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Does Trading Anonymously Enhance Liquidity?

Patrick J. Dennis and Patrik Sandås*

Abstract

Is liquidity better when a trade counterparty's brokerage firm is unknown (anonymous) or known (transparent)? We examine a quasinatural experiment where some firms switched from transparent to anonymous trading and then, 1 year later, switched back. Our results for inside spread, price impact, and limit order book depth suggest that liquidity improves when anonymous post-trade reporting is introduced and liquidity worsens when anonymous post-trade reporting is reversed.

I. Introduction

Is liquidity better when a trade counterparty's brokerage firm is unknown (anonymous) or known (transparent)? The question of optimal market transparency is not settled. This uncertainty is probably the reason why different exchanges around the world differ in the degree of transparency and why exchanges continue to make changes to their market structure. For example, the London Stock Exchange is anonymous, Nasdaq Nordic is transparent, and the Toronto Stock Exchange is a hybrid where, while anonymous, one can subscribe to a report that reveals the counterparty's brokerage firm associated with each trade. In addition, both the theoretical and empirical literature have not conclusively resolved whether anonymity helps or hurts liquidity. Using a quasinatural experiment that allows us to control for confounding events, we provide additional evidence in the debate on trade transparency, specifically that anonymous markets are more liquid.

*Dennis, pjd9v@virginia.edu, University of Virginia McIntire School of Commerce; Sandås (corresponding author), patriks@virginia.edu, University of Virginia McIntire School of Commerce. Sandås was a SIFR research affiliate and a Riksbanken visiting professor when most of this research was undertaken. We are grateful for financial support from the McIntire School of Commerce, Nasdaq Nordic, SIFR, Sveriges Riksbank, for comments from Petter Dahlström, Richard Evans, Anders Green, Björn Hagströmer, Björn Hertzberg, Matti Keloharju, Mika Koskinen, Marc Lipson, Raymond Liu, Timo Rothovious, Matti Suominen, Jared Stanfield, Andras Vajlok, and Xin Zhang, and seminar participants at Aalto University, Australian National University, City University of Hong Kong, Hong Kong Polytechnic University, Nasdaq Nordic Stockholm, Stockholm Business School, Sveriges Riksbank, University of Auckland, University of New South Wales, University of Virginia, and the 2017 Annual Meeting of the Midwest Finance Association.

Post-trade anonymous reporting was implemented by the Nasdaq Nordic's exchanges on June 2, 2008 and applied to all equity-related markets in Helsinki, Reykjavik, and the 5 most traded stocks in Stockholm. For stocks under the new post-trade anonymous reporting regime, the broker codes were removed from all real-time market data feeds. In contrast, stocks in Stockholm, except for the 5 most traded ones, and all stocks in Copenhagen, continued to trade with broker codes for each trade included in the real-time market feed. Since previous research (e.g. Linnainmaa and Saar (2012), Frino, Johnstone, and Zhang (2010)) document that there is information in knowing the counterparty's brokerage firm,[1] removing these codes reduces transparency. The implementation of the change therefore created a quasinatural experiment with stocks in Helsinki, Reykjavik and the top 5 in Stockholm forming the treatment group that became anonymous and the remaining ones in Stockholm and all those in Copenhagen forming the control group that remained transparent.[2] Less than a year later, on Apr. 14, 2009, the initial experiment was almost completely reversed when all but the 5 most traded stocks in Helsinki reverted back to transparent post-trade reporting. We exploit this pair of experiments to identify the effects of both the switch to anonymous post-trade reporting and the switch back to transparent post-trade reporting 1 year later.

We compare the spread, price impact, and limit order book depth (hereafter simply "book depth") for a treatment sample consisting of those stocks that became anonymous to a matched control sample of those that remained transparent. First, we find that anonymous trading resulted in an economically and statistically significant decrease of the quoted spread of approximately 76 basis points (bps). One year later, when the switch was made back to transparent post-trade reporting, the quoted spread increased by 12 bps for the stocks that switched, but the change was only statistically significant for large stocks.

Second, we examine price impact effects for large (small) trades, defined as those above (below) the median trade size. We find that large (small) buys associated with stocks that became anonymous had a statistically significant 14 (20) bps lower price impact compared to those stocks that remained transparent. Sells associated with stocks that became anonymous also had a lower price impact, but had a smaller economic size than the price impact for buys. These changes in price impact are consistent with the reported changes in the quoted spread. However, 1 year later when the switch was made back to transparent post-trade reporting, the price impact for large (small) buys in those stocks that switched from anonymous to transparent trading also had a statistically significant 23 (14) bps lower price impact. There was little difference in the price impact for sells.

Last we examine the effect of anonymity on book depth. The limit order book for those stocks that became anonymous deepened, with anonymous stocks having a book depth of 132,205 euros higher than those that remained transparent. When the anonymous stocks switched back to transparent post-trade reporting

[1] For example, perhaps it is costly for investors to randomize their order submissions sufficiently to make information in the broker codes worthless.

[2] This is not a pure natural experiment with randomized assignment to control and treatment groups, but rather, like most experiments in economics, a quasinatural one in the spirit of Card and Krueger (1994).

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1 year later, we find that the book depth decreased by 146,380 euro for those stocks that switched to transparency compared with the control group. Our results for the spread, price impact, and book depth suggest that liquidity improves when anonymous post-trade reporting is introduced and liquidity worsens when anonymous post-trade reporting is reversed.

There is little theory that specifically focuses on post-trade anonymity[3] and empirical results about the effect of anonymity are mixed. Empirical studies can be categorized as pretrade transparency, those that examine different aspects of limit order book dissemination, and post-trade transparency, those that examine information such as past trades, prices, and counter-parties.[4] We will focus on the latter. Specifically, the form of transparency that we study in this paper refers to being able to discern the identity of the brokerage firm that originated the trade.[5] Post-trade transparency studies of block trades by Board and Sutcliffe (2000) and Gemmill (1996) focus on experiments on the London Stock Exchange where the publication of block trades was delayed. Both studies found little evidence that this change in transparency influenced liquidity. The introduction of the TRACE (Trade Reporting and Compliance Engine) system for U.S. corporate bond markets increased transparency and a number of studies (e.g., Bessembinder, Maxwell and Venkataraman (2006), Edwards, Harris and Piwowar (2007), and Goldstein, Hotchkiss and Sirri (2007)) document that this led to improvements in liquidity. Post-trade anonymity, which is a special form of post-trade transparency, has been examined by Hachmeister and Schiereck (2010), Friederich and Payne (2014), Frino et al. (2010), Lepone, Segara and Wong (2012), Linnainmaa and Saar (2012), Meling (2018), Pham, Swan and Westerholm (2015), Menkhoff and Schmeling (2010), and Poskitt, Marsden, Nguyen, and Shen (2011). Most, but not all, find that a reduction in transparency of this form improved liquidity. We discuss how our study relates to the existing literature in more detail in Section II. The main difference between our study and existing work lies in the nature of the experiment we study, that is, the switch to and from transparency and possessing a control sample of stocks that did not undergo a change in trade reporting.

II. Motivation and Related Literature

Next, we expand on how our work fits into the existing literature outlined in the introduction. Specifically, we briefly review studies that establish that counterparty broker codes contain information and then turn to the empirical evidence, focusing on studies that examine the impact of post-trade transparency versus anonymity.

A. Broker Codes

In markets that feature some form of post-trade disclosure of information about trade counter-parties, such as broker codes, one might expect traders to use multiple brokers to randomize any information content in the broker codes.

[3]For example, Pagano and Röell (1996), Rindi (2008), and Buffa (2013) all focus on pretrade transparency.

[4]See Foucault, Pagano and Röell (2013) for a good introduction.

[5]Throughout this paper "broker" and "broker code" refers to the firm representing an investor.

Frino et al. (2010) and Lepone et al. (2012) examine the information content of broker codes and find that the market attributes greater information content to successive unidirectional trades by a single broker rather than by different brokers. Menkhoff and Schmeling (2010) report similar findings for the foreign exchange market. Linnainmaa and Saar (2012) combine data on the identities of individual investors with order flow data and find that while individual investors can and do follow mixed strategies to some extent, the broker codes are still predictive signals.[6] To summarize, there must be frictions in the market, such as transaction costs or agency problems, that prevent investors from using multiple brokers and, as a consequence, the counter-party identifiers or broker codes convey information.

B. Empirical Results

Poskitt et al. (2011) find that spreads widened on the New Zealand Stock Exchange when anonymity was introduced. In contrast, Hachmeister and Schiereck (2010) and Friederich and Payne (2014), found that the introduction of central counter-party clearing (CCP), which causes anonymity to change from bilateral post-trade transparency to complete anonymity, improved liquidity on the XETRA (Exchange Electronic Trading, a trading platform of Deutsche Börse Group) in Frankfurt and the SETS (Stock Exchange Electronic Trading Service) in London, respectively. Meling (2018) uses a regression discontinuity design and finds anonymity improves liquidity on the Oslo Stock Exchange.

We also find that liquidity improves under anonymity but our experimental design differs from these studies in several ways. First, our experiment has 2 sequential events, first a change to anonymity and then, 10 months later, a change from anonymity, whereas Hachmeister and Schiereck (2010) and Friederich and Payne (2014) examine a single change to anonymity. The reversal of the anonymity event in our sample helps to assure us that any liquidity changes around the change to/from anonymity are not due to other confounding events. Second, the specific type of change we examine is one of multilateral to bilateral transparency. In our setting, transparency is multilateral, meaning that *all* members of the exchange could see the counterparty broker codes associated with any trade in real time. When the switch was made to anonymity, *only the counter-parties* observe the broker codes associated with the trade in real time. Hence, while our anonymous regime is still bilaterally transparent, it is a significant change to go from a transparent regime where all participants observe the counterparty's broker codes associated with each side of a trade to one in which only 2 participants know the broker codes on the other side of the trade. This contrasts with other empirical studies such as Hachmeister and Schiereck (2010) and Friederich and Payne (2014) that examine a change from bilateral transparency to complete anonymity (i.e. a change from 2 participants to no participants knowing the broker on each side of a trade). In a sense a change from multilateral transparency to bilateral transparency is a "larger" change compared to a change from bilateral

[6]Though they examine intermediated markets, Ellis, Michaely, and O'Hara (2002) and Schultz (2003) find that dealers tend to be fairly concentrated in particular stocks, making inferences about information easier.

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transparency to complete anonymity. Third, our control sample is better matched. Hachmeister and Schiereck have no control sample whereas Friederich and Payne use a control sample drawn from the Stoxx Europe 600 Index which is a pan-European index including the Eurozone, the United Kingdom, Swiss and Swedish stocks. In contrast, our control sample consists of stocks that all trade on Nasdaq Nordic, some of which switched to anonymity and some of which did not.

III. Institutional Setting and Sample

A. Institutional Setting

On June 2, 2008, the Nasdaq Nordic Exchanges started post-trade anonymous trading on the Helsinki and the Reykjavik exchanges across all equity-related markets. Under anonymity, the real-time broker codes were removed from the market data feed and SAXESS[7] trade ticker, however the counter-parties still observed each other's broker in real time. Later, the broker codes for each trade were revealed to all market participants at 18:00 hours each day. In Stockholm, the switch to anonymity was limited to the 5 most actively traded stocks: Ericsson B, Volvo B, Telia Sonera, Nordea, and H&M B. Based on turnover figures from the previous year, these 5 stocks represented roughly 33% of total turnover in Stockholm. There was no change to post-trade transparency on the Copenhagen exchange or the Stockholm exchange with the exception of the 5 stocks listed above.

On Apr. 14th, 2009, the exchange reversed the decision on anonymous post-trade reporting and switched back to the earlier transparent post-trade reporting regime for all but the 5 most traded stocks in Helsinki (Nokia, Fortum, UPM-Kymmene, Sampo, and Stora Enso), which remained post-trade anonymous. This event created another quasinatural experiment with the treatment group consisting of those stocks that switched from anonymity to transparency and the control group consisting of those stocks that were traded under the transparent post-trade reporting regime both before and after this date. Table 1 summarizes when each group of firms was either post-trade anonymous or transparent.

TABLE 1

Timeline Of Changes Between Transparent and Anonymous Post-Trade Reporting

Table 1 reports the timeline of transparent/anonymous post-trade reporting changes for equities on various exchanges. Firms are assigned to one of 3 groups: TAA (Transparent-Anonymous-Anonymous), TAT (Transparent-Anonymous-Transparent), or TTT (Transparent-Transparent-Transparent). The 5 most traded stocks in Stockholm that switched to anonymity reporting on June 2, 2008 and then became transparent on Apr. 14, 2009 are Ericsson B, Volvo B, Telia Sonera, Nordea, and H&M B. The 5 most traded stocks in Helsinki that switched to anonymity on June 2, 2008 and remained in that group on Apr. 14, 2009 are Nokia, Fortum, UPM-Kymmene, Sampo, and Stora Enso.

Firms	Group	Before June 1, 2008	June 2, 2008 – Apr. 13, 2009	After Apr. 14, 2009
5 most traded in Helsinki	TAA	Transparent	Anonymous	Anonymous
Remaining stocks in Helsinki	TAT	Transparent	Anonymous	Transparent
5 most traded in Stockholm	TAT	Transparent	Anonymous	Transparent
Remaining stocks in Stockholm	TTT	Transparent	Transparent	Transparent
Copenhagen	TTT	Transparent	Transparent	Transparent

[7] A trading system developed by the Stockholm Stock Exchange and OMX that is still used by some exchanges in Asia and North America.

The motivation for the original changes given by the exchange management was that anonymous trading was believed to lower trading costs, reduce market impact, and increase competitiveness. Some proponents of the change cited the increased used of algorithms that are used to spot patterns in trading. It is worth noting, however, that this change takes place largely before the broad use of more sophisticated algorithms.[8] On the other hand, an argument given for transparency was that it leveled the playing field for smaller participants. Larger members could already observe a representative sample of counterparty broker codes from their own trades, but the public disclosure of broker codes was particularly helpful for smaller members who naturally participate in fewer trades.

Coincidentally there was a close membership vote that explains how the quasinatural experiment came to be. The vote split so that local members based in the Nordic countries were mostly against the proposal, and international members represented by the Investment Management Association in London were mostly in favor of it. The relative balance between these 2 groups explains how the changes that were implemented were structured. Specifically, the London based member firms have a larger market share in both Helsinki and Reykjavik, whereas the local members have a larger market share in Stockholm and even more so in Copenhagen.

Table A1 in the Supplementary Material reports the breakdown of member firms in groups according to Nordic exchange membership. For example, the HCS label refers to members of the Helsinki, Copenhagen, and Stockholm exchanges, with a total of 36 member firms in this group. Their market share based on turnover for the Jan.–Sept. 2008 period was just over 50%, and their market share based on the number of trades was around 56%. A total of 11 members belong to all four exchanges (Helsinki, Copenhagen, Stockholm, and Reykjavik), and they account for 26% of turnover and 22% of the number of trades. The third largest market share group are the 15 firms that are members of Helsinki and Stockholm and account for 15% of turnover and 23% of trades. Together these 52 firms account for the bulk of the trading activity across the four markets. While the smaller subgroups of firms that do not belong to HCS, HCSR or HS do not account for much of the total turnover and number of trades, they account for a bit more in terms of the local exchange turnover and trade figures. As one would expect, member firms who are based in London primarily show up in the first 3 groups. The high degree of integration (i.e. a single exchange operator, largely identical trading rules, largely overlapping exchange membership) make this a good setting for applying the difference-in-difference approach as detailed in Section IV.

B. Our Sample

Data on trades and the limit order book state is provided by Nasdaq Nordic market research. Since our ultimate goal is to construct a matched sample of firms that are similar except for anonymity/transparency, our first step is to impose a simple liquidity filter. In the third quarter of 2008 a number of the Icelandic index

[8]For example, the introduction of the INET trading system (an electronic trading platform based on system developed by Instinet in the 1970s that merged with Island in 2002 and was subsequently acquired by Nasdaq in 2005.), which was viewed as a major change toward a faster equity market trading system, occurred in Feb. 2010. See http://ir.nasdaq.com/releasedetail.cfm?releaseid=443390.

constituents became insolvent, were nationalized or opted to delist from the exchange; hence we do not include Icelandic stocks in our sample. Furthermore, to be included in our sample we require that each firm have at least 1 trade per day in a minimum of 50% of the days in our sample. We began with 751 firms and dropped 195 due to this filter, which yields 556 firms in Helsinki, Copenhagen, and Stockholm from Mar. 3, 2008 to July 10, 2009[9] that meet this trading volume criteria.

On June 2, 2008 all equities that trade in Helsinki as well as 5 equities that trade in Stockholm became post-trade anonymous, accounting for 132 firms in our sample. The remaining equities in Stockholm and all those in Copenhagen did not become post-trade anonymous, accounting for the remaining 424 firms. On Apr. 14, 2009 those firms that were post-trade anonymous, with the exception of 5 firms in Helsinki[10] switched back to being transparent. To exclude any confounding effects due to changes in the anonymity of trade reporting, we exclude observations in a 2-week window from May 25, 2008 to June 9, 2008 surrounding the switch to anonymity and exclude observations in a 2-week window from Apr. 8–21, 2009 surrounding the switch from anonymity.

Table 2 reports the number of firms in the control and treatment groups. Firms are assigned to one of 3 groups depending on their classification from Table 1. For example, in Table 2 the group "TAT" are those firms whose trades were transparent before June 2, 2008, anonymous from June 3, 2008 to Apr. 14, 2009 and transparent after Apr. 14, 2009. The treatment groups for those stocks that became anonymous on June 2, 2008 are "TAA" and "TAT"; the control group for this event is "TTT." The treatment group for those stocks that became transparent on Apr. 14, 2009 is "TAT"; the control group for this event is also "TTT."

TABLE 2

Grouping of Firms Sorted By Market Value of Equity (MVE) Terciles

Table 2 reports the breakdown of firms by treatment/control groups and market value of equity terciles. Firms are assigned to 1 of 3 groups. For example, the group "TAT" is composed of those firms that switched from transparency to anonymity on June 2, 2008, and then from anonymity to transparency on Apr. 14, 2009. The treatment groups for those stocks that switched to anonymity on June 2, 2008 are TAA (Transparent-Anonymous-Anonymous) and TAT (Transparent-Anonymous-Transparent); the control group for this event is TTT (Transparent-Transparent-Transparent). The treatment group for those stocks that switched to transparency on Apr. 14, 2009 is TAT; the control group for this event is also TTT.

Group	June 2, 2008	Apr. 14, 2009	All Firms	Number in Lowest MVE Tercile	Number in Middle MVE Tercile	Number in Highest MVE Tercile
TAA	Treatment group: switched from transparent to anonymous	Not used	5	0	0	5
TAT	Treatment group: switched from transparent to anonymous	Treatment group: switched from anonymous to transparent	127	39	39	49
TTT	Control group: remained transparent	Control group: remained transparent	424	147	146	131
Total			556	186	185	185

[9]Due to a data collection omission on the part of the exchange, we are missing order book data from July 14–18, 2008. The omission is technical and unrelated to market dynamics and should not bias our findings in any way.

[10]The 5 firms that did not switch were Nokia, Fortum, UPM-Kymmene, Sampo, and Stora Enso.

The unit of observation in our analysis is a firm-day, and we compute several variables for each firm, each day. Historical daily foreign exchange rates were obtained from the European Central Bank (https://www.ecb.europa.eu/stats/exchange/eurofxref/html/index.en.html) and merged with the Nasdaq Nordic data. These rates are used to convert prices in Swedish krona and Danish krone to euro. Daily euro volume for each firm is computed by summing the daily euro value of all trades.

We compute the spread as the difference between the inside bid and ask divided by the quote midpoint at 15-minute intervals from 09:00 to 17:00 using the quote closest to 0, 15, 30, and 45 minutes past the hour. We use these to construct an average daily spread.[11] We use the corresponding bid–ask midpoint at 15 minute intervals to construct the daily return standard deviation.[12] To compute the daily market value of equity, the prices for each firm are sampled every 15 minutes to compute an average daily share price. Shares outstanding for each firm are obtained from Nasdaq Nordic (http://www.nasdaqomx.com/transactions/markets/nordic/statistics), and the number of shares outstanding is multiplied by the firm's average daily price to compute the firm's market value of equity in euros.

We also measure broker concentration by constructing a Herfindahl index based on trade volume. The daily volume (defined as the sum of the shares bought and sold) in a firm by each broker is divided by the sum of the daily volume in that firm for all brokers. The sum of squares of this fraction across all N brokers trading a firm's shares represents a Herfindahl index of trading activity ranging from $1/N$ (each broker trades an equal volume of shares for a given firm) to 1.0 (one broker has all the volume in a given firm and the others have 0).

Table 3 reports the mean, median, and standard deviation for the daily values of both spreads and control variables across all firms (all), treatment firms (trt), and potential control firms (ctl) before the matching procedure. The sample is split across the terciles based on market capitalization. Panel A reports results for the switch to anonymity during the period from Mar. 3, 2008 to Sept. 26, 2008 and Panel B reports results for the switch back to transparency. During the period from Mar. 3, 2008 to Sept. 26, 2008 the unconditional mean (median) spread for all firm days in the entire sample is 226 (138) bps. The average spread is high due to the large spread for firms with small market capitalization; the mean (median) spread for the firms in the largest market capitalization tercile is 87 (40) bps, whereas it is 209 (149) bps for the middle tercile and 383 (261) bps for the smallest tercile of all firms. The second and third row in each subpanel of Table 3 reports the means,

[11]The Helsinki, Copenhagen, and Stockholm exchanges are in different timezones and trading stops and starts at slightly different times (see http://www.nasdaqomxnordic.com/tradinghours for details). Since our unit of observation is a firm-day this presents no special obstacle for the purposes of computing the daily observations of liquidity measures such as spread, price impact, etc.

[12]15-minute returns are computed as $r_t = \frac{m_t - m_{t-1}}{m_{t-1}}$ where m_t is the inside quote midpoint at time t and the daily return volatility is

$$\sigma = \sqrt{\frac{1}{T-1}\sum_{t=1}^{T}(r_t - \bar{r})^2}$$

where $\bar{r}$ is the mean daily return for T observations.

TABLE 3
Descriptive Statistics

Table 3 reports the means, medians, and standard deviations for firm-day observations for the full sample and the sample sorted by market value of equity (MVE) terciles before the matching procedure. Statistics are shown for spreads (bps), firm size (market value of equity in millions of euro), daily return volatility (σ, b.p.), share price (euro), broker concentration (conc., as a Herfindahl index), and daily trading volume (thousands of euro). The 15-minute returns are computed as $r_t = \frac{m_t - m_{t-1}}{m_{t-1}}$, where m_t is the inside quote midpoint at time t and the daily return volatility is $\sigma = \sqrt{\frac{1}{T-1}\sum_{t=1}^{T}(r_t - \bar{r})^2}$ where $\bar{r}$ is the mean daily return for T observations. The treatment group (trt) in Panel A compromises those firms whose trading switched from transparency to anonymity on June 2, 2008 (i.e. groups TAA and TAT in Table 1). The treatment firms (trt) in Panel B compromises those whose trading switched from anonymity to transparency on Apr. 14, 2009 (i.e. group TAT in Table 1). The potential control firms (ctl) in both panels compromises those that did not have a change in trading transparency.

Panel A. Descriptive Statistics for the Switch To Anonymity (Mar. 3, 2008–Sept. 26, 2008)

	Full Sample			Lowest MVE			Middle MVE			Highest MVE		
	Mean	Med.	Std. Dev.	Mean	Med.	Std. Dev.	Mean	Med.	Std. Dev.	Mean	Med.	Std. Dev.
spread - all	226	138	305	383	261	391	209	149	221	87	40	185
spread - trt	209	121	298	409	261	420	211	153	199	62	26	99
spread - ctl	231	143	307	376	261	383	208	148	227	98	45	210
size - all	1,472	165	5,458	40	37	26	204	163	126	4,158	1,326	8,846
size - trt	2,460	186	8,028	39	36	24	173	150	89	5,882	1,218	11,764
size - ctl	1,158	159	4,286	41	37	27	212	171	133	3,440	1,365	7,175
σ - all	93	52	148	128	71	199	89	55	139	62	43	73
σ - trt	46	37	47	56	39	68	45	34	44	41	37	23
σ - ctl	107	60	165	148	86	217	101	62	153	71	46	84
price - all	46.65	8.20	436.46	17.83	2.74	123.21	19.09	7.34	47.15	98.27	13.80	721.24
price - trt	10.35	7.82	9.64	2.96	1.50	3.33	8.91	6.96	7.21	16.75	14.31	9.99
price - ctl	59.41	8.37	506.71	22.81	3.10	142.01	22.06	7.54	53.09	133.22	13.43	859.73
conc - all	0.226	0.197	0.134	0.322	0.307	0.115	0.244	0.216	0.118	0.130	0.096	0.091
conc - trt	0.227	0.202	0.140	0.347	0.343	0.108	0.271	0.253	0.117	0.122	0.089	0.086
conc - ctl	0.225	0.195	0.131	0.314	0.296	0.116	0.236	0.206	0.118	0.133	0.098	0.092
volume - all	7,374	96	36,693	50	13	209	426	77	1,323	20,174	4,299	59,230
volume - trt	14,059	85	62,780	44	8	259	307	40	1,152	32,510	5,648	92,646
volume - ctl	5,161	100	21,629	51	14	191	458	95	1,363	14,923	3,980	35,442

Panel B. Descriptive Statistics for the Switch from Anonymity (Jan. 13, 2009–July 10, 2009)

	Full Sample			Lowest MVE			Middle MVE			Highest MVE		
	Mean	Med.	Std. Dev.	Mean	Med.	Std. Dev.	Mean	Med.	Std. Dev.	Mean	Med.	Std. Dev.
spread - all	283	166	411	494	329	519	258	176	346	96	44	193
spread - trt	283	160	434	550	340	634	271	199	267	81	38	120
spread - ctl	283	167	404	479	326	482	255	170	365	101	45	213
size - all	935	93	3,382	22	20	15	116	93	72	2,665	846	5,458
size - trt	1,198	126	3,989	27	22	16	113	94	55	2,995	670	6,000
size - ctl	856	88	3,171	21	19	14	117	93	76	2,544	918	5,241
σ - all	80	54	115	109	69	157	77	56	110	54	46	43
σ - trt	65	45	113	90	54	183	61	44	77	48	43	28
σ - ctl	84	56	116	114	74	148	81	58	117	56	47	47
price - all	25.65	4.40	246.76	8.26	1.37	59.20	10.30	4.11	32.91	56.53	8.09	412.62
price - trt	6.49	4.23	6.65	1.85	1.04	2.05	6.15	4.02	5.81	10.45	8.91	7.18
price - ctl	31.81	4.44	283.37	10.25	1.56	67.63	11.47	4.16	37.06	73.68	7.85	482.21
conc - all	0.231	0.207	0.134	0.322	0.311	0.111	0.253	0.226	0.119	0.130	0.095	0.092
conc - trt	0.238	0.222	0.140	0.342	0.334	0.105	0.290	0.276	0.119	0.130	0.095	0.090
conc - ctl	0.229	0.203	0.132	0.317	0.303	0.112	0.244	0.213	0.117	0.131	0.095	0.092
volume - all	5,008	57	21,918	37	7	183	300	49	1,253	13,960	2,556	35,285
volume - trt	5,799	46	21,656	26	7	107	174	22	598	14,047	2,116	32,134
volume - ctl	4,769	60	21,990	40	7	199	333	60	1,371	13,928	2,667	36,381

medians, and standard deviations for the treatment and potential control firms for the small, middle, and the highest market capitalization (MVE) subgroups. Below the spread figures we report the corresponding statistics for market value of equity in million of euro (size), return volatility in basis points (σ), share price in euro, broker concentration as a Herfindahl index (conc.), and trading volume in thousands of euro. The statistics in Panel B covering the period from Jan. 13, 2009 to July 10, 2009 are the same order of magnitude as in Panel A, however since this period is post-crisis, market capitalizations and trading volume are lower while overall spreads are higher. Table 4 reports the correlations between our control variables for all 556 firms in the sample during the period from Mar. 3, 2008 to July 10, 2009, and, as one would expect, larger firms have higher trading volume, lower bid–ask spreads and lower return volatility.

TABLE 4
Correlations Between Spread and Covariates

Table 4 reports the correlations of the daily bid–ask spread and covariates used for propensity score matching: Log market value of equity in millions of euro (ln(size)), return volatility in basis points (σ), log price in euro (ln(price)), and broker concentration (conc., as a Herfindahl index). For all 556 firms in the sample during the period from Mar. 3, 2008 to July 10, 2009. The 15-minute returns are computed as $r_t = \frac{m_t - m_{t-1}}{m_{t-1}}$, where m_t is the inside quote midpoint at time t and the daily return volatility is $\sigma = \sqrt{\frac{1}{T-1}\sum_{t=1}^{T}(r_t - \bar{r})^2}$ where $\bar{r}$ is the mean daily return for T observations.

	Spread	Log Market Value Equity (ln(size))	Return Volatility (σ)	Log Price (ln(price))
Log market value equity (ln(size))	−0.47			
Return volatility (σ)	0.48	−0.22		
Log price (ln(price))	−0.31	0.57	−0.13	
Broker concentration (conc)	0.48	−0.62	0.14	−0.18

IV. Methodology

A natural way to investigate the impact of anonymity is a difference-in-difference approach: Examine the difference in spreads between firms that became anonymous and those that did not, both before and after the date that anonymous trading took effect. This has the advantage of isolating the change in trade reporting from any confounding events. However, we are still left with a problem, while we have a quasinatural experiment, the propensity of a firm being assigned to the treatment group rather than the control group is not necessarily random.

For example, the statistics in Table 3 suggest that large firms and firms with lower return volatility have a higher propensity to be in the treatment group. Table 4 shows that spreads are correlated with firm size and return volatility, hence simply examining the results of a difference-in-difference between the 2 groups will not represent the average treatment effect. Controlling for firm size in the context of a regression is not necessarily straightforward. For example, when we plotted the average daily spread in basis points vs. the natural log of the average daily firm size, the plot shows that smaller firms have higher spreads, but the relationship is not loglinear. Thus, using the natural logarithm of the market value of equity as a control variable in a linear regression is not a perfect solution to control for the effect of firm size on spreads.

More worrisome, however, is the fact that anonymity is not randomly assigned across firms. One aspect that contributes to this is that all firms in Helsinki and the top 5 stocks in Stockholm switched to anonymity whereas none of the stocks in Copenhagen switched. In our sample, firms that became anonymous tend to have a larger size and smaller spreads compared to the control group that remained transparent. A t-test of difference in means could lead to false conclusions since spreads depend on firm size and firm size is correlated with the propensity of a firm to be in the treatment group.

A. Propensity Score Matching

To overcome these problems we employ propensity score matching (PSM) to match a control firm to each treatment firm.[13] We also chose PSM over a regression approach with control variables such as firm size, return volatility, etc., since PSM avoids having to impose a linear functional form on the relationship between measures of liquidity and the control variables. Rosenbaum and Rubin (1983) show that if it is valid to match on characteristics such as firm size and return volatility, then it is equally valid to match on propensity scores.

Each variable in Table 3 is observed daily for each firm. To match treatment and control firms using PSM, we use the firm-level timeseries mean of each of the following variables before the change to anonymous trading was made on June 2, 2008: the natural log of firm size, the natural log of the share price, return volatility, broker concentration, and the average spread in Mar. 2008.

We include firm size and share price since they are known to be positively correlated with spread, and include return volatility since it is negatively correlated with spread (Harris (1994)). As the statistics in Panel A of Table 3 show, the covariate balance between the treatment and control groups is relatively reasonable with the exception of price. While the median share prices in the treatment and control groups are comparable, the mean share price of the control group is roughly 6 times larger than that of the treatment group, indicating large outliers. PSM will try to balance all 5 covariates, resulting in some low-priced treatment firms possibly being matched to high-priced control firms. To avoid this, we drop 37 control firms that have share prices above 70 euro before we perform PSM, which represents a balance between having a large number of possible control firms available for matching yet eliminating outliers.[14]

We include broker concentration for a couple of reasons. In the limit where all order flow in a particular stock is concentrated with one broker, a trader would be in a better position to glean information from that order flow and perhaps try to anticipate other trader's orders. This would be more difficult if orders were evenly distributed across multiple brokers, hence switching to anonymity may have a larger impact on liquidity for more concentrated order flow. Alternatively, if order flow was divided across multiple brokers, transparency may result in collusive behavior (for an example in a different market setting see Simaan, Weaver, and Whitcomb (2003)). This would be less of an issue if one broker handled all or most of the order flow in a particular stock, hence switching to anonymity may have a smaller impact on liquidity for more concentrated order flow. We include the spread in Mar. 2008 as the last control variate. This may capture differences between the treatment and control groups that are not accounted for by other control variates. We are careful to use the spread in Mar. 2008 as a control variate, while our difference-in-difference analysis will use daily observations of the

[13] See Imbens (2004) for a good overview.

[14] We chose 70 euro since that was the highest average price of stocks in the treatment group over the period from Apr. 2, 2008 to June 2, 2008.

spread beginning on Apr. 2, 2008. Hence we do not introduce bias by matching on a variable that we will be using to assess the effect of treatment assignment (Imbens (2004) and Stuart and Rubin (2008), Chapter 11).

The observation that a firm is in the control or treatment group (0 or 1 respectively) is logistically regressed on the 5 control variates outlined above. The fitted values from this logistic regression are the propensity scores that are used to match each treatment firm with a control firm.

B. Industry Matching

Firms in the same industry may share characteristics that are not necessarily captured by the control variates outlined above. One possibility is that firms in different industries attract traders of different types (e.g. long-term investors vs. quantitative hedge funds). While we cannot observe these characteristics, to improve the reliability of the match we require both the treatment and control firm to be in the same industry: It is better to assess the treatment effect by comparing 2 firms in the financial sector than a firm in the financial sector and one in the energy sector. Table 5 summarizes the distribution of our sample by industry. Note that the distribution of industries varies between the treatment and control groups. For example, while 11.4% of the firms in the treatment group are in the financial sector, the proportion of financial firms that could serve as potential control firms is more than double that of the treatment group (26.4%). Also note in Table 5 that there are generally 2–3 times more potential control firms in each industry than treatment firms. For example, we have 15 treated firms in the financial sector, but 112 possible control firms to choose a match from, which helps in achieving close matches.

TABLE 5
Number of Firms in Each Sector

Table 5 reports the number and percentage of firms in each sector of the treatment and potential control groups.

	Treatment Firms		Control Firms		Total	
Sector	Number	Percent	Number	Percent	Number	Percent
Consumer discretionary	18	13.6	47	11.1	65	11.7
Consumer staples	8	6.1	14	3.3	22	4.0
Energy	1	0.8	8	1.9	9	1.6
Financials	15	11.4	112	26.4	127	22.8
Health care	8	6.1	44	10.4	52	9.4
Industrials	37	28.0	103	24.3	140	25.2
Information technology	29	22.0	66	15.6	95	17.1
Materials	12	9.1	23	5.4	35	6.3
Telecommunication services	3	2.3	6	1.4	9	1.6
Utilities	1	0.8	1	0.2	2	0.4
Total	132	100.0	424	100.0	556	100.0

C. Matching and Assessment

We match each treatment firm to a control firm in the same industry that has the closest propensity score to the treatment firm. Requiring an industry match means that the other covariates that we match on (price, return volatility, size, and broker concentration) will not necessarily be as close as if we matched without the industry constraint. However matching on industry is advantageous since it accounts for industry effects that may affect treatment and control firms in the same fashion.[15]

Propensity score matching was done using the values of the covariates before the switch to anonymity on June 2, 2008. These matches were kept the same throughout the switch to anonymity on June 2, 2008 and the switch back to transparency on Apr. 14, 2009. Hence, the results across the 2 dates are directly comparable in the sense that the same matched-pair of firms is being used for both events.

In order to draw correct inferences using PSM, we have to have conditional independence; that is, after accounting for our control variables of size, return volatility, share price, and concentration, potential outcomes (a decrease in spread for example) would be the same for observations in the treatment group as the control group if the firms in the treatment group did not trade anonymously. Size, return volatility, and share price should capture any variation in spreads not due to anonymous trading (Harris (1994)). Also, the probability densities of the propensity scores have to overlap: For each firm in the treatment sample, there should be a high likelihood that we will be able to find a closely-matched firm from the control sample. We have verified that the overlap assumption is met in our specification.

All tables from this point on in the paper use the matched treatment–control sample. From Table 2, we have $5+127=132$ treatment firms that need matching control firms, and we were able to match 129 of them to a control firm.[16] Similarly, we have 127 treatment firms for 2009, but 2 of those were in the 3 firms that were dropped in 2008, leaving 125 matched treatment firms that switched to transparency in 2009. After the matching was complete, we examined how close the match is by tabulating the values of the covariates for the period before the switch to anonymity in 2008 (from Apr. 1, 2008 to May 23, 2008). The mean values as well as key percentiles in the distribution are shown in Panel A of Table 6. Recall that PSM is a logistic regression, hence we will not achieve an exact match on a covariate-by-covariate basis, but rather PSM results in a match as if the treatment were assigned randomly. From Table 6, we see that the distribution of pre-event spreads and broker concentrations are quite close. The treatment group tends to have slightly higher market capitalizations and slightly lower prices and return volatility, but overall the standard deviations for each of the covariates is an order of magnitude larger than the difference in mean or median values.

We keep the same match for both the 2008 switch to anonymity and the 2009 switch back to transparency. While this makes the 2 events directly comparable, we want to make sure that the characteristics of the treatment and control group have not changed from the time the match was formed using covariates between Apr. 1, 2008 and May 23, 2008, to the switch back to transparency in 2009. Panel B of Table 6 shows that the set of treatment and control firms is still roughly comparable; for example the average size a treatment firm is 1,104 million euro compared to 1,187 million euro for the group of control firms. In sum, the summary statistics in Table 6 reassure us the matches are reasonable.

[15]When we drop the requirement for industry match our results are qualitatively similar.

[16]We dropped firms that had propensity scores outside of [0, 1].

TABLE 6
Covariate Match

Table 6 reports the distribution of covariates for both the treatment (trt) and matched control (ctl) firms for the period before the 2008 switch to anonymity, Apr. 1, 2008–May 23, 2008 (Panel A, $N=129$), and the period before the 2009 switch from anonymity, Jan. 15, 2009–April. 7, 2009 (Panel B, $N=125$). Statistics are shown for spreads (bps), firm size (market value of equity in millions of euro), daily return volatility (σ, bps), share price (euro), and broker concentration (conc., as a Herfindahl index). The 15-minute returns are computed as $r_t=\frac{m_t-m_{t-1}}{m_{t-1}}$, where m_t is the inside quote midpoint at time t and the daily return volatility is $\sigma=\sqrt{\frac{1}{T-1}\sum_{t=1}^{T}(r_t-\bar{r})^2}$, where $\bar{r}$ is the mean daily return for T observations.

	Mean	5%	25%	Med.	75%	95%	σ
Panel A. Covariate Match (Apr. 1, 2008–May 23, 2008)							
spread - trt	154	13	30	121	187	461	157
spread - ctl	157	22	32	157	221	424	135
size - trt	2,610	16	79	212	1,005	14,425	8,526
size - ctl	2,332	13	22	130	2,250	8,084	4,701
σ - trt	40	24	30	35	45	69	18
σ - ctl	48	29	39	46	53	73	13
price - trt	11.30	0.74	2.64	8.64	16.13	30.63	10.39
price - ctl	12.94	3.02	4.03	9.90	12.73	44.58	12.18
conc - trt	0.23	0.07	0.10	0.24	0.33	0.45	0.13
conc - ctl	0.24	0.07	0.10	0.26	0.38	0.39	0.13
Panel B. Covariate Match (Jan. 15, 2009–Apr. 7, 2009)							
spread - trt	271	21	81	215	358	769	264
spread - ctl	378	21	41	305	633	996	339
size - trt	1,104	14	41	123	410	3,617	3,657
size - ctl	1,187	6	7	49	1,228	6,927	2,266
σ - trt	77	35	46	62	81	162	60
σ - ctl	102	36	47	87	153	214	64
price - trt	6.16	0.35	1.58	4.10	8.98	19.00	6.23
price - ctl	7.04	1.21	1.42	2.94	6.70	27.34	8.32
conc - trt	0.26	0.06	0.13	0.28	0.36	0.44	0.13
conc - ctl	0.27	0.07	0.10	0.34	0.39	0.42	0.14

V. Results

In this section we present our results which, taken as a whole, show that anonymity improves liquidity. Specifically, we will discuss the effect of anonymity on the bid–ask spread in Section V.A, on the price impact of trades in Section V.B, and on the book depth in Section V.C. We briefly discuss robustness in Section V.D and differences between 2008 and 2009 in Section V.E.

A. Quoted Bid–Ask Spreads

As explained in Section IV, we use propensity score matching to match each treatment firm to a control firm. Our unit of observation is a firm-day. Before computing the test statistics below, we winsorize the spread for the treatment and control groups at 2.5/97.5%. The direction and statistical significance of our results are unchanged using the nonwinsorized data (we present the nonwinsorized results in Table A2 of the Supplementary Material). However due to the presence of outliers, winsorizing makes the economic effect of anonymity on treatment and control firms more directly comparable.

Each day we compute the difference in spread between the treatment and control firm, and then test if the mean difference before and after the switch to anonymity on June 2, 2008 is statistically significant. Spreads could be correlated across different firms on the same day and for the same firm on sequential days.

Since we are using a difference-in-difference with treatment and control firms from the same sector, this helps to mitigate both cross-sectional and timeseries correlation since any common shock should affect the treatment and control firms in a similar fashion. This may not, however, eliminate all correlation since we cannot match treatment and control exactly on all characteristics (e.g. a bank could be matched with an insurance company). Since what we have is a panel data set, we use the method outlined in Petersen (2009) to adjust the standard errors of the t-statistics so that they are unbiased. Specifically, we compute the standard errors by summing the variance-covariance of residuals clustered by firm with that of the variance-covariance matrix clustered by time. Since both the firm- and time-clustered variance-covariance matrices include the diagonal of the variance-covariance matrix, we then subtract the variance-covariance matrix computed using robust standard errors to avoid double-counting.[17]

Panel A of Table 7 contains the mean spread before and after the event date for those firms that became anonymous on June 2, 2008 (the treatment group) and those firms that did not become anonymous (the control group) for both the full sample and the lowest, middle, and highest market capitalization terciles. The before and after figures are reported for both the treatment and control groups. The difference-in-differences are reported as the rightmost number in the column labeled "Difference $(a-b)$." The bottom row of each subpanel reports the number of observations and the t-statistic for the difference-in-difference, corrected for cross-sectional and serial correlation as previously outlined. The statistics in Table 7 are computed using daily observations from Apr. 1, 2008 to Aug. 31, 2008. A concern with this sample period is that the onset of the financial crisis may have impacted the results even though the quasinatural experiment setting should control for any effect that the crisis had on spreads. To minimize the impact of the crisis on our results, we exclude observations on and after Sept. 1, 2008 from our analysis.[18] We exclude observations before Apr. 1, 2008 since we use the spread in Mar. 2008 as a covariate for matching. The number of observations, N, is the total number of firm-days in each group from Apr. to Aug. 2008.

We find that for the full sample, the difference-in-difference in spreads is a statistically significant -76 bps. While the average spread for the treatment group increased by $167-131 = 36$ bps, the spread for the control group increased by an even higher $238-126 = 112$ bps. Broken down by size terciles, the difference-in-difference ranges from -109 bps for small firms to -55 bps for large firms, where both the economic and statistical significance of anonymity on spreads is directly related to the size of the firm. In all 3 size terciles, the average quoted spreads widen after the switch to anonymity on June 2, 2008, however they widened less for the treatment firms and, hence, this dimension of liquidity improves under anonymity. These results also illustrate the importance of having a good control sample; looking only at the treated sample, one would incorrectly conclude that post-trade anonymity did not enhance liquidity. Since our treatment and control firms trade in different currencies, one concern is that if the tick interval is not

[17]See Petersen ((2009), p. 458, eq. 16).

[18]When we include observations from Sept. 2008 the statistical significance of the results in Table 7 increases.

TABLE 7

Spreads Before/After Changes in Trading Transparency

Table 7 reports the average daily quoted spread in basis points, sampled at 15-minute intervals, of those firms that switched to anonymity on June 2, 2008 (Panel A) and those firms that switched back to transparency on Apr. 14, 2009 (Panel B). The firms that switched are considered the treatment firms. Each treatment firm on June 2, 2008 is matched with a control firm based on propensity scores. N denotes the sample size. t-statistics are shown in parentheses in the rightmost column and correspond to a difference-in-difference test that the difference in spreads between treatment and control firms is the same before and after anonymity. Standard errors used to compute the t-statistics are clustered by both firm and time as in Petersen (2009). Statistical significance at the 1% (5%) level is indicated by ** (*).

	Treatment Firms (t)	Control Firms (c)	Difference ($t-c$)
Panel A. Change to Anonymity On June 2, 2008			
All firms - before (b)	131	126	5
All firms - after (a)	167	238	−71
Difference ($a-b$)	36	112	−76**
N	7,661	7,661	(−7.32)
Small firms - before (b)	265	182	83
Small firms - after (a)	329	355	−25
Difference ($a-b$)	64	173	−109**
N	1,855	1,855	(−5.11)
Medium firms - before (b)	153	131	22
Medium firms - after (a)	201	264	−63
Difference ($a-b$)	48	132	−84**
N	2,220	2,220	(−5.51)
Large firms - before (b)	49	94	−45
Large firms - after (a)	61	161	−100
Difference ($a-b$)	12	67	−55**
N	3,586	3,586	(−9.09)
Panel B. Change from Anonymity on Apr. 14, 2009			
All firms - before (b)	236	317	−81
All firms - after (a)	176	244	−69
Difference ($a-b$)	−61	−73	12
N	11,552	11,552	(1.06)
Small firms - before (b)	445	456	−11
Small firms - after (a)	336	361	−25
Difference ($a-b$)	−109	−95	−14
N	3,025	3,025	(−0.61)
Medium firms - before (b)	279	344	−65
Medium firms - after (a)	203	268	−65
Difference ($a-b$)	−76	−76	0
N	3,459	3,459	(0.02)
Large firms - before (b)	79	214	−135
Large firms - after (a)	64	161	−96
Difference ($a-b$)	−14	−53	39**
N	5,068	5,068	(5.91)

proportional to the relative exchange rate, spurious bid/ask differences may be induced.[19] However, since we use a difference-in-difference measure, any component of the bid–ask spread induced by different exchange rates should not bias our results.

While Rosenbaum and Rubin (1983) show that matching on propensity scores is equivalent to matching on characteristics such as firm size, share price, return volatility, and broker concentration, one may still be concerned that there is something different about the control sample that is driving the results in Panel A of Table 7. Any empirical study faces this concern when matching a treatment to a control sample, but the Nasdaq Nordic experiment is unique since there was a switch to anonymity and then, 1 year later, a switch from anonymity. By observing these two changes for the same group of firms through time in essence

[19]For example, in June 2008 the Danish krone-to-euro exchange rate was roughly 7.5 to 1.

we are controlling for firm fixed effects. Panel B in Table 7 reports the difference-in-difference of the spread when all trades became post-trade transparent on Apr. 14, 2009. For those firms that switched from anonymous to transparent, reporting spreads widened by 12 bps, though it is not statistically significant. Broken down by market capitalization, the largest change is for the largest firms. The spread for those firms in the top tercile that switch from anonymous to transparent reporting increased a statistically significant 39 bps, whereas the change for the small and medium sized firms is not statistically significant. After the change back to transparency in 2009 the spreads are generally wider than they were after the change to anonymity in 2008, which reflects the changes in liquidity around the 2008 financial crisis. In sum, the evidence in Table 7 indicates that post-trade anonymity is associated with lower spreads.

We also find that our results for the switch to anonymity in 2008 are robust if we use volume-weighted spreads instead of the quoted spread sampled every 15 minutes. To compute volume-weighted spread we match every trade to the current quote. We compute the percentage spread as the inside ask minus the inside bid price divided by the quote midpoint and then multiply the spread by the size of the trade in shares. For each firm, we sum these and then divide by the total shares traded on that day. The results are presented in Table A3 of the Supplementary Material. We find that the change to anonymity on June 2, 2008 caused a drop in the average volume-weighted spread in the full sample of 54 bps, which is significant at the 1% level. As with the equally-weighted spread, the economic magnitude of the change in spread decreases with firm size while the associated statistical significance increases. However, when the switch back to transparency was made on Apr. 14, 2009, volume-weighted spreads in the full sample decreased by a statistically insignificant 10 bps and results by size tercile were mixed.

Discussion of Effect on Spreads

Our experimental setup is less than perfect in that we do not truly randomly select firms that become anonymous, and, due to this, concerns arise about the timeseries pattern we observe and concerns about market segmentation. First, in the timeseries, the spreads reported in Table 7 generally increase after June 2, 2008, but they increased less for the stocks that switched to anonymity. Likewise, spreads generally declined after Apr. 14, 2009 for the largest firms, but they decreased less for the stocks that switched to transparency. Recall that this period overlaps the financial crisis of 2008 and, similar to the United States, overall spreads in the three Nordic markets increased during the latter half of 2008 and then decreased during 2009 (see Table A4 in the Supplementary Material). Hence, the timeseries pattern of spreads we observe is largely consistent with the financial crisis. The fact that we observe spreads for the treatment group relative to the control group first declining after the switch to anonymity and subsequently increasing for the largest firms after the switch to transparency is somewhat reassuring in that the effect we observe is due to the switch to anonymous post-trade reporting and not the crisis.

Second, while all three exchanges are part of Nasdaq Nordic, there is some degree of market segmentation, primarily due to different currencies and languages.[20] However, there are several factors that mitigate this. First, all three countries neighbor each other and investors have a bias for stocks in firms that are geographically close (Coval and Moskowitz (1999)). Second, all three exchanges have the same operator with largely identical trading rules; for example, trading hours are aligned despite time zone differences. Third, exchange member firms that represent the bulk of all trading are members of all three exchanges.[21] Finally, as with concerns about the timeseries, liquidity improves when stocks switched to anonymity in June 2008 and then deteriorates when stocks switched back to transparency in Apr. 2009, mitigating concerns about the possible effect of market segmentation.

B. Price Impact

To compute the price impact of a trade, we only use trades that occur after 10:00 a.m. but before 4:25 p.m. and we drop any quotes that have a bid or ask price less than or equal to 0 or that have bid–ask spread greater than 50% of the quote midpoint. Each trade is matched with the midpoint of the immediately preceding quote, m_t. The trade is also matched with the soonest bid–ask midpoint that is at least 5 minutes after the trade occurs, m_{t+5}, and the price impact for that particular trade is computed as the percentage change of quote midpoints, $(m_{t+5} - m_t)/m_t$. If no quote can be found within 30 minutes of a trade then the trade is not used for computing price impact.

For each firm, the median trade size for all days in the sample window is calculated. Then for each firm-day, large trades, defined as those above the median trade size for a particular firm are separated from small trades, defined as those that are below the median for that same firm. Also, trades that are buyer-initiated (occur at the ask) are separated from those that are seller-initiated (occur at the bid). This results in 4 categories of trades, large buys (LB), small buys (SB), large sells (LS), and small sells (SS) whose price impacts are averaged for each firm on each day.

Panel A of Table 8 reports the average price impact during switch to anonymity on June 2, 2008 for the treatment and control sample broken down by time (before and after), buys vs. sells, and size (large vs. small). The sample size, N, is the total number of firm-days in that group. Compared to the spreads in Table 7, the sample sizes here are smaller for two reasons. First, there has to be at least 1 trade during the day to compute the price impact, for days that have quotes but no trades we can compute a spread but not a price impact. Second, all trades in a given day may be of one type (e.g. buys above the median size). In this case, that firm-day observation will appear in only 1 of the 4 groups (LB, SB, LS, SS) and not the others. We compute the t-statistics in the same fashion as those for the spreads in Table 7; namely we use clustered standard errors (Petersen (2009)), drop Sept. 2008, and winsorize at 2.5/97.5%.

The difference-in-difference figures are reported in the last 4 columns in Panel A of Table 8. The difference-in-difference for large (small) buys is a

[20]However, to an English speaking trader in London all three markets may look roughly equivalent.

[21]From Table A1 of the Supplementary Material, 47 brokers are common members of Helsinki, Copenhagen, and Stockholm and account for 77% of the aggregate volume in euro.

TABLE 8

Price Impact of Trades During Switch to Anonymity on June 2, 2008

Table 8 reports the average daily price impact in basis points of buyer and seller initiated trades for those firms that switched to anonymity on June 2, 2008 (the treatment group) and those firms that did not switch (the control group) before and after the event date. Each treatment firm is matched with a control firm based on their respective propensity scores. LB and SB refer to large buys (above the daily median trade size for a firm) and small buys, respectively. LS and SS are defined similarly for large and small sells, respectively. *N* denotes the sample size. *t*-statistics are shown in parentheses in the four rightmost columns and correspond to a difference-in-difference test that the price impact difference between treatment and control firms is the same before and after the switch to anonymity. Standard errors used to compute the *t*-statistics are clustered by both firm and time as in Petersen (2009). Statistical significance at the 1% (5%) level is indicated by ** (*).

	Transparent to Anonymous (treatment, *t*)				Remained Transparent (control, *c*)				Difference ($t-c$)			
	LB	SB	LS	SS	LB	SB	LS	SS	LB	SB	LS	SS
Panel A. All Firms												
Before (*b*)	7.8	5.6	−6.4	−5.7	9.6	14.0	−7.0	−8.5	−1.8	−8.4	0.5	2.8
After (*a*)	11.7	7.9	−8.0	−7.3	27.7	36.4	−14.2	−10.2	−16.0	−28.5	6.2	2.9
($a-b$)	3.9	2.3	−1.6	−1.6	18.1	22.4	−7.3	−1.7	−14.2**	−20.1*	5.7*	0.1
N	4,938	4,070	4,880	3,914	4,938	4,070	4,880	3,914	(−3.20)	(−2.28)	(1.89)	(0.02)
Panel B. Small Capitalization Firms												
Before (*b*)	25.1	18.8	−14.1	−17.9	15.1	28.3	−7.3	−1.9	15.5	−9.5	−6.8	−16.0
After (*a*)	29.1	26.3	−15.1	−21.2	56.4	72.5	−35.8	−20.0	1.5	−46.2	20.7	−1.2
($a-b$)	4.0	7.5	−1.0	−3.3	41.3	44.2	−28.5	−18.2	−37.3*	−36.8	27.6*	14.8
N	670	471	655	401	670	471	655	401	(−2.34)	(−1.27)	(2.40)	(0.93)
Panel C. Medium Capitalization Firms												
Before (*b*)	8.1	6.2	−7.7	−6.8	11.0	15.7	−4.5	−9.0	−1.5	−9.5	−3.2	2.2
After (*a*)	16.4	9.6	−9.6	−11.2	34.8	53.0	−13.5	−12.9	−11.2	−43.4	3.9	1.8
($a-b$)	8.3	3.4	−1.9	−4.4	23.8	37.3	−9.0	−3.9	−15.5*	−33.9*	7.0	−0.4
N	1,299	1,062	1,293	1,038	1,299	1,062	1,293	1,038	(−2.24)	(−2.57)	(1.51)	(−0.08)
Panel D. Large Capitalization Firms												
Before (*b*)	3.8	2.5	−3.9	−2.8	7.7	10.1	−8.0	−9.7	−5.8	−7.6	4.1	6.8
After (*a*)	5.7	4.4	−5.9	−4.1	18.1	24.4	−10.3	−8.0	−21.9	−20.0	4.4	3.8
($a-b$)	2.0	1.9	−2.0	−1.3	10.4	14.2	−2.3	1.7	−8.4**	−12.4*	0.3	−3.0*
N	2,969	2,537	2,932	2,475	2,969	2,537	2,932	2,475	(−3.24)	(−2.40)	(0.15)	(−2.10)

statistically significant −14.2 (−20.1) bps, indicating that buys for those firms that switched to anonymity increased the price less than for those firms that remained transparent. The difference-in-difference for large sells is 5.7 bps, indicating that sells for those firms that switched to anonymity decreased the price less than for those firms that remained transparent. The positive difference-in-difference means that prices were, on average, higher after the trade for sales of shares of anonymous firms. The difference-in-difference for small sells is not significant. Panels B, C, and D breakdown price impacts for small, medium, and large firms, respectively. As firm size increases, the magnitude of the price impacts decreases. With the exception of small sells for large firms, the sign of the difference-in-difference in price impact is robust across subsamples: Buys in the treatment group have a lower increase and sells have a smaller decrease, though in some cases it is not significant.

Table 9 reports the average price impact during the switch from anonymity (to transparency) on Apr. 14, 2009. The setup of the table parallels that of Table 8. The price impact results in this table are puzzling since they indicate that firms that switch back to transparency have a better outcome in terms of price impact (i.e. lower for buys and higher for sells). As with Table 8, Panels B, C, and D of Table 9 break down price impacts for small, medium, and large firms, respectively. As one

TABLE 9

Price Impact of Trades During Switch to Transparency on Apr. 14, 2009

Table 9 reports the average daily price impact in basis points of buyer and seller initiated trades for those firms that switched to transparency on Apr. 14, 2009 (the treatment group) and those firms that did not switch (the control group) before and after the event date. Each treatment firm is matched with a control firm based on their respective propensity scores. LB and SB refer to large buys (above the daily median trade size for a firm) and small buys, respectively. LS and SS are defined similarly for large and small sells, respectively. *N* denotes the sample size. *t*-statistics are shown in parentheses in the four rightmost columns and correspond to a difference-in-difference test that the price impact difference between treatment and control firms is the same before and after the switch to transparency. Standard errors used to compute the *t*-statistics are clustered by both firm and time as in Petersen (2009). Statistical significance at the 1% (5%) level is indicated by ** (*).

	Anonymous to Transparent (treatment, *t*)				Remained Transparent (control, *c*)				Difference (*t* − *c*)			
	LB	SB	LS	SS	LB	SB	LS	SS	LB	SB	LS	SS
Panel A. All Firms												
Before (*b*)	14.6	9.1	−20.6	−15.9	14.9	11.5	−26.5	−18.8	−0.3	−2.3	5.9	2.9
After (*a*)	11.1	7.4	−13.9	−13.2	34.6	24.1	−28.3	−18.8	−23.6	−16.7	14.4	5.5
(*a* − *b*)	−3.5	−1.7	6.7	2.6	19.7	12.6	−1.8	0.0	−23.3**	−14.4**	8.6	2.6
N	5,906	5,426	5,977	5,353	5,906	5,426	5,977	5,353	(−4.69)	(−3.81)	(1.92)	(0.83)
Panel B. Small Capitalization Firms												
Before (*b*)	44.5	32.4	−56.7	−45.1	17.4	14.3	−51.0	−31.9	29.6	18.1	−5.7	−13.2
After (*a*)	33.9	27.4	−33.0	−35.0	99.0	71.4	−55.1	−27.9	−0.7	−44.0	22.0	−7.1
(*a* − *b*)	−10.6	−5.0	23.6	10.0	81.6	57.1	−4.1	4.0	−92.2**	−62.1**	27.7	6.1
N	717	543	753	555	717	543	753	555	(−3.58)	(−2.72)	(1.74)	(0.39)
Panel C. Medium Capitalization Firms												
Before (*b*)	23.5	14.8	−32.2	−27.4	12.6	11.8	−29.7	−21.8	8.6	3.0	−2.5	−5.5
After (*a*)	13.4	8.6	−20.4	−21.4	40.5	25.1	−30.7	−19.7	−21.2	−16.4	10.4	−1.6
(*a* − *b*)	−10.1	−6.2	11.8	6.0	27.9	13.3	−1.1	2.1	−38.0**	−19.4**	12.9	3.9
N	1,412	1,246	1,443	1,257	1,412	1,246	1,443	1,257	(−5.21)	(−3.86)	(1.84)	(0.60)
Panel D. Large Capitalization Firms												
Before (*b*)	6.1	4.2	−9.8	−8.2	15.3	11.0	−20.8	−16.1	−8.8	−6.8	11.0	7.9
After (*a*)	5.6	3.6	−7.1	−6.2	19.5	15.9	−21.6	−16.8	−29.0	−12.3	14.5	10.6
(*a* − *b*)	−0.5	−0.6	2.7	1.9	4.2	5.0	−0.8	−0.7	−4.7	−5.5*	3.4	2.7
N	3,777	3,637	3,781	3,541	3,777	3,637	3,781	3,541	(−1.83)	(−2.53)	(1.24)	(1.19)

would expect, as firm size increases the magnitude of the price impacts decreases and the results parallel those of the full sample. The results for the nonwinsorized sample are consistent in both sign and significance with those of the winsorized sample and are reported in Tables A5 and A6 of the Supplementary Material.

So far we have found that post-trade anonymity decreases quoted spreads in an economically and statistically significant fashion across firms of different size. Furthermore, price impact, which measures the asymmetric information content of trades, is also economically and statistically lower when the switch was made to reporting trades anonymously in 2008, though the 2009 results are not consistent with this. Price impact is associated with a particular trade and, hence, depends on how liquidity demanded and supplied interact at a particular point in time. To understand the effect of anonymity on the supply of liquidity in isolation, next we examine the depth of the limit order book.

C. Book Depth

Rather than simply evaluate the depth at the inside bid and ask, we make use of the data for the entire book to construct a more comprehensive measure of depth. Nasdaq Nordic reports the depth of the limit order book (LOB) to a maximum of 20 different price levels on both the bid and ask side, however all

20 levels may not be occupied by limit orders at any given time. Furthermore, the price grid is not fixed. If there were limit sell orders at 101, 102, and 104, then these would occupy the first 3 levels in the reported data as opposed to 4 levels with 103 being set to 0.

As with spreads and price impact, we compute the t-statistics in this section using clustered standard errors, dropping Sept. 2008, and winsorizing at 2.5/97.5%. Also, as with spreads, we sample the LOB at 15-minute intervals and average these observations to compute a daily depth measurement. We use 2 measures for depth: the sum of the depth in euro, and the summed depth normalized by daily volume.

Since the LOB data are reported as the first 20 occupied price levels, summing over these levels may not ensure a fair comparison. For example, for one firm the first 20 levels may correspond to price movements within 1% of the bid–ask midpoint, yet for another the first 20 levels could be within 5%. To avoid noise introduced by the LOB reporting conventions, in addition to summing book depth over all levels, we also sum book depth only for those price levels within a fixed percentage of the bid–ask midpoint. To determine what percentage is optimal, we classify price movements into 3 cases: those that are not large enough to move the price beyond the inside bid–ask spread (I), those that move the price beyond the bid–ask spread but are not large enough to exhaust all the depth quoted in the book (N), and those that are so large that they move the price beyond the outside level in our data (O).[22]

We want to fix a price movement such that it is large enough to minimize occurrences of case I, but yet not too large so that we get too many occurrences of case O. We choose two reasonable price movements, 1% and 2%, impose these hypothetical price movements on the book for all firms in our sample period, and record where the hypothetical ending price winds up in relation to the top and bottom of the ask and bid side of the book. The results are shown in Table A8 of the Supplementary Material. While 1% price movements seem to work a bit better, we report depth within both 1% and 2% of the quote midpoint. These price movements are large enough to move the price beyond the inside quote, yet not too large to move beyond the outside quotes. For example, the first column in Panel A of Table A8 shows that 83.0% of all 1% price changes from the quote midpoint for the treatment group before the switch to anonymity wound up in the middle of the ask side of the book.

We compute the depth of the book for each firm by converting share prices to euro, multiplying by shares at each level, and summing over all 20 levels of the book. The impact of the switch to anonymity on book depth is reported in the first 2 rows of Panels A and B (All - Before and All - After) in Table 10. The results in the table indicate an improvement in book depth for the treatment group after the switch to anonymity on June 2, 2008 and a corresponding decrease after the switch back to transparency on Apr. 4, 2009. Our refined measures, where we only sum book depth at price levels within 1% or 2% of the bid–ask midpoint, are shown in the middle and last 2 rows of each panel in Table 10. For example, summing depth all levels of the book within 1% of the quote midpoint, the book depth on the

[22] See Table A7 in the Supplementary Material for an illustration.

TABLE 10

Book Depth in Euro Before/After Changes in Trading Transparency

Table 10 reports the average daily book depth in euro of those firms that switched to anonymity on June 2, 2008 (Panel A) and those firms that switched back to transparency on Apr. 14, 2009 (Panel B). The statistics are computed by summing the depth of the book in euro for all 20 levels (All) and also only those levels within 1% and 2% of the inside bid–ask midpoint. The book depth for each firm is sampled at 15 minute intervals and the daily average is computed for each firm. N denotes the sample size. t-statistics are shown in parentheses in the two rightmost columns and correspond to a difference-in-difference test that the book depth between treatment and control firms is the same before and after anonymity. Standard errors used to compute the t-statistics are clustered by both firm and time as in Petersen (2009). Statistical significance at the 1% (5%) level is indicated by ** (*).

	Treatment Firms (t)		Control Firms (c)		Difference ($t-c$)	
	Bid	Ask	Bid	Ask	Bid	Ask
Panel A. Change to Anonymity on June 2, 2008						
All - before (b)	466,998	511,055	1,046,490	1,093,409	−579,492	−582,353
All - after (a)	319,920	331,747	767,207	761,336	−447,287	−429,589
Difference ($a-b$)	−147,078	−179,308	−279,283	−332,073	132,205**	152,764**
N	9,537	9,537	8,139	8,139	(2.7)	(3.1)
1% - before (b)	285,545	292,276	657,072	617,974	−371,527	−325,699
1% - after (a)	170,716	171,438	463,642	425,514	−292,926	−254,076
Difference ($a-b$)	−114,829	−120,837	−193,430	−192,460	78,601*	71,623*
N	9,537	9,537	8,139	8,139	(2.2)	(2.1)
2% - before (b)	379,601	397,940	872,611	863,128	−493,010	−465,188
2% - after (a)	241,837	241,766	610,527	573,360	−368,690	−331,595
Difference ($a-b$)	−137,763	−156,174	−262,084	−289,767	124,320**	133,593**
N	9,537	9,537	8,139	8,139	(2.9)	(3.2)
Panel B. Change from Anonymity on Apr. 14, 2009						
All - before (b)	241,981	242,787	494,230	525,677	−252,249	−282,890
All - after (a)	300,779	326,459	699,407	797,442	−398,629	−470,983
Difference ($a-b$)	58,797	83,672	205,177	271,765	−146,380**	−188,092**
N	13,735	13,735	12,249	12,249	(−6.3)	(−7.2)
1% - before (b)	107,189	110,078	256,567	258,515	−149,378	−148,437
1% - after (a)	141,449	149,109	341,033	351,792	−199,584	−202,683
Difference ($a-b$)	34,259	39,031	84,465	93,277	−50,206**	−54,246**
N	13,735	13,735	12,249	12,249	(−3.6)	(−3.8)
2% - before (b)	160,454	161,484	359,782	372,159	−199,328	−210,674
2% - after (a)	205,078	220,212	494,903	540,391	−289,825	−320,179
Difference ($a-b$)	44,624	58,727	135,121	168,233	−90,497**	−109,505**
N	13,735	13,735	12,249	12,249	(−4.9)	(−5.5)

ask side was 71,623 euro higher for those firms that became anonymous in June 2008 compared to those that remained transparent. When the switch was made back to transparency in Apr. 2009, those firms that switched from anonymity to transparency had a book depth 54,246 euro lower than those that had no change in trade reporting. These results are consistent with those that sum the book depth over all levels, namely that anonymity improves book depth. Also, consistent with the impact of the financial crisis of 2008, the book depths reported in Table 10 are smaller for both treatment and control groups in Panel B (2009) than Panel A (2008).

The results in Table 10 are somewhat difficult to interpret since the depth of the control firms before the switch to/from anonymity is roughly twice that of the treatment firms, however since we did not explicitly match control firms to treatment firms using book depth there is no reason that these have to match. Also while spreads and price impacts are ratios, the sum of the book depth is not. To correct for this we focus on LOB price levels within 1% of the bid–ask midpoint, and, as we did for the results for spreads and price impact, separate our sample in to small, medium, and large firms. We then divide the sum of LOB depth within

1% of the quote midpoint by average daily volume for each firm to get a daily measure of standardized book depth.

The results are shown in Table A9 in the Supplementary Material. Consistent with the results in Table 10, the switch to anonymity in 2008 increased book depth relative to daily volume for all firm sizes. For example, medium-sized firms that switched to anonymity experienced a slight increase in book depth relative to volume (from 0.296 to 0.300), while those that remained transparent had turnover cut by more than half (from 0.200 to 0.072). These results are consistent with our finding that the switch to anonymity decreased spreads and price impact. The effect of switching back to transparency in 2009 on book depth is mixed. Depth improved across the board for both treatment and control firms, however there is not a statistically significant effect for either small or large firms, and the depth of medium sized treatment firms actually increased relative to the control firms. In sum, while the results for 2009 are mixed, the book depth results for the 2008 switch to anonymity in Tables 10 and A9 are consistent with both the spread and price impact results that anonymity improves liquidity.

D. Robustness

One concern is that the errors for the spreads could be cross-sectionally and/or serially correlated. While we addressed this by using the method outlined in Petersen (2009), another approach is simply to reduce each firm to a single observation. Specifically, we compute the timeseries average of the spread for each treatment and control firm before and after the switch to anonymity. We then take the difference in this average spread between the treatment and matched control firms and test if the difference between the treatment and matched control is statistically different after the switch to anonymity. Consistent with Table 7, we find that the spread for the treatment firms is 95 bps lower than that of the control firms after the switch to anonymity ($N = 129$, t-statistic $= 3.6$). For the switch to transparency in 2009, we find that the spread for the treatment firms is 10 bps higher than that of the control firms, though it is not statistically significant ($N = 125$, t-statistic $= -0.3$).

When we match on size, price, return volatility, and spread in Mar. 2008, and broker concentration, but not industry, we find that the results for spread, price impact, and book depth are of the same size and significance as in Tables 7–10. Last, when we match on size, return volatility, and volume (no industry, price, or concentration) we find that the 2008 results are the same as reported in this paper and the 2009 results are even stronger in that they point to a decrease in liquidity, an increase in spreads, a more positive (negative) price impact for buys (sells), and a thinner limit order book after the switch to transparency.

E. Differences Between 2008 and 2009

In general, the magnitude of the results for the switch to anonymity in 2008 are larger than those for the switch to transparency in 2009. There could be some contemporaneous changes that may explain this. From Mar. 2008 to May 2009 multilateral trading facilities became active trading platforms for Nordic Stocks. Chi-X began trading Swedish equities in Mar. 2008 and eventually included trading of stocks across all Nordic markets. After Chi-X's successful entry into the

Nordic markets, Turquoise, NASDAQ-OMX Europe, and Burgundy followed by May 2009. As a consequence, in early 2009 traders had access to many trading venues with most alternatives using anonymous trade reporting, which may have muted the effect of the switch to transparency in Apr. 2009.

VI. Conclusion

We investigate the effects of anonymous vs. transparent post-trade reporting in a unique experimental setting where firms in a treatment group switched to anonymous post-trade reporting in 2008 and then the same firms switched back to transparent post-trade reporting in 2009, yielding 2 independent events for a group of treatment and control firms. Using data from the Nasdaq Nordic exchange we find a statistically and economically significant tightening of spreads of 76 basis points (bps) for those firms that switched to post-trade anonymity compared with those that did not switch. When the switch was made back to transparent post-trade reporting we find that spread widened by 39 bps for the largest firms that switched compared with those that did not switch, though the changes in spread were statistically insignificant for small- and medium-sized firms.

Similar to those for spreads, the results for price impact and book depth for the 2008 switch to anonymity also show better liquidity under anonymity. Since we have both a control sample of firms on the Nasdaq Nordic that did not switch, the post-trade reporting regime itself is left as a plausible explanation for the beneficial liquidity changes brought about by anonymity.

Given that we find liquidity improves under anonymity, it seems odd that Nasdaq Nordic so quickly abandoned the experiment with post-trade anonymous reporting. The reason given for switching back to transparency was vague and based on exchange member "consultation" (https://globenewswire.com/news-release/2009/04/08/122851/0/en/As-of-April-14-NASDAQ-OMX-Nordic-changes-Post-Trade-Anonymity-for-the-equity-market-trading-in-Stockholm-and-Helsinki-3-09.html). One possibility is that the exchange looked at stocks that switched to anonymity in isolation and, observing the increase in spreads concluded that anonymity was not effective in reducing spreads. Of course, this ignores the fact that spreads for all stocks increased during the crisis (Table A4) and transparent stocks fared worse. Far from being resolved, the question of the optimal level of transparency is still being debated. For example, while Nasdaq OMX switched to transparent trade reporting in 2009, in 2014 the exchange began allowing members to hide their identity if they so choose (https://newsclient.omxgroup.com/cdsPublic/viewDisclosure.action?disclosureId=599546). Future research is needed to evaluate whether this or other versions of post-trade reporting lead to better liquidity.

Supplementary Material

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