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ANONYMOUS TRADING IN EQUITIES

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Abstract

I explore a reform at the Oslo Stock Exchange to assess the causal effect of trader anonymity on liquidity and trading volume. Using a regression discontinuity approach, I find that anonymity leads to a reduction in bid-ask spreads by 40% and an increase in trading volume by more than 50%. The increase in trading volume is mostly accounted for by an increase in trading activity by institutional investors. These results are consistent with theoretical frameworks where informed traders supply and improve liquidity in anonymous markets.

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1 Introduction

Stock exchanges continually fine-tune their markets to promote liquidity. A much-used strategy in the last decade has been to alter the degree to which traders are anonymous. In this paper, I assess the effect of trader anonymity on stock liquidity and trading volume. An anonymity reform at the Oslo Stock Exchange allows for causal inference. Consistent with theoretical frameworks where informed traders supply and improve liquidity in anonymous markets, I find that trader anonymity increases stock liquidity and trading volume, and that the increase in aggregate trading volume is mostly accounted for by increased activity by institutional investors.

How transparent should trading in equity markets be? Market regulators have long advocated for more transparency. For example, in 2009, the former SEC chairman Schapiro stated that "Transparency is a cornerstone of the U.S. securities market (...) We should never underestimate or take for granted the wide spectrum of benefits that come from transparency" (SEC 2009). Regulators both in the United States and in Europe are currently considering comprehensive market structure changes to increase market transparency.[1]

Market participants, on the other hand, caution that too much transparency frustrates traders' ability to efficiently work large orders because transparent markets expose trader demands and may increase trading costs — thus, harming liquidity.[2] At least partly in response to trader demands, leading stock exchanges, such as Nasdaq, London Stock Exchange, and Deutsche Börse, have recently increased trader anonymity (the Appendix provides an overview of policy changes in this area).[3]

[1]For example, the European MiFID II regulation, due in 2018, is expected to introduce mechanisms that cap the volumes that can be traded in the least transparent venues (European Commission 2014). Similarly, in the United States, the Financial Industry Regulatory Authority (FINRA) recently announced plans to expand its ongoing 'Transparency Initiative' by mandating the public disclosure of block-sized transactions in so-called ATSs, a class of low-transparency trading venues (FINRA 2016).

[2]For example, Øyvind Schanke, head of equity trading at NBIM, the world's largest sovereign wealth fund, recently expressed concerns over transparent markets, to the *Wall Street Journal* (2013): "If we sent our orders into the market, we would have to wait days or weeks for our brokers to execute the trade. Even then, there are risks of information leakage."

[3]In a regulatory appeal to introduce trader anonymity in NASDAQ's SuperMontage system, the exchange stated that: "Nasdaq proposes to add a post-trade anonymity feature to SuperMontage in response to demand from members (...) Anonymity is important to market participants because sometimes the identity of a party can reveal important 'market intelligence' and complicate a member's ability to execute its customer orders" (Federal Register 2003).

Theoretical predictions on the effect of trader anonymity on stock liquidity and trading volume are ambiguous. The literature on liquidity-supplying informed traders (e.g. Boulatov and George 2013, Rindi 2008) posits that informed traders are more willing to supply liquidity when they can do so anonymously. As a consequence, anonymous markets attract liquidity suppliers who improve stock liquidity. In contrast, Huddart et al. (2001) find a negative effect of trader anonymity on liquidity and trading volume because anonymity exacerbates adverse selection which reduces the willingness to transact. Theoretical ambiguity makes the impact of trader anonymity on market outcomes an empirical question.

The purpose of this paper is to empirically assess how trader anonymity affects stock liquidity and trading volume. An anonymity reform in the period $2008 - 2010$ at the Oslo Stock Exchange (OSE) provides a rare source of exogenous variation. Semi-annually, the 25 most traded stocks at OSE were selected for anonymous trading; all others were not. Comparing just-included and just-excluded stocks in a so-called regression discontinuity design provides causal estimates. I find that anonymity significantly increases liquidity and trading volume. For example, relative bid-ask spreads, a standard measure of illiquidity and transaction costs, are 40% lower for anonymous stocks, and trading volume is higher by more than 50%.

Improvements in stock liquidity and trading volume may not be due to trader anonymity but index inclusion effects. Anonymity at OSE was determined by membership in the OBX index, a composition of the most traded shares at OSE. Systematic differences between index and non-index stocks, caused (for example) by index benchmarking strategies, can confound the estimated effects of anonymity. To examine if OBX index stocks are systematically different from non-index stocks, I compare index and non-index stocks in periods before anonymity was introduced. I find no differences between marginal index and non-index stocks in periods without anonymity. Moreover, index funds typically track the broader Oslo benchmark index, in which all sampled stocks are included, and not the OBX by itself. For example, only two index funds track the OBX in the sample period, and their combined net assets amount to 5% of the net assets tracking the benchmark index. Thus, it seems unlikely that index effects are driving the results.

That trader anonymity improves stock liquidity and trading volume is inconsistent with theoretical models that emphasize the adverse selection costs of anonymous markets (e.g. Huddart et al. 2001) but is consistent with models that emphasize the benefits of informed liquidity supply in anonymous markets (e.g. Rindi 2008, Boulatov and George 2013). To further explore the empirical support for the latter class of models, I use detailed transaction-level data on the trading of all investors at the OSE. As empirical proxies for 'informed' and 'uninformed' investors, I follow Linnainmaa and Saar (2012) and use institutional and retail investors, respectively. I find that the increase in aggregate trading volume is mostly accounted for by an increase in trading activity by institutional investors while retail investors do not adjust their trading behavior in response to anonymity. I interpret the simultaneous increase in trading by institutional investors and stock liquidity under anonymity as consistent with informed traders supplying and improving liquidity in anonymous markets.

This paper connects to current debates in the academic literature. First, the existing empirical literature on trader anonymity (e.g. Theissen 2003, Waisburd 2003, Comerton-Forde et al. 2005, Foucault et al. 2007, Thurlin 2009, Comerton-Forde and Tang 2009, Hachmeister and Schiereck 2010, Friederich and Payne 2014, Dennis and Sandås 2015) has produced mixed results. This literature is based on non-exogenous variation where identification is difficult.[4] In contrast, I exploit exogenous variation in trader anonymity for causal inference. My research contributes to this literature with cleanly identified positive effects of trader anonymity on stock liquidity and trading volume.

Second, recent empirical work by Collin-Dufresne and Fos (2015) finds that standard measures of adverse selection and stock liquidity are uninformative about the presence of informed traders. They argue that the classical inverse relation between informed trading and stock liquidity breaks down, among other reasons, because informed traders supply liquidity. My results complement their findings by showing a positive relationship between informed trading activity and stock liquidity in anonymous markets.

Moreover, my research may provide guidance to policy makers in the United States and Europe who are currently considering market structure changes to increase equity market transparency (see footnote 1). The results in this paper suggest to them that increasing equity market transparency may worsen overall market quality by discouraging informed

[4]The existing literature on trader anonymity is based mostly on between-market comparisons and before-and-after variation in anonymity, which does not allow for a separation of the effect of trader anonymity from confounding factors. Recent studies use difference-in-differences strategies with different markets as control groups to improve identification (Dennis and Sandås 2015, Friederich and Payne 2014). This variation is unlikely to be exogenous, as the choice to implement anonymity for a given market is likely to be endogenous to its future market quality trend.

investors from participating and providing liquidity to the market.

The paper is organized as follows: Section 2 presents the anonymity reform at the Oslo Stock Exchange; Section 3 describes the data; Section 4 describes the empirical design; Section 5 presents the main results; Section 6 investigates the validity and robustness of the empirical design; Section 7 explores the mechanisms driving the main results; and Section 8 concludes.

2 The reform

This section begins with a brief presentation of the Oslo Stock Exchange before providing details on a trader anonymity reform from $2008-2010$ at the Oslo Stock Exchange.

2.1 The Oslo Stock Exchange

The Oslo Stock Exchange is a medium-sized stock exchange by European standards, currently ranking among the 35 largest equity markets in the world by market capitalization (World Federation of Stock Exchanges 2016). At the end of 2010, the total market capitalization of OSE was about 1.7 trillion NOK (1USD $\approx$ 8NOK), spread out over 220 companies.[5] Turnover velocity in the period $2008-2010$ ranged between 124.9% and 156.8%. Faced with competition from alternative trading venues, OSE market shares for trading in OSE listed stocks declined from 100% in 2008 to approximately 90% in 2010. By 2015, this figure is close to 60%.[6]

The OSE operates a fully electronic centralized limit order book and has done so since 1999. The OSE order book allows conventional limit, market, and iceberg orders, along with various other order types. As is common in electronic order-driven markets, order placements follow price-time priority: orders are first sorted by their price and then, in case of equality,

[5] These figures are extracted from Oslo Stock Exchange annual statistics, publically available on the OSE web site. Statistics on the trading of OSE listed stocks on alternative trading venues are based on publically available data collected from Fidessa, a data vendor.

[6] The Oslo Stock Exchange has, in recent years, been the testing-ground of several empirical studies. For example, Ahern and Dittmar (2012) use a Norwegian gender quota reform to investigate the impact of female board representation on firm valuations at the Oslo Stock Exchange. Døskeland and Hvide (2011) leverage high-quality administrative data to investigate the trading performance of individual investors in professionally close stocks, while Næs et al. (2011) explore the connection between stock market liquidity and the business cycle.

by the time of their arrival. The trading day at the OSE consists of three sessions: an opening call period, a continuous trading period, and a closing call period. In late 2012, the continuous trading session was shortened from 09:00 – 17:20 to 09:00 – 16:20. Call auctions may be initiated during continuous trading if triggered by price monitoring or to restart trading after a trading halt.

2.2 Trader anonymity at the Oslo Stock Exchange

The Oslo Stock Exchange (OSE) introduced post-trade anonymous trading on June 2, 2008.[7] Anonymity was introduced to the 25 most traded stocks at the OSE — the constituents of the OBX list. The OBX list is aimed to be a highly liquid composition of shares that reflects the Oslo Stock Exchange investment universe. The stock composition of the OBX list has been revised twice a year (end of June and December) since 1987. After June 2, 2008, all OBX stocks were traded anonymously, and all other stocks at OSE were traded non-anonymously. Stocks entering the OBX after this date received anonymity, and stocks leaving the OBX lost anonymity. See Figure 1 for a time-line.

Stocks are selected for the OBX list based on cumulative trading volume in the six months leading up to a new OBX composition. Table 1 illustrates the selection process. On all list revision dates in Table 1, the 25 stocks with the highest currency trading volume accumulated over the previous six months are chosen from the broader Oslo benchmark index (OSEBX) to comprise the OBX for the subsequent six months.[8] If, for example, two stocks X and Y have accumulated trading volumes of 10 billion NOK and 10.1 billion NOK, respectively, then stock Y is ranked above stock X and is more likely to become an OBX stock. If both stocks rank among the 25 most traded, they will both become OBX listed stocks. If, however, stock Y is ranked 25, and stock X is ranked 26, the former will be an OBX stock, and the latter will not.

[7] The Oslo Stock Exchange often consults members before making major changes to the market model. Members were consulted on whether to introduce trader anonymity or not in a letter dated April 2007. The consultation response was only slightly in favor of implementing anonymity, which may explain why anonymity was implemented only for a small group of the stocks. The decision to implement anonymous trading was first announced February 19, 2008.

[8] The OSEBX is the benchmark index at the OSE. The OSEBX index is an investible index which comprises the most traded shares of the Oslo Stock Exchange. It is revised semi-annually on a free-float adjusted basis. Revisions of the OSEBX index take place on 1 December and 1 June. The OSEBX index typically holds between 60 and 80 stocks, from which the 25 OBX list stocks are chosen.

OSE can supersede the volume-based assignment procedure if "special circumstances so indicate." When the OSE chooses to do so, there is a disparity between the predicted assignment and actual assignment, which needs to be accounted for in the empirical application. A stock may, for example, be exempt from the semi-annual volume-ranking if trading frequency is too low, turnover is too volatile, or the stock is intended for delisting from the exchange. If the OSE chooses to override the main assignment rule, it fully excludes the stock from the ranking process due to non-eligibility.

The trader anonymity introduced by the OSE significantly reduced the amount of information disclosed from the trading process. The top panel of Table 2 illustrates the information available to market participants when trading is non-anonymous. All market participants observe the identities of buyers and sellers (at the brokerage firm level) instantaneously after transactions, in addition to prices and volumes. In contrast, when trading is anonymous, this information is no longer available (bottom panel of Table 2).[9] Market participants observe that transactions have been executed, with corresponding prices and volumes, but do not observe the identities of buyers or sellers.

Transparency was restored for all stocks after two years. On April 12, 2010, the OSE adopted a new trading platform and, at the same time, reversed the trader anonymity rule. Therefore, trading in all stocks at the OSE is currently fully transparent and non-anonymous.

3 Data

Data are collected from several sources. I collect daily frequency data on all common stock at the Oslo Stock Exchange from *Børsprosjektet* at the Norwegian School of Economics (similar to CRSP). The data covers the period December 2001 - December 2010. This dataset holds information on opening and closing prices, daily price dispersion (highest and lowest prices), measures of trading volume (in currency and in shares), end-of-day bids and asks, and OBX

[9]Identities were available in real time bilaterally to the parties of the trade, and to all market participants after the close of each trading day (daily batch updates at 18:00). The OSE introduced a central clearing party (CCP) in June 2010 after both the introduction and reversal of trader anonymity. This means that, in order to facilitate clearing and settlement, the identities in each specific transaction had to be disclosed to the specific counterparty of the transaction, even with anonymous trading. The anonymity reform implies a move from multilateral to bilateral exposure of identities.

and OSEBX index constituency indicators.[10] I supplement this data with the daily number of transactions, obtained from the OSE. I use these data to assess the impact of trader anonymity on market quality (Section 5).

From *Børsprosjektet*, I also collect yearly frequency data on a variety of firm characteristics and accounting measures. This dataset contains information on firms' total equity, total assets, market capitalization, price-to-book ratio, operating profits, operating income, and cash holdings. Firm characteristics are collected on the last trading day of each calendar year.[11] I use these data to assess whether just-included and just-excluded OBX stocks are comparable in their observable characteristics.

In the analysis on heterogeneity in trader response to anonymity (Section 7), I use proprietary transaction-level data obtained from the OSE. The data contains time-stamped (to the nearest second) information on all transactions in all common stock at the OSE. Each entry in the dataset is a trade and gives the identity of buyers and sellers as well as volumes, prices, and stock identifiers. Trader identifiers were not available to market participants in this period, but the OSE kept record for market surveillance purposes. Buyer and seller identities are at the brokerage level and do not identify underlying accounts.

3.1 Sample selection

In the main analysis (Section 5), I investigate the effects of trader anonymity at the Oslo Stock Exchange and restrict the sample period to June 2008 – April 2010. In falsification tests (Section 6), I employ the full sample period, from 2002 – 2010, to analyze revisions of the OBX list both before and after trader anonymity was introduced. In both analyses, I restrict the sample to the 70 trading days following each OBX revision date. Relevant OBX revision dates are found in Table 1. These 70-day trading windows are defined as events and identified by subscript e. This restriction is imposed to ensure that each event is of equal duration, as transparency was restored April 12, 2010, between OBX revision dates.

[10] Due to minor errors in the OBX constituent data from *Børsprosjektet*, data on OBX list constituency have been corrected using hand-collected data from electronic archives at the OSE. Historical data on tick sizes have been compiled from the same source.

[11] While some of the firm characteristics, such as market capitalization and price-to-book, may be defined on a higher frequency, for simplicity, I define all firm characteristics on the same, yearly frequency. In order to assign firm characteristics and accounting variables to firms that are delisted from the OSE during the calendar year, I collect (from *Børsprosjektet*) a weekly frequency dataset containing the same set of firm characteristics and assign characteristics to firms on the final observation date before delisting.

The transaction-level data used in Section 7 covers four weeks of trading following each of the four OBX revisions in $2008-2010$. For balance, I restrict the sample to the 16 trading days following each revision (analogously defined as an event e). As is customary with transaction-level datasets, I keep only automatically matched on-order book trades that are executed during normal exchange opening hours. When, in Section 7, I compute the number of trades and trading volume for different investors, I only consider buy transactions to avoid double-counting transactions.

In sections $5-7$, I collapse the data at the event-level. Variables are first defined on a daily frequency, then averaged within each event e. For example, the log of number of trades is defined daily as $lntrades_{it}$ for stock i on date t and averaged into a single observation $lntrad_{ie}$ for event e.[12] I do this to ease the intuition of the regression discontinuity design, which is often associated with cross-sectional data, applied throughout the analysis.

Throughout the analysis, I only keep stocks listed on the benchmark index at the OSE (the OSEBX index). Only OSEBX stocks are eligible for the semi-annual volume-ranking that determines OBX list constituency and, consequently, anonymous trading. The OSEBX index usually holds $60-80$ stocks, from which the 25 OBX list stocks are chosen.

3.2 Summary statistics

Table 3 summarizes stock characteristics in the full sample period $2002-2010$. Two features of the data stand out. First, OBX listed shares are (on average) vastly different from other shares listed at the Oslo Stock Exchange across all observable characteristics. For example, OBX shares are significantly more valuable, more frequently traded, and have lower transaction costs than non-OBX shares. This is the natural consequence of the volume-based OBX list selection mechanism.

Second, the sampled stocks are mostly small- or medium-capitalization firms, by international standards. For example, the average firm market capitalization is 18.6 billion NOK (1 USD $\approx$ 8 NOK), which is comparable to large S&P600 (small-cap) stocks or small S&P400 (mid-cap) stocks. The stocks are, however, actively traded. The average share volume is 1.6

[12] The stock panel is not balanced because some stocks are delisted from the Oslo Stock Exchange before the 70 day event window is over. For these stocks, outcomes are computed using the number of trading days available. Applying the regression discontinuity design to the full panel of daily observations, instead of on event-level averages, produces almost identical results.

million shares, with a standard deviation of 6.6 million shares. The average stock-day has 451 transactions and a monetary trading volume of 81 million NOK. The average trade size is 4327 shares, and the average trade value is greater than 150 000 NOK.

4 Methodology

I wish to estimate the causal impact of trader anonymity at the Oslo Stock Exchange on stock outcomes. The 'naïve' regression compares outcomes y_{ie} (e.g. stock liquidity) for anonymously traded stocks and non-anonymously traded stocks:

$$y_{ie} = \alpha + \gamma D_{ie} + u_{ie},$$

where D_{ie} is an indicator for anonymous trading in stock i during event e. The effect of interest is captured by the coefficient γ, while the error term u_{ie} represents all other determinants of the outcome. While straightforward to derive, the coefficient γ is unlikely to represent the causal impact of trader anonymity on outcomes y_{ie}. The reason for this is that only the most traded stocks at the Oslo Stock Exchange are traded anonymously such that D_{ie} is likely to be correlated with omitted variables that are themselves correlated with y_{ie} — causing a biased estimate of γ.

The rank-based anonymity assignment mechanism at the Oslo Stock Exchange provides a source of exogenous variation that can be used to overcome this endogeneity problem. The 25 most traded stocks at the OSE are semi-annually assigned to anonymity, while stocks ranked 26 and below are not. Lee (2008) demonstrates that comparing just-included and just-excluded stocks provides quasi-random variation in anonymity since, for narrowly decided races, the outcomes are unlikely to be correlated with other characteristics as long as there is some unpredictable component of the ultimate rank outcome.

The regression discontinuity (RD) design exploits this quasi-random variation (see Lee and Lemieux 2010 for a review). The RD relates discontinuities in outcomes at some treatment threshold to discontinuities in the probability of treatment at the same point. In the case of trader anonymity at the Oslo Stock Exchange, the RD approach implies comparing stocks that are ranked (by previous six month trading volume) marginally inside the top 25 to those ranked marginally outside the top 25.

The first step in the RD design is to define the mechanism that determines eligibility to anonymous trading. I generate a variable r_{ie} that ranks all stocks (1 highest, n lowest) based on the total trading volume in the six-month turnover period leading up to event e. This variable is updated on each OBX list announcement date in the period $2002 - 2010$ (see Table 1). Stocks with a ranking, r_{ie}, at or below the threshold, 25, are predicted for anonymous trading by the main assignment rule:

$$T_{ie} = \mathbf{1}\left[r_{ie} \leq 25\right],$$

where T_{ie} is an indicator variable equal to one for stock i predicted to be traded anonymously after revision e. I normalize the ranking variable by subtracting r_{ie} from 25. The assignment rule becomes:

$$T_{ie} = \mathbf{1}\left[r_{ie} \geq 0\right]. \tag{1}$$

The second step is to identify the relationship between the predicted treatment T_{ie} and actual treatment D_{ie}. In my setting, there is a disparity between T_{ie} and D_{ie} due to non-compliance to the assignment rule 1. I account for this disparity by using a two-stage least-squares procedure (2SLS). Intuitively, the 2SLS approach identifies a discontinuity in the probability of treatment, exactly at $r_{ie} = 0$, and uses this discontinuity to scale any discontinuities in y_{ie} at the same point. The first stage regression can be stated as:

$$D_{ie} = \alpha_0 + \phi r_{ie} + \psi T_{ie} + \omega T_{ie} \times r_{ie} + \varpi_{ie} \tag{2}$$

Since r_{ie} is centered on zero, its inclusion as a regressor in equation 2 ensures that all identification is centered on $r_{ie} = 0$. Notice that if $\psi = 1$, then T_{ie} perfectly predicts D_{ie}, and the probability of treatment jumps from zero to one at $r_{ie} = 0$. Since there is non-perfect compliance to the assignment rule, the coefficient ψ will be less than one.[13] It is the magnitude of ψ that distinguishes this 'fuzzy' RD design from a 'sharp' RD design.

Finally, the second stage regression relates outcomes y_{ie} to treatment status D_{ie} and the ranking variable r_{ie}:

$$y_{ie} = \alpha_1 + \nu r_{ie} + \tau D_{ie} + \beta D_{ie} \times r_{ie} + \varepsilon_{ie}. \tag{3}$$

[13]Estimates from the first-stage relationship in equation 2 are discussed in detail in appendix A.2.

The coefficient τ identifies a discontinuous change in y_{ie} exactly at $r_{ie} = 0$, properly scaled by the first stage relationship. This coefficient can be interpreted as the causal effect of trader anonymity on y_{ie}, under the identifying assumption that stocks are comparable on both their observable and unobservable stock characteristics at $r_{ie} = 0$.[14]

While it is impossible to assess whether stocks close to $r_{ie} = 0$ are similar in their unobservable characteristics, it is straightforward to assess whether or not they are similar in their observable characteristics. In Figure 3 I plot observable stock characteristics over r_{ie}, for all realizations of r_{ie} in the period $2002 - 2010$. The figure shows that all stock characteristics evolve smoothly across $r_{ie} = 0$. This implies that observations close to $r_{ie} = 0$ are, at the very least, comparable in their observable characteristics.

Moreover, the data allow for a powerful falsification test of the RD design. Out of all the realizations of r_{ie} in the period $2002 - 2010$, only the realizations of r_{ie} in the period $2008 - 2010$ actually assigned trader anonymity to OBX listed stocks. This enables me to estimate the coefficient τ both before and after trader anonymity was implemented. Doing so, I document non-zero estimates of τ exclusively in periods with trader anonymity. This addresses a justified concern of simultaneous shocks to y_{ie} at $r_{ie} = 0$. Particularly, if OBX constituency by itself is correlated with outcomes, then estimates of τ are biased. My falsification test, however, suggests that there is no OBX constituency effect in periods without trader anonymity.

The unbiased estimation of τ requires a strong assumption about the functional form of the relationship between r_{ie} and y_{ie}. This assumption is required because, in order to estimate the effects that occur close to $r_{ie} = 0$, it is necessary to use data away from this point as well (Lee and Lemieux 2010). The RD literature has proposed two main approaches to estimating equation 3 when the functional form of r_{ie} is unknown. The first approach, which is widely preferred, is to restrict the sample size on either side of $r_{ie} = 0$ and estimate equation 3 non-parametrically with so-called local linear regressions. If there is a concern that the regression function is not linear over the entire range of r_{ie}, restricting the estimation range to values closer to the cutoff point $r_{ie} = 0$ is likely to reduce biases in the RD estimates (Hahn et al. 2001, Lee and Lemieux 2010). In contrast, the second approach uses all the available data and allows for a flexible relationship between y_{ie} and r_{ie} by expanding equation 3 with polynomials in r_{ie}.

[14] Figure 2 provides a graphical illustration of the 'fuzzy' regression discontinuity design.

I estimate equation 3 non-parametrically with local linear regressions. This implies estimation within so-called bandwidths. In my setting, the bandwidth is the number of stocks included on either side of the treatment cutoff $r_{ie} = 0$. For example, if the bandwidth is $h = 15$, this implies estimating equation 3 for a sample of stocks ranked $r_{ie} \in [-15, 14]$. For transparency and robustness, I present estimates from a wide range of bandwidths.

Tick sizes, the minimum pricing increment, are determined differently for anonymous and non-anonymous stocks and have been found to affect stock characteristics — in particular, stock liquidity (e.g. Bessembinder 2003, Buti et al. 2015). For this reason, I include $ticksize_{ie}$ as a control variable in all specifications.[15]

I follow Card and Lee (2008) and cluster standard errors at the level of r_{ie}.

5 Main results

In this section, I estimate the impact of trader anonymity on stock liquidity and trading volume in the period $2008 - 2010$. The theoretical literature on liquidity-supplying informed traders (e.g. Boulatov and George 2013, Rindi 2008) posits that informed traders are more willing to supply liquidity when they can do so anonymously. Consequently, anonymous markets attract liquidity suppliers who improve stock liquidity. In contrast, Huddart et al. (2001) posit a negative effect of trader anonymity on liquidity and trading volume because anonymity exacerbates information asymmetries which reduce the willingness to transact. Estimates of the empirical effect of trader anonymity at the Oslo Stock Exchange are presented in Table 4.

5.1 Results

I first investigate how trader anonymity affects stock liquidity. I measure stock liquidity with the natural logarithm of relative bid-ask spreads (end-of-day quotes divided by the quote midpoint). Wider bid-ask spreads imply lower stock liquidity and higher transaction costs.[16]

[15] Tick sizes at the OSE are determined as step functions of prices such that higher prices give higher tick sizes. The price cutoffs that determine tick sizes are different for OBX and non-OBX stocks.

[16] The end-of-day relative spread is a crude measure of stock liquidity. The effects documented with this liquidity measure, however, also hold for high-frequency measures of liquidity. For example, in unreported regressions, I evaluate the impact of trader anonymity on common measures of liquidity, such as effective and realized spreads, and document similar effects. I only have access to high-frequency measures of liquidity in

Trader anonymity causes a marked reduction in bid-ask spreads. The estimated effect ranges from -0.86 log points (-58%) to -0.56 log points (-43%), depending on the bandwidth choice. All coefficients are highly significant both statistically and economically. Estimates stabilize at lower levels for larger bandwidths (see also Figure 4 for a richer set of bandwidth specifications).

A second question is whether trader anonymity has any effect on trading behavior. If traders engage in the same transactions irrespective of the anonymity of the trading process, then a reduction in bid-ask spreads simply redistributes revenue from liquidity suppliers to liquidity demanders and has no impact on aggregate welfare. To detect any changes in trading behavior, I estimate the impact of trader anonymity on trading volume, measured both by the number of transactions and currency volume traded. The estimated effect of trader anonymity on log(number of trades) ranges between 0.99 log points ($h = 10$) and 0.51 log points ($h = 20$) with t-statistics between 2.63 ($h = 15$) and 3.35 ($h = 20$). Similar effects are found for the log of value traded. All estimates are statistically significant and imply a tremendous willingness to trade anonymous stocks, relative to transparent stocks.

As an additional test of the impact of trader anonymity on the quality of equity trading, I investigate how anonymity affects the efficiency of prices, proxied by close-to-close returns volatility.[17] Greater volatility is viewed as a trading friction such that the lower the volatility, the more efficient the market. Table 4 shows that anonymous trading has no impact on this measure of price efficiency.

In the appendix of this article, I propose several extensions to the baseline RD model and show that the results in Table 4 are robust. First, I show that the results are not driven by a functional form assumption on the relationship between outcomes y_{ie} and the ranking variable r_{ie}. The results hold for a wide range of polynomials in r_{ie}. Second, I follow Cellini et al. (2010) and Cuñat et al. (2012) and expand the static RD design into a dynamic RD design. The RD design in equation 3 is static in the sense that it does not take into account that anonymous trading in one period potentially affects the probability of receiving

the 'treatment' period $2008 - 2010$ and not in the 'placebo' period ($2002 - 2007$). For comparability between sample periods, I use the end-of-day bid-ask spread throughout the analysis.

[17]My approach is to compute returns volatility for each stock as the sample variance of the close-to-close returns process within each event e. In contrast, much of the existing empirical microstructure literature focuses instead on high-frequency within-day measures of volatility. In unreported regressions, I use a within-day measure of price dispersion — the daily high price divided by daily low price — and the inference remains identical.

anonymous trading in subsequent periods. Such dynamics can arise because 1) anonymous trading is assigned based on trading volume and 2) anonymous trading increases trading volume. In the appendix, I show that the results are not driven by dynamics.

5.2 Summarizing the results

The results in this section suggest that trader anonymity improves stock liquidity. Estimates from a regression discontinuity design show that trader anonymity causes a reduction in bid-ask spreads of more than 40% and an increase in trading activity (trades and trading volume) of more than 50%. These benign effects of trader anonymity on the quality of trading cannot be reconciled with theoretical models that emphasize the adverse selection costs of anonymous markets. Instead, the results appear consistent with theoretical models that emphasize the role of informed liquidity suppliers in anonymous markets.

6 Identification concerns

The previous section established a positive relationship between trader anonymity at the Oslo Stock Exchange and measures of stock liquidity and trading volume. In this section, I discuss whether these relationships can be interpreted as causal. Supportive of a causal interpretation, I find non-zero regression discontinuity estimates exclusively in periods with trader anonymity and not in 'placebo' periods without trader anonymity. Moreover, I show that the effects documented in Section 5, do not seem to be driven by time-varying confounders.

For expositional purposes, I henceforth report estimates only from bandwidth specification $h = 15$.

6.1 Index inclusion effects

The main identification concern in my setting is index inclusion effects. Trader anonymity at the Oslo Stock Exchange was determined by membership in the OBX index. Consequently, all empirical specifications so far have represented joint tests of the effect of trader anonymity and OBX index constituency. If index constituency by itself is correlated with outcomes —

for example due to index benchmarking strategies — my estimates of the effect of trader anonymity may be confounded.[18]

To examine if OBX index stocks are systematically different from non-index stocks, I compare index and non-index stocks in periods before trader anonymity was introduced. Particularly, I exploit that the full sample covers all OBX index revisions in the period $2002-2010$ and that only the index revisions in the sub-period $2008-2010$ assigned trader anonymity to OBX stocks. In column two of Table 5, I apply the baseline regression discontinuity design to data from the 'placebo' period $2002-2007$.[19] I find no differences between marginal index and non-index stocks in periods without trader anonymity.

To formally quantify the difference in regression discontinuity estimates between the trader anonymity period and the 'placebo' period, and to improve statistical precision, I pool all the data and estimate the following difference-in-differences model:

$$y_{ie} = a + \underbrace{\nu r_{ie} + \tau D_{ie} + \gamma r_{ie} \times D_{ie} + \delta ticksize_{ie}}_{Baseline\,model} + \underbrace{\theta ANON_{ie} + \tau^{Diff} D_{ie} \times ANON_{ie}}_{Added\,terms} + \varepsilon_{ie}, \tag{4}$$

where r_{ie} and D_{ie} are defined as earlier. $ANON_{ie} = 1$ for the anonymity period $(2008-2010)$ and 0 for the 'placebo' period $(2002-2007)$, and controls for level differences in y_{ie} between the two periods. The coefficient τ now represents the regression discontinuity estimate in the 'placebo' period. Consequently, the coefficient τ^{Diff} gives the added effect of OBX index constituency in the period $2008-2010$ relative to the period $2002-2007$. Estimates of τ and τ^{Diff} are presented in column three of Table 5. Estimates of the 'placebo'

[18]The literature points to several reasons as to why index stocks might be different from non-index stocks. For example, Boone and White (2015) find that just-included Russell 2000 index stocks have higher liquidity and trading activity and lower information asymmetries than just-excluded stocks. They argue that this is due to greater institutional ownership driven by indexing and benchmarking strategies. A substantial literature shows how index inclusion leads to pricing effects due to excess demand from passive funds tracking the index (see Shleifer, 1986, Harris and Gurel, 1986, Chang et al. 2014). Moreover, limited investor attention could cause salience such that indexed stocks are more heavily traded (see Barber and Odean, 2008, Hirshleifer et al., 2009).

[19]Ideally, I would apply the regression discontinuity design to placebo periods both before anonymity was introduced and after transparency was restored. However, shortly after the OSE restored transparency for all stocks, the exchange introduced new trading rules differentiated between OBX and non-OBX stocks. For example, a central clearing party (CCP) was introduced for OBX stocks only, and new rules for hidden liquidity, differentiated between OBX and non-OBX stocks, were implemented. For this reason, the placebo sample only covers OBX index revisions in the period $2002-2007$.

regression discontinuity effect (τ) remain statistically indistinguishable from zero for all outcome variables, suggesting that marginal OBX and non-OBX stocks are comparable in the absence of trader anonymity. Moreover, the table shows that coefficient estimates of τ^{Diff} are quantitatively similar to estimates from the baseline specification (column 1), although now estimated with significantly more precision.

While the difference-in-differences model efficiently addresses the concern of a fixed index confounder, it does not address potentially time-varying index confounders. In particular, index benchmarking strategies have grown in popularity over the last decade (e.g. Chang et al. 2014).[20] The impact of such a trend might reveal itself through the absence of an index effect in early periods and the existence of one in later periods, which is consistent with the results in Table 5. In an attempt to address such a confounding trend, I conjecture that an increase in index benchmarking only affects the stocks that actually move in or out of the OBX index and not the stocks that remain inside or outside of the index. Therefore, I add to specification 4 separate indicator variables for stocks that move in or out of the OBX, following a revision. This approach allows me to separate any excess effect for moving stocks, from the direct effect of trader anonymity. Column five in Table 5 shows that coefficient estimates and statistical significance are unaffected by the inclusion of mover dummies.

Three institutional details may explain why index constituency seems to have little impact on the results in this paper. First, the bulk of index funds track the broader Oslo benchmark index (OSEBX), in which all sampled stocks are included, and not the OBX by itself. For example, only two index funds track the OBX in the sample period, and their combined net assets amount to 5% of the net assets tracking the benchmark index.[21] Second, OBX index weights are calculated based on market capitalization, a variable with significant positive

[20] Similarly, the use of so-called exchange traded funds (ETFs) has surged over the sample period (Ben-David et al. 2014). ETFs, like index funds, facilitate exposure towards, among other assets, baskets of stocks such as the OBX index. This surge, however, seems unlikely to explain my results. Although the literature is not conclusive, recent empirical evidence by Hamm (2014) suggests that ETF trading negatively correlates with measures of underlying stock liquidity. The driving mechanism, according to Hamm (2014), is that uninformed traders reduce their participation in the underlying asset if given the option to invest in ETFs, which reduces the liquidity of the underlying asset. If so, a surge in the ETF trading of OBX listed stocks would lead to opposite effects (from what I document).

[21] These figures are based on data from Morningstar in the time period June 2008 - April 2010. Net asset values are reported on different frequencies (monthly, quarterly, yearly) for different funds. Quarterly and yearly holdings are carried forward to create a monthly time-series. Average combined monthly net assets for funds tracking OBX in the sample period are approximately 5% of the net assets tracking OSEBX.

skewness (see Table 3). Consequently, for the marginal OBX stock, where the regression discontinuity effect is measured, this translates into a negligible index weight. For example, in the period $2008-2010$, the average index weight of the marginal OBX stock is 1.04%, which seems unlikely to explain the effects in Section 5.1. Finally, future OBX constituents are announced approximately two weeks prior to actual index reconstitution. Any excess trading activity caused by index benchmarking strategies is likely to be exhausted before the sample data begins, which is at revision date.

6.2 Control variables

If the RD design is valid, there is no need to control for observable characteristics (Lee and Lemieux 2010). This is because the randomness of treatment assignment close to the treatment threshold ensures that marginally included and excluded stocks, on average, are similar in their observable characteristics. Including control variables, however, may increase precision or even reduce estimation bias if observables are not entirely balanced between just-included and just-excluded stocks. In column four of Table 5, I add a comprehensive vector of firm characteristics to specification 4. Estimates of the effect of trader anonymity on stock liquidity and trading volume become slightly smaller in the inclusion of control variables but remain highly significant, both statistically and economically.

6.3 Confounding market structure trends ($2008-2010$)

European market structure developments unrelated to trader anonymity at the Oslo Stock Exchange but correlated with OBX index membership, could drive the results in Section 5.1. For example, the introduction of trader anonymity in 2008 coincides with the most disruptive market structure development in recent European equity trading history. Effective in late 2007, a new pan-European legislative (MiFID) abolished local stock exchange monopolies, and opened competition between exchanges. Anecdotal evidence suggests that entrant exchanges systematically challenged market shares in the most liquid shares before gradually expanding their selection of stocks.[22] Competition for order flow can correlate

[22] Multi-lateral trading facilities (MTFs) began competing for order flow in the largest OSE stocks first, then gradually expanded their selection. For example, the MTF Chi-X opened trading in the five largest OSE stocks in 2008 (Norsk Hydro ASA, Renewable Energy Corp A/S, StatoilHydro ASA, Telenor ASA, and Yara International ASA). Chi-X now offers trading in more than 50 OSE products. Similarly, the MTF

with OBX constituency, by virtue of being the most liquid shares at OSE, and confound the estimated effect of trader anonymity.

To address this concern, I generate a variable $Frag_{ie}$, which measures stock-level order flow fragmentation as the share of traded volume on competing trading venues relative to total traded volume across all venues (see appendix A.5 for details), and include it as a regressor in the baseline regression discontinuity design.[23] Column six of Table 5 shows that the estimates are robust to the inclusion of $Frag_{ie}$ as a regressor, which suggests that order flow fragmentation is not driving the results in Section 5.1.

Meanwhile, I am unable rule to out confounding effects from other concurrent market structure developments. The trader anonymity sample period ($2008-2010$) is characterized by, among other things, an explosion in high-frequency trading (e.g. Jørgensen et al. 2014, Angel et al. 2011,2013), aggressive use (by stock exchanges) of new fee structures, such as maker-taker fees (e.g. Malinova and Park 2015), and a financial crisis. If these developments systematically correlate with OBX list membership, they may bias my estimates. To minimize the potential for time-varying confounders, in appendix A.6 I estimate a short-run difference-in-differences (DiD) model surrounding only the first introduction of trader anonymity at OSE. The DiD specification in appendix A.6 produces broadly the same results as the regression discontinuity design.

7 Mechanisms

In Section 5, I show that trader anonymity improves stock liquidity and trading volume. These results are consistent with theoretical models where informed traders supply and improve liquidity in anonymous markets, such as Boulatov and George (2013) and Rindi (2008).[24] To further explore the empirical support for these models, this section tests

Turquoise initially offered trading in 28 OSE stocks but has since expanded to offer trading in 169 OSE products.

[23] I include $Frag_{ie}$ in the baseline specification (equation 3), and not the extended RD model (equation 4), because $Frag_{ie} = 0$ for the entire period $2002-2007$. Including $Frag_{ie}$ in the extended RD model produces the same results.

[24] In Rindi (2008), the net effect of trader anonymity on stock liquidity depends on the exact modeling of information acquisition. When information acquisition is endogenous, anonymity improves liquidity, but when information acquisition is exogenous, anonymity degrades liquidity. In Boulatov and George (2013), the impact of anonymity on stock liquidity also depends on the aggressiveness by which informed traders supply liquidity. In their model, anonymity induces informed traders to supply rather than to demand liquidity

whether anonymity induces informed traders to transact more. I test this hypothesis using transaction-level data that allow me to create empirical proxies for informed and uninformed traders. Consistent with informed traders supplying and improving liquidity under anonymity, I find that the increase in aggregate trading volume documented in Section 5 is mostly accounted for by informed investors.

7.1 Data and descriptives

I use proprietary data on the trading of all investors at the OSE, obtained from the OSE. Each entry in this dataset is a trade and identifies the buying and selling brokerage firm as well as volumes, prices and stock identifiers (see Section 3 for more details). As empirical proxies for 'informed' and 'uninformed' investors, I follow Linnainmaa and Saar (2012) and use institutional and retail investors, respectively. Based on the brokerage firm identifiers reported in the data, I classify order flow from online discount brokerages as retail. The residual order flow is collectively referred to as 'institutional.' Similar to Linnainmaa and Saar (2012), I distinguish between foreign and domestic institutions. Appendix A.7 provides further detail on this order flow decomposition.

Table 6 describes the trading behavior of retail and institutional investors. Domestic institutions are the most active investors at the OSE with an average (stock-day) trading volume of 23 million NOK spread across 316 trades, followed by foreign institutions (13 million NOK, 215 trades) and finally retail investors (5 million NOK, 112 trades). To provide some evidence supporting that institutions are more sophisticated or 'informed' than retail investors, I follow Malinova et al. (2013) and compute intraday trading profits for each of the trader groups. The average per-stock-day trading loss of retail investors is 5.91 basis points.[25] Both the foreign and domestic institutions in my sample, in contrast, are able to generate positive trading profits, which is suggestive of higher sophistication among these traders. Moreover, consistent with previous literature comparing the trading behavior of

and, in addition, increases the aggressiveness by which they supply liquidity. The interaction of these effects generate improvements in stock liquidity under anonymity. In a recent theoretical framework, Roşu (2016) shows that when informed traders can choose whether to supply or demand liquidity, an exogenous increase in the share of informed traders in the market improves both stock liquidity and price efficiency. Roşu (2016), however, does not model the consequences of anonymous and non-anonymous markets.

[25] For comparison, Malinova et al. (2013) report average daily trading losses of 5.1 basis points for their sample of retail investors.

retail and institutional investors (e.g. Lee and Radhakrishna 2000, Barber et al. 2009), retail investors in my sample execute the smallest trades with an average value of 42 663NOK, while institutional trades average 47 824NOK (foreign) and 60 948NOK (domestic) in size.

7.2 Results

To explore whether anonymity induces informed traders to transact more, I estimate the causal impact of trader anonymity on the trading activity (trades and trading volume) of institutional and retail investors using a regression discontinuity design. The regression discontinuity design compares how the same group of investors trade in two otherwise similar stocks — those just-included and just-excluded from the OBX index — where trading in one stock is anonymous and in the other it is not.

I implement the regression discontinuity design with the same two-stage least-squares approach used in Section 5 to measure the impact of trader anonymity on stock liquidity and trading volume. In the first stage regression, I relate predicted treatment status $T_{ie} = \mathbf{1}[r_{ie} \geq 0]$ for stock i during event e to actual treatment status D_{ie}:

$$D_{ie} = \alpha_0 + \phi r_{ie} + \psi T_{ie} + \omega T_{ie} \times r_{ie} + \varpi_{ie}. \tag{5}$$

In the second stage regression, I relate the trading activity y^g_{ie} of trader group g to treatment status D_{ie}:

$$y^g_{ie} = \alpha_1 + \nu r_{ie} + \tau D_{ie} + \beta D_{ie} \times r_{ie} + \varepsilon_{ie}. \tag{6}$$

The inclusion of the ranking variable r_{ie}, which is centered on zero, in both regression stages ensures that all identification is centered on $r_{ie} = 0$ — the cutoff point between anonymous and transparent trading. Thus, the coefficient τ measures a discontinuous change in the trading behavior of investors exactly at $r_{ie} = 0$, properly scaled by the first stage relationship.

Figure 5 presents graphical evidence on the change in investor behavior at $r_{ie} = 0$. Table 7 presents estimates of τ using a bandwidth specification of $h = 15$. Table 7 shows that both foreign and domestic institutions transact much more frequently when they can do so anonymously. For example, foreign institutions increase their trading volumes by more than 300%, and domestic institutions more than double their trading volumes. In fact, since retail investors do not change their trading behavior in response to anonymity, the entire increase in aggregate trading volume documented in Section 5, can be attributed to institutions.

7.3 Discussion

I have shown that trader anonymity increases stock liquidity and trading volume, and that the increase in aggregate trading volume is mostly accounted for by increased activity by institutional investors. I interpret the simultaneous increase in institutional trading and stock liquidity under anonymity as consistent with theoretical frameworks where informed investors supply and improve liquidity in anonymous markets (e.g. Boulatov and George 2013, Rindi 2008).

Meanwhile, I cannot exclude the possibility that the positive relationship between institutional trading and stock liquidity is spurious.[26] This is because the data available do not allow me to detect whether investors supply liquidity (place limit orders) or demand liquidity (place market orders). Empirical evidence from other markets, however, suggest that informed traders causally improve stock liquidity through liquidity provision. For example, in experimental securities markets, Perotti and Rindi (2006) show that anonymous markets attract informed traders who supply and improve liquidity while Bloomfield et al. (2005) provide evidence that informed traders endogenously take on the role as liquidity suppliers. Similarly, Kaniel and Liu (2006) provide empirical evidence that liquidity providers at the NYSE are informed.

The positive relationship between institutional trading and stock liquidity observed in the current paper may also be explained by the order anticipation framework promoted by Friederich and Payne (2014). They argue that liquidity-motivated institutions, who are not necessarily informed, enter anonymous markets to avoid the transaction costs associated with exposing their trading demands in transparent markets. Because trader anonymity pro-

[26] Another possibility is that causality runs from stock liquidity to institutional trading, and not the other way around. For example, Collin-Dufresne and Fos (2015) present empirical evidence supporting a positive relationship between informed trading and stock liquidity and argue that it can be explained by two mechanisms — 1) informed traders strategically choose to trade when liquidity is high and 2) informed traders supply liquidity. By the latter mechanism, informed trading causally improves stock liquidity while by the former mechanism causality is reversed.

tects institutions from order anticipation (front-running), Friederich and Payne (2014) argue, anonymity allows institutions to patiently accumulate positions which adds competition to the market's liquidity supply — thus, improving stock liquidity. I interpret this empirical framework to be analogous to the theoretical informed liquidity supply framework promoted in the current paper since they both describe how trader anonymity induces a certain group of investors (who move prices under transparency) to transact and supply liquidity.

8 Conclusion

In this paper, I assess the effect of trader anonymity on stock liquidity and trading volume. An anonymity reform in the period $2008-2010$ at the Oslo Stock Exchange provides a source of exogenous variation. The 25 most traded stocks at the OSE were semi-annually assigned to anonymity; all others were not. Comparing just-included and just-excluded stocks in a so-called regression discontinuity design provides causal estimates. I find that trader anonymity increases stock liquidity and trading volume. Retail investors, arguably the least informed investors in the market, do not adjust their trading behavior in response to anonymity. In contrast, institutional investors, perhaps the most informed market participants, transact much more when they can do so anonymously.

These results are consistent with theoretical models where informed traders supply and improve liquidity in anonymous markets (e.g. Boulatov and George 2013, Rindi 2008). The results are inconsistent with theoretical models that emphasize the adverse selection costs of anonymous markets — the main competing mechanism.

The results in this paper contribute to the existing empirical literature on anonymous trading in equities (e.g. Theissen 2003, Waisburd 2003, Comerton-Forde et al. 2005, Foucault et al. 2007, Thurlin 2009, Comerton-Forde and Tang 2009, Hachmeister and Schiereck 2010, Friederich and Payne 2014, Dennis and Sandås 2015). This literature is based on non-exogenous variation, where identification is difficult. In contrast, I exploit exogenous variation in trader anonymity for causal inference. My research contributes to this literature with clean identification and unambiguous results on the effect of anonymity on stock liquidity and trading volume.

My research may provide guidance to regulators in the United States and Europe who are currently considering comprehensive market structure changes to increase equity market transparency. My results suggest that increasing equity market transparency may in fact worsen overall market quality by discouraging informed traders from participating and providing liquidity to the market.

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9 Tables

Table 1: OBX revisions $2002-2010$

Treatment revisions (June 2008 - April 2010)			
Event	*Announced*	*Revision*	*Turnover period*
4	9 Dec 2009	18 Dec 2009	1 June 2009 - 30 Nov 2009
3	11 June 2009	19 June 2009	1 Dec 2008 - 29 May 2009
2	11 Dec 2008	19 Dec 2008	1 June 2008 - 30 Nov 2008
1	9 June 2008	20 June 2008	1 Dec 2007 - 31 May 2008
Placebo revisions (June 2002 - December 2007)			
Event	*Announced*	*Revision*	*Turnover period*
0	07 Dec 2007	21 Dec 2007	1 June 2007 - 30 Nov 2007
-1	13 June 2007	22 June 2007	1 Dec 2006 - 31 May 2007
-2	11 Dec 2006	22 Dec 2006	1 June 2006 - 30 Nov 2006
-3	12 June 2006	16 June 2006	1 Dec 2005 - 31 May 2006
-4	12 Dec 2005	16 Dec 2005	1 June 2005 - 30 Nov 2005
-5	07 June 2005	17 June 2005	1 Dec 2004 - 31 May 2005
-6	10 Dec 2004	17 Dec 2004	1 June 2004 - 30 Nov 2004
-7	10 June 2004	18 June 2004	1 Dec 2003 - 31 May 2004
-8	12 Dec 2003	19 Dec 2003	1 June 2003 - 30 Nov 2003
-9	12 June 2003	20 June 2003	1 Dec 2002 - 31 May 2003
-10	10 Dec 2002	20 Dec 2002	1 June 2002 - 30 Nov 2002
-11	13 June 2002	21 June 2002	1 Dec 2001 - 31 May 2002

Note: The table presents announcement dates and revision dates for all OBX list revisions in the time period $2002-2010$. On OBX revision dates (*Revision*), the 25 stocks with the highest currency trading volume accumulated over the previous six months (*Turnover period*) are chosen from the broader index OSEBX to comprise the OBX the subsequent six months. New OBX stock compositions are announced 1-2 weeks before revisions. Revisions of the OBX list between June 2, 2008 and April 12, 2010, assigned trader anonymity to OBX listed stocks. Revisions of the OBX list before this period did not assign trader anonymity to OBX listed stocks.

Table 2: Examples of anonymous and non-anonymous trade feeds

Non-anonymous trade feed				
Broker ID (buy)	*Broker ID (sell)*	*Stock ID*	*Volume*	*Price*
ESO	NEO	STL	500	195.6
NON	NON	NEC	4000	8.13
ND	ND	QEC	2000	20.9
JPM	NEO	DNBNOR	600	71.9
UBS	NEO	DNBNOR	1600	71.9
ESO	NEO	DNBNOR	700	71.9
NON	NEO	DNBNOR	1400	71.9
LBI	SHB	EKO	1200	81.5
Anonymous trade feed				
Broker ID (buy)	*Broker ID (sell)*	*Stock ID*	*Volume*	*Price*
.	.	STL	500	195.6
.	.	NEC	4000	8.13
.	.	QEC	2000	20.9
.	.	DNBNOR	600	71.9
.	.	DNBNOR	1600	71.9
.	.	DNBNOR	700	71.9
.	.	DNBNOR	1400	71.9
.	.	EKO	1200	81.5

Note: The table illustrates the difference between post-trade anonymity and post-trade non-anonymity. The top panel shows the information available to market participants when trading is non-anonymous. The bottom panel shows the information available to market participants when trading is anonymous.

Table 3: Summary statistics

	Sample descriptives (2002–2010)					
	μ	σ	Min.	Median	Max.	N
Trading characteristics						
Share volume	1.6	6.6	0.0	0.2	469.3	72201
Currency volume	81.3	251.0	0.0	7.5	10345.0	72201
Trades	451.2	930.1	0.0	88.0	19510.0	72298
Trade value	154.3	562.1	0.3	80.1	59731.2	69278
Trade size	4326.7	20921.2	5.7	1601.1	2370560.0	69278
Relative spread (bps.)	148.8	318.2	0.6	66.9	14482.8	72097
Firm fundamentals						
Market cap.	18628.2	50919.5	46.5	4484.2	538881.4	71114
Total equity	8934.3	21397.3	-859.1	1943.8	214079.0	71747
Total assets	40189.1	156162.5	0.0	5663.9	1832699.0	71747
Price/Book	3.8	5.8	-10.7	2.3	60.7	70627
Operating profit	3266.0	17627.1	-14574.0	343.5	228858.6	69789
Operating income	17515.5	57054.5	0.1	2670.7	651977.0	69383
Cash and deposits	1405.7	2997.3	0.9	452.0	27148.0	69090
	OBX vs Non-OBX (2002–2010)					
	μ^{OBX}	$\mu^{Non\,OBX}$	Diff.	$\sigma^{diff.}$	N_1	N_2
Trading characteristics						
Share volume	3.1	0.6	2.5***	0.0	27992	44209
Currency volume	194.2	9.9	184.3***	1.8	27992	44209
Trades	1020.5	91.5	928.9***	6.2	27995	44303
Trade value	184.2	134.0	50.2***	4.3	27987	41291
Trade size	3131.3	5136.9	-2005.6***	161.8	27987	41291
Relative spread (bps.)	44.4	215.2	-170.8***	2.3	27992	44105
Firm fundamentals						
Market cap.	41250.0	3941.1	37308.9***	364.9	27995	43119
Total equity	18840.9	2708.0	16132.9***	152.6	27690	44057
Total assets	91600.9	7876.5	83724.3***	1156.1	27690	44057
Price/Book	3.5	4.0	-0.5***	0.0	26570	44057
Operating profit	6935.5	1113.6	5822.0***	136.5	25802	43987
Operating income	38431.3	5395.4	33035.9***	431.6	25455	43928
Cash and deposits	2896.2	532.4	2363.8***	21.8	25527	43563

Note: The top panel lists means (μ), standard deviations (σ), minimum (Min.) and maximum values (Max.), medians, and number of observations (N) for the full sample of stock-day observations, the first 70 trading days after each OBX revision in the period 2002-2010. Share volume is the number of shares traded, in million shares. Currency volume is the value of shares traded, in million NOK. Trades is the number of transactions. Trade value is currency volume divided by trades, expressed in thousand NOK. Trade size is share volume divided by trades. All stock fundamendals, except Price/Book, are expressed in million NOK. The bottom table shows a t-test of different means between OBX index stocks, and non-OBX stocks, for all observations in the period 2002-2010. μ^{OBX} and μ^{NonOBX} represent the means for OBX and non-OBX stocks, respectively. Diff. is the difference between μ^{OBX} and μ^{NonOBX}. $\sigma^{diff.}$ is the standard error of the difference-in-means. N_1 is the number of observations in the OBX sample. N_2 is the number of observations in the non-OBX sample.

** $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$*

Table 4: Main results

	Bandwidth		
	h=10	*h=15*	*h=20*
Dep. variable: Relative spread (log)			
τ	-0.86***	-0.56***	-0.56***
	(-4.33)	(-4.41)	(-6.03)
%Δ	-57.82	-42.85	-42.77
N	80	120	160
Dep. variable: Trades (log)			
τ	0.99***	0.62**	0.51***
	(3.27)	(2.63)	(3.35)
%Δ	169.22	85.12	67.24
N	80	120	160
Dep. variable: Trading volume (log)			
τ	1.25***	0.63**	0.45**
	(3.12)	(2.25)	(2.47)
%Δ	247.49	88.62	57.36
N	80	120	160
Dep. variable: Volatility			
τ	-0.00	-0.00	0.00
	(-0.02)	(-0.31)	(0.82)
%Δ	-	-	-
N	80	120	160

Note: The table gives estimates of τ from the baseline fuzzy regression discontinuity design (equation 3). Relative spread is the end-of-day quoted spread divided by the end-of-day quote midpoint, log-transformed. Trades is the daily number of transactions, log-transformed. Value traded is the daily currency trading volume, log-transformed. These outcomes are first defined on the stock-day level, and subsequently averaged into a single stock-event observation. Volatility is the variance of the close-to-close returns process, computed at the stock-event level. τ is estimated in a two-stage least-squares (2SLS) specification, where predicted treatment status T_{ie} for stock i during event e, is used as an instrumental variable for actual treatment status, D_{ie}. $T_{ie} = \mathbf{1}[r_{ie} \geq 0]$, where r_{ie} is a ranking variable determined each June and December in $2008-2010$, by previous six months trading volume. r_{ie} has been normalized to zero by subtracting it from 25. $D_{ie} \times r_{ie}$ is included in the estimation to allow r_{ie} to vary with D_{ie}, and is instrumented with $T_{ie} \times r_{ie}$. Exogenous controls include the ranking variable r_{ie} and $ticksize_{ie}$. The 2SLS is estimated non-parametrically within bandwidths h. h indicates the number of stocks included on either side of the treatment cutoff ($r_{ie} = 0$). Percentage change, %Δ, is calculated as $e^{\tau}-1$. Standard errors are clustered at r_{ie}. t-statistics in parentheses. ** $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$*

Table 5: Robustness specifications

	Robustness specifications					
	1	*2*	*3*	*4*	*5*	*6*
Dep. variable: Relative spread (log)						
τ	-0.56***	0.11	0.07	0.15	0.10	-0.45***
	(-4.41)	(0.61)	(0.64)	(0.78)	(0.68)	(-2.88)
τ^{Diff}			-0.60***	-0.62***	-0.37***	
			(-6.81)	(-6.12)	(-3.68)	
%Δ	-42.85	11.36	-44.92	-46.01	-30.66	-36.55
N	120	360	480	480	461	111
Dep. variable: Trades (log)						
τ	0.62**	0.03	-0.05	-0.01	0.03	0.60**
	(2.63)	(0.07)	(-0.19)	(-0.03)	(0.09)	(2.30)
τ^{Diff}			0.79***	0.79***	0.56***	
			(4.36)	(3.86)	(2.79)	
%Δ	85.12	2.61	121.23	119.70	74.77	81.50
N	120	360	480	480	461	111
Dep. variable: Trading volume (log)						
τ	0.63**	0.02	-0.07	-0.10	0.02	0.47
	(2.25)	(0.05)	(-0.19)	(-0.19)	(0.04)	(1.29)
τ^{Diff}			0.79***	0.81***	0.43*	
			(3.57)	(3.16)	(1.83)	
%Δ	88.62	2.45	121.14	124.98	53.06	60.48
N	120	360	480	480	461	111
Dep. variable: Volatility						
τ	-0.00	0.00	0.00	0.00	0.00	0.00
	(-0.31)	(0.79)	(0.44)	(0.66)	(0.36)	(1.05)
τ^{Diff}			0.00	-0.00	0.00	
			(0.00)	(-0.13)	(1.30)	
%Δ	-	-	-	-	-	-
N	120	360	480	480	461	111
Placebo	No	Yes	No	No	No	No
Pre-Post	No	No	Yes	Yes	Yes	No
Mover dummies	No	No	No	Yes	Yes	No
Controls	No	No	No	No	Yes	No
Fragmentation	No	No	No	No	No	Yes

Note: The table gives estimates of τ from extensions of the fuzzy regression discontinuity design (equation 3). The baseline specification is a two-stage least-squares (2SLS) approach, where predicted treatment status T_{ie} for stock i during event e, is used as an instrumental variable for actual treatment status, D_{ie}. $T_{ie} = \mathbf{1}[r_{ie} \geq 0]$, where r_{ie} is a ranking variable determined each June and December in $2002 - 2010$, by previous six months trading volume. r_{ie} has been normalized to zero by subtracting it from 25. $D_{ie} \times r_{ie}$ is included in the estimation to allow r_{ie} to vary with D_{ie}, and is instrumented with $T_{ie} \times r_{ie}$. Exogenous controls include the ranking variable r_{ie} and $ticksize_{ie}$. The 2SLS is estimated within a bandwidth $h = 15$. h indicates the number of stocks included on either side of the treatment cutoff $r_{ie} = 0$. Column one gives the baseline specification, using data from $2008 - 2010$. Column two applies the baseline specification to the period $2002 - 2007$. Column three estimates the difference-in-differences between treatment periods $(2008 - 2010)$ and placebo periods $(2002 - 2007)$. Column four adds to the difference-in-differences model separate dummy variables for stocks moving in and out of the OBX list. Column five adds to column four a set of control variables (market capitalization (log), stock price (log), price-to-book (log), shares issued (log), total equity (log), total assets (log), operating profit, operating income, and cash and deposits (log)). Column six adds to the baseline specification a proxy for order flow fragmentation. This proxy is defined as the share of currency trading volume that occurs on all trading venues (dark pools, lit order books, SIs, and other OTC) excluding OSE, relative to the total currency trading volume across all trading venues including OSE. Percentage change, %Δ, is calculated as $e^{\tau}-1$. Standard errors are clustered at r_{ie}. t-statistics in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Summary statistics by trader group

	μ	σ	Min.	Median	Max.	N
Retail						
Intraday returns	-5.91	61.54	-207.75	-1.49	219.63	1901
Currency volume	5.62	9.75	0.00	2.03	100.10	1874
Share volume	444.37	1154.89	0.03	45.71	16116.30	1874
Price paid	58.25	62.34	1.01	36.90	371.17	1874
Trades	112.81	169.58	1.00	51.00	1566.00	1874
Trade value	42663.09	25056.54	576.50	37719.53	169587.50	1874
Institutions^f						
Intraday returns	0.84	63.65	-207.75	-1.45	219.63	1895
Currency volume	12.95	23.77	0.00	5.01	365.06	1872
Share volume	465.81	1016.11	0.00	124.28	10702.63	1872
Price paid	58.68	62.45	1.01	36.91	368.86	1872
Trades	215.66	244.67	1.00	122.00	2443.00	1872
Trade value	47824.32	46988.93	63.00	40605.51	856272.13	1872
Institutions^d						
Intraday returns	3.37	42.72	-207.75	1.50	219.63	1910
Currency volume	23.16	40.52	0.00	10.85	792.38	1897
Share volume	1094.15	2684.03	0.26	267.40	32146.63	1897
Price paid	58.18	62.15	1.01	36.83	368.63	1897
Trades	316.64	335.94	1.00	219.00	2527.00	1897
Trade value	60948.23	53079.29	3221.67	50682.63	1204010.50	1897

Note: The table provides summary statistics separately for the trading of retail investors, foreign institutions ($Institutions^f$), and domestic institutions ($Institutions^d$). Observations are at the stock-day-trader group level, aggregated from transaction-level data covering stocks ranked $r_{ie} \in [-15, 14]$, during the four trader anonymity events in the period $2008 - 2010$. Intraday returns are computed as $\frac{sell_{itg}^{Value} - buy_{itg}^{Value} + \left(buy_{itg}^{shares} - sell_{itg}^{shares}\right) \times ClosingPrice_{it}}{sell_{itg}^{Value} + buy_{itg}^{Value}}$, where $sell_{itg}^{Currency} - buy_{itg}^{Currency}$ is the profit from intraday trading. The term $buy_{itg}^{shares} - sell_{itg}^{shares}$ is the end-of-day position, assuming zero inventory at the beginning of each day, which is evaluated at the closing price. $sell_{itg}^{Currency} + buy_{itg}^{Currency}$ is the overall traded currency volume. Intraday returns are expressed in basis points, and are winsorized at the 1 per cent level. Currency volume is the daily total trading volume, in millions NOK. Share volume is the daily share volume, in thousand shares. Price paid is the daily average per-share price paid, in NOK. Trade value is the daily average transaction value, in NOK. In contrast to intraday returns, which are computed over both buy and sell transactions, Currency volume, Share volume, Price paid, Trades, and Trade value, are computed over buy transactions only, to avoid double-counting transactions. The table lists means (μ), standard deviations (σ), minimum (Min.) and maximum values (Max.), medians, and number of observations (N).

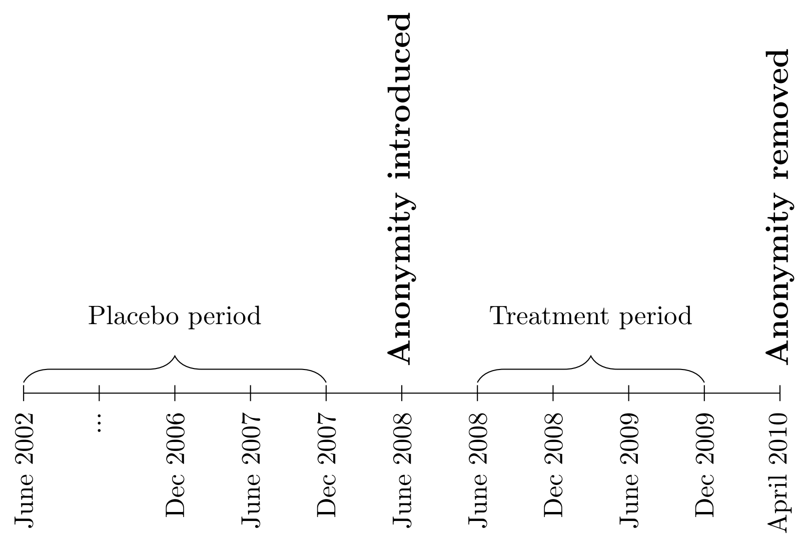
Table 7: Heterogeneity in trader response

	Trader group		
	Retail	*Institutions^f*	*Institutions^d*
Dep. variable: Trades (log)			
τ	0.08	1.11**	0.63**
	(0.14)	(2.43)	(2.72)
%Δ	8.42	202.43	87.13
N	120	120	120
Dep. variable: Trading volume (log)			
τ	0.16	1.45**	0.72**
	(0.24)	(2.40)	(2.10)
%Δ	17.02	326.69	105.82
N	120	120	120

Note: The table gives estimates of τ from the fuzzy regression discontinuity design (equation 3). The RD design is estimated separately for retail investors, foreign institutions ($Institutions^f$), and domestic institutions ($Institutions^d$). The outcomes considered are the daily number of trades (log) and daily monetary trading volume (log). These outcomes are first computed on the stock-day-trader group level, then averaged into a single stock-event-trader group observation. Trading volume and Trades are computed using buy transactions only, to avoid double-counting transactions. τ is estimated in a two-stage least-squares specification, where predicted treatment status T_{ie} for stock i during event e, is used as an instrumental variable for actual treatment status, D_{ie}. $T_{ie} = \mathbf{1}[r_{ie} \geq 0]$, where r_{ie} is a ranking variable determined each June and December in $2008 - 2010$, by previous six months trading volume. r_{ie} has been normalized to zero by subtracting it from 25. $D_{ie} \times r_{ie}$ is included in the estimation to allow r_{ie} to vary with D_{ie}, and is instrumented with $T_{ie} \times r_{ie}$. Exogenous controls include the ranking variable r_{ie} and $ticksize_{ie}$. The 2SLS is estimated within a bandwidth $h = 15$. h indicates the number of stocks included on either side of the treatment cutoff ($r_{ie} = 0$). Percentage change, %Δ, is calculated as $e^{\tau}-1$. Standard errors are clustered at r_{ie}. t-statistics in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

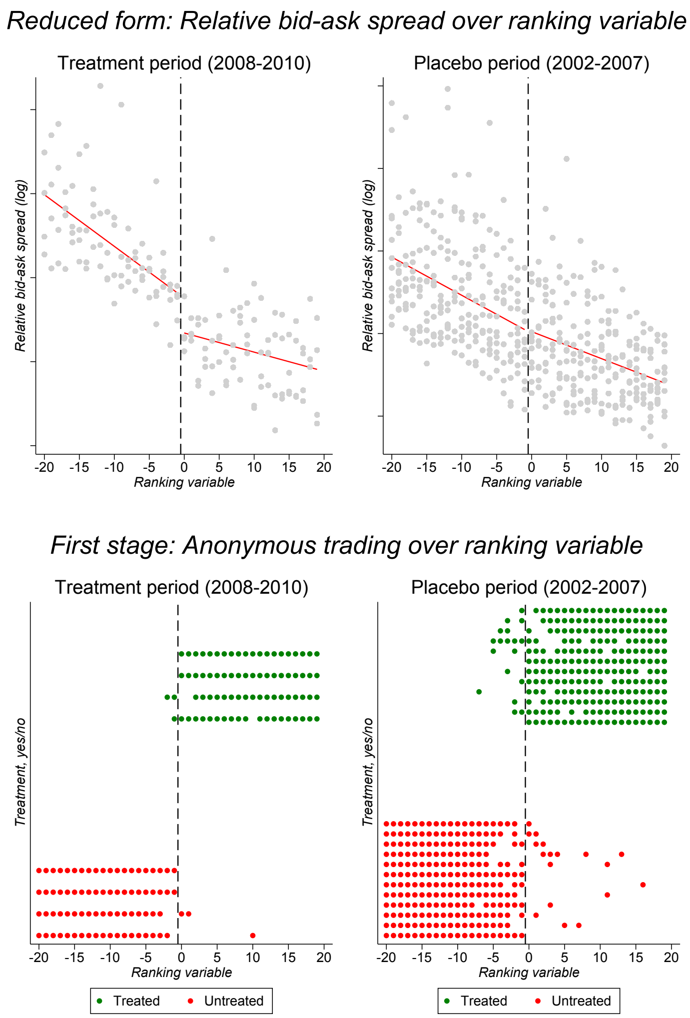
10 Figures

Figure 1: Time-line



Note: The figure presents a time-line of the introduction and removal of anonymous trading at the Oslo Stock Exchange (OSE). Each tick on the time-line represents a revision of the OBX list composition. The OBX list is composed of the most traded stocks at OSE, and the composition is revised twice a year (June and December). OSE introduced post-trade anonymity of brokerages on June 2, 2008 for constituents of the OBX list. Anonymity was removed April 12, 2010. In the period June 2 to April 12 the OBX list was revised four times, each time giving anonymity to a new set of constituent stocks (Treatment period). The OBX list was also created before June 2, 2008 but constituents did not receive anonymity (Placebo period).

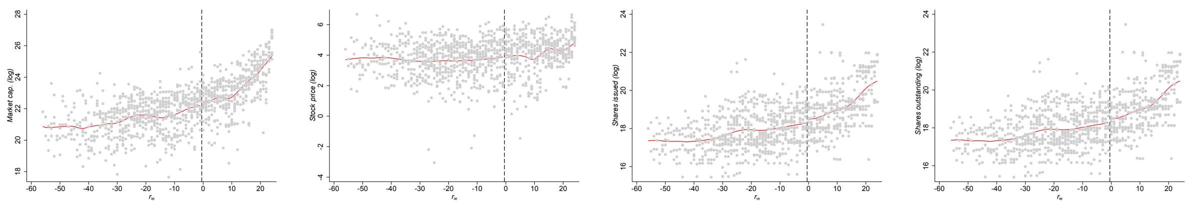
Figure 2: Illustration empirical design



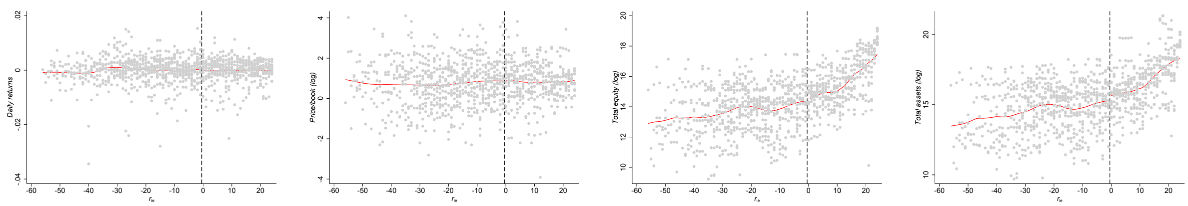
Note: The figure illustrates the fuzzy regression discontinuity (FRD) design applied to the logarithm of relative bid-ask spreads, a commonly used measure of illiquidity and transaction costs. The bottom panel relates treatment assignment to the ranking variable. Stocks are ranked semi-annually (June and December) based on previous six months trading activity. All observations to the right of the vertical break are intended for treatment based on this ranking variable. The ranking variable (r_{ie}) has been normalized to zero by subtracting it from 25. Green observations receive treatment, red observations do not. In the period 2008-2010, treatment implies anonymous trading and OBX index constituency. In the period 2002-2007, treatment implies OBX index constituency alone. The top panel relates relative bid-ask spreads to the same ranking variable. Linear regressions are fit separately on both sides of the vertical break. The FRD design estimates the effect of anonymous trading on relative bid-ask spreads as the vertical distance between regression intercepts at the vertical break, properly scaled by the treatment probability discontinuity at the same point.

Figure 3: Smoothness of stock characteristics

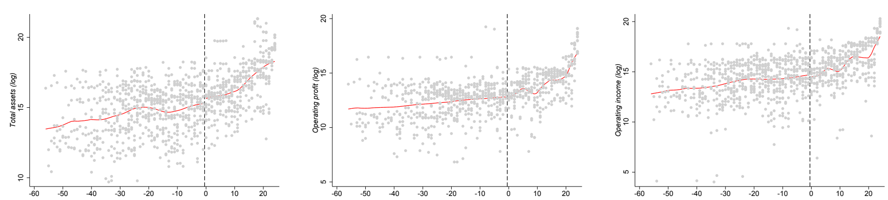
(a) Market cap. (log) (b) Stock price (log) (c) Shares issued (log) (d) Shares outstanding (log)



(e) Daily returns (f) Price/book (log) (g) Total equity (log) (h) Total assets (log)

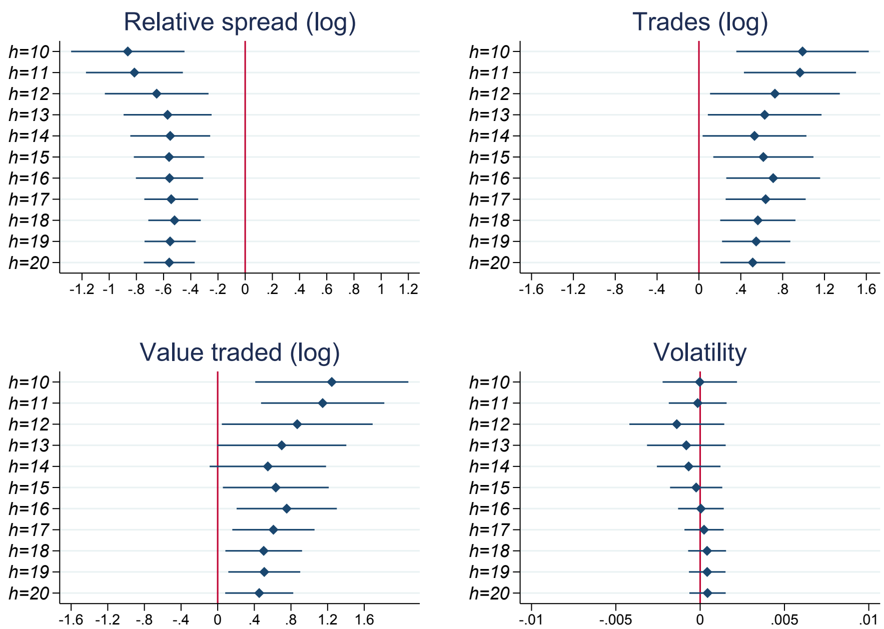


(i) Cash and deposits (log) (j) Operating profit (log) (k) Operating income (log)



Note: The figure presents evidence on the smoothness of selected stock characteristics across the treatment threshold $r_{ie} = 0$ for all realizations of r_{ie} in the period $2002 - 2010$. The characteristics are market capitalization (log), stock price (log), shares issued (log), daily returns, price-to-book (log), total equity (log), operating profits (log), and operating income (log). The figure relates these characteristics to the ranking variable, r_{ie}, which is computed semi-annually (June and December) based on previous six months trading volume. Local polynomial regressions (red) are fit separately on both sides of the vertical break ($r_{ie} = 0$).

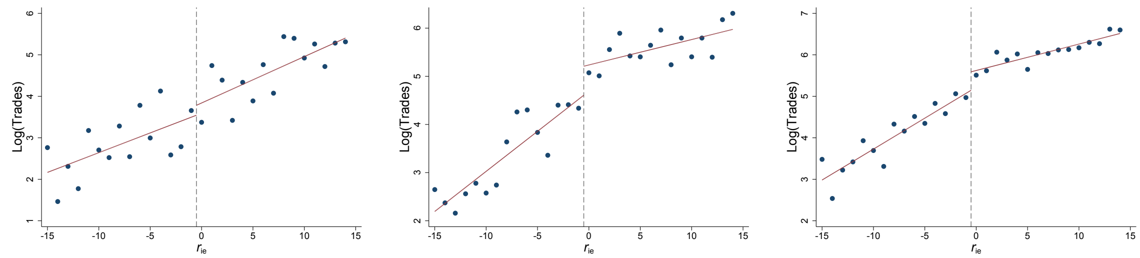
Figure 4: Coefficient estimates and bandwidth choice



Note: The figure presents estimates of τ from the fuzzy regression discontinuity (RD) design (equation 3), with corresponding 95% confidence bands. Standard errors are corrected for clustering at r_{ie}, with a finite sample adjustment. The RD design is estimated non-parametrically within bandwidths h. h indicates the number of stocks included on either side of the treatment cutoff ($r_{ie} = 0$). The figure presents estimates from $h \in [10, 20]$. τ is estimated in a two-stage least-squares (2SLS) specification, where predicted treatment status T_{ie} for stock i during event e, is used as an instrumental variable for actual treatment status, D_{ie}. $T_{ie} = \mathbf{1}\left[r_{ie} \geq 0\right]$, where r_{ie} is a ranking variable determined in each June and December in $2008 - 2010$ by previous six months trading volume. r_{ie} has been normalized to zero by subtracting it from 25. $D_{ie} \times r_{ie}$ is included in the estimation to allow r_{ie} to vary with D_{ie}, and is instrumented with $T_{ie} \times r_{ie}$. Exogenous controls include the ranking variable r_{ie} and $ticksize_{ie}$.

Figure 5: Log(Trades) over r_{ie}, by trader group

(a) *Retail* (b) $Institutions^f$ (c) $Institutions^d$



Note: The figure plots the natural logarithm of number of trades over the ranking variable r_{ie}, separately for retail investors, foreign institutions ($Institutions^f$), and domestic institutions ($Institutions^d$). Stocks to the right of the vertical break ($r_{ie} = 0$) are predicted for anonymous trading while stocks to the left of the vertical break are predicted for transparency. Log(Trades) is first computed on the stock-day-trader group level, then averaged into a single stock-event-trader group observation for each of the four realizations of r_{ie} in the period $2008 - 2010$. Each observation in the figure represents the average across these four realizations of r_{ie}.

A Appendix

A.1 Overview: Trader anonymity policy changes

The choice of transparency is one of the most hotly debated issues in equity market regulation, as it can affect price discovery, liquidity, and the distribution of rents between market participants (Foucault et al. 2013). Market transparency is defined by the amount of trading information, on prices, quantities, or identities, that is available to participants. Desirable transparency is determined by the individual trading venue and varies substantially between markets and over time.

Many leading stock exchanges, such as the Nasdaq, London Stock Exchange, and Deutsche Börse, have reduced transparency over the last decade by increasing trader anonymity. Practically, trader anonymity is implemented by concealing trader identifiers from orders in the order book (pre-trade anonymity) and/or from completed transactions in the trade feed (post-trade anonymity). Table A.1 summarizes recent stock exchange policy changes to both forms of trader anonymity. The summary focuses on trader anonymity policy changes analyzed in academic articles, or policy changes that have received attention by the media, and is not exhaustive.

Table A.1: Summary of trader anonymity policy changes

Exchange	**Event date**	**Policy change**	**Source**
Copenhagen	March 13, 2006	Introduction pre-trade anonymity	Nasdaq OMX note [a]
Frankfurt	March 27, 2003	Introduction post-trade anonymity	Hachmeister and Schiereck (2010)
Helsinki	March 13, 2006	Introduction pre-trade anonymity	Thurlin (2009)
Helsinki	June 2, 2008	Introduction post-trade anonymity	Dennis and Sandås (2015)
Helsinki	April 14, 2009	Removal post-trade anonymity	Nasdaq OMX note [b]
Istanbul	October 8, 2010	Introduction post-trade anonymity	ISE note [c]
London	February 26, 2001	Introduction post-trade anonymity	Friederich and Payne (2014)
Nasdaq	December, 2002	Increased pre-trade anonymity	Benhami (2006)
Nasdaq	October, 2003	Increased post-trade anonymity	Benhami (2006)
Oslo	October 22, 2007	Introduction pre-trade anonymity	OSE officials
Oslo	June 2, 2008	Introduction post-trade anonymity	This paper
Oslo	April 12, 2010	Removal post-trade anonymity	This paper
Paris	April 23, 2001	Introduction pre-trade anonymity	Foucault et al. (2007)
Reykjavik	June 2, 2008	Introduction post-trade anonymity	Dennis and Sandås (2015)
Riga	November 1, 2007	Introduction pre-trade anonymity	Nasdaq Baltic note [d]
Seoul	November 25, 1996	Removal post-trade anonymity	Pham et al. (2014)
Seoul	October 25, 1999	Removal pre-trade anonymity	Comerton-Forde et al. (2005)
Stockholm	March 13, 2006	Introduction pre-trade anonymity	Nasdaq OMX note [a]
Stockholm	June 2, 2008	Introduction post-trade anonymity	Dennis and Sandås (2015)
Stockholm	April 14, 2009	Removal post-trade anonymity	Nasdaq OMX note [b]
Sydney	November 28, 2005	Introduction pre-trade anonymity	Comerton-Forde and Tang (2009)
Tallinn	November 1, 2007	Introduction pre-trade anonymity	Nasdaq Baltic note [d]
Tokyo	June 30, 2003	Introduction pre-trade anonymity	Comerton-Forde et al. (2005)
Toronto	March 22, 2002	Introduced voluntary trader anonymity	Comerton-Forde et al. (2011)
Vilnius	November 1, 2007	Introduction pre-trade anonymity	Nasdaq Baltic note [d]

Note: The table gives an overview of stock exchange policy changes in trader anonymity. [a] *Changing the Nordic Market Microstructure*, April 2007.

[b] *NASDAQ OMX changes Post Trade Anonymity for the equity market trading in stockholm and Helsinki*, March 2009.

[c] *Markets and Operations*, October 2011.

[d] *Implementation of pre-trade anonymity*, November 2007.

A.2 First-stage regressions

In Section 5, I use a two-stage least-squares approach to estimate the causal effect of trader anonymity on stock outcomes. The specification in Section 5 is a fuzzy regression discontinuity (RD) design, where the predicted treatment T_{ie} (predicted by previous six month trading volume) is used as an instrumental variable for the actual treatment D_{ie}. In this section, I report the first-stage regressions of the fuzzy RD design. The first-stage regressions relate D_{ie} to T_{ie} and the ranking variable r_{ie}:

$$D_{ie} = b + \phi r_{ie} + \psi T_{ie} + \omega T_{ie} \times r_{ie} + \varphi ticksize_{ie} + \varpi_{ie}. \tag{7}$$

Estimates of ψ are presented in Table A.2. I present estimates separately for bandwidths $h = 10$, $h = 15$, and $h = 20$. The bandwidth is the number of stocks included on either side of the treatment cutoff ($r_{ie} = 0$). Standard errors are clustered by r_{ie} with a finite sample adjustment; t-statistics are in parentheses.

ψ increases from a low of 0.55 at $h = 10$ to a high of 0.76 at $h = 20$. All point estimates are statistically significant, t-statistics increasing from 4.16 ($h = 10$) to 7.93 ($h = 20$). Crossing the treatment threshold increases the probability of treatment by 55% - 76%. The larger the bandwidth, the stronger the instrument. The centered R^2 varies around 0.80 for all bandwidths. R^2 is not necessarily monotonically increasing in h because of variation in $ticksize_{ie}$. The Angrist-Pischke multivariate F test of excluded instruments (statistic not reported in table) shows that T_{ie} is a sufficiently strong instrument for all bandwidths.

Table A.2: First-stage regressions of fuzzy RDD

	Bandwidth		
	h=10	*h=15*	*h=20*
ψ	0.55***	0.69***	0.76***
	(4.16)	(6.09)	(7.93)
R^2	0.80	0.82	0.87
F	216.92	287.85	812.66
N	80	120	160

** p < 0.10, ** p < 0.05, *** p < 0.01*

A.3 Robustness tests: Polynomials in r_{ie}

In the main analysis, I investigate how trader anonymity at the Oslo Stock Exchange (OSE) affects market quality by using a regression discontinuity (RD) design. With the RD design, I find that anonymity increases stock liquidity (smaller relative bid-ask spreads) and trading activity (number of trades and traded value) but has no effect on returns volatility. Unbiased estimation of the RD effects requires an assumption about the functional form of the relationship between the running variable r_{ie} and outcomes y_{ie}. The RD literature has proposed two main approaches to estimating the RD design when this functional form is unknown. The first approach, which I use in the main text, is to estimate the RD design non-parametrically with so-called local linear regressions. This approach implies estimating linear regressions within a confined estimation range surrounding the treatment threshold $r_{ie} = 0$.

The second approach, which I take in this appendix, is to expand the estimation range and allow for a flexible relationship between y_{ie} and r_{ie} through a polynomial expansion in r_{ie}. The benefits of this approach are twofold. First, using a larger portion of the overall sample may increase the statistical precision of the estimation procedure. In this section, I use a bandwidth $h = 25$ (the number of stocks included on either side of $r_{ie} = 0$). This is the widest bandwidth attainable in my setting, while still preserving a symmetric sample surrounding $r_{ie} = 0$. Second, reporting estimates from a wider range of regression discontinuity specifications increases the credibility and transparency of the empirical design. I estimate the following equation set by two-stage least-squares:

$$y_{ie} = \alpha_1 + \nu f\left(r_{ie}\right) + \tau D_{ie} + \delta ticksize_{ie} + \varepsilon_{ie} \tag{8}$$

$$D_{ie} = \alpha_0 + \phi f\left(r_{ie}\right) + \psi T_{ie} + \varphi ticksize_{ie} + \varpi_{ie}, \tag{9}$$

where y_{ie} is the outcome of interest (e.g. liquidity, trading activity); $f\left(r_{ie}\right)$ is a global polynomial function of the ranking variable r_{ie}; D_{ie} is an indicator for anonymous trading; and $T_{ie} = \mathbf{1}\left[r_{ie} \geq 0\right]$, where r_{ie} has been normalized to zero by subtracting r_{ie} from 25 (see Section 4 for details). Notice that I do not include interaction terms between $f\left(r_{ie}\right)$ and D_{ie} or T_{ie}. Such interaction terms allow for a more flexible relationship between r_{ie} and y_{ie}, which may reduce the potential for bias in the RD estimates, but at the same time create an expanding set of endogenous variables that need to be instrumented. In this section, I sacrifice some flexibility in order to preserve statistical power. In Table A.3, I present estimates of τ from five models with different polynomial specifications of the relationship between r_{ie} and y_{ie}.

Table A.3: Robustness: Polynomials in r_{ie}

	Polynomial specification				
	Linear	*Quadratic*	*Cubic*	Quartic	Quintic
Dep. variable: Relative spread (log)					
τ	-0.53***	-0.54***	-0.64***	-0.64***	-0.64***
	(-3.84)	(-4.56)	(-3.83)	(-4.96)	(-3.17)
%Δ	-41.34	-41.70	-47.30	-47.06	-47.32
N	200	200	200	200	200
Dep. variable: Trades (log)					
τ	0.55**	0.56***	0.62**	0.61***	0.87***
	(2.47)	(3.22)	(2.19)	(2.85)	(2.87)
%Δ	73.24	75.30	86.16	84.57	138.92
N	200	200	200	200	200
Dep. variable: Trading volume (log)					
τ	0.44*	0.45*	0.65*	0.63***	0.89**
	(1.80)	(1.98)	(1.89)	(2.75)	(2.43)
%Δ	55.17	56.16	91.06	88.69	143.67
N	200	200	200	200	200
Dep. variable: Volatility					
τ	0.00	0.00	0.00	0.00	-0.00
	(1.17)	(1.17)	(0.01)	(0.01)	(-0.00)
%Δ	0.06	0.06	0.00	0.00	-0.00
N	200	200	200	200	200

Note: The table provides estimates of τ from five separate separate fuzzy regression discontinuity designs. The specification is estimated using the trader anonymity events in the period $2008 - 2010$. The second stage regression specification is $y_{ie} = \alpha + \nu f(r_{ie}) + \tau D_{ie} + \delta ticksize_{ie} + \varepsilon_{ie}$. τ is estimated in a two-stage least-squares (2SLS) approach, where predicted treatment status T_{ie} for stock i during event e, is used as an instrumental variable for actual treatment status, D_{ie}. $T_{ie} = \mathbf{1}[r_{ie} \geq 0]$, where r_{ie} is a ranking variable determined each June and December in $2008 - 2010$, by previous six months trading volume. Exogenous controls include the ranking variable r_{ie} and $ticksize_{ie}$. The five models in this table are estimated using different polynomial specifications on the relationship between r_{ie} and outcomes y_{ie}, ranging from a 1st order polynomial (linear) to a fifth order polynomial (quintic). The 2SLS is estimated within a bandwidth $h = 25$. h indicates the number of stocks included on either side of the treatment cutoff ($r_{ie} = 0$). Percentage change, %Δ, is calculated as $e^{\tau}-1$. Standard errors are clustered at r_{ie}. t-statistics in parentheses.

** $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$*

A.4 Robustness test: Dynamic RD design

In the main analysis, I investigate how trader anonymity at the Oslo Stock Exchange (OSE) affects stock-level outcomes by using a regression discontinuity (RD) design. With the RD design, I find that trader anonymity increases stock liquidity (smaller relative bid-ask spreads) and trading activity (number of trades and traded value) but has no effect on returns volatility. The RD design I employ in the main analysis is static in the sense that it does not take into account that uptake of anonymous trading in one period potentially affects the probability of receiving anonymous trading in subsequent periods. Such dynamics can arise because 1) anonymous trading is assigned based on trading volume, and 2) anonymous trading increases trading trading volume (see Table 4).

In this appendix, I allow for such dynamic effects by estimating a dynamic fuzzy regression discontinuity design. The specification I employ is inspired by Cellini et al. (2010) and Cuñat et al. (2012) but takes into account that there is imperfect compliance to the trader anonymity assignment rule. I estimate the following equation set by 2SLS:

$$y_{ie} = \alpha_1 + f\left(r_{ie}\right) + \tau D_{ie} + \delta ticksize_{ie} + \left[\sum_{t=1}^{t=j}\left(\theta_{e-t} D_{i,e-t} + f\left(r_{i,e-t}\right)\right)\right] + \varepsilon_{ie} \quad (10)$$

$$D_{ie} = \alpha_0 + f\left(r_{ie}\right) + \psi T_{ie} + \varphi ticksize_{ie} + \left[\sum_{t=1}^{t=j}\left(\Omega_{e-t} T_{i,e-t} + f\left(r_{i,e-t}\right)\right)\right] + \varpi_{ie}, \quad (11)$$

where y_{ie} is some outcome (e.g. stock liquidity); stock i during event e is predicted for anonymous trading if $r_{ie} \geq 0$; and D_{ie} is an indicator variable for anonymous trading. Since there is imperfect compliance to the main assignment rule $r_{ie} \geq 0$, I use $T_{ie} = \mathbf{1}\left[r_{ie} \geq 0\right]$ as an instrumental variable (IV) for actual treatment D_{ie}. I include a full set of lags $D_{i,e-t}$, that are instrumented by the corresponding $T_{i,e-t}$, to account for the potential impact of previous treatment status on contemperaneous outcomes. Both D_{ie} and T_{ie} are constrained to zero for all events e before trader anonymity was introduced. I include a full set of lags in $f\left(r_{ie}\right)$ as exogenous regressors. Recall that r_{ie} was determined also in periods before trader anonymity was introduced, and its inclusion as a dynamic regressor controls for the impact of previous high or low rankings on current outcomes. $ticksize_{ie}$ is added to control for stock-level differences in tick size. The treatment effect τ in equation 10 can now be interpreted as the contemporaneous effect of anonymous trading in event e, net of effects operating through successive trader anonymity assignments.

I follow Cellini et al. (2010) and Cuñat et al. (2012) and estimate the dynamic RD design parametrically. To do so, I employ the polynomial expansion approach described in Appendix A.3 with a fifth order polynomial in $f\left(r_{ie}\right)$. For transparency and robustness, I estimate the dynamic RD design separately for one, two, and three lags in D_{ie}, T_{ie}, and r_{ie}. Estimates of τ are presented in Table A.4. Notice from the table that the number of observations decreases in the number of lags applied. This is because more lags require a stock to have been eligible for trader anonymity, by being an OSEBX index listed stock, for consecutive periods. Consequently, the number of observations will be lower in the dynamic specification than in the baseline polynomal approach (Appendix A.3).

Table A.4: Robustness: Dynamic RD design

	Dynamic specification		
	One lag	*Two lags*	*Three lags*
Dep. variable: Relative spread (log)			
τ	-0.58***	-0.48*	-0.48*
	(-2.94)	(-1.81)	(-1.82)
%Δ	-44.25	-37.87	-38.31
N	189	184	177
Dep. variable: Trades (log)			
τ	0.76**	0.90**	0.96***
	(2.40)	(2.47)	(2.78)
%Δ	112.84	146.99	161.98
N	189	184	177
Dep. variable: Trading volume (log)			
τ	0.88**	1.02**	1.06**
	(2.35)	(2.49)	(2.63)
%Δ	142.25	178.48	187.50
N	189	184	177
Dep. variable: Volatility			
τ	-0.00	-0.00	-0.00
	(-0.36)	(-0.06)	(-0.20)
%Δ	-	-	-
N	189	184	177

Note: The table provides estimates of τ from a dynamic fuzzy regression discontinuity design. The specification is estimated using the trader anonymity events in the period $2008-2010$. The second-stage regression is $y_{ie} = \alpha_1 + f(r_{ie}) + \tau D_{ie} + \delta ticksize_{ie} + \left[\sum_{t=1}^{t=j} \left(\theta_{e-t} D_{i,e-t} + f(r_{i,e-t})\right)\right] + \varepsilon_{ie}$. τ is estimated in a two-stage least-squares (2SLS) approach, where predicted treatment status T_{ie} for stock i during event e, is used as an instrumental variable for actual treatment status, D_{ie}. Similarly, $T_{i,e-t}$ is used as an instrument for $D_{i,e-t}$. $T_{ie} = \mathbf{1}[r_{ie} \geq 0]$, where r_{ie} is a ranking variable determined each June and December in $2002-2010$, by previous six months trading volume. Exogenous controls include a full set of fifth order polynomials in r_{ie} and $ticksize_{ie}$. The 2SLS is estimated within a bandwidth $h = 25$. h indicates the number of stocks included on either side of the treatment cutoff ($r_{ie} = 0$). Percentage change, %Δ, is calculated as $e^{\tau}-1$. Standard errors are clustered at r_{ie}. t-statistics in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.5 Generating $Frag_{ie}$

In Section 6.3, I use a variable $Frag_{ie}$ to control for the effect of order flow fragmentation on stock outcomes (e.g. stock liquidity) in the fuzzy regression discontinuity design. In this section, I describe how $Frag_{ie}$ is generated.

To generate $Frag_{ie}$, I use weekly frequency data on pan-European trading activity in all stocks at the Oslo Stock Exchange (OSE) in the period $2008 - 2010$. The data is obtained from Fidessa, a commercial provider of software, trading systems, and market data to both buy-side and sell-side investors. The Fidessa data provide weekly accounts of total trading volume, in both currency values and shares traded, and number of transactions, for all OSE stocks, separately for trading on all European trading venues. All trading venues in the data are defined by Fidessa as either lit order books (LIT), dark order books (DARK), over-the-counter (OTC), or systematic internaliser (SI). For unknown reasons, six OSE firms are missing from the Fidessa data. Their stock tickers are GOGL, SNI, STXEUR, AWO, WWI, and SAS NOK. Results are insensitive to treating these observations as missing, or as zeros. In Section 6.3, I treat these observations as missing; hence the smaller number of observations.

I capture order flow fragmentation by the share of trading volume that takes place on other trading venues than OSE. I make no distinction between trading on LIT, DARK, OTC, or SI trading venues. I define $Frag_{it}$ for stock i on date t as the share of currency trading volume that occurs on all trading venues excluding OSE, relative to the total currency trading volume across all trading venues including OSE. That is, if A^+ is the set of all trading venues v in the Fidessa data, including OSE, and A^- is the set of trading venues excluding OSE, then $Frag_{it}$ is defined as:

$$Frag_{it} = \frac{\Sigma_{v \in A^-} Volume_{it}}{\Sigma_{v \in A^+} Volume_{it}}. \tag{12}$$

I average this measure within each event e (defined in Section 3), and obtain $Frag_{ie}$. The results in Section 6.3 remain quantitatively similar if the sets A^- and A^+ include only LIT trading venues, or if the sets include both LIT and DARK trading venues. The results in Section 6.3 also remain similar if fragmentation is instead measured by a so-called Herfindalh-Hirschman Index (HHI).

A.6 Difference-in-differences

In the main analysis, I investigate how trader anonymity at the Oslo Stock Exchange (OSE) affects market quality by using a regression discontinuity (RD) design. With the RD design, I find that trader anonymity increases liquidity (smaller relative bid-ask spreads) and trading activity (number of trades and traded value) but has no effect on returns volatility. These results may be driven by other market structure developments than anonymity, taking place at the same time. For example, the main sample period ($2008 - 2010$) is characterized by, among other things, an explosion in high-frequency trading (e.g. Jørgensen et al. 2014, Angel et al. 2011,2013), aggressive use (by stock exchanges) of new fee structures, such as maker-taker fees (e.g. Malinova and Park 2015), and a financial crisis. If these developments systematically correlate with OBX list membership, they may bias my estimates.

To minimize the potential for time-varying confounders, I employ a short-run difference-in-differences specification, surrounding only the first assignment of anonymous trading, on June 2, 2008. On this date, anonymous trading was introduced for the 25 stocks in the OBX list, while trading in all other stocks remained non-anonymous. Shortly after, on June 20, the composition of anonymously and non-anonymously traded stocks was revised as part of a routine revision of the OBX list. Therefore, I exclude the period June 2 to June 17 and consider June 20, 2008 to be the 'treatment date' of interest. I estimate the following DiD specification surrounding this date:

$$Y_{it} = a + \nu D_t^{Post} + \gamma D_i^{Treatment} + \tau D_{it}^{Post*Treatment} + \delta ticksize_{it} + \varepsilon_{it}, \tag{13}$$

where $D_t^{Post} = 1$ for all dates t after June 20, 2008 and 0 otherwise. $D_i^{Treatment} = 1$ for the treatment group and 0 for the control group. I define the treatment group as the sample of stocks traded anonymously as of June 20, 2008, and the control group as the sample of stocks traded non-anonymously as of June 20, 2008. $D_{it}^{Post*Treatment}$ is the interaction between D_{it}^{Post} and $D_{it}^{Treatment}$ which equals 1 for anonymously traded stocks in the treatment period and 0 otherwise. I control for stock-level differences in tick size by including $ticksize_{it}$. The treatment effect of anonymous trading is given by the coefficient τ in equation 13.

An added benefit of this simple difference-in-differences approach is that it allows for direct comparability with previous empirical work on trader anonymity, where equation 13 is the preferred specification (e.g. Friederich and Payne 2014, Dennis and Sandås 2015).

For further comparability with this existing literature, I define my sample period similar to that used by Dennis and Sandås (2015). Particularly, I estimate equation 13 using three months of data before and after June 20, 2008. Friederich and Payne 2014, in contrast, employ a sample with six months of data before and after their anonymity introduction date. Estimating equation 13 using six months of data before and after June 20, 2008, instead of three months before and after this date, delivers similar coefficient estimates of τ.

I estimate the difference-in-differences model separately for bandwidths $h = 5$, $h = 10$, $h = 15$, $h = 20$, and $h = 25$. h indicates the number of stocks included on either side of the marginal OBX stock ($r_{ie} = 0$). For example, when $h = 5$, the sample is restricted to the 10 stocks closest to the marginal anonymously traded stock. Restricting the sample this way offers two benefits. First, it provides a homogeneous sample of stocks, where it may be plausible that the common trend assumption of the DiD specification is satisfied. Second, it offers transparency and robustness to the estimation. The drawback of this approach, of course, is that specifications with small h have few observations, which may lead to noisy estimates of τ. For this reason, I consider $h = 25$ to be the main sample.

Volatility, in previous sections defined as the variance of close-to-close returns, is now proxied by the daily high price divided by the daily low price in order to have variation on a daily frequency. The remaining outcome variables — relative bid-ask spreads, number of trades, and trading volume — are defined as in previous analysis but now on a daily frequency.

Table A.5: Robustness: Difference-in-differences

	Bandwidth				
	h=5	*h=10*	*h=15*	*h=20*	*h=25*
Dep. variable: Relative spread (log)					
τ	-0.31**	-0.11	-0.16*	-0.21***	-0.21***
	(-2.35)	(-1.07)	(-1.88)	(-2.86)	(-3.10)
%Δ	-26.71	-10.82	-14.65	-18.85	-19.08
Adj. R^2	0.17	0.22	0.27	0.33	0.38
N	1128	2258	3386	4516	5640
Dep. variable: Trades (log)					
τ	0.36	0.20	0.27	0.24*	0.34***
	(0.83)	(0.82)	(1.55)	(1.75)	(2.73)
%Δ	44.04	21.80	30.70	27.64	40.18
Adj. R^2	0.15	0.36	0.42	0.54	0.60
N	1129	2259	3387	4517	5641
Dep. variable: Trading volume (log)					
τ	0.07	0.12	0.27**	0.24*	0.24**
	(0.26)	(0.80)	(2.20)	(1.97)	(2.25)
%Δ	6.83	12.71	31.11	26.57	27.20
Adj. R^2	0.13	0.37	0.45	0.55	0.59
N	1129	2259	3387	4517	5641
Dep. variable: Volatility					
τ	0.01	-0.00	-0.00	-0.00	-0.00
	(0.58)	(-0.18)	(-0.52)	(-0.86)	(-0.42)
%Δ	-	-	-	-	-
Adj. R^2	0.02	0.05	0.06	0.07	0.06
N	1129	2259	3387	4517	5641

Note: The table provides estimates from a difference-in-difference specification surrounding the first assignment of trader anonymity at the Oslo Stock Exchange, on June 2, 2008. Due to a change in the composition of anonymously traded stocks on June 18, 2008, I exlude all dates between June 2 and June 18. The sample period is defined as March 20, 2008, to September 20, 2008. The regression specification is $Y_{it} = a + \nu D_{it}^{Post} + \gamma D_{it}^{Treatment} + \tau D_{it}^{Post*Treatment} + \delta ticksize_{it} + \varepsilon_{it}$. $D_{it}^{post} = 1$ for all time periods after June 18, 2008, 0 otherwise. $D_{it}^{treatment} = 1$ for stocks traded anonymously as of June 18, 2008, 0 otherwise. $D_{it}^{post*treatment}$ is the interaction between D_{it}^{Post} and $D_{it}^{Treatment}$, and equals 1 for anonymously traded stocks in the post-treatment period, and 0 otherwise. The difference-in-differences model is estimated separately for bandwidths $h = 5$, $h = 10$, $h = 15$, $h = 20$, and $h = 25$. h indicates the number of stocks included on either side of the the marginal OBX stock ($r_{ie} = 0$). τ gives the treatment effect of trader anonymity. %Δ gives the percentage treatment effect for log coefficiencts, $e^{\tau} - 1$. Standard errors are clustered at the stock-level.

** $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$*

A.7 Trader classification

In Section 7, I explore the impact of trader anonymity on the trading activity of institutional and retail investors. In this appendix, I describe how traders are classified as institutional and retail.

The starting point of this trader classification is the transaction-level data described in Section 3. Each transaction in the data reveals the identity of both the buyer and the seller. Unlike some particularly detailed datasets — for example Barber et al. (2009) and Malinova and Park (2015) — the buyer and seller identities in my data are at the brokerage firm level and do not identify underlying accounts. This means that all inference on trader type will be based on observable characteristics at the brokerage level. van Kervel and Menkveld (2015) use a similar approach to identify high-frequency traders at the Nasdaq OMX.

The first step in the trader classification is to compile a list of brokerage firms that execute atleast one transaction during the sample periods defined in Section 3. Brokerages are identified in the data by ticker codes (e.g., XYZ). I translate all ticker codes into full brokerage firm names using membership lists obtained from the Oslo Stock Exchange (OSE). The final list holds 66 unique brokerage firms.

I proceed to hand-collect information on each active brokerage from company home pages, member descriptions at the OSE, and from various financial web sites such as Bloomberg Business. From these sources, I am able to infer, albeit noisily, whether a brokerage firm represents, for example, an investment bank catering to institutional or high-net-worth clients, such as Goldman Sachs or Deutche Bank, a market-maker, such as Knight Capital, or an online discount brokerage such as E-Trade.

I use this information to decompose the overall order flow into components of retail and institutional order flows. I begin by isolating order flow from online discount brokerages, who cater to individual investors. In total, I identify five active discount brokerages at the Oslo Stock Exchange in the period 2008 – 2010. These brokerages are Avanza Bank AB, E*Trade Danmark A/S, Net Fonds ASA, Nordnet Bank AB, and Skandiabanken AB.

The residual order flow, which, judging by brokerage firms' self-descriptions and Oslo Stock Exchange member descriptions, consists predominantly of investment banks catering to institutional clients, market makers, and high-frequency trading firms, is collectively referred to as 'institutional.' I follow Linnainmaa and Saar (2012) and further decompose the institutional order flow into components of domestic and foreign institutional order flows. Domestic brokerages include all Scandinavian brokerages or any foreign subsidiary registered as a Scandinavian company (*Aksjeselskap* (AS) *or Aktiebolag* (AB)). Brokerages head-quartered outside Scandinavia are considered foreign.

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