coefficient	t-statistic Raw	t-statistic Adjusted	Num pos sig	Num neg sig
All	Intercept	0.0066	3.34	1.38	9	2
	MKT	825.0872	36.52	15.07	120	0
	$OIB\$_{t-1}$	0.0960	6.62	2.73	59	12
	$OIB\$_{t-1}$ x HFT	-0.0704	-6.51	-2.69	2	37
Demand	Intercept	0.0066	3.33	1.37	9	2
	MKT	825.0813	36.52	15.06	120	0
	$OIB\$_{t-1}$	0.1098	7.42	3.06	59	10
	$OIB\$_{t-1}$ x HFT	-0.1175	-9.73	-4.01	4	57
Supply	Intercept	0.0066	3.36	1.39	9	2
	MKT	825.1111	36.52	15.07	120	0
	$OIB\$_{t-1}$	0.0651	4.38	1.81	47	24
	$OIB\$_{t-1}$ x HFT	0.0249	2.52	1.04	17	6

Panel B: 1-minute returns

HFT Participation Definition	Variable	Coefficient	t-statistic Raw	t-statistic Adjusted	Num pos sig	Num neg sig
All	Intercept	0.0004	0.91	0.39	6	2
	MKT	691.5641	31.78	13.60	120	0
	$OIB\$_{t-1}$	0.1047	14.48	6.20	112	1
	$OIB\$_{t-1}$ x HFT	-0.0193	-5.55	-2.37	7	53
Demand	Intercept	0.0004	0.92	0.39	7	2
	MKT	691.5636	31.78	13.60	120	0
	$OIB\$_{t-1}$	0.1066	14.71	6.29	114	1
	$OIB\$_{t-1}$ x HFT	-0.0244	-6.75	-2.89	8	64
Supply	Intercept	0.0004	0.95	0.41	6	2
	MKT	691.5649	31.78	13.60	120	0
	$OIB\$_{t-1}$	0.0992	13.58	5.81	111	3
	$OIB\$_{t-1}$ x HFT	-0.0024	-0.79	-0.34	19	23

Table 2.8 Comparisons of Price Delay Measures across High and Normal HFT Participation Regimes

Price Delay measures use regressions of a stock's return on contemporaneous and lagged market returns (unrestricted regression) compared to regressions on contemporaneous returns only (restricted regression) to measure the speed with which market information is incorporated into the stock's price. D1 is derived from R^2 from the restricted and unrestricted regressions. D2 uses ratios of lagged coefficients to all coefficients and gives more weight to longer lags. D3 is similar to D2 but uses t-statistics instead of coefficients, down weighting less precise estimates. Higher values indicate greater delays. Six lags of market returns are used. Returns are from TAQ and are calculated using the last midpoint in each interval. SPY ETF returns are used as the market proxy. Price Delays are calculated from 5-minute returns in Panel A and 1-minute returns in Panel B. A stock is defined as having a high-HFT participation day when its participation share is in its highest tercile for that stock over the entire sample. Participation share is defined as HFT dollar volume divided by the stock's total dollar volume. Three versions of participation shares are calculated, differing in whether participation is calculated for trades where an HFT participates in any side, the liquidity-demanding side, or the liquidity supplying side. Trades where an HFT participates in both sides are used in all three measures. Price Delay differences are calculated separately for each stock, and cross-sectional means across all stocks are reported. T-statistics test the null that the mean is 0. Adjusted t-statistics are corrected for cross-correlation in the residuals. The sample contains 120 stocks.

HFT Participation Definition	PD measure	High	Normal	Diff	t-statistic Raw	t-statistic Adjusted
Panel A: 5-minute returns						
All	D1	0.030	0.038	-0.008	-3.04	-1.22
	D2	0.262	0.375	-0.113	-4.57	-1.83
	D3	0.264	0.381	-0.117	-4.46	-1.79
Demand	D1	0.030	0.040	-0.010	-3.89	-1.56
	D2	0.250	0.382	-0.131	-4.69	-1.88
	D3	0.251	0.386	-0.135	-4.54	-1.82
Supply	D1	0.037	0.033	0.003	1.49	0.60
	D2	0.301	0.349	-0.047	-2.12	-0.85
	D3	0.302	0.354	-0.051	-2.14	-0.86
Panel B: 1-minute returns						
All	D1	0.068	0.084	-0.016	-4.31	-1.83
	D2	0.312	0.445	-0.133	-5.14	-2.18
	D3	0.311	0.448	-0.136	-4.71	-2.00
Demand	D1	0.070	0.086	-0.016	-2.70	-1.15
	D2	0.283	0.468	-0.186	-6.29	-2.67
	D3	0.278	0.473	-0.195	-6.01	-2.55
Supply	D1	0.077	0.077	0.000	-0.10	-0.04
	D2	0.403	0.389	0.014	0.65	0.28
	D3	0.407	0.389	0.018	0.72	0.31

Table 2.9 HFT VWAP Difference Summary Statistics

Differences between VWAP on HFT trades of various categories and same-stock, same-day market VWAP, and differences between VWAP on HFT sells and buys. VWAP differences are scaled by market VWAP and reported as percentages. VWAP differences are signed so that positive numbers indicate that HFTs are outperforming the market benchmark. VWAP differences are first calculated separately for each stock-day and then the stock-day values are summarized with different weightings. Panel A reports summary statistics weighted equally over all stock-days, Panel B reports summary statistics equally weighted by day, and Panel C reports summary statistics equally weighted by stock. Trade data provided by NASDAQ. Trade sample period is January 2008 – December 2009 and February 22, 2010 – February 26, 2010. Trades are missing on October 10, 2008. Only trades between 9:30 am and 4:00 pm are used.

Panel A: Stock-day weighting									
HFT Trade Type	All			Demand			Supply		
VWAP Difference	mkt - buy	sell - mkt	sell - buy	mkt - buy	sell - mkt	sell – buy	mkt - buy	sell - mkt	sell - buy
N	60,692	60,716	60,585	60,260	60,360	59,966	60,084	60,108	59,524
Mean	0.033	0.032	0.065	0.011	0.010	0.023	0.064	0.065	0.128
Std Dev	0.484	0.431	0.560	0.539	0.516	0.659	0.639	0.611	0.813
T	16.70	18.17	28.48	5.12	4.71	8.57	24.50	25.96	38.30
10th %ile	-0.177	-0.179	-0.244	-0.267	-0.268	-0.356	-0.298	-0.292	-0.317
Median	0.011	0.011	0.023	0.002	0.001	0.004	0.023	0.025	0.050
90th %ile	0.269	0.264	0.424	0.305	0.301	0.434	0.469	0.467	0.668
Panel B: Day weighting									
N	509	509	509	509	509	509	509	509	509
Mean	0.033	0.032	0.065	0.011	0.010	0.023	0.064	0.065	0.128
Std Dev	0.056	0.050	0.066	0.071	0.063	0.075	0.100	0.115	0.111
T	13.24	14.38	22.27	3.56	3.55	6.93	14.45	12.71	25.94
10th %ile	-0.017	-0.019	-0.004	-0.053	-0.056	-0.059	-0.018	-0.024	0.027
Median	0.028	0.027	0.058	0.008	0.005	0.020	0.049	0.055	0.108
90th %ile	0.087	0.090	0.147	0.077	0.078	0.109	0.159	0.157	0.257
Panel C: Stock weighting									
N	120	120	120	120	120	120	120	120	120
Mean	0.034	0.032	0.066	0.011	0.009	0.021	0.066	0.067	0.135
Std Dev	0.046	0.033	0.069	0.041	0.047	0.070	0.077	0.062	0.137
T	8.06	10.47	10.50	3.02	1.98	3.35	9.45	11.76	10.80
10th %ile	0.000	0.001	0.003	-0.024	-0.024	-0.031	0.007	0.010	0.019
Median	0.023	0.020	0.052	0.006	0.006	0.013	0.051	0.056	0.111
90th %ile	0.077	0.078	0.139	0.055	0.058	0.095	0.138	0.131	0.258

Table 2.10 HFT VWAP Difference Decomposition

Decomposition of the differences between VWAP on HFT same-stock, same-day sell and buy trades of various categories into a positioning performance component and a short-term timing component. VWAP differences and components are scaled by market VWAP and reported as percentages. VWAP differences and components are signed so that positive numbers indicate positive HFT performance. The positioning performance is calculated by first replacing the price on each HFT trade with the market VWAP for the same 5-minute interval, computing VWAPS for HFT sells and buys using the modified prices (positioning VWAPs) for every stock-day, and then taking the difference between the sell and buy positioning VWAPs. Timing performance is the HFT sell-buy VWAP difference calculated using actual trade prices less the positioning performance. Each observation is a stock-day. Trade data is provided by NASDAQ. Trade sample period is January 2008 – December 2009 and February 22, 2010 – February 26, 2010. Trades are missing on October 10, 2008. Only trades between 9:30 am and 4:00 pm are used.

Panel A: HFT Sell-Buy VWAP Difference			
HFT Trade Type	All	Demand	Supply
N	60,585	59,966	59,524
Mean	0.065	0.023	0.128
Std Dev	0.560	0.659	0.813
T	28.48	8.57	38.30
10th %ile	-0.244	-0.356	-0.317
Median	0.023	0.004	0.050
90th %ile	0.424	0.434	0.668
Panel B: HFT Positioning Performance			
Mean	0.050	0.028	0.081
Std Dev	0.558	0.638	0.783
T	21.91	10.77	25.39
10th %ile	-0.251	-0.338	-0.358
Median	0.016	0.006	0.026
90th %ile	0.396	0.431	0.587
Panel C: HFT Short-term Timing Performance			
Mean	0.015	-0.005	0.046
Std Dev	0.207	0.146	0.235
T	17.96	-8.4	47.98
10th %ile	-0.031	-0.059	-0.027
Median	0.007	-0.002	0.020
90th %ile	0.062	0.047	0.136

Table 2.11 HFT Activity in 5-minute Intervals Ranked by VWAP

Mean levels of HFT activity measures in groups of 5-minute intervals ranked by VWAP, and comparisons across high and low VWAP groups. For each stock-day, the market VWAP is calculated for every 5-minute interval, and then each interval is ranked into thirteen groups by VWAP. Each group contains six 5-minute intervals. Group 1 contains the intervals with the lowest prices of the day, and Group 13 contains the intervals with the highest prices of the day. HFT activity measures are HFT Net Dollar Volume Ratio and HFT Order Imbalance. HFT Net Dollar Volume Ratio is the net HFT dollar volume traded (buy dollar volume less sell dollar volume) divided by the gross HFT dollar volume (buy dollar volume plus sell dollar volume), and indicates how much of the HFT trading in that interval was buying vs. selling. HFT Order Imbalance is calculated with the same formula using only trades where HFTs demanded liquidity. HFT activity measures are calculated within each interval and then averaged across intervals within a group for each stock-day, and summarized over stock-days. Differences in activity measures between Group 13 and Group 1 are reported. Bold values indicate significance at the 5% level based on a t-test. Trade data are provided by NASDAQ. Trade sample period is January 2008 – December 2009 and February 22, 2010 – February 26, 2010. Trades are missing on October 10, 2008. Only trades between 9:30 am and 4:00 pm are used.

VWAP Rank	HFT Net Dollar Volume Ratio	HFT Order Imbalance
1	**0.033**	**0.017**
2	**0.015**	**0.010**
3	**0.011**	**0.011**
4	**0.006**	**0.008**
5	**0.003**	**0.008**
6	0.001	**0.006**
7	**-0.005**	0.002
8	**-0.007**	0.000
9	**-0.009**	0.000
10	**-0.012**	-0.003
11	**-0.016**	**-0.004**
12	**-0.024**	**-0.011**
13	**-0.045**	**-0.022**
13-1	**-0.074**	**-0.042**
t(13-1)	-31.00	-16.46

Table 2.12 Decile Portfolios and Decile Spread Portfolios Formed on HFT Positions and Position Changes

Portfolio returns are calculated from TAQ midpoints. t-statistics test the null that the spread portfolio return is equal to 0. Holding periods are 5-minutes in Panels A and C and 1-minute in Panel B and D. In Panels A and C, positions are based on cumulative observed trades ending just prior to start of the holding period. In Panels B and D, portfolios are formed on lagged position changes based on prior intervals of the same length as the holding period. Mean positions and position changes across stocks in each portfolio are reported. Trade data used to calculate HFT positions are provided by NASDAQ. Sample period is January 2008 – December 2009 and February 22, 2010 – February 26, 2010. Trades are missing on October 10, 2008. Only intervals between 9:30 am and 4:00 pm are used. The first interval is dropped when positions are used and the first two intervals are dropped when position changes are used.

Panel A. HFT position portfolios, 5-minute Intervals.

Portfolio	1	2	3	4	5	6	7	8	9	10	spread 10-1	t(spread)
Mean P_{t-1} ($000)	-4,577	-624	-135	-38	-9	6	35	134	632	4,427		
return (bps)	0.07	0.29	0.39	0.52	0.25	-0.11	-0.32	-0.15	-0.07	0.01	-0.06	-1.14

Panel B. HFT position portfolios, 1-minute Intervals.

Portfolio	1	2	3	4	5	6	7	8	9	10	spread 10-1	t(spread)
Mean P_{t-1} ($000)	-4,570	-621	-134	-38	-8	7	35	134	632	4,421		
return (bps)	0.04	0.08	0.12	0.12	0.06	-0.06	-0.11	-0.07	-0.06	-0.01	-0.05	-4.21

Panel C. HFT position change portfolios, 5-minute Intervals.

Portfolio	1	2	3	4	5	6	7	8	9	10	spread 10-1	t(spread)
Mean ΔP_{t-1} ($000)	-348	-44	-10	-3	0	0	3	10	44	353		
return (bps)	0.42	0.71	1.04	0.96	0.30	-0.31	-0.71	-0.80	-0.49	-0.28	-0.70	-13.96

Panel D. HFT position change portfolios, 1-minute Intervals.

Portfolio	1	2	3	4	5	6	7	8	9	10	spread 10-1	t(spread)
Mean ΔP_{t-1} ($000)	-130	-15	-3	-1	0	0	1	3	15	130		
return (bps)	0.29	0.36	0.31	0.01	0.17	-0.16	-0.02	-0.25	-0.31	-0.26	-0.55	-48.88

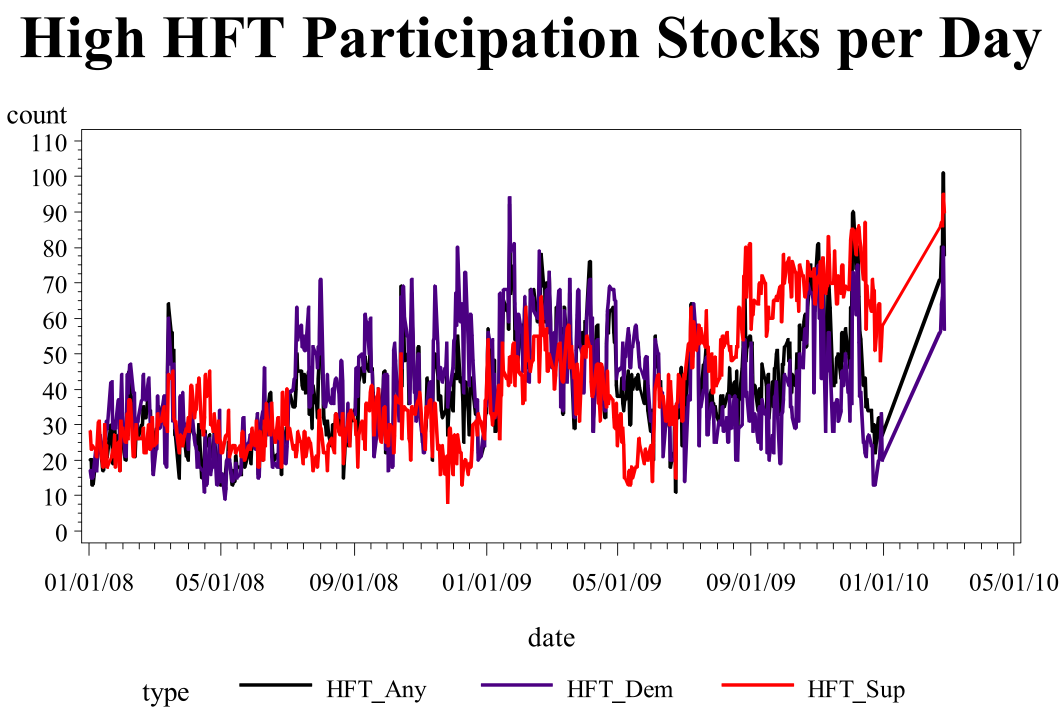
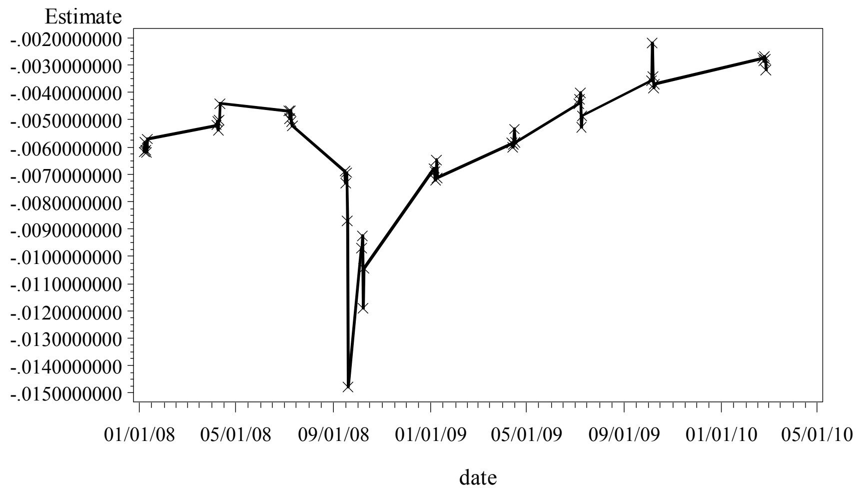


Figure 2.1 Time Variation in Count of High HFT Participation Stocks

The graph shows how many stocks per day have high HFT participation. A stock is defined as having a high-HFT participation day when its participation share is in its highest tercile for that stock over the entire sample. Participation share is defined as HFT dollar volume divided by the total dollar volume stock. Three versions of participation shares are calculated, differing in whether participation is calculated for trades where an HFT participates in any side, the liquidity-demanding side, or the liquidity supplying side. Trades where an HFT participates in both sides are used in all three measures.

Panel A.

HFT_Demand vs Time



Panel B.

HFT_Demand vs Stock Ranking

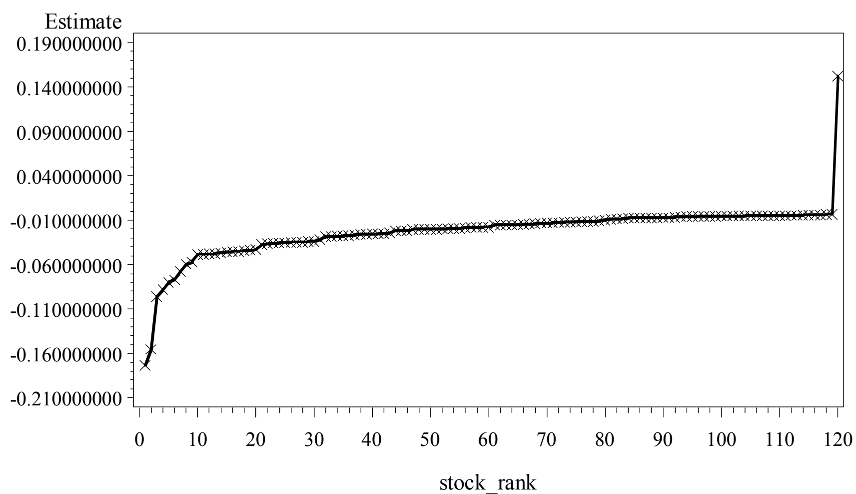


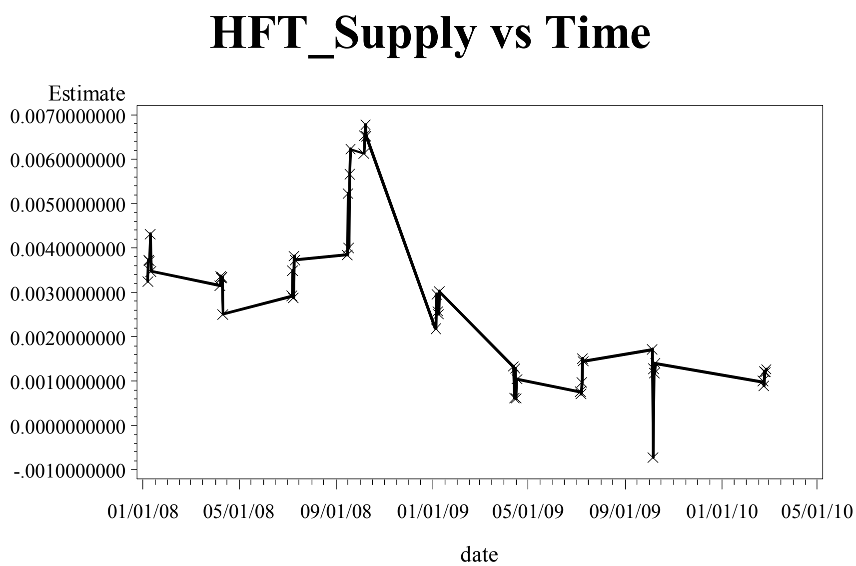
Figure 2.2 Time and Cross-sectional Variation in Impact of HFT Participation on Effective Spreads

The graphs show coefficients from effective spreads regressed on HFT participation indicator variables and controls estimated one day at a time or one stock at a time. The regression model is:

$$\text{Effective Spread}_{itn} = \alpha_{it} + \beta\text{*HFT} + \text{controls}$$

where i indexes stocks, t indexes day-half hours, n indexes trades, and HFT is alternately set to HFT_Demand or HFT_Supply. When regression is estimated one stock at a time, stocks are sorted in order of estimated loading on HFT.

Panel C.



Panel D.

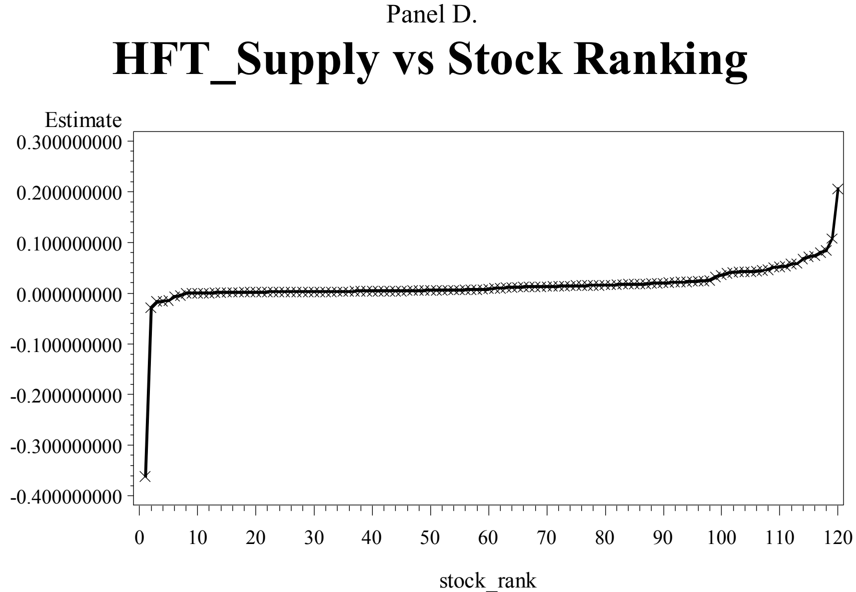
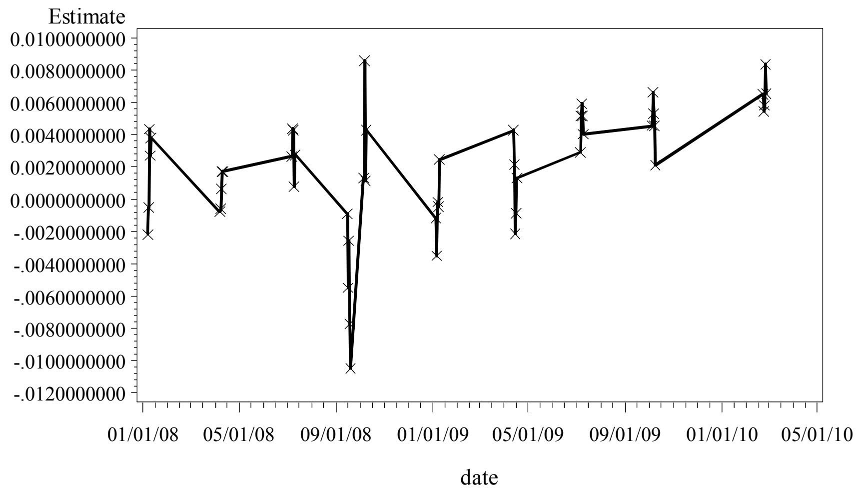


Figure 2.2 Continued

Panel A.

HFT_Demand vs Time



Panel B.

HFT_Demand vs Stock Ranking

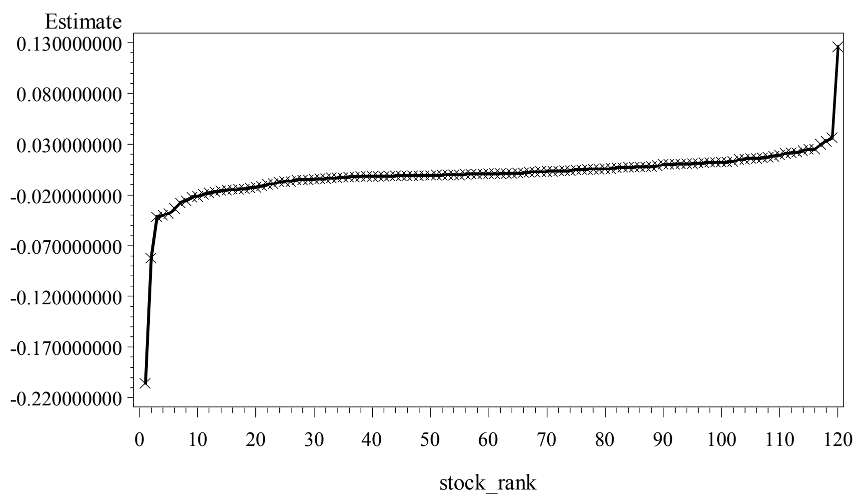


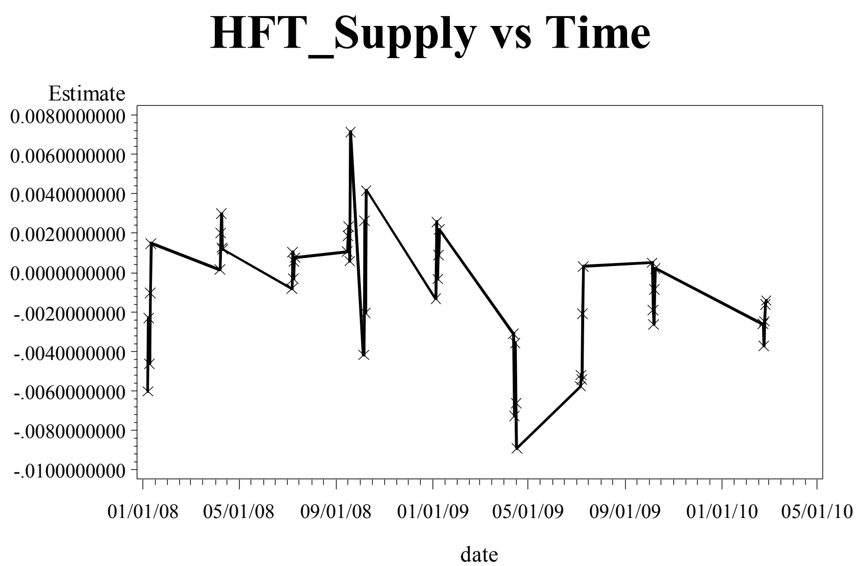
Figure 2.3 Time and Cross-sectional Variation in Impact of HFT Participation on 1-minute Price Impacts

The graphs show coefficients from 1-minute price impacts regressed on HFT participation indicator variables and controls estimated one day at a time or one stock at a time. The regression model is:

$$\text{Price Impact}_{itn} = \alpha_{it} + \beta^{*}\text{HFT} + \text{controls}$$

where i indexes stocks, t indexes day-half hours, n indexes trades, and HFT is alternately set to HFT_Demand or HFT_Supply. When regression is estimated one stock at a time, stocks are sorted in order of estimated loading on HFT.

Panel C.



Panel D.

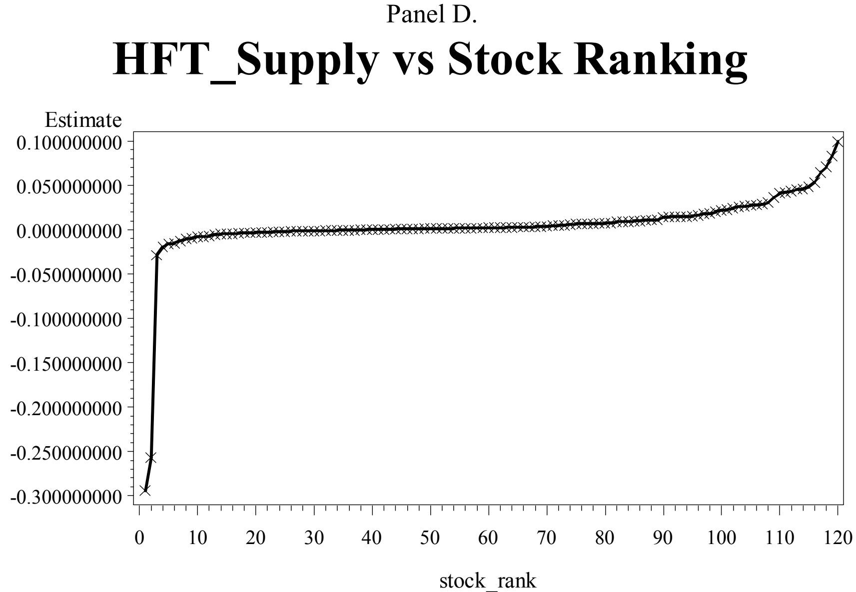
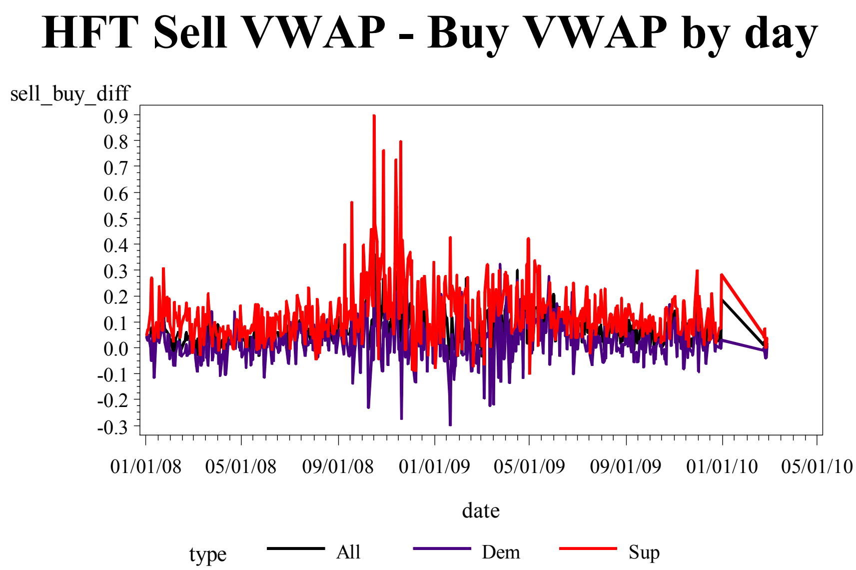


Figure 2.3 Continued

Panel A.



Panel B.

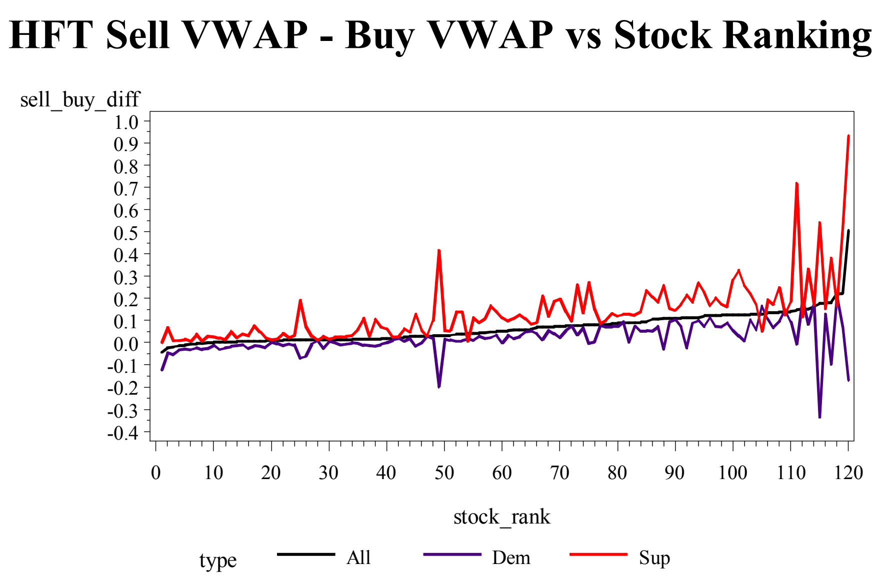


Figure 2.4 Time and Cross-sectional Variation in HFT VWAP Performance Measures

The graphs show averages of HFT sell VWAP – buy VWAP differences measured as a percent of market-wide VWAP for each stock. Differences are calculated for each sample stock–day, and summarized by time or stock. Three measures are computed, the first using all HFT trades (All), the second using liquidity-demanding HFT trades only (Demand), and the third using liquidity-supplying HFT trades only (Supply). Panel A shows time series variation in the daily equal-weighted averages across stocks. Panel B shows cross-sectional variation in the time series averages for each stock over the full sample. Stocks in Panel B are ranked by performance in the "ALL" group.

3. HIGH-FREQUENCY TRADING IN EXTREME MARKET CONDITIONS

3.1 Abstract

Does High-frequency Trading (HFT) stabilize or destabilize markets under stress? I address this question by studying HFT activity around extreme returns and order flow imbalances in a proprietary sample of NASDAQ trades that identifies HFT participation. First, I study HFT trading around extreme negative returns over short horizons, which are commonly referred to as "mini-flash crashes." I define mini-flash crashes as returns of -5% or less in 5 minutes or less. My findings indicate that, on average, HFTs buy during mini-flash crashes and sell in the minutes afterwards, and they provide more liquidity than they consume during the crashes. However, their trading volumes are small relative to the market. Results are qualitatively similar for price spikes, characterized by returns of 5% or greater in 5 minutes or less. Second, motivated by theory in Jarrow and Protter (2011) and the SEC's stated concerns regarding "momentum ignition," I examine returns around large HFT order imbalances. I find only economically small momentum after imbalances, which is eventually followed by reversals for sell imbalances. Finally, I examine two aspects of HFT interaction with the order flow of others. I study how resiliency, the speed at which the order book replenishes after an order imbalance, varies with HFT participation. I also test HFT activity around sustained market order flow imbalances, called "toxic order flow." Using a measure based on

Easley, Lopez de Prado, and O'Hara (2011a) (ELO (2011a) hereafter), I find that HFT participation levels decrease as toxic order flow increases, but I find no evidence supporting concerns that HFTs increase their liquidity demand at very high levels. Overall, my results suggest that HFTs play a neutral or stabilizing role around extreme events.

3.2 Introduction

The possibility that high-frequency trading (HFT) can destabilize markets has been raised by academics, practitioners, regulators, and the press. The hypothesized mechanisms vary, but can be classified into three categories. HFTs could directly cause a destabilizing shock through their active trading, HFTs could alter their trading behavior after an exogenous shock in a way that amplifies or prolongs the shock, or a decrease in HFT participation by could cause liquidity to disappear to the point where normal order imbalances would result in extreme price moves. All of these mechanisms could result in crashes or spikes in prices. Many observers have blamed HFT for recent examples of these events. A few examples of these assertions follow. An article from CBS's Money Watch claims:

> A dip in the market can trigger millions of automatic sales, which like dominoes, trigger millions more in just seconds. Before humans even realize what's happening, a panic has begun.[23]

Kirilenko, Kyle, Samadi, and Tuzun (2011) (KKST (2011) hereafter) study trading in the S&P 500 futures market around the May 6th, 2010 Flash Crash, and conclude that HFTs

[23] "Stock Market's Robo-Panic Is Sign of Things to Come," by Ben Popper, CBSNEWS.com, May 7, 2010.

did not cause the crash but did exacerbate it.[24] Sornette and von der Becke (2011) state: "Can high frequency trading lead to crashes? We believe it has in the past, and it can be expected to do so more and more in the future" (p. 3). These assertions seem plausible considering the large volumes traded by HFT, their speed, and the lack of human intervention in their trading process. It is not hard to imagine that HFT herding, technical malfunctions, or even manipulative strategies could destabilize markets. Given the economic importance of market stability and the consequences of a potential loss of investor confidence in the markets, these claims should be taken seriously and warrant further investigation.

While many of the assertions regarding the destabilizing effects of HFT seem plausible, it is an empirical question whether HFTs behave as hypothesized and, if so, whether the impacts are significant. Much of the blame placed on HFT for these events is based on conjecture, theory, or in the case of the Flash Crash, a single event. There are also reasons to believe that HFTs may be play a neutral or even a stabilizing role. As some observers have noted, extreme price moves occurred long before the existence of HFT. A Wall Street Journal article notes: "On May 28, there was a 'flash crash.' If you didn't notice it, that is because it occurred not in 2010, but in 1962."[25] This is also acknowledged by Sornette and von der Becke (2011). Gregg Berman of the SEC attributes many of these events to expected news or "fat finger" trading errors.[26]

[24] The Flash Crash is the popular name for an event that occurred on May 6, 2010, where within a half hour, the major U.S. equity indexes dropped more than 5% and quickly reversed most of the losses. Volatility in some ETFs and individual stocks was even greater. See KKST (2011).
[25] "Back to the Future: Lessons from the Forgotten 'Flash Crash' of 1962," by Jason Zweig, The Wall Street Journal, May 29, 2010, p. B7.
[26] These comments are from a speech titled "Market Structure: What We Know, and What We Need to Know," transcript available on the SEC's website at http://www.sec.gov/news/speech/2011/

Deutsche Börse AG found that a mini-flash crash in index futures was actually caused by "a barrage of institutional selling" and not HFT, as initially suspected.[27] There are also potential HFT trading strategies that would prevent extreme price moves or hasten the recovery after they occur. For example, Brogaard (2012b) and KKST (2011) find evidence of contrarian trading by HFTs in some circumstances.

The trading behavior of HFT in extreme market conditions has received little attention in the empirical literature. In addition to KKST (2011), Brogaard (2012a) and Hirschey (2011) are the only studies I am aware of that touch on these issues using data that identify HFT participation. Brogaard (2012a) finds that HFTs increase their trading when short-term volatility is at extreme levels, but reduce their trading when longer term intraday volatility is high, and the effects are stronger for liquidity-demanding trades. Hirschey (2011) finds that large 1-second periods of aggressive HFT trading precede market-wide order imbalances and returns in the same direction, and concludes that this is consistent with anticipatory trading strategies. However, the definition of aggressive order imbalance used in that paper is the extreme deciles from a cross-sectional sort, which means roughly 20% of the sample is classified as aggressive. In contrast, I am interested in much more extreme events, which are of greater importance in the context of the market stability issues that are my focus. Other relevant studies that do not employ HFT data include Easley, Lopez de Prado, and O'Hara (2011c) (ELO (2011c) hereafter), ELO (2011a), and Golub and Keane (2011). ELO (2011c) suggest that HFTs decreased liquidity provision during the Flash Crash as a result of perceived asymmetric

spch092111geb.pdf. The specific comments are based on a review of events that tripped circuit breakers or led to consideration of trade cancellations

[27] "High-Speed Traders Off Hook in Germany," by Jacob Bunge, The Wall Street Journal, October 19, 2011.

information signaled by extreme order flow imbalances, which they refer to as toxic order flow. They contend that when toxic order flow reached extreme levels, HFTs became liquidity consumers as they liquidated inventory. ELO (2011a) develops the VPIN (Volume-synchronized Probability of Informed Trading) measure of order flow toxicity, which aims to measure the risk of liquidity-induced volatility large price movements.[28] Golub and Keane (2011) documents stylized facts regarding mini-flash crashes, and conjectures that HFT may play a role. Several other studies examine the behavior of other types of market participants in extreme market conditions. Gennotte and Leland (1990) model the interactions of various types of market participants around a crash and show their model explains some characteristics of the Crash of October 1987. Dennis and Strickland (2002) employ institutional ownership data to study the trading of institutional investors in the equity market on extreme return days, and find evidence suggesting that institutions herd, trade with the market momentum, and contribute to volatility on these days. Lipson and Puckett (2010) address the same questions using trade level data, and find that institutional investors actually trade in the opposite direction of large market moves and provide a stabilizing influence.

I study these issues by examining HFT activity and returns around extreme return and order flow events in a proprietary sample of NASDAQ trades that identifies HFT participation. First, I identify a set of mini-flash crashes and price spikes. My crash/spike selection criteria is a move of +/- 5% or more with a 5-minute period. This is motivated by the SEC's proposed "Limit Up-Limit-Down" rule, which would prevent trades from occurring outside price bands of a similar magnitude. This rule was not in

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[28] The authors of ELO (2011a) have applied for a patent on the VPIN algorithm.

place during my sample period, and therefore does not influence price formation or the behavior of market participants in this analysis. I find that, on average, HFTs buy during mini-flash crashes, and they provide more liquidity than they consume during the crashes. This trading behavior should provide a stabilizing effect and reduce the severity of the crashes. However, their trading volumes are small relative to the market. Results are qualitatively similar for price spikes. Second, motivated by theory in Jarrow and Protter (2011) and the SEC's stated concerns regarding "momentum ignition," I examine returns around large HFT order imbalances. While the prior analysis examines HFT trading around extreme return events, here the focus is on returns around extreme levels of HFT trading. It is possible that these events result in price distortions that are economically important but do not enter the extreme return sample discussed above. I find only economically weak momentum after imbalances, which is eventually followed by reversals for sell imbalances. The sell imbalance price patterns are weakly consistent with Jarrow and Protter (2011) and the SEC's theory of momentum ignition, but are also consistent with the price pattern that would result from a large liquidity demand, as in Kraus and Stoll (1972). Finally, I examine two dimensions of the relationship between HFT and the order flow imbalances of others. I test whether resiliency, the speed at which the order book replenishes after an order imbalance, varies with HFT participation. Resiliency is relevant to price stability because when resiliency is low, a stock will potentially experience large price swings after sustained order flow imbalances. I find the measure of resiliency I use (based on Degryse et al. 2005) does not perform well in this sample and I can draw no conclusions from this analysis. Finally, I examine HFT activity around sustained order flow imbalances, termed "toxic order flow" by ELO

(2011a). Using VPIN, a measure based on ELO (2011a), I find HFT participation levels decrease as toxic order flow increases, confirming one of the predictions in ELO (2011c). However, I find no evidence supporting their other prediction that HFTs increase their liquidity demand at very high levels of flow toxicity as they urgently reduce inventory to mitigate risk. Overall, my results suggest HFTs play a neutral or stabilizing role around extreme events.

The rest of this paper is organized as follows. Section 3.3 describes the data. Section 3.4 examines HFT trading behavior around mini-flash crashes and up crashes. Section 3.5 studies whether large HFT order imbalances destabilize prices. Section 3.6 examines resiliency and whether it varies with HFT participation. Section 3.7 examines how HFT participation varies with order flow toxicity. Section 3.8 concludes.

3.3 Data

3.3.1 Overview

The primary data source employed is a proprietary dataset provided by NASDAQ consisting of trades, quotes, and limit order book snapshots for a sample of 120 stocks. The stock sample was chosen by Terrence Hendershott and Ryan Riordan. See Table 2.1 for a list of sample stocks. It is stratified by market capitalization and is evenly split by NASDAQ and NYSE listing. The sample period covers all of 2008 and 2009 and one week in 2010.[29] The trade sample consists of all trades executed on the exchange in continuous trading, excluding crosses and NASDAQ TRF-reported trades. Trades are time stamped to the millisecond and signed to indicate whether they were initiated by a

[29] There is one day, October 10, 2008, missing from the dataset which may become available in the future.

buyer or seller. The trade signs are high quality, and are based on records of rebate payments.[30] NASDAQ Inside Quotes (BBOs) and 1-minute limit order snapshots are provided for subsamples of the data. These subsamples cover the first full trading week in each quarter, the week of Oct 6-10, 2008 (the week of the Lehman collapse), and the week of Feb 22-26, 2010. The BBO data are time stamped to the millisecond and do not have the problems with timestamp discrepancies that are present in alternate sources. The limit order snapshots have 10 levels of bids and 10 levels of asks, and show hidden orders. The only filter applied to the full trade sample was the removal of trades before 9:30 am and after 4:00 pm. A subsample used for trading cost analysis also required a usable quote before and after each trade. The limit order book was filtered for snapshots with locked or crossed markets, negative sizes, or no size at the top of the book when depth was present below. For some analyses, additional filters were applied, and specifics are provided in the relevant sections.

A unique feature of this dataset is that high-frequency trader participation is identified in the data. NASDAQ has manually identified 26 high-frequency trading firms and flagged their activity. Specifically, trades contain a field with the following codes: HH, HN, NH, or NN. H identifies a high-frequency trader and N identifies a non-HFT. The first term in a pair classifies the liquidity taker, and the second term classifies the liquidity provider. For example, a trade marked HN would mean a high-frequency trader took liquidity from a non-HFT on that trade. Similarly, HFT quotes are flagged in the LOB snapshots and a subsample of quotes.

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[30] Rebate payments are payments made to the liquidity supplier in a maker-taker market. These are partial rebates of the fees collected by the exchange from the trade initiator.

The identities of the HFT firms are not provided. The selection process was manual and apparently somewhat subjective. The principles are described in Brogaard (2012a) as follows:

> The characteristics of firms identified as being HFTs are the following: They engage in proprietary trading… They use sponsored access providers whereby they have access to the co-location services and can obtain large-volume discounts and reduce latency. They tend to switch between long and short net positions several times throughout the day…. Orders by HFT firms are of a shorter time duration than those placed by non-HFT firms. Also, HFT firms normally have a lower ratio of trades per orders placed than non-HFT firms. (p. 7)

Brogaard (2012a) and Hasbrouck and Saar (2011) note that the selection process excludes certain types of firms that engage in HFT, such as firms whose primary business is not HFT but sometimes engage in HFT or HFT firms that route trades through a non-HFT firm. This concern is valid but is somewhat mitigated by the large percentage of trading volume that the sample firms participate in, which is described in further detail in Section 2.4.

I also obtain supplemental data from CRSP and TAQ. I use CRSP data for the sample stock descriptive statistics only. I use TAQ CT trade data to assess NASDAQ's volume shares in sample stocks.

3.3.2 Descriptive Statistics

Table 2.2 presents trade summary statistics. The second column reports values for the full sample. The full sample covers 509 days and contains 550,118,372 trades for approximately 106 billion shares and a total dollar volume of $3.9 trillion. The daily average share volume in the sample is 208 million shares and the dollar volume is $7.7 billion. There is substantial variation in the daily trading activity. On the 10^{th} percentile day, there is \$4.4 billion traded while on the 90th percentile day, \$11.9 billion is traded. The trade size is of particular interest because there is a common perception that trade sizes are much smaller than in the past. They are in fact small in this sample: the average size is 192.3 shares, the median is 100 shares, and the 90th percentile is 400 shares. The third column reports values for the subsample where matching NASDAQ pretrade and posttrade quotes are available. This subsample contains 61,272,712 trades for 11.6 billion shares and \$444 billion dollars. By comparing the two columns, we can informally assess whether the quote subsample is reasonably representative. The days with quotes have somewhat more trading activity, but in general appear similar. The subsample covers roughly 10% of the trading days in the full sample, and the aggregate trades, share volume, and dollar volume are around 11% of the full sample values. The daily mean share volume and dollar volume in the subsample are 14% and 18% higher than the full sample means. The trade size distributions are very close.

Table 3.1 reports order book summary statistics. The sample includes 2,311,201 limit order book snapshots after applying the screens described above. These cover the best ten bids and asks and are provided at 1-minute intervals. The mean quoted half spread is \$.031 (12 bps). As expected, these are positively skewed, with a median of \$0.01 (6 bps). The mean best bid depth is 2,162.9 shares and the mean best ask depth is 2,205.6 shares. The mean total bid depth is 22,436.9 shares and the mean total ask depth is 22,245.1 shares. Depths are also highly skewed. The medians are 300 shares, 300 shares, 6,020 shares, and 6,180 shares for the best bid, best ask, total bid, and total ask depths, respectively.

3.4 Mini-flash Crashes and Price Spikes

As discussed above, HFTs are often suspected of playing a role in extreme return events. Since the May 6, 2010 Flash Crash, the press has frequently called large negative return events mini-flash crashes. I adopt this term and also refer to large positive return events as price spikes.[31] The assertions about the role of HFTs in these events are largely speculative. To my knowledge, the only studies addressing these concerns using data that identify HFT participation are the CFTC-SEC May 6 Report and KKST (2011), and they only study a single event (the Flash Crash). In this section, I create a sample of mini-flash crashes and price spikes and study the behavior of HFTs in periods before, during, and after these events.

3.4.1 Methodology

There is no formal definition of a mini-flash crash in the literature and there does not seem to be a consensus in the press. I elect to use a return of +/- 5% or greater within 5 minutes or less as my definition. This choice is motivated by the "Limit Up-Limit Down" rule proposed by the SEC on April 5, 2011.[32] This rule was not implemented during my sample period, so it does not affect the trading behavior of the HFTs in this analysis. I argue that the use of thresholds of a magnitude similar to those proposed by the SEC ensures that the events selected are extreme enough to be of interest.

[31] This term implies that a reversal follows the price increase. While this is true on average in my sample, I do not screen for it.
[32] See http://www.sec.gov/news/press/2011/2011-84.htm for details on the SEC's Limit Up-Limit Down proposal. My procedure is motivated by the proposed rule but does not attempt to reproduce all its features.

More specifically, for all stocks and every 5-minute interval in the sample, I employ the high and low trade prices to calculate the most extreme return within the interval. If the low came first, the most extreme return is positive and vice-versa. Intervals with returns of -5% or less enter the crash sample and returns of 5% or greater enter the price spike sample. I record the times of the price extremes and use the time of first price extreme (the high for crashes and the low for price spikes) as the start of the event and the time of the second extreme as the end of the event. I define the 5 minutes prior to the start of the event as the before period, and the 5 minutes after the event as the after period, but only use the portion of the before and after periods that falls within regular trading hours (RTH). I exclude events that start before 9:37 or end after 3:58, to avoid possible effects from the open or close and to assure that periods before and after the crash are available for analysis. I also keep only the first event in a stock on each day.

For each event, I collect all RTH trades from 5 minutes before the start of the event through 5 minutes after the end of the event. Trade records contain the price, size, a sign indicating whether the trade was buyer-initiated or seller-initiated, a field indicating whether an HFT is on the liquidity-demanding side of the trade, the liquidity supplying side, or both, and a millisecond timestamp. For each event, the various categories of trades are aggregated over the periods before, during, and after the event to produce metrics that capture the dimensions of HFT behavior that are of interest. Descriptions of the measures that may not be immediately obvious follow. Market Order Imbalance is the ratio of net trading dollar volume (Buyer Initiated Dollar Volume – Seller Initiated Dollar Volume) to total dollar volume (Buyer Initiated Dollar Volume +

Seller Initiated Dollar Volume. This variable is essentially the same as OIB introduced in Section 2.6 but measured in event time, and can be interpreted as the market-wide buying pressure from liquidity-demanding trades. It is renamed to distinguish it from other similar variables used in this section. HFT Order Imbalance is the same ratio calculated using only the trades where HFTs demanded liquidity, and can be interpreted as the buying pressure from HFT liquidity-demanding trades. HFT Net Dollar Volume is the dollar volume of HFT buys less the dollar volume of HFT sells. HFT Net Dollar Volume Ratio scales the HFT Net Dollar Volume by the market dollar volume. This measure ranges from 1.0, which would indicate that HFTs were the buyers in every trade, to -1.0, which would indicate that HFTs were the sellers in every trade. HFT Demand Ratio is the fraction of total HFT dollar volume where HFTs demanded liquidity.

3.4.2 Results

The results of this analysis are presented in Table 3.2. Crash results are shown in Panel A, and price spike results are shown in Panel B. Using the criteria described in the previous section, I identify 315 crash events in 64 stocks, with a mean return of -6.65%, and 402 price spike events in 74 stocks with a mean return of 7.19%. Given that the universe consists of 120 stocks and the sample period covers 509 days, it appears these events are not uncommon. Also, considering the attention received by crashes, it is somewhat surprising that price spikes are more common and more extreme. Both types of events are preceded by small and statistically insignificant returns in the opposite direction, which indicates that the start times are reasonable. More importantly, both types of events are followed by partial and statistically significant reversals over the next

5 minutes. The reversal pattern suggests that these extreme events are not simply an efficient reaction to information about fundamentals in a liquid market. The fact that the reversals are incomplete suggests that these events are related to information, but at this point I cannot rule out the possibility of further reversal after the postevent window. Also, perhaps not surprisingly, mean contemporaneous market order imbalances are large and strongly significant in the direction of the event (-0.565 for crashes and 0.493 for price spikes), and are not significantly different from zero in the before and after periods.

The main goal of this analysis is to examine HFT behavior and participation in the periods before, during, and after these events. The before period measurements are shown in column 2. First, we see that HFTs often do not trade in the period prior to the crashes. Only 204 (250) of the 315 crashes (402 spikes) have any HFT trading in the prior period. The absence of HFT activity during some of these periods does not seem unusual, although it may occur more frequently than usual. On nonevent stock days, there is no HFT trading in a mean of 14.2 5-minute intervals, out of the 78 intervals in the trading day. Also, an untabulated regression with stock fixed effects finds that there are 2.5 less nontrading intervals on event days than on nonevent days. Of the events with HFT pre-event trading, HFT net trading accounts for an average of only 1% (2%) of the market's trading dollar volume for crashes (spikes), neither of which is significantly different from zero. This is a reflection of balanced trading rather than a lack of participation, however. HFTs participate in 31.0% (27.8%) of the dollar trading volume in the before period for crash (spike) events. This is somewhat lower than normal, as shown by the negative and often significant HFT abnormal participation shares, but not dramatically so. The liquidity-demanding trades of HFTs are also balanced during these periods, as HFT order imbalances are close to zero for both event types. The HFT Demand Ratio shows that HFTs demand liquidity in 56% of their trades. Overall, these results suggest that HFTs did not cause these events, but they may have identified unusual market conditions in advance of the crashes/spikes and reduced their trading activity slightly in response

The during period HFT activity measurements are shown in column 3. HFTs trade in more during periods than before periods (273 of 315 crashes and 336 of 402 price spikes), but still are absent in a nontrivial number of events. Of those events with HFT trading, HFT net trading accounts for an average of 5% (4%) of the market's trading dollar volume during crashes (price spikes), both of which are significantly different from zero. HFTs are net buyers during crashes and net sellers during price spikes, indicating that their trading on balance mitigates the severity of these events. Similar to the before intervals, the relatively small HFT net trading as a share of market volume is explained by balanced mix of buys and sells rather than a low overall participation, as evidenced by the raw participation shares of 31.6% (29.0%) for crashes (price spikes). The abnormal participation shares show that overall HFT participation shares are close to normal levels during these events, but their liquidity-demanding participation drops and liquidity-supplying participation increases. The abnormal participation shares are mostly significantly or marginally significantly different from zero for both crashes and price spikes and all three definitions of participation, but again are not dramatic in magnitude. All point estimates are within +/- 5%. HFT order imbalances are significantly different from zero in the direction of the return for both event types. However, this only measures the effect of their liquidity-demanding trades, and does not result in overall price pressure because it is more than offset by their tendency to buy during crashes and sell during price spikes on their liquidity-supplying trades. This can be seen from their net trading as discussed above, and from the HFT Demand Ratio which shows that HFTs demand liquidity in less than half of their trading volume during these events. The results in the crash/spike periods suggest that HFTs trading direction and increased liquidity provision are stabilizing, but their net trading volumes relative to the market imply that the effects are probably small.

Finally, the after period HFT activity measurements are shown in column 4. HFTs are again absent in many after-event periods, but less frequently than in pre-event periods. Of those events with HFT trading, HFT net trading accounts for an average of 3% (3%) of the market's trading dollar volume after crashes (price spikes), both of which are significantly different from zero. After crashes, mean HFT Net Dollar Volume is positive while the mean HFT Net Dollar Volume Ratio is negative and significant. This is not a contradiction. HFTs trade in larger volumes during the crashes in which they are buying than in the crashes in which they are selling. However, HFT Net Dollar Volume is positively correlated with market dollar volume in this subsample, meaning HFTs tend to buy when market volume is high and sell when market volume is low, and this correlation is high enough to generate the negative ratios. After price spikes, HFTs are unambiguously net buyers. In both cases, the magnitude of HFT net dollar volume is small relative to the market. Again, the relatively small HFT net trading as a share of market volume is explained by balanced trading rather than a low overall participation. The abnormal participation shares show that HFT participation is slightly elevated after these events (with the exception of liquidity-demanding participation after crashes) and is often significantly positive. HFT order imbalances return to near-zero levels, and HFTs resume demanding liquidity in a normal fraction of their trades. In the periods after crashes and price spikes, HFTs do not seem to follow very consistent or dramatic patterns.

Overall, the results in this section suggest that HFTs' trading activity does not cause the crashes and price spikes studied, and tends to reduce their magnitude. However, based on their low net trading volumes relative to the market, HFTs are probably not the major drivers of price formation in these events. The HFT abnormal participation shares indicate that they decrease overall participation slightly prior to the extreme return events, and decrease liquidity-demanding participation while increasing liquidity-providing participation during the events. However, while the deviations from normal participation levels are often statistically significant, they are relatively small.

It is of interest to compare my results with the HFT behavior documented during the Flash Crash by the CFTC-SEC May 6 Report and KKST (2011). One of the most striking findings from the CFTC-SEC Report is that HFTs were net sellers in individual stocks before, during, and after the crash. They also find that HFTs increased their individual-stock participation before and during the crash and decreased their participation after the crash.[33] In contrast, I find buying during crashes and little evidence of net buying or selling before and after. My abnormal participation results are also much less extreme, and often have different signs. In the S&P 500 futures market, KKST (2011) finds that HFTs initially bought during the crash, then reversed and sold their inventory. While I do find evidence consistent with initial buying during crashes, the

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[33] It is arguably hard to determine the end of the Flash Crash for individual stocks. This assumes a market-wide end at 2:46 pm, May 6, 2010.

evidence of position reversals afterwards is mixed. In total, my results show that many of the HFT dynamics documented by the CFTC-SEC Report and KKST during the Flash Crash are not consistently seen in extreme return events. It is also of interest to compare these results with the finding in Brogaard (2012a) that HFTs increase their trading when volatility is high. My results show that this pattern seems to reverse in the extreme tail event volatility in this sample.

3.5 Price-destabilizing HFT Order Imbalances

Jarrow and Protter (2011) as well as the SEC have suggested that HFT order imbalances can lead to the destabilization of prices. I assess this issue. In particular, Jarrow and Protter (2011) develop a model where HFTs trade in unison on commonly observed signals and create momentum. The key assumptions of their model are that demand curves for stocks are downward sloping, HFTs do not anticipate their price impacts, and they can react faster to common signals than ordinary traders. While the authors do not specifically label this behavior as herding, it appears to be an appropriate description. The model does not explicitly predict reversals afterwards, but because the HFT trading pushes the price past its fundamental value and creates volatility, this seems to be implied. The SEC's Concept Release on Equity Market Structure describes the separate but similar phenomenon of momentum ignition strategies. In momentum ignition, an HFT seeks to cause momentum through issuing trades or quotes that either "spoof" other market participants into believing they are informed or trigger stop orders. No model is referenced, but the mechanisms described in De Long et al. (1990) and Allen and Gale (1992) seem capable of generating the effect they describe. The document does not provide evidence of the prevalence of momentum ignition or background on how it has become a concern, but the SEC asks for feedback on whether momentum ignition is currently a problem in the market. There is no herding necessarily implied by momentum ignition, as it could be caused by a single HFT. While the mechanisms differ, in either the Jarrow and Protter (2011) model or the trade-based version of momentum ignition, we would expect to see an HFT order flow imbalance followed by momentum and an eventual reversal.

3.5.1 Methodology

I cannot identify the trades of individual HFTs, so I cannot assess if multiple HFTs trade in unison as described in Jarrow and Protter (2011). However, if this behavior is present and extreme enough to affect prices, it should result in large aggregate HFT order imbalances. Similarly, the trade-based version of momentum ignition would likely require a large order imbalance. Therefore, an event study around large order imbalances is a natural test for the hypothesized patterns. To define the events, I measure HFT signed volumes over 5-second intervals in the full sample and then rank them into groups of 1000 for each stock. The events with signed volumes in the 1000th group are HFT buy events and those in the 1st group are HFT sell events. This corresponds to approximately 4.7 events of each type per stock per day assuming independence. I perform the ranking in the full sample but keep only events on days where NASDAQ inside quotes are available. This procedure results in a sample of 27,632 buy events and 28,108 sell events. The mean signed volumes are 3,813 shares for buy events and 3,896 shares for sell events. Next, I measure the NASDAQ midpoint returns from 30 seconds before the end of each event through 1800 seconds after the event ends at 1-second intervals. I form event windows for the 25-second period before each event, the 5-second event window, and postevent windows of the first and second 5-second intervals, the next 10-second interval, followed by a 20-second interval, a 30-second interval, a 10-minute window, and finally, a 20-minute window. Within each window, I calculate the mean return across all buy events and sell events, and perform a t-test to determine whether it is significantly different from zero. To examine whether the aggregation into event windows misses any obvious patterns, I also plot the 1-second mean midpoint prices with the midpoint 30 seconds before the end of the event normalized to 100. If the Jarrow and Protter (2011) model describes these events or they cause momentum ignition, we would expect to see initial momentum and an eventual reversal.

3.5.2 Results

The results are presented in Table 3.3. Panel A presents results for Buy events. There is a small insignificant negative return in the pre-event window, then a positive and significant return of 9.0 bps during the event. After the event, there are small but significant positive returns (momentum) up to 60 seconds after the event. This accumulates to 1.9 bps at 60 seconds. Subsequent returns are negative but insignificant, and do not offset the initial momentum. Panel B shows the results for sell events. The pre-event return is small and insignificant, and the event return is a significant -9.1 bps. For the 30 seconds after the event, returns are negative and significant (indicating momentum). This accumulates to a loss of 1.5 bps at 30 seconds. After 30 seconds, a reversal starts that is marginally significant from 30 to 60 seconds and significant from 1 minute to 10 minutes. The reversal is 3.2 bps and offsets the previous momentum as well as some of the initial event return. Figure 3.1 shows the 1-second midpoints, normalized to start at 100 at 30 seconds before the end of the event, plotted for the whole event window to verify that the event windows capture the relevant price dynamics. Overall, it appears there is moderate postevent momentum that does not reverse appreciably for buys and reverses fully for sells. However, these magnitudes are arguably not economically significant. Note that the returns reported for each event window do not compound to the cumulative returns because I use events that are missing observations in some intervals. This mostly affects events near the end of the trading day.

To the extent that there is momentum followed by reversal for sells, this is consistent with the Jarrow and Protter (2011) model and with trade-based momentum ignition. There are, however, other potential explanations. CRS (2005, 2008) find that overall order imbalances positively predict future returns. They attribute this to market maker inventory effects, and find this in data that predates HFT. And since Kraus and Stoll (1972), numerous studies have observed reversals after large trades, which are typically attributed to the price of immediacy paid to liquidity providers. These explanations have very different implications. Kraus and Stoll (1972) and CRS (2005, 2008) describe the effects of benign market frictions, De Long et al. (1990) and Jarrow and Protter (2011) present mechanisms that cause prices to overreact to real information, while Allen and Gale (1992) is entirely manipulative. My current tests are not able to distinguish between these mechanisms, but are only able to find (or fail to find) evidence consistent with their existence. For buys, given that no significant reversal is observed, I consider it more likely that the initial imbalance was informed. It is possible, however, that there is a reversal that is not visible in the test horizon. Possible future directions are to attempt to disentangle these explanations and to examine other timeframes and event severity.

3.6 Resiliency

Resiliency refers to how liquidity returns to a market after a shock. Kyle (1985) defines resiliency as "*the speed with which prices recover from a random, uninformative shock*." Other authors have extended the definition to include all shocks, as discussed in Large (2007). Resiliency is important to study in this setting for several reasons. First, it is considered particularly important in a limit order book market, where liquidity depends on public limit orders and there is no specialist to smooth volatility (see Large 2007). Second, market stability has received increased attention since the Flash Crash, and intuitively resiliency after routine shocks could be related to the ability to recover from larger shocks which cannot be studied as easily due to their infrequent occurrence. Finally, a more resilient market may be less susceptible to predatory trading (see Bessembinder, Carrion, Tuttle, and Venkataraman 2012). It is plausible that HFT affects resiliency and the direction is not clear. When a shock ends, HFT could rush to fill the book to gain time priority at the new prices. Alternately, HFT could avoid trading after shocks if they have no comparative advantage in determining when they have ended, and their presence in the market could cause adverse selection risk that dissuades other traders from replenishing the book rapidly. In this section, I compare the resiliency on high-HFT participation days to normal days.

3.6.1 Methodology

Resiliency has not been studied extensively, and the literature has not reached a consensus on how best to measure it. I follow a procedure loosely based on Degryse et al. (2005). I define resiliency events as large signed order imbalances, and focus on how long it takes for the prices for hypothetical orders of various sizes imputed from the limit order book to stabilize. This differs from Degryse et al. (2005), who define events as aggressive orders under the Biais, Hillion, and Spatt (1995) classifications, because I wish to study more extreme events.

I define the resiliency events as the top and bottom percentile signed volume over 1-minute intervals during regular trading hours within each stock. I calculate the percentiles in the full sample, but study only the events that occur on days where order book data is available. Next, I match events with limit order book snapshots from 300 seconds before the end of the event to 300 seconds after. The snapshots are at 60-second intervals. From each snapshot, I calculate the midpoint and the prices for hypothetical buy and sell trades of 100, 500, 1,000, and 5,000 shares. This gives nine prices at 11 points in time for each event. I trim snapshots at the 99th percentile buy prices and 1st percentile sell prices to avoid problems with stub quotes.[34] For presentation purposes I normalize the midpoint at 5 minutes prior to the event end to a value of 100, and adjust the hypothetical trade prices to give the same percent effective spreads as in the original snapshot. In the following snapshots, I apply the log price changes observed in the original sample to the normalized starting prices. I plot and tabulate the evolution of the average prices over the event window for buy events and sell events individually. To

[34] Stub quotes are very low bids or high asks posted by market makers to meet quoting obligations but avoid actually trading.

determine the permanent price impact, I take the mean change in normalized prices over the full event window. To determine the total price impact, I calculate the mean price change from the start of the event window to the maximum (minimum) prices for buy (sell) events. To assess "time to resiliency" for each price, I calculate the return from each point in time to the last price in the event window, and measure the point when the mean return across events becomes statistically insignificant at the 5% level. This assumes prices will be resilient within the 5-minute window.

I split the sample into days of high and normal HFT participation, as defined in Section 2.4, and perform this analysis separately in each subsample. Using the three definitions of HFT participation (All, Demand, and Supply), this results in six iterations of the procedure. I then compare the permanent price impact, total price impact, and time to resiliency for each price type across high and normal HFT participation days.

3.6.2 Results

This procedure results in a reasonable event sample, both in terms of severity and number of observations. The mean signed volumes are 14,677 shares for buys and 14,520 shares for sells. After matching with the limit order book, the sample contains 24,167 buy events and 24,801 sell events.

Table 3.4 and Figure 3.2 show the evolution of mean prices over the event window for events decomposed by HFT liquidity supply participation. I do not report results for the other participation categories in this level of detail to conserve space. The most notable feature is that, regardless of HFT participation, the market seems to be able to absorb these large imbalances without much temporary price impact. There is no visible reversal in the graphs, and upon inspection of the numerical values, the reversals are sometimes absent and other times small (1-3 bps). Another observation is that to the extent small lags in resiliency are present, they seem to be stronger in the opposite side of the book from the event imbalance (i.e., the sell side of the book for buy events). So we are observing small lags in new orders that chase the price move, rather than same-side resting orders fleeing the book after the shock. The table identifies prices that are not yet statistically resilient with asterisks, and there are differences in time to resiliency across prices within an event and across HFT participation. For example, after buy events on normal all HFT participation days, resiliency is immediate in the whole book, while on high participation days, it takes from 4 minutes on the sell side of the book to immediate at the 1000 share buy price. However, it appears the differences between resilient and nonresilient observations using the current methodology are often more related to test power than real differences. This is especially true when comparing high-HFT to normal HFT participation categories, because there are large differences in the number of observations between the two groups. One surprising result is that some of the prices experience momentum after the event has ended. For example, in Panel D, the mean midpoint drops a further 3 bps after the sell events end. This merits further analysis. It is possible that this is due to not precisely capturing the end of the order flow imbalance. Otherwise, we would expect the price to peak/trough at the end of the event and reverse afterwards as the temporary price impact dissipates. However, the magnitude is relatively small.

Table 3.5 summarizes the permanent and total price impacts and times to resiliency based on all three participation type decompositions. The results in this table also point to possible problems with the current tests. The permanent price impacts for different levels of the order book are different. For example, in sell events on normal HFT participation days, the permanent price impact for 1,000 share sells is 16 bps, while it is 20 bps for 1,000 share buys. This suggests that even if prices near the top of the book are stable, full book resiliency is still not achieved over this event window length, and I need a longer window to estimate the true permanent price changes. Alternately, it could be the case that the order book is very nonstationary. I consider it premature to draw any strong conclusions from this analysis.

3.7 Toxic Order Flow

In the debate about whether HFTs should have affirmative obligations (see discussion in Section 2.2), the concern is that HFTs will choose to exit the market when conditions are inhospitable and their withdrawal will harm the market. There is anecdotal evidence that some HFTs did exit during the flash crash. ELO (2011c) argue that this was a result of perceived asymmetric information, which they also refer to as toxic order flow. The academic literature provides both theoretical and empirical support for this concern. The model in Glosten and Milgrom (1985) predicts that liquidity will decline with increased information asymmetry, and the market shuts down when it reaches a threshold level where there are no prices that can facilitate trade. Also, Chaboud et al. (2009) find that ATs reduce liquidity provision just before economic data announcements and increase it afterwards. Furthermore, ELO (2011c) argue that when order flow is extremely toxic, in addition to ceasing to provide liquidity, "market makers [may] turn into liquidity consumers ... reinforc[ing] a brewing market imbalance ... destroying the market they were making" (p. 14) as they dump their inventory. KKST suggests that this occurred in the Flash Crash. In this section, I will test whether, in this sample, HFTs endogenously exit the market and/or demand liquidity on the way out when a proxy for information asymmetry is high.

As I proxy for information asymmetry, I use VPIN (Volume-synchronized Probability of Informed Trading). VPIN is a measure introduced in ELO (2011a) that uses order imbalances to detect informed trading. It is a descendent of PIN, (Probability of Informed Trading) from Easley, Kiefer, O'Hara, and Paperman (1996) and is theoretically motivated with a sequential trade model where informed trading results in trading imbalances. The main innovations of VPIN are ease of estimation and the use of a fixed-volume look-back window instead of a calendar time look-back window. The logic behind the fixed-volume window is that volume and information arrival are correlated, so with fixed volume windows, each observation contains roughly the same amount of information. See ELO (2011a) for a detailed VPIN calculation algorithm, but I reproduce the key elements here and note the implementation choices I have made.

The calculation is performed one stock at a time. The first step is to assign all trading volume into equal-sized buckets. Each bucket should contain 1/50th the average day's trading volume. I calculate average daily NASDAQ trading volumes over each quarter from TAQ data, and use these values to select bucket sizes for the next quarter. I impose a minimum bucket size of 1,000 shares. A starting point for the bucketing process needs to be selected, and ELO (2011a) does not provide specific guidance on this. I restart the bucketing process at 9:30 am every trading day, so the first trade after the opening cross starts a new bucket, and prior buckets are reformed moving back in time, so for each day's calculation, the prior bucket is always full. Second, the absolute value of order imbalance for each bucket is calculated as:

(3.1) $OI_i = |V_i^S - V_i^B| / (V_i^S + V_i^B)$

where V_i^S is the sell volume in bucket i, and V_i^B is the buy volume in bucket i. Third, after each bucket is filled, a VPIN observation is calculated. The VPIN calculation uses the last 50 buckets, so on average it is based on the last 24 hours of trading volume and is updated 50 times per day but the calendar time look-back window and update frequency vary with trading intensity. The first calculation of the day starts with the last bucket formed before the open and looks up to 3 days back to form 50 buckets. The first update of the day uses the first full bucket of the day plus 49 prior buckets, and so on. The VPIN formula is:

(3.2) $VPIN_i = (1/50) \text{ x} \sum_{j=i}^{i\text{-}49} OI_i$

I create a time series of VPINs for each stock-day by assigning each VPIN to the second after the last trade used in its calculation, and keeping it in effect until it is replaced in the next VPIN update.

Note that there is an unresolved controversy in the literature on whether the volume bucketing should be performed on transaction data or 1-minute bars. ELO (2011a) argue that trade signing is error-prone in fast markets and empirical results with transaction data give counterintuitive results, so 1-minute bars are preferred. Andersen and Bondarenko (2011) argue that 1-minute bars introduce extra apparent imbalance in fast markets by classifying all the volume in one minute either a buy or sell, when in reality it is mixed. In this analysis, I use transaction level data instead of 1-minute bars. I do not take a stand on the proper technique for general applications, but in the NASDAQ data, high-quality trade signs are provided so the ELO (2011a) concerns do not apply.

Next, to observe the relationship between VPIN and HFT participation, I perform a sorting procedure to assign trades to VPIN groups and compare the HFT participation levels across VPIN groups. For each stock, I sort all trades into twenty equal-size stock-specific VPIN groups. Group 1 contains the lowest 5% of all trades by VPIN rank, and group 20 contains the highest 5%. Within each group, I calculate HFT participation shares for all HFT participation, HFT demand participation, and HFT supply participation by stock. Then I subtract the stock's mean HFT participation levels for the whole sample to obtain abnormal participation levels. The result is a dataset with an HFT abnormal participation level for each stock in each group.

The results are shown in Table 3.6. For all three definitions of HFT participation, abnormal participation shares decline near-monotonically as VPIN increases from group 1 to group 20. The mean differences in abnormal participation between group 20 and 1 are -8.51%, -7.63%, and -3.70% for all HFT participation, HFT demand participation, and HFT supply participation, respectively. To assess the difference between high and normal levels of VPIN, I also report mean differences between abnormal participation in group 20 and the stock-specific mean of groups 5-15. These are -3.20%, -2.46%, and -1.96%, respectively. As these are lower than the 20-1 differences, VPIN seems to have an effect as it moves from normal to low levels as well as normal to high. All are significantly different from 0 at the 5% level. While the spreads on supply participation are lower, abnormal supply participation becomes negative at more moderate levels of VPIN than the other categories. Also, there are two low volume stocks in my sample that produce very infrequent and noisy VPIN updates. My results are robust to their exclusion.

I interpret these results as supportive of the hypothesis that HFTs reduce *both* their liquidity provision and liquidity-demanding participation when information asymmetry is high. It is worth noting that this seems to be a relatively smooth process, where participation declines continuously over the entire range of VPIN levels instead of remaining normal until very high VPIN levels and disappearing suddenly. I find no evidence of HFTs aggressively demanding liquidity and "destroying the market they were making" at high levels of VPIN. These results are consistent with the idea that VPIN is a reasonable proxy for information asymmetry, and strongly support the idea that VPIN is a useful measure that predicts HFT trading behavior. To the best of my knowledge, VPIN has not been validated in individual equity data previously in the literature. Future directions may be to create a market-wide VPIN using TAQ data, to test the variations of VPIN suggested by Anderson and Bondarenko (2011), to test the relationship between VPIN and prices and liquidity, and to test whether either rapid changes in VPIN or very extreme levels trigger the liquidity-demanding behavior hypothesized in ELO (2011c).

3.8 Conclusion

In this paper, I study HFT activity and returns around extreme return and order flow events in a proprietary sample of NASDAQ trades that identifies HFT participation. First, I examine HFT trading around extreme returns, or mini-flash crashes and price spikes. I find that, on average, there is no HFT selling pressure in the period before crashes and no HFT buying pressure in the periods before price spikes, so it is unlikely that HFTs trigger these events. Furthermore, HFTs tend to buy during crashes and sell during price spikes, and provide more liquidity than they consume during these events. These trading patterns are likely to have stabilizing effects, but because HFT net trading is small relative to the market, these effects are probably small. Next, I study returns around large HFT order imbalances to test whether these lead to price momentum, as suggested by Jarrow and Protter (2011) and the SEC's concept of momentum ignition. I find weak evidence of momentum after HFT order imbalances, but this effect is arguably economically insignificant. I also fail to find evidence of reversals after buy imbalances, which casts doubt on distortion-based explanations. In my third set of tests, I examine whether stock price resiliency is higher on days when HFT participation is high. The methodology I employ does not perform well in this sample, and I consider the results inconclusive. Finally, I examine how HFT participation levels vary with levels of sustained market-wide order flow imbalances, also called toxic order flow. Using the VPIN measure of order flow toxicity based on ELO (2011a), I find that HFT participation levels decrease as toxic order flow increases, but I find no evidence supporting concerns that HFTs increase their liquidity demand at very high levels. Overall, my results suggest that HFTs play a neutral or stabilizing role around extreme events.

3.9 References

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Table 3.1 Limit Order Book Summary Statistics

Book data provided by NASDAQ for a 120 stock sample. Sample period is the first full week of each quarter during 2008 and 2009, September 15, 2008 – September 19, 2008 (the week of Lehman's failure), and February 22, 2010 – February 26, 2010. Only limit order book snapshots between 9:30 am and 4:00 pm are used, exclusive of 9:30 am because this is before the opening cross. Screens include removing observations with locked or crossed markets or negative sizes, and with zero size at the top of the book but with some depth below.

Descriptive Statistics	
Days in Sample	50
Number of LOB Snapshots, total	2,342,090
Number of LOB Snapshots, screened	2,311,201
Quoted Half Spread ($)	
Mean	0.031
Std Dev	0.139
10th %ile	0.005
Median	0.010
90th %ile	0.055
Quoted Half Spread (%)	
Mean	0.12
Std Dev	0.31
10th %ile	0.01
Median	0.06
90th %ile	0.26
Best Bid Size	
Mean	2,162.9
Std Dev	8,582.6
10th %ile	100
Median	300
90th %ile	3,977
Best Ask Size	
Mean	2,205.6
Std Dev	9,116.8
10th %ile	100
Median	300
90th %ile	4,128
Total Bid Depth	
Mean	22,437.9
Std Dev	57,449.0
10th %ile	2,101
Median	6,020
90th %ile	44,750
Total Ask Depth	
Mean	22,245.1
Std Dev	56,954.3
10th %ile	2,132
Median	6,180
90th %ile	45,207

Table 3.2 Mini-Flash Crashes and Price Spikes

Mini-Flash Crashes (Price spikes) are defined as a return of less (greater) than -5% (5%) in 5 minutes or less. Returns are from NASDAQ trades and are the minimum (maximum) return experienced during the event interval. Order imbalances are liquidity-demanding buy dollar volume less liquidity-demanding sell dollar volume divided by total dollar volume. Two versions of order imbalance are calculated, differing in whether all trades or only HFT trades are used. HFT Demand Ratio is the fraction of HFT dollar volume that demanded liquidity. HFT participation shares are measured as dollar volume of trades with HFT participation divided by total dollar volume of trades. Three versions of participation shares are calculated, differing in whether HFT participation is defined as trades where an HFT participates in any side (All), the liquidity-demanding side (Demand), or the liquidity supplying side (Supply). HFT abnormal participation shares are defined as HFT participation share less the mean daily stock-specific HFT participation share. The sample period is January 2008 - December 2009 and February 22, 2010 - February 26, 2010. The sample contains 120 stocks.

Panel A: Crashes			
Crash Description	Before	During	After
Number of events	275	315	299
Number of stocks	63	64	64
Return			
Mean	0.15%	-6.65%	1.24%
t-statistic	0.86	-42.8	7.11
Mean Market Dollar Volume	1,264,774	1,757,086	2,186,958
Market Order Imbalance			
Mean	-0.046	-0.565	0.025
t-statistic	-1.11	-23.07	0.78
HFT Activity			
Periods with HFT activity	204	273	249
Mean HFT Net Dollar Volume	35,315	134,954	82,087
HFT Net Dollar Volume Ratio			
Mean	-0.01	0.05	-0.03
t-statistic	-0.82	3.75	-2.13
HFT Order Imbalance			
Mean	-0.033	-0.146	0.015
t-statistic	-1.04	-4.62	0.44
Mean HFT Demand Ratio	56.0%	42.1%	56.5%
HFT Participation			
Demand	18.6%	15.4%	21.9%
Supply	15.2%	20.3%	20.5%
All	31.0%	31.6%	37.3%
HFT Abnormal Participation			
Demand			
Mean	-2.4%	-4.6%	1.3%
t-statistic	-1.81	-4.62	1.23
Supply			
Mean	-1.4%	4.4%	4.3%
t-statistic	-1.22	4.24	3.78
All			
Mean	-2.6%	-0.6%	4.3%
t-statistic	-1.62	-0.49	3.05

Table 3.2 Continued

Panel B: Price spikes			
Crash Description	Before	During	After
Number of events	347	402	378
Number of stocks	72	74	74
Return			
Mean	-0.05%	7.19%	-1.30%
t-statistic	-0.38	28.66	-7.72
Mean Market Dollar Volume	363,321	743,494	933,924
Market Order Imbalance			
Mean	0.054	0.493	-0.020
t-statistic	1.53	21.49	-0.67
HFT Activity			
Periods with HFT activity	250	336	309
Mean HFT Net Dollar Volume	-10,895	-3,038	23,304
HFT Net Dollar Volume Ratio			
Mean	-0.02	-0.04	0.03
t-statistic	-1.3	-3.34	2.35
HFT Order Imbalance			
Mean	0.012	0.121	0.035
t-statistic	0.41	4.25	1.30
Mean HFT Demand Ratio	56.8%	48.2%	55.5%
HFT Participation			
Demand	16.4%	15.4%	19.2%
Supply	13.8%	16.6%	17.9%
All	27.8%	29.0%	32.9%
HFT Abnormal Participation			
Demand			
Mean	-4.6%	-4.4%	-1.5%
t-statistic	-4.7	-5.22	-1.51
Supply			
Mean	-1.9%	1.6%	2.6%
t-statistic	-1.95	1.88	2.50
All			
Mean	-5.2%	-2.4%	0.5%
t-statistic	-4.3	-2.19	0.38

Table 3.3 Event Study of Large 5-second HFT Order Imbalances

Returns are from NASDAQ quotes and are calculated using the last midpoint in each interval and are expressed in basis points. Order imbalances are HFT liquidity-demanding buy dollar volumes less HFT liquidity-demanding sell dollar volumes. Large HFT order imbalances are defined as imbalances in the top or bottom 1000th quantile for each stock. Quantile cutoffs are defined using full trade sample, but events only enter the sample on days when quotes are available. Event times are in seconds. T-statistics test the null that the mean return in each window is 0. The sample contains 120 stocks.

Event Window	Return	Cum. Return from t= -30	Cum. Return from t= 0
Panel A: HFT Buy Imbalances			
(-29, -5)	-0.3	-0.3	
	(-1.53)	(-1.53)	
(-4, 0)	9.0	8.7	
	(33.38)	(33.79)	
(1, 5)	1.1	9.7	1.1
	(7.30)	(44.3)	(7.30)
(6, 10)	0.3	10.0	1.4
	(3.36)	(44.76)	(8.20)
(11, 30)	0.3	10.4	1.7
	(2.48)	(41.46)	(8.30)
(31, 60)	0.2	10.7	1.9
	(1.86)	(38.81)	(8.43)
(61, 600)	-0.4	10.6	1.6
	(-0.85)	(19.41)	(3.10)
(601, 1800)	-0.8	10.8	1.6
	(-1.20)	(11.78)	(1.79)
Panel B: HFT Sell Imbalances			
(-29, -5)	0.0	0.0	
	(0.02)	(0.02)	
(-4, 0)	-9.1	-9.1	
	(-67.28)	(-40.19)	
(1, 5)	-0.8	-9.9	-0.8
	(-9.40)	(-42.60)	(-9.40)
(6, 10)	-0.2	-10.2	-1.0
	(-2.78)	(-42.51)	(-9.56)
(11, 30)	-0.5	-10.6	-1.5
	(-3.77)	(-40.02)	(-10.06)
(31, 60)	0.2	-10.4	-1.3
	(1.68)	(-36.95)	(-6.73)
(61, 600)	2.0	-8.6	0.8
	(4.27)	(-15.73)	(1.67)
(601, 1800)	1.0	-6.9	2.9
	(1.47)	(-7.73)	(3.38)

Table 3.4 Resiliency Event Study of Large 1-minute Order Imbalances

Resiliency events are defined as the 1st and 100th percentiles of 1-minute signed volumes for each stock. The prices are averages of normalized midpoints and prices for hypothetical buys and sells of various sizes calculated from NASDAQ limit order book snapshots from 300 seconds before the event to 300 seconds after. Percentiles are defined using full trade sample, but events only enter the sample on days when limit order book snapshots are available. ** signifies that the mean return from a price (midpoint) to the price for the same size trade (midpoint) at t=300 across stocks is significantly different from 0 at the 5% level based on a t-test. Event times are in seconds. The sample contains 120 stocks.

Panel A: Buy Events, Normal HFT Supply Participation Days

	Hypothetical sell prices					Hypothetical buy prices			
Event Time	5,000	1,000	500	100	mid	100	500	1000	5000
-300	99.22**	99.71**	99.81**	99.90**	100.00**	100.10**	100.16**	100.24**	100.59**
-240	99.26**	99.72**	99.81**	99.90**	100.00**	100.10**	100.15**	100.23**	100.55**
-180	99.27**	99.73**	99.82**	99.90**	100.00**	100.09**	100.14**	100.21**	100.53**
-120	99.29**	99.74**	99.83**	99.91**	100.00**	100.09**	100.14**	100.20**	100.51**
-60	99.31**	99.76**	99.85**	99.92**	100.01**	100.09**	100.13**	100.18**	100.48**
0	99.47	99.90	99.99	100.07	100.17	100.26	100.32	100.40	100.68**
60	99.46	99.91	100.00	100.09**	100.18**	100.27	100.33	100.41	100.71
120	99.46	99.91**	100.01**	100.08**	100.18**	100.27	100.33	100.41	100.72
180	99.46	99.91**	100.01	100.09**	100.18**	100.27	100.33	100.41	100.73
240	99.45	99.91	100.01	100.09**	100.18**	100.28	100.33	100.41	100.73
300	99.45	99.90	100.00	100.08	100.18	100.27	100.33	100.41	100.72

Panel B: Buy Events, High HFT Supply Participation Days

-300	99.54**	99.84**	99.89**	99.93**	100.00**	100.07**	100.10**	100.15**	100.33**
-240	99.58**	99.85**	99.90**	99.94**	100.00**	100.07**	100.10**	100.14**	100.30**
-180	99.59**	99.86**	99.90**	99.94**	100.00**	100.07**	100.10**	100.14**	100.29**
-120	99.61**	99.87**	99.91**	99.95**	100.01**	100.08**	100.10**	100.14**	100.28**
-60	99.64**	99.89**	99.93**	99.97**	100.03**	100.09**	100.11**	100.14**	100.28**
0	99.79	100.04	100.09**	100.12	100.18	100.25	100.28	100.32	100.45
60	99.79	100.05**	100.10**	100.13**	100.20**	100.27**	100.30**	100.34	100.48
120	99.78	100.04	100.08	100.12**	100.19**	100.26	100.29	100.34	100.48
180	99.77	100.03	100.08	100.12	100.18	100.25	100.29	100.34	100.47
240	99.76	100.02	100.07	100.11	100.18	100.25	100.29	100.33	100.47
300	99.76	100.03	100.07	100.11	100.18	100.25	100.28	100.33	100.47

Table 3.4 Continued

Panel C: Sell Events, Normal HFT Supply Participation Days									
	Hypothetical sell prices					Hypothetical buy prices			
Event Time	5,000	1,000	500	100	mid	100	500	1000	5000
-300	99.42**	99.79**	99.85**	99.90**	100.00**	100.10**	100.18**	100.28**	100.69**
-240	99.45**	99.80**	99.86**	99.90**	100.00**	100.09**	100.16**	100.26**	100.65**
-180	99.47**	99.81**	99.86**	99.90**	99.99**	100.08**	100.16**	100.25**	100.62**
-120	99.49**	99.81**	99.86**	99.90**	99.98**	100.07**	100.14**	100.24**	100.60**
-60	99.51**	99.82**	99.86**	99.89**	99.97**	100.05**	100.12**	100.21**	100.57**
0	99.30	99.64	99.69	99.74	99.83	99.91	99.98	100.07**	100.42**
60	99.28	99.62	99.68**	99.73**	99.82**	99.91**	99.98**	100.08**	100.42**
120	99.28	99.63	99.69	99.74**	99.82	99.91**	99.98	100.08**	100.43**
180	99.29	99.64	99.69	99.74**	99.83	99.92	99.99	100.08	100.44
240	99.28	99.63	99.69	99.74**	99.83**	99.92	99.99	100.09	100.44
300	99.28	99.64	99.70	99.75	99.84	99.93	100.00	100.10	100.45

Panel D: Sell Events, High HFT Supply Participation Days									
-300	99.61**	99.86**	99.90**	99.93**	100.00**	100.07**	100.12**	100.18**	100.40**
-240	99.65**	99.87**	99.90**	99.93**	100.00**	100.07**	100.11**	100.17**	100.35**
-180	99.66**	99.88**	99.91**	99.93**	100.00**	100.06**	100.11**	100.16**	100.34**
-120	99.67**	99.88**	99.91**	99.93**	99.99**	100.06**	100.10**	100.16**	100.33**
-60	99.67**	99.87**	99.90**	99.91**	99.97**	100.04**	100.08**	100.13**	100.31**
0	99.47**	99.68**	99.71**	99.74**	99.81**	99.88**	99.92**	99.98	100.14
60	99.44	99.67	99.70	99.73	99.80	99.87	99.91	99.97	100.14
120	99.44	99.66	99.70	99.73	99.80	99.87	99.91	99.97	100.14
180	99.43	99.66	99.69	99.73**	99.79	99.86	99.91	99.97	100.14
240	99.42	99.65	99.69	99.72**	99.79	99.86	99.90	99.96	100.13
300	99.41	99.65	99.68	99.72	99.78	99.85	99.90	99.96	100.13

Table 3.5 Resiliency Event Study Summary

Permanent Price Impacts, Total Price Impacts, and Times to Resiliency for midpoints and hypothetical trades of various sizes calculated from NASDAQ limit order book snapshots estimated across buy and sell resiliency events categorized by HFT participation on the event day. Events are defined as the 1st and 100th percentiles of 1-minute signed volumes for each stock. Total price impact is the mean return from the highest (lowest) observed price for buy (sell) events in the event window through 300 seconds after the event. Permanent price impact is the mean return from 300 seconds before the end of the event through 300 seconds after. Returns are in basis points. Time to resiliency is the time after which further mean returns through 300 seconds postevent are insignificantly different from 0 at the 5% level based on a t-test.

Panel A. Buy Events

HFT Participation	Resiliency Measure	Hypothetical sell prices					Hypothetical buy prices			
		5,000	1,000	500	100	mid	100	500	1000	5,000
All, Normal	Total PI	25	20	20	19	18	18	18	18	13
	Perm. PI	23	20	19	19	18	17	17	17	13
	Res. Time	0	0	0	0	0	0	0	0	0
All, High	Total PI	27	22	21	21	20	19	19	19	17
	Perm. PI	23	18	18	18	17	17	17	17	14
	Res. Time	240	240	240	240	240	120	120	0	0
Demand, Normal	Total PI	25	21	20	20	19	19	18	18	14
	Perm. PI	23	20	19	19	18	18	18	18	14
	Res. Time	0	0	0	0	0	0	0	0	0
Demand, High	Total PI	28	21	20	18	17	17	17	17	15
	Perm. PI	24	18	18	17	16	15	16	16	12
	Res. Time	0	240	240	240	300	180	0	0	0
Supply, Normal	Total PI	25	20	20	19	18	17	17	17	14
	Perm. PI	23	19	19	19	18	17	17	17	13
	Res. Time	0	240	180	300	300	0	0	0	60
Supply, High	Total PI	25	21	21	21	20	20	20	19	14
	Perm. PI	22	19	18	18	18	18	18	18	14
	Res. Time	0	120	120	180	180	120	120	0	0

Table 3.5 Continued

Panel B. Sell Events

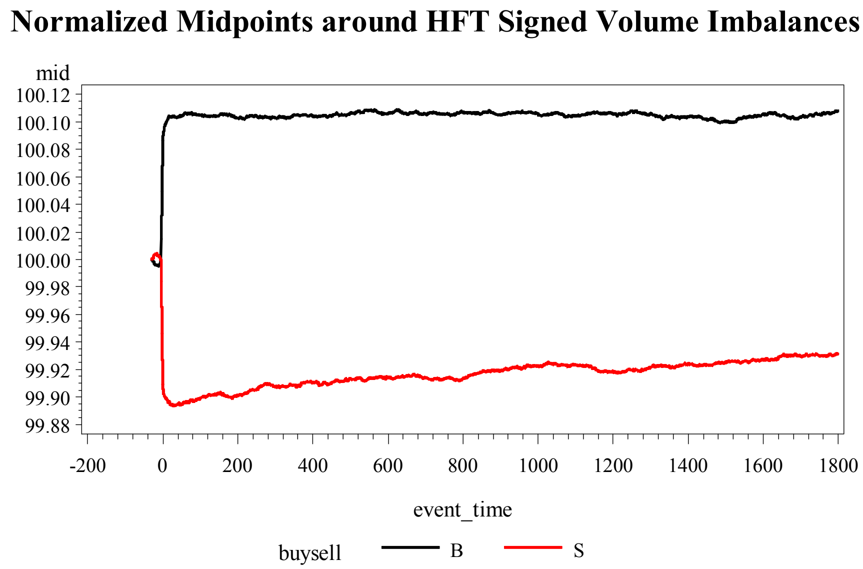
HFT Participation	Resiliency Measure	Hypothetical sell prices 5,000	1,000	500	100	mid	Hypothetical buy prices 100	500	1000	5,000
All, Normal	Total PI	-14	-17	-17	-18	-18	-19	-20	-21	-27
	Perm. PI	-14	-16	-16	-16	-17	-18	-19	-20	-25
	Res. Time	0	0	0	120	120	120	0	0	120
All, High	Total PI	-14	-17	-17	-17	-18	-19	-20	-21	-27
	Perm. PI	-13	-15	-15	-15	-16	-17	-18	-18	-24
	Res. Time	60	0	0	0	0	0	0	0	0
Demand, Normal	Total PI	-15	-18	-18	-18	-19	-19	-20	-21	-27
	Perm. PI	-15	-17	-17	-17	-18	-18	-19	-20	-26
	Res. Time	0	0	0	0	0	0	0	0	0
Demand, High	Total PI	-17	-18	-18	-18	-19	-20	-22	-23	-28
	Perm. PI	-17	-17	-17	-17	-17	-18	-18	-18	-22
	Res. Time	0	0	0	0	180	180	180	180	0
Supply, Normal	Total PI	-14	-17	-17	-17	-18	-19	-20	-21	-27
	Perm. PI	-13	-15	-15	-15	-16	-17	-18	-18	-24
	Res. Time	0	0	120	300	300	180	120	180	180
Supply, High	Total PI	-20	-21	-21	-21	-22	-22	-22	-22	-27
	Perm. PI	-20	-21	-21	-21	-22	-22	-22	-22	-27
	Res. Time	60	60	60	300	60	60	60	0	0

Table 3.6 HFT Abnormal Participation Shares by VPIN Groups

Abnormal participation shares are calculated as the HFT participation share for a stock measured in a specific group minus the overall participation share for that stock, and are averaged across stocks. Three versions of abnormal participation shares are calculated, differing in whether HFT participation is defined as trades where an HFT participates in any side (ALL), the liquidity-demanding side (DEMAND), or the liquidity supplying side (SUPPLY). VPIN is an order-flow imbalance based proxy for information asymmetry from Easley, Lopez DePrado, and O'Hara (2011). VPIN is calculated using NASDAQ trades only. VPIN groups are formed by ranking all trades within specific stock into 20 groups by VPIN. T-statistics test the null that the mean abnormal participation share in a group is 0. The sample contains 120 stocks.

VPIN Group	ALL_abnormal	DEMAND_abnormal	SUPPLY_abnormal
1	4.92%	4.99%	1.03%
2	2.93%	2.94%	0.63%
3	2.43%	2.34%	0.59%
4	2.12%	2.03%	0.65%
5	1.51%	1.60%	0.03%
6	0.83%	0.85%	-0.13%
7	0.42%	0.50%	-0.27%
8	0.03%	0.29%	-0.52%
9	-0.10%	0.08%	-0.60%
10	-0.38%	-0.09%	-0.83%
11	-0.65%	-0.50%	-0.79%
12	-0.81%	-0.52%	-0.89%
13	-0.97%	-0.82%	-0.89%
14	-0.88%	-0.61%	-1.01%
15	-1.35%	-0.98%	-1.23%
16	-1.10%	-0.72%	-1.20%
17	-1.67%	-1.21%	-1.53%
18	-1.87%	-1.40%	-1.64%
19	-2.45%	-1.70%	-2.08%
20	-3.59%	-2.64%	-2.67%
20-1			
Mean	-8.51%	-7.63%	-3.70%
T	-8.65	-7.66	-6.18
Min	-49.51%	-49.98%	-27.67%
Max	37.48%	43.82%	16.65%
20 – mean(5-15)			
Mean	-3.20%	-2.46%	-1.96%
T	-4.35	-3.22	-5.07
Min	-16.81%	-17.65%	-11.92%
Max	32.58%	39.09%	15.98%

Panel A.



Panel B.

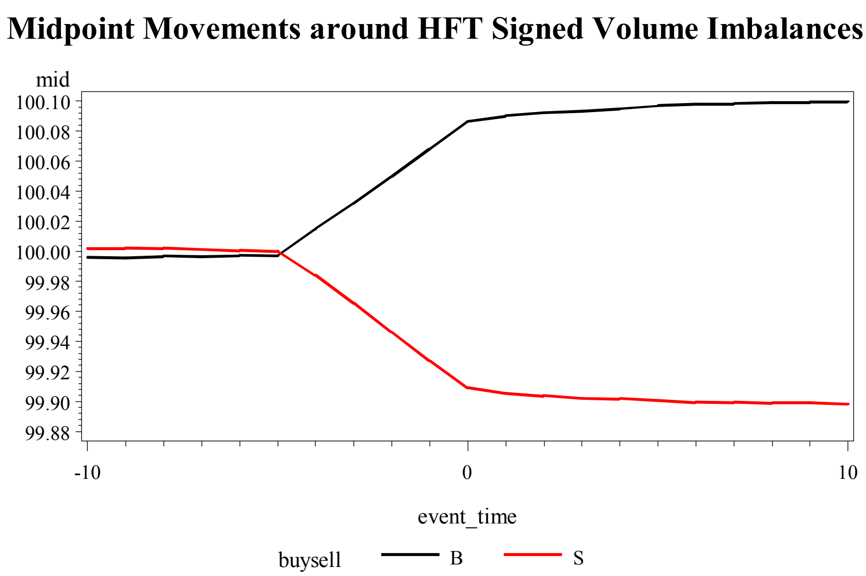
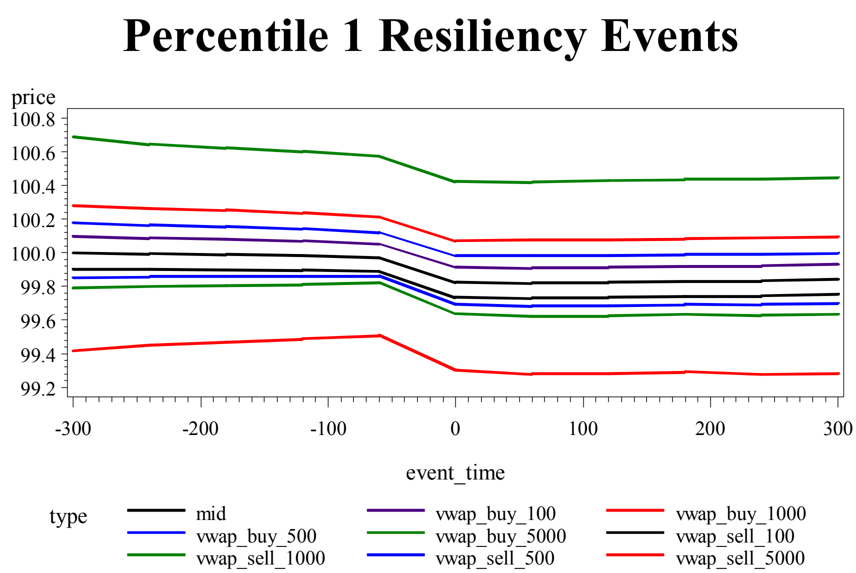


Figure 3.1 HFT Signed Volume Event Normalized Midpoint Plots

HFT signed order flow events are defined as the 1st and 1000th quantiles of 5-second signed HFT order volumes for each stock. The graphs show averages of normalized midpoints from 25 seconds before the event to 1800 seconds after. Midpoints are normalized to 100.00 30 seconds before each event. Event time is indexed such that the event ends at time 0.

Panel A. Sell Events on Normal HFT Liquidity Supply Participation Days



Panel B. Sell Events on High HFT Liquidity Supply Participation Days

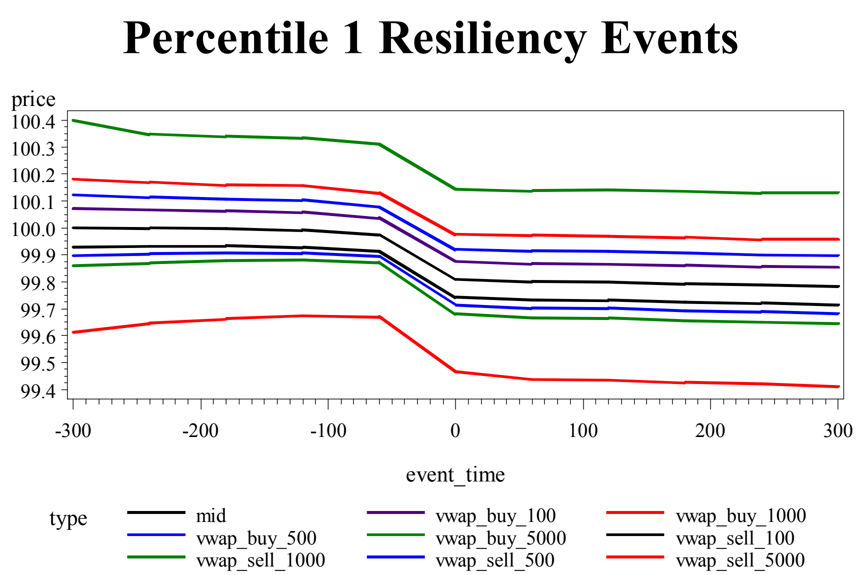
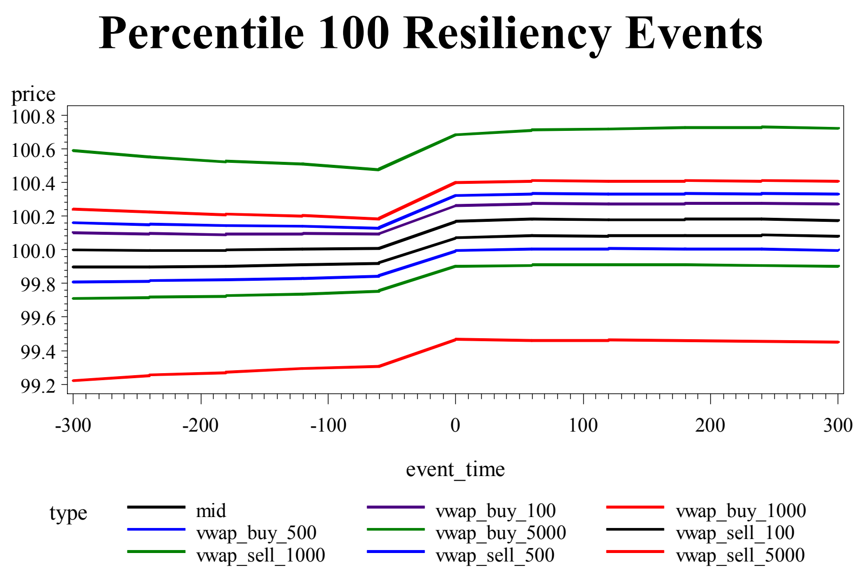


Figure 3.2 Resiliency Tests by HFT Liquidity Supply Participation Levels

Resiliency events are defined as the 1^{st} and 100^{th} percentiles of 1-minute signed order volumes for each stock. The graphs show averages of normalized midpoints and prices for hypothetical buys and sells of various sizes from 300 seconds before the event to 300 seconds after. Midpoints are normalized to 100.00 at 300 seconds before each event, and hypothetical trades are normalized to have the same percent effective spreads as actual trades. Event time is indexed such that the event ends at time 0.

Panel C. Buy Events on Normal HFT Liquidity Supply Participation Days



Panel D. Buy Events on High HFT Liquidity Supply Participation Days

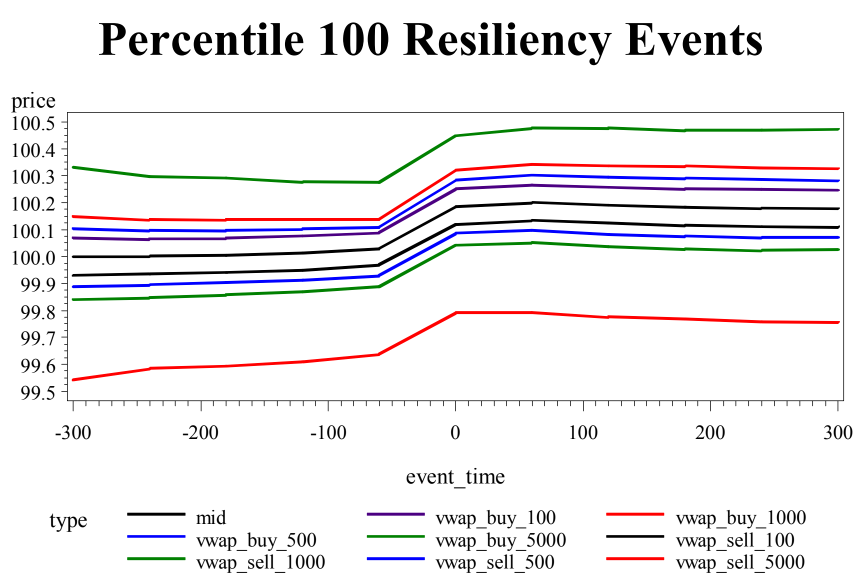


Figure 3.2 Continued

4. PRETRADE TRANSPARENCY AND CORPORATE BOND TRADING COSTS: EVIDENCE FROM THE NYSE AND OTC MARKETS

4.1 Abstract

I document dramatically lower trading costs for corporate bond transactions executed on the NYSE when compared to a matched sample from the OTC market. The difference in effective spreads is on the order of 100 bps, which is highly economically and statistically significant and has not been previously documented. This result survives controls for bond- and day-fixed effects and various trade characteristics, and holds whether cost differences are constrained to be constant across trades or are allowed to vary with trade characteristics. I attempt to identify the factors responsible for the cost difference, and find that the greater pretrade transparency on the NYSE is the most likely explanation. This contributes to our knowledge about market design and may help explain the negative relationship between spreads and trade sizes for OTC bond trades. I also develop and employ a methodology to estimate the impact of trade signing errors in data with stale quotes, and show my results are not likely to be driven by stale quote-related errors. This methodology may be useful in other applications where stale quotes are of concern.

4.2 Introduction

Trading costs in the corporate bond market have recently received increased attention in the academic literature. One factor driving the interest has been the introduction of the TRACE reporting system, which was implemented in stages beginning in 2002. The TRACE reporting system provides a rich and near-comprehensive data set of actual trade prices and sizes. More importantly, because TRACE data are available to market participants in near real-time, the introduction of TRACE allowed researchers to test conflicting theoretical predictions of the effects of increased transparency on the market. Bessembinder, Maxwell, and Venkataraman (2006), Goldstein, Hotchkiss, and Sirri (2006), and Edwards, Harris, and Piowar (2007) all address various aspects of the impact of TRACE. Bessembinder, Maxwell, and Venkataraman (2006) examine a sample of large insurance company trades around the time TRACE reporting was initiated, and find that improved transparency decreases trading costs. Their sample allows them to estimate trading costs before TRACE initiation for comparison. They also document a liquidity externality effect, where improved transparency in a group of bonds increases liquidity in related but less transparent bonds. Edwards, Harris, and Piowar (2007) examine a broader proprietary sample of OTC transactions from January 2003 through January 2005, and find that trading costs are lower for transparent bonds than for opaque bonds, and trading costs are reduced when a bond becomes more transparent. Goldstein, Hotchkiss, and Sirri (2006) study a group of BBB bonds that initiated TRACE reporting in 2003 and a matched sample that remained opaque during that period. They find that TRACE reporting lowered trading costs for all but the largest trades and the least frequently traded bonds, and that trading volume effects were not significant. Bessembinder and Maxwell (2008) provide a review of these studies.

The majority of corporate bond trading is conducted over-the-counter (OTC), where transactions are typically negotiated bilaterally with dealers. Compared to the equity markets, the corporate bond market is relatively opaque, even post-TRACE. TRACE provides only posttrade transparency, and pretrade transparency is limited. There is no centralized and widely-disseminated source of real-time quote data. Prior to trading, market participants often must contact dealers directly to obtain firm quotes and it is surely not practical to survey all dealers prior to a trade. There are proprietary market information systems (i.e., Bloomberg), and private electronic trading networks (i.e., MarketAxess) that provide quote data, but these are fragmented and have other limitations. Participation is not universal; in some cases quotes are indicative rather than firm, depth of market information is lacking, and infrequently traded issues may not be covered. Bessembinder and Maxwell (2008) provide a description of the OTC corporate bond market.

Some corporate bond trading occurs on the NYSE, in a very different environment. The NYSE's Automated Bond System (ABS) is structured as an electronic limit order book system with extensive pretrade transparency. [35] Biais and Green (2005) describe the NYSE's bond trading environment and compare it to the OTC market. Some corporate bond issues trade on both markets, so the ABS provides an opportunity to examine the impact of pretrade transparency on trading costs. Pretrade transparency

[35] Since the sample was collected, the ABS system has been replaced by the NYSE Bonds system. It appears trading rules on NYSE Bonds are largely the same, and the main differences are relaxed listing requirements and technology updates.

has been the subject of several papers, but there is not a strong consensus on how it should impact trading costs. Baruch (2005) develops a model predicting that pretrade transparency increases liquidity. Madhavan, Porter, and Weaver (2005) present a model that predicts wider spreads in more transparent markets, and they document decreased liquidity and increased trading costs when the Toronto Stock Exchange began to disclose more detailed order book data. Their findings are opposite those of Boehmer, Saar, and Yu (2005), who find that an increase in pretrade transparency in the NYSE's equity market led to decreased trading costs.

Two prior studies address the impacts of NYSE listing or trading on corporate bond transaction costs. Edwards, Harris, and Piowar (2007) include an indicator variable for NYSE ABS listing in their cross-sectional analysis of transaction costs, and find that ABS listing results in a small but statistically significant reduction for smaller trades. This result is not a major focus of their paper and is not discussed in detail. Further, they do not clarify whether their sample includes trades that were actually executed on the NYSE, and in any case, their regression specification does not directly measure the impact of the trading venue on execution costs. Hong and Warga (2000) do compare corporate bond transaction costs on the NYSE with those in the OTC market, and report that transaction costs are similar across markets.

In this paper, I use NYSE trade and quote data from February 6, 2002 to April 20, 2007 and TRACE transaction data from the same period to compare effective spreads across markets. [36] I use fixed-effects regressions to directly compare same-bond same-day trades, and add additional controls for trade size and direction. I find that effective

[36] The TRACE system was not in operation until July 2002, and was introduced in stages, so some analyses in this paper that require TRACE data do not use the NYSE data from early 2002.

spreads are on the order of 100 bps lower for trades on the NYSE using several different estimation methodologies. The mean spread for the OTC trades in my sample is 219 bps, so this is a significant difference across markets. [37] I discuss differences in market design and conclude that pretrade transparency is the most likely reason for the difference in trading costs.

This paper differs from Hong and Warga (2000) in several important ways. First, their sample is prior to the implementation of the TRACE system, so their results are affected by the lack of both pre- and posttrade transparency on the dealer market. Second, they are not able to obtain reliable small trade transaction costs from their sample of OTC trades, so they are forced to compare large OTC trades (500 bonds and over) with small NYSE trades in their analysis and therefore cannot effectively control for trade size. I use small OTC trades (less than 100 bonds), which are similar in size to typical NYSE trades, and show that it is important to control for trade size. Third, there are other methodological differences. They calculate trading costs based on round trips, while my sample allows me to use quotes. They identify and control for several bond-specific characteristics and market state variables explicitly, while I control for all possible bond-specific characteristics and daily variations in the market state using fixed effects regressions. Finally, my results are dramatically different from theirs.

An additional contribution this paper makes is the introduction of a new technique to assess the accuracy of trade buy vs. sell classifications in data characterized by potentially stale quotes. I rely on the quote rule (see Hasbrouck 1988 and Lee and Ready 1991), to sign trades, and my estimates of transaction costs are sensitive to

[37] This is not representative of all OTC trades, as my sample is restricted to trades of 100 bonds or less for comparability with NYSE trades.

misclassifications. A shortcoming of TAQ data for bonds is that quotes are only available at the close, and therefore are likely to be stale by the time a trade occurs, which introduces error into the quote-rule assignments. This new technique estimates the probability of error in the quote-rule assignments for each trade. I show that my key results are unlikely to be materially affected by this problem. This technique potentially has applications in other empirical microstructure work where the quotation data may be stale.

The rest of this paper is organized as follows. Section 4.3 discusses characteristics of the NYSE and OTC corporate bond markets that are relevant to trading costs. Section 4.4 describes the data sample and Section 4.5 describes my research methodology. Section 4.6 presents the results for the full sample of transactions, and Section 4.7 presents results for the subset of the transactions where dealers trade on the NYSE platform. Section 4.8 analyzes the robustness of these results to stale quote data. Section 4.9 concludes.

4.3 Corporate Bond Trading in the NYSE and OTC Markets

Hong and Warga (2000) and Biais and Green (2005) provide comparisons of the NYSE and OTC markets for bond trading, and Bessembinder and Maxwell (2008) provide additional detail on the OTC market. The following differences in market design, level of trading activity, and composition of the trader population are most relevant to this study:

1) Pretrade transparency. The NYSE has more pretrade transparency than the OTC market. With the possible exception of hidden orders, the full limit order book is displayed.[38] There is not a uniform theoretical prediction of the impact of this pretrade transparency on transaction costs. I reason that this should decrease trading costs on the NYSE, based on the evidence from the TRACE-related literature that increased transparency increases liquidity for corporate bonds, and because of the recent empirical equity market evidence from the same exchange in Boehmer, Saar, and Yu (2005). In contrast, the Madhavan, Porter, and Weaver (2005) finding of higher trading costs after an increase in pretrade transparency is based on older evidence from the TSE.

2) Trading volume. Trading volume is greater in the OTC market. This is true both overall and on a per-issue basis. According to Edwards, Harris, and Piowar (2007), fewer than 5% of all bonds are listed on the NYSE and NYSE trading in these issues accounts for 0 to 40% of all transactions. Biais and Green (2005) document that corporate bonds traded actively on the NYSE until volume migrated to the OTC market in the mid 1940s. The academic literature is consistent in predicting that increased trading volume lowers trading costs, so this effect should increase trading costs on the NYSE relative to the OTC market.

3) Price discrimination opportunities. We know from the literature on the OTC bond markets that liquidity providers charge lower spreads for larger orders. It can be argued that this is either a result of rent-seeking behavior or of dealers' fixed costs, but in either case, larger orders are empirically cheaper (from the customer's perspective) to execute. In the OTC market, a dealer is free to price discriminate by quoting different prices for different trade sizes. In the exchange environment, this is not possible. In a limit order book market with price priority rules, trading costs must be increasing in trade

[38] NYSE Bonds, the successor system to ABS, supports Reserve Orders where a portion of the order is hidden. I have not verified whether ABS also allowed hidden orders.

size. A liquidity provider posts a limit order with a specified price and size, and then a liquidity taker can choose to trade any lower size at that price. Larger trades will "walk the book" and execute at wider spreads.

This does not appear to have been addressed in the literature to date. It is reasonable to expect that this would cause dealers to quote wide spreads in anticipation of small trades. This effect should raise transaction costs for large trades on the NYSE relative to the OTC. The impact on small trades is unclear. These trades transact at the tip of the book bid-ask spread, which could be tighter if the dealer prices in some probability of a large trade or in the event a patient investor becomes the marginal liquidity provider. The spread for small trades would be unchanged, however, if a small trade is priced in with 100% probability and the dealer remains the marginal liquidity provider.

I empirically address these issues by estimating the difference in trading costs between the two markets separately for different transaction sizes. Although my sample does not contain truly large trades by OTC standards, the larger trades are large enough to trade at tighter effective spreads on the OTC market, and therefore should be large enough to capture this effect. I interpret a pattern where the NYSE trading costs increase relative to OTC costs as trade size increases as evidence consistent with this hypothesis. I interpret lower observed NYSE relative to OTC costs for the largest trades as evidence that this is not the only factor influencing the cost differences across the markets.

4) Investor participation. Observed trading costs on the NYSE could be lower due to higher investor participation. Dealers participate in every trade on the OTC market by definition, but the NYSE provides an environment where investors may trade directly with each other. Trades between two liquidity seekers may be executed more cheaply due to the lack of the requirement to compensate the liquidity provider. Note that this is not the same as claiming lower dealer participation increases overall liquidity, as we would expect depth to suffer and fewer trades to occur. I control for the possibility that my results are driven by differing levels of investor participation between the two markets by analyzing a subset of the NYSE trades that are likely to have dealers on at least one side.

5) Trade size. Trade sizes are smaller on the NYSE than the OTC market. As mentioned above, smaller corporate bond trades are known to have wider effective spreads. I address this by using a small trade subset of the TRACE data in my sample, and by controlling for trade size in most regression specifications.

6) Proportion of buys vs. sells. It is possible that trading costs are different depending on whether the buyer or the seller is seeking liquidity. Green, Hollifield, and Schurhoff (2007) conjecture that sellers in small municipal bond trades are generally more motivated than buyers, due to the availability of close substitutes. It is reasonable to assume this could apply to corporate bonds as well. If this is true, sales would be more expensive than buys. If the mix of buyers and sellers on each market are different, this could cause a difference in overall trading costs. I control for this possibility with regression specifications that control for trade direction.

My research design reflects these considerations. I use regression analyses that control for trade size, differences in costs for buys and sells, and dealer participation. The estimated residual difference in trading costs between the two markets can reflect the effects of pretrade transparency, market-wide trading volume, and price discrimination opportunities.[39] Assuming omitted factors are of secondary importance, the finding of lower NYSE costs can be interpreted as evidence that pretrade transparency is the driving factor. The other two effects are believed to increase costs for trades on the exchange, and the net difference is lower, so it appears that pretrade transparency lowers trading costs and is of sufficient magnitude to overcome the other factors.[40] If NYSE trading costs had been found to be higher, I would not have been able to separate out the impact of pretrade transparency with this analysis.

In the current version of this paper, I hope to have analyzed the most important differences between the two markets but there are details I have not yet considered. Specifically, I have omitted examination of possible impacts of minimum tick sizes, commissions, and accrued interest conventions. I am relying on documentation of trading rules from 2007, and still need to determine if there were any major changes during the sample period. I have not determined if there are any rules or registration records available for market makers operating on the NYSE. Finally, if the motivated seller hypothesis is true, this could result in midpoints that are lower than fair value. This may raise econometric issues that warrant attention, but should not affect my conclusions regarding the differences across markets.

4.4 Data Description

I obtain data for this paper from the NYSE and from TRACE. Sample descriptive statistics are presented in Tables 4.1-4.3. The NYSE initial sample contains trade and

[39] Technically the component of this factor that is unexplained by the size-market interaction terms in the regressions.
[40] With the possible exception of the price discrimination effect lowering NYSE costs for the smallest trades, which is discussed in more detail in Sections 5 and 6.

quote data for all ABS listed bonds from Feb 6, 2002 through April 20, 2007. Trade data consist of bond identifying information, trade date and time, price, number of bonds traded, and several trade condition flags. Quote data consist of bond identifying information, date, bid and ask prices and sizes (tip of the book only), several bond characteristic fields and flags, and information on the last trade and interest payment. Quotes are only available at the close. The initial TRACE sample contains all small trades (less than 100 bonds) for bonds in the NYSE sample that traded on the same day an NYSE trade occurred. The size restriction is used to select only OTC trades that are of similar size to typical NYSE trades. TRACE data consist of bond identifying information, trade date and time, price, number of bonds traded, and several trade condition flags.

4.4.1 Main Sample Selection

My study uses effective spreads as the primary measure of transaction costs, so I require that quote data be available for each trade. I impose additional trade-level screens described below. I am interested in comparing NYSE and OTC transaction costs, so I also require that a TRACE-reported OTC trade in the same bond be available on the same day.

The quote screens begin with the requirement that the bond must be a domestic corporate issue not in default and there must be both a valid bid and ask. I remove observations with extreme midpoint prices, defined as prices less than or equal to 90 or greater than or equal to 115, and several malformed records. These screens reduce the sample from 1,476,686 quotes to 198,675 quotes.

I eliminate NYSE trades that do not match screened two-sided quotes from the prior close. Then I remove records with errors, unusual status codes, and extreme trade prices. Extreme prices are defined as prices less than or equal to 80 or greater than or equal to 125, which is equal to the extreme quote midpoint definition +/- 10 points. For the remaining trades, I select all TRACE trades of less than 100 bonds in the same bond issue that trade on the same day. I eliminate TRACE trades that have unusual or error status codes. I flag TRACE trades that appear to have been executed on the NYSE, based on trade price, size, and time, and do not use these for matching candidates. Finally, I form a matched sample, keeping only NYSE trades where a TRACE-reported OTC trade is available. These screens reduce the NYSE sample from 346,471 to 54,405 NYSE trades. The final sample includes 114,860 matched TRACE trades. I refer to this final sample as the full sample in the rest of this paper.

4.4.2 Dealer Trade Sample Selection

I construct a subsample of my data consisting of dealer trades executed on the NYSE and matched with same-bond same-day TRACE-reported OTC trades that is used for the analysis in Section 6. To construct this subsample, I define a trade in the NYSE data as a dealer trade if it appears to be reported on TRACE. I implicitly assume that dealers participate in all trades reported to TRACE. This assumption may warrant further research; I have not verified that there is not some way a trade without dealer participation (such as two liquidity traders trading through a broker) could be reported. Specifically, for each trade in the NYSE data, I search for a TRACE trade in the same bond with the same price and size with a trade time that is within 15 minutes of the

NYSE trade time. If there is a match, I designate it as a dealer trade. I acknowledge that it is possible there are a few cases where the trade did not involve a dealer and was similar by coincidence, but this seems to be a reasonable approach. Then, following the same procedure used in the primary dataset, I match these with small same-bond same-day OTC-executed TRACE trades.

This approach may undercount the dealer trades on the NYSE for two reasons. First, if a trade is executed on the NYSE it is not required to be reported to TRACE. I conjecture that the reason some are is that some dealers find it expedient to report all trades on TRACE rather than sorting them. Second, I have not attempted to match trades allowing for the possibility of aggregation. If there are situations where two trades in the same issue on the NYSE are reported as one trade in TRACE or vice-versa, my algorithm would not identify them as dealer trades.

4.4.3 Main Sample Characteristics

Descriptive statistics for the initial and final samples are presented in Tables 4.1 and 4.2. The most striking features of the data are that the quoted spreads are wide, the trades are small, and most issues trade infrequently.

In the final sample, quoted spreads average \$2.2 per \$100 of face value, which is a significant portion of the expected annual return for a corporate bond. Most bonds trade near par, so this is close to a relative spread. For comparison, Bessembinder (2003) reported quoted relative spreads of 58 bps for all equities on the NYSE and 15.9 bps for large issues.

The average trade size in the NYSE sample was 28.5 bonds, the median was 15 bonds, and the 90th percentile was 55 bonds, where each bond has $1000 face value. These are clearly small retail-sized trades, which is consistent with Hong and Warga (2000) and Biais and Green (2005). This compares with average institutional trade sizes of 2,500-3,000 bonds in the insurance company sample described in Bessembinder, Maxwell, and Venkataraman (2006). The small trade sizes in this sample are significant because bond trading costs are known to be larger for small trades, and because retail traders are thought to be less informed about fair value and therefore more vulnerable to rent-seeking behavior by dealers. Also, the average trade size is lower than the average quoted size in the final sample, so most of these trades were unlikely to have walked the book.

The bonds in this sample did not typically trade daily. In the initial sample, which is more descriptive of actual market conditions, the average issue traded on the NYSE about once every 3 trading days, and the median issue traded only once every 22 trading days.

The characteristics of the final sample differ somewhat from the initial sample due to the screens imposed. This is a consequence of assembling a dataset with the goal of estimating transaction costs for a "normal" trade and meeting the data availability requirements. As shown in Tables 4.1 and 4.2, the final sample has higher prices, tighter quoted spreads, and slightly larger quoted and traded sizes.

4.4.4 Dealer Trade Sample Characteristics

Descriptive statistics for the dealer trade samples are presented in Table 4.3. After the selection process described in Section 4.4.2 is applied, the sample contains 3,704 NYSE trades and 41,785 OTC trades. NYSE dealer trades were identified in 154 out of the 227 issues from the full sample. The large number of OTC matches found for these trades suggests that dealers trade primarily in the more actively traded bonds. Trade sizes were marginally higher than the matched OTC trades, and lower than those in the full sample.

Comparing the number of trades, dealer participation is identified for 3,704 out of a total of 53,811 full sample trades, or about 7%. By multiplying the daily trading volume by the number of sample days in the dealer sample and comparing these totals to the full sample values in Table 4.2, we can see that dealer participation is identified for less than 5% of the full sample trading volume. However, the caveat from Section 4.4.2 that my algorithm may undercount dealer trades warns against weighting this observation too heavily.

4.5 Methodology

4.5.1 Effective Spreads

This study uses effective spreads as the primary measure of transaction costs and the dependent variable for all regression analyses. Effective spreads are the absolute value of the difference between the execution price and the midpoint of the quotes:

(4.1) $\text{Spread} = 2 \times |\text{Trade Price} - \text{Midpoint}|$

In my sample, the quotes in at the time of the trade are not available, so I use the prior day's closing quotes as a proxy. In Section 4.5.4, I address the error introduced by this approach and provide a mitigation strategy. Table 4.4 summarizes the effective spreads in this sample.

4.5.2 Fixed-Effects Regressions

I use a regression framework to analyze the determinants of observed effective spreads, with the goals of identifying components of the spread that can be attributed to the market on which the trade is executed, and if possible, drawing a conclusion about the impact of pretrade transparency. I use a fixed-effects model that estimates individual intercepts for each bond-day combination.

This approach controls for the possibility that unobserved factors that are correlated with the trading venue influence the effective spreads, and to a degree allows me to separate these factors from the market design issues of interest. For example, if trading costs are higher on lower credit quality issues, and these issues are traded more heavily OTC, this will decrease observed transaction costs on the NYSE relative to the OTC market. If trading costs are higher on more volatile days, and on these days there is more OTC trading volume, this will also decrease observed transaction costs on the NYSE relative to the OTC market. These effects will be absorbed by the bond-day intercepts, and will not influence the estimated coefficient on the NYSE dummy variable.[41]

[41] Assuming conditions required for OLS estimation hold.

I use variations of two main regression specifications. The first specification estimates a difference in spreads between the NYSE and OTC markets that is constrained to be constant across all trades:

$$\text{(4.2)} \quad \text{Spread}_{itn} = \alpha_i + \alpha_{it} + \beta_1 \text{ NYSE} + \beta_{2j} \text{ SIZE}_j + \beta_3 \text{ BUY} + \beta_4 \text{ SELL} + \varepsilon$$

where i indexes issues, t indexes days, n indexes trades, and j indexes trade size groups. NYSE is a dummy variable equal to 1 if a trade was executed on the NYSE and 0 otherwise. The coefficient on NYSE is of primary interest; this is an estimate of the transaction cost difference between the two markets not explained by the other explanatory variables. The fixed effects intercepts do not accommodate trade specific factors, so I add trade size and sign as additional controls. SIZE_j is a dummy variable equal to 1 if a trade belongs to size group j and 0 otherwise. The six trade size groups are 1-10, 11-20, 21-30, 31-49, 41-50, and >50 bonds. BUY is a dummy variable equal to 1 if a trade is a buy and 0 otherwise. Similarly, SELL is a dummy variable equal to 1 if a trade is a sell and 0 otherwise. When estimating versions of the regression model where trade side is controlled for, I remove unclassified trades from the data and rebalance the panel. Including an unclassified group would be equivalent to a dummy variable equal to 1 if the dependent variable of the regression is 0. In some specifications, SELL must be omitted to identify the regression.

The second regression specification allows the difference in spreads between the NYSE and OTC markets to vary with trade size and direction using interactions between the NYSE dummy and the trade-related control variables:

$$(4.3)\ \text{Spread}_{itn} = \alpha_i + \alpha_{it} + \beta_{1j}\,(\text{NYSE} * \text{SIZE}_j) + \beta_2\,(\text{NYSE} * \text{BUY}) + \beta_3\,(\text{NYSE} * \text{SELL}) + \beta_{4j}\,\text{SIZE}_j + \beta_5\,\text{BUY} + \beta_6\,\text{SELL} + \varepsilon$$

where indexes and variable definitions are identical to the first specification. When estimating versions of Equation (4.3) that include trade sign interactions or controls, I remove unclassified trades from the data and rebalance the panel. Unclassified trades have a measured effective spread of zero, so it is meaningless to compare transaction costs of unclassified trades between the two markets.

4.5.3 Trade Signing

Trade sign classifications are required to estimate regression Equations (4.2) and (4.3) and are not available in the data. I use the quote rule to determine whether a trade was buyer or seller initiated. The quote rule classifies all trades above the midpoint as buys and below the midpoint as sells. I leave trades at the midpoint unclassified. I choose not to apply the tick rule to these trades because I believe it will be unreliable due to the infrequency of trading and the price discovery that occurs in other markets between trades.

4.5.4 Stale Quote Error Analysis

As noted, NYSE quotes are only available at the close, so trades are matched with quotes that may be stale. It is clear that this can introduce error into actual effective spread measurements, but the primary issue in this study is the error that is introduced into the estimated differences between effective spreads across the two markets.

Consider two trades that occur at the same time, given that the quote has moved since the prior close, and let δ equal the movement in the midpoint since the prior close. Equation (4.4) shows the measured difference in spreads, Equation (4.5) shows the true difference in spreads, and Equation (4.6) shows the error.

$$(4.4) \quad Diff_{Measured} = Spread_{1Stale} - Spread_{2Stale} = 2(| price_1 - mid_{Stale} | - | price_2 - mid_{Stale} |)$$

$$(4.5) \quad Diff_{True} = Spread_{1True} - Spread_{2True} = 2(| price_1 - mid_{True} | - | price_2 - mid_{True} |)$$

$$= 2(| price_1 - (mid_{Stale} + \delta) | - | price_2 - (mid_{Stale} + \delta) |)$$

$$(4.6) \quad Error = Diff_{Measured} - Diff_{True} = 2(| price_1 - mid_{Stale} | - | price_2 - mid_{Stale} |$$

$$- | price_1 - (mid_{Stale} + \delta) | + | price_2 - (mid_{Stale} + \delta) |)$$

Note that when comparing trades of the same sign, Equation (4.6) reduces to:

$$Error = 2(price_1 - mid_{Stale} - price_2 + mid_{Stale} - price_1 + mid_{Stale} + \delta + price_2 - mid_{Stale} - \delta)$$

$$= 0$$

However, when trade 1 is a buy and trade 2 is a sell, Equation (4.6) reduces to:

$$Error = 2(price_1 - mid_{Stale} - mid_{Stale} + price_2 - price_1 + mid_{Stale} + \delta + mid_{Stale} + \delta - price_2)$$

$$= 4\delta$$

Similarly, when trade 1 is a sell and trade 2 is a buy, the error is -4δ.

From this analysis, it may appear that the use of stale quotes would not induce an error if the difference in effective spreads is estimated separately for buys and sales, which I do in some specifications of regression Equation (4.3). This is not the case, however. First, the trades do not execute at the same time except by chance. Second, buy and sell designations are not part of the data and are estimated with error. I leave the first issue for future work, but I develop a technique to partially address the second concern. I argue that, if it is possible to estimate this regression in such a way that there is higher confidence in the trade signs than in the baseline regression, the effective spread coefficients in this estimation should be less affected by stale quote errors than the baseline regression.

For every trade, I use a Brownian bridge to estimate the probability of a trade signing error as described below, and define sign confidence as 1- probability of signing error. I then estimate a variation of regression Equation (4.3) using only observations with a sign confidence of >= 85%. I also repeat the analysis using weighted least squares with each observation weighted by its sign confidence. If the results from either of these regressions differs materially from the baseline results, it is likely that the baseline results are significantly affected by stale quote-induced errors.

A Brownian bridge is a Brownian motion over an interval conditioned on the two fixed endpoints. A closed form formula exists which can be used to find a probability distribution for points along the interval. For a description of Brownian bridges, see Monte Carlo Methods in Financial Engineering by Glasserman (2003). To the best of my knowledge, I am the first to use Brownian bridges to model price movements between observations in an empirical market microstructure application, but they have found other uses in the finance literature. Ball and Torus (1983) and Stock (1990) model the pull-to-par price dynamics of bonds with Brownian bridges. Brennan and Schwartz (1990), Liu and Longstaff (2004) and Roll, Schwartz, and Subrahmanyam (2007) use Brownian bridges to model arbitrage convergence processes.[42]

I use the midpoints from the prior close (mid_0) and the trade day close (mid_1), the daily volatility of changes in midpoint closes for the issue over the entire sample (σ), and the time of the trade (t) to estimate the probability distribution of the midpoint at the time of the trade. I make the simplifying assumption that no price discovery occurs overnight, so the midpoint at 8:00 am (the TRACE open) is the same as at the prior day's at the 4:00 pm NYSE close. I also assume normality of midpoint price changes. The expected value of the midpoint at trade time using the Brownian bridge formula is just a linear interpolation:

$$(4.7) \quad E[mid_t] = [(close - t)\, mid_0 + (t\text{-}open)\, mid_1]/(close\text{-}open)$$

and the variance is given by:

$$(4.8) \quad var[mid_t] = [(t\text{-}open)(close - t)/(close\text{-}open)]\sigma^2$$

[42] Other articles using Brownian bridges include Duffie (1990), Cheng (1991), Back (1992), Manaster and Mann (1996), Cho (2003), and Chelley-Steeley (2005).

where time intervals are measured in fractions of a day for consistency with the volatility units.

For a trade classified as a buy to be signed in error, the midpoint must be higher than the trade price at the time of the trade. The test statistic for this is:

(4.9) $Z_{crit} = (\text{trade price} - E[mid_t])/ \, var[mid_t]^{1/2}$

And similarly for a trade classified as a sell to be signed in error, the midpoint must be lower than the trade price at the time of the trade. The test statistic for this is:

(4.10) $Z_{crit} = (E[mid_t] - \text{trade price})/ \, var[mid_t]^{1/2}$

So for each trade, the probability of a misclassification is:

(4.11) $P(\text{sign error}) = P(Z > Z_{crit})$

This analysis may be extended in several obvious ways. First, it may be possible to incorporate time elapsed between trades into the weighting scheme. Second, this scheme determines weights on individual trades, while the regression effectively operates on same-day, same-issue clusters of trades. A weighting scheme that applied the same weight to all trades in an issue on the same day based on some composite error probability may be more appropriate. Third, it may be possible to devise a weighting scheme to be applied to other regressions where the estimated difference in effective spreads is held constant across buys and sales. In this application, stale quote-induced errors should also be a function of the distribution of the midpoint at trade time but would be more complicated. Finally, I use a constant daily volatility of midpoint price moves estimated for the full sample. A GARCH-estimated volatility may be more realistic. I leave these extensions for future research. Despite these limitations, I argue that my strategy as implemented should work in the direction of reducing stale quote error in a large sample and provides a reasonable starting point for further work.

4.6 Full Sample Empirical Results

Table 4.5 presents the results from regression Equation (2) estimated in the full sample of trades. In interpreting the results in this and the tables that follow, it is important to understand that there are unreported intercepts for each bond-day combination, so the coefficients do not reveal the actual level of the effective spread for a particular type of trade.

The coefficient on the NYSE dummy variable is of primary interest, and can be interpreted as the effective spread premium for a corporate bond trade that is executed on the NYSE compared with a trade in the OTC market. In all regression specifications, this coefficient is negative, strongly economically and statistically significant, and changes little in magnitude with the inclusion of other explanatory variables. Negative values indicate lower transaction costs on the NYSE. Estimates range from -118 to -115 basis points.

Trade size is controlled for with dummy variables indicating whether a trade falls into a particular size group. In unreported regressions replacing the trade size dummies with continuous trade size and log of trade size, the results were qualitatively unchanged. The dummy variable approach was selected for ease of interpretation. The trade size dummy variables can be interpreted as the additional cost of executing a trade in that size group compared to a trade in the largest (>50 bonds) group. These show a pattern of effective spreads decreasing with trade size, which is consistent with the prior literature on the bond markets. Except for the 41-50 bond group, these coefficients are statistically significant, which indicates a difference in effective spread from the large trade group. Note that this specification constrains the relationship between effective spread and trade size to be constant across both markets. Table 4.4 suggests that the relationship is much stronger in the OTC market, and unreported regressions confirm this. This relates to the discussion in Section 4.3 regarding the ability to set different prices for different trade sizes in the OTC market but not on the exchange.

Table 4.5 also shows that effective spreads are wider for buys than sells. This provides evidence against the hypothesis that the availability of substitutes causes sells to be more expensive that buys.

Table 4.6 presents the results from regression Equation (4.3) estimated in the full sample of trades. Where Equation (4.2) constrained the coefficient on the NYSE to be constant across all trades, Equation (4.3) allows it to vary across trade size and direction by using interaction terms.

In every model in Table 4.6 that includes NYSE-size group interaction terms, these terms are all negative and economically and statistically significant. Model 1 shows that the effective spreads on NYSE trades of every size category are lower than effective spreads in the OTC market. The difference is greater for smaller trades, beginning at 128 bps for the trades of 1-10 bonds and decreasing near-monotonically to 75 bps for trades of more than 50 bonds. Model 4 shows that when a NYSE-buy interaction term is included, the magnitude of the differences for each size category decline but remain economically and statistically significant and retain their near-monotonic trend.

The observation that trading costs are lower on the NYSE for even the largest trade size groups suggests that the differences in trading costs are not entirely driven by the lack of price discrimination opportunities on the NYSE. As discussed in Section 4.3, this effect is expected to increase NYSE costs relative to the OTC market for the large trades.

Models 2-4 include NYSE-trade direction interaction terms. Again, these terms are all negative and economically and statistically significant. Model 2 shows that without controlling for size, effective spreads for buys on the NYSE are 140 bps lower than for buys on the OTC market and effective spreads for sales are 96 bps lower. Buys are 41 bps more expensive than sells on the OTC market, so these results suggest the difference between the interaction terms is primarily driven by the difference between the costs of buys and sells on the OTC market. Model 3 adds controls for trade size, and the estimated coefficients on the interaction terms are substantially identical. Model 4 adds trade size interaction terms and omits the NYSE-SELL interaction term to identify the regression. In this specification, the coefficient on the NYSE-BUY term drops by 96 bps and is very close to the incremental cost of a buy over a sale in the OTC market. These results indicate that, for a given trade size, the differential transaction cost differences between the two markets related to trade direction are explained by differences within the OTC market.

The stylized facts we can draw from this analysis are that (1) corporate bond trades are less expensive to execute on the NYSE than in the OTC market, (2) the cost difference is greater for small trades than large trades, (3) the cost difference is greater for buys than for sells, (4) the variations in cost differences related to trade size and direction are largely driven by variation within the OTC market, and (5) the difference in trading costs between the two markets is not entirely due to the lack price discrimination opportunities on the NYSE.

Based on the results in this section, we can conclude that the differences in trading costs between the two markets persist when we control for bond- and day-fixed effects, trade sizes, and relative differences in buys and sales, and must include some factor other than the lack of price discrimination opportunities on the NYSE. From the factors discussed in Section 4.3, this leaves pretrade transparency, trading volume, and level of dealer participation to explain the remainder. Since we observe lower trading costs on the NYSE, and the lower volume on the NYSE would predict higher costs, this implies that the combined effects of greater pretrade transparency and level of dealer participation effects decrease trading costs.

4.7 Dealer Trade Sample Empirical Results

Section 4.3 raises the possibility that lower trading costs on the NYSE are due to higher investor participation. In this sample, we do observe that dealers participate in a small fraction of the NYSE trades, implying higher investor participation, but this does not show causality.[43] Under the hypothesis that high investor participation causes lower trading costs, dealers charge higher spreads than liquidity traders to extract rents and/or cover costs regardless of the trading venue, and observed transaction costs are lower on the NYSE because low dealer participation causes a large percentage of the trades to occur with liquidity-seeking investors on both sides. In this section, I repeat the estimation of Equations (4.2) and (4.3) in a subsample of the NYSE trades that appear to be dealer-intermediated. If this hypothesis is correct, transaction costs will be roughly the same in both markets in this subset of trades. Note that this assumes that dealers are primarily liquidity providers when trading on the NYSE, which cannot be confirmed by the available data. This analysis is also interesting because it sheds light on the question of whether dealers behave differently when subject to the NYSE trading rules.

Table 4.7 presents the results from regression Equation (4.2) estimated in the dealer trade sample. As in the full sample, the coefficient on the NYSE dummy variable is strongly economically and statistically significant, and changes little with the inclusion of other explanatory variables. Estimates range from -104 to -109 basis points. This result is striking; the effective spread difference for trades executed with dealers on the NYSE compared to those on the OTC market are close to those in the full sample results. The null of no difference in execution costs for dealer trades between the two markets can be strongly rejected. Dealers appear to behave differently when trading in the NYSE. Low dealer participation does not explain the cost differences; if there were high levels of

[43] See discussion on the assumptions underlying this statement in Section 3.2.

dealer trading at the spreads we observe in this sample, trading costs on the NYSE would still be lower than on the OTC market.

Table 4.8 presents the results from regression Equation (4.3) estimated in the dealer trade sample. As in Table 4.6, in every model in Table 4.8 that includes NYSE-size group interaction terms, these terms are all negative and economically and statistically significant. Overall, the results in Table 4.8 are qualitatively similar to those in Table 4.6. The main exception is that the general pattern of the trading cost differences decreasing with trade size still exists, but is noticeably weaker and no longer near-monotonic. These results also lead to a strong rejection of the null of no difference in execution costs for dealer trades between the two markets.

From the dealer trade analysis, we can add two more stylized facts to those presented in Section 4.6: (6) the difference in the level of investor participation does not drive the difference in trading costs between the two markets, and (7) dealers trade at tighter spreads when transacting on the NYSE than when transacting in the OTC market.

These results, taken together with those in Section 4.6, combine to support the hypothesis that pretrade transparency lowers trading costs in the corporate bond market. We observe dramatically lower costs in the trading venue with greater pretrade transparency, and all the other factors we can account for would either tend to increase NYSE costs relative to OTC costs or have little impact.

4.8 Robustness to Stale Quote Errors

Section 4.5.4 discusses the possibility that the use of stale quotes introduces errors into the estimates of trading cost differences between the NYSE and OTC markets and presents a methodology to reduce this error. In this section, I apply this methodology to Model 3 from Table 4.6, and present the results in Table 4.9.

The basic concept is that stale quote error is reduced when the trading cost differences are estimated for buys and sells separately, conditional on the trades being signed correctly. If my main findings of lower trading costs on the NYSE are driven or boosted by stale quote errors, when the baseline regression is repeated with a technique that omits low confidence trades or places less weight on them, the trading cost differences between the two markets should be reduced. Using the technique described in Section 4.5.4, I estimate a trade sign confidence level for each trade where sufficient data exist. This results in a sample of 144,942 trades, compared to 163,729 for the full sample.

Model 3 estimates the trading cost differences for buys and sells separately through the use of interaction terms between the NYSE dummy and BUY and SELL dummies, and also controls for trade size and OTC buys. As benchmarks, Table 4.9 repeats the Model 3 results for the full sample of trades, and reports the results of the same regression estimated in the subsample where confidence estimates can be calculated but without using the confidence estimates in any way. Then I estimate the regression using two methods to reduce the influence of low confidence trades: once using a confidence level filter and once using confidence weighting. For the filter regression, I use only trades where the sign confidence level is 85% or higher. For the weighting regression, I use Weighted Least Squares estimation on all trades with sufficient data, with each trade weighted by confidence level.

The results show that my findings are not likely to be driven by stale quote-related errors. In both the filter regression and the weighting regression, the estimated differences between the two markets increase. The estimated cost reduction for buys executed on the NYSE increases from 144 bps using the standard regression to 154 bps (159 bps) in the filter (weighting) regression. The estimated cost reduction for sales executed on the NYSE increases from 99 bps using the standard regression to 105 bps (107 bps) in the filter (weighting) regression. These results are all strongly statistically significant, and suggest that stale quote errors might actually bias these estimates downwards.

4.9 Conclusions

This paper documents dramatically lower trading costs for corporate bond transactions executed on the NYSE than on the OTC market. This result persists when controlling for bond- and day-fixed effects, trade size, trade direction, and dealer participation. This also holds whether differences are constrained to be constant across trades or are allowed to vary with trade characteristics. Based on a new methodology I use to analyze the impact of stale quote error, the estimated trading cost differences do not appear to be driven by stale quotes and may be biased downwards.

The difference in effective spreads between the two markets is on the order of 100 bps without conditioning on trade characteristics. Conditioning on trade characteristics, the differences are always economically and statistically significant, and range from a low of 52 bps for large sells to a high of 152 bps for small buys. There is little variation in effective spreads across trade characteristics on the NYSE, so most of the variation in the differences is driven by variation in spreads on the OTC market.

I find that dealer-intermediated trades are less costly when executed on the NYSE than in the OTC market; this is surprising and important for two reasons. First, it rules out the hypothesis that the cost differences result from differences in investor participation. If every trade on the NYSE were dealer intermediated and nothing else changed endogenously, NYSE trading costs would still be lower. Second, the stylized fact that dealers appear to trade at tighter spreads when operating under the exchange rules is interesting in its own right. This parallels observations by Bessembinder, Maxwell, and Venkataraman (2006) regarding dealer behavior when subject to TRACE reporting rules, and raises the same question as to whether this can be attributed to lower operating expenses or rent extraction.

The results in this paper do not allow for a precise breakdown of the sources of the cost differences between the two markets, but are consistent with the hypothesis that pretrade transparency reduces trading costs in the corporate bond market. This is important because pretrade transparency is not well understood, and the academic literature contains conflicting theoretical predictions and empirical evidence of its effects. To the best of my knowledge, this paper is the first to present evidence relating to the effect of pretrade transparency in the corporate bond market since posttrade transparency has existed through TRACE. These results also highlight a previously unknown puzzle: why are small corporate bond trades executed OTC when an NYSE execution is available as an alternative? Agency problems that arise when a broker makes or influences the routing decisions seem to be a plausible explanation.

Finally, the method I develop to analyze stale quote-induced errors should increase confidence in these specific results and may be applicable to other problems in empirical market microstructure. I show that, under certain conditions, errors in the trading cost differences are related to trade signing errors. I introduce a procedure to estimate the probability of trade signing errors when applying the quote rule on stale quotes, using the time and price of the trade, quotes before and after the trade, and quote midpoint volatility. This analysis should be repeatable in any dataset containing this information.

4.10 References

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Table 4.1 Quote Summary Statistics

Quotes are NYSE closing quotes for corporate bonds reported from Feb 6, 2002 through April 20, 2007, a period of 1,313 trading days. Screens include: issue type must be domestic corporate; not in default; there must be a two-sided market; and the midpoint must be > 90 and <115. Malformed records were also removed.

Descriptive Statistics	Initial Sample	Final Sample
Number of Quotes	1,476,686	198,675
Market Type, Number obs		
2-sided	341,908	198,675
Ask only	9,986	0
Bid only	314,159	0
None	810,633	0
Midpoints		
Mean	101.29	103.70
Std Dev	37.37	4.73
Median	102.31	103.31
10th %ile	75.19	98.28
90th %ile	114.31	110.19
Quoted Spreads, $ per 100$ par		
Mean	3.04	2.21
Std Dev	2.92	1.90
Median	2.00	1.50
10th %ile	0.50	0.50
90th %ile	6.88	5.13
Ask Sizes		
N	351,894	198,675
Mean	41.6	44.7
Std Dev	56.5	60.8
Median	30	35
10th %ile	5	8
90th %ile	89	94
Bid Sizes		
N	656,067	198,675
Mean	43.9	46.1
Std Dev	52.7	64.9
Median	40	40
10th %ile	10	10
90th %ile	82	90

Table 4.2 Trade Summary Statistics

Corporate bonds trades from NYSE ABS system from Feb 6, 2002 through April 20, 2007 and matched small OTC trades from TRACE from July 1, 2002 through April 20, 2007. Screens include: all trades must be matched with a quote passing screening criteria; trades with extreme prices, unusual status codes, or errors are removed; NYSE trades must be matched with a same-bond, same-day TRACE trade; and TRACE trades must be for <100 bonds and not appear to be executed on the NYSE. Extreme prices are defined as <= 80 or >=125. Statistics for trades on an issue in a particular sample are only based on trades entering that sample, not all trades for that issue.

Descriptive Statistics	Initial NYSE Sample	Final NYSE Sample	Matched OTC Sample
Days in Sample	1,313	1,199	1,199
Number of trades	346,471	53,811	114,860
Num Issues	757	227	227
Trade size			
Mean	23.0	28.6	22.5
Std Dev	53.5	44.0	18.2
Median	12	15	17
10th %ile	3	5	5
90th %ile	50	56	50
Num of trades/day			
Mean	263.9	44.9	95.8
Std Dev	235.1	33.4	77.6
Median	180	40	78
10th %ile	33	8	22
90th %ile	614	86	174
Daily volume			
Mean	6,069.9	1,284.0	2,153.0
Std Dev	5,864.0	1,284.6	1,739.1
Median	3,813	943	1,790
10th %ile	559	136	436
90th %ile	14,413	2,795	4,082
Number of trades/day per issue			
Mean	0.349	0.198	0.422
Std Dev	0.986	0.684	1.606
Median	0.045	0.026	0.040
10th %ile	0.002	0.002	0.002
90th %ile	0.699	0.425	0.711
Daily trading volume per issue			
Mean	8.0	5.7	9.5
Std Dev	26.4	24.9	35.6
Median	0.8	0.4	0.9
10th %ile	0.0	0.0	0.0
90th %ile	14.6	9.0	17.3

Table 4.3 Dealer Trade Summary Statistics

Corporate bonds trades from NYSE ABS system meeting full sample screens that appear to be dealer trades and matched small OTC trades from TRACE. An NYSE trade is identified as a dealer trade if a same bond, same price, same size trade is reported on TRACE with a trade time +/- 15 minutes of the NYSE trade time. Statistics for trades on an issue in a particular sample are only based on trades entering that sample, not all trades for that issue.

Descriptive Statistics	NYSE Sample	OTC Sample
Days in Sample	1,006	1,006
Number of trades	3,704	41,785
Num Issues	154	154
Trade size		
Mean	18.5	22.0
Std Dev	16.9	17.8
Median	10	16
10th %ile	4	5
90th %ile	49	50
Num of trades/day		
Mean	3.7	41.5
Std Dev	2.9	60.6
Median	3	24
10th %ile	1	4
90th %ile	7	82
Daily volume		
Mean	68.2	914.6
Std Dev	64.3	1303.2
Median	50	511
10th %ile	10	65
90th %ile	155	1,963
Number of trades/day per issue		
Mean	0.024	0.443
Std Dev	0.064	1.324
Median	0.006	0.081
10th %ile	0.001	0.010
90th %ile	0.052	0.882
Daily trading volume per issue		
Mean	0.270	5.939
Std Dev	1.397	30.503
Median	0.018	0.309
10th %ile	0.002	0.025
90th %ile	0.346	6.860

Table 4.4 Mean Effective Spread Summary for Full Selected Sample

TRACE trades suspected of being executed on the NYSE are omitted from the OTC sample. Trades are signed using the quote rule.

	NYSE		OTC	
Trade Group	N	Effective Spread	N	Effective Spread
All	53,811	1.11	114,860	2.19
Trade Size				
1-10	22,170	1.15	43,537	2.34
11-20	10,215	1.11	26,190	2.30
21-30	8,088	1.12	21,163	2.15
31-40	2,626	1.09	5,657	1.98
41-50	4,958	1.10	11,457	1.86
>50	5,754	0.98	6,856	1.70
Trade Sign				
Buy	21,739	1.15	59,162	2.49
Sell	29,817	1.17	54,357	1.93
Unclassified	2,255	0.00	1,341	0.00

Table 4.5 Regression Estimates of Effective Spread Difference between Bond Trades on the NYSE and in the OTC Market using Various Controls

The regression model is:

$$Spread_{itn} = \alpha_i + \alpha_{it} + \beta_1 \text{ NYSE} + \beta_{2j} \text{ SIZE}_j + \beta_3 \text{ BUY} + \beta_4 \text{ SELL} + \varepsilon$$

where i indexes issues, t indexes days, n indexes trades, and j indexes trade size groups. The regression is estimated with bond and date fixed effects. NYSE is a dummy indicating whether trade was executed on NYSE. Trade size is in bonds. BUY and SELL are dummies indicating whether the trade was a buy or sell. Trades are signed using the quote rule. When controlling for trade sign, unclassified trades are removed from the data and the panel is rebalanced.

	Model			
Explanatory Variable	(1)	(2)	(3)	(4)
NYSE	-1.18	-1.17	-1.15	-1.15
	(-139.89)	(-139.14)	(-133.84)	(-133.22)
Trade Size				
1-10		0.38		0.38
		(24.77)		(24.87)
11-20		0.31		0.31
		(19.43)		(19.10)
21-30		0.22		0.22
		(13.56)		(13.05)
31-40		0.11		0.10
		(5.10)		(4.60)
41-50		0.03		0.03
		(1.85)		(1.66)
Trade Sign				
Buy			0.28	0.29
			(29.05)	(29.71)
N	168,671	168,671	163,729	163,729
R^2	48.3 %	48.8%	48.8%	49.1%

Table 4.6 Regression Estimates of Effective Spread Difference between Bond Trades on the NYSE and in the OTC Market Using Various Controls and Interactions

The regression model is:

$$Spread_{itn} = \alpha_i + \alpha_{it} + \beta_{1j} (NYSE * SIZE_j) + \beta_2 (NYSE * BUY) + \beta_3 (NYSE * SELL) + \beta_{4j} SIZE_j + \beta_5 BUY + \beta_6 SELL$$

where i indexes issues, t indexes days, n indexes trades, and j indexes trade size groups. The regression is estimated with bond and date fixed effects. NYSE is a dummy indicating whether trade was executed on NYSE. Trade size is in bonds. BUY and SELL are dummies indicating whether the trade was a buy or sell. Trades are signed using the quote rule. When controlling for trade sign, unclassified trades are removed from the data and the panel is rebalanced.

	Model			
Explanatory Variable	(1)	(2)	(3)	(4)
Trade Size				
1-10	0.61		0.37	0.60
	(30.08)		(24.17)	(29.77)
11-20	0.54		0.30	0.53
	(25.99)		(18.45)	(25.13)
21-30	0.42		0.21	0.41
	(19.63)		(12.48)	(18.74)
31-40	0.26		0.10	0.24
	(9.32)		(4.30)	(8.58)
41-50	0.15		0.02	0.14
	(6.55)		(1.27)	(5.95)
Trade Size Interactions				
1-10 x NYSE	-1.28			-1.07
	(-99.02)			(-71.32)
11-20 x NYSE	-1.31			-1.08
	(-73.11)			(-55.86)
21-30 x NYSE	-1.18			-0.95
	(-59.16)			(-44.04)
31-40 x NYSE	-1.02			-0.79
	(-28.65)			(-21.25)
41-50 x NYSE	-0.92			-0.68
	(-35.57)			(-24.69)
>50 x NYSE	-0.75			-0.52
	(-27.48)			(-17.93)
Trade Sign				
Buy		0.41	0.41	0.41
		(37.40)	(37.58)	(37.84)
Trade Sign Interactions				
Buy x NYSE		-1.40	-1.39	-0.45
		(-106.05)	(-105.10)	(-25.29)
Sell x NYSE		-0.96	-0.96	
		(-83.00)	(-83.44)	
N	168,671	163,729	163,729	163,729
R^2	48.9 %	49.0%	49.4%	49.5%

Table 4.7 Regression Estimates of Effective Spread Difference between Bond Trades Executed by Dealers on the NYSE and in the OTC Market Using Various Controls

A trade executed on the NYSE is considered a dealer trade if it is reported in TRACE. The regression model is:

$$Spread_{itn} = \alpha_i + \alpha_{it} + \beta_1 \text{ NYSE} + \beta_{2j} \text{ SIZE}_j + \beta_3 \text{ BUY} + \beta_4 \text{ SELL} + \varepsilon$$

where i indexes issues, t indexes days, n indexes trades, and j indexes trade size groups. The regression is estimated with bond and date fixed effects. NYSE is a dummy indicating whether trade was executed on NYSE. Trade size is in bonds. BUY and SELL are dummies indicating whether the trade was a buy or sell. Trades are signed using the quote rule. When controlling for trade sign, unclassified trades are removed from the data and the panel is rebalanced.

	Model			
Explanatory Variable	(1)	(2)	(3)	(4)
NYSE	-1.06	-1.09	-1.04	-1.06
	(-37.87)	(-38.74)	(-36.36)	(-37.27)
Trade Size				
1-10		0.43		0.43
		(12.39)		(12.04)
11-20		0.36		0.33
		(9.90)		(8.97)
21-30		0.31		0.28
		(8.31)		(7.55)
31-40		0.18		0.15
		(3.74)		(3.11)
41-50		0.09		0.08
		(2.29)		(1.94)
Trade Sign				
Buy			0.44	0.45
			(22.15)	(22.46)
N	45,489	45,489	43,395	43,395
R^2	40.4 %	40.8 %	41.5%	41.9%

Table 4.8 Regression Estimates of Effective Spread Difference between Dealer Bond Trades on the NYSE and the OTC Market Using Various Controls and Interactions

A trade executed on the NYSE is considered a dealer trade if it is reported in TRACE. The regression model is:

$$\text{Spread}_{itn} = \alpha_i + \alpha_{it} + \beta_{1j}\,(\text{NYSE} * \text{SIZE}_j) + \beta_2\,(\text{NYSE} * \text{BUY}) + \beta_3\,(\text{NYSE} * \text{SELL}) + \beta_{4j}\,\text{SIZE}_j + \beta_5\,\text{BUY} + \beta_6\,\text{SELL}$$

where i indexes issues, t indexes days, n indexes trades, and j indexes trade size groups. The regression is estimated with bond and date fixed effects. NYSE is a dummy indicating whether trade was executed on NYSE. Trade size is in bonds. BUY and SELL are dummies indicating whether the trade was a buy or sell. Trades are signed using the quote rule. When controlling for trade sign, unclassified trades are removed from the data and the panel is rebalanced.

	Model			
Explanatory Variable	(1)	(2)	(3)	(4)
Trade Size				
1-10	.45		0.43	0.44
	(12.51)		(12.03)	(12.11)
11-20	0.38		0.33	0.35
	(10.20)		(8.96)	(9.19)
21-30	0.32		0.28	0.30
	(8.62)		(7.56)	(7.84)
31-40	0.19		0.15	0.16
	(3.82)		(3.11)	(3.13)
41-50	0.10		0.08	0.08
	(2.47)		(1.95)	(2.01)
Trade Size Interactions				
1-10 x NYSE	-1.08			-0.87
	(-26.83)			(-18.55)
11-20 x NYSE	-1.16			-0.92
	(-18.46)			(-13.32)
21-30 x NYSE	-1.15			-0.94
	(-16.43)			(-12.39)
31-40 x NYSE	-0.97			-0.72
	(-6.59)			(-4.76)
41-50 x NYSE	-1.00			-0.70
	(-10.19)			(-6.67)
>50 x NYSE	-0.78			-0.60
	(-5.46)			(-4.07)
Trade Sign				
Buy		0.48	0.48	0.48
		(23.46)	(23.74)	(23.76)
Trade Sign Interactions				
Buy x NYSE		-1.32	-1.34	-0.48
		(-29.68)	(-30.24)	(-8.24)
Sell x NYSE		-0.84	-0.86	
		(-22.14)	(-22.89)	
N	45,489	43,395	43,395	43,395
R^2	40.8 %	41.6%	41.9%	42.0%

Table 4.9 Impact of Stale Quotes on Estimates of Effective Spread Difference between Bond Trades on the NYSE and OTC Market Controlling for Trade Size and Sign

Sign interaction terms are used to estimate differences separately for buys and sells. The regression model is:

$$Spread_{itn} = \alpha_i + \alpha_{it} + \beta_1 (NYSE * BUY) + \beta_2 (NYSE * SELL) + \beta_{3j}\, SIZE_j + \beta_4\, BUY$$

where i indexes issues, t indexes days, n indexes trades, and j indexes trade size groups. The regression is estimated with bond and date fixed effects. NYSE is a dummy indicating whether trade was executed on NYSE. Trade size is in bonds. BUY and SELL are dummies indicating whether the trade was a buy or sell. Trades are signed using the quote rule. When controlling for trade sign, unclassified trades are removed from the data and the panel is rebalanced. The baseline model is Model 3 from Table 5 and is estimated in the full sample. Confidence level refers to the confidence that the trade is signed correctly. The "Conf. Data" model is the same regression repeated on the subset of data where trade sign confidence levels can be estimated. The "Conf > =85%" model is the same regression estimated on the subset of data where the confidence level is >= 85%. The WLS model is estimated using Weighted Least Squares where trades are weighted by sign confidence. Stale quote errors introduce less error into estimated effective spread differences when trade sign confidence is high and differences are estimated separately for buys and sells.

	Model			
Explanatory Variable	Baseline	Conf. Data	Conf . > =85%	WLS
Trade Size				
1-10	0.37	0.33	0.29	0.35
	(24.17)	(20.46)	(11.94)	(20.80)
11-20	0.30	0.26	0.20	0.27
	(18.45)	(15.35)	(8.02)	(15.53)
21-30	0.21	0.16	0.12	0.17
	(12.48)	(9.53)	(4.50)	(9.67)
31-40	0.10	0.07	0.00	0.06
	(4.30)	(2.98)	(0.02)	(2.67)
41-50	0.02	-0.02	-0.06	-0.02
	(1.27)	(-0.80)	(-2.13)	(-0.84)
Trade Sign				
Buy	0.41	0.44	0.46	0.46
	(37.58)	(38.77)	(29.28)	(40.17)
Trade Sign Interactions				
Buy x NYSE	-1.39	-1.44	-1.54	-1.59
	(-105.10)	(-107.84)	(-72.02)	(-110.02)
Sell x NYSE	-0.96	-1.00	-1.05	-1.07
	(-83.44)	(-85.18)	(-65.71)	(-89.98)
N	163,729	144,942	83,282	144,942
R^2	49.4%	48.0 %	51.8 %	51.0 %