Trading anonymity and order anticipation*

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Abstract

Does it matter to market quality if broker identities are revealed after a trade and only to the two traders involved? We find that implementing full anonymity dramatically improves liquidity and reduces trader execution costs. To explain this, we compare theories based on asymmetric information to an order anticipation mechanism, where identity signals trader size, allowing strategic agents to predict the future order flow of large traders. Evidence supports the anticipation hypothesis: liquidity improves most in stocks where trading is heavily concentrated among a few brokers and in stocks susceptible to temporary price pressure. Also, only traders having large market-shares benefit from anonymity.

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In an electronic trading context, anonymity refers to whether the brokers that intermediate trading can be nominally identified by other participants. This can occur before the trade, if broker identities are shown alongside unexecuted orders on trading screens, or after the trade, if the IDs of the brokers are revealed. In this paper, we study the effects of introducing anonymity to trading on the London Stock Exchange (LSE). The anonymity change we look at is very different to that studied in previous work, as ID disclosure was initially very restricted on the LSE. Prior to the change, the market was already pre-trade anonymous and only the two parties involved in a trade learned each other's identities. With the introduction of a central counterparty (CCP) to electronic equity trading in London in February 2001, post-trade counterparty identification ceased, rendering the trading process completely anonymous.

Using data on 134 stocks from 6 months before the introduction of the CCP to 6 months after, we find that under full anonymity spreads decline by around 20%, the order book deepens significantly, and the price impacts of single trades and worked executions decrease substantially. A matched control sample of European and U.K. stocks that did not experience any anonymity change displays no such liquidity improvement.

Why did this seemingly small change in transparency cause such striking improvements in liquidity? Related work presents results from analysis of the introduction of *pre-trade* anonymity, studying markets around the times that exchanges stopped disclosing the identities of brokers alongside their unexecuted orders (Foucault, Moinas, and Theissen, 2007; Comerton-Forde and Tang, 2009). They find that market liquidity improved and explain this using an asymmetric information argument: revealing the identities of agents who are better informed before they trade broadcasts their information while, under anonymity, those agents can expose their orders to the market without fear that others will trade in front of them. Our results share some features with those in extant work, but they are hard to interpret along the same lines. Why would revealing identities only after a trade has been completed and only to the pair of traders involved lead to concerns about information leakage?

We proceed to shed light on the mechanism that generates our results. We compare the implications of two theories that relate anonymity to liquidity. The first relies on asymmetric information (AI) arguments – examples include Huddart, Hughes, and Levine (2001), Foucault, Moinas, and Theissen (2007), and Rindi (2008).[1] These models generate very different predictions. Huddart, Hughes, and Levine (2001) suggest that, with exogenous endowments of private information, post-trade anonymity degrades liquidity as it perpetuates information asymmetries. However, Rindi (2008) argues that if information acquisition is endogenous then anonymity may improve liquidity and efficiency as it strengthens agents' incentives to acquire information.

The second mechanism we consider is order anticipation (OA). Order anticipators use order flow data to predict the direction of future institutional trades and to profit from those predictions, perhaps by moving prices against the anticipated trader or trading in front of them. This style of opportunistic trading was described several years ago, most clearly by Harris (1997) and Harris (2002). Harris (2002) devotes

[1]Note, however, that the latter two papers focus on pre-trade anonymity.

a whole chapter to OA, describing it as "parasitic." Harris (1997) states that "To trade profitably, [anticipators] do not need to know why traders want to trade. They merely need to know that a large trader strongly intends to complete a trade." This statement makes it clear that OA is one of a family of strategies that profit from predictability in order flow direction. Those strategies include strategic trading around index rebalances or fire sales (Harris and Gurel, 1986; Beneish and Whaley, 1996; Coval and Stafford, 2007) and "predatory trading" (Attari, Mello, and Ruckes, 2005; Brunnermeier and Pedersen, 2005; Carlin, Lobo, and Viswanathan, 2007). In turn, they all rely on the ability of a trader to move prices and thus the existence of price pressure effects, a thread originating in Shleifer (1986).

OA has been much in the news recently, through its alleged use by high-frequency trading firms. In its 2010 "Concept Release on Equity Market Structure," the U.S. Securities and Exchange Commission (SEC) called for evidence on OA strategies, described as "any means to ascertain the existence of a large buyer (seller) that does not involve violation of a duty (...) or other misconduct" (pp. 54-56). The SEC explicitly asked the following question: "Do commenters believe that order anticipation significantly detracts from market quality and harms institutional investors (...)?"[2]

We argue that bilateral disclosure of trader identities harms traders who are known to account for a sizeable portion of total volume and who trade repeatedly in the same

[2]Appendix A gives details of earlier policy debates. The implications of transparency for OA were, for example, very clearly spelled out in the National Association of Securities Dealers' request to the SEC for a rule change to introduce post-trade anonymity to "SuperMontage." Our own discussions regarding the introduction of the CCP with block brokers on the LSE bear this out. They categorically described non-anonymity as generating OA in a concentrated market and welcomed the introduction of anonymity post-CCP.

direction because it facilitates anticipation of their orders. Executing against such a trader allows an anticipator to (noisily) infer the large trader's sustained presence on one side of the market. The anticipator can then shift their own quotes against the investor and/or trade ahead of them to resell liquidity at a higher price. Such practices are called "quote-shading" or "fading" by Harris (2002) and Angel, Harris, and Spatt (2011). (We use the term "order anticipation" to cover both the quote shading sense of the phrase and also the predatory trading sense and avoid referring to "front-running," as it is generally taken to mean a situation where a broker trades in front of a client order.) Both have the effect of reducing the liquidity available to large traders and increasing their trading costs. Note that OA does not require the large trader to be informed about future payoffs. It does, however, require large traders to "work" orders (i.e., split them into small orders and execute dynamically), as is common practice in institutional trading. In a setting where a few brokers intermediate a significant proportion of all trades and trade with autocorrelated direction, even very limited information on who is trading will make the anticipator's job easier.

A direct connection between OA and average market liquidity is also intuitive. Harris (1997) states that "Front-runners increase large trader transactions costs by taking liquidity that might otherwise have gone to the large trader. The large trader must therefore pay more for liquidity." Harris (2002) argues that the activities of anticipators "[...] affect liquidity through their effects on other traders [as they] alter their trading strategies to avoid losing to front runners. Some [...] may price their orders more aggressively or they may demand liquidity rather than supply it. [...

Others] trade less aggressively [...] their withdrawal from the market decreases liquidity." Concretely, in our case, if identity revelation allows anticipators to exploit large traders, in equilibrium one would expect those large traders to execute more aggressively and quickly. They will likely choose to demand liquidity rather than supply it, so that spreads rise and depth falls. Conversely, anonymity reduces the scope for OA and should encourage more patient execution by large traders and thus greater displayed liquidity. We also hypothesize that the effects of OA vary in the cross-section of stocks. OA is likely to be more prevalent in less liquid securities and in securities with more concentrated order flow. Moreover, a direct implication of the OA hypothesis is that large traders should see their execution costs decline the most under anonymity.

Our empirical results much more strongly favour the OA hypothesis than they do the asymmetric information mechanism. First, we provide evidence that, as OA requires, trading in our sample is highly concentrated. On average, the five largest traders in a stock participate in over half of that stock's executions and the trade directions of the most active traders are strongly positively autocorrelated. The liquidity improvement we observe under anonymity rules out the standard asymmetric information argument of Huddart, Hughes, and Levine (2001). In the cross-section, we observe greater liquidity improvements for small stocks and for stocks with higher trading concentration. Both of these results are in line with the predictions of the OA hypothesis but, as small stocks tend to be those with the largest information asymmetries, the former does not support the AI story. Rindi (2008), for example, argues that stocks with large exogenous information asymmetries (small stocks) are likely to see liquidity fall under anonymity, while stocks in which traders endogenously acquire information advantages (large stocks) will show improved liquidity. This result is reversed in our analysis. In sum, our evidence is inconsistent with both types of the AI hypothesis.[3]

Finally, we show that the traders who benefit most from anonymity are those who trade repeatedly and trade the largest volumes, as our hypothesis requires. Large, repeat traders generate smaller price impacts under anonymity and they trade more patiently (i.e., in smaller trade sizes and in more correlated fashion) in the anonymous regime, consistent with reduced fear of anticipation. Price impacts for the aggressive executions of all other traders show no significant change with anonymity. Thus, overall we show that even a very limited form of transparency in identity can facilitate OA and thus substantially degrade market quality and increase trader costs, with striking effects for small stocks, stocks with high concentration in trading, and for repeat traders.

In related work on anonymity, Linnainmaa and Saar (2012) report evidence that in a fully non-anonymous market – with broker IDs being disclosed alongside unexecuted limit orders and also for each execution – prices adjust to reflect the information held by the brokers' clients, the end investors. Whilst this finding has very different flavor from ours, it supports our contention that market participants find identity

[3]Note also that our result on improvements in liquidity under anonymity and stock size is the reverse of that found in the empirical pre-trade anonymity study of Comerton-Forde and Tang (2009), suggesting that the mechanism at work in our data is different to that in theirs.

information valuable as, in this context, it allows them to infer the type of investor behind each broker. Recent work by Bessembinder et al. (2012) is related to ours, in that they also study the effects of predictable trading activity on liquidity. They study the predictable monthly "roll" trades of oil ETFs but empirically they find little support for predatory trading, in that depth increases around these trades. This result is different from ours, but so is their setting. Most importantly, both the timing and direction of their roll trades are predictable. Both of these effects serve to attract liquidity suppliers at a single point in time but are absent in our setting. Further, OA requires markets that are not very deep, as the anticipator needs to be able to move the market through his trading. It is likely that depth is lower in our single stock setting than it is for a very liquid commodity future such as oil. As such, our results and those of Bessembinder at al. (2012) may complement one another and help to clarify under which trading conditions and in which assets opportunistic behavior may or may not arise.

The rest of the paper is set out as follows. In Section 1 we describe the data and the anonymity event. We then present our results in three steps. In Section 2 we analyse how SETS market quality changed with anonymity. Then, we focus on identifying the mechanism that generated the change in liquidity. Section 3 documents the extent of autocorrelation in trade direction and order flow concentration in the London order book. Section 4 presents analysis of the stocks and the market participants that benefited or lost out from the move to anonymity. We conclude in Section 5.

1. The market and the data

1.1 The trading system and the event

SETS was introduced in 1997 and, at the time of our event in 2001, was available for trade in around 200 of the most liquid stocks from the 1,500 on London's Daily Official List. These 200 stocks accounted for around 95% of U.K. equity market activity. SETS operates as a standard electronic order-driven system, opened and closed with batch auctions.[4] During our sample period, SETS was among the most pre-trade transparent of the limit order books available in major equity markets, as full market depth (although not the identities of order originators) was continuously displayed to member firms. Hidden and iceberg orders were not available to traders. Post-trade publication of the details of all order book trades was immediate.

On February 26, 2001, the LSE, in conjunction with the London Clearing House and CRESTCo, launched a central counterparty (CCP) service for order book trades. Until then, trades had been settled bilaterally and the identity of each trader was revealed to the other immediately after the trade. The interposition of the CCP between every pair of traders thus had the effect of rendering all SETS executions anonymous. We can isolate the effects of the introduction of anonymity to SETS trading as the year surrounding the event contained no other significant changes to

[4]Standard limit and market orders made up over 99% of all order entries in the sample shares. Other types of orders that were available during our sample period were a variant of limit orders called "execute and eliminate" (where unexecuted quantities were removed from the book); and "fill or kill" orders, which either executed in full or were removed from the system.

the trading environment. In particular, two other changes that often accompany the introduction of a CCP – the removal of default risk and settlement netting – occurred much before and after (respectively). Default risk had been long protected against through an LSE-funded insurance mechanism called the SETS "Trade Compensation Scheme," which was closed on the day the CCP was launched. Settlement netting was introduced more than a year *after* the CCP launch.

The decision made in 2000 to launch a CCP in London was clearly motivated, at least in part, by the need for trading anonymity. The LSE themselves indicated "an increasing realization that market quality will be improved, with better liquidity on SETS, if post-trade anonymity is provided."[5]

1.2 Data

1.2.1 Main sample

Our sample of stocks affected by the introduction of anonymity (the treated sample) comprises 134 shares that were continuously traded on the LSE's order book during the sample period and did not experience a major corporate action or exhibit unusual price movements. The firms were all components of either the blue-chip FTSE-100 or the mid-cap FTSE-250 indices. These shares provide very broad cross-sectional coverage in terms of industry sectors, ownership structures, as well as size, with market values at the time of the event ranging from GBP 150 million (British Biotech)

[5] *Central counterparty for SETS*, Service outline, LSE/LCH/Crest, March 2000, p. 5. See also comments by exchange officials on the likely liquidity benefits from post-trade anonymity in McKenzie (2000).

to GBP 133 billion (BP). The sample companies represented over 70% of the total market capitalisation of the LSE, and over 90% of all trading interest by value in 2001.

We define our sample period to include six months of trading either side of the date on which the CCP was introduced to SETS (February 26, 2001). We exclude the month of February during which live testing of the new trading arrangements took place. We also exclude the last five trading days of December 2000 as activity was very low due to the Christmas holiday.[6] Overall, our sample period contains 125 trading days pre-event (end July 2000 to end January 2001) and 125 trading days post-event (March 1, 2001 to August, 30 2001) respectively.[7]

The data we hold for our treated stocks are extremely detailed and were supplied by the LSE shortly after the introduction of the CCP. First, they include all order events, thus allowing us to rebuild the order book. Second, they contain a numeric identifier for each broker that allows us to track their order submission and trading activity. Third, the dataset includes a variable that enables us to link the orders and trades that were part of the same client or in-house execution instruction. Following the terminology used in Chan and Lakonishok (1995, 1997), we refer to these linked executions as trade *packages*.[8]

In what follows, we will often use each security's "normal market size" (NMS) to

[6]Our results are not sensitive to this exclusion or to the choice of a wider exclusion period.

[7]Market sentiment was bearish across our sample period, with both the FTSE-100 and FT All-Share indices exhibiting a decline of about 15% between July 31, 2000 and August 31, 2001.

[8]Our variable allows us to link all trades and orders that were submitted as part of an instruction, whether they subsequently executed, were modified, cancelled or expired. Note that this variable was not published to those involved in trading. We do not know the size that the firm originally intended to trade.

express some of our variables in units that are comparable across stocks. The NMS was a stock-specific measure of the number of shares in an average institutional execution, computed and regularly reviewed by the LSE. It was computed, with some adjustment, as 2.5% of recent average daily volume, and so a 1 NMS trade was very large.[9]

Panel A of Table 1 gives data on the cross-stock distribution of some liquidity and trading activity variables for our main sample. The table shows that the stocks are a diverse group. For example, in terms of daily trading activity, De Vere Group traded only about 30 times a day on average over the sample period, while BT traded over 2,800 times a day. Similarly, spreads varied widely across stocks. HSBC had a mean spread of only 15 bps, while Kewill, an IT firm, had a mean spread of about 360 bps.

There were about 17.6 million trades on and off the order book in our main sample over the 12 months we analyse. The order data comprise slightly fewer than 60 million events, almost 90% of which were related to limit orders, with the rest being market order events (9.3%) and orders for execution in the batch auctions (0.7%).

1.2.2 Control sample

We construct a control sample of shares that saw no change in anonymity during our sample period. The data for the control sample come from SIRCA/TRTH. We draw our control stocks from the list of the StoxxEurope 600 Index components at

[9]Note that even though NMS values were reviewed and may have been changed every quarter, we use only one NMS value for each stock, taken at the middle of our sample period. Therefore, endogenous changes in NMS that could have been driven by changes in trading practices related to the CCP introduction do not cause problems in our analysis.

the time of our event. The StoxxEurope 600 is a pan-European index that represents the bulk of total European market capitalisation and includes eurozone, U.K., Swiss and Swedish stocks. From the index constituents, we remove U.K. stocks as well as French and Dutch stocks traded on Euronext, to avoid our sample window being shortened by the anonymity change on Euronext at the end of April 2001 (studied by Foucault, Moinas, and Theissen, 2007). We also lose a few stocks that died within a year of our event. This leaves us with 317 possible European control stocks, to which we add a list of U.K. midcaps that were not traded on SETS but on the other system operated by the LSE, a dealership system called SEAQ, which did not experience a change in anonymity.

We then match these candidate stocks with our 134 treated stocks by estimating their propensity scores – the probability of receiving treatment conditional on two covariates: the stocks' average market capitalisations and turnover by value over the pre-event period. Tests shows that the "balancing property" is satisfied and that there is common support across the samples. We remove two outliers in propensity score terms ("off-support") from the main sample and use nearest neighbor matching with replacement to identify, for each stock in the main sample, the two control stocks that are the closest in terms of propensity scores. After removing stocks exhibiting extreme price movements or showing outlier data, we have 155 individual stocks as first or second nearest neighbor (122 European and 33 U.K. SEAQ stocks). Thus our final panel contains 287 main and control sample stocks.

Some of our analysis relies on observation of depth but relevant data are not available for all of our control sample markets. Thus, when analyzing depth we use a smaller panel of stocks, balanced using the propensity score matching technique. This panel contains 124 treated stocks and 69 control sample stocks. Further, in a few estimations where the details of individual order entries, or broker or trade "package" identifiers are required, we focus analysis on the treated stocks only.

Panel B of Table 1 presents descriptive statistics for the stocks in the control sample, where the figures are constructed from daily averages and then equally-weighted across stocks. Both main and control samples are skewed by very large companies, as would be expected, and stocks in the U.K. sample tend to be somewhat larger. Whether measured by liquidity (inside spreads) or activity (by number of trades or by value), the main and control sample securities tend to be very comparable.

2. Post-trade anonymity and liquidity

In this section, we document the effects of the introduction of anonymity on order book spreads and depth. We then provide further evidence on liquidity changes using time-series analysis to evaluate how the price impacts of trades, both individual executions and worked orders, altered with anonymity.

2.1 Panel specification

Our baseline empirical evidence involves panel estimation of models for stock-day liquidity variables. We measure liquidity using spreads and depth, taken at the best quotes and also further into the order book. We estimate difference-in-differences specifications that use our control stocks to "difference out" confounding factors and isolate the effect of the anonymity event on the liquidity of treated stocks.[10] Our difference-in-differences model for spreads ($S_{i,t}$) is as follows:

$$
\begin{aligned}
S_{i,t} &= \alpha_i + \beta_1 V_{i,t} + \beta_2 RVol_{i,t} + \beta_3 MktCap_{i,t} \\
&\quad + \gamma_1 D_i^{Treat} + \gamma_2 D_t^{Anon} + \gamma_3 D_{i,t}^{Treat \times Anon} + \epsilon_{i,t} \ , \qquad (1)
\end{aligned}
$$

where D_i^{Treat} is an indicator variable that isolates the treated sample stocks and D_t^{Anon} is an indicator for the post-CCP period. The final interaction term between the main sample and the anonymity dummies is the key variable in this specification – its coefficient (γ_3) is the difference-in-differences estimate of the event effect. We include three right-hand side control variables to account for stock or stock-day-specific conditions: (i) the aggregate traded value for stock i on day t ($V_{i,t}$), expressed in money terms and rescaled by a measure of average daily volume to increase comparability across securities;[11] (ii) the daily stock-level realized volatility denoted $RVol_{i,t}$, based on a 15-minute sampling of traded prices; and (iii) the log market cap for each stock ($MktCap_{i,t}$). All controls are time-varying. We demean them prior to inclusion in the

[10] This methodology has been widely used in the economic analysis of "natural experiments" such as the impact of the Sarbanes-Oxley legislation or the introduction of the euro (Li, Pincus, and Rego, 2008; Gao, Wu, and Zimmerman, 2009). For a review of the technique, see Imbens and Wooldridge (2009, Sec. 6.5).

[11] For the few estimations that use only treated U.K. stocks, we express this turnover control in NMS, defined in the penultimate paragraph of Section 1.2.1.

regression, such that the estimated intercept combined with the appropriate dummy variable coefficients give average spread values for the main sample pre- and post-CCP and also for the control sample.

To address econometric concerns of endogeneity of the right-hand side variables in equation (1), we have estimated all of our panel regressions via IV, using two lags of the regressors as instruments, with no qualitative change in results (these results are available on request). To ensure that multi-way dependencies in the panel residuals do not distort our statistical inference, we estimate all panel models using the robust covariance matrix estimators developed in the recent econometric literature on unobserved heterogeneity (Petersen, 2009; Cameron, Gelbach, and Miller, 2011). The procedure we adopt corrects standard errors for stock and time-specific clustering in the errors.

There may be "deep" stock-specific factors affecting our dependent variables that must be modelled as constant over the sample period.[12] A test, described in Wooldridge (2002, p. 291), that is robust to dependence in the panel regression errors rejects the null of no fixed effects in several of the panel specifications we use below. In practice though, inclusion of fixed effects made hardly any economic or statistical difference to our results, hence we chose to report the simplest estimates based on a common constant.

[12]For instance, the nature and distribution of a stock's ownership will be a determinant of informational asymmetries and therefore impact spreads.

2.2 Baseline estimations: anonymity, spreads, and depth

We focus first on inside spreads. For both samples, we compute the daily time-weighted inside spread, expressed in basis points. A priori, theories based on asymmetric information have unclear implications for the effect of anonymity on liquidity, while the OA hypothesis points to greater liquidity with anonymity, and hence lower spreads.

The results from estimation of the difference-in-differences model in equation (1) are reported in Table 2. The coefficient on the treated sample indicator shows that SETS stocks have wider spreads than those in the control sample, which we know from Table 1 unconditionally. The estimated coefficient on the anonymity indicator implies that spreads in the control sample have fallen by a small amount in the anonymous regime, with borderline statistical significance. The coefficient on the interaction variable shows that anonymity has improved liquidity in the main sample dramatically, with a very significant drop in inside spreads of over 10 bps. The sum of the anonymity coefficients represents a downward shift in spreads of about 20% for the treated stocks.

Economically, these estimates imply that once anonymity was implemented, inside spreads in U.K. stocks became very comparable to those of the control sample stocks, which had been anonymously traded from the start. The removal of broker IDs thus seems to broadly bring U.K. stock liquidity in line with that of a basket of matched European and UK control stocks.

The coefficients on the control variables conform with intuition. The estimates indicate that liquidity is consistently and significantly improved on high activity days (the turnover regressor is always negative and significant) and increased volatility widens spreads, consistent with volatility proxying for information and/or inventory risk. Finally, larger firms have consistently narrower spreads, perhaps because firm size is inversely related to information asymmetry.

Table 2 also reports the results of a panel model featuring the spreads measured at the fifth price level instead of at the best quotes, for the main and the control sample securities. (To be clear, these spreads measure the (percentage) distance between the fifth best limit sell price and the fifth best limit buy price on the order book. They are not the weighted average cost resulting from "walking up" the book schedules.) The result is very similar to the previous one: anonymity causes a strongly significant narrowing of spreads at the fifth limit of about 20%, only this time solely in treated sample stocks, the coefficient on the anonymity dummy being insignificant. These results are consistent with anonymity affecting order placement within the book, increasing its depth. We will demonstrate the impact that this has on realized price impact below.

We then extend this analysis of quantity-based measures of depth. In Table 3, we report the results of estimations that use the value of shares available at the inside and up to the fifth price limit, respectively, as dependent variables. These values are expressed in a common currency (GBP), averaged across the bid and ask to a daily frequency and rescaled by a measure of stock-specific average daily turnover. The first estimation, using depth at the best prices, shows no anonymity-related change in treated stock depth. However, the second estimation indicates that cumulative depth up to the fifth limit significantly improves for treated stocks relative to control stocks under anonymity. Thus, not only have the best and the fifth price levels on the SETS order book got closer together under anonymity, but the aggregate quantity available on the buy and sell sides has also risen. Overall, the effect of anonymity on depth or price impact seems to have operated via a change in price placement and less so via a change in the quantities offered. We study this further below.

2.3 Liquidity beyond the inside spread

We now focus more clearly on the price placement effects revealed by the difference-in-difference analyses above by studying the cost of trading fixed quantities in the order book before and after anonymity. To that end, we construct time-weighted average percentage spreads between the price of aggressively buying the marginal unit in a K NMS trade and the price of selling the marginal unit in the same size for the treated U.K. stocks. We call these measures outside spreads. They are related to the two measures of spreads we considered in Table 2 but, as the NMS measure is a proportion of ADV, they measure liquidity at quantity points throughout the U.K. order book in a way that is more comparable across assets (while spreads at the fifth limit may correspond to quantities that are hugely varying in the cross-section). Taken together, outside spreads tell us something about price impact in a Kyle λ sense.

We are only able to construct these measures for our main sample securities as we do not have full order submission data for the control stocks. Therefore, we use a simple specification containing the same set of right-hand side controls as in equation (1) plus a post-CCP dummy variable and a variable measuring changes in market-wide liquidity, computed as the daily average bid-ask spread across the entire universe of control sample stocks. This time series is intended to control for market-wide changes in liquidity that may have affected our dependent variables. The empirical coefficient attached to this variable thus has the interpretation of a "liquidity beta." Denoting the dependent variable of interest by $y_{i,t}$, we estimate:

$$y_{i,t} = \alpha_i + \beta_1 V_{i,t} + \beta_2 RVol_{i,t} + \beta_3 MktCap_{i,t} + \beta_3 MktLiq_t + \gamma D_t^{Anon} + \epsilon_{i,t}\,. \qquad (2)$$

We use the estimated coefficient on D_t^{Anon} to determine the effect of anonymity on order book "outside spreads" in the main sample. We construct outside spreads for $K = 0.2, 0.4, 0.6, 0.8$, and 1 NMS. Our maintained hypothesis is that outside spreads should narrow, as agents should be less worried about adverse price drift and be willing to trade more patiently under anonymity.

Table 4 presents estimates of equation (2). (We include inside spreads as one of the dependent variables to verify the consistency of the results of this and our previous analysis.) Results demonstrate greatly increased order book liquidity in the anonymous regime, both in tightness and depth terms. The anonymity dummies have the expected negative sign and are all significant. Consistent with the difference-in-differences estimation, inside spreads are reduced by 12 bps on average, a fall of about 18% given an average pre-CCP spread of 66 bps for these stocks. Within the order book, the spread between the implied price of the marginal unit in a 1 NMS buy and a 1 NMS sell drops by over 100 bps, also about 18%. Outside spreads fall consistently throughout the order book. Note that the numerical consistency between these and the difference-in-differences estimates of inside spreads in Table 2 indicates that it is safe to focus on the main sample when comparable data are not available for the control stocks.

2.4 Transaction-level analysis: market impact

We now investigate how the introduction of anonymity via the CCP changed the impact that trades have on subsequent prices in the main and control samples. If anonymity reduces the scope for OA, we would expect impacts to be smaller. If it increases the information asymmetries between informed aggressive traders and uninformed liquidity suppliers (Huddart, Hughes, and Levine, 2001), we might expect impacts to be larger.

We estimate the price impact of trades using a regression methodology. First, we construct transaction price changes (in bps) in event time. We regress these on eight sets of signed trade indicator variables (plus a constant). Each set of trade indicators contains five leads and lags, as well as the contemporaneous regressor. There are eight sets of indicators as we split trades into four disjoint size categories (with endpoints of 0.1, 0.25, 0.5, and 10 NMS) and for each size category we distinguish pre- and post-anonymity regimes.[13] Thus we estimate:

$$r_{i,t} = \alpha + \sum_{j=1}^{4}\sum_{k=-5}^{5} \beta_{j,k} X_{i,t}^{Pre} D_{i,t}^{Size_j} + \sum_{j=1}^{4}\sum_{k=-5}^{5} \gamma_{j,k} X_{i,t}^{Post} D_{i,t}^{Size_j} + e_t\,, \quad (3)$$

where $r_{i,t}$ are transaction level returns for stock i at observation t. $X_{i,t}^{Pre}$ is a signed transaction indicator variable for all trades occurring before the CCP introduction and it takes the value zero for all trades after the CCP introduction. Similarly, $X_{i,t}^{Post}$ is zero for all trade observations pre-CCP and is then a signed transaction indicator variable for all trades occurring after the CCP introduction. Finally, the four trade size dummy variables, $D_{i,t}^{Size_1}$ to $D_{i,t}^{Size_4}$, take the value one if and only if the trade in stock i at time t is in the appropriate size category. $\beta_{j,k}$ and $\gamma_{j,k}$ are coefficients which, holding j constant, sum over k to give price impacts for trades in particular size bins before and after the CCP introduction.

We run two impact regressions, one for a pooled set of order book trades from the main sample stocks and the second from a pooled set of order book trades in control stocks. We use the estimated regression coefficients to compute cumulative post-trade returns after five trades, for each size category and anonymity regime. Thus the main sample price impacts come from a different regression than the control sample impacts and so this is not a difference-in-difference estimation. Panel A of Table 5 contains the results of these calculations for the treated sample. Comparison of the price impacts in

[13] Note that as we only consider order book trades in these regressions, there are few observations in the 0.5 to 10 NMS category. Note also that our results are consistent when the number of leads and lags in the regression increased.

the two anonymity regimes shows that order book executions have smaller post-trade price impacts under anonymity. The final column shows a t statistic of the hypothesis that the price impacts are identical pre and post based on a heteroscedasticity robust covariance matrix. The difference between the two cumulative impacts is strongly significant and greater for the larger trade size categories. This is as one might expect – it is the institutions trading bigger size, that suffered from non-anonymity. In terms of economic significance, the estimated impact shifts are considerable, of over 20% in trade sizes of 0.5 NMS and above. Panel B displays results from a similar set of estimations but for the control sample. These results show no significant change in price impacts, such that the decline in impact in the main sample cannot be attributed to time-series variation in market-wide conditions.

2.5 Trading costs: Package-level price drift

In this section, we analyse execution costs for worked orders and how they change under anonymity. This analysis is directly relevant to the OA hypothesis as it provides evidence on the costs incurred by repeat traders. To this end, we employ the variable described in Section 1.1, which allows us to identify linked orders and executions. In aggregate, our data contains about 3 million packages, each comprised of two or more separate executions. Note that we do not have package identifiers for the control sample stocks and therefore cannot estimate a difference-in-difference here.

We employ a specification similar to that used by Conrad, Johnson, and Wahal (2003) or Chiyachantana et al. (2004). The dependent variable in this regression is the price slippage of the package execution, measured as the signed difference, in basis points, between the midquote observed immediately prior to the first observation of a package identifier and the volume-weighted package execution price (VWAP). Thus, if we first observe package K's identifier at time t_0 and the VWAP of that package is $\tilde{P}_K$, slippage (Z_K) is:

$$Z_K = 10000 \times I_K \times \left[\frac{\tilde{P}_K - M_{t_0}}{M_{t_0}} \right],$$

where M_{t_0} is the midquote at t_0 and I_K is an execution direction indicator taking the value +1 for buys and -1 for sells.[14]

The right-hand side variables in the estimation control for the log of market cap of the security, volatility (defined as absolute return over the 24 hours leading up to package initiation), momentum (the signed return over the 24 hours up to the first observation of this package, with sign swapped for sell packages). Package size is captured using a set of four dummy variables that partition the set of packages based on NMS executed. The size cutoffs are similar to those in Section 2.4 but where the largest size category includes everything over 0.5 NMS. To detect any shift in package price drift under anonymity, we create a second set of four dummies by interacting the size dummies with an indicator variable taking a value of one during the anonymous trading period. Finally, we include the market-wide bid-ask spread regressor defined

[14]We mark these package executions to the midquote rather than a transaction price due to the fact that there is a systematic reduction in spreads in the anonymous regime. Thus, using, for example, the most recent trade price on the relevant side of the market as an execution benchmark would lead to the benchmark being *much more demanding* in the anonymous trading period. Extreme outliers are trimmed from the stock-level slippage distributions prior to pooling slippages across stocks.

earlier to control for movements in aggregate liquidity around package execution.

The panel regression estimates are shown in Table 6. Most estimates are strongly statistically significant. Packages of larger securities are associated with smaller price drift. Volatility tends to increase drift, while momentum has no significant influence. Estimates of coefficients on the four trade size dummies indicate that price slippage increases in statistical and economic significance with package size. The key results come from the estimated coefficients on the anonymity interaction terms, which indicate that the slippage associated with large packages has been dramatically reduced. For sizes over 0.5 NMS, the average execution price paid by a dynamic trader is closer to a pre-execution benchmark by about 25%, implying that traders suffer lower adverse price drift. While the effect is economically smaller for smaller worked orders, statistical significance only disappears in the smallest size category. Thus increased anonymity leads to lower dynamic execution costs for repeat traders.

2.6 Summary of the effects of anonymity

Overall, the introduction of post-trade anonymity greatly improved liquidity. The market became tighter and deeper, and price impacts from single executions and worked orders both fell. Neither U.K. stocks nor European stocks of similar liquidity to those in our main sample but that did not experience the anonymity change saw significant liquidity improvement over the same period. These results run counter to the theoretical predictions of Huddart, Hughes, and Levine (2001) and the exogenous information endowment version of the model in Rindi (2008). The results are, however, consistent with the OA hypothesis.

In Appendix B we provide further evidence that the introduction of anonymity has not brought about deep changes in the nature of the information environment on SETS. We compute, using a VAR approach (Hasbrouck, 1991a,b), the size of the asymmetric information problem in our sample stocks and a measure of market efficiency. Neither change in any meaningful way with the introduction of anonymity. Thus, there is no evidence that anonymity strengthens information asymmetries, as Huddart, Hughes, and Levine (2001) predict, nor is there evidence that anonymity improves informational efficiency as the endogenous information acquisition model of Rindi (2008) would indicate.

3. Concentration and predictability of broker order flows

The estimations above and in Appendix B indicate that there was no change in the informational asymmetries facing SETS liquidity suppliers with the introduction of anonymity. Thus we focus attention on the OA hypothesis and evaluate its implications more fully. To that end, we first examine whether two market features that anticipation requires, order flow concentration and correlation in trade direction from large traders, hold in our data.

3.1 Order flow concentration

The data confirm a high degree of order flow concentration. The number of distinct firms that were active on the order book in a typical month ranged from 50 to 180 across our sample stocks, with an average of about 75 per stock. However, a small number of these broker-dealers emerge as key players: the volume-weighted mean market shares of the top five firms in limit order submission across all stocks and months is 54% (55% for executions). For the top 10 firms, the global mean market share is 77.5% for both executions and order submission. This is very stable across the sample period.[15]

Our evidence is consistent with other sources. A June 2010 consultancy report indicates that the top five brokers in the U.S. command a market share of equity trading of close to 9% *each*, while another piece states that the most important 13 brokers receive around 75% of total buy side flow (Schmerken, 2008; Greenwich Associates, 2010). Academic evidence that looks at order flow executed by designated market-makers on NASDAQ, also indicates concentration (Ellis, Michaely, and O'Hara, 2002; Chung, Chuwonganant, and McCormick, 2006).

3.2 Time dependencies in trade direction

Another ingredient that facilitates anticipation is that the trades of large broker-dealers exhibit positive serial correlation in direction. We measure autocorrelation

[15]Herfindahl indices and other concentration statistics are available on request.

in trade direction for individual firms. For each stock, in every month we isolate the trades of the five most active dealers. For each of these dealers we compute the first-order autocorrelation in the direction of their aggressive trades. These autocorrelations are averaged to give a single order flow autocorrelation measure for the biggest dealers in each stock and month. Across our sample stocks and months, the resulting figures are around 0.25 and the autocorrelations are strongly significant. We also conduct a set of runs tests for the dealer-level trade direction series. They strongly reject the null of independence in trade direction. This is again consistent with other evidence. Biais, Hillion, and Spatt (1995, pp. 1686-87), who examined the Euronext Paris limit order book, found that market wide order-flow "(...) exhibits a large degree of positive serial correlation" which they interpret as caused by "order splitting and imitation."[16]

Hence, our results indicate that order flow in the treated stocks is characterised by high broker concentration and strong positive correlation in direction for individual large traders. We exploit these facts to specify estimations relevant to our OA hypothesis.

[16]Positive autocorrelation in trade direction is also empirically well established for the NYSE (e.g., Hasbrouck, 1988; Doran et al., 2008) but the presence of the Specialist may make interpretation less straightforward in that case.

4. Which stocks and which traders benefit most from anonymity?

We now evaluate the cross-stock implications of OA and examine how anticipation might affect execution quality across traders.

4.1 Cross-sectional tests 1: small versus large stocks

Order anticipation should be a greater concern for dynamic traders of small stocks than for those trading large stocks because the low natural trading interest in small caps makes repeat traders easier to isolate and thus easier to move prices against. Thus, if the OA hypothesis holds, one would expect the liquidity improvement of small stocks under anonymity to be larger than that of large stocks. Conversely, a prediction of the asymmetric information story of Rindi (2008) is that stocks with endogenously acquired private information are likely to see greater liquidity improvements than stocks with exogenous information asymmetries.[17] Large caps are likely to fall in the first camp and small stocks in the second. For example, the literature on the profitability of the trades of corporate insiders consistently shows that insiders in smaller firms are better able to predict future firm returns than executives in large firms [see Seyhun (1998) for an overview]. This suggests that the improvement in liquidity associated with anonymity for large caps should exceed that of small caps.

[17] We focus here on this flavour of the asymmetric information story as the other versions are not supported by our prior results on liquidity improvement and anonymity.

Thus, in the cross-section of stocks, the anticipation and asymmetric information stories have contradictory implications.

To discriminate between the OA and asymmetric information stories, we separate both our main and control samples into three subsamples (terciles) by market cap and estimate the difference-in-differences model of equation (1) separately for each pair of sub-samples. The three estimations that result are contained in Table 7. Note first that the volume and volatility variables are, as before, significant and have negative and positive signs as expected. The size indicators show the dependence of spreads on market cap – clearly larger stocks have lower pre-CCP spreads. More importantly, the interactions show a much greater absolute and proportionate improvement in small cap liquidity than they do in large cap liquidity (Size T1 and Size T3 respectively.) In absolute terms, spreads fall by about 23 bps in small caps and by 3.4 bps in large caps. These figures represent, respectively, close to 20% versus 10% of the pre-CCP spread.

Thus, stocks in all size categories benefit from anonymity in terms of liquidity. However, the fact that small stocks benefit most strongly indicates that order anticipation is the driving force behind the improvement, rather than asymmetric information as in Rindi (2008). Note that Comerton-Forde and Tang (2009) also investigate how liquidity improvement varies with stock size. In their pre-trade anonymity setting, they find a result that is the exact opposite of ours, suggesting that different effects are at work in their data.

4.2 Cross-sectional tests 2: order book depth and liquidity

The profitability of anticipation strategies depends on price pressure effects that should be much harder to generate in a naturally deep order book. This yields the cross-sectional prediction that the stocks exhibiting the least depth before the event stand to benefit the most from the introduction of anonymity as they would have been more susceptible to anticipation other things equal.

We therefore estimate a panel model similar to that in the last subsection but this time we group stocks in terciles according to their pre-event *depth*. Table 8 shows the results from this estimation. For low depth stocks in tercile 1, anonymity brought about an economically substantial reduction in spreads of about 20%, and not much less for stocks in depth tercile 2. Stocks in tercile 3 that were endowed with a naturally deep book before the event experienced no significant improvement in liquidity. The results therefore confirm priors stemming from our OA hypothesis.

4.3 Cross-sectional tests 3: order flow concentration and liquidity

The anticipation argument relies on concentration in order flow brokerage – the fewer the participants in a stock's order flow, the more severe the price drift associated with worked orders, and the more traders will take liquidity in blocks. It follows that anonymity should be most beneficial to high-concentration stocks which, in the non-anonymous regime, would see much lower usage of worked orders than traders would like to employ. We therefore examine how the improvement in a stock's liquidity is related to its order flow concentration pre-CCP.

To test this prediction, we compute pre-event concentration for each stock, by measuring the proportion of total order flow submitted to the order book by the five largest brokers over the three months preceding the CCP introduction.[18] We separate the set of stocks into terciles reflecting low, medium, and high order flow concentration, and then augment the panel model with the concentration dummies and their interactions with the anonymity dummy. This regression does not use a control sample as we do not have broker identities for the control stock trades and so we include our market-level bid-ask spread variable to control for aggregate movements in liquidity just as we did in the estimation involving outside spreads (Table 4).

The results from this regression are reported in Table 9. The estimates on the three concentration dummies reveal that inside spreads increase somewhat with order flow concentration. This can be interpreted as evidence that, pre-CCP, more concentration meant greater potential for anticipation and thus excessive demand for immediacy on the part of large traders. More to the point, the estimated coefficients on the interaction variables are consistent with our predictions: the relative improvement in liquidity caused by the introduction of anonymity appears monotonically related to order flow concentration pre-CCP. The stocks exhibiting the highest concentration in the pre-CCP period saw a relative decline in spreads of around 33%. In the middle group it is 25% and the improvement in liquidity drops to 12% in the least concentrated third of our securities.

[18] Using the top ten firms or using actual executions instead of order submissions makes no material difference to the results below.

To summarise this and the preceding two subsections, the changes in liquidity across the cross-section of stocks are consistent with liquidity improvements being generated by reductions in OA. Moreover, the results related to stock market cap are inconsistent with the most plausible version of the asymmetric information story. Note that the cross-stock correlations between the three variables used to create subsamples in these estimations (i.e., market cap, average pre-event depth, and average pre-event concentration) are not especially high.

4.4 Trading costs: Who benefits from anonymity?

Bilateral and post-trade identity revelation can only matter in a world where information drawn from a trade that one has just completed can be used to profitably change one's future quoting behavior or one's future execution strategy. A prediction based on the OA mechanism is that that the agents who benefit from anonymity are the brokers holding large market share, as their (uninformed) order flow is predictable. Their benefit should be visible in reduced price impacts for trades vis-a-vis those of less active traders under anonymity. Moreover, under anonymity we would expect big players to execute more patiently due to reduced fear of predation.

We test this by splitting our population of traders into two groups: the top 10 traders by market share of volume traded and a group consisting of all other traders. This split of traders is stock and month specific.

First, we examine how the trades of our group of larger traders changed relative to those of all other traders with the introduction of anonymity. Figure 1 shows, month-by-month, the mean trade size of the large traders versus those of all others. Figure 2 provides a similar plot but for the autocorrelation in trade direction for the two groups. What these plots make clear is that, relative to all other traders, large traders execute in smaller size and with more autocorrelation in direction under anonymity. Thus, anonymity induces them to be more patient in their execution and to trade, in size terms, almost identically to smaller traders.

We go on to compute the price impact of each individual trade in each sample stock and to compare the impacts experienced by large and small traders. The impact of trade s in stock i is equal to:

$$I_{i,s} = 10,000 \times \frac{(P_{i,s+k} - P_{i,s-1})}{P_{i,s-1}},$$

where $P_{i,s}$ is the price of the sth trade in stock i and k is an integer impact horizon parameter. The impact measure is based on tick-by-tick stock prices and we have computed it for various k from 5 to 100. For each trade we also record the size of the trade in NMS and the aggressive counterparty to the trade.

We then compute two daily average impact measures for every stock-day in the sample. The first is for trades in a stock for which one of the top 10 traders was the aggressive counterparty. The second is the average impact for traders outside the top 10. We relate those daily average impacts to a set of control variables and a set of dummies, yielding the following model:

$$\begin{aligned}\tilde{I}_{i,j,t} &= \alpha_0 + \beta_1 V_{i,t} + \beta_2 RVol_{i,t} + \beta_3 MktCap_{i,t} \\ &\quad + \gamma_1 D^{Large}_{i,j} + \gamma_2 D^{Post}_t + \gamma_3 D^{Large}_{i,j} \times D^{Post}_t + \epsilon_{i,j,t}\,, \end{aligned} \tag{4}$$

where i indexes stocks, subscript j distinguishes big from small traders and t indexes time. $\tilde{I}_{i,j,t}$ is the mean daily impact of aggressive trades from trader type j, on day t and for stock i. The dummies and interactions allow us to identify differences in price impacts across the two groups pre-CCP and then to see whether the difference changes with anonymity.[19] If large traders benefit most from anonymity we would expect to see $\gamma_3 < 0$.

Table 10 shows results from estimating equation (4) for k equal to 10.[20] In running these estimations we have further subsampled the trade data for each stock, day, and trader type to place them into four trade size bins (0-0.1 NMS, 0.1-0.25 NMS, 0.25-0.5 NMS, and above 0.5 NMS). We run a separate regression for each trade size bin.

The estimates in Table 10 accord with our priors. The coefficients on the volume, volatility, and market cap regressors are in line with those from previous estimations. Looking across columns, larger trades tend to have greater price impacts. Further, except in the largest trade category, before anonymity the trades of more active

[19] We are unable to use a difference-in-difference estimation here because we do not have broker identifiers for trades in the control sample securities. Thus we experimented with the inclusion of the market-wide bid-ask spread regressor but it turned out to be insignificant.

[20] Similar analysis performed using different values for k gave qualitatively similar results.

agents moved prices significantly more than those of less active agents. Large traders generate price impacts between 40% and 100% larger pre-CCP, depending on the trade size category. What is clear, though, is that under anonymity the impact differential between executions of small and large traders is greatly reduced, in some cases eliminated. The coefficients on the large trader and anonymity interactions are always negative and are significant in precisely those cases where pre-CCP there was a sizeable large/small trader impact differential. Note also that the price impacts of small traders are unchanged under anonymity.

Hence the traders who benefit from anonymity are those who trade most frequently. Their price impacts drop, due to others being less able to exploit their serially correlated trading activity. Traders other than the very largest derive no benefit, in price impact terms, from anonymity. We can interpret this as due to the fact that they did not suffer from anticipation when their identities were published.

5. Conclusion

We study the implications of post-trade anonymity for the liquidity of equity markets.

In a first set of results, we report that a seemingly small change in the degree of order book anonymity has a large positive effect on liquidity, whether liquidity is measured via spreads, depths, price impacts or dynamic price drift. In a second stage of the analysis, we show that the introduction of anonymity is not associated with a change in asymmetric information in this market. We predict which stocks should benefit more than others from anonymity under the hypothesis that a type of order anticipation behavior is driving the effect on liquidity. These predictions hold in the data: we find that the effect of anonymity on liquidity exhibits strong heterogeneity across stocks. Small stocks, stocks with naturally shallow order books and stocks where trading is highly concentrated benefit the most from full anonymity. Turning to individual traders, we find that it is large, repeat traders who benefit most from the concealment of broker IDs. All of these results support our order anticipation hypothesis.

Our results are novel in several ways. First, a standard result in the transparency literature is that transparency benefits the uninformed (Foucault, Pagano, and Röell, 2010). In our study, transparency harms large agents irrespective of the informational content of their order flow. In models of the order exposure problem such as Burdett and O'Hara (1987), the large investor manages their order flow to prevent information leakage – their trade size is correlated with the information they hold. In our work, large but passive traders may suffer the same adverse price drift, consistent with a long literature on index rebalances (Harris and Gurel, 1986; Shleifer, 1986). Second, we relate market microstructure effects to the structure of the brokerage industry. We document strong concentration in execution among a few sell-side firms as a stylised fact of real-world order books. Electronic markets are much farther from atomistic ideals than we are used to thinking. Given that the U.K. equity market is the second largest national market in the world after the U.S., the reality of less mature markets may correspond to higher concentration still. Third, we supply evidence consistent with anticipatory behavior, itself the result of interaction between "deep" features of the order flow in a stock – that could be related to the nature and distribution of the stock's ownership – and elements of market design such as transparency. We analyse order flow and stock characteristics that can be conducive to anticipation.

Claims are often made that order anticipation and predatory behavior are endemic in current markets due to high-frequency trading (HFT).[21] Our results may help understand this claim. Some of the factors that we identify as facilitating predation may have been made worse by HFT. First, *price pressure* effects may be larger in an HFT world, as HF liquidity suppliers have been found to contribute "only a fraction of [the depth] provided by non-HFTs" (Brogaard, 2010). This makes intuitive sense: small inventory positions that are turned around very quickly are an integral part of HFT strategies.[22] Low depth makes anticipation more attractive (Section 4.2). Second, *autocorrelation* in trade direction may have increased, as a result of orders being broken up and "worked" in order books to an ever greater extent. This is apparent in the dramatic decline in average trade sizes observed on all major exchanges since the mid-2000s (Chordia, Roll, and Subrahmanyam, 2011). Through these new liquidity demand and supply conditions, order flow may suffer from what Schwartz (2010) terms *temporal fragmentation*, a trading environment where repeat liquidity consumers, in particular, find it hard not to be conspicuous and so become targets to strategic agents. More research is required to understand the time-series properties of this new type of order flow, but our analysis may shed light on some of the reasons

[21] An attempt to detect such behavior is Hirschey (2013).

[22] The concomitant decline in tick sizes may have been an aggravating factor.

why anticipatory or predatory strategies are so much in the news by identifying a set of conditions that may be sufficient to engender a degree of order anticipation.

Our results have market design implications. The optimal degree of order flow transparency remains controversial in theory and empirically (see Boehmer, Saar, and Yu, 2005 and Madhavan, Porter, and Weaver, 2005 for very different conclusions) and our findings add to the evidence that certain forms of transparency, still implemented in some major markets (e.g., FX markets), can be detrimental to liquidity. They also help understand why some features of current order flow may encourage anticipation and by consequence increase the fragmentation of order flow towards "dark" venues and the use of hidden orders, both of which allow large traders to avoid revealing their full trading intentions. While early microstructure analysis assumed that uninformed agents tended to trade patiently, and that disclosing the entirety of their trading interest whether bilaterally (Röell, 1990; Seppi, 1990) or multilaterally (Admati and Pfleiderer, 1991) could be beneficial to them, our results suggest that this may not be true in a setting characterised by repeat trading and strategic counterparties because of the inability to retaliate against strategic agents.

Finally, the strong cross-sectional flavour of our results raises the possibility of a bad equilibrium for assets that are less endowed with natural liquidity than others (smaller caps, assets lacking depth for exogenous reasons): such assets may be prime targets for opportunistic traders whose activity, in turn, further degrades liquidity. This should be a concern to investors and, perhaps even more, to security issuers and regulators.

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Table 1: Summary statistics for main and control sample stocks

	Mean	Std Dev	Minimum	25^{th}	Median	75^{th}	Maximum
Panel A: Main sample							
Mkt Cap (GBP bn.)	8.31	16.16	0.15	1.44	3.43	7.93	132.55
Spread (bps)	78.04	55.32	15.75	41.79	61.67	93.11	359.89
Trades	481.62	447.30	32.51	163.71	363.63	584.33	2822.70
Quantity Traded (GBP mn)	13.21	19.07	0.15	2.09	6.53	14.46	107.94
Panel B: Control sample							
Mkt Cap (GBP bn.)	5.03	9.05	0.42	0.94	1.9	4.46	57.45
Spread (bps)	62.62	57.06	8.73	26.02	40.81	79.07	323.38
Trades	491.13	655.02	3.4	43.85	284.44	607.47	3528.69
Quantity Traded (GBP mn)	14.76	28.83	0.05	1.67	4.69	12.86	176.15

Notes: the table reports summary statistics for liquidity and trading variables for our sample stocks. For each stock we compute average market cap in billions of pounds sterling over the sample period, average time-weighted daily spreads in basis points, mean daily number of trades, and mean daily quantity traded, expressed in GBP mn. The table reports, for each variable, the mean, standard deviation, minimum, maximum and 25^{th}, 50^{th} and 75^{th} percentiles from the equally-weighted cross-stock distribution.

Table 2: Difference-in-differences analysis of inside and outside spreads

	Inside spread	*Spread at 5^{th} price level*
Volume	-4.531***	-52.780***
	(10.73)	(7.62)
Volatility	0.632***	7.288***
	(9.78)	(10.38)
Log market cap	-22.16***	-202.29***
	(13.55)	(8.02)
Treated sample	12.88***	433.08***
	(3.67)	(7.07)
Anonymity indicator	-2.759**	-13.829
	(2.18)	(0.58)
Treated $\times$ Anonymity	-10.40***	-149.71***
	(6.47)	(4.55)
Constant	51.81***	317.07***
	(17.06)	(5.90)
R^2	0.47	0.40
N	69,288	42,812

Notes: The table reports the results of panel estimation of bid-offer spreads of the main and control sample shares measured at the inside and at the fifth price level of the order book, against measures of activity, realized volatility, firm size and an indicator variable taking a value of one on sample days when trading in the order book was conducted anonymously. The dependent variables are defined in Section 2 and the regressors in Section 2.1. The estimator used is robust to clustering effects both within and across panels. (***) indicates 1% significance.

Table 3: Difference-in-differences analysis of inside and outside depth

	Depth at inside	*Depth at 5^{th} price level*
Volume	0.2430***	1.3253***
	(9.15)	(6.26)
Volatility	-0.0148***	-0.0437
	(2.96)	(1.11)
Log market cap	-0.4658***	-3.527***
	(7.17)	(4.61)
Treated sample	0.2257	-1.214
	(1.41)	(0.62)
Anonymity indicator	-0.0635	-1.691**
	(1.04)	(2.19)
Treated × Anonymity	0.0328	2.074**
	(0.47)	(2.52)
Constant	0.905***	7.834***
	(6.64)	(4.04)
R^2	0.26	0.17
N	46,574	46,902

Notes: The table reports the results of panel estimation of depth measured at the inside and at the fifth price level of the order book, expressed as a proportion of average daily turnover computed for each stock over the pre-event period in the main and control sample shares. The controls include measures of activity, realized volatility, firm size, and a set of treatment variables. The dependent variables are defined in Section 2 and the regressors in Section 2.1. The estimator used is robust to clustering effects both within and across panels. (***) indicates 1% significance.

Table 4: Panel regression analysis of the effect of anonymity on spreads measured at fixed quantity points

	Inside spr.	*Outside Spr. 0.2*	*Outside Spr. 0.4*	*Outside Spr. 0.6*	*Outside Spr. 0.8*	*Outside Spr. 1*
Volume	-15.85***	-20.71***	-23.77***	-27.15***	-29.46***	-26.26
	(9.90)	(11.72)	(8.88)	(5.92)	(3.62)	(1.63)
Volatility	0.965***	1.674***	2.606***	3.888***	5.570***	8.183***
	(17.43)	(17.15)	(14.67)	(14.50)	(13.42)	(10.78)
Log market cap	-7.298***	-3.455	-0.624	2.889	7.762	17.083
	(3.14)	(1.41)	(0.16)	(0.44)	(0.66)	(0.73)
Market liquidity	0.371***	0.729***	1.213***	1.715***	2.471***	4.159***
	(3.85)	(5.23)	(5.03)	(4.60)	(3.78)	(3.34)
Anonymity indicator	-12.05***	-14.04***	-20.67***	-31.76***	-51.17***	-101.46***
	(10.55)	(8.55)	(7.51)	(6.65)	(6.16)	(6.35)
Constant	65.61***	105.83***	158.90***	233.39***	344.63***	557.71***
	(43.84)	(48.87)	(39.80)	(33.76)	(28.23)	(22.94)
R^2	0.58	0.47	0.31	0.21	0.13	0.07
N	31,801	31,832	31,819	31,804	31,802	31,785

Notes: The table reports the results of panel estimation of variables measuring liquidity of the main sample shares against measures of activity, realized volatility, firm size, and an indicator variable taking a value of one on sample days when trading in the order book was conducted anonymously. The dependent variables are defined in Section 2 and the regressors in Section 2.1. The estimator is robust to clustering effects both within and across panels. The reported intercept is computed as the average value of the stock-specific estimated fixed-effects.

Table 5: Difference in price impact of single trades across anonymity regimes

Size cutoff (NMS)	*Pre-CCP impact*	*Post-CCP impact*	t-test diff.
(a) Main sample			
0.1	2.769	1.727	3.73
0.25	9.089	6.691	3.24
0.5	12.316	9.867	2.79
10	17.193	13.383	3.44
(b) Control sample			
0.1	4.459	2.815	1.56
0.25	3.215	2.610	1.48
0.5	3.476	3.320	0.28
10	6.008	5.036	1.13

Notes: The table presents the five trade impact of single trades in the anonymous and the non-anonymous regimes and a heteroscedasticity-robust t-statistic for the null hypothesis that their difference is zero. Estimates are based on time-series regression estimation of single-trade returns on signed trade indicator variables. Two regressions are run, one for pooled data from main sample stocks and a second using pooled data from control sample stocks. Each regression is based on several million individual trades. Impacts are computed for trades grouped into four size-based bins (0 to 0.1 NMS, 0.1 to 0.25 NMS, 0.25 to 0.5 NMS and 0.5 to 10 NMS).

Table 6: Regression analysis of the effect of anonymity on trade package price drift

Variable	Coefficient	t-stat
Market Cap	$-2.4\times10^{-5***}$	6.30
Buy dummy	-0.717^{***}	2.97
Volatility	0.146^{***}	14.14
Momentum	-0.00743	1.04
Market liquidity	0.0412^{***}	4.70
Size dummy 0.1	3.519^{***}	9.27
Size dummy 0.25	4.398^{***}	14.48
Size dummy 0.5	6.639^{***}	19.93
Size dummy > 0.5	10.64^{***}	26.55
Size × Anonymity interaction 0.1	-0.0113	0.04
Size × Anonymity interaction 0.25	-0.682^{***}	3.64
Size × Anonymity interaction 0.5	-1.194^{***}	5.90
Size × Anonymity interaction > 0.5	-2.382^{***}	8.67
R^2	0.05	
N	2,897,512	

Notes: The table presents panel regression analysis of the effect of anonymity on trade package price drift (the signed basis points difference between the midquote observed immediately prior to the first observation of a package identifier and the value-weighted package price), controlling for firm size (the log of market value in GBP M.), trade direction (a dummy picking out buy packages), volatility (the absolute return over the 24 hours leading up to the first observation on the package ID), momentum (the return over the 24 hours up to the first observation of this package ID, with the sign swapped for sell packages) and package "complexity" (proxied by package execution size in NMS terms). A first set of four dummy variables captures package difficulty, proxied by their size in NMS terms. They have upper bounds of 0.1, 0.25, 0.5, and ∞. To capture the effect of anonymity, a further set of dummies interacts the four size categories defined above and an variable taking on a value of unity during the anonymous trading period. Regression based on 2.98 million observations from treated stocks only. *** indicates 1% significance.

Table 7: Difference-in-differences estimation of the relationship between inside spreads and pre-event stock size

	Size T1	*Size T2*	*Size T3*
Volume	-8.746***	-5.410***	-3.018***
	(6.46)	(4.07)	(4.42)
Volatility	1.769***	1.269***	1.044***
	(6.01)	(4.33)	(6.19)
Log market cap	-0.0507***	-0.0106***	-0.0005***
	(5.87)	(4.35)	(4.62)
Treated sample	34.767***	4.653	-3.720
	(3.02)	(0.58)	(0.75)
Anonymity period indicator	-1.948	-1.128	-2.412**
	(0.84)	(0.30)	(2.19)
Treated × Anonymity	-22.66***	-10.11**	-3.424***
	(4.18)	(2.28)	(2.76)
Constant	85.27***	52.24***	32.41***
	(10.28)	(7.89)	(6.78)
R^2	0.26	0.24	0.19
N	22,779	23,159	23,775

Notes: The table reports the results of the panel difference-in-difference model for the relationship between bid-ask spreads and anonymity, but where we have run separate estimations for three market cap based subsamples of the universe of stocks. The column headed Size T1 , for example, runs the difference-in-difference analysis but only using stocks from the first size tercile of the main sample and the first size tercile of the control sample. The column headed Size T2 uses stocks from the second size terciles of the main and control samples respectively and the column headed Size T3 uses main and control sample stocks from the third size terciles. In each specification, right-hand variables comprise measures of trading activity, realized volatility, firm size and the set of treatment dummies. The dependent variables are defined in Section 2 and the regressors in Section 2.1. The estimator used is robust to clustering effects both within and across panels. (***) indicates 1% significance.

Table 8: Difference-in-differences estimation of the relationship between inside spreads and pre-event order book depth

	Depth T1	*Depth T2*	*Depth T3*
Volume	-9.05***	-3.605***	-3.052***
	(10.90)	(6.47)	(3.21)
Volatility	1.272***	0.8235***	1.170***
	(9.52)	(10.08)	(5.81)
Log market cap	-26.78***	-17.91***	-17.97***
	(6.19)	(8.97)	(4.59)
Treated sample	52.72***	21.60***	-13.91
	(9.77)	(6.02)	(1.58)
Anonymity indicator	-1.943	-3.684***	-1.470
	(0.62)	(2.74)	(0.34)
Treated $\times$ *Anonymity*	-19.93***	-8.16***	-5.636
	(5.20)	(4.68)	(1.26)
Constant	45.92***	28.98***	47.51***
	(10.69)	(9.67)	(5.42)
R^2	0.45	0.39	0.30
N	15,145	15,712	15,712

Notes: The table reports the results of the panel difference-in-difference model for the relationship between bid-ask spreads and anonymity, but where we have run separate estimations for three subsamples of the universe of stocks based on pre-event order book depth. The column headed Depth T1, for example, runs the difference-in-difference analysis but only using stocks from the first tercile of the depth distribution of the main sample and the first depth tercile of the control sample. The column headed Depth T2 uses stocks from the second depth terciles of the main and control samples respectively and the column headed Depth T3 uses main and control sample stocks from the third depth terciles. In each specification, right-hand variables comprise measures of trading activity, realized volatility, firm size, and the set of treatment dummies. The dependent variables are defined in Section 2 and the regressors in Section 2.1. The estimator used is robust to clustering effects both within and across panels. (***) indicates 1% significance.

Table 9: Regression estimation of the relationship between inside spreads and pre-event concentration in order book intermediation

Variable	Coefficient	t-stat
Volume	-0.0490	0.84
Volatility	0.850***	9.35
Log Market Cap	-23.88***	7.19
Market liquidity	0.2907***	3.18
Low Concentration stocks	55.21***	16.39
Medium Concentration stocks	54.56***	13.26
High Concentration stocks	69.29***	14.48
Low Concentration × *Anonymity interaction*	-3.703*	1.93
Medium Concentration × *Anonymity interaction*	-16.60***	4.06
High Concentration × *Anonymity interaction*	-23.10***	7.32
R^2	0.68	
N	31880	

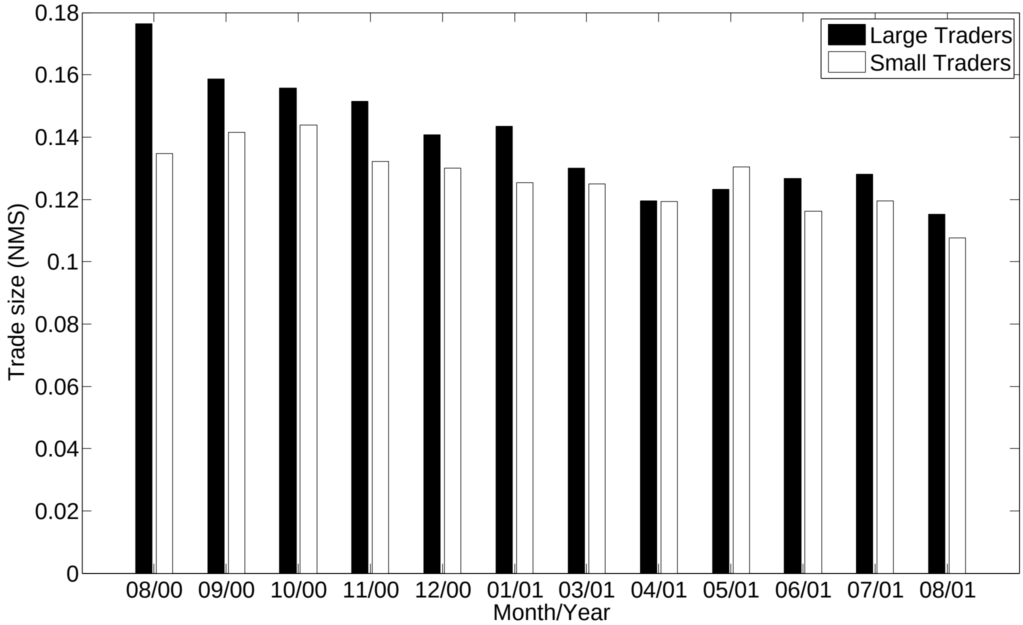
Notes: The table reports the results of panel regressions of inside bid-offer spreads against measures of activity, realized volatility and stock size, at daily frequency. The sample covers treated stocks only. This specification is augmented by a set of three indicator variables constructed by grouping the sample shares into three subsets based on the extent of concentration in their order book intermediation over the three months preceding the introduction of anonymity. This concentration is measured by the market share of order submissions held by the five largest brokers. We add three further variables interacting the previously defined indicators of concentration and an anonymity time dummy. The dependent variables are defined in Section 2 and the regressors in Section 2.1. The estimator used is robust to clustering effects both within and across panels. (***) indicates 1% significance.

Table 10: Regression estimates of price impacts by trader size: pre and post-CCP: 10 trade impact horizon

Variable	*0-0.1 NMS*	*0.1-0.25 NMS*	*0.25-0.5 NMS*	*0.5-10 NMS*
Volume	-0.002	-0.010*	-0.007	0.005
	(-0.30)	(-1.80)	(-1.07)	(0.58)
Volatility	0.28***	0.38***	0.34***	0.34***
	(10.71)	(12.33)	(7.96)	(5.47)
Log market cap	-0.34***	-1.13***	-0.82**	-1.86***
	(1.56)	(4.46)	(2.21)	(2.68)
Large	4.14***	4.06***	3.84***	4.67
	(7.12)	(6.58)	(4.66)	(1.59)
Anonymity	0.16	-0.40	-0.88	1.17
	(0.33)	(-0.82)	(-1.02)	(0.40)
Large $\times$ *Anonymity*	-3.40***	-3.02***	-1.96**	-1.37
	(-5.17)	(-4.38)	(-2.10)	(-0.40)
Constant	4.00***	8.38***	9.36***	7.89***
	(9.25)	(15.57)	(12.59)	(2.92)
R^2	0.02	0.04	0.02	0.02

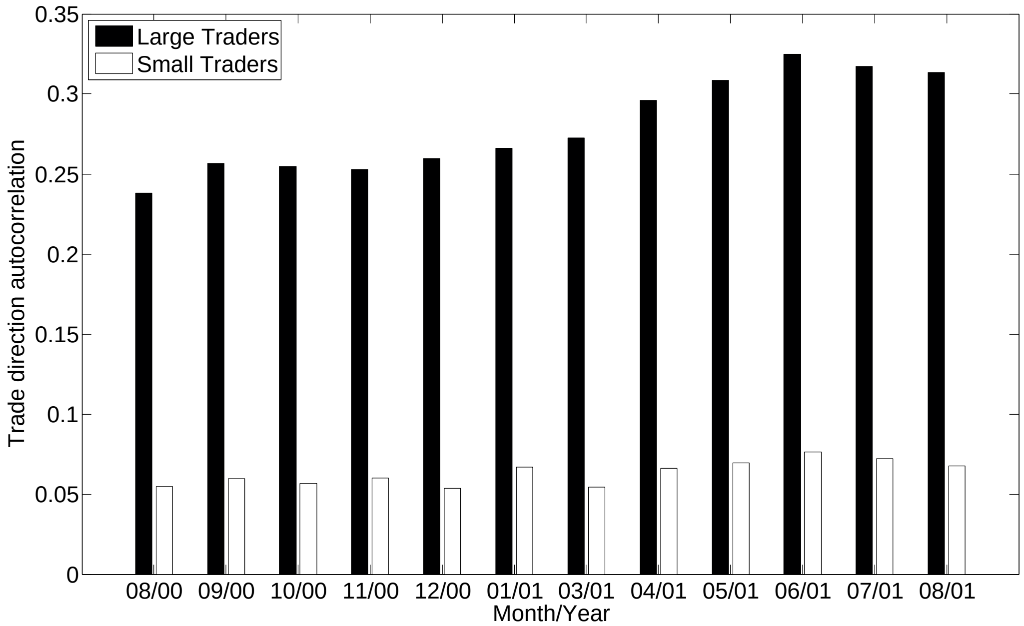
Notes: The table presents results from estimating equation (4). Only treated sample stocks are included. Each row presents coefficients for regressions for impacts within particular trade size bin on volatility and market cap controls, plus dummies to pick out large traders, the post-CCP period and an interaction of the larger trader and post-CCP dummies. Robust t-statistics for the estimates are in parentheses. (***) indicates 1% significance.

Figure 1: Mean trade size by sample month: large traders versus all other traders



Notes: for each month in the main sample we construct the average trade size for all trades in which a large trader participated and mean trade size for all other trades. These are expressed in NMS. Large traders are defined as the most active 5 traders per month in overall volume terms.

Figure 2: Average trade direction autocorrelation by sample month: large traders versus all other traders



Notes: for each month in the main sample we construct autocorrelation in trade direction for all traders. We then compute a monthly average of these autocorrelations for large traders and another average for all other traders. Large traders are defined as the most active 5 traders per month in overall volume terms.

Appendices

A. U.S. regulatory debate and evidence on the value of anonymity

Arguments based on the implications of non-anonymity and order-flow predictability for institutional trading costs were made very clearly during the 2003 debate on post-trade anonymity in NASDAQ stocks. They are of particular relevance to the current study because here post-trade transparency was discussed on its own merits, rather than as a desirable by-product of the introduction of a central counterparty.

This debate arose as trading with market-makers on NASDAQ's SuperMontage was not post-trade anonymous for more than one year after its inception in July 2002.[23] As a result, the NASD quickly argued for reform to improve the trading service offered by its market-makers. Their view of the implications of non-anonymity was made very clear in the manner in which they argued for a rule change with the SEC:[24]

> 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. For example, if members see a pattern in which a particular member is actively buying a security, and it is commonly known that this member handles the orders of several very large institutional customers, such as pension funds or mutual funds, the other

[23]Hendershott (2003), for example, noted that "(...) SuperMontage does not provide post-trade anonymity for traders. The SIZE moniker allows pre-trade anonymity; but, unlike trades on ECNs, the clearing and settlement process for SuperMontage trades reveals trader identities."

[24]Securities and Exchange Commission; Notice of Filing of Proposed Rule Change by the National Association of Securities Dealers, Inc. (...) Relating to a Post-Trade Anonymity Feature in SuperMontage, *Federal Register*, July 2, 2003, Vol. 68, No. 127, Notices.

> members can adjust their trading strategy for that security in anticipation of the strong demand that should develop as the member attempts to fill the order of one or more of its large institutional customers. In such a scenario, the natural result is that the price of the security increases and it becomes more expensive to fill the order. This result commonly is referred to as "market impact." Nasdaq believes post-trade anonymity diminishes market impact, which can help members satisfy their duty of best execution.

The SEC approved the change, agreeing that non-anonymity was likely to "frustrate a firm's ability to efficiently work large orders for its customers or obtain executions at improved prices" and that adopting the Nasdaq proposal would likely "reduce the type of market intelligence that can contribute to market impact." Thus, the SEC argued that the change would "assist broker-dealers in their efforts to satisfy their duty of best execution in working customer orders."[25][26]

Further evidence, that is directly relevant to our work, appears in a series of large-scale academic surveys of the trading practices of institutional investors, which analyse the trade-off between immediacy and trading costs faced by buy-side firms (Economides and Schwartz, 1995; Schwartz and Steil, 1996; Demarchi and Thomas, 2001; Schwartz and Steil, 2002). Two key findings emerge from these studies. First, there is a high demand for anonymous trading from all large investors. Schwartz and Steil (1996) survey U.K. and European traders and report that respondents place very high value on anonymity, regardless of whether they follow active or passive investment strategies. Similarly, the survey of U.S. investment managers by Economides and Schwartz (1995) indicates that majorities of both active and passive managers are

[25] Order Approving Proposed Rule Change by the National Association of Securities Dealers, Inc. (...) Relating to a Post-Trade Anonymity Feature in SuperMontage, *Federal Register*, Vol. 68, No. 189, September 30, 2003, Notices.

[26] Our own discussions with traders at the block desks of large London-based sell-side firms confirmed the relevance of this argument for trading in London. These brokers emphasised the role of stock-specific concentration in intermediation and order flow predictability. and described to us a market structure where the shares of the key players in a stock were well-known to those who regularly traded in that security. Whilst these large firms welcomed the introduction of post-trade opacity, Exchange officials reported to us that other traders missed the "gaming" opportunities that non-anonymity had allowed.

"concerned" or "very concerned" about information leakage.

Second, when anonymity is not enforced, institutional traders demand more immediacy in execution. Schwartz and Steil (2002) argue that this "excessive immediacy" [their words] is integral to non-anonymous trading because of endemic front-running: "(...) it is primarily information on their *identity* and *order size* captured by intermediaries that triggers adverse price movements for institutions." (...) "An institution (...) must give up its identity and order information when it trades, thereby offering signals to broker-dealers as to its *future* buying or selling intentions." While the focus of this evidence is on non-anonymity as a consequence of the intermediated (brokered) nature of the trading process, the same reasoning applies to any environment where trading interest may be inferred via the revelation of identities.

B. Changes in information asymmetries under anonymity

One potential route to explaining why anonymity affects market quality relies on asymmetric information. Huddart, Hughes, and Levine (2001), for example, present a model in which post-trade anonymity is associated with more intense and prolonged informational asymmetries and possibly less efficient markets. The converse argument is made regarding efficiency by Rindi (2008) in the endogenous information acquisition version of her model.

To assess whether the key economic implication of introducing post-trade anonymity is to worsen information asymmetries between aggressive and passive traders, we use the familiar time-series microstructure models introduced by Hasbrouck (1991a,b) to evaluate and compare the size of the information asymmetries before and after the introduction of the CCP. These models give three measures. The most primitive is the equilibrium price impact of a trade, similar in spirit to those we have already calculated in Section 2.4. A variance decomposition then delivers the size of the permanent component of the price process. This is used in recent work to measure the efficiency of the market (Hendershott and Moulton, 2011). Finally, the variance decomposition also gives us the contribution of trading to the size of the permanent component. This is used to measure the scale of any asymmetric information problem.

We estimate the VAR models and associated variance decompositions for each stock in the sample separately for each month in the sample. We then average the results for the stock-months in the pre-anonymity regime and the anonymous regime and present these averages, plus standard errors for these means, in Table 11. For price impacts, the table tells a similar story to that we have already seen – anonymity brings reductions of, in this case, around 30%. However, there is no economically significant change in either the permanent or trade correlated components. The former is just below 50% in both subsamples and that latter close to 25% pre- and post-CCP.

Thus, this exercise reveals no clear evidence of any change in the scale of the asymmetric information problem facing liquidity suppliers to SETS with the introduction of anonymity. Moreover, by these metrics, there is no evidence of any change in market efficiency with the introduction of anonymity. Our results thus provide little support for the implications of models of the effects of anonymity based on asymmetric information.

Table 11: Anonymity and information asymmetries

Measure	Transparent subsample		Anonymous subsample	
	Mean	(s.e.)	Mean	(s.e.)
PI	13.63	(0.386)	10.62	(0.366)
PC	0.47	(0.004)	0.47	(0.004)
TC	0.23	(0.003)	0.24	(0.003)

Notes: The Table presents results from month by month stock-level estimations of return-trade VARs as in Hasbrouck (1991a, 1991b). For each stock and month we compute the price impact of a trade (PI), the size of the permanent component of prices (PC) and the size of the trade correlated component of the permanent price process (TC). The table presents averages, and in parentheses standard errors, of these three measures across stocks and months in the pre-anonymity and the anonymity periods, respectively. Standard errors are equal to the standard deviations of the given measure for the relevant subsample of stocks and months divided by the square root of the number of stock-months in that subsample.