Dark Trading Volume and Market Quality: A Natural Experiment

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Abstract

We exploit an exogenous shock to dark trading volume in order to identify the effect of dark trading on market quality. Following a 34% reduction in dark trading volume, we find that the cost of trade (e.g., effective spreads, realized spreads, price impact, and quoted spreads) does not change. While our findings stand in contrast to those of prior studies, a number of supplemental tests confirm that conflicting inferences cannot be attributed to different stock samples or time periods. Our research highlights the benefit of structured experimentation from the Securities and Exchange Commission (SEC) for understanding causal effects in capital markets.

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I. Introduction

Dark trading occurs on platforms that do not display orders prior to execution and accounts for roughly one-third of all equity trading volume in U.S. markets.[1] Yet as dark venues aggressively compete for market share and traders decide how best to fulfill the fiduciary task of order routing, basic questions regarding the consequences of trading "in the dark" remain unanswered. The current lack of understanding fuels an intense and very fluid policy debate concerning market structure. Regulatory bodies worldwide, tasked with protecting overall trader welfare, are considering and/or implementing policies to curb the use of dark venues. For example, European policymakers plan to restrict dark trading to 8% of overall trading volume when MiFID II rules take effect.[2] At the current time, regulators in the United States, Australia, Canada, and Hong Kong are debating similar policies.

Economic theory offers opposing predictions for how dark trading might influence market outcomes. Theoretical models argue that dark trading may damage overall market quality (e.g., wider spreads, higher volatility, and less efficient prices) by segmenting informed from uninformed order flow (Admati and Pfleiderer, 1998; Madhavan 1995; Zhu, 2014). Bolton, Santos, and Scheinkman (2016) offer the similar message that cream-skimming (i.e., selectively routing uniformed order flow to dark venues) is harmful. Likewise, the availability of dark venues may detract from liquidity externalities that arise in a centralized market. On the other hand, the proliferation of dark trading might enhance the overall quality of equity markets by increasing

[1] Dark venues include more than 60 different alternative trading systems (ATS) and internalized trades at hundreds of broker-dealers. Statistics on dark trading volume are obtained from BATS Global Markets for the month of July, 2017: https://www.bats.com/us/equities/market_share/ . Recent reports from the TABB Group point to a higher fraction of dark trading volume (44.9%), but include hidden orders in lit markets in this total: https://research.tabbgroup.com/report/v15-034-tabb-equity-digest-q2-2017

[2] See discussion in Davies and Sirri (2017). – p, 28.

competition (Economides, 1996; Hendershott and Mendelson, 2000). Dark venues may also inhibit predatory trading activity by allowing traders to hide their intentions (Harris, 1997).

Ultimately, the question of how dark trading affects market quality is an empirical one. While the question is both relevant and straightforward, answers are ambiguous. The central problem is identification, since trading on dark versus lit venues is the endogenous outcome of a complex trading landscape. Simply put, traders choose execution strategies, which may include routing orders to dark venues, based on expectations of trading costs and many unobservable constraints. So while empirical studies use various econometric corrections (e.g., instrumental variables, selection bias corrections, etc.) to obtain inference, one must recognize the inherent difficulty in establishing a causal relation within the "complex ecosystem" in which securities trade.[3] Not surprisingly, the body of empirical research lacks a cohesive message. Degryse, de Jong, and van Kervel (2014), Weaver (2014), Comerton-Forde and Putnins (2015), Hathaway, Kwan, and Zheng (2017; hereafter "HKZ") find evidence of increased transactions costs and diminished market quality as dark trading volumes rise. In stark contrast, O'Hara and Ye (2011), Jiang, McInish, and Upson (2012), Buti, Rindi, and Werner (2016), Foley and Putnins (2016), and others associate greater levels of dark trading volume with significant improvements in transactions costs, price efficiency, and execution speeds.

The contribution of this study lies squarely on identification. We exploit a large exogenous shock to dark trading that arises from the SEC's 'Tick Size Pilot' enacted in October 2016. The pilot, designed to examine liquidity for smaller firms, increases the tick size (to one nickel) for stocks in three randomly assigned treatment groups and holds constant the trading environment of

[3] Staff of the Division of Trading and Markets, U.S. Securities and Exchange Commission, "Equity Market Structure Literature Review, Part I: Market Fragmentation," (available at http://www.sec.gov/marketstructure/research/fragmentation-lit-review-100713.pdf)

an equal number of control firms. Our experiment detracts from the pilot's primary thrust and instead utilizes a nuanced distinction between two of the pilot's treatment groups – Group 2 (G2) and Group 3 (G3). While the pilot restricts quoting and trading to nickel increments for stocks in treatment groups G2 and G3, it imposes an additional restriction on stocks in G3. This additional restriction, commonly called the *Trade-At* rule, prohibits venues from executing a trade at the National Best Bid or Offer quote unless it is the venue displaying that quote. Instead, it must either offer a five-cent price improvement or route the order to the venue with the best quote.[4] Since dark facilities do not display quotes, the provision reduces the competitive position and market share of these venues. Hence, we argue that *Trade-At* creates an exogenous drop in dark trading.

[Insert Figure 1 about here.]

We summarize our experiment in three pictures that describe the forty trading days surrounding October 2016, when the Pilot was implemented in a staggered fashion. Figure 1 reveals that the average fraction of stock-level trading on dark venues drops by four standard deviations, from 35% to 23% in G3 stocks. In contrast, dark trading in group G2 rises slightly. Figure 2 displays a similar plot for effective spreads, a common measure of transactions costs. Despite the large shock to dark trading around the event, there is no discernible change in the difference in effective spreads between the two groups of stocks. Finally, Figure 3 plots a market quality measure based on variance ratios, which changes only trivially around implementation. On their face, these results suggest trading on dark venues has a largely benign effect on overall market quality.

[Insert Figure 2 about here.]

[4] In Appendix A, we provide three examples of how *Trade-At* affects order routing.

[Insert Figure 3 about here.]

We test the veracity of our 'inference from pictures' using formal statistical tests. Specifically, we implement a difference-in-difference framework in which G3 stocks serve as the "treatment" group and G2 stocks are the "control" group. When implementing the Pilot, the SEC stratified groups based on market capitalization, volume weighted average price, and average daily volume to facilitate random assignment. We verify our two groups of stocks are similar along these characteristics, which lends support to the differences-in-differences analysis.

For each stock, we construct daily firm-level observations over the twenty days before and the twenty days after the pilot's staggered implementation during October 2016. The dependent variables that we analyze include spread measures – effective spread, quoted spread, realized spread, and price impact – along with an intra-day variance ratio to evaluate price efficiency. Our regressions provide no evidence that the large exogenous shift in dark trading volume causes a change in effective or quoted spreads. When investigating the components of the effective spread – the realized spread and price impact – we find a similar result. In fact, the only variable that appears affected is the variance ratio. The variance ratio measure for G3 increases by 0.0185 (*t*-statistic=1.81) when compared to G2. While this change is only marginally significant, this is modest evidence of a loss to price efficiency following restrictions on dark trading.

Our inference relies critically on the exogenous nature of the drop in dark trading displayed in Figure 1. We conduct a rigorous set of exercises to demonstrate validity. Importantly, we offer statistical evidence that differences in dark trading and our market quality variables across groups are stable during the period leading up to the Pilot's enactment and that stocks in groups G2 and G3 are similar across a number of other trading characteristics. We argue this evidence is supportive of the parallel trends assumption, which is key for inference in differences-in-differences analyses.

Equally crucial is demonstrating the pilot does not meaningfully alter other characteristics that possibly correlate with market quality. This is akin to the exclusion criteria for instrumental variables. We demonstrate the fraction of lit trading that occurs on inverted venues only mildly increases in G3 stocks compared to G2 stocks. This finding is important because Comerton-Forde, Gregoire, and Zhong (2018) discuss how inverted fee venues offer sub-penny price improvement and argue that any effect of dark trading on market quality may be confounded by an inverted venue share effect. We show that while inverted venue share of total trading increases substantially, much of this change is attributable to trades that shift from dark to lit venues being proportionally allocated across all lit venues. We also show little treatment effect on total trading volume and other characteristics of how trades are dispersed across the lit exchanges.

We investigate the robustness of our results in several ways. First, we include control variables in our difference-in-difference regressions that aim to capture trading intentions. Second, because prior studies have found differential effects for dark trading when looking at different subsamples of stocks, we parse our sample along multiple stock characteristics and repeat the regression analysis. Specifically, we divide our sample by the median market capitalization, turnover, dark trading, and fragmentation across lit venues. We also segment stocks according to whether the tick pilot change is likely to represent a binding constraint. Taken together, all robustness test point to a common inference – dark trading does not affect transaction costs and there is only marginal evidence that it affects price efficiency.

We reconcile our results with others in the literature who conclude dark trading deteriorates liquidity. Notably, HKZ use a 2-stage procedure to control for endogeneity and find that a 10%

rise in dark volume leads to a 9.2% increase in effective spreads. Our message is quite different as we find a larger shock to dark trading has no effect on liquidity. One possibility is that our different sample period and sample of stocks (our sample is skewed towards smaller stocks) are responsible for the stark difference in conclusions. With this in mind, we replicate HKZ during both their original sample period using their sample of stocks and during our sample period using our sample of stocks. In both replications we find results that are quantitatively and qualitatively similar to those reported by HKZ. Thus, our differences with HKZ are not driven by our stock sample or time period. Rather, they are more reasonably attributable to our respective identification strategies. Our work and replication highlights the inherent difficulty in selection of instruments and the importance of structured experimentation from the Securities and Exchange Commission (SEC) for understanding causal effects in capital markets.

An important caveat is in order. Dark venues are typically associated with both a lack of pre-trade transparency and a finer pricing grid (*e.g.*, subpenny executions) when compared to lit exchanges. The nature of our experiment isolates the effect of pre-trade transparency because stocks in both groups are subject to the same five-cent pricing grid. We view this as a strength of our approach because any study of market structure that isolates individual aspects provides a cleaner set of guidelines for regulators and those who experiment with future market design. Foley and Putnins (2016) study a shock to dark trading in Canada due to a 2012 requirement that price must be improved by a full tick (as opposed to a fraction of a penny previously). We view their analysis as complementary to ours as it better isolates the effects of dark trades occurring on a finer pricing grid. They associate dark trading with lower spreads and improved informational efficiency.

The remainder of our paper proceeds as follows: Section II reviews the SEC's tick size pilot, Section III summarizes our data, Section IV discusses our research design and results, and Section V concludes.

II. Natural Experiment: SEC Tick Size Pilot

The 2012 Jumpstart Our Business Startups Act ("JOBS Act") directed the SEC to assess how decimalization affects the liquidity and trading of smaller capitalization companies. The directive stems from a concern that decimalization reduced incentives to make markets, produce sell-side research, and underwrite public offerings in smaller firms. Advocates of a wider minimum tick size argue that under such a policy market making would be more profitable, sell side analysts would increase coverage, and institutions would be more likely to invest in smaller firms. In response, the SEC implemented the "Tick Size Pilot" in October of 2016, which increased quoting and trading increments from $0.01 to $0.05 for randomly selected samples of small- and mid-capitalization stocks.

Stratifying by market capitalization, volume weighted average price, and average daily volume, the pilot randomly assigns approximately 2,400 stocks to a control group and three treatment groups[5]:

- Treatment Group 1 (G1) - stocks must be quoted in nickel increments;
- Treatment Group 2 (G2) – same treatment as G1, plus stocks must also *trade* in nickel increments or at a half-nickel midpoint.
- Treatment Group 3 (G3) – same treatment as G2, plus stocks are subject to the *trade-at provision*, which prohibits a venue from executing a trade at the "Best Protected Bid" (NBB) or "Best Protected Offer" (NBO) unless it is displaying that quote.[6]

[5] The complete SEC Tick Size Pilot plan is available at https://www.sec.gov/rules/sro/nms/2015/34-74892-exa.pdf
[6] Appendix **A** provides three examples of trade-at from the SEC implementation plan.

The treatments for G1 and G2 clearly align with the JOBS Act directive as they change the pricing grid from pennies to nickels.[7] In contrast, the additional treatment effect in Group 3, commonly known as a *Trade-At* provision, specifies that any trading venue not displaying protected quotes (*e.g.* all dark venues) cannot execute at the inside quote (NBB or NBO) – effectively shifting trade from dark to lit venues.[8]

We exploit the nuanced *difference between Group 3 and Group 2 stocks* to identify an exogenous shock to dark trading volume. Dark venues' inability to execute trades at the prevailing inside quote coupled with the coarser pricing grid (improvements to the inside quote must be at least five cents) should result in a significant transfer of trading volume from dark to lit trading venues. Moreover, comparing effects between Groups 3 and 2 holds constant the pricing grid, thus isolating any pure "dark trading" effect. The exogenous shift in trading volume, random assignment of stocks into treatment groups, and the existence of a suitable counterfactual group (G2) present an unparalleled opportunity to study the *causal* effects of dark trading on market quality.

From the onset, controversy surrounded the inclusion of a *Trade-At* provision in the Tick Size Pilot. The SEC noted very clearly the relevance of the *Trade-At* provision when directing exchanges and FINRA to submit a tick pilot plan:

> The Commission believes that if trading volume in Test Group Two Pilot Securities moves to undisplayed trading centers, then including the trade-at requirement in Test Group Three could test whether trading remains on lit venues and what impact, if any, the migration of trading from lit venues to dark venues would have on liquidity and market quality for the Pilot Securities… (SEC, 2014, p. 36846).

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[7] Rindi and Werner (2017) discuss the background leading up to the SEC's tick size pilot program and provide a comprehensive analysis of pilot stocks verses controls. They show that stocks with increased tick sizes have greater quoted and effective spreads but also increased depth.

[8] There are several exemptions from trade-at, all of which generally follow exemptions to RegNMS Rule 611 ("trade-through"). Trade-at exemptions include block trades, fractional shares, trades during a locked market or self-help condition, trades part of a non "regular way" contract (i.e. not settled T+3), and stop trades. In addition, retail price improvement is exempt from pilot trading rules provided the inside quote is improved by at least a half penny. However, it is unclear how any of these exemptions might bias inference from our study.

As exchanges have long advocated tests involving a *Trade-At* provision (Lynch, 2015), it is perhaps not surprising the Pilot included this feature. Operators of dark pools naturally voiced strong opposition:

> We see no connection between the goal of the Pilot – widening tick sizes to determine the impact on small cap issuers and their securities – and the imposition of a Trade-At Requirement which is simply a measure to increase market share for [lit] exchanges (SIFMA, 2014).

As we show in Figure 1, the drop in dark trading market share was indeed large, swift, and long-lasting. We scrutinize the characterization of this change as an exogenous shock to dark trading in Section IV.

III. Data

III.a. Sample construction

A stock's eligibility for the Pilot program was determined over a "measurement" period from April 4 until September 2, 2016 in accordance with the following criteria:

- National Market System (NMS) common stocks trading publicly for at least six months prior to the beginning of the pilot
- Market capitalization of no greater than $3 billion
- Closing price of at least $2.00 on the last day of the measurement period
- Closing price of at least $1.50 on each day during the measurement period
- Average daily volume (*ADV*) of no greater than one million shares
- Volume weighted average price (*VWAP*) of at least $2.00

On September 3, 2016, the SEC published a list of 2,399 stocks that met the eligibility requirements and assigned each stock to three different tercile groups based on market capitalization, volume weighted average price, and average daily volume. These tercile assignments produce 27 unique fractile portfolios.[9] One thousand two hundred stocks were then randomly drawn from these fractile portfolios and assigned to one of the three mutually exclusive treatment groups (400 stocks in each group) described in Section II. The random draw was conducted such that there is an even distribution between each listing exchange in any treatment group. Remaining stocks comprised the control group.

We obtain a daily list of Pilot stocks, their corresponding group assignments (*i.e.* control group, G1, G2, or G3), and the effective date for each record from the listing exchanges (NYSE and NASDAQ).[10] From the list, we identify 2,388 unique firms during the period from September 2, 2016 until November 29, 2016 and match each firm's ticker symbol with CRSP in order to obtain exchange listing, sharecode, shares outstanding, and trading volume.[11] We then filter the sample to include only common shares (sharecode=10 or 11), leaving 2,026 unique firms.

We gather data necessary to construct measures of market quality (spreads, price impact, and variance ratios) as well as several control variables used in our regressions from the NYSE's daily millisecond trade and quote data (TAQ). To ensure the integrity of the TAQ data, we match trades and quotes following Holden and Jacobsen (2014) and exclude all trades executed before 9:30 am or after 4:00 pm, as well as those associated with the opening or closing auctions.[12] We also exclude executions exempt from the RegNMS Rule 611 (also known as the trade through

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[9] Portfolios containing less than ten stocks were combined with other portfolios containing under ten stocks until each portfolio contained at least ten stocks.

[10] Listing exchanges provide daily lists that reflect any updates to the sample groups that might arise from mergers, delistings, etc. A list of pilot stocks is also available from FINRA: www.finra.org/industry/tick-size-pilot-program.

[11] Dates for our study are chosen to ensure four weeks of market data before and after the staggered implementation of the 'Tick Size Pilot', which occurred from October 3 to 31, 2016.

[12] Trades that occur outside of the regular trading session are coded in TAQ with trade condition T or U. Auction trades are coded with trade conditions O and 6 on all exchanges except for NYSE. For NYSE listed securities the first and last regular session trades, which are not stop orders, executed with exchange code "N" identify NYSE auction trades.

rule), because these trades are not necessarily related to the prevailing quote at the time of the trade.[13] After requiring sufficient TAQ data to compute market quality measures each day, we are left with 1,993 firms in the final sample.

We identify dark venue executions as those with exchange code 'D' in TAQ.[14] This 'flagged' dark trading volume includes all trading within dark pools (i.e. registered alternative trading systems, ATS) as well as internalized trades at broker-dealers.[15] To assess the prevalence of dark trading volume for each stock and day, we calculate the dollar value traded in dark venues scaled by total traded dollar value (*DarkTrading)*. This proportion based measure is typical in the literature (e.g. O'Hara and Ye, 2011; Hatheway, Kwan, and Zheng, 2017) and serves as our primary independent variable of interest.[16]

III.b. Market Quality Measures

To assess market quality, we calculate daily spread measures and variance ratios using intraday trade and quote data from TAQ. Our spread measures include both quoted and effective spreads. We compute quoted spread (*QS*) as:

$$QS = \frac{NBO_t - NBB_t}{midpoint_t}, \quad (1)$$

[13] For example: stop, derivatively priced and prior reference price trades.
[14] This measure excludes executions against hidden orders on exchanges.
[15] We retain RegNMS exempt trades since these reflect trader decisions and without them we would have an incomplete picture of order flow allocation. Thus our measure encompasses all regular trading session transactions executed against undisplayed trading interest away from any exchange.
[16] We re-run all results using shares traded and find results that are both quantitatively and qualitatively similar to those presented using dollar values.

where *NBO* and *NBB* reflect the national best offer and bid price respectively, and *midpoint* is a simple average of the two. For each stock-day, we compute a time-weighted average of the quoted spread to ensure that longer persisting spreads are more heavily weighted than fleeting quotes.

While quoted spreads are often viewed as an accurate estimate of the cost of small market orders (Anand, et al, 2012), we also proxy for the realized cost of trade by calculating effective spreads. The effective spread (*ES*) compares the execution price of a trade to the prevailing midpoint at the time of trade, as follows:

$$ES = 2 * \frac{abs(price_t - midpoint_t)}{midpoint_t} \quad (2)$$

If the midpoint is a “fair price”, then the effective spreads measures a trader’s willingness to pay for immediacy.

We decompose the effective spread into the realized spread (*RS*) and price impact (*PI*).

$$RS = BuySell * 2 * \frac{price_t - midpoint_{t+s}}{midpoint_t} \quad (3)$$

$$PI = BuySell * 2 * \frac{midpoint_{t+s} - midpoint_t}{midpoint_t} \quad (4)$$

In the above equations, *price* is the price of an execution, *midpoint* is the average of the *NBO* and *NBB*, *t* is the time a trade occurred, and *BuySell* equals 1 (-1) if the trade is buyer (seller) initiated following the Lee and Ready (1991) algorithm.

Realized spreads compare the execution price at time *t* with the midpoint at a later time (*t* + *s*). We choose 5 minutes for *s*, which is a common choice in the literature (see, *e.g.*, Hendershott, Jones, and Menkveld, 2011). Fundamentally, this construct measures compensation for market makers or other liquidity providers. The other component of the effective spread, price impact, proxies for the effect a given trade has on the stock's price. The comparison of a future midpoint to the prevailing midpoint at the time of the trade allows us to infer the permanent price change attributable to a given trade. For each firm-day, we compute dollar-weighted averages for effective spread, realized spread, and price impact.

The final measure that we construct, the variance ratio, focuses on how efficiently stock prices incorporate new information. To the extent that stock prices fully and immediately impound new information, one should expect stock returns to follow a random walk and the variance in returns to scale linearly over time. Under these assumptions, the variance ratio (*VR*) serves as a viable proxy of price efficiency (Lo and MacKinlay, 1988). Specifically, we look at the ratio between 30-minute and 15-minute return variances as in O'Hara and Ye (2011):

$$VR = abs\left(\frac{Ret\_var30}{2*Ret\_var15} - 1\right) \tag{5}$$

We capture the variance of midpoint returns over 30 (15) minute periods as *Ret_var30* (*Ret_var15*). If prices follow a random walk, the variance of 30-minute returns should be twice that of 15-minute returns, and *VR* should be zero.

Given the nature of microstructure measures, throughout our analysis we winsorize all continuous variables at the 2.5th and 97.5th percentiles of the relevant sample.[17] We include a comprehensive list of all variable definitions in Table I.

[Insert Table I about here.]

[17] Results are qualitatively and quantitatively consistent when winsorizing at the 1st and 99th percentiles.

III.c. Summary Statistics

In Table II, we present summary statistics for all stocks included in the SEC pilot. Variables are measured during the twenty trading days before the 'Tick Size Pilot' began on October 3, 2016. In this period, pilot stocks and their corresponding groups were publicly known, but the various treatment effects had not yet been imposed. We calculate a time series average measure for each stock over the twenty-day period and report the cross-sectional average across all stocks in the sample. Average trade size, dark block trades, depths, market capitalization, traded value, and *VWAP* are reported as dollar values.

[Insert Table II about here.]

Table II confirms that sample stocks are small- to mid-capitalization firms with mean and median market capitalization of $626 million and $350 million, respectively. Stock price (*VWAP*) has a mean of $22.70 and an interquartile range of $8.27 to $30.62. Nasdaq-listed firms account for 69% of the sample and 26% of firms are listed on the NYSE. The average trade size for our sample stocks is $2,551. Quoted spreads average 0.78% ($0.12), while the average effective spread is 0.48% ($0.07). We also find that dark trading accounts for a sizeable fraction of sample firms' trading activity and is consistent with commonly-cited estimates for all stocks. The mean (median) value for the percentage of dark trading volume is 34% (32%) with an interquartile range of 26% to 41%.

IV. Empirical Analysis

IV.a. Research Design

Our research design exploits differential treatments across groups G2 and G3 in the 'Tick Size Pilot'. This setting naturally lends itself to a difference in difference framework since the only difference between the two groups is the *Trade-At* provision imposed on G3. Thus, any effects purely derived from the *Trade-At* provision should be detectable by differencing market quality measures between groups G2 and G3. Henceforth, we refer to G3 stocks as "treated" stocks, G2 stocks as "control" stocks, and the post-implementation period as the "treatment" period.

Formally, we estimate the following regression model using daily stock-level data:

$$Y_{i,t} = \beta_0 + \beta_1 TA_i + \beta_2 Post_t + \beta_3 TA_i * Post_t + \boldsymbol{\gamma X_{i,t}} + \epsilon_{i,t} \tag{6}$$

Variables designed to measure aspects of market quality (e.g., quoted spreads, effective spreads, etc.) are represented by *Y*. Stocks in the *Trade-At* treatment group (G3) have *TA* equal to one and control stocks (G2) have *TA* equal to zero. The indicator variable *Post* equals one after the 'Tick Size Pilot' is implemented and zero otherwise. Thus, our coefficient of interest, β_3, captures the marginal effect of treatment on the treated. The vector $\boldsymbol{X}$ contains a set of control variables we expand upon below. To the extent that stocks' assignment to G2 and G3 is truly random, the vector of control variables should not be necessary. Because the 'Tick Size Pilot' was implemented in staggered fashion between October 3 and October 31, 2016, we drop observations during the implementation period and consider the twenty trading days prior to October 3 as the pre-period and the twenty trading days following October 31 as the post period.[18]

IV.b. Parallel Trends Analysis

Establishing clean identification is of paramount importance for our study. We therefore begin with the parallel trends assumption, which is the key identification assumption for differences-in-differences analysis (Roberts and Whited, 2013). For our purposes, validating this

[18] We drop the shortened trading day on the Friday after Thanksgiving, November 25th, 2016.

assumption requires a detailed examination of the *first difference* – the difference between treated stocks and control stocks in the period prior to the Tick Size Pilot. The stratified random sampling method employed by the SEC provides reasonable confidence that the parallel trend assumption holds. Nevertheless, we empirically test for differences across control group (G2) and treatment group (G3). Limiting our sample to only G2 and G3 stocks narrows the sample to 660 unique firms (333 in G2 and 327 in G3).

We plot in Figure 1 the *DarkTrading* over the 40-day period surrounding treatment (20 days before and after treatment occurs), again noting we drop the staggered implementation period from October 3rd through October 31st. In the figure, day 1 corresponds to November 1, 2016, which is the first day the pilot is fully in force for all groups. We draw attention to the left-hand side of the figure, which reveals similar patterns in mean daily *DarkTrading* for stocks in the G2 and G3 groups leading up to the pilot. For both groups, the mean values are around 35% and we note that the time series plots are almost identical.

We next compare our variables of interest that measure dimensions of market quality across groups by estimating a differences in means regression, which is a simplified version of (6):

$$Y_{i,t} = \beta_0 + \beta_1 TA_i + \epsilon_{i,t}. \tag{7}$$

In this estimation, we use only daily firm-level observations from the twenty days in September 2016 leading up to the treatment period. Table III Panel A shows that dark trade ratio is statistically indistinguishable across groups prior to the pilot, as the estimate for β_1 is 57 basis points with a *t*-statistic of 0.69. We also compare characteristics used by the SEC in the stratified random sampling procedure (*e.g.* market capitalization, traded volume, and price). Mean differences of these variables, also reported in Panel A, affirm that the SEC's stratification approach effectively controlled for these variables; none significantly differ across groups G2 and G3. We report in Panels B and C differences in mean values of other trading environment variables and our main market quality measures (*e.g.*, effective spreads, quoted spreads, etc.). Four variables show marginally significant differences – dark block trades, effective spread, price impact, and quoted spread. For example, percentage effective spread in G3 is about nine basis points lower than in our control group (G2) with a *t*-statistic of 1.79. Similarly, quoted spread is also lower for G3 stocks. The magnitude of the difference is 14 basis points (*t*-statistic = 1.84)

[Insert Table III about here.]

While the statistical and economic similarities across groups prior to the pilot are comforting, the parallel trends assumption only requires that any difference (for our variables of interest) between control and treatment groups be constant over the pre-treatment time horizon. Visual inspection of Figures 1-3 suggests this to be the case for our main independent variable, *DarkTrading*, and two key market quality variables, effective spread and variance ratio. In particular, we are interested in the green line that plots the difference between 'control' (G2) and 'treatment' (G3) groups over the pre-treatment period – from 20 trading days before treatment until the treatment.

We turn now to formal statistical analysis and augment (7) as follows:

$$Y_{i,t} = \beta_0 + \beta_1 TA_i + \sum_{\tau \in (-3,-1)} \gamma_\tau W_\tau + \sum_{\tau \in (-3,-1)} \gamma_\tau \, W_\tau * \, TA_i + \epsilon_{i,t} \qquad (8)$$

where the dummy variables W_τ reference each of the three weeks prior to the pilot period and the intercept captures the fourth week prior to the pilot. Table IV displays the results. The most important numbers in the table are the coefficients on the interactions between the *TA* dummy and the week indicators.[19] Insignificant interaction terms reflect statistically indistinguishable trends across groups in the pre-Pilot period. And this is indeed what we find. For example, while *DarkTrading* drops and spreads increase significantly during the third week prior to the Pilot (approximately the second week in September) as indicated by the significant W_{-3} term, the changes are similar across groups—the $W_{-3} * TA$ interaction term is insignificant. Based on this analysis, we fail to reject the parallel trend assumption and believe assumptions for difference in difference analysis are satisfied. Moreover, these findings support the view that the SEC's pilot is not tainted by any obvious sample selection issues.

[Insert Table IV about here.]

IV.c. Trade-At as a Shock to Dark Trading

The most striking feature of Figure 1 is that on the first day of the pilot regime, *DarkTrading* for treated stocks (G3) drops from near 35% of value traded to about 23%. In stark contrast, dark trading for control stocks (G2) increases slightly. To put the magnitude of the shock to dark trading into context, we note that the time-series standard deviation of the dark trading ratio was 3% during the pre-period. Thus, the treatment represents a shock greater than four standard deviations. In addition to being large in magnitude, the difference in dark trading, between treatment and control, persists through the end of the 20-day window. We estimate our main

[19] Hastings (2004) uses a similar framework to analyze trends in gasoline prices around the vertical integration between retail gas stations and refiners.

differences-in-differences specification using *DarkTrading* as the dependent variable and present the results in the first column of Table V Panel A. The effect of treatment on the treated is contained within the coefficient estimate for the *TA * Post* term (the bottom row of the table). Not surprisingly, the change in dark trading is statistically significant. Consistent with Figure 1, the interaction coefficient reveals dark trading in Group 3 dropped by 12.1% (p-value<0.001). This move represents approximately a 34% decline from pre-treatment levels and validates our identification strategy of finding an exogenous shock to dark trading. Analogous language from an instrumental variables framework would state the enactment of *Trade-At* for the G3 group meets the relevance condition. Moreover, if dark trading has any impact on market quality, we deem a shock of this size more than sufficiently powerful to uncover the effect.

One immediate concern with our test design is that the *Trade-At* provision itself may incrementally affect market quality, dark trading effects aside. Such an effect would be akin to a violation of the exclusion condition in instrumental variables. We first explore this possibility by estimating Equation (6) with a host of trading characteristics through which a more general *Trade-At* effect might manifest: turnover, trade size, and *VWAP*. We use this analysis to contrast the sharp drop in dark trading volume to changes in other important variables that might be related to market quality. The balance of Table V Panel A contains these estimates.

[Insert Table V about here.]

Table V reveals no significant treatment effect on stock price (*VWAP*). The variables that do exhibit a treatment effect are turnover and trade size. Turnover decreased by about 4 basis points relative to the control group, significant at the 10% level, from a pre-treatment average of

59 basis points. Trade size for the treated group declines significantly by $105 when compared to the control group (the pre-treatment average is $2,841). In comparison to the drop in dark trading volume, these effects are quite small in magnitude. In summary, we find significant drops to dark trading, a small but significant decrease to trade sizes, and a marginal decline of turnover. We interpret these findings as strong support of *Trade-At* as a negative shock to dark trading with only modest impact on other facets of the trading environment.

A more specific concern is that *Trade-At* alters the competitive landscape among lit venues. Comerton-Forde, Gregoire, and Zhong (2018) discuss how inverted fee venues' potential sub-tick price improvements represent a competitive advantage, particularly when tick size is discrete and dark trading is constrained. Indeed, they show that inverted venue share increased for the *Trade-At* group under the Tick Pilot and argue that any effect of dark trading on market quality may be confounded by an inverted venue share effect.[20] In Figure 4, Panel A, we corroborate this result by showing inverted venue trading as a share of total trading increases substantially for group G3 relative to G2. We estimate (6) and show in Table V Panel B inverted share increases by 3.35%, and this change is statistically significant.

[Insert Figure 4 about here.]

However, this finding may be, at least in part, a mechanical result of the dramatic decline in trading on dark venues. If trading that shifts from dark to lit venues is simply allocated across various lit venues according to their pre-Pilot market share, every lit venue's post-Pilot share of total trading will increase. Whether the shift in trading from dark to lit venues is *disproportionately*

[20] Cox, Van Ness, and Van Ness (2017) find that trades and orders migrate from maker-taker to inverted fee venues for stocks with tick size increases.

allocated to inverted venues is an important empirical question that critically affects our interpretations. To address this issue, we compute the inverted venue trading as a fraction of lit exchange volume. We plot daily values of this variable for groups G2 and G3 around the Pilot in Figure 4 Panel B. The relative change in the re-computed inverted venue share is visually smaller than the shift depicted in Panel A. We test for statistical differences by estimating (6) with inverted share of lit trading as the dependent variable. The coefficient estimate is approximately cut in half to 1.61%. While statistically significant, the coefficient's economic magnitude is small relative to the similar coefficient explaining dark trading. The change in inverted share is (1.61% / 12.42% =) 13% of its pre-Pilot mean and one-third of a standard deviation as reported in Table II. We also note that both groups G2 and G3 have a substantial increase in inverted share of lit trading as the coefficient estimate for the *Post* dummy is 9.33% and highly significant, which represents a (9.33% / 12.00% =) 78% increase relative to the pre-Pilot mean. Thus, any competitive advantage of inverted venues appears more manifest by the increased tick size that occurs for both groups G2 and G3, where trades must occur at nickel increments or greater than through an incremental effect of *Trade-At*. We associate the first order effect of *Trade-At* with changes to dark volume, not to inverted venue competitiveness.

A second specific concern is that researchers have found a positive relationship between lit fragmentation and liquidity (Degryse, De Jong, and van Kervel 2014). If either *Trade-At* itself or the associated drop in dark trading induces a reallocation of orders among lit venues (inverted venue effects aside), such that relative market shares are changed, then confounding inferences could emerge. The extent to which trading activity amongst lit venues changes is an empirical question.

To offer a more holistic glimpse of how *Trade-At* affects trade dispersion across lit markets, we estimate the model with lit fragmentation and the number of lit venues as dependent variables. Our fragmentation measure is similar to the Herfindahl metric in Degryse, De Jong, and van Kervel (2014).[21] We calculate the inverse of an HHI based measure using the market share of dollar volume per displayed venue. Therefore, the lower bound (one) indicates all trades occurred within a single venue, while the upper bound is the number of lit venues and would indicate equal market share among them. For the share-based measure, the interaction coefficient is 0.10 and statistically significant, indicating trading becomes slightly more concentrated on lit venues. This effect is economically small, as the point estimate is about one-eighth of the standard deviation reported in Table II. The lit venues result is similar. While the number of lit venues show a significant drop, the economic magnitude of 0.08 fewer lit venues is trivial. For a trade-based fragmentation measure (not reported), the coefficient on *TA * Post* is economically quite small and statistically indistinguishable from zero. We interpret these regressions along with the others in Table V Panel B together as evidence that treatment reduced dark trading and whatever order flow was reallocated among lit venues did not materially alter the relative allocation of trades among lit exchanges. Collectively, the results in this section bolster confidence in our research design and the feasibility of using the *Trade-At* pilot as a natural experiment.

IV.d. Main Results

The sharp contrast between Figure 1 and Figures 2 and 3 succinctly summarizes our main message. Simply put, the drastic shock to dark trading at the Pilot's initiation is not mirrored by

[21] They use one minus the HHI calculation, but we use one divided by HHI. The former allows for better comparison of relative fragmentation over various capital markets, while ours does not normalize. This inverse measure differentiates between dispersion among a given group of venues and dispersion over all venues in a given market.

any meaningful economic change to market quality metrics. Figure 2 shows effective spreads widening for both control and treatment stocks, but to a similar degree in each group. Similarly, Figure 3 shows little change to the variance ratio for either treatment or control samples. Overall these graphs present clear visual evidence of a shock to dark trading, but little indication of differential impacts to market quality between treatment and control.

We estimate Equation (6) with market quality measures as the dependent variable. We present the results in Table VI. The first five columns of the table report estimates of coefficients using the model from Equation (6) with no controls, where our dependent variables include effective spread, quoted spread, realized spread, price impact, and the variance ratio. Consistent with the findings of Rindi and Werner (2017), the first two columns of the table provide evidence that effective and quoted spreads rise for both groups 2 and 3 after quotes and trades are required to occur in nickel increments. The point estimates for the *Post* dummy coefficient are 17.8 basis points and 25.4 basis points for effective and quoted spreads, respectively. However, as with Table V, we are primarily interested in the coefficient on *TA * Post*, which describes the unique effect of *Trade-At.* Quite interestingly, while the introduction of *Trade-At* leads to a precipitous decline in dark trading volume, there is no discernible effect on effective or quoted spreads. The interaction coefficients are -0.47 basis points and 2.97 basis points, both indistinguishable from zero.

We also investigate the components of the effective spread – the realized spread and price impact. The dramatic shift in dark trading could potentially affect either. Changes in competition for liquidity provision would likely manifest in realized spread, which is commonly viewed as a proxy for market making profit. Forced pooling of informed and uninformed order flow could drive changes in price impact, a common proxy for adverse selection. The results in Table VI reveal, however, that neither measure is affected by the negative shock to dark trading. While realized spread and price impact increase statistically for each group of stocks, the coefficients on *TA * Post* are once again both insignificant. The only market quality variable that appears to be affected is the variance ratio, which increases by 0.0173 (*t*-statistic=1.88) for G3 when compared to G2. While this change is only marginally significant, this is modest evidence of a loss to price efficiency following restrictions on dark trading.

[Insert Table VI about here.]

In light of the intense policy debate on dark trading, these results are surprising. The sharp exogenous drop in dark trading had no impact on the cost to trade, but made prices only somewhat less efficient. These results stand in stark contrast to those in HKZ, who argue dark trading is detrimental to overall market quality. They are somewhat more in line with of Ohara and Ye's (2011) message that dark trading has neutral to slight positive economic effect on market quality.

While our primary regressions produce valid inference under the assumption that stock assignments to G2 and G3 are truly random, we attempt to bolster confidence in our tests by including a number of control variables in our difference-in-difference specification, including market capitalization, price, and trade size, which Ohara and Ye (2011) show to be related to dark trading, and trading volume, which is known to be related to spread measures. We note, however, that the SEC stratified the sample based on three of these four variables, so the sample selection already accounts for them in part. We follow HKZ and select additional control variables that aim to capture inputs to trading decisions.

One factor that can influence order routing decisions and execution costs is the availability of large blocks of liquidity. Specifically, the potential to trade against a block in dark venues might attract order flow. If there are any systematic relationships between the expected cost of an order and the availability of dark block liquidity, we must control for block trades to avoid such effects from being attributed to all dark trading. We include a day-stock measure of dark block trading calculated as the value of dark trades within the top one percent of trade size scaled by total dark traded value.

Another important factor that may influence order routing is trading risk, broadly defined as the risk that orders will execute at disadvantaged prices due to adverse selection or the combination of poor timing and extensive volatility. Orders that are riskier to trade typically generate higher implementation costs. If traders route more orders to lit venues when trading risk is high, then a relationship between risk and transaction costs could be erroneously attributed to dark trading. HKZ emphasize these controls directly influence inferences, so we include their measures to control for trading risk. The first is a volatility measure, computed as the standard deviation of 1-second midpoint returns for the 30 second period following each trade. We average this standard deviation measure over all trades within a stock-day to generate a trade-weighted measure of volatility. The second is the probability of informed trading (*PIN*) as constructed in Easley, Keifer, and O'Hara (1997), which we estimate using a rolling window of the prior 30 trading days.

The remaining five columns on the right side of Table VI report the same difference in difference regression with the addition of controls discussed above. With added control variables, our results are nearly identical. We still find no impact to trading costs with a modest reduction in price efficiency, though only marginally significant. These findings imply dark trading has only beneficial effects on market quality, though these effects are mild. Even following a four standard deviation shock to dark trading, there are no discernible impacts on the cost to trade.

IV.e. Subsample analyses

Previous studies show differential effects from dark trading in subsamples. For example, O'Hara and Ye (2011) find that their main results, from a 2008 sample of dark trading, do not hold across listing exchanges or market capitalization subsamples. Similarly, HKZ, obtain conflicting results using a 2011 sample depending on whether they examine stocks with wide or narrow quoted spreads. Thus, despite our sample being limited to smaller stocks, there is a chance the relationship between dark trading and market quality is concentrated in one particular set of stocks, and pooling all stocks introduces noise that prevents precise inferences. To address this concern, we group our sample on various stock characteristics.

We form subsamples using market capitalization, turnover, dark trading, inverted venue trading, and lit fragmentation. There is already prior evidence that size and quoted spreads may impact the relationship between dark trading and execution costs.[22] However, rather than use quoted spreads which have a mechanical impact on effective spreads, we examine factors that may be individually related to all spread measures. High and low turnover stocks may react differently to shocks to dark trading. The former may not be impacted while the latter may face increasing search costs after a drop to dark trading. Moreover, stocks more frequently traded in dark venues may have faced the largest incremental shock leading to a differential impact to market quality. Samples cut on inverted trading volume aim to address concerns regarding possible effects of finer pricing grids within inverted trading venues. We also include lit fragmentation to generate subsamples, in case the prevalence of ex-ante trade dispersion proxies for differences in the way traders decide where to route their orders. For each of these five stock traits we generate

[22] For example, O'Hara & Ye (2011); Haslag & Ringgenberg (2015); and Hatheway, Kwan, & Zheng (2017).

subsamples following the same procedure. We calculate the median value of each variable over the second quarter of 2016 (or end of June 2016 for market capitalization), and then split the sample into stocks with values above that median (*High*) and below (*Low*). We then rerun the difference in difference regression from Table V, including all controls variables.

After splitting our sample on various trading characteristics, we find little evidence of an impact on market quality from the shock to dark trading. Table VII reports estimates for the marginal effect of treatment (i.e. *TA * Post*) using a model including the same control variables from Table VI, but omits reporting coefficient estimates on control variables for brevity. In most cases, the shock to dark trading has no effect on market quality. For example, only two subsamples with statistically significant treatment effects on effective spread are stocks with high lit fragmentation or low dark trading. Both coefficients for these subsample are negative and only marginally significant. Additionally, we find lower price efficiency (i.e. larger variance ratios), among stocks with higher market capitalization and lower dark trading.

[Insert Table VII about here.]

Taken together, our findings indicate dark trading is benign and even mildly beneficial for some stocks. These findings support the view that within US equities any negative effects from dark trading are offset or dominated by competition among liquidity suppliers and trading venues. We interpret these findings as convincing evidence that dark trading does not increase implementation costs or discourage liquidity provision.

V.c. Replication of HKZ

Earlier studies have been limited to drawing inferences about dark trading without the benefit of a natural experiment. Without an exogenous shock, these studies have attempted to control for potential endogeneity in the form of reverse causality and selection bias. O'Hara and Ye (2011) employ a two-stage Heckman selection model. HKZ augment this method by adding variables to control for traders' expectations regarding trading risk. We use our setting to assess the suitability of these methods to infer the relationship between market quality and dark trading.

We estimate a simple OLS regression and compare the results to a two-stage Heckman selection procedure using the sample of G2 and G3 over the pre-Pilot period. Importantly, these estimations use the exact same firms as our differences-in-differences models. Moreover, as indicated by the summary statistics we report in Table II, there exists substantial cross-sectional variation in dark trading fraction. The first stage of the Heckman selection model is a probit model that predicts *DarkTrading*. This facilitates the calculation of an inverse mills ratio that, when included in the second stage, corrects for the possibility that traders allocate their orders between lit and off exchange venues based on the difficulty of the order. We present the results in Table VIII. For both the OLS model and the two-stage model, *DarkTrading* loads highly significant and positive, implying dark trading increases effective spreads. To interpret marginal effects, a 10% shock to *DarkTrading* predicts effective spread will increase 4.46 bps (4.23 bps). These estimates are consistent with HKZ, but they contradict our difference in difference analysis.

[Insert Table VIII about here.]

This comparison of methods highlights the challenges of empirical research within an area fraught with endogeneity. We agree with previous studies that there is a high likelihood that an

OLS analysis may produce biased coefficient estimates due to endogeneity. However, the Heckman correction model may not sufficiently mitigate these concerns. For the Heckman correction to be correctly specified, there must be at least one unique regressor in the first stage regression that impacts the suspect variable (e.g. dark trading) but does not impact the second stage dependent variable (e.g. effective spreads). The challenge this assumption presents has been discussed throughout numerous papers grappling with the difficulty of finding suitable instruments. The SEC tick pilot provides an invaluable opportunity to circumvent these obstacles. As beneficiaries of such pilot programs, we hope regulators continue to make use of natural experiments to shed light on causal relationships within market microstructure.

V. Conclusion

Our main message is that a large shift in trading from dark to lit venues around the 2016 Tick Size Pilot had no meaningful impact on standard measures of market quality. This finding should inform policy makers worldwide in the midst of ongoing discussion of the potential benefits and dangers of dark trading. The academic literature to date lacks a cohesive empirical voice, no doubt due to the endogenous nature of trade routing decisions, as well the fact that dark venues, in addition to lacking pre-trade transparency, often utilize a more granular pricing grid than is available in lit markets. We circumvent these concerns by exploiting an exogenous shock to dark trading that, when compared to the appropriate counterfactual, is distinct from any differences in the pricing grid.

We acknowledge our insignificant results have alternative interpretations. For example, the shift in dark trading may result in countervailing and roughly offsetting effects on market quality. However, when we analyze changes in other trading characteristics (*e.g.*, turnover, inverted venue share of lit trading, and the dispersion of trading across lit venues) that may relate to market quality, we find that such changes, when statistically significant, are economically modest compared to the drastic shift in dark trading. Thus, while we cannot rule out a perfectly offsetting effect, we find this interpretation less plausible than the simple message that dark trading is mostly benign.

It is important to note the conclusion that dark trading is largely innocuous to market quality does not imply that it is either inconsequential or uninteresting. For the 327 G3 firms over the twenty days following the Pilot's implementation, aggregate dark trading drops by about $4.5 billion from its level during the twenty days prior to the pilot. From the perspective of dark pool operators and exchanges alike, the ultimate destination of these trades and policies that alter this flow are quite meaningful as it determines who receives rents from market-making. Moreover, since trading algorithms often employ top-level choices of whether to include dark venues, frictions that alter the myriad of routing decisions may prove costly by way of implementation.

Finally, our insignificant results prompt additional analysis at the trader level. None of the various metrics we calculate represent sufficient statistics for the welfare of a representative trader. Data at the trader level would be useful in this regard as one could construct measures that better describe realized investor experience. The work of Jones and Lipson (2001) examining how the change from eighths to sixteenths affected institutional trading costs offers a useful template. We encourage future researchers to follow this path.

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Appendix A: Examples of Trade-at

Example 1

The NBBO for Pilot Security ABC is $20.00 x $20.10. Trading Center 1 is displaying a 100-share protected bid at $20.00. Trading Center 2 is displaying a 100-share protected bid at $19.95. There are no other protected bids. Trading Center 3 is not displaying any shares in Pilot Security ABC but has 100 shares hidden at $20.00 and has 100 shares hidden at $19.95. Trading Center 3 receives an incoming order to sell for 400 shares. To execute the 100 shares hidden at $20.00, Trading Center 3 must respect the protected bid on Trading Center 1 at $20.00. Trading Center 3 must route a Trade-at Intermarket Sweep Order to Trading Center 1 to execute against the full displayed size of the protected bid, at which point Trading Center 3 is permitted to execute against the 100 shares hidden at $20.00. To execute the 100 shares hidden at $19.95, Trading Center 3 must respect the protected bid on Trading 19 Center 2 at $19.95. Trading Center 3 must route a Trade-at Intermarket Sweep Order to Trading Center 2 to execute against the full displayed size of the protected bid, at which point Trading Center 3 is permitted to execute against the 100 shares hidden at $19.95.

Example 2

The NBBO for Pilot Security ABC is $20.00 x $20.10. Trading Center 1 is displaying a 100-share protected bid at $20.00. Trading Center 2 is displaying a 100-share protected bid at $20.00. Trading Center 2 also has 300 shares hidden at $20.00 and has 300 shares hidden at $19.95. Trading Center 3 is displaying a 100-share protected bid at $19.95. There are no other protected bids. Trading Center 2 receives an incoming order to sell for 900 shares. Trading Center 2 may execute 100 shares against its full displayed size at the protected bid at $20.00. To execute the 300 shares hidden at $20.00, Trading Center 2 must respect the protected bid on Trading Center 1 at $20.00.

Trading Center 2 must route a Trade-at Intermarket Sweep Order to Trading Center 1 to execute against the full displayed size of Trading Center 1's protected bid, at which point Trading Center 2 is permitted to execute against the 300 shares hidden at $20.00. To execute the 300 shares hidden at $19.95, Trading Center 2 must respect the protected bid on Trading Center 3 at $19.95. Trading Center 2 must route a Trade-at Intermarket Sweep Order to Trading Center 3 to execute against the full displayed size of Trading Center 3's protected bid, at which point Trading Center 2 is permitted to execute against the 300 shares hidden at $19.95.

Example 3

The NBBO for Pilot Security ABC is $20.00 x $20.10. Trading Center 1 is displaying a 100-share protected bid at $20.00. Trading Center 1 is also displaying 300 shares at $19.90 on an SRO quotation feed. Trading Center 2 is displaying a 100-share protected bid at 20 $19.95. Trading Center 2 is also displaying 200 shares at $19.90 on an SRO quotation feed and has 200 shares hidden at $19.90. Trading Center 3 is displaying a 100-share protected bid at $19.90. There are no other protected bids. Trading Center 2 receives an incoming order to sell for 700 shares. To execute against its protected bid at $19.95, Trading Center 2 must comply with the trade-through restrictions in Rule 611 of Regulation NMS and route an intermarket sweep order to Trading Center 1 to execute against the full displayed size of Trading Center 1's protected bid at $20.00. Trading Center 2 is then permitted to execute against its 100-share protected bid at $19.95. Trading Center 2 may then execute 200 shares against its full displayed size at the price of Trading Center 3's protected bid. To execute the 200 shares hidden at $19.90, Trading Center 2 must respect the protected bid on Trading Center 3 at $19.90. Trading Center 2 must route a Trade-at Intermarket Sweep Order to Trading Center 3 to execute against the full displayed size of Trading Center 3's protected bid, at which point Trading Center 2 is permitted to execute against the 200 shares hidden at $19.90. Trading Center 2 does not have to respect Trading Center 1's displayed size at $19.90 for trade-at purposes because it is not a protected quotation.

Figure 1
The effect of *Trade-At* on dark trading

This figure plots the mean share of dollar volume executed in dark trading venues for our control and treatment stocks. G2 represents our control stocks and G3 represents our treatment group. We also plot the difference in dark market share, between control and treatment groups. The tick size pilot was implemented gradually from October 1st until October 31st, which was the first trading day in which the pilot is fully enacted. We drop the implementation period from our data but present this period by a gray vertical bar. The plot is in calendar time, tracking 20 days up until implementation begins, and 20 days subsequent to the pilot being fully implemented. Dark dollar volume is calculated as value of dark traded value scaled by total traded value (winsorized at 2.5th and 97.5th percentiles).

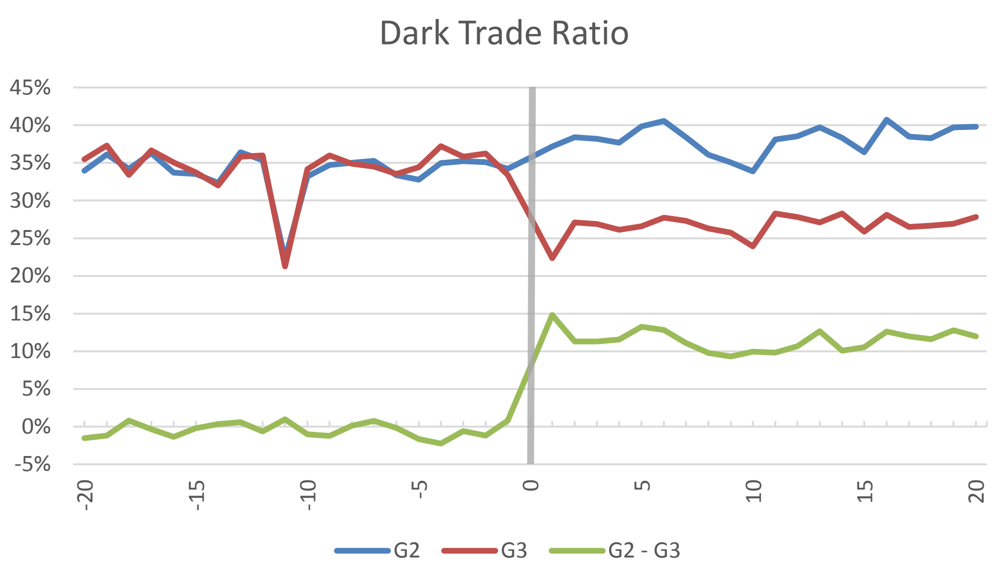


Figure 2
The effect of *Trade-At* on transaction cost

This figure plots the mean effective spread of stocks for our control and treatment stocks. G2 represents our control stocks and G3 represents our treatment group. We also plot the difference in effective spread, between control and treatment groups. The tick size pilot was implemented gradually from October 1st until October 31st, which was the first trading day in which the pilot is fully enacted. We drop the implementation period from our data but present this period by a gray vertical bar. The plot is in calendar time, tracking 20 days up until implementation begins, and 20 days subsequent to the pilot being fully implemented. Effective spread is round trip, dollar weighted, scaled by midpoint at trade time, and expressed in basis points (winsorized at 2.5th and 97.5th percentiles).

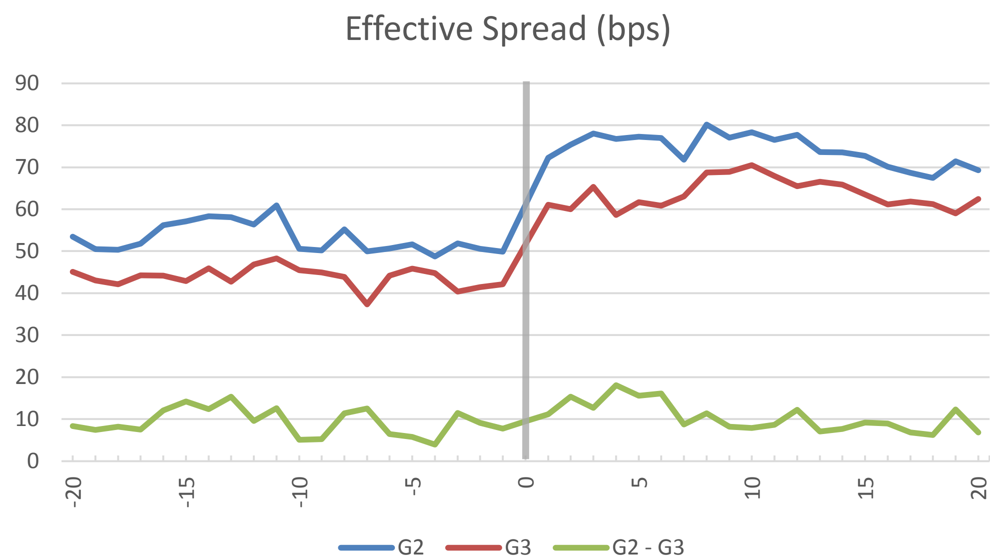


Figure 3

The effect of *TradeAt* on price efficiency

Panel A plots the share of dollar volume executed on inverted venues scaled by total value traded. Panel B plots the share of dollar volume executed on inverted venues scaled by all lit executions. Panel C plots differences between control and treatment groups for: dark market share, inverted market among lit venues, and inverted market share over all venues. G2 represents our control stocks and G3 represents our treatment group. The pilot was implemented gradually from October 1st until October 31st, the first trading day in which the pilot is fully enacted. We drop the implementation period but presented this period by a gray vertical bar. The plot is in calendar time, tracking 20 days up until implementation begins, and 20 days subsequent to the pilot being fully implemented. All variables are winsorized at 2.5th and 97.5th percentiles.

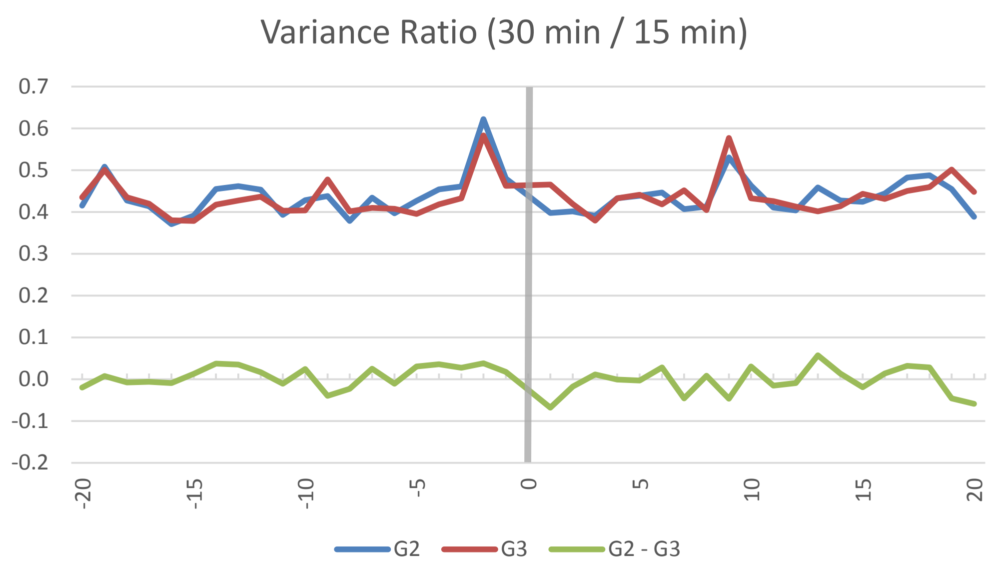
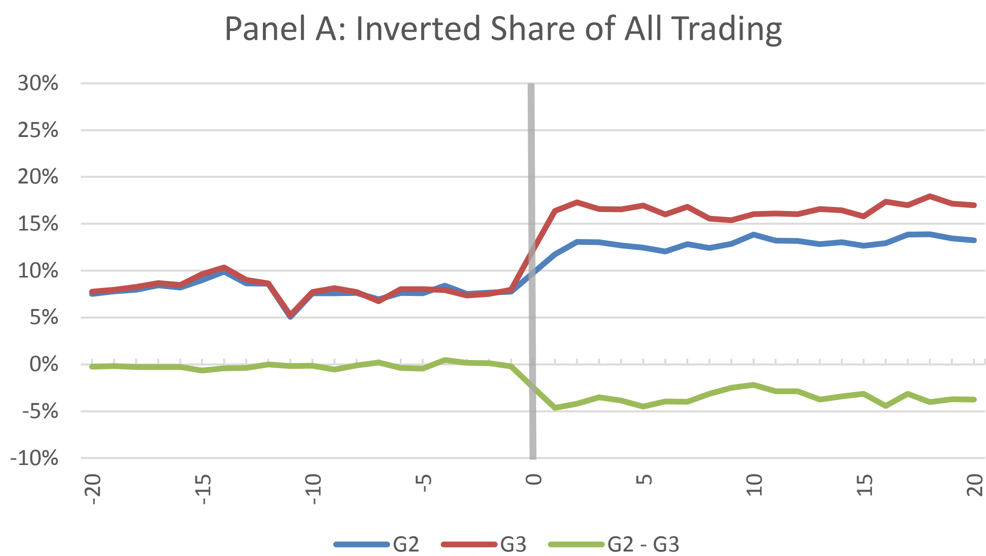
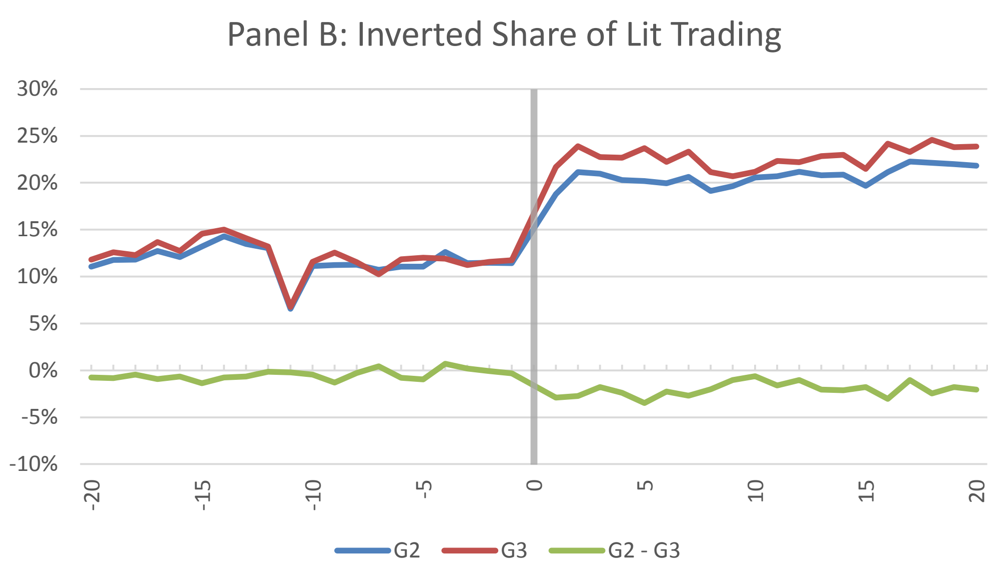
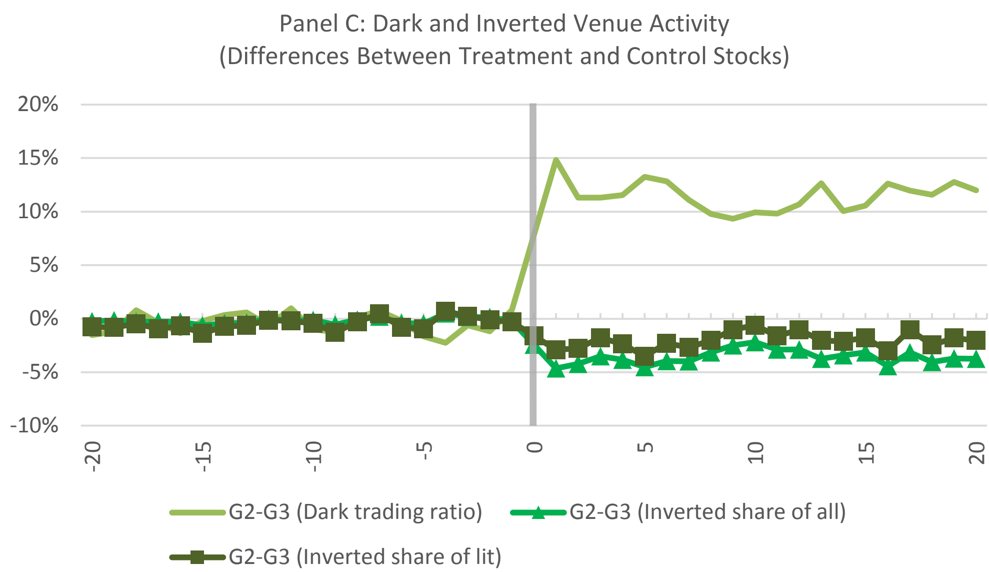


Figure 4
Comparing the effects of *TradeAt* between dark and inverted trading

Panel A plots the share of dollar volume executed on inverted venues scaled by total value traded. Panel B plots the share of dollar volume executed on inverted venues scaled by all lit executions. Panel C plots differences between control and treatment groups for: dark market share, inverted market among lit venues, and inverted market share over all venues. G2 represents our control stocks and G3 represents our treatment group. The pilot was implemented gradually from October 1st until October 31st, the first trading day in which the pilot is fully enacted. We drop the implementation period but presented this period by a gray vertical bar. The plot is in calendar time, tracking 20 days up until implementation begins, and 20 days subsequent to the pilot being fully implemented. All variables are winsorized at 2.5th and 97.5th percentiles.







Panel C: Dark and Inverted Venue Activity
(Differences Between Treatment and Control Stocks)
20%
15%
10%
5%
0%
-5%
-10%
-20
-15
-10
-5
0
5
10
15
20
G2-G3 (Dark trading ratio)
G2-G3 (Inverted share of all)
G2-G3 (Inverted share of lit)

Table I - Variable Definitions

	Variable	Description	Source
Variables of interest	*DarkTrading*	*Dark Trading Ratio*. Dollar value traded in undisplayed markets (identified in TAQ as execution destination equals 'D'), divided by total consolidated dollar value traded. Measure is at the stock day level using all trades executed during the regular trading session.	TAQ
	AskDepth	Average value of interest quoted at the national best offer price (NBO), during the regular trading session, calculated as the daily time-weighted mean per stock.	TAQ
	BidDepth	Average value of interest quoted at the national best bid price (NBB), during the regular trading session, calculated as the daily time-weighted mean per stock.	TAQ
	ES	*Effective spread*. Calculated as the absolute value of twice the difference between execution price and the prevailing midpoint (trades exempt from RegNMS rule 611 are excluded). Dollar-weighted averages are calculated for each stock-day. Expressed either in dollars or as a percentage scaled by the prevailing midpoint.	TAQ
	PI	*Price impact*. Calculated as a buy/sell indicator multiplied by twice the difference between execution price and prevailing midpoint five minutes after the trade. Dollar-weighted averages are calculated for each stock-day. Expressed either in dollars or as a percentage scaled by the prevailing midpoint at the time of execution.	TAQ
	QS	*Quoted spread*. Calculated as the NBO minus NBB, for each consolidated quote observed. Time-weighted averages are computed for each-stock day. Expressed either in dollars or as a percentage scaled by the prevailing midpoint.	TAQ
	RS	*Realized spread*. Calculated as a buy/sell indicator multiplied by twice the difference between midpoint five minutes after the trade and the prevailing midpoint at execution time. Dollar-weighted averages are calculated for each stock-day. Expressed either in dollars or as a percentage scaled by the prevailing midpoint at the time of execution.	TAQ
	VR	*Variance ratio*. Calculated as absolute value of the daily mean ratio of variance of 30 second midpoint returns divided by twice the variance of 15 second midpoint returns, minus one.	TAQ

Table I (Cont.)

	Variable	Description	Source
Control variables	*DarkBlockRatio*	Calculated as dark block trades scaled by total dollar volume traded in dark venues per stock day, using all regular session executions.	TAQ
	DarkBlocks	Measure of day stock level block trades in undisplayed markets, calculated as the sum of dollar value of block trades, where block trades are any regular session execution valued at greater than the top 1% of trades evaluated over the period of April 1st through June 30th 2016.	TAQ
	NYSE	Dummy variable set to one if a stock is listed on the NYSE as of October 31, 2016.	CRSP
	IdioVol	Proxied using the daily mean per stock of standard deviations of midpoint point returns using the prevailing quote at each second over the 30 seconds following each trade during the regular trading session (excluding trades exempt from RegNMS rule 611).	TAQ
	InvShare_lit	Measure of trading at venues with inverted fees, i.e. liquidity suppliers pay a fee on executions and liquidity demanders receive a rebate. Calculated as the dollar value traded in inverted markets (during our sample period BATS-Y, EDGA, and Nasdaq BX are the only inverted venues in operation), divided by total consolidated dollar value traded across all displayed trading venues. Measure is at the stock day level using all trades executed during the regular trading session.	Derived
	InvShare_all	Measure of trading at venues with inverted fees, i.e. liquidity suppliers pay a fee on executions and liquidity demanders receive a rebate. Calculated as the dollar value traded in inverted markets (during our sample period BATS-Y, EDGA, and Nasdaq BX are the only inverted venues in operation), divided by total consolidated dollar value traded. Measure is at the stock day level using all trades executed during the regular trading session.	Derived
	LitFrag_value	Measure of dispersion of trades during the regular trading session across displayed markets. Calculated as the inverse of an HHI measure using relative market shares of exchange venues. Market shares are calculated per stock venue day as executions in a given exchange venue divided by the total executions across all exchanges. Each stock venue day market share is squared and summed per stock day. Our final measure is the inverse of this stock day sum, i.e. one divided by the sum. This measure is bounded by one (perfectly consolidated) and the total number of venues that had a least one execution in any stock that day (perfectly fragmented).	TAQ
	LitFrag_trades	Calculation of lit fragmentation that measures executions as the number of trades executed.	TAQ
	LitVenues	Number of lit venues with trades.	TAQ

Table I (Cont.)

	Variable	Description	Source
Control variables	*MktCap*	Market capitalization is the product of shares outstanding and daily closing price, for pre period evaluations, we use the last market capitalization available from CRSP in June 2016. When used as a regressor, we take the natural log.	CRSP
	PIN	Measure of ex-ante trading risk from asymetric information that estimates the probability of informed trading (Pin) following Easley, Kiefer, & O'Hara (1997). Trades are categorized as buyer or seller initiated following Lee & Ready (2001), quotes and trades are matched following Holden & Jacobsen (2014), and trades exempt from RegNMS rule 611 are excluded.	TAQ
	TradeSize	Dollar value executed scaled by number of trades executed, measured at a stock day level.	TAQ
	TradeSizeRatio	Measures the trade size for a given stock day as compared to the typical trade size in that stock. Calculated as trade size for a given stock day divided by the mean trade size for that stock over the period.	TAQ
	TradedShares	Sum of shares executed.	TAQ
	TradedValue	Sum of dollar value executed. When used as a regressor, we take the natural log.	TAQ
	Trades	Count of number of executions.	TAQ
	Turnover	Measures daily trading activity per stock, calculated as the sum of shares executed divided by shares outstanding.	CRSP, TAQ
	UninfSupply	Proxied using the share of stock day order imbalances not explained by stock day returns. Calculated as the residual from a pooled regression over all stock days for a given period, regressing absolute dollar value imbalances on absolute returns, following Hatheway, Kwan, & Zheng (2017).	CRSP, TAQ
	VWAP	Volume weighted average prices are calculated using all executions during the regular trading session, scaling dollar value executed by shares executed.	TAQ

Table II - Summary Statistics

This table reports summary statistics for the cross-section of stocks in the SEC's tick pilot. Stocks in Pilot groups G1, G2, and G3 as well as control firms are includes. The variable *MktCap* and the *NYSE* dummy are measured as of June 30, 2016. All other variables are first averaged at the stock level over the 20 day period before the pilot (September 2nd through the 30th, 2016). Variables are as defined in Table I.

Panel A	**N**	**Mean**	**Std Dev**	**Q1**	**Median**	**Q3**
MktCap	1,993	626,408	661,850	124,905	350,477	931,673
NYSE	1,993	0.26	0.44	0.00	0.00	1.00
Trades	1,993	1,440	1,544	216	914	2,142
TradedValue	1,993	4,625,365	6,467,941	314,860	1,791,154	6,093,819
Turnover	1,993	0.0056	0.0049	0.0020	0.0043	0.0074
VWAP	1,993	22.70	19.56	8.27	16.76	30.62

Panel B	**N**	**Mean**	**Std Dev**	**Q1**	**Median**	**Q3**
DarkTrading	1,993	0.3398	0.1064	0.2600	0.3176	0.4087
DarkBlocks	1,993	527,226	808,123	36,955	191,816	649,844
LitFrag_Trades	1,993	4.04	1.28	3.18	3.75	5.11
LitFrag_Value	1,993	3.26	0.88	2.68	3.21	3.80
InvShare_lit	1,993	0.1190	0.0481	0.0820	0.1286	0.1523
InvShare_all	1,993	0.0799	0.0374	0.0482	0.0882	0.1076
TradeSize	1,993	2,551	1,748	1,262	2,076	3,261

Panel C	**N**	**Mean**	**Std Dev**	**Q1**	**Median**	**Q3**
ES (bps)	1,993	48.54	63.40	9.75	20.55	57.35
ES ($)	1,993	0.07	0.10	0.02	0.03	0.07
QS (bps)	1,993	78.86	99.17	17.57	36.02	92.92
QS ($)	1,993	0.12	0.16	0.03	0.06	0.14
PI (bps)	1,993	19.34	16.69	7.75	13.35	25.15
RS (bps)	1,993	28.21	48.50	1.49	5.67	31.01
VR	1,993	0.4362	0.0844	0.3788	0.4328	0.4913

Table III - Treatment and Control Differences, Pre-Pilot Period

This table reports estimates of the differences in means regression in Equation (8) using observations for the treatment group (G3) and control group (G2) only. For all variables except *MktCap*, which is sampled on June 30, 2016, the sample includes daily stock-level observations from the 20 day period prior to pilot launch (September 2nd through the 30th, 2016). The indicator variable TA is set to one for stocks in group G3 and zero for stocks in group G2. All variables are as defined in Table I. Statistical significance is denoted by *, **, and *** to indicate significance at the 0.10, 0.05, and 0.01 levels, respectively. Standard errors are presented in parentheses.

Panel A	**Intercept**	***TA***
DarkTrading	0.3412***	0.0057
	(.0059)	(.0083)
Mktcap	585,100***	68,160
	(35,230)	(50,060)
TradedValue	4,356,000***	728,200
	(361,500)	(513,600)
VWAP	21.87***	1.39
	(1.06)	(1.51)
Panel B	**Intercept**	***TA***
TradeSize	2,495***	150
	(94.95)	(134.89)
DarkBlocks	479,900***	130,000**
	(46,267)	-65,730
LitFrag_Value	3.26***	0.03
	(.05)	(.07)
InvShare_lit	0.1168***	0.0047
	(.0026)	(.0037)
InvShare_all	0.0790***	0.0018
	(.0020)	(.0029)
Panel C	**Intercept**	***TA***
ES (bps)	54.03***	-8.99*
	(3.54)	(5.02)
QS (bps)	86.96***	-14.40*
	(5.52)	(7.84)
PI (bps)	21.16***	-3.11**
	(.97)	(1.37)
RS (bps)	31.08***	-4.74
	(2.64)	(3.76)
VR	0.4401***	-0.0081
	(.0048)	(.0068)
AskDepth ($)	6,011***	285
	(205)	(291)
BidDepth ($)	5,785***	299
	(189)	(269)

Table IV - Pre-Pilot Trends

This table reports estimates of Equation (7) by regressing the dependent variables in the top row on indicator variables for stocks assigned to the treatment group (*TA*), week fixed effects for all four of the five day periods before the pilot began, and interactions between the treatment indicator variable and each week fixed effect. The sample includes stock-day observations for treatment (G3) and control (G2) stocks for the 20-day period prior to the Pilot (September 2nd through September 30th, 2016). The weekly dummy indicators W_{-k} for k = 1, 2, 3 refer to observations k weeks prior to the Pilot. Observations from the days prior to the Pilot are represented by the Intercept. Variables are as defined in Table I. All variables are winsorized at the 2.5th and 97.5th percentile. Statistical significance is denoted by *, **, and *** to indicate significance at the 0.10, 0.05, and 0.01 levels, respectively. Standard errors, shown in parentheses, are clustered by firm.

	DarkTrading	***ES***	***QS***	***PI***	***RS***	***VR***
TA	0.0079	-7.78*	-12.41*	-2.71*	-3.77	0.0064
	(.0091)	(4.60)	(7.2)	(1.43)	(3.58)	(.0122)
W_{-3}	-0.0306***	4.19**	4.70*	3.01***	1.07	0.0030
	(.0045)	(1.65)	(2.46)	(.93)	(1.68)	(.0119)
W_{-2}	-0.0065	-3.16*	-3.17	1.25	-4.21**	-0.0084
	(.0052)	(1.82)	(2.69)	(1.1)	(1.7)	(.0111)
W_{-1}	-0.0064	-4.49**	-4.03	0.09	-3.76**	0.0636***
	(.0055)	(1.86)	(2.79)	(1.03)	(1.77)	(.0128)
$TA * W_{-3}$	-0.0080	-1.96	-0.29	-0.30	-2.42	-0.0259
	(.0066)	(2.24)	(3.1)	(1.27)	(2.28)	(.0167)
$TA * W_{-2}$	0.0004	1.87	2.39	-0.37	1.99	-0.0065
	(.0072)	(2.54)	(3.58)	(1.4)	(2.43)	(.0168)
$TA * W_{-1}$	0.0011	2.50	4.16	0.92	0.62	-0.0375**
	(.0076)	(2.53)	(3.84)	(1.36)	(2.46)	(.0178)
Intercept	0.3492***	48.79***	78.22***	19.14***	28.11***	0.4276***
	(.0066)	(3.47)	(5.41)	(1.13)	(2.64)	(.0084)
Observations	12,610	12,610	12,610	12,610	12,610	12,610
R-squared	0.008	0.005	0.004	0.004	0.002	0.005

Table V - Impact of the *Trade-At* Provision

This table reports estimates of a difference in difference model as in Equation (6). We regress the dependent variables in the top row on indicator variables for dates after the pilot is implemented (*Post*), stocks assigned to the treatment group (*TA*), and the interaction between these indicator variables (*TA * Post*). Thus the effect of treatment on the treated is the estimated coefficient on *TA * Post*. The sample includes stock-day observations for treatment (G3) and control (G2) stocks. Observations span 20 days before and after pilot implementation, September 2nd through November 29th, 2016. Since the pilot was implemented gradually over the month of October 2016, such that the pilot was fully implemented as of October 31st we omit data from October 1st through October 30th. Variables are as defined in Table I. All variables are winsorized at the 2.5th and 97.5th percentile. Statistical significance is denoted by *, **, and *** to indicate significance at the 0.10, 0.05, and 0.01 levels, respectively. Standard errors, shown in parentheses, are clustered by date and firm.

Panel A	***DarkTrading***	***Turnover***	***TradeSize***	***VWAP***
Post	0.0363***	0.0011***	252**	1.16***
	(.0080)	(.0003)	(104)	(.21)
TA	0.0063	0.0001	243	3.67
	(.0008)	(.0004)	(176)	(2.56)
*TA * Post*	-0.1206***	-0.0004*	-105***	-0.27
	(.0045)	(.0002)	(19)	(0.37)
Intercept	0.3383***	0.0059***	2,598***	22.84***
	(.0088)	(.0003)	(140)	(1.18)
Observations	24,652	24,652	24,652	24,652
R-squared	0.064	0.006	0.003	0.003

Panel B	***InvShare_all***	***InvShare_lit***	***LitFrag***	***LitVenues***
Post	0.0539***	0.0933***	0.61***	0.32***
	-0.0027	-0.0046	(.09)	(.03)
TA	0.0013	0.0042	0.01	0.09
	-0.0027	-0.0034	(.06)	(.13)
*TA * Post*	0.0335***	0.0161***	0.10**	-0.08*
	-0.0028	-0.0004	(.04)	(.04)
Intercept	0.0811***	0.12***	3.31***	8.35***
	-0.0028	-0.0042	(.09)	(.09)
Observations	24,652	24,652	24,652	24,652
R-squared	0.273	0.271	0.070	0.006

Table VI - Impact of *Trade-At* on Market Quality

This table reports estimates of a difference in difference model as in Equation (6). We regress the dependent variables in the top row on indicator variables for dates after the pilot is implemented (*Post*), stocks assigned to the treatment group (*TA*), and the interaction between these indicator variables (*TA* * *Post*), which represents the effect of treatment on the treated. The sample includes observations for treatment (G3) and control (G2) stocks for the 20 days before and after pilot implementation, September 2nd through November 29th, 2016. Since the pilot was implemented gradually over the month of October 2016, we omit data from October 1st through October 30th. Variables are as defined in Table I. All variables are winsorized at the 2.5th and 97.5th percentile. Statistical significance is denoted by *, **, and *** to indicate significance at the 0.10, 0.05, and 0.01 levels, respectively. Standard errors are clustered by date and firm and presented in parentheses.

	ES	***QS***	***PI***	***RS***	***VR***	***ES***	***QS***	***PI***	***RS***	***VR***
Post	17.75***	25.39***	9.21***	7.75***	-0.0068	18.34***	27.43***	7.94***	9.96***	-0.0101
	(2.04)	(3.15)	(.95)	(1.55)	(.0151)	(1.88)	(3.06)	(.85)	(1.59)	(.0148)
TA	-7.20*	-10.87*	-2.66**	-3.74	-0.0109**	-3.40*	-4.84	-1.43*	-1.22	-0.0124**
	(4.17)	(6.56)	(1.27)	(3.02)	(.0054)	(2.05)	(3.26)	(.79)	(1.68)	(.0053)
*TA * Post*	-0.47	2.97	0.68	-1.66	0.0173*	0.18	4.02	1.4293	-2.04	0.0160*
	(2.42)	(3.79)	(1.17)	(1.83)	(.0094)	(2.14)	(3.43)	(1.07)	(1.66)	(.0092)
DarkBlockRatio						-5.45**	-11.63***	-7.65***	5.45**	0.0252**
						(2.56)	(4.05)	(1.28)	(2.17)	(.0119)
IdioVol						3.28***	4.68***	1.77***	0.94***	0.00
						(0.21)	(0.32)	(0.13)	(0.14)	(0.00)
MktCap						-5.52***	-8.77***	-6.78***	1.08	-0.0101*
						(1.88)	(2.91)	(.85)	(1.32)	(.0056)
PIN						182.25***	261.67***	30.78***	132.30***	-0.0827**
						(19.59)	(31.59)	(7.54)	(16.55)	(.0417)
VWAP						-5.26**	2.26	-4.43***	-0.65	0.0011
						(2.40)	(3.58)	(.83)	(1.77)	(.0046)
TradeSizeRatio						5.93**	13.16***	4.80***	1.64	-0.0081
						(2.50)	(3.87)	(1.2)	(2.14)	(.0112)
Ln(TradedValue)						-10.32***	-20.44***	2.09***	-12.75***	0.0149***
						(1.06)	(1.61)	(.44)	(.88)	(.0028)
Intercept	47.96***	77.63***	20.23***	26.41***	0.4419***	269.03***	459.92***	121.73***	157.47***	0.4449***
	3.20	4.92	1.02	2.26	0.0124	(30.18)	(46.38)	(12.40)	(23.39)	(.0834)
Observations	24,657	24,652	24,652	24,652	24,657	24,657	24,657	24,657	24,657	24,657
R-squared	0.020	0.019	0.025	0.006	0.001	0.622	0.646	0.309	0.420	0.007

Table VII

Stock Trait Subsamples and the Impact of Dark Trading on Market Quality

This table reports estimates, for subsamples based on stock traits, for a coefficient interacting an indicator variable equal to one during dates when the pilot was implemented and zero otherwise, with an indicator variable equal to one for stocks in the treatment group. The time period covers September through November 2016, excluding October. Regressors include indicator variables for the post period, selection into the treatment group, as well as control variables. Thus we report the effect of the treatment on the treated and omit all other coefficient estimates for brevity. The top row contains our dependent variables for each regression. The first column lists the stock traits used to split the sample. For each trait there is a row reporting estimates obtained in regressions limited to observations above (below) the median value for that trait, denoted as High (Low). The last row, Quoted spread, divides the sample into stocks with an average quoted spread above (below) a nickel, denoted as High (Low). All variables are winsorized at 2.5th and 97.5th percentiles, and standard errors appear in parentheses. Statistical significance is denoted by *, **, and *** to indicate significance at the 0.10, 0.05, and 0.01 levels, respectively.

		ES	***QS***	***PI***	***RS***	***VR***
DarkTrading	High	-1.29	2.62	4.21*	-5.81*	0.0024
		(3.90)	(6.31)	(2.30)	(3.13)	(.0117)
	Low	-2.21*	-1.33	-1.43**	-0.82	0.0313**
		(1.16)	(1.67)	(.68)	(.55)	(.0131)
InvShare_lit	High	-2.95	-2.98	-1.67**	-1.01	0.0141
		(1.78)	(2.75)	(.82)	(1.10)	(.0125)
	Low	2.99	9.07	6.98***	-4.19	0.0197
		(3.97)	(6.59)	(2.30)	(3.11)	(.0124)
LitFrag_Value	High	-4.26*	-3.41	-1.54	-2.34	0.0172
		(2.29)	(3.53)	(1.05)	(1.46)	(.0146)
	Low	5.39	10.33*	5.25**	-0.51	0.0152
		(3.40)	(5.67)	(2.12)	(2.78)	(.0097)
MktCap	High	-1.25	0.88	-0.78	-0.55	0.0353***
		(0.86)	(1.26)	(0.51)	(0.44)	(.0129)
	Low	1.80	8.99	5.02*	-3.48	-0.0052
		(4.22)	(6.86)	(2.63)	(3.40)	(.0134)
Turnover	High	-1.78	-0.19	-0.63	-1.12	0.0124
		(1.84)	(2.68)	(.87)	(1.01)	(.0140)
	Low	2.48	6.26	4.58*	-2.56	0.0184
		(3.78)	(6.47)	(2.36)	(3.11)	(.0126)
Quoted spread	High	1.38	4.36	2.03	-2.04	0.0196*
		(2.32)	(4.42)	(1.54)	(2.32)	(0.0111)
	Low	-3.65	0.08	-0.32	-3.29	0.0125
		(3.24)	(4.84)	(1.28)	(2.44)	(0.013)

Table VIII
OLS and Heckman Correction Estimates - Impact of Dark Trading on Transaction Costs

This table reports estimates of the relationship between dark trading and transaction costs over the 20 day period before the tick size pilot was implemented (September 2nd through September 30th, 2016). The first column estimates an OLS model. The second column reports estimates of the first-stage of the Heckman correction model, where the dependent variable is the inverse normal of *DarkTrading*. The third column mirrors the OLS specification with the exception of including the inverse mills ratio (*IMR*) estimated in the first-stage Heckman regression (correcting for sample selection). Both columns one and three use effective spread as the dependent variable. Variables are as defined in Table I. All variables are winsorized at the 2.5th and 97.5th percentile. Statistical significance is denoted by *, **, and *** to indicate significance at the 0.10, 0.05, and 0.01 levels, respectively. Standard errors, displayed in parentheses, are clustered by date and firm.

	OLS	**Heckman Correction**	
	Effective Spread	Dark Trading (First Stage)	Effective Spread (Second Stage)
IMR			8.89
			(10.71)
DarkTrading	42.29***		44.61***
	(6.52)		(7.20)
DarkBlocks	-10.99***	0.89***	-5.79
	(3.26)	(.15)	(7.23)
IdioVol	28,463.94***	-27.85	28,399.42***
	(2,907.84)	(17.66)	(2,934.64)
PIN	207.39***	0.75***	211.82***
	(23.16)	(.20)	(24.14)
VWAP	3.98***	-0.03*	3.27*
	(1.54)	(.02)	(1.78)
TradeSize	10.49***	-0.07	10.41***
	(3.46)	(.20)	(3.60)
Ln(TradedValue)	-12.26***	-0.01	-12.53***
	(1.03)	(.01)	(1.10)
MktCap		-0.17***	
		(.02)	
UninfSupply		-1.02***	
		(.11)	
Constant	140.88***	2.94***	134.03***
	(14.06)	(.37)	(15.31)
Observations	12,610	12,610	12,610
R-squared	0.608	0.418	0.608