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Measuring institutional trading costs and the implications for finance research: The case of tick size reductions[☆]

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ABSTRACT

Using proprietary institutional trade data, we construct a price impact measure that represents the costs faced by institutional investors. We show that many widely used liquidity measures do not adequately capture institutional trading costs. We then find that institutional trading costs are not dramatically impacted by decimalization, casting doubt on the widely used identification strategy that employs decimalization as an exogenous shock to liquidity, particularly institutional liquidity. Indeed, we find that conclusions from prior research are significantly altered when we measure liquidity using institutional trading data.

1. Introduction

How should one measure institutional trading costs? This is an important question, as there is a large and growing literature that studies the impact of institutional investors and their trading costs on firm behavior and asset prices.[1] To study the effects of institutional or blockholder liquidity, researchers often employ measures constructed with trade-level data, such as the effective spread, or daily stock price data, such as the Amihud measure.[2] However, we argue that this practice is problematic for a number of reasons, including the fact that these measures typically do not distinguish between institutional and non-institutional trades, nor do they properly capture the common practice where institutions break orders into smaller trades (which can take multiple days to fully execute). Therefore, it is questionable whether commonly used measures of stock liquidity adequately capture institutional trading costs.

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[1] See Holden et al. (2014) for a review of the trading costs literature, including research on institutional trading costs.

[2] Examples include Edmans et al. (2013), Roosenboom et al. (2013), and Norli et al. (2014).

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A setting where the measurement of stock liquidity is particularly important is the case of minimum tick size changes in equity markets. Bessembinder (2003) shows that various measures of bid-ask spreads significantly improve following tick size reductions. As a consequence, researchers have used tick size reductions, particularly decimalization, as an exogenous shock to liquidity to draw causal inference on the effect of liquidity on corporations. Given the endogenous nature of the economic environment where firms operate and researchers' desire to make causal statements, the use of decimalization as an exogenous shock to liquidity has exploded in recent years: Table 1 shows that in the last ten years, 26 published or active working papers have employed tick size reductions as an identification strategy. Further, the majority of these papers use theories on institutional or blockholder liquidity to motivate their studies. However, although spreads tend to improve following decimalization, it is questionable whether institutional investors are better off. Depth, a key factor that affects institutional trading costs, can worsen following tick size reductions (Goldstein and Kavajecz, 2000; Werner et al., 2019). Further, Seppi (1997) analytically argues that institutional investors prefer a larger tick size than retail investors.

Bessembinder (2003) is frequently cited by studies contending that decimalization is an exogenous shock to trading costs. However, he concludes (p. 775) with the statement that while his results "support a firm conclusion that smaller traders who use market orders have benefited," he also notes that his results "do not provide direct evidence on trading costs for large institutional trading programs" and that "a complete assessment of the impact of decimalization on market quality will also require access to proprietary data on institutional trading programs, in order to assess whether trading costs for large institutions have also declined."

Motivated by the conclusions in Bessembinder (2003), we primarily rely on a proprietary database of institutional investors' US equity transactions provided by Abel Noser to perform our analysis. This data set tracks institutional trade orders from the time they are placed until the time they fully execute, which allows us to construct a relatively accurate measure of institutional trading costs. Our data not only allow us to construct a measure of institutional trading costs that captures the bid-ask spread and depth effects but also allow us to capture other important aspects of trading, such as the propensity for information to leak from the time the order is placed until the time it is fully executed, or the practice of breaking up orders into many smaller trades on the same side. Although our data set is fairly novel, we are not the first to use Abel Noser data. Hu et al. (2018) show that over 50 papers have employed Abel Noser data in a variety of settings. Further, prior research suggests that the institutional investors in the Abel Noser data set have an incentive to provide accurate trade data, and they are representative of the institutional investor universe. We expound further on our data in Section 4.

To evaluate how well common liquidity measures reflect institutional liquidity, we compute cross-sectional and time-series correlations between the price impact measure of institutional trading costs and various alternative stock liquidity proxies. We find little evidence that common measures of liquidity adequately capture the costs faced by an institution: firm-level correlations between institutional liquidity and alternative liquidity measures range from -0.013 to 0.068.[3] The low correlations suggest that many widely used measures of stock liquidity do not adequately capture institutional trading costs. This observation highlights the importance of linking theoretical arguments about how liquidity impacts corporations with the microstructure evidence.

We next show that institutional trading costs for a sample of NYSE and Nasdaq stocks are not significantly affected by the switch to decimalization in US equity markets. When we focus on NYSE stocks, we find that although institutional trading costs fall, on average, following decimalization, the drop is insignificant if we control for firm characteristics and patterns in trading behavior. Further, the price impact measure for Nasdaq stocks increases following decimalization. These results lead to questions about papers that assume that tick size reductions generate a significant improvement in institutional liquidity. In particular, the literature examining the effects of liquidity on corporations must consider the type of trader who is proposed as the primary mechanism for the hypotheses and whether tick size reductions actually represent an improvement in liquidity for that trader.

To illustrate our point, we replicate and extend three recent studies that use an exogenous change in the minimum tick size to represent a shift in liquidity: Fang et al. (2009) who argue that increased stock liquidity causes an increase in firm value; Fang et al. (2014) who find that improved stock liquidity reduces firm innovation; and Brogaard et al. (2017) who show that increased liquidity causes lower default risk. All three of these studies cite theories of institutional or blockholder liquidity to motivate their analysis, yet each study measures stock liquidity with effective spread. As previously discussed, spreads do not capture important aspects of trading, particularly for large trades placed by institutional investors.

We first replicate the findings in each study using percent effective spread, and we then extend each study by instead measuring stock liquidity using the institutional price impact. We start with investigating how liquidity affects firm value, as in Fang et al. (2009). Consistent with Fang et al. (2009), we find that change in percent effective spread is significantly related to change in the market-to-book ratio. However, using the price impact measure, we do not find any evidence that stock liquidity has a positive causal effect on market to book.

We next examine how liquidity affects innovation and default risk using both effective spread and price impact to measure liquidity. Consistent with Fang et al. (2014) and Brogaard et al. (2017), we find that an increase in liquidity, measured by percent effective spread, reduces innovation

[3] This finding is consistent with the results presented in Bogousslavsky et al. (2018) and Frazzini et al. (2018). Both of these studies, using different data sources than ours, find that institutional trading costs have low correlations with many of the liquidity measures we examine.

Table 1
Literature review of studies that use tick size reduction as an exogenous shock to stock liquidity.
This table describes empirical research studies that use tick size reductions in US equity market to draw causal inference on the effects of stock liquidity on various outcome variables. This table lists the studies and briefly describes their main results. It also states whether or not the study uses at least one theory of reduced institutional trading costs to motivate its analysis, and if it does, we briefly summarize the theory.

	Study	Inference drawn from use of tick size reduction	Does study use at least one theory of improved institutional liquidity to motivate its analysis, and what is the theory?
1	Chordia et al. (2008)	Reduced spreads improve market efficiency.	No.
2	Fang et al. (2009)	Lower spreads increase firm value.	Yes. Reduced trading costs facilitate blockholder formation and monitoring as well as threat of exit. These channels improve efficacy of corporate goverance.
3	Han and Lesmond (2011)	Improved liquidity reduces pricing ability of idiosyncratic volatility.	No.
4	Bharath et al. (2013)	Increased liquidity improves firm value via enhanced efficacy of corporate governance.	Yes. Reduced trading costs enables threat of exit by blockholders.
5	Bodnaruk and Östberg (2013)	Increased shareholder base increases firm payouts.	No.
6	Edmans et al. (2013)	Increased liquidity improves efficacy of corporate governance.	Yes. Reduced trading costs facilitate blockholder formation and monitoring as well as threat of exit.
7	Banerjee et al. (2014)	Increased liquidity cost positively impacts stock return kurtosis.	No.
8	Fang et al. (2014)	Reduced spreads impede innovation.	Yes. Selling pressure by nondedicated institutional investors encourages manager myopia.
9	Gerken (2014)	Liquidity positively impacts efficacy of corporate governance.	Yes. Reduced trading costs facilitate block formation and monitoring as well as threat of exit.
10	Norli et al. (2014)	Liquidity positively impacts shareholder activism.	Yes. Reduced trading costs facilitate blockholder formation and monitoring.
11	Chen et al. (2015)	Increased liquidity reduces earnings management.	Yes. Reduced trading costs facilitate blockholder formation and monitoring as well as threat of exit. These channels improve efficacy of corporate goverance.
12	Cheung et al. (2015)	Higher REIT stock liquidity increases firm value.	Yes. Reduced trading costs facilitate blockholder formation and monitoring as well as threat of exit. These channels improve efficacy of corporate goverance.
13	Francis et al. (2016)	Higher liquidity leads to lower loan spreads.	Yes. Reduced trading costs facilitate blockholder formation and monitoring as well as threat of exit. These channels improve efficacy of corporate goverance.
14	Massa and Yadav (2016)	Superior performance of variable annuity funds is not explained by liquidity of their portfolios.	Yes. Improved fund liquidity could explain fund performance.
15	Brogaard et al. (2017)	Lower spreads reduce default risk.	Yes. Reduced trading costs enables threat of exit by blockholders, which improves efficacy of corporate goverance.
16	Chang et al. (2017)	Increased liquidity increases stock price crash risk.	Yes. Reduction in trading costs facilitates the exit of transient institutional investors, causing manager myopia.
17	Dass et al. (2017)	Increased liquidity improves bidders' acquisition outcomes.	No.
18	Kang and Kim (2017)	Liquidity increases probability that CEO turnover is related to short-term performance when level of transient investors is high.	Yes. Reduction in trading costs facilitates the exit of transient institutional investors, causing manager myopia.
19	Amihud and Levi (2018)	Higher liquidity induces increased corporate investment.	No.
20	Chang et al. (2018)	Liquidity negatively impacts a firm's corporate social responsibility rating.	Yes. Reduction in trading costs facilitates the exit of transient institutional investors, causing manager myopia.
21	Cheng et al. (2018)	Improved liquidity reduces earnings-return asymmetry.	Yes. Reduced trading costs facilitates blockholder formation, which enables short selling and timely news incorporation.
22	Dou et al. (2018)	Liquidity via the threat of exit improves financial reporting quality.	Yes. Reduced trading costs enables threat of exit by blockholders, which improves efficacy of corporate goverance.
23	Nyborg and Wang (2018)	Firms increase cash holdings as liquidity improves.	No.

(*continued on next page*)

Table 1 (*continued*)

	Study	Inference drawn from use of tick size reduction	Does study use at least one theory of improved institutional liquidity to motivate its analysis, and what is the theory?
24	Brogaard et al. (2019)	Liquidity impacts bank syndicate lending.	Yes. Reduced trading costs enables threat of exit by blockholders, which improves efficacy of corporate goverance.
25	Chen et al. (2019)	Increased liquidity mitigates tax avoidance.	Yes. Reduced trading costs facilitate blockholder formation and monitoring as well as threat of exit. These channels improve efficacy of corporate goverance.
26	Cheung et al. (2019)	Stock liquidity increases debt financing and reduces cost of debt.	No.

and default risk, respectively. These results are particularly strong for Nasdaq firms. However, using the price impact measure, we find no evidence that liquidity affects either firm innovation or default risk.

Our study is in the same vein as Karpoff and Wittry (2018), who also question the validity of a widely used identification strategy. Specifically, Karpoff and Wittry (2018) examine whether it is appropriate to use state antitakeover laws to exploit exogenous variation in firm governance. They revisit nine prior studies that employ state antitakeover laws as exogenous shocks to corporate governance and find that the results become insignificant once they include controls related to legal context. These findings cast doubts on the use of state antitakeover laws as an identification strategy.

Our paper is also related to concurrent research by Bogousslavsky et al. (2018) and Frazzini et al. (2018) who find that institutional trading costs are not significantly correlated with several of the liquidity measures we examine. The former paper arrives at this finding by using large-order data from an investment bank to examine the period surrounding a major trade glitch in 2012. They primarily focus on effects of the glitch, but they report that institutional liquidity is weakly correlated with standard measures of liquidity, suggesting they measure distinct aspects of trading costs. However, given their limited sample period, the long-period results we report not only enhance our ability to draw general conclusions about the relation between institutional liquidity and common alternative measures but also enables us to investigate the effect of decimalization on institutional liquidity. Frazzini et al. (2018) compute institutional trading costs using data from a single institutional investor. While their work is a thorough examination of the price impact costs for this institution, their results should be interpreted with care. Anand et al. (2012) examine the price impact for the institutions in Abel Noser at a monthly frequency, and Sağlam et al. (2019), using a different trading sample, examine institutional trading costs within a single day. Both studies report considerable cross-sectional heterogeneity in institutional trading costs. These results suggest caution in assessing the representativeness of any single institution.

Other research on tick size changes in equity markets includes Jones and Lipson (2001), who examine the impact of the 1997 switch to sixteenths and find that institutional trading costs rise, on average, following the tick size reduction, particularly for large executions. In contrast to our results and those in Jones and Lipson (2001), Chakravarty et al. (2005) find that institutional trading costs fall following decimalization. However, they examine only 34 large institutions that trade almost exclusively in large, high-volume stocks. Additionally, they do not examine Nasdaq stocks. Thus, their analysis leads to conclusions that are not representative for the majority of institutional traders.

The rest of the paper is organized as follows. In Section 2, we discuss the implications of our study for current research on the effects of liquidity on corporate actions. Section 3 reviews background information and existing literature on minimum tick size reductions. In Section 4, we describe the sample and key variables. We examine how the price impact measure constructed using institutional order-level data compares to alternative liquidity measures in Section 5. In Section 6, we investigate how decimalization impacts institutional trading costs. In Section 7, we consider the implications of using decimalization as an identification strategy by revisiting prior research. We conclude in Section 8.

2. The problem

The primary contention in this paper is that institutional liquidity is best measured using the price impact of an order since this measure captures the effective trading cost of executing a large trade better than measures related to the inside spread or single executions. From this perspective, we examine the theoretical underpinning of recent research examining the effects of institutional liquidity on firm behavior or asset prices.

This perspective will naturally be the most important for studies that rely on theory suggesting that blockholders or institutional investors are the key investors driving the changes in corporate activity or stock price formation. Institutions are large organizations that trade significant volume; the average trade size for the institutional trades in our database is approximately 15,000 shares. The size of these trades presents a challenge to institutional trading desks. Of course, they could execute the trade against the outstanding limit order book, as retail traders do, but if the depth offered at the inside quotes is insufficient to fill the order, then the cost of the trade depends on the depth and price discounts deeper in the limit order book.

Concerned about these costs, it is common for institutions to break up their trade into smaller pieces and execute these trades sequentially. The risk with this strategy is that the price could drift in the direction of the trade before the order is complete, increasing the effective cost of filling the order. This price drift is exacerbated when high-frequency traders or other market participants actively try to determine the direction of the institutional trade and "backrun" the order (Yang and Zhu, 2020). Under these conditions, inside spread measures of liquidity, such as percentage effective spread that only measures the immediate liquidity for relatively small trades, do not align with the trading costs of most institutional investors.

Other measures of liquidity, such as the Amihud (2002) measure, are designed to capture the price impact costs of larger trades. However, it is not clear that the Amihud measure, which is constructed with daily stock data, is a reliable proxy for institutional trading costs. The Amihud measure does not distinguish between institutional and non-institutional trades, nor does it properly capture the common practice where institutions break orders into smaller trades, which can take multiple days to fully execute. To summarize, if existing measures of liquidity are not highly correlated with the price impact of large institutional trades, the contentions that these measures represent institutional trading costs or that tick size reductions facilitate a significant exogenous shock to institutional liquidity are not tenable.

For example, a large literature examines how liquidity affects corporate governance and argues that improved liquidity impacts blockholder behavior (Edmans et al., 2013; Bharath et al., 2013; Norli et al., 2014). If trading costs for blockholders are uncorrelated with the measures used in these studies, or if blockholder trading costs are unaffected by tick size reductions, then the conclusion that liquidity causes the documented governance changes is called into question.

Other studies apply theories of liquidity and corporate governance in alternative settings. For example, Brogaard et al. (2017) argue that increased liquidity reduces default risk. In their paper, one of the drivers of this effect is blockholder governance. Fang et al. (2009) find evidence that liquidity positively impacts firm performance, and one explanation for their result is that increased liquidity leads to enhance institutional monitoring activity. Again, the same arguments apply, if the costs of trading large orders do not significantly change around a tick size event, then the evidence presented on changes in default risk and firm performance, respectively, should not be interpreted as liquidity effects.

Fang et al. (2014) argue that improved liquidity reduces innovation by both increasing the risk of hostile takeovers and increasing the ease of exit by institutional investors dissatisfied by poor short-term performance. The importance of the former reason is debatable due to the greatly decreased frequency of hostile takeovers since the late 1980s. More relevant to our study, if the cost of trading a large block of stock did not, in fact, decrease at the time of the tick size reduction, the cost of exit by institutional investors does not materially change. Therefore the conclusion that reduced liquidity causes firms to reduce innovation is questionable.

Applying a similar argument, Chang et al. (2017) argue that improved liquidity increases stock crash risk. Their paper suggests that management is loath to disappoint transient institutional investors, so firms hoard bad news and are eventually forced to dump all of the bad news in a single quarterly report. Improved liquidity enhances crash risk because it allows transient investors to more easily unwind their position upon the arrival of the bad corporate news. The price impact of forming and unwinding large positions clearly matters in this setting.

Another strand of literature argues that improved liquidity makes it less costly for informed investors to trade, which increases price efficiency (Chordia et al., 2008). Brogaard et al. (2019) borrow this theory and apply it to explain their results that liquidity impacts bank syndicate lending; Fang et al. (2009), and Brogaard et al. (2017) also include this theory in their motivation. This theory does not explicitly specify whether the informed trader is a retail or institutional investor; therefore the degree to which price impact proxies for liquidity in these settings depends on beliefs about the likely identity of the informed traders.

Finally, some studies use decimalization to identify the causal impact of liquidity but rely on arguments that are independent of the question of whether the price impact of large trades actually changes surrounding a tick size reduction. For example, Han and Lesmond (2011) contend that improved liquidity significantly reduces the ability of idiosyncratic volatility to price stocks in the cross-section. Their study explicitly relies on the bid-ask bounce and frequency of zero returns to motivate and explain their results. Other studies that rely on decimalization to draw causal inference but do not rely on theories of institutional liquidity include Bodnaruk and Östberg (2013), who examine the impact of shareholder base on payout policy, and Amihud and Levi (2018), who study the effects of liquidity on corporate investment. See Table 1 for a more thorough list of studies that use tick size reductions to identify the causal effect of liquidity for various outcome variables.

In summary, most of the arguments for the effects of liquidity on corporate activity and asset prices suggest that the mechanism driving the result is the changing behavior of large traders, be they blockholders or institutional traders. Many of these papers use mandated minimum tick size variation to provide the exogenous liquidity shock necessary to validate their results. Since we proceed to show that many of the liquidity measures used in these papers are unrelated to institutional liquidity, and that trading costs for large traders do not fall surrounding the exogenous tick size reduction, our paper challenges this literature to examine the theoretical foundations of their work. Going forward, research on liquidity and corporate activity should be cautious to presume that spreads or existing measures of price impact, such as the Amihud measure, accurately measure institutional trading costs or that the trading costs of large investors are materially affected by exogenous tick size reductions.

3. The existing evidence on tick size reductions

Major exchanges in US equity markets reduced their minimum price increment from eighths to sixteenths in

1997 and further reduced tick size to decimals in 2001. Theory suggests that reduced tick size leads to lower bid-ask spreads for stocks that were constrained by the previous minimum price increment. However, improvements in spreads may be more than offset by reductions in depth. Liquidity providers have less incentive to supply quotes following tick size reductions because lower spreads reduce profits and because it is less costly for market participants to step ahead of existing quotes (see Harris, 1997 for a discussion on the issues associated with tick size reduction and market quality).

Empirical studies of tick size effects often proxy for stock liquidity using various bid-ask spread measures. The evidence suggests that both quoted spreads and effective spreads fall following tick size reductions. For example, spreads fell, on average, following the 1997 tick size reduction to sixteenths and the 2001 switch to decimalization (see Bollen and Whaley, 1998 and Van Ness et al., 2000 for an analysis of the move to sixteenths and Bessembinder, 2003 for an analysis of decimalization). These findings suggest that liquidity for small orders improves following tick size reductions.

The evidence described in the preceding paragraph relies on trade and quote data obtained from the NYSE Trade and Quote (TAQ) database to measure stock liquidity, but these data have several limitations. For instance, it is difficult to accurately measure depth, an important determinant of trading costs, using TAQ data. Researchers can use TAQ data to construct quoted depth, defined as the number of shares available at the best bid and ask quotes, but this measure is incomplete since it does not capture the number of shares available outside of the best bid and offer. Cumulative depth, the quantity of shares available throughout the order book, is the preferred measure but requires limit order data, which are not readily available. There is, however, some empirical research on the effects of tick size reduction on cumulative depth. Using proprietary limit order data provided by the NYSE, Goldstein and Kavajecz (2000) find that depth falls throughout the limit order book following the switch to sixteenths in US equity markets. This finding suggests that the cost of executing a large order may not change in the same way as the cost of executing a small order following a minimum tick size reduction.

In addition to issues with measuring depth, stock liquidity measures constructed using trade and quote level data imperfectly measure execution costs for large orders. For instance, orders placed by investors are routinely broken into a number of smaller trades to minimize the price impact of a single large trade. During the execution of these orders, information regarding the order could leak to other market participants between the time the investor places the order and the time that it is fully executed (Korajczyk and Murphy, 2018 and Yang and Zhu, 2020 provide contemporary examples). Stock liquidity measures constructed using trade and quote level data do not capture these important issues related to price impact.

In contrast, order-level data allow researchers to more accurately measure the price impact of a trade because execution costs can be measured from the time the order is placed until the time the order is fully executed. Given this divergence, using order-level data instead of TAQ data can alter conclusions drawn from research regarding the costs of executing large orders. For example, Jones and Lipson (2001) use proprietary institutional order data from the Plexus group and find that execution costs rise following the implementation of sixteenths on the NYSE. This evidence, taken together with the lower order book depth reported in Goldstein and Kavajecz (2000), questions the idea that stock liquidity significantly improves for institutional traders following minimum tick size reductions.

However, there is conflicting evidence on the effects of tick size reductions on institutional liquidity. In their appendix, Frazzini et al. (2018) regress their institutional trading cost measure on a decimalization dummy variable that is equal to one if the time period is before decimalization and is equal to zero if after. Their results suggest that trading costs are lower following decimalization (which is in contrast to the findings for the switch to sixteenths reported by Jones and Lipson, 2001). However, Frazzini et al. (2018) employ a long sample period, and it is difficult to determine whether lower institutional trading costs in recent years are due to decimalization.[4]

Chakravarty et al. (2005), using a sample from the same data source as Jones and Lipson (2001), also study the effects of decimalization on institutional trading, and they report that institutional trading costs for NYSE stocks fall, on average, following the tick size regime change. However, Chakravarty et al. (2005) examine a sample of only 34 large institutions that trade large quantities, predominately in high-volume stocks. Their sample is much less representative than the broader sample employed in our work and in Jones and Lipson (2001). These sample differences are not innocuous, as the evidence presented in Goldstein and Kavajecz (2000) suggests that the sample of large, actively traded stocks employed in Chakravarty et al. (2005) might lead to inaccurate inference because active stocks are the ones that benefit the most from decimalization. Indeed, we find in unreported results that any evidence of decreased institutional trading costs is concentrated in the largest NYSE stocks in our sample.[5] Thus, Chakravarty et al. (2005)'s limited institutional sample, along with their inability to examine the impact of tick size reductions on Nasdaq stocks, account for their different conclusions.

Finally, regulators have expressed a particular concern about whether small minimum tick sizes are optimal for small-capitalization stocks. In 2012, the US Congress passed the Jumpstart Our Business Startups Act (JOBS Act), which directed the Securities and Exchange Commission (SEC) to conduct a study and report to Congress on how tick size reduction in equity markets impact the market quality of small-capitalization stocks. Following this directive, the SEC issued an order to the major equity exchanges to implement a tick size pilot program to ascertain the effects of an increase in minimum price increment on market quality. The program, which recently ended, increased

[4] Fig. 8 in Frazzini et al. (2018) exhibits time-series plots of institutional trading costs that suggest costs do not dramatically fall in the months or even years following decimalization.

[5] These results are available upon request.

tick size from $0.01 to $0.05 for a group of small stocks, defined as market capitalization of less than $3 billion. Chung et al. (2020) examine the effects of the Tick Size Pilot Program. They find that the increase in the minimum price increment leads to lower expected execution costs for large orders. This result is consistent with those we obtain with our sample of Nasdaq stocks, which contains a number of small stocks with an average market capitalization of $1.3 billion.

4. Data sources and variable construction

We construct alternative stock liquidity measures at the stock-month level using a sample of NYSE- and Nasdaq-listed stocks. We initially consider a sample period covering 1999 through the third quarter of 2011.[6] We subsequently focus our analysis to the period surrounding the 2001 switch to decimalization in US.[7] We retain stock-months only if they have executions on both the Abel Noser and TAQ databases.

We construct institutional price impact using order-level data provided by Abel Noser, which is a consulting firm that provides transaction cost analysis services to institutional clients. Two main types of institutional investors provide trading data to Abel Noser: investment managers and retirement fund sponsors. The former class are composed of money managers, such as Massachusetts Financial Services (MFS), Putnam Investments, Lazard Asset Management, and Fidelity. The latter include pension plan sponsors, such as California Public Employees Retirement System (CalPERS), the Commonwealth of Virginia, and the Young Men's Christian Association (YMCA) retirement fund. Previous studies report that Abel Noser data cover a significant proportion (8%—15%) of all Center for Research in Security Prices (CRSP) trading volume (Puckett and Yan, 2011; Hu et al., 2018).

An observation in the Abel Noser database corresponds to a particular client's trade execution. For each execution, the data include fields capturing the symbol and CUSIP associated with the traded stock, whether the trade is a buy or sell, the execution price, the number of shares traded, and a trade order identifier. The data also include a set of variables providing benchmark information (price and volume) at various times and for various time horizons. This rich data set of institutional trades allows us to create an illiquidity measure that captures institutional trading activities. Academic studies using Abel Noser data include Goldstein et al. (2009), Chemmanur et al. (2009), Goldstein et al. (2011), and Puckett and Yan (2011).

Since we rely on the Abel Noser database, it is important to consider potential sampling issues. First, while it is possible that the institutional investors provide inaccurate or incomplete data to Abel Noser, it is unlikely to be a pervasive issue. The institutions have an incentive to send accurate trading data so that Abel Noser has the opportunity to give appropriate advice. Second, it is possible that the Abel Noser data are subject to sample selection biases. However, prior research shows that Abel Noser institutions are generally comparable to institutions from more comprehensive data sources, such as the 13F filings (Puckett and Yan, 2011; Anand et al., 2012), which reduces concerns about a biased sample.

However, one dimension in which the Abel Noser institutions differ is size; they are a little larger, on average, than the institutions from the 13F data. This characteristic is potentially problematic, as larger institutions may submit larger trades, which typically generate higher trading costs. To address this concern, we perform robustness tests where we drop large orders from our sample (such as orders greater than 100,000 shares), and we obtain qualitatively similar results. Finally, Abel Noser institutions tend to trade in stocks that are slightly larger than average, which can lead to potential inference issues.[8] However, we confirm that our main results continue to obtain if we focus on the smallest stocks, defined as stocks below the 20th percentile of equity market value (using NYSE breakpoints), in our sample.

To measure institutional trading costs, we construct a price impact measure, which is defined as

$$\textit{Price impact}_{i,j} = D_{i,j}\,\frac{P^1_{i,j} - P^0_{i,j}}{P^0_{i,j}}, \tag{1}$$

where $P^1_{i,j}$ measures the volume-weighted execution price of order j for stock i, $P^0_{i,j}$ is the price prevailing at the time the broker receives the order, and $D_{i,j}$ is a variable that equals 1 for a buy order and -1 for a sell order.[9] We construct each stock's *Price impact* as the dollar volume-weighted average *Price impact* across orders. *Price impact* captures several important dimensions of institutional trading costs, including the bid-ask spread, the effects of trade volume on price, slippage costs due to delayed executions, and order-splitting effects.

The measure of *Price impact* we use is similar to Perold (1988)'s implementation shortfall. One difference is that the implementation shortfall includes the opportunity cost associated with unfilled trades, which we do not observe. However, Keim and Madhavan (1997), using Plexus data with information on fill rates, conclude that 95% of an order is filled on average.[10] Implementation shortfall also uses the price at the time of trade origination as the benchmark price. We do not observe this price, but for robustness, we do consider alternative benchmark prices, such as the closing price from the

[6] Although we have data through June of 2014, client identification is masked after the third quarter of 2011. Since this time period is far from the relevant decimalization date, we end our analysis in 2011.

[7] The NYSE fully converted to decimal pricing on January 29, 2001, and the Nasdaq followed suit on April 9, 2001. In the months leading up to decimalization, the NYSE and Nasdaq implemented pilot programs in which a select group of stocks converted to decimal pricing prior to the rest of the listed stocks. We hand collect pilot stocks and the dates they converted to decimal pricing, and we include these stocks in our analysis.

[8] In unreported tests, we find that average equity market values for 13F stocks surrounding decimalization are about $5 billion for NYSE stocks (compared to about $7.4 billion in our sample; see Table 4) and about $1 billion for Nasdaq stocks (compared to $1.3 billion for our sample).

[9] We allow orders to span up to five days. Results are qualitatively similar if we do not restrict order time or if we cap order time at one day.

[10] Conversations with Abel Noser confirmed their data have similar properties.

previous day and the market open price, and our main findings remain unchanged. Other studies that employ similar measures of trading costs include Jones and Lipson (2001), Chakravarty et al. (2005), Anand et al. (2013), and Frazzini et al. (2018).

In addition to trading costs, we compute the number of institutional orders placed for each stock each day and the average institutional order size for each stock-day. To minimize observations with errors, we follow Anand et al. (2013) and delete orders in which *Price impact* exceeds an absolute value of 10%. However, we retain our main conclusions if we do not apply this filter.

In addition to *Price impact*, we construct a number of additional measures of stock liquidity using either intra-day trade and quote data from the TAQ database or daily stock data from the CRSP database. We next list and briefly describe these measures (the Appendix provides additional discussion).

Quoted spread: A stock's *Dollar quoted spread* is computed with TAQ data and is the difference between the best bid and best ask price. *Percent quoted spread* scales this spread by the bid-ask midpoint. We initially construct the quoted spread measures at the stock-day frequency by computing a weighted average, where the weight is the amount of time that the quote is in force.

Effective spread: *Dollar effective spread* is the absolute value of the difference between the trade price and the prevailing bid-ask midpoint, multiplied by two to convey the cost for a round-trip trade. *Percent effective spread* scales this spread by the prevailing bid-ask midpoint. These measures are also constructed with intra-day (TAQ) data, and we construct each firm's daily effective spread as the dollar volume-weighted average spread across trades.

Amihud measure: Amihud (2002) proposes a measure of liquidity based on the ratio of the daily absolute return to daily trading volume. The measure is computed as the average of this ratio over a certain time interval (e.g., month), including only days with positive trading volume. The measure is designed to capture the magnitude of daily price response per dollar of trading volume.

Roll measure: The Roll (1984) liquidity measure is an estimator of the percent effective spread based on the serial covariance of successive daily price movements.

Corwin Schultz measure: The Corwin and Schultz (2012) liquidity measure uses high and low daily price quotes to approximate the percent effective spread. The key idea behind the proxy is that, although the high-low ratio reflects both variance and the spread, the variance component is proportional to the return interval, while the spread component is not.

FHT measure: The Fong et al. (2017) percent effective spread proxy incorporates two key elements of transaction costs: return volatility and the proportion of zero returns. The measure represents a simplification of a more general variation proposed by Lesmond et al. (1999).

Closing spread measure: This measure is a proxy for bid-ask spread, constructed with daily closing bid and ask quotes from CRSP.

Effective tick measure: This spread proxy, proposed by Holden (2009) and Goyenko et al. (2009), is based on observable price clustering.

Zeros measure: This liquidity measure, proposed by Lesmond et al. (1999), is based on the proportion of trading days with zero returns.

We obtain closing stock price and market value of equity, defined as shares outstanding multiplied by closing stock price, from CRSP. Consistent with prior studies, we drop observations where the stock price at the beginning of the period is below \$2 or above \$1000 (see Amihud, 2002 and Pástor and Stambaugh, 2003 for examples of other studies that apply a minimum or maximum price filter). The minimum price filter is consistent with the recent SEC Tick Size Pilot Program, which excludes stocks priced below \$2.

5. How does price impact relate to alternative stock liquidity measures?

Many widely used measures of stock liquidity do not adequately capture institutional trading costs. To examine this contention, we conduct a correlation analysis where we compare our *Price impact* measure to alternative liquidity proxies using a panel of firm-month observations from the beginning of 1999 through quarter three of 2011.[11] In addition to *Price impact*, we include high-frequency measures, such as effective spreads, and low-frequency proxies, described in Section 4, in our correlation analysis. For measures that require intra-day data, we first find the volume-weighted average within each day, then compute the simple average across days (within each month).[12] To reduce the impact of outliers, we add one and compute the natural log for each monthly measure. We present summary statistics of the various stock liquidity measures in Table 2. Consistent with the findings in Frazzini et al. (2018), we find that institutional trading costs are smaller than spread estimates. Frazzini et al. (2018) suggest that this occurs because institutional traders often employ patient trading strategies that supply, rather than demand, liquidity.

The correlation analysis is presented in Table 3. Panel A presents cross-sectional correlations between the various stock liquidity measures. We compute correlations in the spirit of Fama and MacBeth (1973): Pearson correlations are computed across firms each month and are then averaged across months (similar to the approach taken in Goyenko et al., 2009).[13] Panel B presents firm-level

[11] We obtain generally similar results if we construct the liquidity measures at an annual, rather than monthly, frequency.

[12] For robustness, we consider an alternative aggregation approach where we compute volume-weighted averages within each firm-month. This method leads to similar results.

[13] In untabulated results, we also compute Spearman correlations and obtain qualitatively similar conclusions.

Table 2
Descriptive statistics for stock liquidity measures.
This table presents summary statistics for monthly stock liquidity measures for the sample period 1999Q1—2011Q3. All measures are log transformed after adding one. See Section 4 and the Appendix for details on variable construction.

	N	Mean	S. D.	P5	P25	P50	P75	P95
Price impact (%)	534,783	0.224	1.058	−1.054	−0.059	0.171	0.501	1.656
Amihud	534,698	0.131	0.415	0.000	0.001	0.008	0.055	0.725
Closing spread (%)	531,027	0.917	1.433	0.048	0.136	0.382	1.126	3.492
Corwin Schultz (%)	534,693	1.162	0.819	0.321	0.613	0.949	1.473	2.719
Effective tick (%)	534,783	0.373	0.633	0.024	0.062	0.156	0.407	1.466
FHT (%)	534,514	0.300	0.646	0.000	0.000	0.000	0.366	1.401
Roll (%)	534,514	1.600	2.077	0.000	0.000	1.030	2.414	5.538
Zeros	534,783	0.036	0.058	0.000	0.000	0.000	0.049	0.154
Percent effective spread (%)	534,783	0.703	1.327	0.054	0.135	0.334	0.856	2.497

Table 3
Correlations between monthly stock liquidity measures.
This table presents correlations for alternative monthly stock liquidity measures. Panel A presents cross-sectional correlations, where Pearson correlations are computed across firms each month and are then averaged across months. Panel B presents firm-level time-series correlations, where correlations are computed over time (months) for each firm and are then averaged across firms. Panel C presents market-wide time-series correlations, where we compute a market-wide variable for each measure by finding the simple average across firms each month, and then we compute the time-series correlations between the monthly market-wide measures. See Section 4 and the Appendix for details on variable construction.

	PI	*Ami*	*CSprd*	*CS*	*ETick*	*FHT*	*Roll*	*Zeros*	*ESprd*
Panel A: Firm-level cross-sectional correlations									
Price impact	1								
Amihud	0.014	1							
Closing spread	0.016	0.744	1						
Corwin Schultz	0.040	0.224	0.343	1					
Effective tick	0.005	0.381	0.445	0.339	1				
FHT	0.005	0.322	0.381	0.264	0.403	1			
Roll	0.017	0.242	0.257	0.436	0.194	0.216	1		
Zeros	−0.013	0.298	0.326	0.055	0.338	0.733	0.041	1	
Percent effective spread	0.020	0.749	0.817	0.414	0.477	0.394	0.286	0.320	1
Panel B: Firm-level time-series correlations									
Price impact	1								
Amihud	0.060	1							
Closing spread	0.062	0.607	1						
Corwin Schultz	0.058	0.307	0.414	1					
Effective tick	0.039	0.389	0.480	0.248	1				
FHT	0.020	0.192	0.294	0.139	0.330	1			
Roll	0.023	0.228	0.215	0.414	0.127	0.117	1		
Zeros	−0.008	0.080	0.149	−0.059	0.242	0.789	−0.043	1	
Percent effective spread	0.068	0.683	0.801	0.473	0.508	0.302	0.235	0.135	1
Panel C: Market-wide time-series correlations									
Price impact	1								
Amihud	0.525	1							
Closing spread	0.635	0.475	1						
Corwin Schultz	0.651	0.823	0.654	1					
Effective tick	0.566	0.298	0.965	0.516	1				
FHT	0.553	0.251	0.932	0.480	0.984	1			
Roll	0.578	0.769	0.514	0.896	0.383	0.354	1		
Zeros	0.432	0.074	0.844	0.252	0.934	0.960	0.153	1	
Percent effective spread	0.678	0.631	0.966	0.779	0.906	0.864	0.637	0.740	1

time-series correlations, where correlations between liquidity measures are computed over time (months) for each firm and are then averaged across firms. Panel C presents market-wide time-series correlations, where we compute a market-wide variable for each measure by finding the simple average across firms each month, and then we compute the time-series correlations between the market-wide measures.

Panel A of Table 3 reveals that every alternative measure of stock liquidity has a low cross-sectional correlation with *Price impact*, with correlations ranging from -0.013 to 0.040. Not only do the alternative measures of spreads have a low correlation with the institutional trading cost measure but so does the *Amihud* measure, which is designed to proxy for the price impact of a trade. Other than *Price impact*, the liquidity measures are much more highly correlated with each other. For example, *Percent effective spread*'s cross-sectional correlation with every liquidity measures besides *Price impact* ranges from 0.286 to 0.817. The firm-level time-series correlations in Panel B of Table 3 convey similar results. These results suggest that, at the stock level, common measures of stock liquidity do not adequately capture the costs of institutional trading. These results are consistent with those in Bogousslavsky et al. (2018) and Frazzini et al. (2018).

The time-series correlations for market-wide (or aggregate) liquidity measures in Panel C of Table 3 tell a different story. Not only are correlations between the commonly used measures of liquidity generally stronger (consistent with Goyenko et al., 2009), but correlations between *Price impact* and the alternative measures are also markedly higher, ranging from 0.432 to 0.678 (consistent with the findings in Frazzini et al., 2018). This result suggests that all of the measures of liquidity contain a common component (Chordia et al., 2000). For example, during the 2008 financial crisis, all measures of market-wide trading costs spiked.

We conduct two additional robustness tests for correlation. First, to alleviate any concern that the low correlation between *Price impact* and alternative liquidity measures is due to noisy single-stock estimates, we estimate correlations for liquidity measures that are computed using portfolio averages. Second, we conduct subsample correlation analyses for the pre- and post-decimalization periods. We consistently find low correlations between *Price impact* and alternative liquidity measures in both of these tests. We report these results in the Internet Appendix, Tables IA1 and IA2.

In summary, although the positive time-series correlations between the aggregate liquidity measures suggest that there is a common market component of all trading costs, the low firm-level correlations reflect the fact that liquidity measures constructed with trade and quote data or daily stock data do not adequately capture the costs faced by institutional investors. These differences have important implications because they question whether common proxies for liquidity accurately measure institutional liquidity, particularly in the cross-section. To explore these implications further, we next consider the importance of accurately measuring stock liquidity in a specific setting: minimum tick size reductions in equity markets.

6. The effects of decimalization on stock liquidity

This section provides empirical evidence on the impact of decimalization in US equity markets on stock liquidity.

6.1. Summary statistics

Table 4 provides descriptive statistics, measured over 30 days before and 30 days after decimalization. Each variable is first constructed at the firm-day level and are then averaged across firms within each time period. Trading volume and share turnover is unchanged or, in some cases, decreases following decimalization. This evidence does not support the idea that stock liquidity increased following decimalization since increased trading activity is typically indicative of a more liquid market. Consistent with previous research on the effects of tick size reduction (Jones and Lipson, 2001; Chakravarty et al., 2005), institutional order size falls following decimalization, particularly for NYSE stocks.

Table 4 also reports that NYSE-listed stocks in our sample are considerably larger, have a higher average stock price, and have higher trading volume than Nasdaq-listed stocks. Given the differences across exchanges, we separately analyze NYSE and Nasdaq stocks in our subsequent analysis.

6.2. Univariate analysis

This section provides univariate evidence on the impact of decimalization on trading costs. We present results for effective spreads, quoted spreads, and *Price impact*. For brevity, we do not present results for low-frequency measures of liquidity, which generally give qualitatively similar results to the spread measures. Table 5 presents the mean change in estimated trading costs from 30 days before to 30 days after the implementation of decimalization.[14] Each of the trading cost measures is initially a weighted average within each firm-day. We then average the measures within each firm-month (30 days). Table 5 reports equally weighted averages across firms.

Consistent with prior literature, we find that both quoted spreads and effective spreads significantly drop following decimalization, regardless of exchange listing. However, the *Price impact* measure gives much different results. The full sample tests (Panel A of Table 5) suggest that *Price impact* does not fall, and if anything rises, following decimalization. In the NYSE subsample (Panel B of Table 5), average *Price impact* declines, but the decline is small, the average reduction is only 5.1 basis points, and this number is statistically insignificant at the 5% significance level. Further, Panel C of Table 5 reports that *Price impact* increases after the minimum tick size reduction (mean change of 0.145 and t-statistic of 2.85) for Nasdaq-listed stocks. This result is consistent with those in Chung et al. (2020), who analyze the recent SEC Tick Size Pilot Program. The authors find that the pilot program, which increases the minimum price increment from $0.01 to $0.05 for a group of randomly selected small stocks (defined as market capitalization below $3 billion) leads to lower expected execution costs for large orders.

In summary, results reported in Table 5 support our earlier argument that widely used measures of liquidity may not be appropriate proxies for blockholders' and institutional investors' trading costs. The results for *Price impact*, a more appropriate liquidity measure for institutional investors, show that blockholders' and institutional investors' trading costs do not receive a significant exogenous improvement following decimalization; in fact these costs slightly increase.

[14] Our main conclusions continue to obtain for this and subsequent analysis if we measure liquidity at an annual, rather than monthly, frequency. We prefer to perform our analyses at a monthly frequency because using longer windows increases the likelihood that stock liquidity is impacted by factors other than decimalization.

Table 4
Firm characteristics and trading activity surrounding decimalization.
This table presents summary statistics for the full sample (Panel A), a subsample of NYSE-listed stocks (Panel B), and a subsample of Nasdaq-listed stocks (Panel C). The pre-dec. and post-dec. windows consist of the 30 days preceding and 30 days following decimalization implementation, respectively. Each variable is initially constructed at the firm-day frequency. Mean values are equally weighted averages across firms, within time group. See Section 4 for details on variable construction.

	N	Mean pre-dec.	Mean post-dec.	Mean change	*t*-stat
Panel A: Full sample					
Stock price	3,395	21.46	22.29	0.83	1.42
Market equity value (millions)	3,395	3,840.11	3,912.18	72.06	0.16
Daily dollar volume (millions)	3,395	29.25	26.89	−2.36	−0.82
Daily turnover (%)	3,395	0.80	0.76	−0.03	−1.32
Daily # of institutional orders	3,395	6.20	6.07	−0.13	−0.51
Institutional order size	3,395	16,247.19	15,064.59	−1,182.60	−2.03
Panel B: NYSE-listed stocks					
Stock price	1,412	29.80	30.27	0.47	0.45
Market equity value (millions)	1,412	7,399.01	7,433.11	34.10	0.04
Daily dollar volume (millions)	1,412	36.45	31.18	−5.27	−1.57
Daily turnover (%)	1,412	0.54	0.49	−0.06	−3.35
Daily # of institutional orders	1,412	8.83	8.32	−0.50	−1.07
Institutional order size	1,412	14,682.16	12,231.94	−2,450.22	−4.78
Panel C: Nasdaq-listed stocks					
Stock price	1,983	15.52	16.60	1.08	1.76
Market equity value (millions)	1,983	1,305.99	1,405.08	99.09	0.32
Daily dollar volume (millions)	1,983	24.12	23.83	−0.29	−0.07
Daily turnover (%)	1,983	0.98	0.96	−0.02	−0.42
Daily # of institutional orders	1,983	4.33	4.47	0.14	0.53
Institutional order size	1,983	17,361.57	17,081.58	−279.98	−0.30

6.3. Multivariate analysis

Firm characteristics, as well as trading conditions, could impact the relation between decimalization and trading costs; therefore we estimate the following regression model:

$$Price\,impact_{i,t} = \alpha + \beta\ D_{i,t} + \delta'\mathbf{C}_{i,t} + \epsilon_{i,t}, \tag{2}$$

where i denotes the firm and t denotes the time period. *Price impact* is measured as the difference between the volume-weighted execution price and the price prevailing at the time the broker receives the order, scaled by the price when the broker receives the order. This measure is multiplied by -1 for sell orders. $D_{i,t}$ represents an indicator variable that is equal to one in the period after decimalization implementation and is equal to zero for the period prior to decimalization. $\mathbf{C}_{i,t}$ is a vector of control variables that includes variables potentially related to trading costs. These control variables include *log(order size), log(dollar volume), log(market value equity), Market to book*, and idiosyncratic volatility (*Ivol*). *Price impact* and the control variables are measured over the month (30 days) before and month after decimalization. Each variable is first constructed at the firm-day level and is then averaged within each firm-month.

We present regression estimates in Table 6 for the full sample and subsamples of NYSE and Nasdaq listings. The coefficient and the associated *t*-statistic on the decimalization indicator variable estimates the significance of decimalization on *Price impact*. The results in Table 6 largely confirm the findings from the univariate analysis, and our main conclusion that institutional trading costs do not dramatically improve following decimalization continues to hold when we control for firm characteristics and measures of trading activity.

6.4. Analysis for liquidity-demanding and liquidity-supplying traders

We next consider whether decimalization has differential effects for different types of institutional traders. Specifically, we examine whether liquidity-demanding traders are affected differently than liquidity-supplying traders. As motivated in Section 3, if the benefits of providing liquidity decrease following decimalization, then we would expect liquidity suppliers to be worse off. On the other side, since spreads decrease, it is possible that liquidity-demanding institutions are at least a little better off following decimalization (depending on how much of the trade executes within the inside quotes). To proxy for liquidity-demanding and liquidity-supplying traders, we follow the approach in Anand et al. (2013), which has been widely used in other studies (see, e.g., Franzoni and Plazzi, 2013, Jame, 2017, Cheng et al., 2017, and Anand et al., 2018).

We construct the trading style measure as follows. First, we compute a variable capturing how frequently each trader trades with or against the market. Specifically, if the sign of the trade is the same sign as the stock's return on that day, then the trade is with the market, and

Table 5
The effects of decimalization on trading costs.
This table presents the mean change in estimated trading costs from the 30 days before to the 30 days after decimalization implementation. *Percent quoted spread* is the best offer quote minus the best bid quote, divided by the midpoint of the quotes. *Dollar quoted spread* is defined in a similar manner except that it is not scaled by the midpoint quote. *Dollar effective spread* is defined as twice the absolute value of the difference between the trade price and the prevailing midpoint of the best bid and offer quotes. *Percent effective spread* scales dollar effective spread by the prevailing midpoint. *Price impact* is measured as the difference between the volume-weighted execution price and the price prevailing at the time the broker receives the order. The difference is scaled by the price prevailing at order time and is multiplied by –1 for sell orders. *Percent quoted spread*, percent effective spread, and price impact is in percentage units. All of the measures are averaged within each firm-day. We then average the measures within each firm-month (30 days). This table reports equally weighted averages across firms. Panel A presents results for the full sample, Panel B for NYSE-listed stocks, and Panel C for Nasdaq-listed stocks. See Section 4 and the Appendix for further details on variable construction.

	N	Mean pre-dec.	Mean post-dec.	Mean change	*t*-stat
Panel A: Full sample					
Dollar quoted spread	3,395	0.154	0.121	−0.033	−10.33
Percent quoted spread (%)	3,395	1.292	0.962	−0.330	−11.63
Dollar effective spread	3,395	0.138	0.112	−0.026	−10.13
Percent effective spread (%)	3,395	1.161	0.886	−0.275	−10.51
Price impact (%)	3,395	0.291	0.354	0.063	1.96
Panel B: NYSE-listed stocks					
Dollar quoted spread	1,412	0.144	0.107	−0.036	−9.31
Percent quoted spread (%)	1,412	0.822	0.599	−0.223	−7.66
Dollar effective spread	1,412	0.120	0.095	−0.025	−10.33
Percent effective spread (%)	1,412	0.675	0.518	−0.157	−6.89
Price impact (%)	1,412	0.295	0.243	−0.051	−1.71
Panel C: Nasdaq-listed stocks					
Dollar quoted spread	1,983	0.162	0.131	−0.031	−6.54
Percent quoted spread (%)	1,983	1.626	1.219	−0.407	−9.81
Dollar effective spread	1,983	0.150	0.124	−0.026	−6.65
Percent effective spread (%)	1,983	1.507	1.148	−0.359	−9.21
Price impact (%)	1,983	0.288	0.433	0.145	2.85

Table 6
Multivariate analysis on the effects of decimalization on trading costs.
This table presents estimated coefficients and robust *t*-statistics in parentheses for regressions of price impact on a decimalization indicator variable and control variables. The *Decimalization indicator* variable is equal to one in the period after decimalization implementation and is equal to zero for the period prior to decimalization. *Price impact* is measured as the difference between the volume-weighted execution price and the price prevailing at the time the broker receives the order, scaled by the price when the broker receives the order. This measure is multiplied by -1 for sell orders. *Price impact* and the control variables are measured over the 30 days before and 30 days after decimalization. Each variable is first constructed at the firm-day level. We then average the measures within each firm-month (30 days). See Section 4 for further details on variable construction.

	Full sample y = *Mo. Price impact*	NYSE stocks y = *Mo. Price impact*	Nasdaq stocks y = *Mo. Price impact*
Decimalization indicator	0.073	−0.011	0.144
	(2.16)	(−0.35)	(2.61)
log(order size)	0.093	0.071	0.105
	(3.02)	(2.59)	(2.34)
log(dollar volume)	−0.050	−0.056	−0.067
	(−2.04)	(−2.00)	(−1.67)
log(market value equity)	0.023	0.026	0.056
	(0.83)	(0.83)	(1.07)
Market to book	0.006	0.002	0.010
	(2.03)	(1.54)	(1.59)
Ivol	3.178	4.950	2.740
	(2.61)	(2.69)	(1.86)
Constant	−0.784	−0.606	−1.105
	(−2.28)	(−2.12)	(−1.91)
Observations	5,585	2,473	3,112
R^2 (%)	1.044	2.011	0.976

Table 7
The effects of decimalization on institutional trading costs for liquidity-demanding and liquidity-supplying traders.
This table presents the mean change in *Price impact* from the 30 days before to the 30 days after decimalization implementation. In this table, we consider two alternative versions of *Price impact*, one version considers only liquidity-demanding traders, while the other version considers only liquidity-supplying traders. See Section 6 for details on how we measure liquidity-demanding and liquidity-supplying traders. The table reports volume-weighted averages across orders, within each group. All of the measures of *Price impact* are in percentage units. Panel A presents results for both NYSE-listed and Nasdaq-listed stocks, Panel B for NYSE-listed stocks, and Panel C for Nasdaq-listed stocks.

	Mean pre-dec.	Mean post-dec.	Mean change	*t*-stat
Panel A: NYSE- and Nasdaq-listed stocks				
Price impact (%) — liquidity-demanding traders	1.371	1.274	−0.097	−0.75
Price impact (%) — liquidity-supplying traders	−0.514	−0.198	0.316	1.57
Panel B: NYSE-listed stocks				
Price impact (%) — liquidity-demanding traders	1.338	0.951	−0.386	−2.24
Price impact (%) — liquidity-supplying traders	−0.543	−0.164	0.379	1.97
Panel C: Nasdaq-listed stocks				
Price impact (%) — liquidity-demanding traders	1.423	1.714	0.291	1.52
Price impact (%) — liquidity-supplying traders	−0.441	−0.246	0.195	0.50

if the signs disagree, then the trade is against the market. Then for each time period and each institution, we compute the difference between the trading volume with the market and trading volume against the market and scale this difference by total volume. We then sort institutions each time period into five groups based on the metric capturing how frequently they trade with or against the market. We keep the two extreme quintiles. The quintile with the largest values are considered liquidity demanders, as they tend to trade with the market, and the group with the smallest values are considered liquidity suppliers, because they tend to trade on the opposite side of the market.

In Table 7, we present trading cost estimates surrounding decimalization for those we code as liquidity demanders or liquidity suppliers. The table reports volume-weighted averages of *Price impact*.[15] We see that average *Price impact* increases for liquidity suppliers, which is consistent with our expectation that liquidity suppliers are worse off following decimalization. *Price impact* tends to decrease for liquidity demanders, though this effect is concentrated in NYSE stocks. Liquidity-demanders' *Price impact* actually increases for Nasdaq stocks, and the change is insignificant for the full sample (Panel A).

6.5. Robustness tests

Despite the fact that our findings are consistent with the evidence in institutional trading cost papers discussed earlier, they are unconventional enough that they require us to perform several robustness tests. We present the results from these tests in the Internet Appendix. First, we separately examine the effects of decimalization for pilot stocks. Leading up to decimalization, both the NYSE and Nasdaq randomly selected pilot stocks that switched to decimal pricing before the exchanges fully implemented decimalization. Studying the pilot stocks gives us some diversity in sample periods, which helps alleviate concerns that our results are driven by spurious factors associated with the time period when the exchanges fully switched to decimal pricing. The results for the pilot stocks are generally similar to our main results: spreads fall following the switch to decimal pricing, but *Price impact* is not significantly affected.

Next, we consider a number of alternative approaches to constructing *Price impact*. The first alternative measure we consider adds commissions to *Price impact*. The second and third alternatives use different benchmark prices to measure *Price impact*. The two alternative benchmarks we consider are the closing stock price the day before first execution (used in Chakravarty et al., 2005) and the opening price on the day of first execution (used in Anand et al., 2013), respectively. In the Internet Appendix, we perform univariate tests for these alternative *Price impact* measures, and our main conclusions continue to obtain.

Finally, we consider an alternative aggregation method for constructing institutional trading costs. Specifically, we construct *Price impact* at the order level rather than at the firm level. For this alternative approach, we construct a volume-weighted *Price impact* measure (across all orders) over the month before and month after decimalization. Further, we sort trades into groups based on order size, and we compare pre-decimalization *Price impact* to post-decimalization *Price impact* (Fig. IA1). Our main results continue to obtain for this alternative analysis.

7. Analysis of research that uses decimalization to exploit exogenous variation in stock liquidity

As we discussed in Section 2, a large number of published and active working papers use decimalization to examine the causal effects of improved stock liquidity on sev-

[15] These results are volume weighted across orders, within each group. In unreported tests, we find qualitatively similar results if we aggregate *Price impact* at the stock level (as we do in Table 5).

eral important issues, such as firm value, innovation activity, and default risk (see Table 1 for the full list of studies). However, we show in Section 6 that *Price impact*, a better measure of trading costs for large traders such as institutional investors and blockholders, does not dramatically improve following decimalization. This result leads to questions about the conclusions drawn from studies employing decimalization as an exogenous shock to liquidity, particularly studies that rely on the liquidity of large traders to develop their hypotheses. To explore this issue, we replicate and extend the main findings from three studies that use decimalization to draw causal inference: Fang et al. (2009), who find that increased stock liquidity positively impacts firm value; Fang et al. (2014), who conclude that improved liquidity negatively impacts firm innovation; and Brogaard et al. (2017), who find that increased liquidity reduces default risk. Each of these studies measure stock liquidity using percent effective spread.[16]

7.1. Stock liquidity's effect on firm value

We first examine the causal impact of liquidity on firm value by following Fang et al. (2009) and by estimating the following regression model:

$$\Delta Value_i = \alpha + \beta\ \Delta Liquidity_i + \delta' \mathbf{C}_i + \epsilon_i, \tag{3}$$

where $\Delta Value_i$ represents the change in stock *i*'s value surrounding decimalization, $\Delta Liquidity_i$ represents stock *i* 's change in liquidity due to the tick size reduction, and $\mathbf{C}_i$ represents a vector of control variables.

We measure firm value (of equity) as the change in market to book (ΔMB) from the month (30 days) before to the month following decimalization implementation. Market value of equity is computed using daily stock data, while book value represents book equity from the previous fiscal year-end. We first construct market to book and the liquidity measures at the stock-day level. We then compute the simple average for the measures each stock-month. Following Fang et al. (2009), we also compute the change in a number of control variables surrounding decimalization. These control variables are Δ*(SP500 indicator)*, which represents the change in S&P 500 membership; Δ*(log[Assets])*, which is the change in book assets; Δ*(Ivol)*, which is the change in idiosyncratic volatility (constructed with the market model); Δ*(log[Analysts])*, which represents the change in the number of analysts covering the firm; and Δ*(Cumret)*, which represents the change in lagged monthly returns. We also include industry fixed effects using two-digit SIC codes.

We consider three alternative stock liquidity measures. The first measure we consider is the measure used in Fang et al. (2009), Δ (*Percent effective spread*), which is the change in *Percent effective spread* from the month before to the month after decimalization. *Percent effective spread* is defined as effective spread divided by the contemporaneous bid-ask midpoint price. However, scaling effective spread by contemporaneous price is problematic in this setting because the y-variable, change in firm equity value, is also dependent on changes in the stock price. Thus, the change in the spread measure could be correlated with the change in firm equity value not because of a change in spread (the numerator in *Percent effective spread*) but because of a change in the firm's stock price (the denominator). We therefore consider an alternative measure, Δ (*Percent effective spread 1*), where *Percent effective spread 1* is defined as effective spread divided by the closing stock price 30 days before decimalization. Finally, we construct Δ(*Price impact 1*), where *Price impact 1* is measured as the difference between the volume-weighted execution price and the price prevailing at the time the broker receives the order, scaled by the closing stock price 30 days prior to decimalization. This measure is multiplied by -1 for sell orders.

We present regression estimates of Eq. (3) in Table 8. We report estimated slope coefficients and associated *t*-statistics in parentheses for the full sample, the NYSE sample, and the Nasdaq sample. Consistent with Fang et al. (2009), we find that Δ(*Percent effective spread*) is significantly related to concurrent change in firm value. These results suggest that increased stock liquidity (reduced *Percent effective spread*) leads to increased firm value. However, our conclusions differ if we proxy for stock liquidity using *Price impact*. The estimated slope coefficient on $\Delta(Priceimpact 1)$ is insignificant for the full sample and NYSE specifications. Further, we do not find any evidence that Δ (*Percent effective spread 1*) is related to firm market value. This result suggests that Δ(*Percent effective spread*) is related to ΔMB because of the change in the stock price rather than the change in the spread. In summary, using tests similar to those employed in Fang et al. (2009), we do not find compelling evidence that stock liquidity has a positive causal effect on firm value.

7.2. Stock liquidity's impact on corporate innovation

In this section, we replicate and extend the main results from Fang et al. (2014), who find that increased stock liquidity reduces corporate innovation. We closely follow Fang et al. (2014)'s approach and perform a difference-in-differences (DiD) test surrounding decimalization. We consider three samples—the full sample, NYSE stocks only, and Nasdaq stocks only. Further, we analyze two measures of stock liquidity, *Percent effective spread* (the measure used in Fang et al., 2014) and *Price impact*. For each liquidity measure and each sample, we sort firms into three groups based on change in liquidity from the month before to the month after decimalization. The tercile with the largest improvement in stock liquidity is considered the "treatment" group, while the tercile with the smallest liquidity improvement, including stocks in which liquidity worsened, is considered the "control" group.

We then generate propensity scores using probit regression estimates, where the dependent variable is an indicator variable equaling one for the treatment group and zero for the control group. Using these propensity scores, we match treatment and control firms using a one-to-many (5) approach. Finally, we compute the change in innovation from the three years before to the three years after deci-

[16] To replicate and extend each of these analyses, we construct the liquidity measures at a monthly frequency, but our main conclusions continue to obtain if we instead employ an annual frequency.

Table 8
Effect of liquidity on firm value, using decimalization as an instrument.
This table presents estimated slope coefficients and robust *t*-statistics in parentheses for regressions of a firm value proxy on alternative measures of trading costs and control variables. The methodology closely follows Fang et al. (2009). The firm value measure is the change in market to book (Δ*MB*) from the month (30 days) before to the month following decimalization implementation. Market value of equity is computed using daily closing stock prices, while book value represents book equity from the previous fiscal-year-end. Δ(*Percent effective spread*), Δ(*Percent effective spread 1*), and Δ(*Price impact 1*) represent the change in alternative liquidity measures from the month before to the month following decimalization. *Percent effective spread* is effective spread, scaled by the concurrent midpoint quoted price. *Percent effective spread 1* scales effective spread by the closing stock price at the beginning of the spread measurement period (30 days before decimalization). *Price impact 1* is measured as the difference between the volume-weighted execution price and the price prevailing at the time the broker receives the order, scaled by the closing stock price at the beginning of the trading cost measurement period. This measure is multiplied by –1 for sell orders. We first construct *MB* and the liquidity measures at the firm-day level. We then average the measures within each firm, within the two time groups. Δ*(SP500 indicator)*, Δ*(log[Assets])*, Δ*(Ivol)*, Δ*(log[Analysts])*, and Δ*(Cumret)* are control variables. We also include industry fixed effects using two-digit SIC codes. See Sections 4 and 7 for further details on variable construction.

	Full sample			NYSE-listed stocks			Nasdaq-listed stocks		
	Δ*MB*	Δ*MB*	Δ*MB*	Δ*MB*	Δ*MB*	Δ*MB*	Δ*MB*	Δ*MB*	Δ*MB*
Δ(*Percent effective spread*)	−0.047			−0.178			−0.032		
	(−1.91)			(−3.33)			(−2.29)		
Δ(*Percent effective spread 1*)		0.028			−0.014			0.051	
		(1.04)			(−0.26)			(1.26)	
Δ(*Price impact 1*)			−0.021			0.005			−0.032
			(−1.56)			(0.53)			(−1.94)
Δ*(SP500 indicator)*	0.016	0.007	0.008	0.140	0.126	0.125	−0.045	−0.056	−0.060
	(0.09)	(0.04)	(0.04)	(0.73)	(0.65)	(0.64)	(−0.14)	(−0.17)	(−0.18)
Δ*(log[Assets])*	0.756	0.748	0.752	0.238	0.230	0.229	0.857	0.847	0.853
	(5.13)	(5.06)	(5.11)	(1.84)	(1.77)	(1.77)	(4.69)	(4.62)	(4.68)
Δ*(Ivol)*	−3.175	−3.448	−3.299	−2.452	−3.166	−3.281	−3.466	−3.712	−3.590
	(−2.00)	(−2.16)	(−2.10)	(−1.57)	(−1.97)	(−2.12)	(−1.66)	(−1.78)	(−1.73)
Δ*(log[Analysts])*	0.284	0.284	0.285	0.078	0.073	0.073	0.390	0.387	0.389
	(3.12)	(3.11)	(3.12)	(1.57)	(1.43)	(1.43)	(2.55)	(2.53)	(2.52)
Δ*(Cumret)*	4.092	4.312	4.344	2.032	1.948	1.881	4.496	4.781	4.714
	(2.26)	(2.39)	(2.41)	(0.99)	(0.95)	(0.90)	(1.83)	(1.94)	(1.93)
Observations	2,788	2,788	2,788	1,235	1,235	1,235	1,553	1,553	1,553
Industry fixed effects	YES	YES	YES	YES	YES	YES	YES	YES	YES
R^2 (%)	11.59	11.45	11.56	10.58	9.882	9.891	14.11	14.11	14.31

malization for both the treatment and control groups. The DiD estimator is the difference in the change in innovation between these two groups.

To construct the innovation measures, we use data obtained from the NBER Patent Citation Data File, which ends in 2006. We augment this data set with patent data from 2007 to 2010, using data employed in Kogan et al. (2017).[17] The first innovation measure is the number of patents applications a firm files each year that are eventually granted, and the second measure is the number of nonself-citations each firm-year. Since patent grants and citations continue to occur after the sample period ends, we follow Fang et al. (2014) and scale up the number of patents and citations using weight factors developed from the shape of the patent application lag distribution and citation lag distribution, respectively.[18] The variables used to estimate the propensity scores include return on assets (*ROA*); *Investment; log(market value equity)*; Tobin's Q (*Q*); property, plant and equipment (*PPE*); capital expenditures (*Capex*); research and development (*R&D*); *log(age); Leverage; log(1+cites) growth*; and *log(1+patents) growth*. These variables are measured in the year immediately preceding decimalization, and growth in the cites and patents are measured from year *t*-3 to year *t*-1, where decimalization occurs in year 0.

To conserve space, we use the Internet Appendix to present the table that has the estimated probit regression coefficients used to estimate propensity scores and the table that presents mean differences in firm characteristics between treatment and control firms. Small differences in observables between treatment and control firms reduce concerns that something other than a shock to liquidity impacts innovation. The Internet Appendix results show that the differences in observables are generally insignificant, regardless if we measure liquidity with *Percent effective spread* or *Price impact*.

[17] We thank the authors for making their data available.

[18] Dass et al. (2017) show that NBER patent data ending in 2006, the data used in Fang et al. (2014), do not accurately capture patent grants and cites that occur after 2006, even after attempts to correct for this truncation bias. Dass et al. (2017) use a recently available database that extends patent data through 2010 to better capture patent grants and cites. They replicate Fang et al. (2014)'s finding that liquidity is significantly negatively related to innovation using 2006 data but find no evidence of a significant relation once they use patent data ending in 2010. We also use patent data ending in 2010, but we replicate Fang et al. (2014)'s finding that a drop in *Percent effective spread* is significantly related to innovation (see Panel A in Table 9). However, once we employ the same empirical methods used in Dass et al. (2017), which include i) measuring liquidity at an annual rather than monthly frequency, ii) measuring change in innovation from the year of decimalization to two or three years following decimalization (rather than from the three years before to the three years after decimalization), and iii) estimating a model where we regress the change in innovation on the change in liquidity surrounding decimalization (rather than using a propensity score matching approach), we successfully replicate Dass et al. (2017)'s findings that a drop in *Percent effective spread* is unrelated to future innovation.

Table 9
Difference-in-differences analysis: liquidity's effect on innovation.
This table presents results for difference-in-differences tests where we examine the effects of stock liquidity on innovation. We present results for three samples—the full sample, NYSE stocks, and Nasdaq stocks. We consider two alternative stock liquidity measures, *Percent effective spread* and *Price impact*. For each liquidity measure and each sample, we sort firms into three groups based on change in liquidity from the month before to the month after decimalization. The stocks with the largest improvement in stock liquidity are considered the "treatment" group, while the stocks with the worst improvement are considered the "control" group. For each of these two groups, we present the mean change in innovation (either logged cites or logged patents) from the three years before to the three years after decimalization. Additionally, this table reports the difference between the treated and control groups, along with the associated *t*-statistic. See Sections 4 and 7 for further details on variable construction.

	Obs.	Treated	Control	Difference	*t*-stat
Panel A: *Percent effective spread*—full sample					
Δ *log(1+cites)* surrounding decimalization	724	−1.180	−0.378	−0.801	−3.10
Δ *log(1+patents)* surrounding decimalization	724	−0.159	0.054	−0.214	−1.84
Panel B: *Percent effective spread*—NYSE sample					
Δ *log(1+cites)* surrounding decimalization	326	−0.609	−0.363	−0.246	−0.82
Δ *log(1+patents)* surrounding decimalization	326	−0.041	−0.032	−0.009	−0.06
Panel C: *Percent effective spread*—Nasdaq sample					
Δ *log(1+cites)* surrounding decimalization	398	−1.502	−0.620	−0.883	−2.63
Δ *log(1+patents)* surrounding decimalization	398	−0.236	0.078	−0.313	−2.20
Panel D: *Price impact*—full sample					
Δ *log(1+cites)* surrounding decimalization	724	−0.572	−0.790	0.218	1.04
Δ *log(1+patents)* surrounding decimalization	724	0.074	−0.020	0.094	0.97
Panel E: *Price impact*—NYSE sample					
Δ *log(1+cites)* surrounding decimalization	326	−0.543	−0.516	−0.027	−0.10
Δ *log(1+patents)* surrounding decimalization	326	−0.022	−0.025	0.003	0.02
Panel F: *Price impact*—Nasdaq sample					
Δ *log(1+cites)* surrounding decimalization	398	−0.628	−1.215	0.586	1.90
Δ *log(1+patents)* surrounding decimalization	398	0.134	−0.039	0.173	1.28

Table 9 presents the main results from the innovation analysis. Panels A, B, and C in Table 9 show results for *Percent effective spread* for the full sample, NYSE sample, and Nasdaq sample, respectively. The full sample results are consistent with those in Fang et al. (2014); stocks with the largest improvement in stock liquidity (treatment group) experience a significantly larger reduction in corporate innovation compared to stocks with the smallest improvement in liquidity (control group). This finding appears to be driven by Nasdaq-listed stocks, as the difference in innovation is significant in the Nasdaq sample but is insignificant for the NYSE sample. Inference drastically changes if we measure stock liquidity with *Price impact*. These results, presented in Panels D, E, and F of Table 9, suggest that stock liquidity does not negatively impact firm innovation, regardless of the innovation measure. Though Fang et al. (2014) suggest that transient institutional investors are responsible for the drop in firm innovation, our results suggest that institutional investors did not receive a shock to their ability to trade. Therefore, any significant change in corporate innovation is unlikely to be driven by changes in institutional liquidity.

7.3. Stock liquidity's effect on default risk

In this section, we replicate and extend the main analysis in Brogaard et al. (2017), who find that improved stock liquidity leads to reduced default risk. We follow Brogaard et al. (2017) and perform a DiD analysis, which closely follows the approach testing firm innovation described in the previous section. Consistent with Brogaard et al. (2017), we measure default risk using expected default frequency (EDF), which is a simplified version of distance-to-default (DD). We construct EDF as follows:

$$DD_{i,t} = \frac{\log(\frac{Equity_{i,t}+Debt_{i,t}}{Debt_{i,t}}) + (r_{i,t-1} - \frac{\sigma^2_{Vi,t}}{2})}{\sigma_{Vi,t}}, \tag{4}$$

$$\sigma_{Vi,t} = \frac{Equity_{i,t}}{Equity_{i,t}+Debt_{i,t}} \times \sigma_{Ei,t} + \frac{Debt_{i,t}}{Equity_{i,t}+Debt_{i,t}} \times (0.05 + 0.25 \times \sigma_{Ei,t}), \tag{5}$$

$$EDF_{i,t} = N(-DD_{i,t}), \tag{6}$$

where $Equity_{i,t}$ represents year-end market value of equity, in millions; $Debt_{i,t}$ is the sum of debt in current liabilities and one-half of long-term debt; $r_{i,t-1}$ represents annual stock returns from the previous year; $\sigma_{Vi,t}$ approximates asset volatility; $\sigma_{Ei,t}$ is the standard deviation of returns, estimated with monthly returns from the previous year; and $N(\cdot)$ represents the cumulative standard normal distribution function. We measure EDF over the year before and year after decimalization. The variables used to estimate propensity scores include *log(equity)*, *log(debt)*,

Table 10
Difference-in-differences analysis: liquidity's effect on default risk.
This table presents results for difference-in-differences tests where we examine the effects of stock liquidity on default risk. We present results for three samples—the full sample, NYSE stocks, and Nasdaq stocks. We consider two alternative stock liquidity measures, *Percent effective spread* and *Price impact*. For each liquidity measure and each sample, we sort firms into three groups based on change in liquidity from the month before to the month after decimalization. The stocks with the largest improvement in stock liquidity are considered the "treatment" group, while the stocks with the worst improvement are considered the "control" group. For each of these two groups, we present the mean change in default risk, measured with expected default frequency (*EDF*), from the year before to the year after decimalization. Additionally, this table reports the difference between the treated and control groups, along with the associated *t*-statistic. See Sections 4 and 7 for further details on variable construction.

	Obs.	Treated	Control	Difference	*t*-stat
Panel A: *Percent effective spread*—full sample					
Δ *EDF* surrounding decimalization	953	0.011	0.037	−0.026	−1.83
Panel B: *Percent effective spread*—NYSE sample					
Δ *EDF* surrounding decimalization	394	−0.023	−0.018	−0.005	−0.25
Panel C: *Percent effective spread*—Nasdaq sample					
Δ *EDF* surrounding decimalization	559	0.019	0.050	−0.031	−1.66
Panel D: *Price impact*—full sample					
Δ *EDF* surrounding decimalization	953	0.025	0.011	0.013	0.85
Panel E: *Price impact*—NYSE sample					
Δ *EDF* surrounding decimalization	394	0.007	−0.001	0.008	0.44
Panel F: *Price impact*—Nasdaq sample					
Δ *EDF* surrounding decimalization	559	0.035	0.036	−0.001	−0.07

the inverse of annualized stock volatility (*1/σ*), the difference between a stock's annual return and the CRSP value-weighted return (*Excess return*), and the ratio of net income to assets (*Income/assets*). Following Brogaard et al. (2017), we drop financial firms (industry codes 60–69) and compute *Percent effective spread* using equal-weighted, rather than volume-weighted, trades.

We present estimated probit regression coefficients used to estimate propensity scores and mean differences in firm characteristics between treatment and control firms in the Internet Appendix. If we measure liquidity with *Percent effective spread*, most of the differences in firm characteristics between the treatment and control groups are insignificant, and all of the differences are statistically insignificant when we measure liquidity with *Price impact*.

Table 10 presents the DiD results. Panel A of Table 10 shows that, for the full sample, increased stock liquidity leads to reduced default risk if we measure stock liquidity with *Percent effective spread*. This result confirms the main findings in Brogaard et al. (2017). Panels B and C of Table 10 reveal that this effect is primarily driven by Nasdaq stocks, as the difference in default risk is small and insignificant for NYSE-listed stocks. Panels D, E, and F in Table 10 present effects for the *Price impact* measure, which yields considerably different findings. These results suggest that any change in institutional liquidity surrounding decimalization is not related to default risk, regardless of exchange listing.

7.4. Further discussion of the impact of decimalization

Although our results show that decimalization does not significantly impact institutional trading costs, we do not wish to suggest that decimalization has no effect on trading. For example, consistent with Bessembinder (2003)'s conclusion that small traders have benefited post-decimalization, Bodnaruk and Östberg (2013) find that decimalization reduces trading costs for retail investors and the resulting increase in retail trader participation results in a larger shareholder base. This effect has been shown by Bodnaruk and Östberg (2013) to cause firms to increase their payout ratios and decrease their cash holdings. In support of these findings, the model of Holmström and Tirole (1993) predicts that price informativeness is related to the size of the shareholder base, and the link between retail participation and price efficiency is supported by the empirical findings in Kelley and Tetlock (2013), who show that trading by retail investors can contribute to market efficiency. Chordia et al. (2008) also report that market efficiency improves post-decimalization since the short-horizon return predictability from order flows drops significantly, particularly for large firms.

The goal of our paper is to suggest that researchers carefully identify possible channels that could drive their results. Decimalization can identify exogenous changes in trading costs for retail investors who trade a small amount of shares. Thus, researchers who rely on retail investors as a primary mechanism may still find decimalization as a plausible identification strategy. However, we contend that assuming institutional liquidity falls significantly can lead researchers to draw incorrect conclusions about the underlying cause of the documented changes.

8. Concluding remarks

This paper provides new evidence on institutional trading costs and how they relate to widely used measures of stock liquidity. Using a *Price impact* measure we construct from proprietary order-level data, we find that institutional trading costs are weakly related to many common measures of liquidity. Further, we find that *Price impact* is not dramatically impacted by the switch to decimals pricing in US equity markets. These findings have important implications for research in financial economics.

Given the prevailing view in the academic literature that stock liquidity significantly improves following tick size reductions, a large number of studies use decimalization to identify the causal effects of increased stock liquidity on corporate activity. A number of these studies rely on theories predicting that institutional liquidity is the key driver of their results. Since we find that institutional trading costs do not significantly change following decimalization, our results raise questions about studies that use decimalization as an identification strategy. To explore this issue further, we replicate and extend prior research, and we find that conclusions drawn from several studies are dramatically altered once we measure liquidity using institutional trading costs.

We conclude by encouraging future research to thoughtfully consider the mechanisms that result in the firm decisions or outcomes considered in the analysis. If the main driver is actions by blockholders or institutional investors, then our analysis suggests that many commonly used measures of liquidity do not accurately measure institutional liquidity and using decimalization as an identification mechanism is not an effective instrument for institutional trading costs.

Appendix A. Alternative liquidity measures

Quoted spread: *Percent quoted spread* is defined as

$$Percent\ quoted\ spread_{i,s} = \frac{|O_{i,s} - B_{i,s}|}{M_{i,s}}. \tag{A.7}$$

Percent quoted spread for firm i is the best offer quote at time (in seconds) s, denoted $O_{i,s}$, minus the best bid quote at time s, denoted $B_{i,s}$, divided by the midpoint of the quotes, $M_{i,s}$. *Dollar quoted spread* is defined in a similar manner except that it is not scaled by the midpoint quote. Each firm's daily weighted average quoted spread is time weighted by the amount of time that the quote is in force. To construct stock-month versions of these measures, we compute the simple average across days in the month.

Effective spread: *Percent effective spread* is defined as

$$Percent\ effective\ spread_{i,k} = 2\,\frac{|P_{i,k} - M_{i,k}|}{M_{i,k}}. \tag{A.8}$$

Percent effective spread for firm i's k'th trade is the absolute value of the difference between the trade price ($P_{i,k}$) and the prevailing midpoint of the best bid and offer quotes ($M_{i,k}$), multiplied by two to measure round-trip trade cost. *Dollar effective spread* is constructed similarly except it is not scaled by the prevailing midpoint. We match each trade with the prevailing quote that immediately precedes the trade. We construct each firm's daily effective spread as the dollar volume-weighted average spread across trades. To construct stock-month versions of these measures, we compute the simple average across days in the month. We primarily follow the procedures described in Holden and Jacobsen (2014) to construct these spread measures. Specifically, we drop trades occurring out of sequence, trades recorded outside of normal trading hours, and trades with special settlement conditions.[19] Also, we exclude quotes in which the ask quote is more than $5 greater than the bid quote, crossed quotes on the same exchange (where the bid quote exceeds the offer quote), withdrawn quotes, and quote conditions that are abnormal (such as quotes associated with trading halts).[20]

Amihud measure: Amihud (2002) proposes a liquidity measure designed to capture the sensitivity of stock price movements to trading volume level. The Amihud liquidity measure is computed using daily CRSP data and aggregated over month t and is constructed as follows

$$Amihud_{i,t} = \frac{1}{D_{i,t}} \times \sum_{t=1}^{D_{i,t}} \frac{|R_{i,d,t}|}{DVOL_{i,d,t}}, \tag{A.9}$$

where $D_{i,t}$ is the number of trading days for stock i during interval t, $|R_{i,d,t}|$ is the absolute value of return for stock i on day d during time interval i, and $DVOL_{i,d,t}$ is the respective dollar trading volume. Consistent with prior studies, we multiply this measure by 10^6.

Closing spread measure: This measure is constructed using daily closing bid and ask quotes from CRSP. Each firm-day, we compute the difference between the ask and the bid, scaled by the bid-ask midpoint. This measure is averaged across days to give closing spread for each firm-month.

Roll measure: The Roll (1984) liquidity measure is an estimate of the percent effective spread based on the serial covariance of successive price movements. The Roll measure is constructed for each firm i over each month t as follows

$$Roll_{i,t} = 2\sqrt{-Cov(R_d, R_{d-1})}, \tag{A.10}$$

where R_d (R_{d-1}) is the return on day d (day $d-1$). We set the Roll estimator equal to zero when the first-order serial covariance of returns is positive so that the liquidity measure is well defined.

Effective tick measure: Holden (2009) and Goyenko et al. (2009) propose the effective tick measure, a proxy for percent effective spread measure based on observable price clustering. Effective

[19] Specifically, we exclude TAQ trades with the following codes: C, D, G, L, N, O, R, T, U, W, Z, 2, or 3.

[20] Specifically, we exclude TAQ quotes with the following quote condition codes: 0, 4, 5, 7, 9, 11, 13, 14, 15, 19, 20, 27, or 28.

tick is constructed under the assumption that price clustering is completely determined by spread size. This implies that the simple frequency in which closing prices occur in particular price clusters can be used to estimate spread probabilities. Using these estimated probabilities, firm *i*'s effective tick measure is a weighted average of its estimated spreads over period (month) *t*. See Holden (2009) and Craig Holden's website for further details on variable construction.[21] We construct the effective tick measure using a 1/8 fractional price grid up until 1997 when the major US stock exchanges implemented a 1/16 tick size. We use a 1/16 fractional price grid from 1997–2000. In 2001, the major exchanges implemented decimal pricing, and we use a decimal price grid from 2001 until the end of our sample period.

Corwin Schultz measure: The Corwin and Schultz (2012) liquidity measure uses high and low daily price quotes to approximate the percent effective spread. Daily high prices are almost always buyer-initiated orders, while daily low prices are almost always seller-initiated orders, which suggests that the ratio of high to low prices reflects both the stock's variance and its spread. The variance component grows proportionately with the time interval, but the spread component does not. This fact allows the spread to be isolated from the variance by setting up two equations. We estimate the Corwin Schultz spread for firm *i* over time (month) *t* as follows

$$Spread_{i,t} = \frac{2(e^{\alpha}-1)}{1+e^{\alpha}}, \tag{A.11}$$

where α is computed as

$$\alpha = \frac{\sqrt{2\beta}-\sqrt{\beta}}{3-2\sqrt{2}} - \sqrt{\frac{\gamma}{3-2\sqrt{2}}}, \tag{A.12}$$

where β and γ are computed as

$$\beta = \sum_{j=0}^{1}\left[\ln\left(\frac{H^0_{d+j}}{L^0_{d+j}}\right)\right]^2, \tag{A.13}$$

$$\gamma = \left[\ln\left(\frac{H^0_{d,d+1}}{L^0_{d,d+1}}\right)\right]^2, \tag{A.14}$$

where $H^0_{d,d+1}$ and $L^0_{d,d+1}$ are the observed high and low prices, respectively, over a two-day window beginning at day d and ending on day $d+1$. We apply Corwin and Schultz's suggested filters including adjustments due to overnight price changes and an adjustment to zero if the spread estimate is negative. See Corwin and Schultz (2012) for further details.

FHT measure: The Fong et al. (2017) liquidity measure captures two key elements of transaction costs: return volatility and the proportion of zero returns. This measure represents a simplification of the Lesmond et al. (1999) (LOT) model. Specifically, for each stock *i* over time period (month) *t*, the FHT liquidity estimator is computed as

$$FHT_{i,t} = 2\hat{\sigma}\,\Phi^{-1}\left(\frac{1+zeros}{2}\right), \tag{A.15}$$

where $\hat{\sigma}$ is the standard deviation of returns for stock *i* over time window *t*, Φ^{-1} represents the inverse of the cumulative normal distribution, and *zeros* is the number of zero-return days divided by the number of trading days in time interval *t*.

Zeros measure: This liquidity measure, proposed by Lesmond et al. (1999), is the proportion of zero-return trading days for firm *i* during time interval (month) *t*. Therefore, the zeros measure serves as a liquidity proxy because zero-return days often occur when transaction costs keep marginal investors from trading. The frequency of zero returns are increasing in transaction costs.

[21] http://www.kelley.iu.edu/cholden/examples.pdf

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