Dark Pool Exclusivity Matters*

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Abstract

Recent dark pool proliferation has magnified regulatory and academic concerns about equal access and market quality implications. Some dark pools, hoping to create an environment more amenable to buy-side institutional investors, craft their rules to discourage – or even exclude – brokers, high frequency traders and order-flow-information traders. We examine the role participation constraints play in large trade execution and find that a dark pool targeting buy-side counterparties experiences less serial correlation in returns, less volume and volatility increase pre-trade, and more trade clustering within and across days. Exclusivity influences execution quality. Not all dark pools are created equal.

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1 Introduction

A fundamental issue of interest to regulators and academics is the trade-off between equal access and market quality. For example, the U.S Securities and Exchange Commission (SEC) phased-in the Regulation NMS Order Protection Rule in 2006 to increase the probability that investors' displayed limit orders receive equal treatment in terms of price and time priority.[1] Among the rules' substantial, and perhaps unintended, consequences for U.S equity markets have been the drastic reduction of average trade size, increased high-frequency trading activity, extreme intradaily volume and volatility spikes, and the proliferation of "dark pool" alternative trading systems (ATSs). These ATSs are commonly referred to as "dark" because they do not disseminate their quotes to the public.[2]

Regulators continue to wrestle with the trade-off between equal access and market quality as they contemplate new rules for dark pools. In 2009, the SEC stated that its concerns included dark pools' conveying indications of interest (IOIs) only to an exclusive group of market participants, and that this "could lead to a two-tiered market in which the public does not have fair access to information about the best available prices and sizes for a stock that is available to some market participants." However the SEC argues for exempting "certain narrowly targeted IOIs related to large orders":

> These size discovery mechanisms are offered by dark pools that specialize in large trades. In particular, the proposal would exclude IOIs for $200,000 or more that are communicated only to those who are reasonably believed to represent current contra-side trading interest of equally large size. The ability to have a method for connecting investors desiring to trade shares in large blocks can enable those investors to trade efficiently in sizes much larger than the average size of trades in the public markets.[3]

The SEC's proposed display exemption suggests that dark pool participation could be based on minimum trade size. While motivated by the notion that large trades will more likely originate from contra-side large firms, such an approach does not directly consider the

[1] Securities and Exchange Commission (2005).

[2] As most ATSs do not publish their quotes, we will use "ATSs" and "dark pools" interchangeably. We recognize that some electronic communications networks are not dark and yet are classified as ATSs.

[3] Securities and Exchange Commission (2009).

attributes and trading behavior of said contra-side large firms. An alternative approach is to address directly criteria restricting the likely trading population. In practice, the degree to which trading venues restrict the trading population varies widely. Exchanges commonly exclude no one, allowing all market participants to place orders (displayed or undisplayed). In contrast, many dark pools seek to restrict the eligible trading population. Access can depend on whether a given dark pool admits institutional investors, some or all broker-dealers, high-frequency traders, and specific execution algorithms.[4] In the extreme, a few dark pools design their rules and monitor trading in an attempt to limit access to buy-side (natural contra-side) institutional investors.

An interesting question, previously unaddressed in the academic literature, is whether dark pool exclusivity affects execution quality. If greater restrictions to access do not improve execution quality, one might well ask why regulators should allow dark pool rules which permit unequal access. To analyze whether access restrictions affect execution quality, we obtain execution data originating in a dark pool designed to match buy-side institutional investors with other buy-siders (Liquidnet Classic, hereafter referred to as the exclusive venue or exclusive dark pool) and provide a contrast to execution data originating in less-exclusive venues. Because we are unable to obtain order-level data, we develop four hypotheses whose tests rely only on execution data.

Our first and second hypotheses address an important subset of execution-quality metrics which we characterize as a large trade's "execution footprint." Due to the size of their orders, buy-side traders are an important class of serial traders who coordinate multiple executions in a single stock within, and frequently across, days. Such traders likely prefer that any single execution in a series create minimal disruption. In other words, they wish to leave as small an execution footprint as possible.

[4] Access to certain order types can also be restricted. In May 2013, the Financial Industry Regulatory Authority (Finra) sent examination letters to 15 dark pool operators. The letters ask whether access to any order type is restricted and, if so, for an explanation "why the order type is limited to certain parties." "New Scrutiny of 'Dark Pools': Regulators Ask for Details on Stock Trading in Murkiest Parts of the Market," by Scott Patterson, Wall Street Journal, June 6, 2013, page C1.

We first hypothesize that access restriction will lead to a smaller execution footprint as reflected in smaller serial correlation in returns before and after large trades. High magnitudes of these correlations – positive or negative – would likely be considered undesirable by buy-side traders. Consistent with our first hypothesis, we find lower magnitude return correlation around large trades at the exclusive dark pool.

Similarly, our second hypothesis is that access restriction will lead to a smaller execution footprint as reflected in less volume and volatility prior to large trades. If an exclusive dark pool successfully mitigates order-flow information leakage and front-running, the aggregate market should be more prone to relative quiet prior to the execution of large trades at that dark pool. Consistent with our second hypothesis, we document that other dark pools exhibit significant increases in volume and volatility above those at the exclusive dark pool.

We employ several methods to control for market conditions and stock characteristics, and to examine the possibility that expected smaller footprint trades are routed to the exclusive venue. The differences we observe are not due to lower trade difficulty at the exclusive venue. Comparing trades with similar ones at other dark pools, executions are more difficult, at least if difficulty is calibrated by the large trade's volume share (trade size divided by average daily volume) or the security's liquidity as proxied by average daily volume. Our results are similar in linear regressions that include controls for market conditions and stock characteristics. Using a Heckman (1979)-like two-stage estimation procedure provides additional confidence that predictable selection bias is not driving our results.

A smaller execution footprint at the exclusive venue does not necessarily imply higher overall execution quality for the buy-side trader. We expect variation in probability of execution, trading fees and trader preferences to all affect the trader's overall execution quality. Moreover, it is possible that these costs are correspondingly high when the execution footprint is small. As we lack detailed order data to examine directly the individual components of overall execution quality, we develop our third and fourth hypotheses to test overall execution quality broadly by comparing intra-day and inter-day trade clustering among venues.

If all venues provide equal overall execution quality, we would expect to see similar trading patterns for all venues both within days (Hypothesis 3) and across days (Hypothesis 4). However, we find that trades at the exclusive venue occur earlier in the day, consistent with a preference for executions at the exclusive venue. We also document greater inter-daily large trade clustering at the exclusive venue, consistent with traders' willingness to trade boldly and sequentially, presumably due to a perception of higher overall execution quality.

Our results contribute novel empirical evidence to the policy discussion regarding the costs and benefits to execution quality of darkness and exclusivity in trading venues. We also document relevant empirical regularities for those seeking to model the role different types of dark pools may play in multi-venue trading contexts.

2 Background and Related Literature

Dark pools have recently experienced increased attention from regulators, media outlets and academics. The previously-quoted SEC statements indicate a clear policy interest in dark pools' contributions to market quality and equality of market access. The Tabb Group estimates that, as of the end of 2010, there are 52 dark pools in the U.S. and 36 in Europe, accounting for 12.5% of U.S. and 10% of European volume.[5] More recently, Rosenblatt Securities estimates March 2013 dark pool volume was 14.7% of all U.S. equity volume.[6] Researchers have recently turned their attention to developing theories and documenting empirical regularities about the use of dark pools, determinants of their market shares, and their impact on market quality.

Recent theoretical research analyzes the relative costs and benefits of execution in dark pools. Buti, Rindi & Werner (2011), Degryse, Van Achter & Wuyts (2009), Ye (2011) and Zhu (2011) present models where traders weigh the benefit of reduced price impact in the dark

[5]The Economist, "Off-Exchange Share Trading: Shining Light on Dark Pools," http://www.economist.com/blogs/2011/08/exchange-share-trading.

[6]Bloomberg, "Finra Considering Rule to Require More Dark Pool Data Disclosure," http://www.bloomberg.com/news/2013-05-21/finra-considering-rule-to-require-more-dark-pool-data-disclosure.html

pool against the probability of non-execution. Kratz & Schöneborn (2010) similarly models reduced trading costs and non-execution probability, but also considers adverse selection costs in the dark pool due to information leakage. To our knowledge, no theoretical papers have addressed competition among dark pools or the role of exclusivity in dark pools.

Recent empirical studies examine dark pool trading impacts on market quality and execution quality. Weaver (2011) shows that increased internalization of volume (in dark pools) has resulted in wider spreads and high return volatility, and concludes that increased internal order crossing correlates with degraded market quality. O'Hara & Ye (2011) studies increased market fragmentation, to which dark pools contribute, and finds no harm to market quality. Buti, Rindi & Werner (2010) also finds that dark pools do not decrease market quality. Nimalendran & Ray (2011) provides evidence suggesting that informed traders use dark pools and that their information spills over to other trading venues, increasing quoted spreads. Camerton-Forde & Putniņš (2013) finds that high levels of dark trading (not including block trades) impede price discovery. Conrad, Johnson & Wahal (2003) finds institutional traders' execution costs are lower for dark pool executions relative to traditional broker executions. Bessembinder & Venkataraman (2004) use data from the Paris Bourse and find upstairs markets provide lower execution costs for large trades and provide opportunities for institutional traders to "tap into pools of 'hidden' or 'unexpressed' liquidity." Næs & Ødegaard (2006) highlight the substantial cost of trading failures (non-execution) in crossing networks by utilizing order data similarly to Conrad, Johnson & Wahal (2003).

Our analysis extends the dark pool literature by emphasizing heterogeneity among dark pools, particularly restrictions on who can participate. Due to data limitations, many empirical studies treat dark pools as homogeneous. Neither O'Hara & Ye (2011) nor Buti, Rindi & Werner (2010) appear to have venue-specific data. Ye (2010) combines data from eight dark pools to examine execution rates. Nimalendran & Ray (2011), using a single firm's data, documents differences in the information content of trade on the firm's crossing network and its upstairs desk.[7] Ready (2010) analyzes aggregate quarterly data to examine determinants of dark pool volume.

Treating dark pool volume as a single homogeneous sample can be problematic if there are important differences in dark pool structures. Butler (2007) classifies 24 U.S. equity dark pools in 2007 according to three characteristics: pricing, order types, and counterparties. Sixteen unique classifications for only 24 dark pools demonstrate their heterogeneity. In addition to dark pools that target buy-side institutional investors, large brokerages provide internal dark pools that Weaver (2011) suggests constitute more than 75% of dark pool order flow. Credit Suisse Crossfinder, UBS PIN, and Goldman Sachs Sigma X are examples (Mittal 2008). Domowitz, Finkelshteyn & Yegerman (2009) suggests that differences in dark pools lead to large differences in execution quality, and that part of the variation in performance may be due to the trading clienteles permitted to use the venue.

3 Hypothesis Development

A trading venue's rules and policies have the potential to influence the composition or behavior of its trading population. We consider venues with rules and policies intended to attract those with natural trading interest seeking to avoid potential losses from others' learning about their trade intentions. Venues that somehow successfully limit the flow of such information, or the exploitation thereof, may be able to reduce the execution footprint, increase overall execution quality, and facilitate less disruptive transfer of large volumes. Such venues may, as a consequence, attract repeat business.[8]

Structuring a venue to attract natural contra-side trading interests involves limitations on the protections offered. Traders, particularly those who execute large orders and have

[7] Because the firm is not identified, we do not know what restrictions, if any, are placed on the trading population for either of the two trading mechanisms.

[8] That certain types of traders might want to concentrate in a particular venue to provide for credible protected clustering is not a new insight. For example Admati & Pfleiderer (1988) remark that "It is intuitive that, to the extent that liquidity traders have discretion over when they trade, they prefer to trade when the market is 'thick' - that is, when their trading has little effect on prices. This creates strong incentives for liquidity traders to trade together and for trading to be concentrated."

more to trade, might prefer to keep all information that they have traded private. SEC rules require, however, that trades be publicly disclosed in a timely manner. Therefore venue rules to maintain total privacy of post-trade information are not an option, even though they might be highly attractive to natural contra-side traders.

Dark pool venues have options for designing rules to protect information pre-trade. Through an SEC exemption, dark pools avoid public order display and thereby limit, but possibly do not eliminate, the dissemination of pre-trade order information (Mittal 2008). Beyond this starting point, dark pools seeking to offer additional protection can design trading rules, policies and enforcement aimed at attracting desired traders. As a complement to the protection afforded by permitted darkness, venues can seek a trading population disinclined to exploit order flow information.

Buy-side institutional investors are often thought of as one such population. These traders are caricatured as having little tolerance or incentive to engage in gaming by trading opposite their intended net position. That is, if they were to encounter order flow information related to a counterparty's desire to trade more, they would be reluctant to switch sides to front-run. If, by using trading rules, policies and enforcement, one could create a dark pool environment that would attract a preponderance of such disinclined traders, then that pool might justifiably claim to have created a safer trading environment for natural contra-side trading interests.[9]

Alternatively, a dark pool's enforcement mechanisms could discourage the exploitation of order flow information and similarly create a safer trading environment for buy-side traders. Mittal (2008) states that many dark pools "make trading safe by various policing controls." The potential loss of trading opportunities from being excluded from a venue due to such policing could lead participants to be less inclined to exploit order flow information and,

[9]For the motivation behind our empirical investigation, all we need is that an allegedly exclusive venue's espoused rules, policies and enforcement could lead to a relatively higher proportion of trades taking place between those disinclined to exploit other traders' order flow information *or* that the venue's rules, policies and enforcement inhibit the exploitation of order flow information (in the venue and otherwise). We are not aware of any venue, dark or otherwise, claiming to have successfully restricted the trading population or behavior to completely eliminate the exploitation of order flow information.

in the process, concentrate natural trading. Whether through enforcement mechanisms or attracting a specific trading population, exclusivity can result in a venue's providing a smaller execution footprint and higher overall execution quality.

If a dark pool's design features succeed in reducing the exploitation of order flow information, we should observe systematic differences in execution footprint measures relevant to the targeted dark pool customers.[10] We consider return correlations, and volume and volatility increases related to large trades. For such measures, we contrast a dark pool specifically targeting buy-side traders with the aggregation of other TAQ-reporting dark pools.[11]

More specifically, if a dark pool's rules and policies have the desired effect, one would expect less magnitude in any correlation between returns in intervals before and after a large transaction: (i) negative correlation would suggest related temporary price pressure in other markets (since the dark pool transaction is typically between the bid and ask prevailing elsewhere); (ii) positive correlation would suggest the possibility that the dark pool transaction supplemented or reacted to price pressure that did not dissipate after the dark pool transaction printed. High magnitudes of such correlations would most likely be considered undesirable by buy-side institutional traders seeking to complete their trading objectives without having other market intermediaries exacerbate the task.[12] Other things being equal,

Hypothesis 1. *Around large trades, more exclusive trading venues should exhibit smaller magnitudes of serial correlation in returns than exhibited by other dark pools.*

[10] Butler (2007) states that "Experience has shown that not all ATSs are the same. Because of its specific order types, constituents, and mechanics, any given ATS may be more or less prone to various forms of market impact or information leakage. This is commonly lumped together as 'gaming' and often comes in the form of predatory traders who seek out orders in ATSs to 'game' them for maximum advantage." Mittal (2008) states that "If there is one thing we can emphasize, it is that all dark pools are different. Yet there is massive push by broker dealers selling dark pool aggregators and algorithms to ignore that fact and push the focus on the fill rate. ... A dark pool's quality directly reflects that of the players in it. Information leakage is less likely to occur where constituents are less likely to benefit, therefore institutions with 'natural' liquidity sit at the top of the quality pyramid. ... So, if you can, it is worth finding out about the types and concentration of constituents in each dark pool."

[11] We recognize that the aggregate of other dark pools may include other venues designed to attract primarily buy-side traders and may do so effectively. We make no claims regarding the performance of the dark pool from which we have transaction data relative to any other specific venue (for which we do not have venue-specific data).

[12] A contributing factor, particularly undesirable to the buy-side traders, would be the use of other venues to exploit buy-siders' order flow information related to a large dark pool execution, e.g. see Mittal (2008).

More exclusive dark pools that successfully mitigate order-flow free-riding, other things being equal, should experience relative quiet in trading prior to the execution of a large trade. Undesirable order-flow information leakage and front-running can contribute to increases in aggregate market volume and price volatility prior to a large trade.[13] If a dark pool's design mitigates this contributing factor, other things being equal,

Hypothesis 2. *Prior to a large trade execution, more exclusive trading venues should exhibit less volume and volatility increase than exhibited by other dark pools.*

As a venue may influence the composition or behavior of its trading population to provide benefits such as a smaller execution footprint, it is possible that providing such benefits is accompanied by additional costs. For instance, restricting the trading population or increasing enforcement may cause a reduction in available trading parties and a reduced probability of execution. Reduced probability of execution can then be associated with a higher opportunity cost when submitting orders to that venue. It is also possible that higher overall execution quality could be associated with higher fees as monitoring costs to create a protected venue are passed on to customers. The key point is that execution footprint represents only one component of the vector of trading costs and benefits associated with any venue. Smaller execution footprints need not in isolation indicate higher overall execution quality at that venue.

We investigate the overall benefit of exclusivity by examining patterns in trade clustering within and across days. To do so, we test the null hypothesis that the trading venues do not differ in the overall execution quality they provide. If this is true, then there should not be predictable differences in trading patterns among the venues. For instance, if a smaller execution footprint at the exclusive venue is accompanied by offsetting, higher unobservable (to the researcher) costs (such as a higher probability of non-execution), then there is no

[13] Although our study of return correlations involves post-trade data, our current investigation does so to focus on changes from the pre-trade period. More generally, we focus on pre-trade rather than post-trade regularities to avoid introducing post-print problems due to the dark pools' obligation to publicly disseminate trade prints expeditiously. Unfortunately, after other markets see those prints, they will most likely respond thereby contaminating clean post-trade-print implications for volume and volatility.

reason to allocate orders between venues in a systematic manner. Under this hypothesis, observing systematic patterns in trading volume is inconsistent with equal overall execution quality across venues.

We first compare intra-day trade clustering between the exclusive venue and other dark pools. If overall execution quality motivates routing decisions among venues, and overall execution quality is constant across venues, then

Hypothesis 3. *More exclusive trading venues should exhibit the same intra-day trade clustering (or lack thereof) as that exhibited by other dark pools.*

Rejecting Hypothesis 3 will not provide direct support for superior overall execution quality at the exclusive venue. It is possible that traders cluster at different dark venues at different points throughout the day and that while clustered at a specific venue, that venue offers superior overall execution quality. Such a regularity across hours of the day at different venues would not suggest that a single dark venue offered superior overall execution quality *throughout* the day. It would, however, still suggest that there is some expected overall execution quality benefit to trading at a specific venue during its dense trading period. Further, if that density is early in the trading day, it would be consistent with traders' expecting superior overall execution benefits while managing the costs associated with possible non-execution.[14]

We examine trade clustering *across* days to avoid contamination from within-day variation in unobservable (to the researcher) costs. Clustering across days may be particularly relevant to institutional traders due to their large trading demands, which often take multiple days to fill. Satisfactory dark pool trades for such institutions can lead to repeat dark pool business in the same security over the span of the trading program.[15] While trade satisfaction most likely includes some measure(s) of execution footprint, it is also likely that unobservable (to the researcher) factors influence satisfaction. If overall trade satisfaction

[14] This notion is consistent with the theories of Buti, Rindi & Werner (2011), Degryse, Van Achter & Wuyts (2009), Kratz & Schöneborn (2010), Ye (2011) and Zhu (2011).

[15] Others have argued that trade execution satisfaction can lead to clustering in transactions. For example, see Ye (2011) and Degryse, Van Achter & Wuyts (2009). Similarly, traders in a multiday program that are dissatisfied with a dark pool transaction will likely be reluctant to rely on that dark pool going forward.

is a reflection of overall execution quality, and overall execution quality is constant across venues, then

Hypothesis 4. *More exclusive trading venues should exhibit the same inter-day trade clustering (or lack thereof) as that exhibited by other dark pools.*

However, if a venue does provide superior overall execution quality, then we would likely find a higher degree of trade clustering at the superior venue, leading us to reject Hypothesis 4. If differential trade clustering indicates the superiority of a venue, it is also likely that the superior venue "leads" other venues in volume execution. Leadership of one venue suggests that trading demand is first routed to that venue and then migrates to other venues as trading opportunities at the superior venue diminish.

While clustering is consistent with expected superior overall execution quality attracting traders, random clustering of traders may also result in superior overall execution quality. By decreasing the probability of non-execution, clustering may lower the costs at a venue, leading to better overall execution quality for participants. Regardless of the direction of causality, an exclusive venue's demonstrating more clustering than other dark pools is consistent with the broad notion that exclusivity matters.

To investigate these hypotheses, we compare data originating in a dark pool targeting buy-side institutional investors to data for executions originating in the universe of other dark venues that report trades in the TAQ data.

4 Data and Descriptive Statistics

4.1 Data Collection

In order to investigate the possibility that targeting traders with low proclivity for gaming fosters superior large trade execution quality, we employ transactions data from Liquidnet Classic, a dark pool having rules, policies and enforcement intended to concentrate participation among buy-side institutional investors.[16] Our sample includes 62 days of transactions between January 3, 2011 and March 31, 2011. TAQ data allow us to identify large transactions across dark and order-displaying venues, and CRSP and COMPUSTAT data identify the related average daily volumes, market capitalizations and exchange listings.

Following O'Hara & Ye (2011) and Boehmer (2005), we restrict attention to NYSE- and NASDAQ-listed common stock of US companies that are not closed-end funds, REITs, ETFs or carrying dual class common stock. We further require that stocks in our sample have a minimum daily volume of at least 1,000 shares throughout the sample period, and that the closing price as of the end of 2010 is at least \$5.[17] The resulting sample includes 1,694 firms. Table 1 details the sample selection criteria.

Our analysis focuses on trade executions of at least 50,000 shares. While 50,000 shares is arbitrary, we have applied this restriction due to the SEC's (and our) interest in large trade executions.[18] We eliminate from consideration large trades occurring outside of normal trading hours or under the opening or closing cross. We classify trades using the TAQ exchange code. Dark pool trades, including those at Liquidnet Classic, are reported as "D", a code used for the Trade Reporting Facility, and for the NASD Alternative Display Facility. Those facilities include transactions from over-the-counter markets, some non-exchange ECNs, and dark pools.

For our universe of dark pool trades, we classify any trade of at least 50,000 shares with an execution code of "D" as a dark pool trade.[19] We use Liquidnet disclosure to separate all

[16] Appendix A provides additional institutional details. While trading venues vary across many dimensions, Liquidnet and the financial press emphasize buy-side orientation as a critical difference from other trading venues. For example, Liquidnet's website states: "Liquidnet's natural liquidity pool is where the world's buy side meets to trade blocks. We directly link buy-side traders to buy-side traders, keeping information leakage to a minimum and removing the need to slice and dice your order amongst multiple venues to find liquidity." (http://www.liquidnet.com/products/negotiation.html) Additionally, Alpha Trader Forum states: "By bringing together institutional buyers and sellers of large blocks of equity securities, Liquidnet enables them to trade with each other directly and anonymously via the Internet on our electronic trading platform." (http://buysideforum.com/liquidnet) We focus our discussion on differences in exclusivity, but recognize that other differences may contribute to our results.

[17] The restriction to stocks greater than \$5 eliminates only 3% of Liquidnet Classic trades from the sample.

[18] While SEC comments suggest a \$200,000 minimum transaction size, our sample transactions are intended to exceed \$250,000 = 50,000(\$5) with some buffer to accommodate the possibility of prices lower than \$5 during the sample period.

[19] In Weaver (2011), trades coded "D" proxy for internalized trades. The distinction between studies is that

dark pool trades into Liquidnet Classic and other allegedly "less-exclusive" dark pool trades, allowing us to compare trades known to have taken place on a venue designed for buy-side exclusivity with trades from the universe of other dark pools.[20] Our sample of 61,158 large trades is detailed in Table 2.

To test Hypotheses 1 and 2, we calculate pre-trade and post-trade return, volume and trading range using time intervals surrounding each execution. To be included in our reported statistics, we require evidence of continuing trading interest at any venue in the intervals before and after a large trade.[21] Our presented statistics are from a sample winsorized at the 1% and 99% levels, although our results are qualitatively similar without winsorization.[22]

While many studies utilize traditional microstructure measures of market quality, we focus solely on dark pool executions and therefore avoid quote-based and trade-sign-based measures. Realized and effective spread, as well as price impact, rely on assigning trades as either buyer or seller initiated. The nature of dark pools prevents the clear signing of trades for two reasons. First, unlike with visible limit order books, it is not known *ex-ante* whether a dark pool order will take or provide liquidity when it is submitted. A match only occurs when both parties are, in a sense, willing to provide liquidity.[23] Therefore, assigning one side as the trade initiator is misleading. Second, the ability of dark pools to delay reporting prevents researchers from aligning dark pool executions with the prevailing quote at the actual time of executions.[24] We find that roughly 94% of Liquidnet Classic trades occur at

internalized volume is mostly retail order flow and is not likely to be of the large trade sizes we consider.

[20]To the extent this "less-exclusive" classification includes executions from dark pools that, like Liquidnet Classic, attempt to limit access to buy-side institutional investors, our analysis should be less likely to find differences for this classification when compared with Liquidnet Classic. This suggests our results that exclusivity matters would be even stronger were we able to partition further venues within the less-exclusive classification.

[21]Specifically, we require that transactions take place in fifteen out of twenty 30-second intervals before and after the large trade. Alternatively, requiring zero, five, or ten out of twenty intervals gives qualitatively similar results.

[22]An alternative sample formulation also gives qualitatively similar results to those presented in this paper. The alternative sample includes only large trades that are less likely to be contaminated by a lack of independence due to clustered trades in the same security. Inclusion in this clean subsample requires that no other large trade occur in that security within the five minutes preceding or following the trade.

[23]While certain order types, such as immediate-or-cancel limit orders, give a flavor of taking liquidity from dark pools, the large trades we consider are more likely to result from passive, resting limit orders.

[24]Dark pools that report through ADF and TRF must report trades within 30 seconds.
http://www.finra.org/web/groups/industry/@ip/@reg/@notice/documents/notices/p121343.pdf

or within the NBBO, while only 79% of other dark pool trades occur at or within the NBBO. This difference, and the ability to time trade announcement strategically, makes it difficult to employ observable data to sign dark pool trades reliably.

4.2 Descriptive Statistics

Table 2 details the characteristics of our sample and sample terciles based on average daily volume (ADV) and trade difficulty. ADV is determined by using trading volumes from December 2010. We define trade difficulty as the trade size divided by the ADV of the security. For any given security, all large trades of that security are in the same ADV tercile, but are not necessarily in the same trade difficulty tercile (as the trade size varies but ADV remains constant).

The descriptive statistics in Table 2 indicate systematic differences between large trades executed at Liquidnet Classic and those executed at other dark pools. Liquidnet Classic executes 9% of all large trades and the remaining 91% are executed at other dark pools. Our proxy for trade difficulty, trade size divided by ADV, is significantly higher for dark pool executions at Liquidnet Classic (12.4% versus 6.8% for other dark pools).[25] Panel B shows Liquidnet Classic's market share of large trades is greater for more difficult trades (16% in the highest difficulty tercile compared to 7% in the middle tercile). This suggests that Liquidnet Classic is executing more difficult trades on average. Panel C shows Liquidnet Classic executes a relatively higher percentage of the large trades in low volume stocks as compared to high volume stocks (16% market share versus 3% market share).

4.3 Matched Firms

Rather than viewing market quality changes around a large trade as detached from the trading environment, it is appropriate to control for that environment. For example, if we observe a volume run-up in a given security prior to a large trade, it could be simply

[25] Liquidnet Classic trades are significantly more difficult than other dark pool trades, at the 1% level, for the full sample, low and middle trade difficulty terciles, and middle and high ADV terciles.

a reflection of market-wide activity. To avoid potential for misattribution, we control for contemporaneous market conditions by using a matched firm approach. Following Davies & Kim (2008), we match firms based on listing exchange, market capitalization and price.[26] We require matched firms to be listed on the same exchange and we minimize the deviation given by

$$D_{ij} = abs\left(\frac{MktCap_i}{MktCap_j} - 1\right) + abs\left(\frac{Price_i}{Price_j} - 1\right) \tag{1}$$

where i indexes potential matches and j indexes the security in our large trade sample.

In Section 5, we report tables and results using the above matching procedure. To ascertain robustness with respect to our matching procedure, we obtain a second and third match for each target firm. The second match is obtained using the Davies & Kim (2008) methodology, and the second-best minimizer of the quantity defined in Equation (1). The third match is a broad market match, proxied by an S&P 500 Index ETF.

5 Empirical Methods and Results

5.1 Tests for Serial Correlation in Returns

In order to test Hypothesis 1, we analyze the relationship between abnormal returns immediately before and after large trades. Abnormal returns are calculated as the difference between the return of the security and the return of the matched security over the same period. Lack of correlation between abnormal returns is consistent with a smaller execution footprint for buy-side institutional traders, while positive or negative correlations can be viewed as harmful to their trading programs. Figures 1 and 2 display scatter plots of the pre-trade and post-trade abnormal one-minute returns for trades at Liquidnet Classic and other dark pools, respectively. Visual inspection, and the superimposed univariate linear regression lines, suggest slight negative correlation at other dark pools (treated as a whole),

[26] We use pre-sample levels prevailing at the end of December 2010.

but slight positive correlation at Liquidnet Classic (our proxy for a more exclusive dark pool venue). Kolmogorov-Smirnov tests reject the null hypothesis that the OLS residuals in the graphically-inspired linear regressions are normally distributed. Rather than proceed with some form of robust regression, we adopt an approach that combines observed match-adjusted returns from before and after a large trade and proceed nonparametrically.[27] Specifically, we construct the following ratio of returns that facilitates consideration of positive or negative correlation:

$$ReturnRatio_i = \frac{(PostReturn_{i,test} - PostReturn_{i,control})}{(PreReturn_{i,test} - PreReturn_{i,control})} \tag{2}$$

where i indexes trades, *test* indicates the return of the security with the large trade and *control* indicates the return for the matched security. Positive values of the return ratio reflect positive serial return correlation and come from either consecutive positive or consecutive negative returns. Negative return ratios result from either a positive return followed by a negative return, or a negative return followed by a positive return, and therefore reflect negative serial correlation in returns.

A ratio of returns conveniently captures these categories of theoretically interesting covariations by combining two returns into a single quantity amenable to univariate statistical tests. However, the ratio's disadvantage is that it introduces potentially large nonlinear transformations when pre-trade returns are close to zero.[28] In response, we consider nonparametric statistics emphasizing signs and ranks rather than magnitudes of the return ratio.[29] We conduct traditional sign tests for non-zero median return ratios. To compare return ratios from potentially different populations, we employ Wilcoxon rank-sum tests. In

[27] Spearman correlation coefficients for the pre- and post-trade returns of Liquidnet Classic trades and other dark pool trades are qualitatively similar, in both direction and significance, to the nonparametric results we present. However, the correlation coefficients for the two groups are not easily comparable, so we rely on our nonparametric tests for comparison.

[28] Any trade with an abnormal return of zero in the pre-trade period has an undefined (infinite) return ratio and is omitted from the sample.

[29] In an alternative, but related, approach we could have applied a sign transformation to each return (yielding -1, 0 or +1) and then performed statistical analysis on the transformed data. Both approaches are inherently sign-based and lead to similar qualitative inferences.

both cases, rejection of the null hypothesis suggests that return correlations are significantly different from zero (sign test) or each other (rank-sum test).[30]

Regarding the sign test to detect serial correlation, the test statistic of interest is the M-sign, the number of positive return ratios less the number of negative return ratios divided by 2. Table 3 displays M-signs for each type of venue.[31] Panel A shows that large trades at other dark pools reflect significantly negative median return ratios. Despite the low power associated with the sign test, the test statistic (-1297.5 in the total sample) is significant at the 1% level. In contrast, the M-sign for Liquidnet Classic trades (-32.5 in the total sample) is not significantly different from zero. These results suggest that in the other dark pools there is a preponderance of negative return ratios, consistent with large trades' being in the middle of a price reversal. For the Liquidnet Classic sample, the absence of a preponderance of negative or positive co-movements is consistent with either the absence of serial correlation or the nearly perfect balancing of positive and negative co-movements. In the volume and volatility increase tests that follow, we provide additional evidence that the Liquidnet Classic return ratio's insignificance (lack of difference from zero) is most likely due to the absence of serial correlation.

It is possible that Liquidnet Classic demonstrates smaller execution footprints due to traders' utilizing that venue for easier-to-execute trades. If this were the case, one might also expect Liquidnet Classic to derive that smaller execution footprint from the subset of trades considered to be easier to execute. Analysis of large trades by trade difficulty and ADV terciles supports the results from our full-sample tests. Panels B and C of Table 3 show

[30] If the expected return ratio is zero, the sign test's test statistic can be easily interpreted. We recognize, however, that sampling error or non-zero expected returns can lead to systematic biases in the return ratio, and that such biases can also affect the interpretation of Wilcoxon rank-sum test statistics. Accordingly, we test the robustness of our results by bootstrapping the distributions of the test statistics using the sampled pre-trade and post-trade returns. Inferences are qualitatively unchanged using the bootstrapped distributions.

[31] In previous versions, we have presented analyses of order-displaying venue trades and their differences from Liquidnet Classic trades. While order-displaying venues are generally open to the public, it is not clear that large trades printed at these venues occur between public participants through trading in the limit-order book. We believe these trades occur in a number of ways, and therefore we cannot reliably judge the exclusivity of these venues. For this reason, we do not consider comparisons to order-displaying venue trades as helpful to an analysis of the impact of exclusivity on execution footprint or overall execution quality. While not included, the new analyses presented in this version produce qualitatively similar results as those previously presented.

that other dark pool trades consistently exhibit return ratio distributions having significant negative correlation. As in the full sample, examining trades at Liquidnet Classic by tercile shows no evidence of either a preponderance of positive or negative return ratios.

We next test for significant differences between large trade return ratios at Liquidnet Classic and those at other dark pools. Return ratios at Liquidnet Classic are more consistently smaller in magnitude than those at other dark pools; we reject equality at the 1% significance level. Tercile-level analysis supports these results in direction, but statistical significance suffers in the smaller samples. Overall, the segment results provide additional confidence that no single tercile is driving the general result that return serial correlations around large trades at Liquidnet Classic exhibit less net negativity than other dark pools.

To establish robustness in our results, we replicate the analysis using our two alternative matches and using an alternative method for calculating returns.[32] All results using these alternative methods are qualitatively similar to those reported herein.

Liquidnet Classic trade return ratios suggest significantly less correlation between pre-trade and post-trade returns, demonstrating one way exclusivity differences coincide with differences in trade experiences. From the combination of tests, the data appear to support Hypothesis 1 that a more exclusive dark pool shows significantly less correlation in returns surrounding large trades.

5.2 Pre-Trade Volume and Volatility Increases

We hypothesize that pre-trade volume run-ups and volatility increases prior to large trades are significantly lower at an exclusive dark pool relative to other dark pools. Volume run-ups and volatility increases prior to large trade executions are consistent with larger execution footprint, possibly due to order-flow-information leakage and related front-running. We test

[32]The results presented herein are based on returns calculated using prices determined by the last TAQ trade prior to the start of a time interval. For example, if the last trade before 10:00 AM is a trade made at $10.01, then the price as of 10:00 AM is recorded as $10.01. The alternative method uses the first trade price after the start of a time interval. Continuing the example, if the next trade occurs at 10:00:05 AM at a price of $10.03, then the reported 10:00 AM price would be $10.03.

for volume increases by measuring the amount of volume traded in the minutes prior to large trades and then comparing the increases in volume traded per minute between trading venues. We proxy for volatility by measuring the trade price range (highest reported trade price minus lowest reported trade price, excluding the large trade itself) in the minutes prior to large trades and then testing for differences between the increases in range prior to large trades at different venues.

The continuity of the volume and range increase measures allows us to test for differences between venue types using several methods. As a first pass, we undertake a non-parametric estimation approach using matched firms to control for market conditions and stock characteristics, as in the previous section. We then follow the methods used in Bessembinder (2003) to control more directly for market conditions and stock characteristics, and to control for the possibility of selection bias based on observable characteristics. Specifically, we estimate the volume and volatility increases using a linear regression framework and a Heckman (1979)-like two-stage procedure.

In our non-parametric estimation, we use abnormal, rather than absolute, volume measures to account for market conditions at the time of the large trades. We calculate the abnormal volume increase by first normalizing volume using the prior period's volume in the same security. We then subtract the normalized measure from the matched firm to capture any abnormal volume increase. Specifically, the abnormal volume increase measure is given by:

$$VolumeIncrease_i(r,s,t) = \frac{TotalVolume^i_{(s,t)}}{TotalVolume^i_{(r,s)}} - \frac{TotalVolume^{Match(i)}_{(s,t)}}{TotalVolume^{Match(i)}_{(r,s)}} \tag{3}$$

where t is the trade time, $TotalVolume^i_{(r,s)}$ is the volume in stock i from r minutes prior to the trade to s minutes prior to the trade, and $TotalVolume^{Match(i)}_{(s,t)}$ is the volume for the stock matched to i from s minutes prior to the trade to the time of the trade. To estimate this model, the volume in the minute prior to the large trade is normalized by the volume in the prior minute (from two minutes before the trade to one minute before the trade). Any trade with no volume in either pre-trade period is omitted from the sign and Wilcoxon rank-sum tests. This method has the benefit of naturally controlling for volume level by testing for abnormal volume increases on a percentage rather than an absolute basis. We use non-parametric methods to test for median abnormal volume increases and for differences in the median increases between trading venues.

Panel A of Table 4 shows that there are significant (at the 1% level) volume increases prior to large trades for Liquidnet Classic and other dark pools. However, the Wilcoxon rank-sum tests provide evidence that the differences in volume increase are significant. The volume increases prior to large trades are significantly greater at other dark pools (p-value 0.056) as compared to Liquidnet Classic. As in the tests of serial correlation of returns, the results from studying the sample in terciles (shown in Panels B and C) are qualitatively similar, but suffer from a lack of power. Using the other two matching procedures does not qualitatively change our results. We conclude that large trades at Liquidnet Classic experience less pre-trade volume increase. This is consistent with either lower order-flow information leakage, or a trading population less inclined to front-run using leaked information at a dark pool designed and marketed to the buy-side.

Following a similar methodology, we test for volatility increases before large trade executions. Our volatility increase measure is given by:

$$VolatilityIncrease_i(r,s,t) = \frac{Range^i_{(s,t)}}{Range^i_{(r,s)}} - \frac{Range^{Match(i)}_{(s,t)}}{Range^{Match(i)}_{(r,s)}} \tag{4}$$

where t is the trade time, $Range^i_{(r,s)}$ is the trade price range in stock i from r minutes prior to the trade to s minutes prior to the trade, and $Range^{Match(i)}_{(s,t)}$ is the trade price range for the stock matched to i from s minutes prior to the trade to the time of the trade. We normalize the volatility around large trade executions by using the trade price range from two minutes prior to one minute prior to the trade. Any trade with no trade range in either pre-trade period is omitted from tests.

Table 5 shows that volatility increases prior to large trades at the less-exclusive dark pools. Panels A, B and C show that this volatility increase is significant at the aggregate level and across all ADV and trade difficulty terciles. Large trades occurring at Liquidnet Classic do not experience significant volatility increases prior to execution. Wilcoxon rank-sum tests confirm that the difference in volatility increase is significant (p-value 0.005). Using non-parametric methods, we reject the null hypothesis that there is no volatility increase prior to large trades at other dark pools, but we fail to reject the null hypothesis for large trades at Liquidnet Classic.

It is possible that our matching methodology is too crude to control effectively for market conditions and stock characteristics. We examine this possibility, and the possibility that selection bias accounts for the differences in execution footprint, by following the methods of Bessembinder (2003). Bessembinder (2003) first advocates using multiple regression with exogenous variables to control for possible selection bias related to observable variables. In our context, if trades with smaller expected execution footprint are being routed to Liquidnet more often than other dark pools, then a difference in execution footprint can arise due to this potential selection bias.

To verify that our results are not due to this form of selection bias, we regress the natural log of our volume and range increase measures on a vector of mean-adjusted control variables and an indicator variable for the exclusive venue.[33] Formally, we estimate

$$ln(IncreaseMeasure_i) = \beta_0 X_i + \beta_1 \mathbb{1}_{Liquidnet,i} + \epsilon_i. \tag{5}$$

The vector of control variables X_i includes stock specific characteristics including the ADV, price and market capitalization as of December, 2010, and an indicator variable for the listing exchange (1 = NYSE). We also include controls for the current market conditions

[33]We use unadjusted execution footprint measures as stock and market characteristics are included as control variables. Using abnormal execution footprint measures (adjusted by using matched firms) does not qualitatively change our results.

including: the time of day in seconds, the average bid-ask spread, the number of trades, the total trading volume and the trading range (high minus low transaction price) over the five minutes prior to the trade, as well as the squared five-minute-measures and a measure of trade difficulty. Table 6 presents the results of these regressions.

The smaller execution footprint observed for exclusive dark pool trades does not appear to be due to a simple selection bias from order routing decisions. The first column of Table 6 shows that a univariate linear regression provides qualitatively similar results to our non-parametric results. Without considering stock and market characteristics, the average trade at Liquidnet experiences 11% less volume increase than the average trade at other dark pools. The second column shows that including control variables does not significantly alter the univariate result. Columns three and four show the regression results explaining our range increase measure. In both specifications, the volatility increase is lower for trades at Liquidnet Classic relative to other dark pool trades, by approximately 4.5%. Once again, adding control variables does not change our results significantly, indicating that a simple selection bias is not likely responsible for the observed differences in execution footprint.

To further confirm that a selection bias is not the likely cause of the observed difference in execution footprint, we use a two-stage procedure as advocated by Heckman (1979) and Maddala (1983). We first use a probit model to predict trade location. This allows us to study what drives the routing decision between Liquidnet and other dark pools. Rather than using market condition measures based on five minutes prior to the trade, we instead use measures over 60-minutes prior to the trade. We use longer-timed measures as we conjecture that traders expect to wait some time for a counterparty within a dark pool, so their dark pool routing decisions are less short-term oriented.[34] Our probit regression specification is

$$L_i = \Gamma_0 Y_i + \eta_i \tag{6}$$

[34]Results are qualitatively unchanged if the estimation is done using the short-term measures.

where Y is the vector of control variables and L is the binary variable $\mathbb{1}_{Liquidnet}$ given by

$$\mathbb{1}_{Liquidnet,i} = \begin{cases} 1 & \text{if } L_i > 0 \\ 0 & \text{if } L_i \leq 0 \end{cases} \tag{7}$$

and $\mathbb{1}_{Liquidnet} = 1$ if an execution occurred at Liquidnet Classic and 0 otherwise. Table 7 presents the results of estimating this probit regression in the first and third columns. The estimation varies between columns one and three due to sample size differences resulting from the requirements of non-zero volumes and non-zero ranges in constructing the execution footprint measures used as dependent variables in the second stages. Trades routed to Liquidnet Classic have higher bid-ask spreads and volatility, have lower volumes, occur earlier in the day, and are more likely to be NASDAQ listed.

Following Maddala (1983) (and Bessembinder (2003)), we construct two new variables to control for possible selection bias in venue:

$$\gamma_{1,i} = \frac{\phi(\Gamma_0 Y_i)}{1 - \Phi(\Gamma_0 Y_i)} \tag{8}$$

$$\gamma_{2,i} = \frac{-\phi(\Gamma_0 Y_i)}{\Phi(\Gamma_0 Y_i)} \tag{9}$$

where $\Gamma_0 Y_i$ gives the fitted value from estimating Equation (6), ϕ is the standard normal probability density function and Φ is the standard normal cumulative density function. γ_1 is the traditional Inverse Mills Ratio and γ_2 is the analogous measure for the portion of the sample that is not selected in the first-stage probit model (not Liquidnet Classic). We then include both new variables, interacted with their respective indicator variables, into Equation (5) giving a regression specification with explicit selection bias controls:

$$\begin{aligned} ln(IncreaseMeasure_i) &= \beta_0 X_i + \beta_1 \mathbb{1}_{Liquidnet,i} + \beta_2 \gamma_{1,i} \mathbb{1}_{Liquidnet,i} \\ &\quad + \beta_3 \gamma_{2,i} (1 - \mathbb{1}_{Liquidnet,i}) + \zeta_i. \end{aligned} \tag{10}$$

Table 7 shows the results from estimating Equation (10) for both volume and volatility increases. Consistent with our non-parametric and single-stage linear regression results, we find a significant difference in execution footprint between Liquidnet Classic and other dark pools. Liquidnet Classic experiences 9.5% less volume increase and 5% less range increase compared to the other dark pools, which is qualitatively and quantitatively similar to the single-stage linear regression results, with and without control variables. The two variables included to control for selection bias are not statistically significant in any of the regressions, providing additional confidence that an empirically predictable (by the trading public) selection bias is not driving the differences in execution footprint.[35] These tests suggest that our main results are not driven by higher *ex-ante* execution footprint trades' being routed to Liquidnet.

Regression results incorporating controls and multi-stage estimation to deal with predictable selection bias reinforce our early matched-sample findings that exclusive venues can offer smaller execution footprints. These results support Hypothesis 2 and are consistent with less order-flow-information leakage and front-running at more exclusive dark pools.

5.3 Trade Clustering Within Days

We hypothesize that large trades at more exclusive dark pools have smaller execution footprints. It is not clear, however, whether smaller execution footprints are a compensation for some relatively high cost we do not observe in our transaction data. Under a null that Liquidnet Classic is no different from other dark trading venues, we would expect to see a similar distribution of trades throughout the day at Liquidnet Classic and other dark venues. However, systematic differences in trade timing (clustering within the day) would be consistent with differences in overall execution quality. In particular, one venue's executing trades earlier in the day is, at the very least, consistent with better overall execution quality.

We analyze trade timing by first examining the proportion of large trades that are trans-

[35] Lacking dark pool order data is not likely to affect our ability to control for selection in routing decisions as such data is not observable to dark pool participants.

acted in each 30-minute period of the trading day for each class of venue. Figure 3 shows that Liquidnet Classic's intra-day trading pattern presents distinctly from that of other dark venues taken as a whole and from order-displaying venues (also taken as a whole). The traditional "U"-shaped pattern is observed for both order-displaying venues and other dark pools. Liquidnet Classic's trades, however, exhibit a decreasing pattern throughout the day, suggesting a fundamental difference in trading behavior. Liquidnet Classic's transaction pattern is consistent with anecdotal evidence that institutional traders split their orders, allocating a portion to more exclusive dark pools and the rest to trading strategies that have higher execution probabilities. As the day progresses, trades not executed in the exclusive dark pools may be shifted to the other strategies.

To test formally for differences in trade timing between Liquidnet Classic and other dark pools, we compare the median timing of executions between venues. We order all dark pool trades by their timing within the day, and assign ascending rankings to the trades. We then test the null hypothesis that the timing of the median trades at Liquidnet Classic and other dark pools are not different by using a Wilcoxon rank-sum test. Table 8 reports the results for the full sample and by trade difficulty terciles. The median trade at Liquidnet Classic occurs earlier in the day than the median trade at other dark pools. The timing differences in the full sample and across trade difficulty terciles are highly significant (p-values < 0.0001). We therefore reject Hypothesis 3 that intra-day trade timing is equivalent between Liquidnet Classic and other dark pools. This evidence is suggestive of higher overall execution quality at an exclusive dark pool.

5.4 Trade Clustering Across Days

In addition to intra-day clustering's evidence consistent with superior overall execution at Liquidnet Classic, we consider evidence related to trade timing across days. We test for overall execution quality benefits by attempting to reject the null hypothesis that repeat business via follow-on volume (inter-daily trade clustering) is equal between venues. We use a balanced panel dataset of 105,028 stock-day observations, which includes daily observations for each stock with at least one large dark pool trade over the sample period. Each observation in the panel consists of static stock data (ADV, price, exchange listing, market cap) and daily trade data. The daily trade data are counts of the number of large trades per type of trading venue (all dark pools, other dark pools and Liquidnet Classic).

We employ an autocorrelated negative binomial regression predicting the number of daily large trades, $y_{t,i,v}$ (t indexes days, i indexes stocks, and v indexes venues), to examine inter-day clustering. The negative binomial model under the first and second moment restrictions that

$$\begin{aligned} E(y_{t,i,v}|X_{t,i,v}) &= exp(X_{t,i,v} \times \beta) && (11) \\ Var(y_{t,i,v}|X_{t,i,v}) &= exp(X_{t,i,v} \times \beta) + k \times exp(X_{t,i,v} \times \beta)^2 && (12) \end{aligned}$$

is estimated by parametrically maximizing the log-likelihood function

$$\begin{aligned} l_{t,i,v}(\beta, k) &= k^{-1} log\left(\frac{k^{-1}}{k^{-1} + exp(X_{t,i,v} \times \beta)}\right) + y_{t,i,v} \times log\left(\frac{exp(X_{t,i,v} \times \beta)}{k^{-1} + exp(X_{t,i,v} \times \beta)}\right) \\ &\quad + log\left(\Gamma(y_{t,i,v} + k^{-1})/\Gamma(k^{-1})\right) && (13) \end{aligned}$$

over β and k where $\Gamma()$ is the gamma function defined for $r > 0$ by $\Gamma(r) = \int_0^\infty z^{r-1} exp(-z)dz$ (Wooldridge 2002).[36]

We incorporate possible serial correlation in trading by allowing eight lags of the dependent variable to join exchange listing, ADV, year-end price and market capitalization as explanators ($X_{t,i,v}$) of trading frequency. Likelihood ratios from estimates including and excluding the lagged dependent variable provide a statistical test for autocorrelation, and therefore trade clustering, across days. We consider two versions of the dependent variable (trade frequency): (i) Liquidnet Classic Trades; and (ii) All Non-Liquidnet Classic Dark

[36]The coefficient estimate for k is statistically significant in all tests, justifying the use of the negative binomial model versus a Poisson model.

Pool Trades. Table 9 presents the results, although it provides coefficient estimates for only the first four lagged dependent variables. Likelihood ratio tests show statistically significant improvement in model fit for each set of lagged dependent variables. The p-values derived from Chi-Squared test statistics indicate that these lags are important additions in each model, allowing us to reject the null hypothesis that there is no clustering in large trades, for all groups of trades. The coefficients estimated in the model represent the elasticity of the expected number of large trades, i.e. $E(y_{t,i,v}|X_{t,i,v})$, with respect to the explanators. It is hard to interpret the elasticity as trades only occur in a discrete fashion and are not continuous, but the magnitudes of the coefficients are economically significant. For example, a 1% increase in yesterday's number of Liquidnet Classic trades would increase the expectation of the number of Liquidnet Classic trades today by 0.76%.[37]

Trade clustering appears to be more prominent at Liquidnet Classic than at other dark pools. Table 9 shows that the lagged dependent variables predicting large trades at Liquidnet Classic are highly significant and add considerably to the predictive power of the model. The same is true for most other dependent variables; however, the coefficients for Liquidnet Classic trades are the largest in every case. To test formally for differences in clustering, we estimate the clustering of trades at Liquidnet Classic and other dark pools simultaneously.[38] We can then test for the equality of the coefficients of the dependent variables' lagged values by restricting the coefficients to be equal and using a likelihood ratio test to compare the restricted and unrestricted models. Performing this test for each lag of the dependent variables verifies that Liquidnet Classic experiences significantly more clustering than other dark pool venues (χ^2 test statistics are all significant at the 0.01% level except for the sixth lag which is significant at the 5% level). This evidence is consistent with Liquidnet

[37]This effect is economically more significant than other variables' effects. For example (assuming a constant slope), a one-standard deviation increase in the number of yesterday's Liquidnet Classic trades would increase the expected number of today's Liquidnet Classic trades by 0.25, while similar one-standard-deviation increases in ADV, yesterday's other dark pool trades, and market cap results in increased expectations of 0.15, 0.14 and 0.07 trades, respectively.

[38]Results from this estimation are suppressed as the coefficient estimates do not qualitatively differ from columns (1) and (2) of Table 9.

Classic's experiencing more clustering in large trades than other dark pool venues. Our finding of significantly higher clustering at Liquidnet Classic leads us to reject Hypothesis 4 and to infer that traders at the exclusive venue are likely experiencing superior overall execution quality, i.e., net benefits beyond costs we do not observe (such as lower probability of execution or transaction fees).

As a final test, we consider the possibility that Liquidnet Classic trades predict other dark pool trades and vice versa. We include lagged values of Liquidnet Classic trades and other dark pools trades as independent variables and re-estimate the negative binomial regressions. Model restrictions and likelihood ratios tests are used to test for differences in coefficient values. The last two columns of Table 9 show that Liquidnet Classic trades tend to significantly lead other dark pool trades. Column (4) shows that lagged Liquidnet Classic trades have strong predictive power for future other dark pool trades. In fact, lagged Liquidnet Classic trades are equally or more predictive of other dark pool trades as compared to lagged other dark pool trades. At the first lag, the coefficients for past Liquidnet Classic and other dark pool trades are not statistically different. The second and fourth lags are statistically different at the 10% and 5% levels, respectively, and all other lags are statistically significant at the 1% level. This evidence of leadership for Liquidnet Classic is not evident for other dark pool venues. Only the first lag of other dark pool trades is statistically significant for predicting Liquidnet Classic trades, and its economic significance is weak compared to other predictive variables. This evidence provides additional support that Liquidnet Classic provides superior overall execution quality.

While our differential inter-day and intra-day clustering results are consistent with an overall execution quality benefit at an exclusivity-oriented dark venue, our main intent has been to document that exclusivity matters for execution quality. Differential clustering is consistent with exclusivity's having an impact on trade routing and overall execution quality, even if the overall benefit of a more exclusive venue's executions varies within and across days.

6 Conclusion

We document evidence that a dark pool specifically designed to foster buy-side exclusivity exhibits statistical regularities consistent with a smaller execution footprint and higher overall execution quality for large trades. Specifically, our evidence suggests that large trades at that dark pool exhibit patterns consistent with: (i) less serial correlation in returns; (ii) less pre-trade volume and volatility increase; (iii) earlier executions within the trading day; and (iv) more large trade clustering across days. Such indications are consistent with less pre- and post-trade exploitation of order-flow information in a dark pool designed to foster buy-side exclusivity, and are not due to selection bias in trade difficulty.

Our empirical evidence indirectly suggests that large trade executions, likely for institutional investors, are exploited by counterparties in some dark pools. Given institutional investors' prominent roles in managing average citizens' wealth and producing fundamental research, such predatory behaviors in dark pools may have significant social costs, making further research in this area valuable.

Our analysis suggests that regulating dark pools on the basis of trade size leaves room for additional possible execution quality benefits from exclusivity in dark pools. Venue-design and the trading population it attracts (or repels) are potentially important factors for execution footprint and overall execution quality in large trades. Providing exclusive environments for natural contra-side traders to execute large volumes may proffer benefits beyond trade-size-based regulation.

We have demonstrated one way in which designed exclusivity in trading venues matters. Not all dark pools are created equal.

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Appendix A: Institutional Setting

An Exclusive Dark Pool: Liquidnet Classic

As of 2011, Liquidnet offers various execution options to its clients. The majority of Liquidnet's volume is executed by clients using the "negotiated" option (also known as "Liquidnet Classic"). With this option, traders enter indications of interest (IOIs) through their Order Management System (OMS) interfaces. Suppose a trader places an IOI to buy one million shares of IBM at Liquidnet Classic. If another buy-side trader enters or has entered an IOI to sell IBM, both traders are informed that there is a potential counterparty for IBM. Each party is in "passive" mode. In order to execute shares with each other, one of the traders has to make the decision to change his status from "passive" to "active" mode. The other side then can send an invitation to trade, which enables a one-on-one negotiation of price and size. Specifically, each of the counterparties specifies his or her maximum size and negotiates price. Liquidnet indicates that more than 95% of executions occur at the bid-ask midpoint.

To preserve anonymity, negotiations occur through an interface provided by Liquidnet, which is independent of the trader's OMS. Once negotiations begin, typically they are completed within seconds. Therefore neither of the counterparties is expected to have an opportunity to front-run the other at other execution venues. In addition Liquidnet monitors market activity that occurs around the negotiation.

Traders can specify a minimum IOI size that they require from the other side in order to be displayed as a"passive" interest. A trader cannot observe that there is a potential ("passive") counterparty until he places an IOI in that security.

Institutional investors have the option of participating only with other institutional investors. Brokers operate in a separate dark pool operated by Liquidnet, and members have the choice of whether to dedicate a portion of their shares to that pool.

Typically OMS interfaces are designed to allow traders to easily split and modify orders across execution venues, brokers, and strategies (e.g., algorithms). For example, a trader who wants to buy one million shares of IBM that day can use his OMS to enter an IOI for 900,000 shares at Liquidnet Classic while using a broker's Volume Participation algorithm to execute the other 100,000 shares. Throughout the day, if IBM is not executed at Liquidnet Classic, the trader can continually decrease the size of the IOI at Liquidnet Classic while increasing the amount of IBM executed using other strategies.

Undisplayed Liquidity at Other Dark Pools and at Broker-Dealers' Desks

Among the dark pools that are less exclusive than just buy-side-only crossing are those owned and operated by broker-dealers as internal dark pools. Examples are Credit Suisse Crossfinder, UBS PIN, and Goldman Sachs Sigma X. O'Hara & Ye (2011) state that off-exchange volumes must now be reported through trade reporting facilities (TRFs). Weaver (2011) indicates that more than 90% of executions that are reported through the TRFs are either executed in dark pools or are internalized order flow. Furthermore, more than 75% of dark pool order flow is internalized order flow.

For many years institutional traders have had the option of executing blocks with their brokers' upstairs desks. Zhu (2011) states that although this type of broker-dealer internalization is not usually classified as dark pool trading, it is a source of undisplayed liquidity. Like the undisplayed liquidity made available by internalization dark pools, this type of undisplayed liquidity is less exclusive than buy-side-only dark pools.

Nimalendran & Ray (2011) provides evidence of how blurred the line has become between undisplayed liquidity in dark pools and undisplayed liquidity available at broker-dealers' desks. They analyze the executions of a dark pool operator that offers its clients a variety of options for executing orders. It operates a dark pool for manual negotiation for block trades. The dark pool operator also has a brokerage desk, which can execute blocks or work orders for clients.

Undisplayed Liquidity at Exchanges

Exchanges offer a variety of order types that allow traders to hide their willingness to trade. Undisclosed limit orders often maintain price priority but lose time priority to displayed limit orders at the same price. Undisclosed limit orders are not protected by the Regulation NMS Order Protection Rule. Exchanges typically allow all traders to use the undisplayed ("dark") order types they offer. Zhu (2011) refers to hidden order liquidity on exchanges as the other source of undisplayed liquidity not normally classified as dark pool trading.

In order to execute against hidden liquidity, some algorithms are designed to send IOC (immediate-or-cancel) limit orders to "sweep" exchanges at or within the top-of-book quotes. Some algorithms "oversize" the order quantity (i.e., the order is larger than the size displayed at the top-of-book). As a result, executions may exceed the size quoted at the venue. Because the ratio of undisplayed to displayed size can be far greater than one, block executions can occur, even in thinly-quoted stocks.

In addition to allowing a variety of hidden limit order types, NYSE and NASDAQ operate crossing networks. Ye (2010) reports that exchange crossing network executions are not reported independently from their other exchange executions.

Table 1: Security sample selection criteria follows O'Hara & Ye (2011) and Boehmer (2005).

Criterion	**Nasdaq**	**NYSE**
All Securities in December 2010 CRSP File	2814	2450
No data listed in COMPUSTAT	-82	-41
CRSP Filter		
Non-common stock equities	-101	-353
Common stocks of non-US companies, closed-end funds, REITs, ADRs, ETFs	-138	-331
Dual Class Stock	-72	-112
Volume and Price Filter		
Mean daily volume < 1,000	0	0
Price < $5	-390	-68
No trade of at least 50,000 shares in sample	-1249	-633
Final Sample	**782**	**912**

Table 2: Sample summary data. Data are for all trading days from January 3 through March 31, 2010 for securities as shown in Table 1. All trades are included in the full sample. The first column shows the full sample while the remainder split the sample into mutually exclusive groups. Panel A displays unsegmented data. Panel B segments the data into terciles (by number of trades) based on trade difficulty where we define trade difficulty as trade size divided by average daily volume. Panel C segments the data into terciles based on average daily volume.

Panel A: All Large Trades

	All Trades	Liquidnet Trades	Other Dark Pool Trades
Full Sample			
Number of Trades	61,158	9%	91%
Average Trade Size	118,613	114,099	119,067
Average Relative Trade Size	7.3%	12.4%	6.8%

Panel B: Terciles by Trade Difficulty

	All Trades	Liquidnet Trades	Other Dark Pool Trades
Top Tercile			
Number of Trades	21,859	16%	84%
Average Trade Size	153,256	124,666	158,829
Average Relative Trade Size	18.2%	18.5%	18.2%
Middle Tercile			
Number of Trades	21,750	7%	93%
Average Trade Size	108,110	96,766	109,013
Average Relative Trade Size	1.8%	2.0%	1.8%
Bottom Tercile			
Number of Trades	17,549	2%	98%
Average Trade Size	88,477	90,707	88,422
Average Relative Trade Size	0.3%	0.5%	0.3%

Panel C: Terciles by Average Daily Volume

	All Trades	Liquidnet Trades	Other Dark Pool Trades
Top Tercile			
Number of Trades	17,489	3%	97%
Average Trade Size	143,394	229,038	141,102
Average Relative Trade Size	0.6%	1.2%	0.5%
Middle Tercile			
Number of Trades	21,674	7%	93%
Average Trade Size	117,923	129,208	117,029
Average Relative Trade Size	2.4%	2.8%	2.3%
Bottom Tercile			
Number of Trades	21,995	16%	84%
Average Trade Size	99,587	92,534	100,942
Average Relative Trade Size	17.5%	18.2%	17.3%

Table 3: Non-parametric tests of abnormal return correlations surrounding large trades. Negative M-signs indicate negative correlation between the pre-trade one-minute and post-trade one-minute abnormal returns. P-values are shown for the null hypothesis that the M-sign is zero. The numbers between columns are the z-scores and p-values from Wilcoxon rank-sum tests for the equality of the medians between two samples. The values shown compare the column to the right with the column to the left. This data set is generated by using the best match under the Davies & Kim (2008) methodology. Prices are measured based on the last trade price prior to the measurement time.

First-Period Variable:	1-to-0 Minute Pre-Trade Ab. Return		
Second-Period Variable:	0-to-1 Minute Post-Trade Ab. Return		
Panel A: Non-Overlapping Trades Sample			
	Liquidnet Trades		Other Dark Pool Trades
Full Sample			
M-Sign	-32.5		-1297.5
P-Value (H0: Median = 0)	0.285		0.000
Sample Size	3,933		47,278
Wilcoxon Z-Score (Right > Left)		-2.672	
P-value (Equality of Medians)		0.008	
Panel B: Terciles by Trade Difficulty			
	Liquidnet Trades		Other Dark Pool Trades
Top Tercile			
M-Sign	-22.0		-250.5
P-Value (H0: Median = 0)	0.320		0.000
Sample Size	2,087		12,466
Wilcoxon Z-Score (Right > Left)		-0.998	
P-value (Equality of Medians)		0.318	
Middle Tercile			
M-Sign	6.0		-486.0
P-Value (H0: Median = 0)	0.764		0.000
Sample Size	1,449		18,691
Wilcoxon Z-Score (Right > Left)		-2.536	
P-value (Equality of Medians)		0.011	
Bottom Tercile			
M-Sign	-16.5		-561.0
P-Value (H0: Median = 0)	0.097		0.000
Sample Size	397		16,121
Wilcoxon Z-Score (Right > Left)		0.181	
P-value (Equality of Medians)		0.857	
Panel C: Terciles by Average Daily Volume			
	Liquidnet Trades		Other Dark Pool Trades
Top Tercile			
M-Sign	-15.5		-658.0
P-Value (H0: Median = 0)	0.132		0.000
Sample Size	428		16,088
Wilcoxon Z-Score (Right > Left)		-0.235	
P-value (Equality of Medians)		0.814	
Middle Tercile			
M-Sign	-2.5		-386.5
P-Value (H0: Median = 0)	0.914		0.000
Sample Size	1,473		18,820
Wilcoxon Z-Score (Right > Left)		-1.476	
P-value (Equality of Medians)		0.140	
Bottom Tercile			
M-Sign	-14.5		-253.0
P-Value (H0: Median = 0)	0.512		0.000
Sample Size	2,032		12,370
Wilcoxon Z-Score (Right > Left)		-1.631	
P-value (Equality of Medians)		0.103	

Table 4: Non-parametric tests of abnormal volume prior to large trades. The abnormal volume measure compares the trading volume in the minute prior to the large trade to the trading volume of the previous minute. Positive M-signs indicate an abnormal increase in volume prior to the large trade. P-values are shown for the null hypothesis that the M-sign is zero. The numbers between columns are the z-scores and p-values from Wilcoxon rank-sum tests for the equality of the medians between two samples. The values shown compare the column to the right with the column to the left. This data set is generated by using the best match under the Davies & Kim (2008) methodology.

First-Period Variable:	2-to-1 Minute Pre-Trade Volume		
Second-Period Variable:	1-to-0 Minute Pre-Trade Volume		
Panel A: Non-Overlapping Trades Sample			
	Liquidnet Trades		Other Dark Pool Trades
Full Sample			
M-Sign	97.5		1880.5
P-Value (H0: Median = 0)	0.001		0.000
Sample Size	3,500		44,674
Wilcoxon Z-Score (Right > Left)		1.910	
P-value (Equality of Medians)		0.056	
Panel B: Terciles by Trade Difficulty			
	Liquidnet Trades		Other Dark Pool Trades
Top Tercile			
M-Sign	66.0		408.0
P-Value (H0: Median = 0)	0.002		0.000
Sample Size	1,727		10,754
Wilcoxon Z-Score (Right > Left)		0.926	
P-value (Equality of Medians)		0.354	
Middle Tercile			
M-Sign	11.0		777.5
P-Value (H0: Median = 0)	0.572		0.000
Sample Size	1,382		17,908
Wilcoxon Z-Score (Right > Left)		2.369	
P-value (Equality of Medians)		0.018	
Bottom Tercile			
M-Sign	20.5		695.0
P-Value (H0: Median = 0)	0.043		0.000
Sample Size	391		16,012
Wilcoxon Z-Score (Right > Left)		0.190	
P-value (Equality of Medians)		0.849	
Panel C: Terciles by Average Daily Volume			
	Liquidnet Trades		Other Dark Pool Trades
Top Tercile			
M-Sign	10.5		612.0
P-Value (H0: Median = 0)	0.332		0.000
Sample Size	425		16,012
Wilcoxon Z-Score (Right > Left)		0.347	
P-value (Equality of Medians)		0.729	
Middle Tercile			
M-Sign	31.5		838.0
P-Value (H0: Median = 0)	0.099		0.000
Sample Size	1,411		18,166
Wilcoxon Z-Score (Right > Left)		1.999	
P-value (Equality of Medians)		0.046	
Bottom Tercile			
M-Sign	55.5		430.5
P-Value (H0: Median = 0)	0.007		0.000
Sample Size	1,664		10,496
Wilcoxon Z-Score (Right > Left)		1.212	
P-value (Equality of Medians)		0.226	

Table 5: Non-parametric tests of abnormal increases in volatility, proxied by trading range, prior to large trades. The abnormal volatility measure compares the trading range in the minute prior to the large trade to the trading range of the previous minute. Positive M-signs indicate an abnormal increase in volatility prior to the large trade. P-values are shown for the null hypothesis that the M-sign is zero. The numbers between columns are the z-scores and p-values from Wilcoxon rank-sum tests for the equality of the medians between two samples. The values shown compare the column to the right with the column to the left. This data set is generated by using the best match under the Davies & Kim (2008) methodology.

First-Period Variable:	2-to-1 Minute Pre-Trade Range		
Second-Period Variable:	1-to-0 Minute Pre-Trade Range		
Panel A: Non-Overlapping Trades Sample			
	Liquidnet Trades		Other Dark Pool Trades
Full Sample			
M-Sign	-7.5		606.5
P-Value (H0: Median = 0)	0.794		0.000
Sample Size	2,985		40,116
Wilcoxon Z-Score (Other > Liquidnet)		2.780	
P-value (Equality of Medians)		0.005	
Panel B: Terciles by Trade Difficulty			
	Liquidnet Trades		Other Dark Pool Trades
Top Tercile			
M-Sign	-19.0		135.0
P-Value (H0: Median = 0)	0.311		0.003
Sample Size	1,383		8,448
Wilcoxon Z-Score (Right > Left)		2.443	
P-value (Equality of Medians)		0.015	
Middle Tercile			
M-Sign	-4.5		175.0
P-Value (H0: Median = 0)	0.816		0.005
Sample Size	1,225		16,026
Wilcoxon Z-Score (Right > Left)		1.391	
P-value (Equality of Medians)		0.164	
Bottom Tercile			
M-Sign	16.0		296.5
P-Value (H0: Median = 0)	0.096		0.000
Sample Size	377		15,642
Wilcoxon Z-Score (Right > Left)		0.211	
P-value (Equality of Medians)		0.833	
Panel C: Terciles by Average Daily Volume			
	Liquidnet Trades		Other Dark Pool Trades
Top Tercile			
M-Sign	12.5		266.0
P-Value (H0: Median = 0)	0.216		0.000
Sample Size	417		15,703
Wilcoxon Z-Score (Right > Left)		-0.125	
P-value (Equality of Medians)		0.901	
Middle Tercile			
M-Sign	-19.0		195.0
P-Value (H0: Median = 0)	0.290		0.002
Sample Size	1,279		16,396
Wilcoxon Z-Score (Right > Left)		3.154	
P-value (Equality of Medians)		0.002	
Bottom Tercile			
M-Sign	-1.0		145.5
P-Value (H0: Median = 0)	0.978		0.001
Sample Size	1,289		8,017
Wilcoxon Z-Score (Right > Left)		1.213	
P-value (Equality of Medians)		0.225	

Table 6: Linear regressions of the natural log of volume increase and range increase measures on an indicator variable for whether a trade occurs at Liquidnet Classic and a vector of control variables. The bid-ask spread, number of trades, total volume and trading range (high minus low transaction price) are measured over the five minutes prior to the trade. Price, market cap, and ADV measures are taken from December 2010. Trade difficulty is measured as the trade size over the ADV. P-values calculated using bootstrapped standard errors are reported in parentheses. ***,**,* indicate significance at the 1%, 5% and 10% levels.

	Volume Increase		Range Increase	
	(1)	(2)	(3)	(4)
Liquidnet Dummy	-0.1063***	-0.1182***	-0.0481***	-0.0422***
	(0.0000)	(0.0000)	(0.0006)	(0.0013)
Time		0.0000***		0.0000
		(0.0035)		(0.1363)
Bid-Ask Spread		0.5420		-0.3491
		(0.7358)		(0.7053)
Number of Trades		-0.0001***		-0.0000
		(0.0000)		(0.5165)
Total Volume		0.0000***		-0.0000
		(0.0000)		(0.6602)
Trading Range		-0.5125***		-0.2019***
		(0.0002)		(0.0098)
NYSE Dummy		-0.0008		0.0090
		(0.9626)		(0.2670)
Price		-0.0003		0.0002
		(0.4662)		(0.3332)
Market Cap		0.0000		-0.0000
		(0.8412)		(0.5412)
Average Daily Volume		-0.0000		0.0000**
		(0.4436)		(0.0129)
Volume-Squared		-0.0000***		0.0000
		(0.0000)		(0.8079)
Spread-Squared		-4.3768		-3.8169
		(0.6825)		(0.6469)
ADV-Squared		-0.0000		-0.0000**
		(0.7896)		(0.0102)
NumTrades-Squared		0.0000***		0.0000
		(0.0000)		(0.2452)
Range-Squared		0.4454***		0.1484**
		(0.0008)		(0.0144)
Relative Trade Size		0.2898***		0.1316**
		(0.0034)		(0.0104)
Constant	0.1912***	0.1912***	0.0379***	0.0376***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
R-Squared	0.0004	0.0038	0.0003	0.0013
Observations	50,926	50,403	48,910	48,440

Table 7: Two-stage estimation of volume and range increase with a first-stage probit regression to correct for selection bias. Columns (1) and (3) presents the results from a probit regression predicting trade location. Columns (2) and (4) present linear regression results estimating volume increases and range increases that include similar controls as in Table 6 and additional selection bias controls γ_1 and γ_2 (the Inverse Mills Ratio and its non-selected counterpart). Parentheses in the variable names indicate the number of minutes prior to the trade execution that the measure was taken over. Price, market cap, and ADV measures are taken from December 2010. P-values calculated using bootstrapped standard errors are reported in parentheses. ***,**,* indicate significance at the 1%, 5% and 10% levels.

	Volume Increase		Range Increase	
	(1) Liquidnet Dummy	(2) Volume Increase	(3) Liquidnet Dummy	(4) Range Increase
Bid-Ask Spread (60)	14.0770***		13.8421***	
	(0.0000)		(0.0000)	
Number of Trades (60)	0.0000		0.0000	
	(0.6309)		(0.6293)	
Total Volume (60)	-0.0000***		-0.0000***	
	(0.0068)		(0.0080)	
Trading Range (60)	0.2071***		0.1900***	
	(0.0001)		(0.0003)	
NumTrades (60) - Squared	0.0000		0.0000	
	(0.4485)		(0.4505)	
Spread (60) - Squared	-21.3675***		-20.9121***	
	(0.0000)		(0.0000)	
Volume (60) - Squared	0.0000**		0.0000**	
	(0.0274)		(0.0304)	
Range (60) - Squared	-0.0574***		-0.0526***	
	(0.0009)		(0.0023)	
Time (seconds)	0.0000***	-0.0000	0.0000***	-0.0000*
	(0.0000)	(0.1504)	(0.0000)	(0.0994)
NYSE	-0.0000***	0.0000***	-0.0000***	0.0000*
	(0.0000)	(0.0002)	(0.0000)	(0.0692)
Price	-0.1381***	0.0161	-0.1457***	0.0135
	(0.0000)	(0.4053)	(0.0000)	(0.1476)
Market Cap	0.0005	-0.0006	0.0006	0.0002
	(0.3233)	(0.1643)	(0.3191)	(0.4831)
Average Daily Volume	0.0000***	-0.0000	0.0000***	-0.0000
	(0.0000)	(0.4324)	(0.0000)	(0.3907)
ADV - Squared	-0.0000***	0.0000	-0.0000***	0.0000
	(0.0000)	(0.2560)	(0.0000)	(0.1405)
Liquidnet Dummy		-0.0949***		-0.0519***
		(0.0035)		(0.0006)
Gamma 1		0.3532		0.3021**
		(0.1998)		(0.0182)
Gamma 2		0.1697*		0.0308
		(0.0739)		(0.4396)
Bid-Ask Spread (5)		-0.5634		-0.8585
		(0.7788)		(0.4110)
Number of Trades (5)		-0.0001***		-0.0000
		(0.0000)		(0.4692)
Total Volume (5)		0.0000***		-0.0000
		(0.0000)		(0.9611)
Trading Range (5)		-0.6020***		-0.2178***
		(0.0000)		(0.0063)
Volume (5) - Squared		-0.0000***		-0.0000
		(0.0000)		(0.8841)
Spread (5) - Squared		-2.9325		-3.7862
		(0.8039)		(0.6369)
NumTrades (5) - Squared		0.0000***		0.0000
		(0.0000)		(0.2376)
Range (5) - Squared		0.4906***		0.1470**
		(0.0001)		(0.0172)
Constant	-1.6186***	0.1917***	-1.6288***	0.0374***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
R-Squared (Pseudo for probit)	0.0816	0.0036	0.0834	0.0012
Observations	50,403	50,386	48,440	48,423

Table 8: Differences in trade timing across venues. This table shows the z-score and p-values (in parentheses) from Wilcoxon rank-sum tests comparing the median timing of trades at Liquidnet Classic versus those at other dark pools. Negative z-scores indicate that Liquidnet Classic trades occur earlier in the day than other dark pools trades. ***,**,* are suppressed due to the high significance of all estimates.

Panel A: Full Sample	Tests of Median Equality
Full Sample	
Wilcoxon Z-Score	-20.5
P-value (Equality of Medians)	(0.000)
Panel B: Terciles by Trade Difficulty	Tests of Median Equality
Top Tercile	
Wilcoxon Z-Score	-8.7
P-value (Equality of Medians)	(0.000)
Middle Tercile	
Wilcoxon Z-Score	-10.6
P-value (Equality of Medians)	(0.000)
Bottom Tercile	
Wilcoxon Z-Score	-15.7
P-value (Equality of Medians)	(0.000)
Panel C: Terciles by Average Daily Volume	Tests of Median Equality
Top Tercile	
Wilcoxon Z-Score	-16.5
P-value (Equality of Medians)	(0.000)
Middle Tercile	
Wilcoxon Z-Score	-11.3
P-value (Equality of Medians)	(0.000)
Bottom Tercile	
Wilcoxon Z-Score	-8.6
P-value (Equality of Medians)	(0.000)

Table 9: Estimation of trade clustering across days. This table shows the coefficient estimates and p-values (in parentheses) of the independent variables estimated in a negative binomial regression model used to model the number of large trades per day at each venue type. ***,**,* are suppressed due to the high significance of nearly all estimates with the exceptions being the last three coefficients of column (3).

Dependent Variable:	Number of Venue Trades			
	(1) Liquidnet Trades	(2) Other Dark Pool Trades	(3) Liquidnet Trades	(4) Other Dark Pool Trades
Constant	-3.507	-1.959	-3.517	-1.992
	(0.000)	(0.000)	(0.000)	(0.000)
AvgDecVol $\times 10^6$	0.035	0.078	0.016	0.077
	(0.000)	(0.000)	(0.000)	(0.000)
YearEndPrice	0.001	-0.005	0.001	-0.006
	(0.320)	(0.000)	(0.000)	(0.000)
MarketCap $\times 10^4$	0.033	0.047	0.038	0.046
	(0.004)	(0.000)	(0.000)	(0.000)
exchangeNYSE	0.166	0.466	0.150	0.466
	(0.000)	(0.000)	(0.000)	(0.000)
Lag1 Liquidnet Trades	0.761		0.722	0.200
	(0.000)		(0.000)	(0.000)
Lag2 Liquidnet Trades	0.452		0.446	0.122
	(0.000)		(0.000)	(0.000)
Lag3 Liquidnet Trades	0.384		0.366	0.131
	(0.000)		(0.000)	(0.000)
Lag4 Liquidnet Trades	0.302		0.288	0.110
	(0.000)		(0.000)	(0.000)
Lag1 Other DP Trades		0.191	0.054	0.181
		(0.000)	(0.000)	(0.000)
Lag2 Other DP Trades		0.079	-0.008	0.073
		(0.000)	(0.521)	(0.000)
Lag3 Other DP Trades		0.057	0.015	0.053
		(0.000)	(0.227)	(0.000)
Lag4 Other DP Trades		0.056	0.015	0.052
		(0.000)	(0.179)	(0.000)

Figure 1: Liquidnet Classic Pre-Trade and Post-Trade Abnormal Returns. Pre-trade abnormal returns are on the x-axis and post-trade abnormal returns are on the y-axis. Abnormal returns are measured relative to a matched control firm's return over the same time period. The red line is a simple univariate linear regression of the post-trade abnormal returns on the pre-trade abnormal returns.

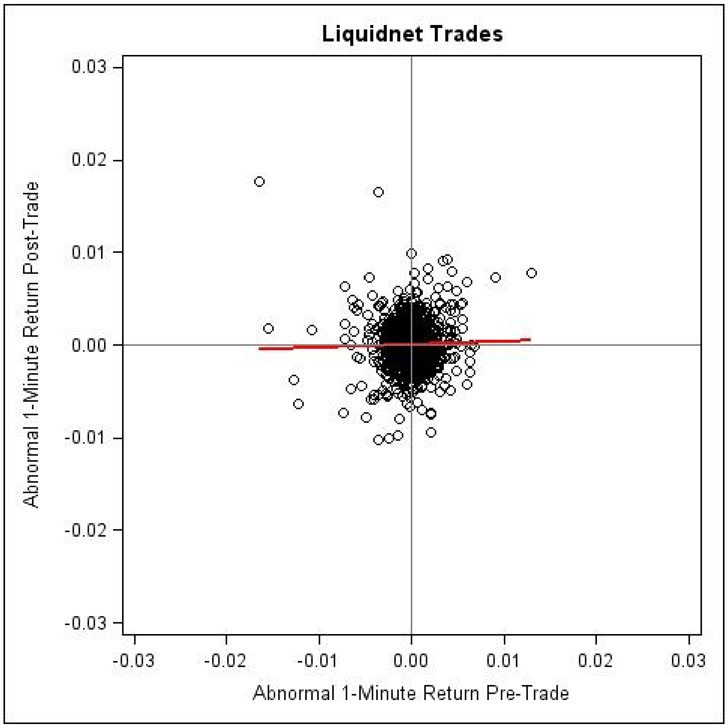

Figure 2: Dark Pool (without Liquidnet Classic) Pre-Trade and Post-Trade Abnormal Returns. Dark pool executions are considered those whose volume is reported to the trade reporting facility or the alternative display facility. Pre-trade abnormal returns are on the x-axis and post-trade abnormal returns are on the y-axis. Abnormal returns are measured relative to a matched control firm's return over the same time period. The red line is a simple univariate linear regression of the post-trade abnormal returns on the pre-trade abnormal returns.

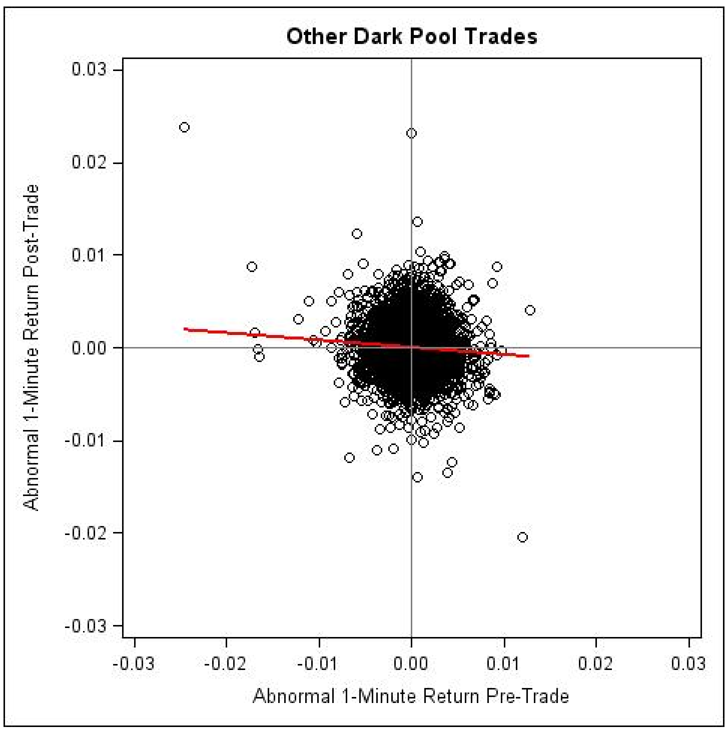

Figure 3: Large Trade Execution Times (by transactions). The graphs show the percentage share of transactions for each time period based on our full sample with all trades in the first or last 15 seconds of the trading day removed in order to minimize inclusion of mislabaled open and close prints in the TAQ data. Results are similar for share of volume.