Does Floor Trading Matter?

Jonathan Brogaard
University of Utah

Matthew C. Ringgenberg
University of Utah

Dominik Roesch
State University of New York at Buffalo

First Draft: May 2020
This Draft: January 2021*

ABSTRACT

While algorithmic trading now dominates financial markets, some exchanges continue to use human floor traders. On March 23, 2020 the NYSE suspended floor trading because of COVID-19. Using a difference-in-differences analysis, we find that floor traders are important contributors to market quality. The suspension of floor trading leads to higher effective and quoted spreads and larger pricing errors for treated stocks, relative to control stocks. Moreover, consistent with theoretical predictions about automation, the effects are strongest around the opening and closing auctions when complexity is highest. Our findings suggest that human expertise can complement algorithms in complex situations.

Keywords: Algorithmic trading, floor trading, human trading, liquidity, market making, market quality, price efficiency

JEL Classification Numbers: G12, G14

*We thank Hank Bessembinder, Ekkehart Boehmer, Kee Chung, PK Jain, Robert Korajczyk, Ananth Madhavan, Steven Poser, Ryan Riordan, Kumar Venkataraman, Ingrid Werner, Pradeep Yadav, and seminar participants at Florida State University and the University of Memphis for helpful conversations and comments. We thank OneMarket Data for the use of their OneTick software. All errors are our own. This work is supported in part by NSF ACI-1541215.

I. Introduction

Since its inception in 1792 the New York Stock Exchange has relied on floor traders, human beings who trade securities while standing on the floor of the exchange. Over the last 20 years these floor traders have increasingly been replaced by electronic algorithms. Proponents of electronic trading argue that human interaction is unnecessary and electronic algorithms are more efficient and capable. Others argue that human traders provide intuition and knowledge that algorithms cannot offer.[1] Yet, to date, it remains unclear whether floor traders improve or harm liquidity and price efficiency.

We find that human floor traders improve market quality. Because of the COVID-19 pandemic, the NYSE suspended all floor trading on March 23, 2020 and moved to fully electronic trading. Using the abrupt change as an exogenous shock to floor trading activity, we conduct a difference-in-differences analysis to identify the effect of floor traders. We find that market quality deteriorates after floor traders are removed. Across a wide-variety of specifications and outcomes, we see that the removal of floor traders leads to higher effective and quoted spreads and larger pricing errors. The results are strongest during and around the opening auction and the closing auction, when complexity is highest. Moreover, the results persist until the NYSE floor partially reopened on May 26, 2020, at which point the effects partially reverse.

Given the increasingly popular belief that "artificial intelligence (AI) will disrupt labor markets" (Grennan & Michaely, 2020) and the increasingly large role played by algorithms, many have argued that floor traders are no longer necessary. The mere fact that humans are being replaced by algorithms suggest the algorithms have advantages on certain dimensions. Whether this change is beneficial or harmful to market quality is another question, and it

[1] See, for example, Condon and Babwin (2015), Detrixhe (2017), Byrne (2019).

is important to understand how these recent changes affect market quality (O'Hara, 2015). As of March 2020 there are thirteen registered equity exchanges in the United States. All except the NYSE are 100% electronic – only the NYSE has continued to use human floor traders.[2]

One major challenge with examining the role of floor traders is that their behavior is not exogenous from firm characteristics. Floor traders *choose* to trade, and these choices are likely related to firm-level measures of market quality. As such, a simple regression that examines the relation between market quality and floor trader activity might be biased. To address this we develop two identification strategies. Both research designs take advantage of the suspension of floor trading activity as an exogenous shock.

The first approach uses a difference-in-differences analysis that examines market quality for NYSE listed stocks before versus after the suspension of floor trading and compares them to a matched sample of NASDAQ stocks that did not experience a change in floor trading activity. The second approach uses a difference-in-differences analysis that compares trading in NYSE stocks on the NYSE to trading *in those same stocks* on other exchanges during the same time period. For example, the first approach compares market quality in IBM, which is listed on the NYSE, to market quality in a stock listed on NASDAQ that is matched on price, trading volume, market capitalization, and industry. The second approach compares market quality for IBM trades executed on the NYSE to market quality for IBM trades executed on other trading venues during the same time period. Both approaches produce economically similar results.

[2]While the NYSE is the only U.S. equity exchange to continue using floor traders, floor traders are still in use on the Chicago Board Options Exchange and the Chicago Mercantile Exchange, among others. Moreover, floor trades are an important component of the NYSE. According to the NYSE (2020), D orders (which are floor trades only) accounted for 32.9% of closing auction orders in January, 34.5% in February, and 27.6% in March.

Our identification approach requires the standard parallel trends assumption. This means the treatment group would have evolved in a similar fashion to the control group if floor trading had not been suspended. The difference-in-differences regressions include firm and time fixed effects, which absorb time-invariant firm characteristics and time-varying aggregate shocks. However, it remains possible that time-varying firm characteristics could be different between the treatment and control groups. For example, when we use NASDAQ stocks as a control group it is possible that NASDAQ stocks are differentially affected by the economic impact of COVID-19, relative to NYSE stocks. We do several things to address this concern.

First, visual evidence suggests that both control groups evolve similarly to the treatment group prior to the announcement and suspension of floor trading (i.e., the parallel trends assumption is supported). Because financial markets start reacting to COVID-19 in late February (before the suspension of floor trading), this suggests that NASDAQ stocks were not differentially affected. Second, in our second identification approach we compare trading in NYSE stocks on the NYSE to trading in those same stocks on other exchanges during the same time period. This approach allows us to include firm $\times$ date fixed effects. The specification accounts for *time-varying* firm-level shocks. A confounding omitted variable, if it exists, would need to differentially affect trading on one exchange, relative to another exchange, in the *same stock* starting around March 23rd. To the best of our knowledge, the only major change to affect one exchange but not other exchanges around this time period is the suspension of floor trading on the NYSE.

The suspension of floor trading halted all floor trading systems.[3] This included the elimination of order types specific to facilitating human interaction on the floor (e.g., Verbal

[3]The NYSE released the following document describing these changes: `www.nyse.com/publicdocs/nyse/NYSE_Floor_Closure_FAQ_20200320.pdf`

Interest and "D" Orders). Of course, many brokers that participate in floor trading also have electronic operations that continued to operate. As such, the only material change was the prevention of humans interacting on the trading floor and the elimination of order types only available to floor traders. Thus, our methodology allows us to precisely isolate the effect of floor trading activity on market quality.

We first examine liquidity. Specifically, we examine effective spreads and quoted spreads using the two identification strategies discussed above. In both identification approaches we find a significant increase in effective spreads for treated stocks relative to control stocks after the floor trading halt. For example, effective spreads for trades on the NYSE versus trades in the same stock on other exchanges experience an 11% increase. We see a similar widening of quoted spreads. Moreover, the coefficients are stable as we add fixed effects and controls, which supports our identification assumptions.

In order to reduce potential confounding effects we focus the analysis on a short window around the closure of floor trading using data from March 16, 2020 to March 27, 2020. However, it is possible that the effects we document are driven by the sudden and frantic nature of the shift away from floor trading. More specifically, it is possible that once market participants adapt to the new market structure the effects we document would evaporate or even go in the other direction. To examine this, we extend the window of analysis to go from February 1, 2020 to April 20, 2020. All of the conclusions are unchanged when using this longer window.

We next examine price discovery to see if prices evolve differently in the absence of floor traders. We find that they do. We use the Hasbrouck (1993) pricing error measure, which decomposes stock prices into an efficient component and an error component; higher values of the pricing error indicate worse price efficiency. We find that pricing errors increase. That is, the price process becomes noisier for stocks that lose their floor traders. For our first identification strategy, which uses NASDAQ stocks as a control group, we find that pricing errors for NYSE stocks increase approximately 6% relative to the control group after floor trading is removed. For our second identification strategy, which compares trading in the same stock on two different exchanges, we find that pricing errors increase approximately 2% after floor trading is removed. Overall, for both of our identification strategies, the results consistently show that market quality deteriorates after the removal of floor traders.

The results show that the removal of floor traders is associated with worse market quality. The natural next question is, why? Theoretically, labor that is easier to automate is more likely to be displaced by technology shocks. For example, Autor and Dorn (2013) develop a model of job market displacement by automation technology and they argue that routine tasks, which follow precise well-defined procedures, are more likely to be automated. In our setting, the question is whether liquidity provision is routine or complex.

To examine whether human floor traders are more beneficial when liquidity provision is more complex, we split our results into half-hour intervals during the trading day to understand when floor traders provide the largest benefit; the first half hours of each day contain the opening auctions (results are robust to excluding trades before the opening auction, see Table A2). Interestingly, the results show that floor traders are most important at the opening auction, from 9:30 am to 10:00 am (i.e., immediately following the opening auction), and at the closing auction. Our findings are broadly consistent with the experimental evidence in Carlin, Kogan, and Lowery (2013) who show that when complexity is high, market quality is lower. We empirically show that the role of floor traders is more important during the times of day when the price process is more complex. Consistent with Autor and Dorn (2013), the results suggest that floor traders matter because some aspects of liquidity provision are not routine and thus not easy to automate.

Our paper contributes to the broad literature of the role of technology in financial markets. Hendershott and Madhavan (2015) and Barclay, Hendershott, and Kotz (2006) focus on the choice of automation versus human interaction with financial markets, specifically the bond market. They find that in the bond market there are instances when it is important for humans to be involved. Costello, Down, and Mehta (2020) examine whether human beings can improve upon credit-scoring algorithms when making lending decisions. They find that human discretion can improve loan decisions. In contrast, Jansen, Nguyen, and Shams (2020) find that loans made by computer algorithms perform better than loans made by human underwriters.

Many studies have analyzed the introduction of a new technology, the most important being the rise of algorithmic and high-frequency trading (Hendershott, Jones, & Menkveld, 2011; Brogaard, Hendershott, & Riordan, 2014; Shkilko & Sokolov, 2020; Chaboud, Chiquoine, Hjalmarsson, & Vega, 2014). We add to this literature by focusing on the exogenous removal of arguably the most influential technology, the human floor trader. We also contribute to the literature that studies the unique aspects of the NYSE. One well-known and well-studied feature of the NYSE is that of the designated market maker (DMM) (e.g., Bessembinder, Hao, and Zheng (2020); Clark-Joseph, Ye, and Zi (2017); Venkataraman and Waisburd (2007)). For example, Bessembinder et al. (2020) find that DMM participation leads to better market quality and similarly Clark-Joseph et al. (2017) find that liquidity got worse when the NYSE was closed and DMMs were removed from the market. Importantly, not all floor traders are DMMs and DMMs continued to operate after the closure of the NYSE trading floor. Put differently, we examine a shock that varies floor trading activity but not DMM activity. As such, our results speak to a different question: specifically, we examine whether human interaction on the trading floor provides benefits in modern financial markets. More broadly, our findings are consistent with Sofianos and Werner (2000) who show that floor brokers who are not specialists (i.e., not DMMs) are active contributors to market liquidity.

Most recently, Hu and Murphy (2020) examine whether the different structure of the NYSE closing auction, relative to the NASDAQ closing auction, is associated with better or worse market quality. They find that market quality around the closing auction tends to be worse for NYSE stocks, relative to NASDAQ stocks. However, there are numerous differences in the closing auction on the NYSE besides the presence of floor traders that could explain this finding. While most of their analyses examine the period 2011 to 2018, when they examine the impact of the removal of floor traders in March of 2020 they find no evidence that overnight reversals change, suggesting floor traders do not adversely affect NYSE closing auctions.[4]

Other papers have examined stock prices and market quality shortly after the introduction of electronic traders (Jain, 2005; Venkataraman, 2001; Handa, Schwartz, & Tiwari, 2004; Battalio, Ellul, & Jennings, 2007; Benveniste, Marcus, & Wilhelm, 1992; Madhavan & Panchapagesan, 2000). For example, Jain (2005) shows that stock prices rise, and equity risk premiums fall, after the introduction of electronic trading in 120 different countries around the world. Similarly, Venkataraman (2001) examines liquidity on the NYSE versus liquidity on the Paris Bourse in 1997 and finds that the NYSE structure, which uses a mix of electronic and floor traders, appears to have advantages over the Paris Bourse structure which is fully automated.

The aforementioned literature predates the implementation of regulation NMS in 2007 and the significant increase in algorithmic trading that has occurred in recent years. As such,

[4] Hu and Murphy (2020) state, "However, the lack of a complete reversal during the reopening period suggests that the larger dislocations may be unrelated to the floor closure."

it remains unclear whether floor traders matter in modern equity markets. To the best of our knowledge, ours is the first paper to compare market quality before versus after the removal of floor traders on an equity exchange. We also add to this literature by having a clean identification strategy and by testing predictions about the relation between complexity and the automation of labor.

The rest of the paper proceeds as follows: Section II describes the data used in this study and provides a detailed overview of our identification strategy. Section III characterizes our findings and Section IV examines the economic mechanism underlying our analyses and describes areas for possible future research. Section V concludes.

II. Data and Identification Strategy

To examine the impact of floor traders, we combine data from the Center for Research in Security Prices (CRSP) with the NYSE Trade and Quotes (TAQ) database. Using this data, we examine the relation between floor trading and various measures of market quality in a difference-in-differences framework.

A. Data

On Wednesday, March 18, 2020 the NYSE announced they would suspend floor trading after a trader tested positive for the COVID-19 virus. Traders began planning for the change immediately, and floor trading was officially suspended starting on Monday, March 23rd. Accordingly, the sample compares market quality in a narrow window around the announcement and suspension of floor trading. In the main analyses we use data from March 16, 2020 to March 27, 2020. We also examine data in a longer time series from

January 1, 2020 through April 30, 2020. We include all U.S. common stocks with a CRSP share code of 10 or 11 that are listed on the NYSE or NASDAQ. We exclude stocks with dual class shares and a market capitalization below $500 million as of December 2019. The resulting sample has approximately 1,600 equities. From CRSP we get the stock price, dollar trading volume (in millions of USD), and market capitalization (in millions of USD).

We measure market quality and trading behavior using variables constructed from the TAQ database. We download stock-day proportional quoted spreads ($PQSPR$) and effective spreads ($PESPR$) from the Wharton Research Data Services (WRDS) Intraday Indicators dataset. $PQSPR$ is the time-weighted average of the difference between the ask and the bid price, scaled by the midpoint price. $PESPR$ is the dollar-weighted average of two times the difference between the signed trade price and the prevailing midpoint price, scaled by the midpoint price. See Holden and Jacobsen (2014) for a more detailed discussion on the calculation of these variables.

For some of our analyses, we examine intraday measures calculated using the TAQ database. Because our second identification strategy compares trading in one stock on the NYSE to trading in that same stock on other exchanges, we calculate spreads within each 30-minute interval during the continuous trading session (from 9:30 to 16:00) separately within the NYSE and on all other exchanges. $PQSPR$ and $PESPR$ on the NYSE is calculated from quotes and trades that occurred on the NYSE. Similarly, $PQSPR$ and $PESPR$ outside the NYSE is calculated from best bid and ask prices and all trades that occurred off the NYSE.[5] We construct national best bid and offer quotes outside the NYSE following Holden and Jacobsen (2014) and filter trades as in Rösch, Subrahmanyam, and Van Dijk (2017). We also construct Hasbrouck (1993) pricing errors as in Rösch et al. (2017).

[5]E.g., for this measure, we use only trades on IEX, NASDAQ, etc.

Table I contains summary statistics for our sample.

INSERT TABLE I ABOUT HERE

Because our analysis focuses on floor trading in NYSE stocks, the stocks in our sample skew towards larger stocks with relatively high liquidity. The mean (median) stock in our sample has a market capitalization of $17 billion ($3.3 billion) and mean (median) effective spreads are 0.18 (0.14) as a percentage of the midpoint price.

B. Identification Strategy

One challenge with identifying the effect of floor traders is that their activity is likely not exogenous from firm-level characteristics. As a result, coefficient estimates from an ordinary least squares (OLS) regression of firm-level characteristics on floor trading activity would likely be biased. Accordingly, to identify the effect of floor traders we use a difference-in-differences regression. To motivate the difference-in-differences regression and formalize our identification assumptions, we first assume that firm-level measures of market quality in the no-treatment state can be expressed according to the additive model:

$$E[\text{MarketQuality}_{0,i,e,t}|i,e,t] = \rho_e + \lambda_t + \Gamma_i + \alpha_{i,t}, \tag{1}$$

where $\text{MarketQuality}_{0,i,e,t}$ is market quality in stock i on exchange e on date t when floor trading is allowed, and $\text{MarketQuality}_{1,i,e,t}$ is market quality in stock i on exchange e on date t when floor trading is not allowed. Equation 1 assumes that market quality in the no-treatment state ($\text{MarketQuality}_{0,i,e,t}$) is determined by the sum of four variables: (i) ρ_e, which is the impact of trading on a particular exchange, (ii) λ_t which is a time effect that impacts market quality for all firms on a particular date, (iii) Γ_i which is a time-invariant firm effect, and (iv) $\alpha_{i,t}$ which represents time-varying firm-level shocks. To summarize, Equation 1 says that market quality in a stock is determined by where a stock trades (ρ_e), aggregate market conditions (λ_t), time-invariant firm-specific conditions (Γ_i), and time-varying firm conditions ($\alpha_{i,t}$).

For example, imagine two stocks, A and B, and assume stock A is consistently more liquid than stock B. Γ_i captures the persistence difference in liquidity, while $\alpha_{i,t}$ captures time-varying differences that occur on certain dates (e.g., when stock A has an earnings announcement its liquidity might change). λ_t captures aggregates shocks that affect both stock A and B (for example, the outbreak of a global pandemic). Finally, ρ_e measures the change in liquidity for both A and B when they trade on different exchanges.

In this paper, our goal is to understand whether the presence of floor traders affects market quality (either positively or negatively). Assuming the additive model shown in equation (1), observed market quality can be written as:

$$\text{MarketQuality}_{i,e,t} = \rho_e + \lambda_t + \Gamma_i + \alpha_{i,t} + \delta \mathbb{1}_{FloorTrading} + \epsilon_{i,e,t}, \tag{2}$$

where $\mathbb{1}_{FloorTrading}$ is an indicator variable that equals one if floor traders are active and zero otherwise. Our goal is to test whether δ is non-zero to see if floor traders have an effect on market quality.

The difference-in-differences estimator compares: (the expected value of treated firms *after* treatment minus the expected value of treated firms *before* treatment) minus (the expected value of control firms *after* treatment minus the expected value of control firms *before* treatment). In our setting (given equation 2) this yields:

$$\begin{aligned}&(\rho_e - \rho_e + \lambda_{t=after} - \lambda_{t=before} + \Gamma_i - \Gamma_i + \alpha_{i,t=after} - \alpha_{i,t=before} + \delta)\\&-(\rho_e - \rho_e + \lambda_{t=after} - \lambda_{t=before} + \Gamma_j - \Gamma_j + \alpha_{j,t=after} - \alpha_{j,t=before}),\end{aligned} \tag{3}$$

which reduces to:

$$= \delta + (\alpha_{i,t=after} - \alpha_{i,t=before}) - (\alpha_{j,t=after} - \alpha_{j,t=before}), \tag{4}$$

where i indexes treatment firms and j indexes control firms. Equation (4) highlights that a traditional difference-in-differences regression will recover the treatment effect from floor trading (δ) when the time-varying firm-level shocks in the control group ($\alpha_{j,t}$) are the same as in the treatment group ($\alpha_{i,t}$); this is the traditional parallel trends assumption.

We use two different sets of control groups in our difference-in-differences regressions: the first group use NASDAQ stocks matched on observable characteristics to generate a counterfactual while the second group uses market quality for NYSE listed stocks traded *off* the NYSE as a counterfactual for the treatment group. For example, the first group compares market quality in IBM, which is listed on the NYSE, to market quality in a stock listed on NASDAQ that is matched on price, trading volume, market capitalization, and industry. The second approach compares market quality for IBM trades executed on the NYSE to market quality for IBM trades executed on other trading venues during the same time period. Below, we discuss these two approaches in greater detail.

B.1. Matched Sample Approach

Our first approach uses a matched sample to form a control group. Our treatment group consists of NYSE listed stocks, and we use NASDAQ listed stocks matched on observable characteristics as the control group. As in Rindi and Werner (2019), we match stocks using the following observable variables: price, trading volume, and market capitalization. We also match on industry as measured by the Fama and French 48 industry classifications. We add industry as a matching variable to control for the possibility that the pandemic differentially affected stocks in certain industries, which may not be represented equally across exchanges. For example, if the economic shock from the pandemic affected technology stocks differently than blue-chip stocks, a comparison of all NYSE stocks to all NASDAQ stocks could lead to biased estimates (since technology stocks are more likely to be listed on NASDAQ).

We use one-to-one nearest neighbor propensity score matching (PSM), without replacement. Average price and trading volume are both measured one week before the treatment event (i.e., from March 16, 2020 to March 20, 2020) while market capitalization is measured as of December 2019.

Table II shows summary statistics for the propensity score matching procedure.

INSERT TABLE II ABOUT HERE

Panel A displays summary statistics for the propensity score matching sample, while Panel B shows the distribution of propensity score for the treatment and control groups. The distributions are generally similar across the two groups. Panel C contains summary statistics for key variables, broken out by treatment and control groups. As expected, there is no statistically significant difference between the treatment and control groups for volume or market capitalization; while there is a slight difference in price, the variable is only statistically significant at the 10% level. We note that the treatment and control groups do exhibit a statistically significant difference for each of the market quality measures we examine. However, the parallel trends assumptions requires that time-varying firm-level shocks in the control group are evolving in the same manner as in the treatment group, it does not require them to have the same *level*. As such, the inclusion of firm-fixed effects in our regressions should absorb this level difference.

Using the matched sample, we then examine difference-in-differences regressions of the form:

$$y_{i,e,t} = \beta D_{i,e,t} + \zeta_i + \eta_t + \gamma X_{i,t-1} + \xi_{i,e,t}, \tag{5}$$

where $y_{i,e,t}$ is a measure of market quality associated with trades in firm i on exchange e on day t. The variable $D_{i,e,t}$ is an indicator variable that equals one if a firm i is in an NYSE listed stock (e=treated) and t is after March 23, 2020, and equals zero otherwise. ζ_i and η_t are firm and date fixed effects, respectively. In some specifications, we include stock price and log of dollar trading volume as control variables ($X_{i,t-1}$). We note that if our identification assumptions hold, the addition of these control variables should not change the coefficient on the treatment effect (β) (Oster, 2019). In all analyses we calculate standard errors clustered by firm.[6]

Of course, matched samples do not necessarily account for unobservable heterogeneity between the treatment and control groups. Our difference-in-differences approach assumes that such unobservable heterogeneity, if it exists, is evolving similarly in the treatment and control groups. In our setting, the parallel-trends assumptions requires that in the absence of treatment the change in the conditional average market quality of NYSE firms would

[6] Because our main specification does not have a large number of time periods, we do not cluster by time since clustered standard errors are asymptotically consistent as the number of clusters grows large.

be equal to the change in the conditional average market quality of the matched NASDAQ firms.

There are two primary ways in which this assumption could be violated in our setting. First, if another shock, besides the suspension of floor trading, occurred around March 23 and differentially effected NASDAQ stocks relative to NYSE stocks, then our treatment estimate might be confounded by this effect. However, to the best of our knowledge, there were no other substantial changes on the NYSE or NASDAQ around this period.[7] Second, if NYSE stocks and NASDAQ stocks were affected *differently* by COVID-19 (and/or the related economic effects of COVID-19) than it is possible they would exhibit a different time-varying trend (i.e., $\alpha_{j,t} \neq \alpha_{i,t}$ in equation (2)); as such, NASDAQ stocks would not be a valid counterfactual. Yet, our treatment effect remains stable across all of our specifications and control groups, suggesting that NASDAQ stocks were affected by COVID-19 in a manner similar to NYSE stocks.

Moreover, while the identifying assumption is inherently untestable, Figures 1 and 2 provide visual evidence consistent with the parallel-trends assumption.

INSERT FIGURE 1 ABOUT HERE

INSERT FIGURE 2 ABOUT HERE

Figure 1 displays the proportional effective spread for trades in NYSE stocks versus trades in NASDAQ stocks. While there is some evidence that spreads increased prior to mid-March (likely due to disruptions from the pandemic), the increase occurs in both NYSE and NASDAQ stocks. Both groups evolve in a similar manner in the weeks prior to the

[7]We note that COVID-19 caused market disruptions that affected all stocks (λ_t in equation (2)), but this effect is absorbed by time-fixed effects in our setting.

announcement and suspension of floor trading. The two vertical lines in the figure indicate the announcement and suspension dates, which occur on Wednesday, March 18th and Monday, March 23rd, respectively. Right around these dates, the figure shows a dramatic increase in spreads on the NYSE relative to the spreads on NASDAQ. Similarly, Figure 2 shows a similar pattern for quoted spreads. The spreads evolve similarly on the NYSE and NASDAQ until mid-March, at which point the NYSE spreads increase significantly relative to the NASDAQ spreads, and they remain elevated throughout our sample period. Overall, the visual evidence supports our identification assumptions and suggests that the suspension of floor trading lead to wider spreads for NYSE stocks.

While the visual evidence suggests that our control firms were evolving in a similar manner to the treatment firms prior to the suspension of floor trading, it remains possible that NASDAQ stocks were somehow differently affected by COVID-19 after March 23rd. Accordingly, we also examine another control group in the next subsection designed to address this concern.

B.2. Within Stock Variation

Our matched sample approach uses NASDAQ stocks as a counterfactual for NYSE stocks if there had been no change in floor trading. As discussed above, it remains possible that NASDAQ stocks were somehow differentially affected by COVID-19. To address this concern, we examine a second control group that uses trades for NYSE listed stocks that occur on other exchanges. The implementation of Regulation NMS in 2007 led to an increase in exchange competition, stocks are now frequently traded on many different exchanges, not just their listing exchange.[8] For example, our approach compares market quality for IBM

[8]See Haslag and Ringgenberg (2020) for a detailed discussion on the effects of regulation NMS and intermarket competition.

stock trades that occur on the NYSE relative to market quality for IBM stock trades that occur on another exchange, before versus after the suspension of floor trading on the NYSE.

Formally, we examine difference-in-differences regressions of the form:

$$y_{i,e,t} = \beta D_{i,e,t} + \kappa_{i,t} + \gamma X_{i,t-1} + \epsilon_{i,e,t}, \tag{6}$$

where the variable $y_{i,e,t}$ is a measure of market quality associated with trades in firm i on exchange e on day t. The variable $D_{i,e,t}$ is an indicator variable that equals one if a firm i is a NYSE listed stock trading on the NYSE and the date t is after March 23, 2020, and equals zero otherwise. In our main specifications we include firm × date fixed effects ($\kappa_{i,t}$) and we calculate standard errors clustered by firm. In some specifications, we include stock price and log of dollar trading volume as control variables ($X_{i,t-1}$). Again we note that if our identification assumptions hold, the addition of these control variables should not change the coefficient on the treatment effect (β).

In our fully saturated specifications, we include firm × date fixed effects. This specification ensures that the treatment effect (β) is estimated using only within-firm variation after accounting for time-varying shocks. In other words, while our matched sample approach required the assumption that time-varying shocks to control firms were the same as time-varying shocks to treatment firms ($\alpha_{j,t} = \alpha_{i,t}$), here the firm × date fixed effects allow us to absorb these time-varying shocks. As a result, the specification accounts for time-varying firm-level heterogeneity. Put differently, time-varying firm-level responses to COVID-19 (or any other time-varying firm effects) are accounted for. A confounding omitted variable, if it exists, would need to differentially affect firm-level trading activity on the NYSE relative to off the NYSE *in the same stock* and it would need to be correlated with the timing of the suspension of floor trading on March 23rd.

For example, to violate the identification assumption, a confounding variable would have to change market quality in IBM when it trades on NASDAQ relative to market quality in IBM when it trades on the NYSE, starting around March 23, for reasons unrelated to the suspension of floor trading activity. Again, while such an effect is possible, we are not aware of any other changes to the operations of exchanges that occurred on March 23rd.[9]

Although the identifying assumption is inherently untestable, Figures 3 and 4 provide visual evidence consistent with the parallel-trends assumption.

INSERT FIGURE 3 ABOUT HERE

INSERT FIGURE 4 ABOUT HERE

Figure 3 displays the proportional effective spread for trades in NYSE stocks that occurred on the NYSE relative to trades in NYSE stocks that occurred off the NYSE. Throughout January, February, and early March, the figure shows that the proportional effective spreads evolved in a nearly identical manner for trades on versus off the NYSE.[10] However, around the time of the announcement and implementation of the suspension of floor trading (indicated by the two vertical lines), the figure again indicates a widening gap between spreads occurring on the NYSE versus off the NYSE. Similarly, Figure 4 shows a nearly identical pattern before but not after the suspension of floor trading for quoted spreads. If

[9]It is also theoretically possible that the suspension of floor trading on the NYSE caused liquidity providers on other exchanges to change their behavior which would violate the stable unit treatment value assumption. However, we are not aware of any evidence of this occurring; we also note that the unexpected nature of the change makes it unlikely that other liquidity providers had time to significantly alter their behavior. We discuss this point further in Section IV, below.

[10]While there appears to be a change a few days prior to the NYSE announcement about the suspension of floor trading, we note that social distancing had already become wide-spread in the days prior to the announcement; this may have disrupted floor trading activity in the days immediately before it was actually suspended.

anything, trades off the NYSE exhibit higher quoted spreads before the suspension of floor traders, but this effect reverses starting precisely on the date floor trading was suspended. Overall, the visual evidence strongly supports our identification assumptions.

III. Results

We next examine whether floor trading matters for market quality using the two difference-in-differences analyses developed in the previous section. We find that it does. Across a wide variety of specifications and control groups, we consistently find evidence that effective spreads, quoted spreads, and pricing errors deteriorate after the suspension of floor trading. In the Internet Appendix we also investigate the effect of floor trading on NYSE market share and find that market share decreases with the closure of the floor during the continuous trading session and the closing auction, but increases during the opening auction (see Tables A4 to A7). The results suggest floor traders are important contributors to market quality even in the age of electronic trading.

A. Effect of Floor Traders on Spreads

We start by examining effective spreads ($PESPR$). If electronic liquidity providers are able to successfully replicate (or even improve upon) floor trader activity, then we would expect either a zero or negative treatment effect after the removal of floor traders. In other words, after the suspension of floor trading, effective spreads should either be unchanged or even decrease.[11] On the other hand, if human beings are able to provide some additional information or skill that is not provided by algorithmic liquidity providers, then the suspension of floor trading might lead to an increase in effective spreads.

INSERT TABLE III ABOUT HERE

Table III shows the results. Panel A displays the difference-in-differences treatment effect for a variety of specifications using NASDAQ stocks as a control group. In Column (2), which uses stock and date fixed effects, the coefficient of 0.09 on $Treated \times After$ indicates that effective spreads increased significantly after the suspension of floor trading. While spreads were generally elevated throughout March for both NASDAQ and NYSE stocks as a result of COVID-19, effective spreads on NYSE stocks increased 9 bps points *more* than spreads on the matched sample of NASDAQ stocks. Relative to the unconditional mean $PESPR$ of 0.18, this represents a dramatic 50% increase in effective spreads as a result of the suspension of floor trading. Moreover, in columns (3) and (4), when we add control variables, the treatment effect is largely unchanged suggesting our results are not sensitive to the possibility of an omitted variable bias.

Panel B displays results from the difference-in-differences analysis using trading in NYSE listed stocks on other exchanges as a control group. For example, this analysis compares trading in IBM on the NYSE to trading in IBM on other exchanges, like the Investors Exchange (IEX) and the BZX Exchange, over the same time period. The results again show that effective spreads increased as a result of the suspension of floor trading. In our fully saturated specification, shown in Column (5), the coefficient of 0.02 on $Treated \times After$

[11] We note that floor brokers might trade with prices closer to midpoint quotes and therefore trades involving floor brokers might have a very small effective spread (Sofianos & Werner, 2000). In a robustness test, we exclude trades at the midpoint and show that our results are robust, though slightly weaker (see Table A1). This result suggests that the effect of the floor closure on effective spreads is not purely due to trades involving floor brokers.

indicates that effective spreads increased 11% for trades on the NYSE, versus trades in the same stock on other exchanges. We note that this specification includes firm × date fixed effects; as a result, it controls for time-varying firm-level heterogeneity. In addition, as with Panel A, the treatment effect in Panel B is highly stable across specifications. As we vary the fixed effects or add control variables, the treatment effect is largely unchanged, supporting our identification assumptions.

Interestingly, the results in this section are consistent with evidence from a recent laboratory experiment. Asparouhova et al. (2020) examine a setting where human traders have the option to deploy a set of algorithms and to continue trading manually. They theorize that algorithms should lead to better market quality, yet they find that participants who use both algorithms and manual trading perform best. Moreover, they find that algorithmic trading, on its own, leads to more flash crashes and price volatility.

A.1. Robustness: Alternate Specifications

Our main results examine market quality in a very narrow window around the announcement and suspension of floor trading in mid-March. Specifically, the sample in Table III examines data from March 16th, 2020 through March 27, 2020. This narrow window helps ensure that our analysis is not confounded by other changes to the exchanges – to the best of our knowledge, the only significant change that occurred in this window was the suspension of floor trading. However, to establish the robustness of our findings, we also examine a larger window that uses data from February 1, 2020 through April 20, 2020. The results are shown in Table IV.

INSERT TABLE IV ABOUT HERE

Again, across both control groups and a wide variety of specifications, the results consistently show the same thing: the suspension of floor trading is associated with larger effective spreads. Depending on the specification, the results shows that effective spreads increased by approximately 11% to 27% after the suspension of floor trading. In Panel A, which uses the matched sample of NASDAQ stocks as a control group, the statistically significant coefficient of 0.02 on $Treated \times After$ on column (4) indicates that effective spreads increased 11% for trades on the NYSE, versus NASDAQ. In panel B, which uses trading in NYSE listed stocks on other exchanges as a control group, the statistically significant coefficient of 0.05 on $Treated \times After$ on column (5) indicates that effective spreads increased 27%. Overall, the findings suggest our conclusions are robust to alternate specification choices.

A.2. Quoted Spreads

The results in Tables III and IV show strong evidence that effective spreads increased. To understand more about the nature of this increase, we next examine whether liquidity providers changed their quoting behavior. While effective spreads can be viewed as a measure of price improvement, quoted spreads contain information about the quality of the limit order book. To examine whether the removal of floor traders matters for the limit order book, Table V examines proportional quoted spreads ($PQSPR$) using both of our difference-in-differences approaches. As before, Panel A presents results using a matched sample of NASDAQ stocks as the control group, while Panel B uses trading in NYSE stocks on other exchanges as the control group.

INSERT TABLE V ABOUT HERE

In Panel A, we again find evidence that floor traders are important contributors to market quality. The coefficient of 0.02 on $Treated \times After$ indicates that, on average, quoted spreads increased by approximately 7% relative to their unconditional mean of 0.29%. Similarly in Panel B the coefficient of 0.11 suggests that quoted spreads increase by 37% on the NYSE versus on other exchanges. Again, the results in both panels are highly stable across specifications, supporting our identification assumptions. Overall, the results in this section show that the suspension of floor trading leads to worse liquidity as measured by both effective and quoted spreads. In the next section, we examine whether the presence of floor traders matters for price discovery.

B. Effect of Floor Traders on Price Discovery

Our results so far show that floor traders matter for liquidity. If decreased liquidity affects trader behavior, it is possible that the removal of floor trading may also affect the price process. Accordingly, in this subsection, we examine whether floor traders affect price discovery. We find that they do. We start by computing the Hasbrouck (1993) pricing error. Hasbrouck (1993) develops a methodology to decompose observed stock prices into an efficient component and a price error component; higher values of the pricing error indicate worse price efficiency. After computing the pricing error, we use the log of pricing error as a dependent variable in the difference-in-differences regressions shown in equations (5) and (6). The results are shown in Table VI.

INSERT TABLE VI ABOUT HERE

As before, Panel A shows results using the matched sample of NASDAQ stocks as a counterfactual, while Panel B uses trades in NYSE stocks on other exchanges as the control group. In Panel A of Table VI, we find a positive and statistically significant coefficient on $Treated \times After$ in all models. Moreover, the results are again stable across a variety of fixed effects and the inclusion of control variables. In columns (1), (2), and (3), the statistically significant coefficient of 0.06 indicates that pricing errors for NYSE stocks increase approximately 6% after floor trading is removed.

In Panel B, when we use trades in NYSE stocks on other exchanges as the control group, we find similar evidence. The coefficient on $Treated \times After$ is positive and statistically significant in all models. In all columns, the statistically significant coefficient of 0.02 indicates that pricing errors increase approximately 2% after floor trading is removed. Overall, the results in Table VI show evidence that the suspension of floor trading is associated with an increase in pricing errors. In other words, the removal of floor traders leads to worse liquidity as measured by effective and quoted spreads and this change in liquidity leads to worse price efficiency.

IV. Interpretation of Results

A. Economic Mechanism

Overall, the results in Section III show that the removal of floor traders is associated with worse market quality. In some sense, it may seem surprising that the removal of human traders could adversely affect market quality. The mere fact that algorithms are the dominant liquidity provider in most modern markets suggests they have an advantage over human traders. Put differently, our results beg a new question: why do human floor traders matter for modern financial markets?

In theory, certain labor market tasks are more easily automated. Autor and Dorn (2013) develop a model of job market displacement by automation technology in service industries. They argue that routine tasks are more likely to be automated, where routine tasks are those “which are readily computerized because they follow precise, well-defined procedures.” In contrast, they define abstract tasks as those that are “creative, problem-solving, and coordination tasks performed by highly-educated workers such as professionals and managers, for whom data analysis is an input into production.”

In our setting, the question is whether liquidity provision is routine or abstract. In some cases, it seems likely that liquidity provision would be largely routine; throughout the day as orders arrive it is likely that an algorithm can respond to regular and frequently occurring events. However, certain times of the day seem more likely to be complicated. For example, each morning the NYSE opens with an auction. After the exchange closes the previous day, information about the economy and individual firms continues to be released. As a result, there is often unknown but significant demand to buy and sell certain stocks prior to the opening of the exchange. The opening auction is designed to facilitate information production and aggregation so that trading can begin in an orderly fashion even though there may be unknown demand or supply shocks that are about to be realized.

To examine whether floor traders matter more when trading is non-routine, we examine our main analysis broken out by half-hour intervals within the continuous trading session. Table VII shows effective spreads, by half-hour, using our difference-in-differences analysis with NYSE stocks trading off the NYSE as the control group. Panel A displays results for the morning trading session, while Panel B shows results for afternoon trading. In the Internet Appendix we show that our results are robust to using the first research design, i.e., comparing NYSE listed stocks to matched NASDAQ stocks (Table A3).

INSERT TABLE VII ABOUT HERE

Consistent with Autor and Dorn (2013), we find that floor traders matter the most immediately following the opening auction when trading is less likely to be routine.[12] Column (1) in Table VII shows the treatment effect from the removal of floor trading for the time interval between 9:30am ET and 10am ET. The remaining columns show additional half-hour intervals, until markets close at 4pm ET. Interestingly, the results are strongest in the interval from 9:30am to 10am, and they decline monotonically throughout the day. While it is difficult to objectively measure the complexity of liquidity provision throughout the day, these findings show that human floor traders add the most value during the opening auction, and the periods soon thereafter.[13] The findings are also consistent with the predictions in Madhavan and Panchapagesan (2000) who show that designated dealers play a crucial role in price discovery relative to automated trading systems.

To explore the roll of complexity, Panel C includes a triple interaction that examines our main treatment effect, $Treated \times After$, multiplied by a measure of complexity. We calculate complexity as 1 - R^2 from a regression of each stock's return on the contemporaneous market return (as measured by the return on the SPY exchange traded fund) for each half-hour interval. As shown in Morck, Yeung, and Yu (2000), 1 - R^2 measures how much firm-specific information is in a given stock's price. A stock with a low value has returns that are almost perfectly explained by the market's movements, while a stock with a high value has returns that are explained by unique firm-specific information. Accordingly, stocks with a high value are more complex. Intuitively, a trader seeking to predict price movements in stocks with low values needs only to understand macro information, while a trader seeking to understand stocks with high values needs to follow firm-specific information for each and every stock.

[12]These findings are consistent with anecdotal evidence. A 2012 *Business Insider* article on the role of floor traders stated, "Right around the opening bell it's 'complete mayhem'...Then around 9:45 a.m. to 10:00 a.m. everything begins to settle down and algos and computer programs do most of the trading." (La Roche, 2012).

[13]While we do not find evidence of an effect in the last half-hour of the day (which contains the closing auction), the results are statistically positive at the 15% significance level during this time interval.

The positive and statistically significant coefficient of 0.17 on $Complex \times Treated \times After$ in Panel C confirms our main result – more complex stocks experience a larger degradation in liquidity, relative to less complex stocks, after the removal of NYSE floor trading. The results suggest algorithms are not perfect substitutes for human floor traders. In times of high complexity, human floor traders continue to be valuable intermediaries that complement algorithms (which dominate trading when complexity is low).

We further explore the role of human floor traders during the opening and closing auctions, two times when complexity is unambiguously high. While the results in Table VII suggest that the effect of floor traders are strongest during and around the opening auction, we note that the opening and closing auctions have unique market structures that make it difficult to asses market quality using traditional measures. Accordingly, to examine market quality in these periods we follow Bogousslavsky and Muravyev (2019) and examine a measure of price deviation between the auction price and the midpoint from the national best bid and offer prices in continuous trading. For the opening auction we use the midpoint after 9:30 am and right before the opening auction. For the closing auction we use the midpoint from continuous trading right before the market closes. The measure $|Deviation\%|$ is estimated as two times the absolute difference in the logarithm of the auction price and the prevailing (open auction) or last midpoint price during the continuous trading session (close auction) using national best bid and ask prices. A higher $|Deviation\%|$ value indicates a lower quality auction. Formally:

$$|Deviation\%| = 2 \times |log(trade) - log(mid)| = 2 \times |log(trade/mid)|, \tag{7}$$

where log(trade) is log of the auction trade price and log(mid) is the last midpoint price during the continuous trading session. Table VIII presents the results using the NYSE versus NASDAQ difference-in-differences setting.

INSERT TABLE VIII ABOUT HERE

The first four columns examine deviations around the opening auction, while the last four columns examine deviations around the closing auction. At both the opening auction and the closing auction, when the trading floor closes, the gap between the continuous market price and the auction price widens. Across columns (1) through (4), the results are the same: the coefficient on $Treated \times After$ ranges from a positive and statistically significant 0.34 to 0.47. Similarly, in columns (5) through (8), the coefficient on $Treated \times After$ is consistently a positive and statistically significant 0.32. The results show that both opening and closing auction quality deteriorates after the suspension of floor trading.

B. Partial Reopening of the Floor

Our results consistently show that market quality decreased after the closure of the NYSE floor. If floor trading is truly beneficial, this suggests market quality should increase when the floor is reopened. In Table IX we examine market quality around the partial reopening of the trading floor. On May 26th, 2020, the NYSE allowed some floor trading to resume "with reduced headcount and restrictions in place to enforce social distancing and other safety protocols" (NYSE (2020)). Later, on June 17, 2020, the NYSE allowed some Designated Market Makers (DMMs) to resume trading on the floor as well.[14] To examine the impact of reopening the floor, we again use a difference-in-differences regression that compares market quality in a window around the events. Specifically, we compare market quality in the week

[14]A small number of NYSE stocks do not have a DMM on the floor even after the reopening. The NYSE posts a list of securities without a DMM on the floor, three out of the approximately 50 stocks in this list are also included in our sample. Our results are robust to dropping these three stocks.

prior to the first reopening event (on May 26th) to market quality in the week after the second reopening (on June 17th).[15].

INSERT TABLE IX ABOUT HERE

Table IX shows an improvement across nearly all of our market quality measures after the floor reopens. Columns (1), (2), and (3) of Panel A show that effective spreads, quoted spreads, and pricing errors all get smaller after some floor trading activity resumed. While the magnitudes are generally slightly smaller than our main results, this makes sense: human interaction and headcount were still restricted during this period. Moreover, in Panels B and C, we examine intraday effective spreads. Following the suspension of floor trading, our main results found an increase in effective spreads during and around the opening and closing auctions. In table IX, we see the opposite result. Following the reopening of floor trading, effective spreads decrease during and around the auctions. In column (5) of Panel A, we also find that closing auction deviations shrink following the reopening, consistent with our main findings. The only result that does not reverse is the opening auction deviation (column (4)).[16]

Overall, our results show that floor traders provide an important complement to electronic algorithms. Consistent with models of labor automation, the results suggest that human floor trading continue to be valuable intermediaries even in the age of algorithmic trading, especially when complexity is high. Taking all of the results together, the implication is

[15]We exclude the period between May 26th and June 17th, since this period included the intermediate stage when some floor traders were back on the floor, but DMMs were not yet on the floor.

[16]There are several possible explanations for this. Our auction deviation measure requires us to compare NYSE stocks to NASDAQ stocks; we cannot use our preferred identification strategy for this test since we cannot calculate opening and closing auction deviations for NYSE stocks on other trading venues. Moreover, to date, the NYSE has only partially reopened the floor. It is possible the opening auction deviation will improve once the floor is fully reopened.

clear: human floor traders lead to improved liquidity and price discovery, particular during and around the opening and closing auctions when the price process is difficult to predict.

C. Limitations

While our results provide strong evidence that human floor traders are important contributors to market quality in our setting, we are careful to note several limitations on our analysis. First, our analysis examines a relatively short time period around the suspension of floor trading. While the effects we document appear to persist throughout our sample period, it is possible that long-run effects would differ from the effects we document. For example, without floor traders in the market, it is possible algorithms would eventually change their behavior in a way that mitigated the impact of the removal of floor traders. Future research should continue to examine the implications of this.

In addition, we note that this limitation is related to our identification assumptions. Implicitly, our difference-in-differences regressions assume that other liquidity providers did not significantly change their behavior in response to the suspension of floor trading. If they did, it could violate the stable unit treatment value assumption. For example, Boehmer, Jones, and Zhang (2020) note that short selling in control stocks appears to change after the suspension of the tick test in 2005, which may invalidate traditional difference-in-differences regressions examining the impact of the removal of the tick test. In our setting, we note the rule change was not anticipated and we focus on a narrow window around the suspension of floor trading, making it unlikely that other liquidity providers had time to react. However, it remains possible that the long-run effects of floor trader removal are different than the effects we document.

V. Conclusion

Historically, financial trading has been dominated by human activity. Yet over the last few decades, algorithms have increasingly replaced human traders. As a consequence, the rise of algorithmic trading raises fundamental questions about financial market structure. In particular, do human traders have a role to play in modern financial markets?

In this paper, we study whether human floor traders have an impact on market quality in U.S. equity markets. We use the suspension of NYSE floor trading on March 23, 2020 as a shock to floor trading activity that is exogenous from firm-level characteristics. The results show that, even in the age of algorithmic trading, floor traders are important contributors to liquidity and price efficiency. Following the suspension of floor trading, we find higher effective spreads, higher quoted spreads, and worse pricing errors. Moreover, these effects are concentrated during and immediately following the opening auction, suggesting that human traders are more important when market complexity is higher. Overall, our results show human floor traders continue to be valuable intermediaries even in the age of algorithmic trading.

References

Asparouhova, E. N., Bossaerts, P., Rotaru, K., Wang, T., Yadav, N., & Yang, W. (2020). Humans in charge of trading robots: The first experiment. *Working Paper*.

Autor, D. H., & Dorn, D. (2013). The growth of low-skill service jobs and the polarization of the US labor market. *American Economic Review*, *103*(5), 1553–97.

Barclay, M. J., Hendershott, T., & Kotz, K. (2006). Automation versus intermediation: Evidence from treasuries going off the run. *Journal of Finance*, *61*(5), 2395–2414.

Battalio, R., Ellul, A., & Jennings, R. (2007). Reputation effects in trading on the New York Stock Exchange. *Journal of Finance*, *62*(3), 1243–1271.

Benveniste, L. M., Marcus, A. J., & Wilhelm, W. J. (1992). What's special about the specialist? *Journal of Financial Economics*, *32*(1), 61–86.

Bessembinder, H., Hao, J., & Zheng, K. (2020). Liquidity provision contracts and market quality: Evidence from the new york stock exchange. *Review of Financial Studies*, *33*(1), 44–74.

Boehmer, E., Jones, C., & Zhang, X. (2020). Potential pilot problems: Treatment spillovers in financial regulatory experiments. *Journal of Financial Economics*, *135*(1), 68–87.

Bogousslavsky, V., & Muravyev, D. (2019). Should we use closing prices? institutional price pressure at the close. *Working Paper*.

Brogaard, J., Hendershott, T., & Riordan, R. (2014). High-frequency trading and price discovery. *Review of Financial Studies*, *27*(8), 2267–2306.

Byrne, J. A. (2019, August 4). *NYSE floor traders are facing job extinction.* Retrieved from `https://nypost.com/2019/08/04/nyse-floor-traders-are-facing-job-extinction/`

Carlin, B. I., Kogan, S., & Lowery, R. (2013). Trading complex assets. *Journal of Finance*, *68*(5), 1937–1960.

Chaboud, A. P., Chiquoine, B., Hjalmarsson, E., & Vega, C. (2014). Rise of the machines: Algorithmic trading in the foreign exchange market. *Journal of Finance*, *69*(5), 2045–2084.

Clark-Joseph, A. D., Ye, M., & Zi, C. (2017). Designated market makers still matter: Evidence from two natural experiments. *Journal of Financial Economics*, *126*(3), 652–667.

Condon, B., & Babwin, D. (2015, July 6). *The trading pit era is officially ending.* Retrieved from `https://www.businessinsider.com/the-trading-pit-era-is-officially-ending-2015-7`

Costello, A. M., Down, A. K., & Mehta, M. N. (2020). Machine+ man: A field experiment on the role of discretion in augmenting ai-based lending models. *Journal of Accounting and Economics*, *70*(2-3), 101360.

Detrixhe, J. (2017, September 26). *Why robot traders haven't replaced all the humans at the New York Stock Exchange—yet.* Retrieved from `https://qz.com/1078602/why-the-new-york-stock-exchange-nyse-still-has-human-brokers-on-the-trading-floor/`

Grennan, J., & Michaely, R. (2020). Artificial intelligence and high-skilled work: Evidence from analysts. *Working Paper*.

Handa, P., Schwartz, R., & Tiwari, A. (2004). The economic value of a trading floor: Evidence from the American Stock Exchange. *Journal of Business*, *77*(2), 331–355.

Hasbrouck, J. (1993). Assessing the quality of a security market: A new approach to transaction cost measurement. *Review of Financial Studies*, *6*(1), 191–212.

Haslag, P. H., & Ringgenberg, M. C. (2020). The demise of the NYSE and NASDAQ: Market Quality in the Age of Market Fragmentation. *Working Paper*.

Hendershott, T., Jones, C. M., & Menkveld, A. J. (2011). Does algorithmic trading improve liquidity? *Journal of Finance*, *66*(1), 1–33.

Hendershott, T., & Madhavan, A. (2015). Click or call? Auction versus search in the over-the-counter market. *Journal of Finance*, *70*(1), 419–447.

Holden, C. W., & Jacobsen, S. (2014). Liquidity measurement problems in fast, competitive markets: Expensive and cheap solutions. *Journal of Finance*, *69*(4), 1747–1785.

Hu, E., & Murphy, D. (2020). Vestigial tails? Floor brokers at the close in modern electronic markets. *Working Paper*.

Jain, P. K. (2005). Financial market design and the equity premium: Electronic versus floor trading. *Journal of Finance*, *60*(6), 2955–2985.

Jansen, M., Nguyen, H., & Shams, A. (2020). Human vs. machine: Underwriting decisions in finance. *Working Paper*.

La Roche, J. (2012, Oct). A floor broker explains what they actually do all day at the new york stock exchange. *Business Insider*. Retrieved from `https://www.businessinsider.com.au/heres-what-a-nyse-floor-broker-does-2012-10#then-things-start-to-settle-down-and-the-computers-and-algos-do-most-of-the-trading-7`

Madhavan, A., & Panchapagesan, V. (2000). Price discovery in auction markets: A look inside the black box. *Review of Financial Studies*, *13*(3), 627–658.

Morck, R., Yeung, B., & Yu, W. (2000). The information content of stock markets: Why do emerging markets have synchronous stock price movements? *Journal of Financial Economics*, *58*(1–2), 215–260.

NYSE. (2020, May). Nyse trading floor partially re-opens. *NYSE Data Insights*. Retrieved from `https://www.nyse.com/data-insights/-nyse-trading-floor-partially-re-opens`

Oster, E. (2019). Unobservable selection and coefficient stability: Theory and evidence. *Journal of Business & Economic Statistics*, *37*, 187-204.

O'Hara, M. (2015). High frequency market microstructure. *Journal of Financial Economics*, *116*(2), 257–270.

Rindi, B., & Werner, I. M. (2019). U.S. tick size pilot. *Working Paper*.

Rösch, D. M., Subrahmanyam, A., & Van Dijk, M. A. (2017). The dynamics of market efficiency. *Review of Financial Studies*, *30*(4), 1151–1187.

Shkilko, A., & Sokolov, K. (2020). Every cloud has a silver lining: Fast trading, microwave connectivity, and trading costs. *Journal of Finance*, *75*(6), 2899–2927.

Sofianos, G., & Werner, I. M. (2000). The trades of NYSE floor brokers. *Journal of Financial Markets*, *3*(2), 139–176.

Venkataraman, K. (2001). Automated versus floor trading: An analysis of execution costs on the Paris and New York exchanges. *Journal of Finance*, *56*(4), 1445–1485.

Venkataraman, K., & Waisburd, A. C. (2007). The value of the designated market maker. *Journal of Financial and Quantitative Analysis*, *42*(3), 735–758.

Figure 1 – Daily PESPR for NYSE stocks relative to NASDAQ stocks
This figure shows proportional effective spreads (*PESPR*) in percent for all matched NYSE and Nasdaq listed stocks in our sample. We calculate changes in *PESPR* since January, 1^{st} 2020 per stock-day and plot the daily market cap-weighted average *PESPR* across all stocks in our sample. *PESPR* is estimated as the trade price across all exchanges in excess of the prevailing midpoint price using NBBO bid and ask prices. The first vertical line (2020-03-18) indicates the announcement of the event, the closing of the NYSE floor. The second vertical line (2020-03-23) indicates the event day.

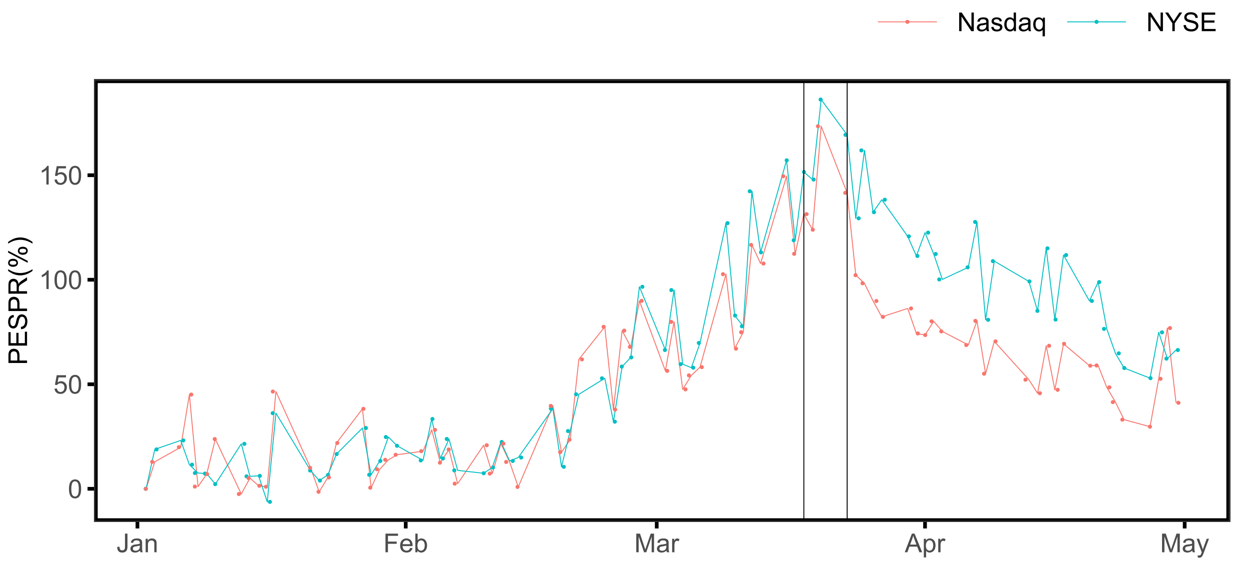

Figure 2 – Daily PQSPR for NYSE stocks relative to NASDAQ stocks
This figure shows proportional quoted spreads ($PQSPR$) in percent for all matched NYSE and Nasdaq listed stocks in our sample. We calculate changes in $PQSPR$ since January, 1^{st} 2020 per stock-day and plot the daily market cap-weighted average $PQSPR$ across all stocks in our sample. $PQSPR$ is estimated using NBBO bid and ask prices. The first vertical line (2020-03-18) indicates the announcement of the event, the closing of the NYSE floor. The second vertical line (2020-03-23) indicates the event day.

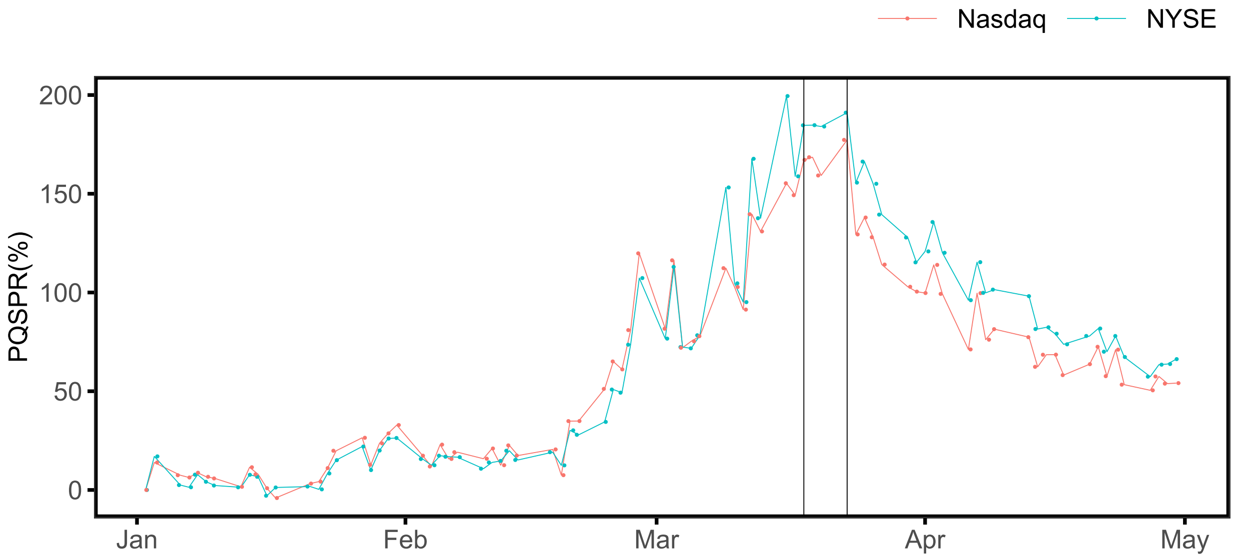

Figure 3 – Daily PESPR for NYSE listed stocks traded on the NYSE vs. off the NYSE
This figure shows proportional effective spreads (*PESPR*) in percent calculated from trades on the NYSE and all other exchanges (*not NYSE*). We calculate changes in *PESPR* since January, 1^{st} 2020 per stock-day and plot the daily equally weighted average *PESPR* across all NYSE listed stocks in our sample. *PESPR* on the NYSE is estimated from trade price on the NYSE in excess of the prevailing midpoint price using bid and ask prices on the NYSE. *PESPR* on other exchanges is estimated from trade price outside the NYSE in excess of the prevailing midpoint price using NBBO without NYSE bid and ask prices. The first vertical line (2020-03-18) indicates the announcement of the event, the closing of the NYSE floor. The second vertical line (2020-03-23) indicates the event day.

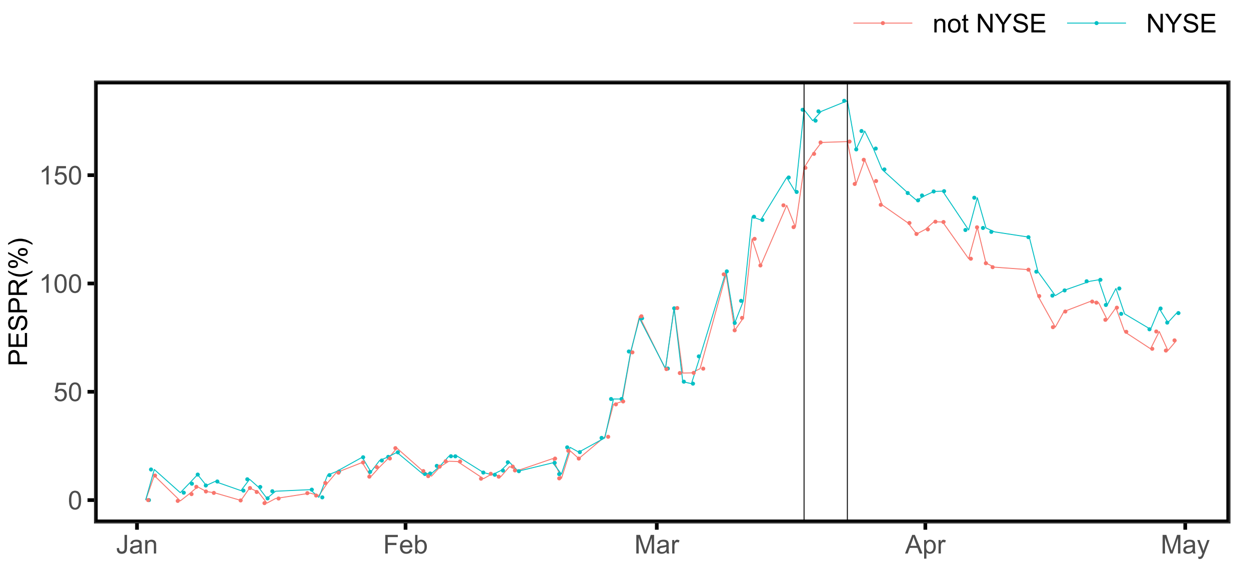

Figure 4 – Daily PQSPR for NYSE listed stocks traded on the NYSE vs. off the NYSE
This figure shows proportional quoted spreads ($PQSPR$) in percent calculated from quotes on the NYSE and all other exchanges (*not NYSE*). We calculate changes in $PQSPR$ since January, 1^{st} 2020 per stock-day and plot the daily equally weighted average $PQSPR$ across all NYSE listed stocks in our sample. $PQSPR$ on the NYSE is estimated using bid and ask prices on the NYSE. $PQSPR$ on other exchanges is estimated using NBBO without NYSE bid and ask prices. The first vertical line (2020-03-18) indicates the announcement of the event, the closing of the NYSE floor. The second vertical line (2020-03-23) indicates the event day.

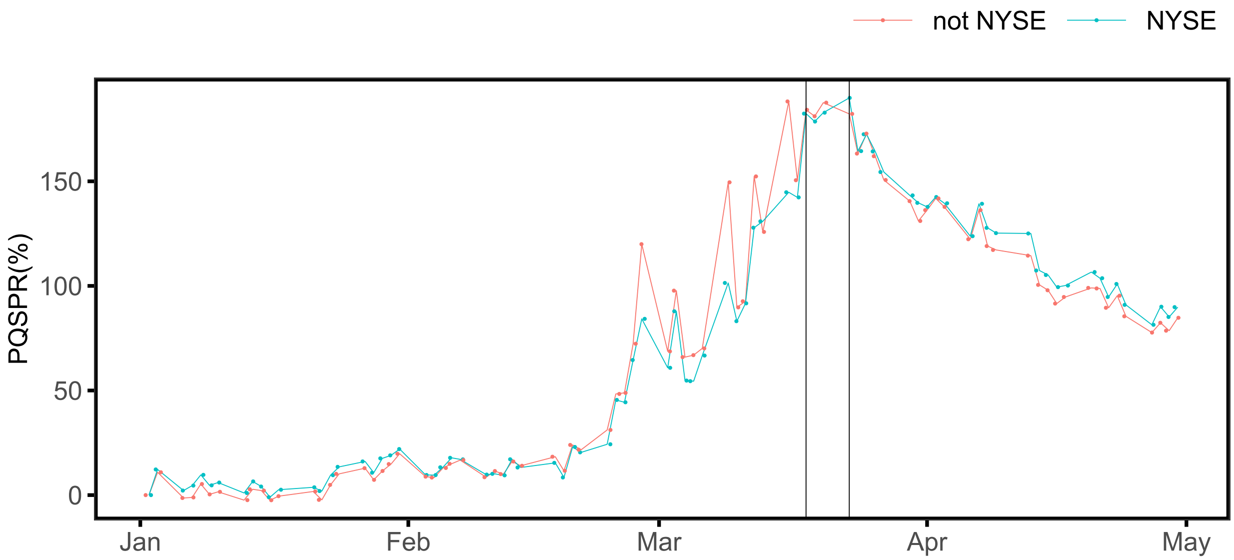

Table I – Cross-sectional summary statistics of time-series averages, Jan 2020 - Apr 2020 This table reports cross-sectional averages, standard deviations, and 5, 50, and 95 percentiles of daily time-series averages by stock. The sample consist of all US common stocks listed at the NYSE or Nasdaq, with a single share class, and with a market capitalization of more than USD 500 million as of December 2019. PQSPR is the time-weighted average of proportional quoted spread (the difference between the ask and the bid price, scaled by the midpoint price); PESPR is the dollar-weighted average of effective quoted spread (two times the difference between the signed trade price and the prevailing midpoint price, scaled by the midpoint price); Price is the closing price; Volume is trade volume in million dollars; Market Cap. is the average price times shares outstanding in million dollars in Dec 2019; and $Log|PricingError|$ is logarithm of the absolute (Hasbrouck, 1993) pricing error. All variables (except Price and Market Cap.) are estimated over the continuous trading session only, i.e., from 9:30 ET to 16:00 ET.

	(1) #Stocks	(2) Mean	(3) SD	(4) 5%	(5) Median	(6) 95%
PQSRP [%]	1,604	0.29	0.28	0.05	0.22	0.73
PESRP [%]	1,604	0.18	0.14	0.05	0.14	0.42
Price [USD]	1,604	67.44	104.88	6.90	39.73	207.74
Volume [million USD]	1,604	175,799	591,139	3,151	39,796	682,608
Market Cap. [million USD]	1,604	17,193	61,456	623	3,336	67,527
$Log\|PricingError\|$	1,599	-8.60	0.77	-9.85	-8.57	-7.44

Table II – Overview of propensity score matching

This table reports the propensity score matching (PSM) of treated stocks (NYSE listed stocks) and controls (Nasdaq listed stocks). We match stocks using one-to-one nearest neighbor PSM, without replacement using average price and trading volume from one week before the event, i.e., from Mar, 16th 2020 to Mar, 20th 2020; mMrket Cap. as of Dec 2019 and by Fama and French 48 industry classifications. The event is the closure of the NYSE floor on Mar, 23rd 2020. Panel A reports summary statistics for the matched sample of treatment and control firms. Panel B reports the 1, 5, 50, 95, and 99 percentiles of estimated propensity scores for the matched sample. Panel D reports univariate comparisons between stock characteristics of the matched sample of treatment and control stocks.

Panel A: Summary Statistics for the Matched Sample

	#Stocks	Mean	SD	5%	Median	95%
PQSRP	1,236	0.31	0.30	0.05	0.24	0.83
PESRP	1,236	0.19	0.15	0.06	0.15	0.44
Price	1,236	64.60	105.49	6.56	39.73	184.64
Volume	1,236	171,775	650,543	2,738	33,531	644,952
Market Cap.	1,236	17,261	66,773	615	2,980	67,193
$Log\|PricingError\|$	1,124	-8.33	0.75	-9.56	-8.30	-7.21

Panel B: Estimated Propensity Score Distributions

	1%	5%	50%	95%	99%
Treatment	0.36	0.46	0.51	0.55	0.72
Control	0.30	0.43	0.50	0.52	0.57

Panel C: Differences in covariates pre-event

	Treatment	Control	Diff	t-stat
Price	45.02	55.08	10.07	1.96
Volume	240,262	235,010	-5,252	-0.09
Market Cap.	18,955	15,566	-3,388	-0.89
$PQSPR$	0.56	0.76	0.20	6.17
$PESPR$	0.30	0.44	0.14	7.80
$Log\|PricingError\|$	-8.28	-7.91	0.37	8.81

Table III – Difference-in-differences Regression: PESPR

This table shows results from a difference-in-differences regression of effective spreads (*PESPR*) before versus after the closure of the NYSE floor, comparing NYSE listed stocks to matched Nasdaq stocks (Panel A) and comparing *PESPR* of NYSE listed stocks on the NYSE versus off the NYSE (Panel B). We estimate the following fixed-effect panel regression using data from one-week before to one-week after the closure of the NYSE floor (March 16, 2020 through March 27, 2020.):

$$PESPR_{i,e,t} = FE + \beta_0 \times Treated_{i,e,t} After_{i,e,t} + \beta_1 \times Treated_{i,e,t} + \beta_2 \times After_{i,e,t} + \gamma \times C_{i,e,t} + \epsilon_{i,e,t},$$

where $PESPR_{i,e,t}$ is the effective spread of stock i on day t trading on exchange e; $After_{i,e,t}$ is an indicator variable equal to one after the closure of the NYSE floor (March, 23rd 2020) and zero otherwise; $Treated_{i,e,t}$ is an indicator variable equal to one if the stock is listed on the NYSE (Panel A) or equal to one if $PESPR$ is estimated on the NYSE (Panel B) and zero otherwise; and $C_{i,e,t}$ is a vector of additional control variables. In Panel A $PESPR$ is estimated as the trade price across all exchanges in excess of the prevailing midpoint price using NBBO bid and ask prices. In Panel B $PESPR$ on the NYSE is estimated from trades on the NYSE and midpoint prices using bid and ask prices on the NYSE. $PESPR$ on other exchanges is estimated from trades off the NYSE and midpoint prices using the NBBO excluding NYSE bid and ask prices (i.e., using the best bid and ask prices across all exchanges excluding the NYSE). T-statistics are calculated using standard errors clustered by stock are shown below the estimates. Statistical significance at the 1%, 5%, and 10% level is indicated by ***, **, and *, respectively.

	Dependent Variable = $PESPR$			
	(1)	(2)	(3)	(4)
Panel A: NYSE- ($Treated = 1$) versus Nasdaq- ($Treated = 0$) listed stocks				
$Treated \times After$	0.09***	0.09***	0.09***	0.09***
	(7.90)	(7.90)	(7.65)	(8.07)
$After$	-0.11***		-0.10***	
	(-11.65)		(-10.26)	
$Price$			-0.00***	0.00***
			(-4.41)	(5.26)
$Log(Volume)$			0.05***	-0.03**
			(2.87)	(-2.05)
#Stocks	1,236	1,236	1,236	1,236
#Days	10	10	10	10
Within R^2 [%]	1.71	0.59	1.96	0.78
Stock FE	Yes	Yes	Yes	Yes
Day FE	No	Yes	No	Yes

Table III continued

	Dependent Variable = $PESPR$				
	(1)	(2)	(3)	(4)	(5)
Panel B: NYSE stocks traded on the NYSE ($Treated = 1$) versus off the NYSE ($Treated = 0$)					
$Treated \times After$	0.01*** (2.68)	0.01*** (2.68)	0.01*** (2.69)	0.01*** (2.74)	0.02*** (3.64)
$Treated$					0.12*** (24.92)
$After$	0.00 (0.73)		-0.00 (-0.79)		
$Price$			-0.00*** (-6.57)	0.00 (0.00)	
$Log(Volume)$			-0.04*** (-6.93)	-0.05*** (-9.03)	
Within R^2 [%]	0.25	0.11	2.54	1.59	37.56
#Stocks	552	552	552	552	552
#Days	10	10	10	10	10
Exchange × Stock FE	Yes	Yes	Yes	Yes	No
Day FE	No	Yes	No	Yes	No
Day × Stock FE	No	No	No	No	Yes

Table IV – Difference-in-differences Regression: PESPR, Feb. 1st 2020 to Apr. 30th 2020 This table shows results from a difference-in-differences regression of effective spreads (*PESPR*) before versus after the closure of the NYSE floor, comparing NYSE listed stocks to matched Nasdaq stocks (Panel A) and comparing *PESPR* of NYSE listed stocks on the NYSE and off the NYSE (Panel B). We estimate the following fixed-effect panel regression using data from Feb, 1st 2020 to Apr, 30th 2020:

$$PESPR_{i,e,t} = FE + \beta_0 \times Treated_{i,e,t}After_{i,e,t} + \beta_1 \times Treated_{i,e,t} + \beta_2 \times After_{i,e,t} + \gamma \times C_{i,e,t} + \epsilon_{i,e,t},$$

where $PESPR_{i,e,t}$ is the effective spread of stock i on day t trading on exchange e; $After_{i,e,t}$ is an indicator variable equal to one after the closure of the NYSE floor (March, 23rd 2020) and zero otherwise; $Treated_{i,e,t}$ is an indicator variable equal to one if the stock is listed on the NYSE (Panel A) or equal to one if $PESPR$ is estimated on the NYSE (Panel B) and zero otherwise; and $C_{i,e,t}$ is a vector of additional control variables. In Panel A *PESPR* is estimated as the trade price across all exchanges in excess of the prevailing midpoint price using NBBO bid and ask prices. In Panel B *PESPR* on the NYSE is estimated from trades on the NYSE and midpoint prices using bid and ask prices on the NYSE. *PESPR* on other exchanges is estimated from trades off the NYSE and midpoint price using the NBBO excluding NYSE bid and ask prices (i.e., using the best bid and ask prices across all exchanges excluding the NYSE). T-statistics are calculated using standard errors clustered by stock are shown below the estimates. Statistical significance at the 1%, 5%, and 10% level is indicated by ***, **, and *, respectively.

	Dependent Variable = $PESPR$			
	(1)	(2)	(3)	(4)
Panel A: NYSE- ($Treated = 1$) versus Nasdaq- ($Treated = 0$) listed stocks				
$Treated \times After$	0.02***	0.02***	0.02***	0.02***
	(4.56)	(4.53)	(3.65)	(4.22)
$After$	0.05***		0.04***	
	(14.77)		(9.10)	
$Price$			-0.00***	0.00***
			(-4.21)	(3.79)
$Log(Volume)$			0.03***	-0.02***
			(8.39)	(-5.50)
Within R^2 [%]	2.39	0.08	5.04	0.27
#Stocks	1,236	1,236	1,236	1,236
#Days	59	59	59	59
Stock FE	Yes	Yes	Yes	Yes
Day FE	No	Yes	No	Yes

Table IV continued

	Dependent Variable = $PESPR$				
	(1)	(2)	(3)	(4)	(5)
Panel B: NYSE stocks traded on the NYSE ($Treated = 1$) versus off the NYSE ($Treated = 0$)					
$Treated \times After$	0.05***	0.05***	0.05***	0.05***	0.05***
	(7.34)	(7.35)	(6.79)	(7.37)	(14.63)
$Treated$					0.05***
					(20.81)
$After$	0.08***		0.05***		
	(24.84)		(8.48)		
$Price$			-0.00***	0.00	
			(-6.73)	(0.05)	
$Log(Volume)$			0.02***	-0.02***	
			(6.19)	(-5.68)	
Within R^2 [%]	11.34	0.92	18.83	1.21	22.04
#Stocks	553	553	553	553	553
#Days	59	59	59	59	59
Exchange × Stock FE	Yes	Yes	Yes	Yes	No
Day FE	No	Yes	No	Yes	No
Day × Stock FE	No	No	No	No	Yes

Table V – Difference-in-differences Regression: PQSPR

This table shows results from a difference-in-differences regression of quoted spreads ($PQSPR$) before versus after the closure of the NYSE floor, comparing NYSE listed stocks to matched Nasdaq stocks (Panel A) and comparing $PQSPR$ of NYSE listed stocks on the NYSE and off the NYSE (Panel B). We estimate the following fixed-effect panel regressions using data from one-week before to one-week after the closure of the NYSE floor (March 16, 2020 through March 27, 2020):

$$PQSPR_{i,e,t} = FE + \beta_0 \times Treated_{i,e,t} After_{i,e,t} + \beta_1 \times Treated_{i,e,t} + \beta_2 \times After_{i,e,t} + \gamma \times C_{i,e,t} + \epsilon_{i,e,t},$$

where $PQSPR_{i,e,t}$ is the quoted spread of stock i on day t trading on exchange e; $After_{i,e,t}$ is an indicator variable equal to one after the closure of the NYSE floor (March, 23rd 2020) and zero otherwise; $Treated_{i,e,t}$ is an indicator variable equal to one if the stock is listed on the NYSE (Panel A) or equal to one if $PQSPR$ is estimated on the NYSE (Panel B) and zero otherwise; and $C_{i,e,t}$ is a vector of additional control variables. In Panel A $PQSPR$ is estimated using NBBO bid and ask prices. In Panel B $PQSPR$ on the NYSE is estimated using bid and ask prices on the NYSE. $PQSPR$ on other exchanges is estimated using the NBBO excluding NYSE bid and ask prices (i.e., using the best bid and ask prices across all exchanges excluding the NYSE). T-statistics are calculated using standard errors clustered by stock are shown below the estimates. Statistical significance at the 1%, 5%, and 10% level is indicated by ***, **, and *, respectively.

	Dependent Variable = $PQSPR$			
	(1)	(2)	(3)	(4)
Panel A: NYSE- ($Treated = 1$) versus Nasdaq- ($Treated = 0$) listed stocks				
$Treated \times After$	0.02*	0.02*	0.02*	0.02**
	(1.74)	(1.74)	(1.73)	(2.09)
$After$	-0.06***		-0.08***	
	(-6.94)		(-8.85)	
$Price$			-0.00***	0.00***
			(-4.43)	(4.58)
$Log(Volume)$			-0.10***	-0.11***
			(-11.17)	(-11.83)
Within R^2 [%]	1.12	0.04	2.59	1.57
#Stocks	1,236	1,236	1,236	1,236
#Days	10	10	10	10
Stock FE	Yes	Yes	Yes	Yes
Day FE	No	Yes	No	Yes

Table V continued

	Dependent Variable = $PQSPR$				
	(1)	(2)	(3)	(4)	(5)
Panel B: NYSE stocks traded on the NYSE ($Treated = 1$) versus off the NYSE ($Treated = 0$)					
$Treated \times After$	0.11***	0.11***	0.11***	0.11***	0.10***
	(10.98)	(10.98)	(11.14)	(11.22)	(16.07)
$Treated$					-0.01***
					(-3.03)
$After$	-0.06***		-0.07***		
	(-9.35)		(-10.22)		
$Price$			-0.00***	0.00***	
			(-7.41)	(2.65)	
$Log(Volume)$			-0.07***	-0.09***	
			(-7.74)	(-9.13)	
Within R^2 [%]	1.77	1.98	3.85	3.40	7.75
#Stocks	552	552	552	552	552
#Days	10	10	10	10	10
Exchange × Stock FE	Yes	Yes	Yes	Yes	No
Day FE	No	Yes	No	Yes	No
Day × Stock FE	No	No	No	No	Yes

Table VI – Difference-in-differences Regression: Hasbrouck Pricing Error
This table shows results from a difference-in-differences regression of Hasbrouck (1993) pricing errors before versus after the closure of the NYSE floor, comparing NYSE listed stocks to matched Nasdaq stocks (Panel A) and comparing NYSE listed stocks on the NYSE and off the NYSE (Panel B). We estimate the following fixed-effect panel regression using data from one-week before to one-week after the closure of the NYSE floor (March 16, 2020 through March 27), 2020:

$$Log|PricingError_{i,e,t}| = FE + \beta_0 \times Treated_{i,e,t}After_{i,e,t} + \beta_1 \times Treated_{i,e,t} + \beta_2 \times After_{i,e,t} + \gamma \times C_{i,e,t} + \epsilon_{i,e,t},$$

where $Log|PricingError_{i,e,t}|$ is the logarithm of the average absolute pricing error of stock i on day t trading on exchange e; $After_{i,e,t}$ is an indicator variable equal to one after the closure of the NYSE floor (March, 23rd 2020) and zero otherwise; $Treated_{i,e,t}$ is an indicator variable equal to one if the stock is listed on the NYSE (Panel A) or equal to one if the pricing error is estimated on the NYSE (Panel B) and zero otherwise; and $C_{i,e,t}$ is a vector of additional control variables. We estimate Hasbrouck (1993) pricing errors as in Rösch et al. (2017) (Section 1.3). Using Equation (13) in Hasbrouck (1993), we estimate the pricing error associated with each trade, and then average absolute pricing errors across all exchanges (Panel A) and separately by exchange on which the trade occurred (Panel B). T-statistics are calculated using standard errors clustered by stock are shown below the estimates. Statistical significance at the 1%, 5%, and 10% level is indicated by ***, **, and *, respectively.

	Dependent Variable = $Log\|PricingError_{i,e,t}\|$			
	(1)	(2)	(3)	(4)
Panel A: NYSE- ($Treated = 1$) versus Nasdaq- ($Treated = 0$) listed stocks				
$Treated \times After$	0.06***	0.06***	0.06***	0.07***
	(4.73)	(4.73)	(4.46)	(5.18)
$After$	0.17***		0.17***	
	(17.71)		(17.22)	
$Price$			-0.01***	-0.00
			(-10.02)	(-1.23)
$Log(Volume)$			-0.09***	-0.19***
			(-8.16)	(-16.39)
Within R^2 [%]	11.59	0.39	14.58	4.13
#Stocks	1,124	1,124	1,124	1,124
#Days	10	10	10	10
Stock FE	Yes	Yes	Yes	Yes
Day FE	No	Yes	No	Yes

Table VI continued

	Dependent Variable = $Log\vert PricingError_{i,e,t}\vert$				
	(1)	(2)	(3)	(4)	(5)
Panel B: NYSE stocks traded on the NYSE ($Treated = 1$) versus off the NYSE ($Treated = 0$)					
$Treated \times After$	0.02*	0.02*	0.02*	0.02*	0.02***
	(1.84)	(1.84)	(1.91)	(1.91)	(8.56)
$Treated$					-0.01***
					(-3.43)
$After$	0.23***		0.21***		
	(25.18)		(22.43)		
$Price$			-0.01***	0.00**	
			(-7.40)	(2.13)	
$Log(Volume)$			-0.10***	-0.21***	
			(-9.40)	(-18.01)	
Within R^2 [%]	15.30	0.06	17.91	4.39	1.59
#Stocks	552	552	552	552	552
#Days	10	10	10	10	10
Exchange × Stock FE	Yes	Yes	Yes	Yes	No
Day FE	No	Yes	No	Yes	No
Day × Stock FE	No	No	No	No	Yes

Table VII – Difference-in-differences Regression: intraday PESPR
This table shows results from a difference-in-differences regression of effective spreads ($PESPR$) before versus after the closure of the NYSE floor, comparing $PESPR$ of NYSE listed stocks on the NYSE and off the NYSE. For each 30-minute interval during the day we estimate the following fixed-effect panel regression using data from one-week before to one-week after the closure of the NYSE floor (March 16, 2020 through March 27, 2020):

$$PESPR_{i,e,t} = FE + \beta_0 \times Treated_{i,e,t} After_{i,e,t} + \beta_1 \times Treated_{i,e,t} + \beta_2 \times After_{i,e,t} + \gamma \times C_{i,e,t} + \epsilon_{i,e,t},$$

where $PESPR_{i,e,t}$ is the effective spread of stock i on day t trading on exchange e; $After_{i,e,t}$ is an indicator variable equal to one after the closure of the NYSE floor (March, 23rd 2020) and zero otherwise; $Treated_{i,e,t}$ is an indicator variable equal to one if $PESPR$ is estimated on the NYSE and zero otherwise; and $C_{i,e,t}$ is a vector of additional control variables. $PESPR$ on the NYSE is estimated from trade prices on the NYSE in excess of the prevailing midpoint price using bid and ask prices on the NYSE. $PESPR$ on other exchanges is estimated from trade prices off the NYSE in excess of the prevailing midpoint price using NBBO excluding NYSE bid and ask prices (i.e., using the best bid and ask prices across all exchanges excluding the NYSE). Panel A displays results during the morning session while Panel B displays results during the afternoon session. In Panel C, we combine all of the half-hour intervals into one panel and interact $after \times Treated$ with a measure of complexity, where complexity is defined as $1 - R^2$ from a regression of returns in stock i on the returns of the market in each half-hour interval. T-statistics are calculated using standard errors clustered by stock are shown below the estimates. Statistical significance at the 1%, 5%, and 10% level is indicated by ***, **, and *, respectively.

Panel A: Morning	10:00	10:30	11:00	11:30	12:00	12:30
$Treated \times After$	0.08***	0.06***	0.04***	0.02***	0.00	0.00
	(5.38)	(6.69)	(4.97)	(3.14)	(-0.20)	(-0.07)
$Treated$	0.14***	0.13***	0.12***	0.13***	0.14***	0.13***
	(12.56)	(18.45)	(18.32)	(17.84)	(21.49)	(20.73)
Within R^2 [%]	11.10	18.01	23.54	20.21	21.26	18.48
#Stocks	553	553	553	553	553	553
#Days	10	10	10	10	10	10
Stock×Day FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel B: Afternoon	13:00	13:30	14:00	14:30	15:00	15:30	16:00
$Treated \times After$	0.01	0.00	0.00	-0.01	0.00	0.00	0.00
	(1.11)	(-0.58)	(-0.74)	(-1.39)	(-0.30)	(-0.52)	(1.53)
$Treated$	0.12***	0.12***	0.13***	0.12***	0.13***	0.12***	0.04***
	(21.13)	(20.69)	(22.29)	(19.34)	(22.63)	(21.94)	(16.23)
Within R^2 [%]	20.48	18.11	20.02	18.56	22.41	21.30	17.10
#Stocks	553	553	553	553	553	553	553
#Days	10	10	10	10	10	10	10
Stock×Day FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table VII continued

Panel C: Explaining $PESPR$ by intraday variation in complexity $(1 - R^2)$

$Complex \times Treated \times After$	0.17*** (7.33)
$Treated \times After$	-0.12*** (-7.84)
$Complex \times After$	0.01 (0.65)
$Complex \times Treated$	0.23*** (8.51)
$Complex$	0.58*** (2044)
Within R^2 [%]	11.13
#Obs.	143,144
Stock×Day FE	Yes

Table VIII – Difference-in-difference Regression: Auction Deviation

This table shows results from a difference-in-differences regression of auction price deviations ($|Deviation\%|$) for opening and closing auction prices before versus after the closure of the NYSE floor. We estimate the following fixed-effect panel regression using data from one-week before to one-week after the event (March 16, 2020 through March 27, 2020):

$$|Deviation\%|_{i,e,t} = FE + \beta_0 \times Treated_{i,e,t} After_{i,e,t} + \beta_1 \times Treated_{i,e,t} + \beta_2 \times After_{i,e,t} + \gamma \times C_{i,e,t} + \epsilon_{i,e,t},$$

where $|Deviation\%|_{i,e,t}$ is the deviation of stock i on day t trading on exchange e; $After_{i,e,t}$ is an indicator variable equal to one after the event and zero otherwise; $Treated_{i,e,t}$ is an indicator variable equal to one if the stock is listed on the NYSE and zero otherwise; and $C_{i,e,t}$ is a vector of additional control variables. $|Deviation\%|$ is estimated as two times the absolute difference in the logarithm of the auction price and the prevailing (open auction) or last midpoint price during the continuous trading session (close auction) using national best bid and ask prices. T-statistics are calculated using standard errors clustered by stock are shown below the estimates. Statistical significance at the 1%, 5%, and 10% level is indicated by ***, **, and *, respectively.

	Dependent = Open Auction $\|Deviation\%\|$				Dependent = Close Auction $\|Deviation\%\|$			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Treated$	0.34*	0.46**	0.37**	0.47***	0.32***	0.32***	0.32***	0.32***
$\times After$	(1.93)	(2.57)	(2.09)	(2.60)	(10.42)	(10.42)	(10.26)	(10.26)
$After$	-1.10***		-1.29***		-0.17***		-0.15***	
	(-6.71)		(-7.32)		(-7.30)		(-6.08)	
$Price$			0.02***	0.01*			-0.00	0.00
			(3.20)	(1.74)			(-0.02)	(0.36)
$Log(Volume)$			-0.62***	0.11			0.07**	0.06*
			(-3.68)	(0.60)			(2.22)	(1.85)
WithinR^2[%]	1.1	0.07	1.27	0.1	1.13	1.14	1.19	1.18
#Stocks	1121	1121	1121	1121	1124	1124	1124	1124
#Days	10	10	10	10	10	10	10	10
Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day FE	No	Yes	No	Yes	No	Yes	No	Yes

Table IX – Difference-in-difference Regression: Re-opening analysis

This table shows results from a difference-in-differences regression of market quality in NYSE listed stocks before versus after the re-opening of the NYSE floor for trading on the NYSE versus off the NYSE. Panel A shows the difference-in-difference in effective spreads (*PESPR*), quoted spreads (*PQSPR*), pricing errors (*HAS*), and *open* and *close* auction price deviations. Panels B and C shows the difference-in-difference in *PESPR* within each 30-minute interval during the continuous trading session. We estimate the following fixed-effect panel regression using data one-week before the first re-opening (May 26, 2020) and one-week after the second re-opening (June 17, 2020) of the NYSE floor (May 18, 2020 through May 22, 2020 and June 17, 2020 through June 23, 2020.):

$$QUALITY_{i,e,t} = FE + \beta_0(Treated_{i,e,t} \times After_{i,e,t}) + \beta_1 Treated_{i,e,t} + \beta_2 After_{i,e,t} + \epsilon_{i,e,t},$$

where $QUALITY_{i,e,t}$ is one of our market quality estimates of stock i on day t trading on exchange e; $After_{i,e,t}$ is an indicator variable equal to one after the second re-opening of the NYSE floor and zero otherwise; $Treated_{i,e,t}$ is an indicator variable equal to one if $QUALITY$ is estimated on the NYSE and zero otherwise. For details of how $QUALITY$ is estimated, see Tables III, V, VI, and VIII. T-statistics are calculated using standard errors clustered by stock are shown below the estimates. Statistical significance at the 1%, 5%, and 10% level is indicated by ***, **, and *, respectively.

	(1) *PESPR*	(2) *PQSPR*	(3) *HAS*	(4) *Open*	(5) *Close*
Panel A: Daily market quality and auction deviations					
$Treated \times After$	-0.01***	-0.01***	-0.01***	0.55***	-0.02***
	(-5.44)	(-8.69)	(-2.62)	(7.18)	(-2.83)
$Treated$	0.04***	0.04***	0.04***		
	(18.98)	(15.90)	(16.94)		
Within R^2 [%]	31.93	20.39	11.17	0.77	0.10
#Stocks	550	550	550	1,116	1,116
#Days	10	10	10	10	10
Exchange × Stock FE	No	No	No	Yes	Yes
Day FE	No	No	No	Yes	Yes
Day × Stock FE	Yes	Yes	Yes	No	No

Table IX continued, intraday PESPR

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel B: Morning	10:00	10:30	11:00	11:30	12:00	12:30	
$Treated \times After$	-0.02***	-0.02***	-0.01***	-0.01***	-0.01***	-0.00**	
	(-4.04)	(-5.44)	(-5.07)	(-4.22)	(-4.69)	(-2.00)	
$Treated$	0.10***	0.06***	0.05***	0.04***	0.04***	0.03***	
	(19.75)	(18.52)	(19.57)	(15.20)	(18.50)	(13.65)	
Within R^2 [%]	16.11	19.96	20.82	17.34	20.56	13.33	
#Stocks	549	549	549	549	549	549	
#Days	10	10	10	10	10	10	
Stock×Day FE	Yes	Yes	Yes	Yes	Yes	Yes	
Panel C: Afternoon	13:00	13:30	14:00	14:30	15:00	15:30	16:00
$Treated \times After$	-0.01***	-0.00	-0.00	-0.00	-0.00	-0.00***	-0.00*
	(-2.77)	(-0.71)	(-1.61)	(-0.99)	(-0.72)	(-2.66)	(-1.94)
$Treated$	0.03***	0.03***	0.03***	0.02***	0.02***	0.03***	0.01***
	(9.78)	(14.87)	(12.25)	(10.87)	(10.78)	(11.89)	(12.92)
Within R^2 [%]	10.55	10.64	10.07	7.39	8.25	10.08	9.15
#Stocks	550	548	549	550	549	550	550
#Days	10	10	10	10	10	10	10
Stock×Day FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Internet Appendix for "Does Floor Trading Matter?"

JONATHAN BROGAARD, MATTHEW C. RINGGENBERG, AND DOMINIK ROESCH[17]

This Appendix provides additional empirical evidence to supplement the analyses provided in the main paper. Below, we briefly describe the analyses.

- Table A1 presents results from a difference-in-differences regression of effective spreads (*PESPR*) before versus after the closure of the NYSE floor, comparing *PESPR* of NYSE listed stocks on the NYSE and outside. It is similar to Table VII in the main paper except we drop all trades with a trade price equal to the midpoint of the prevailing bid and ask quotes.

- Table A2 presents results from a difference-in-differences regression of effective spreads (*PESPR*) before versus after the closure of the NYSE floor, comparing *PESPR* of NYSE listed stocks on the NYSE and off the NYSE. It is similar to Table VII in the main paper except we drop all trades before the opening price auction.

- Table A3 presents results from a difference-in-differences regression of effective spreads (*PESPR*) before and after the closure of the NYSE floor, comparing NYSE listed stocks to matched Nasdaq stocks. It is similar to Table VII in the main paper except we compare PESPR across NYSE and matched Nasdaq stocks (as in in Panel A of Table III).

- Table A4 presents results from a difference-in-differences regression of USD Volume before versus after the closure of the NYSE floor, comparing trading volume of NYSE listed stocks on the NYSE and off the NYSE.

- Table A5 presents results from a difference-in-differences regression of intraday trading volume before versus after the closure of the NYSE floor, comparing trading volume of NYSE

[17]Citation format: Brogaard, Jonathan, Matthew C. Ringgenberg, and Dominik Roesch, Internet Appendix for "Does Floor Trading Matter?," 2021, Working Paper.

listed stocks on the NYSE and off the NYSE for each 30-minute interval during continuous trading.

- Table A6 presents results from a difference-in-differences regression of USD open auction volume before versus after the closure of the NYSE floor, comparing NYSE listed to Nasdaq listed stocks.

- Table A7 presents results from a difference-in-differences regression of USD close auction volume before versus after the closure of the NYSE floor, comparing NYSE listed to Nasdaq listed stocks.

- Table A8 presents results from a difference-in-differences regression of effective spreads (*PESPR*) before versus after the closure of the NYSE floor, comparing NYSE listed stocks to matched Nasdaq stocks (Panel A) and comparing *PESPR* of NYSE listed stocks on the NYSE and off the NYSE (Panel B). The sample consists of only large stocks, defined as stocks with an above median average market capitalisation in the week before the closure of the NYSE floor.

- Table A9 presents results from a difference-in-differences regression of effective spreads (*PESPR*) before versus after the closure of the NYSE floor, comparing NYSE listed stocks to matched Nasdaq stocks (Panel A) and comparing *PESPR* of NYSE listed stocks on the NYSE and off the NYSE (Panel B). The sample consists of only small stocks, defined as stocks with a below median average market capitalisation in the week before the closure of the NYSE floor.

Table A1 – Difference-in-differences Regression: intraday PESPR (dropping midpoint trades)

This table shows results from a difference-in-differences regression of effective spreads (*PESPR*) before versus after the closure of the NYSE floor, comparing *PESPR* of NYSE listed stocks on the NYSE and outside. It is similar to Table VII in the main paper except we drop all trades with a trade price equal to the midpoint of the prevailing bid and ask quotes. For each 30-minute interval during the continuous trading session, we estimate the following fixed-effect panel regressions using data from one-week before to one-week after the closure of the NYSE floor:

$$PESPR_{i,e,t} = FE + \beta_0 \times Treated_{i,e,t}After_{i,e,t} + \beta_2 \times After_{i,e,t} + \beta_1 \times Treated_{i,e,t} + \gamma \times C_{i,e,t} + \epsilon_{i,e,t},$$

where $PESPR_{i,e,t}$ is the effective spread of stock i on day t trading on exchange e; $After_{i,e,t}$ is an indicator variable equal to one after the closure of the NYSE floor (March, 23rd 2020) and zero otherwise; $Treated_{i,e,t}$ is an indicator variable equal to one if *PESPR* is estimated on the NYSE and zero otherwise; and $C_{i,e,t}$ is a vector of additional control variables. *PESPR* on the NYSE is estimated from trade prices on the NYSE in excess of the prevailing midpoint price using bid and ask prices on the NYSE. *PESPR* on other exchanges is estimated from trade prices outside the NYSE in excess of the prevailing midpoint price using NBBO excluding NYSE bid and ask prices (i.e., using the best bid and ask prices across all exchanges excluding the NYSE). T-statistics calculated using standard errors clustered by stock are shown below the estimates. Statistical significance at the 1%, 5%, and 10% level is indicated by ***, **, and *, respectively. Data is from CRSP and TAQ for the period March 16, 2020 through March 27, 2020.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Morning	10:00	10:30	11:00	11:30	12:00	12:30	
$after \times treated$	0.07***	0.05***	0.04***	0.02***	0.00	0.00	
	(4.45)	(6.31)	(4.71)	(2.90)	(0.03)	(0.24)	
$treated$	0.09***	0.11***	0.10***	0.11***	0.12***	0.11***	
	(8.77)	(15.64)	(15.64)	(15.35)	(19.30)	(18.48)	
Within R^2 [%]	6.20	13.57	18.03	15.96	16.99	14.54	
#Stocks	553	553	553	553	553	553	
#Days	10	10	10	10	10	10	
Stock×Day FE	Yes	Yes	Yes	Yes	Yes	Yes	
Panel B: Afternoon	13:00	13:30	14:00	14:30	15:00	15:30	16:00
$after \times treated$	0.01	-0.00	-0.00	-0.01	-0.00	0.00	0.00
	(1.28)	(-0.41)	(-0.72)	(-1.40)	(-0.09)	(0.28)	(0.15)
$treated$	0.10***	0.10***	0.11***	0.10***	0.11***	0.10***	0.03***
	(18.39)	(18.15)	(20.21)	(17.62)	(20.68)	(20.11)	(10.37)
Within R^2 [%]	16.17	13.42	15.92	14.27	18.54	17.72	7.62
#Stocks	553	553	553	553	553	553	553
#Days	10	10	10	10	10	10	10
Stock×Day FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A2 – Difference-in-differences Regression: intraday PESPR (dropping trades before open)

This table shows results from a difference-in-differences regression of effective spreads (*PESPR*) before versus after the closure of the NYSE floor, comparing *PESPR* of NYSE listed stocks on the NYSE and off the NYSE. It is similar to Table VII in the main paper except we drop all trades before the opening price auction. For each 30-minute interval during the continuous trading session, we estimate the following fixed-effect panel regressions using data from one-week before to one-week after the closure of the NYSE floor:

$$PESPR_{i,e,t} = FE + \beta_0 \times Treated_{i,e,t} After_{i,e,t} + \beta_2 \times After_{i,e,t} + \beta_1 \times Treated_{i,e,t} + \gamma \times C_{i,e,t} + \epsilon_{i,e,t},$$

where $PESPR_{i,e,t}$ is the effective spread of stock i on day t trading on exchange e; $After_{i,e,t}$ is an indicator variable equal to one after the closure of the NYSE floor (March, 23rd 2020) and zero otherwise; $Treated_{i,e,t}$ is an indicator variable equal to one if $PESPR$ is estimated on the NYSE and zero otherwise; and $C_{i,e,t}$ is a vector of additional control variables. *PESPR* on the NYSE is estimated from trade prices on the NYSE in excess of the prevailing midpoint price using bid and ask prices on the NYSE. *PESPR* on other exchanges is estimated from trade prices outside the NYSE in excess of the prevailing midpoint price using NBBO excluding NYSE bid and ask prices (i.e., using the best bid and ask prices across all exchanges excluding the NYSE). T-statistics calculated using standard errors clustered by stock are shown below the estimates. Statistical significance at the 1%, 5%, and 10% level is indicated by ***, **, and *, respectively. Data is from CRSP and TAQ for the period March 16, 2020 through March 27, 2020.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Morning	10:00	10:30	11:00	11:30	12:00	12:30	
$after \times treated$	0.07***	0.06***	0.04***	0.02***	-0.00	-0.00	
	(5.09)	(6.62)	(4.94)	(2.93)	(-0.34)	(-0.12)	
$treated$	0.14***	0.13***	0.12***	0.13***	0.14***	0.13***	
	(13.55)	(18.93)	(18.67)	(18.16)	(21.68)	(21.04)	
Within R^2 [%]	0.1154	0.1801	0.2362	0.2028	0.2120	0.1853	
#Stocks	553	553	553	553	553	553	
#Days	10	10	10	10	10	10	
Stock×Day FE	Yes	Yes	Yes	Yes	Yes	Yes	
Panel B: Afternoon	13:00	13:30	14:00	14:30	15:00	15:30	16:00
$after \times treated$	0.00	-0.00	-0.01	-0.01	-0.00	-0.00	-0.00
	(0.82)	(-0.71)	(-0.90)	(-1.62)	(-0.46)	(-0.63)	(-1.37)
$treated$	0.12***	0.12***	0.13***	0.12***	0.13***	0.12***	0.04***
	(21.54)	(21.01)	(22.19)	(19.62)	(22.65)	(22.09)	(16.04)
Within R^2 [%]	0.2048	0.1812	0.1984	0.1863	0.2227	0.2127	0.1632
#Stocks	553	553	553	553	553	553	553
#Days	10	10	10	10	10	10	10
Stock×Day FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A3 – Difference-in-differences Regression: intraday PESPR (comparing NYSE and Nasdaq stocks)

This table shows results from a difference-in-differences regression of effective spreads ($PESPR$) before and after the closure of the NYSE floor, comparing NYSE listed stocks to matched Nasdaq stocks. It is similar to Table VII in the main paper except we compare PESPR across NYSE and matched Nasdaq stocks (as in in Panel A of Table III). For each 30-minute interval during the continuous trading session, we estimate the following fixed-effect panel regressions using data from one-week before to one-week after the closure of the NYSE floor:

$$PESPR_{i,e,t} = FE + \beta_0 \times Treated_{i,e,t} After_{i,e,t} + \beta_2 \times After_{i,e,t} + \beta_1 \times Treated_{i,e,t} + \gamma \times C_{i,e,t} + \epsilon_{i,e,t},$$

where $PESPR_{i,e,t}$ is the effective spread of stock i on day t trading on exchange e; $After_{i,e,t}$ is an indicator variable equal to one after the closure of the NYSE floor (March, 23rd 2020) and zero otherwise; $Treated_{i,e,t}$ is an indicator variable equal to one if $PESPR$ is estimated for NYSE listed stocks and zero otherwise; and $C_{i,e,t}$ is a vector of additional control variables. $PESPR$ is estimated as the trade price across all exchanges in excess of the prevailing midpoint price using NBBO bid and ask prices. T-statistics calculated using standard errors clustered by stock are shown below the estimates. Statistical significance at the 1%, 5%, and 10% level is indicated by ***, **, and *, respectively. Data is from CRSP and TAQ for the period March 16, 2020 through March 27, 2020.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Morning	10:00	10:30	11:00	11:30	12:00	12:30	
$after \times treated$	0.14***	0.10***	0.07***	0.05***	0.07***	0.08***	
	(2.80)	(2.81)	(3.38)	(3.36)	(4.21)	(3.05)	
Within R^2 [%]	0.05	0.09	0.07	0.16	0.33	0.19	
#Stocks	1,236	1,236	1,236	1,236	1,236	1,236	
#Days	10	10	10	10	10	10	
EX×Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	
Day FE	Yes	Yes	Yes	Yes	Yes	Yes	
Panel B: Afternoon	13:00	13:30	14:00	14:30	15:00	15:30	16:00
$after \times treated$	0.05***	0.06***	0.06***	0.06***	0.05***	0.05***	0.03***
	(2.65)	(3.15)	(2.79)	(2.61)	(3.46)	(4.04)	(4.42)
Within R^2 [%]	0.13	0.20	0.12	0.07	0.15	0.20	0.24
#Stocks	1,236	1,236	1,236	1,236	1,236	1,236	1,236
#Days	10	10	10	10	10	10	10
EX×Stock FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table A4 – Difference-in-difference Regression: USD Volume
This table shows results from a difference-in-differences regression of USD Volume before versus after the closure of the NYSE floor, comparing trading volume of NYSE listed stocks on the NYSE and off the NYSE. We estimate the following fixed-effect panel regression using data from one-week before to one-week after the closure of the NYSE floor:

$$log\left(\$Volume_{i,e,t}\right) = FE + \beta_0 \times Treated_{i,e,t}After_{i,e,t} + \beta_2 \times After_{i,e,t} + \beta_1 \times Treated_{i,e,t} + \gamma \times C_{i,e,t} + \epsilon_{i,e,t},$$

where $\$Volume_{i,e,t}$ is the trading volume in USD of stock i on day t trading on exchange e; $After_{i,e,t}$ is an indicator variable equal to one after the closure of the NYSE floor (March, 23rd 2020) and zero otherwise; $Treated_{i,e,t}$ is an indicator variable equal to one if $Volume$ is estimated on the NYSE and zero otherwise; and $C_{i,e,t}$ is a vector of additional control variables. T-statistics calculated using standard errors clustered by stock are shown below the estimates. Statistical significance at the 1%, 5%, and 10% level is indicated by ***, **, and *, respectively. Data is from CRSP and TAQ for the period March 16, 2020 through March 27, 2020.

	log USD Volume (1)	log USD Volume (2)	log USD Volume (3)	log USD Volume (4)
$treated \times after$	-0.12***	-0.12***	-0.12***	-0.12***
	(-7.06)	(-7.06)	(-12.52)	(-12.72)
$after$	-0.20***		-0.03***	
	(-16.68)		(-4.40)	
$Price$			-0.00**	0.00
			(-2.11)	(0.18)
$log(volume)$			0.76***	0.81***
			(37.51)	(33.80)
Within R^2 [%]	16.97	1.11	62.51	54.01
#Stocks	552	552	552	552
#Days	10	10	10	10
Stock FE	Yes	Yes	Yes	Yes
Day FE	No	Yes	No	Yes

Table A5 – Difference-in-difference Regression: intraday trading volume
This table shows results from a difference-in-differences regression of intraday trading volume before versus after the closure of the NYSE floor, comparing trading volume of NYSE listed stocks on the NYSE and off the NYSE. For each 30-minute interval during the continuous trading session, we estimate the following fixed-effect panel regressions using data from one-week before to one-week after the closure of the NYSE floor:

$$log\,(\$Volume_{i,e,t}) = FE + \beta_0 \times Treated_{i,e,t}After_{i,e,t} + \beta_2 \times After_{i,e,t} + \beta_1 \times Treated_{i,e,t} + \gamma \times C_{i,e,t} + \epsilon_{i,e,t},$$

where $\$Volume_{i,e,t}$ is the trading volume in USD of stock i on day t trading on exchange e; $After_{i,e,t}$ is an indicator variable equal to one after the closure of the NYSE floor (March, 23rd 2020) and zero otherwise; $Treated_{i,e,t}$ is an indicator variable equal to one if $Volume$ is estimated on the NYSE and zero otherwise; and $C_{i,e,t}$ is a vector of additional control variables. T-statistics calculated using standard errors clustered by stock are shown below the estimates. Statistical significance at the 1%, 5%, and 10% level is indicated by ***, **, and *, respectively. Data is from CRSP and TAQ for the period March 16, 2020 through March 27, 2020.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Morning	10:00	10:30	11:00	11:30	12:00	12:30	
$after \times treated$	-0.48***	-0.19***	-0.14***	-0.10***	-0.04***	-0.09***	
	(-31.76)	(-12.38)	(-10.08)	(-6.75)	(-2.62)	(-6.05)	
$treated$	-0.08***	-1.40***	-1.43***	-1.46***	-1.49***	-1.45***	
	(-4.41)	(-91.25)	(-99.78)	(-103.3)	(-99.62)	(-97.46)	
Within R^2 [%]	0.2009	0.8655	0.8738	0.8784	0.8699	0.8681	
#Stocks	553	553	553	553	553	553	
#Days	10	10	10	10	10	10	
Stock×Day FE	Yes	Yes	Yes	Yes	Yes	Yes	
Panel B: Afternoon	13:00	13:30	14:00	14:30	15:00	15:30	16:00
$after \times treated$	-0.05***	-0.11***	-0.12***	-0.07***	-0.07***	-0.05***	-0.01
	(-3.47)	(-7.15)	(-8.40)	(-5.24)	(-5.50)	(-3.48)	(-0.89)
$treated$	-1.48***	-1.40***	-1.43***	-1.47***	-1.45***	-1.48***	-1.03***
	(-97.83)	(-91.97)	(-96.95)	(-103.0)	(-103.3)	(-108.0)	(-97.21)
Within R^2 [%]	0.8673	0.8592	0.8709	0.8832	0.8838	0.8910	0.8717
#Stocks	553	553	553	553	553	553	
#Days	10	10	10	10	10	10	
Stock×Day FE	Yes	Yes	Yes	Yes	Yes	Yes	

Table A6 – Difference-in-difference Regression: USD open auction volume

This table shows results from a difference-in-differences regression of USD open auction volume before versus after the closure of the NYSE floor, comparing NYSE listed to Nasdaq listed stocks. We estimate the following fixed-effect panel regression using data from one-week before to one-week after the closure of the NYSE floor:

$$log\,(\$Volume_{i,e,t}) = FE + \beta_0 \times Treated_{i,e,t}After_{i,e,t} + \beta_2 \times After_{i,e,t} + \beta_1 \times Treated_{i,e,t} + \gamma \times C_{i,e,t} + \epsilon_{i,e,t},$$

where $\$Volume_{i,e,t}$ is the trading volume at the open auction in USD of stock i on day t trading on exchange e; $After_{i,e,t}$ is an indicator variable equal to one after the closure of the NYSE floor (March, 23rd 2020) and zero otherwise; $Treated_{i,e,t}$ is an indicator variable equal to one for NYSE listed stocks; and $C_{i,e,t}$ is a vector of additional control variables. T-statistics calculated using standard errors clustered by stock are shown below the estimates. Statistical significance at the 1%, 5%, and 10% level is indicated by ***, **, and *, respectively. Data is from CRSP and TAQ for the period March 16, 2020 through March 27, 2020.

	log USD Volume (1)	log USD Volume (2)	log USD Volume (3)	log USD Volume (4)
$treated \times after$	0.05**	0.05**	0.01	0.05**
	(2.26)	(2.24)	(0.54)	(2.25)
$after$	-0.59***		-0.30***	
	(-34.39)		(-14.05)	
$Price$			-0.02***	0.00***
			(-5.74)	(4.64)
$log(volume)$			1.04***	0.43***
			(24.83)	(14.67)
Within R^2 [%]	9.18	0.06	19.22	4.32
#Stocks	1,123	1,123	1,123	1,123
#Days	10	10	10	10
Stock FE	Yes	Yes	Yes	Yes
Day FE	No	Yes	No	Yes

Table A7 – Difference-in-difference Regression: USD close auction volume

This table shows results from a difference-in-differences regression of USD close auction volume before versus after the closure of the NYSE floor, comparing NYSE listed to Nasdaq listed stocks. We estimate the following fixed-effect panel regression using data from one-week before to one-week after the closure of the NYSE floor:

$$log\left(\$Volume_{i,e,t}\right) = FE + \beta_0 \times Treated_{i,e,t}After_{i,e,t} + \beta_2 \times After_{i,e,t} + \beta_1 \times Treated_{i,e,t} + \gamma \times C_{i,e,t} + \epsilon_{i,e,t},$$

where $\$Volume_{i,e,t}$ is the trading volume at the close auction in USD of stock i on day t trading on exchange e; $After_{i,e,t}$ is an indicator variable equal to one after the closure of the NYSE floor (March, 23rd 2020) and zero otherwise; $Treated_{i,e,t}$ is an indicator variable equal to one for NYSE listed stocks; and $C_{i,e,t}$ is a vector of additional control variables. T-statistics calculated using standard errors clustered by stock are shown below the estimates. Statistical significance at the 1%, 5%, and 10% level is indicated by ***, **, and *, respectively. Data is from CRSP and TAQ for the period March 16, 2020 through March 27, 2020.

	log USD Volume (1)	log USD Volume (2)	log USD Volume (3)	log USD Volume (4)
$treated \times after$	-0.05**	-0.05**	-0.06***	-0.06***
	(-2.42)	(-2.42)	(-3.60)	(-3.70)
$after$	-0.20***		0.02*	
	(-15.61)		(1.68)	
$Price$			0.01***	0.00***
			(5.26)	(3.69)
$log(volume)$			1.05***	0.85***
			(35.42)	(28.37)
Within R^2 [%]	5.62	0.09	42.25	29.57
#Stocks	1,124	1,124	1,124	1,124
#Days	10	10	10	10
Stock FE	Yes	Yes	Yes	Yes
Day FE	No	Yes	No	Yes

Table A8 – Difference-in-differences Regression: PESPR (large stocks)
This table shows results from a difference-in-differences regression of effective spreads (*PESPR*) before versus after the closure of the NYSE floor, comparing NYSE listed stocks to matched Nasdaq stocks (Panel A) and comparing *PESPR* of NYSE listed stocks on the NYSE and off the NYSE (Panel B). The sample consists of only large stocks, defined as stocks with an above median average market capitalisation in the week before the closure of the NYSE floor. We estimate the following fixed-effect panel regression using data from one-week before to one-week after the closure of the NYSE floor:

$$PESPR_{i,e,t} = FE + \beta_0 \times Treated_{i,e,t}After_{i,e,t} + \beta_2 \times After_{i,e,t} + \beta_1 \times Treated_{i,e,t} + \gamma \times C_{i,e,t} + \epsilon_{i,e,t},$$

where $PESPR_{i,e,t}$ is the effective spread of stock i on day t trading on exchange e; $After_{i,e,t}$ is an indicator variable equal to one after the closure of the NYSE floor (March, 23rd 2020) and zero otherwise; $Treated_{i,e,t}$ is an indicator variable equal to one if the stock is listed on the NYSE (Panel A) or equal to one if $PESPR$ is estimated on the NYSE (Panel B) and zero otherwise; and $C_{i,e,t}$ is a vector of additional control variables. In Panel A *PESPR* is estimated as the trade price across all exchanges in excess of the prevailing midpoint price using NBBO bid and ask prices. In Panel B *PESPR* on the NYSE is estimated from trades on the NYSE and midpoint prices using bid and ask prices on the NYSE. *PESPR* on other exchanges is estimated from trades outside the NYSE and midpoint prices using NBBO excluding NYSE bid and ask prices (i.e., using the best bid and ask prices across all exchanges excluding the NYSE). T-statistics calculated using standard errors clustered by stock are shown below the estimates. Statistical significance at the 1%, 5%, and 10% level is indicated by ***, **, and *, respectively. Data is from CRSP and TAQ for the period March 16, 2020 through March 27, 2020.

	Dependent Variable = $PESPR$			
	(1)	(2)	(3)	(4)
Panel A: NYSE- ($Treated = 1$) versus Nasdaq- ($Treated = 0$) listed stocks				
$treated \times after$	0.05***	0.05***	0.05***	0.06***
	(5.61)	(5.61)	(5.33)	(5.72)
$after$	-0.06***		-0.05***	
	(-6.52)		(-6.26)	
$Price$			-0.00***	0.00***
			(-4.79)	(3.42)
$log(volume)$			0.02*	-0.02*
			(1.86)	(-1.81)
Within R^2 [%]	1.44	0.77	1.82	0.92
#Stocks	618	618	618	618
#Days	10	10	10	10
Stock FE	Yes	Yes	Yes	Yes
Day FE	No	Yes	No	Yes

Table A8 continued

Panel B: NYSE stocks traded on the NYSE ($Treated = 1$) versus off the NYSE ($Treated = 0$)

$treated \times after$	-0.01**	-0.01**	-0.01**	-0.01**
	(-2.31)	(-2.31)	(-2.23)	(-2.35)
$after$	0.00		0.00	
	(0.76)		(0.78)	
$Price$			-0.00***	-0.00
			(-4.29)	(-0.99)
$log(volume)$			-0.02***	-0.03***
			(-5.00)	(-5.84)
Within R^2 [%]	0.15	0.14	5.48	1.76
#Stocks	276	276	276	276
#Days	10	10	10	10
Stock FE	Yes	Yes	Yes	Yes
Day FE	No	Yes	No	Yes
Day × Stock FE	No	No	No	No

Table A9 – Difference-in-differences Regression: PESPR (small stocks)

This table shows results from a difference-in-differences regression of effective spreads ($PESPR$) before versus after the closure of the NYSE floor, comparing NYSE listed stocks to matched Nasdaq stocks (Panel A) and comparing *PESPR* of NYSE listed stocks on the NYSE and off the NYSE (Panel B). The sample consists of only small stocks, defined as stocks with a below median average market capitalisation in the week before the closure of the NYSE floor. We estimate the following fixed-effect panel regression using data from one-week before to one-week after the closure of the NYSE floor:

$$PESPR_{i,e,t} = FE + \beta_0 \times Treated_{i,e,t} After_{i,e,t} + \beta_2 \times After_{i,e,t} + \beta_1 \times Treated_{i,e,t} + \gamma \times C_{i,e,t} + \epsilon_{i,e,t},$$

where $PESPR_{i,e,t}$ is the effective spread of stock i on day t trading on exchange e; $After_{i,e,t}$ is an indicator variable equal to one after the closure of the NYSE floor (March, 23rd 2020) and zero otherwise; $Treated_{i,e,t}$ is an indicator variable equal to one if the stock is listed on the NYSE (Panel A) or equal to one if $PESPR$ is estimated on the NYSE (Panel B) and zero otherwise; and $C_{i,e,t}$ is a vector of additional control variables. In Panel A $PESPR$ is estimated as the trade price across all exchanges in excess of the prevailing midpoint price using NBBO bid and ask prices. In Panel B $PESPR$ on the NYSE is estimated from trades on the NYSE and midpoint prices using bid and ask prices on the NYSE. $PESPR$ on other exchanges is estimated from trades outside the NYSE and midpoint prices using NBBO excluding NYSE bid and ask prices (i.e., using the best bid and ask prices across all exchanges excluding the NYSE). T-statistics calculated using standard errors clustered by stock are shown below the estimates. Statistical significance at the 1%, 5%, and 10% level is indicated by ***, **, and *, respectively. Data is from CRSP and TAQ for the period March 16, 2020 through March 27, 2020.

	Dependent Variable = $PESPR$			
	(1)	(2)	(3)	(4)
Panel A: NYSE- ($Treated = 1$) versus Nasdaq- ($Treated = 0$) listed stocks				
$treated \times after$	0.10***	0.10***	0.10***	0.10***
	(5.03)	(5.04)	(4.81)	(5.17)
$after$	-0.15***		-0.12***	
	(-10.07)		(-8.11)	
$Price$			-0.01***	0.00**
			(-4.45)	(2.13)
$log(volume)$			0.07***	-0.05**
			(2.65)	(-2.47)
Within R^2 [%]	2.09	0.44	2.66	0.63
#Stocks	618	618	618	618
#Days	10	10	10	10
Stock FE	Yes	Yes	Yes	Yes
Day FE	No	Yes	No	Yes

Table A9 continued

Panel B: NYSE stocks traded on the NYSE ($Treated = 1$) versus off the NYSE ($Treated = 0$)

$treated \times after$	0.04***	0.04***	0.04***	0.04***
	(3.70)	(3.70)	(3.68)	(3.78)
$after$	-0.00		-0.01	
	(-0.56)		(-1.23)	
$Price$			-0.01***	-0.00
			(-7.59)	(-0.33)
$log(volume)$			-0.04***	-0.07***
			(-5.05)	(-8.04)
Within R^2 [%]	0.59	0.41	3.56	2.29
#Stocks	276	276	276	276
#Days	10	10	10	10
Stock FE	Yes	Yes	Yes	Yes
Day FE	No	Yes	No	Yes
Day $\times$ Stock FE	No	No	No	No