Trading anonymity and predatory behaviour*

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Abstract

We investigate the effect of imposing post-trade anonymity on a pre-trade anonymous market where, previously, identities had been bilaterally revealed post-trade. Introducing anonymity greatly improves liquidity, reduces the price impacts of trades and the execution costs of worked orders. Control samples where anonymity did not change show no liquidity improvement. To identify the source of the liquidity improvements, we compare the implications of an asymmetric information model with a mechanism based on *predatory trading* (Brunnermier and Pedersen, 2005). We find that the evidence supports predatory trading, whereby identities noisily reveal broker market shares and likely future order flow, allowing strategic players to profit from the price pressure caused by large brokers: liquidity improvements are greatest in stocks where trading is heavily concentrated among few brokers and in stocks more susceptible to temporary price pressure. Using trader identifiers, we show that the key beneficiaries of anonymity are the largest traders.

Keywords: Trading anonymity; Limit order trading; Trading costs; Institutional investors; London Stock Exchange.

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Market transparency refers to the extent to which and speed with which information on order flow (trades and quotes) is disclosed to market participants. The study of the effects of transparency on market quality has a long history.[1] In recent times, issues of transparency have played a key role in changes to equity trading systems and practices. The success of 'dark' trading, for example, demonstrates the importance that institutional equity traders attach to keeping their trading intentions concealed.

One particular aspect of transparency concerns whether or not trading is *anonymous*: broker identities may be revealed either before the trade, if they are displayed alongside their quotes or orders, or after the trade, if the parties involved in the execution are identified. Moreover, whether pre or post trade, firm IDs can be revealed either to all market participants or only to a subset of them.

Some recent empirical studies have analysed the introduction of 'multilateral' and *pre-trade* anonymity, where exchanges stopped disclosing the identities of the brokers submitting orders to all other members (see Foucault, Moinas, and Theissen, 2007; Comerton-Forde and Tang, 2009, for Euronext Paris and the Australian Stock Exchange, respectively). These studies find that market liquidity improved and use an asymmetric information argument to explain this improvement: revealing the identities of agents deemed better informed broadcasts the information they hold. This erodes the profits accruing to the informed and therefore their incentives to undertake costly information acquisition in equilibrium (Foucault, Moinas, and Theissen, 2007; Rindi, 2008). Under anonymity, informed agents can expose their orders to the market without fear that prices will adjust before they conduct their own trades.

Our work focusses exclusively on post-trade anonymity, as we study an already pre-trade anonymous market that becomes post-trade anonymous. Further, prior to the change, ID revelation was very restrictive: only the counterparties to a trade learned who they had traded with. Therefore, our anonymity event is the opposite to the pre-trade and multilateral non-anonymity situations studied before. The change was a by-product of the introduction of a central counter-party (CCP) to electronic equity

[1]For examples, see the survey by Foucault, Pagano, and Röell (2010).

trading in London in February 2001. Prior to this date, trading counter-parties learned the identity of the agent on the other side immediately post-execution while after February 2001, traders no longer knew who they had just traded with.[2]

Our empirical work proceeds in two logical steps: does this type of anonymity matter, and if so, why? We first demonstrate that the introduction of post-trade anonymity is associated with a striking improvement in liquidity, which stocks in two control samples do not exhibit. Spreads decline by around 20% and market depth increases by a similar amount. We find that individual trades are associated with significantly smaller price impacts and worked executions suffer much less price drift. We also report that anonymity alters order submission strategies, which become significantly less aggressive. These results are broadly in agreement with those on pre-trade transparency (Comerton-Forde and Tang, 2009) but they require explanation given the seemingly minor change in transparency regime in our case.

The second part of our analysis attempts to shed light on the mechanism that generates the improvements in market quality. We compare the empirical implications of two theories which relate anonymity and liquidity. The first relies on standard asymmetric information (AI) arguments. Examples of this line of work include Huddart, Hughes, and Levine (2001), Foucault, Moinas, and Theissen (2007) and Rindi (2008).[3] The implications of analysis based on AI differ greatly depending on the precise set up of the model. The work of Huddart, Hughes, and Levine (2001) on post-trade anonymity suggests that, with exogenous endowments of private information, anonymity degrades market liquidity and worsens informational asymmetries because the informed remain unidentified and have more scope to use their advantage at the expense of others. In the analysis of Rindi (2008), however, if acquisition of information is endogenous, then anonymity may improve liquidity and efficiency as it strengthens agents' incentives to acquire information.[4]

[2]It is worth noting that the introduction of the CCP did not materially change counter-party default risk in SETS trading, as there was a long-standing LSE insurance mechanism in place to mitigate this risk.

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

[4]In Foucault, Moinas, and Theissen (2007), whether anonymity is good or bad for liquidity is

The second fraemwork we consider as a candidate for understandinfd the links between anonymity and liquidity is the predatory trading (PT) mechanism of Brunnermeier and Pedersen (2005) (hereafter referred to as BP).[5] Predatory trading is conducted by strategic agents who exploit the need to trade on the part of other investors or intermediaries. PT occurs in settings where an investor accounts for a sizeable portion of total volume, at least over a given time window, and where their trades are serially correlated in direction. This makes the investor's order flow predictable to strategic agents, who trade concurrently and then benefit from the price pressure resulting from the combined order flow by reversing their trades at a profit. If, for example, a large investor is buying and is likely to do so across a trading day, then a predator can buy alongside the large investor and when prices have risen due to the pressure exerted on prices, can close the position at a profit. Alternatively, counter-parties might exploit the large investor through liquidity *supply* decisions (i.e. worsening the terms of trade offered and thus inflating the investor's execution costs). Such practices are called 'quote-shading' or 'fading' by Harris (2002) and Angel, Harris, and Spatt (2011). Harris (2002) describes these quoting practices as 'parasitic', and we will use the term 'predatory trading' to cover both the Brunnermeier and Pedersen (2005) sense of the phrase and quote shading. Of course both deteriorate liquidity.

BP establish a clear link between PT and transparency, stating that "The possibility of predatory trading is an argument against very strict disclosure policy" (p. 1827), because transparency may reveal a large trader's need to acquire or offload a position to prospective predators. This reasoning readily extends to anonymity. In a market where a few brokers are known to hold large market shares and to trade with autocorrelated direction, revealing their identities will betray their sustained presence on one side of the market. Note that PT only requires that identities are revealed post-trade but does not require the large trader to be informed about future payoffs. It does,

not clear cut, it depends on the precise values taken by the model parameters.

[5]Closely related theoretical references include Attari, Mello, and Ruckes (2005) and Carlin, Lobo, and Viswanathan (2007).

however, rely on the existence of large traders who 'work' their orders (i.e. split them into small trades and execute dynamically).

A direct link between PT and average market liquidity does not appear in BP as they frame most of their analysis in the context of infrequent, financial distress events that force a large trader to liquidate holdings of shares. This link, however, is intuitively clear and has been made since by other authors (Bessembinder and Venkataraman, 2010): if, as in our case, a feature of market design allows predators to take advantage of other investors frequently, market quality will suffer. Here, knowing they could be gamed once their identities leak, large traders are more aggressive in their order submission strategies. Conversely, the introduction of post-trade anonymity should greatly reduce the scope for predation, thus improving liquidity and reducing the order submission aggressiveness of large traders. Based on the BP mechanism, we develop implications for the effects of anonymity in the *cross-section* of stocks. We argue that PT is likely to be more prevalent in less liquid securities and in securities with more concentrated order flow. Moreover, a direct implication of the PT hypothesis is that repeat traders see the largest benefit in execution costs under anonymity (as they are most likely to be preyed upon).

Our results much more strongly favour the predatory trading hypothesis than they do the asymmetric information one. First we provide evidence that, as the PT hypothesis requires, trading in our sample is highly concentrated. On average, the 5 largest traders in a stock participate in over half of that stock's executions. Moreover, the trade directions of the most active traders are strongly positively autocorrelated. With regard to liquidity, the improvement we observe under anonymity immediately rules out the standard asymmetric information argument of Huddart, Hughes, and Levine (2001). In the cross-section we observe much greater liquidity improvements for small stocks and for stocks with higher trading concentration. The second of these results is in line with the predictions of the PT hypothesis. The former result is also in line with the PT hypothesis but, as small stocks tend to be those with the largest information asymmetries, it is further evidence against the AI story. Rindi

(2008), for example, argues that stocks with large exogenous information asymmetries (small stocks) are likely to suffer, in liquidity terms, from anonymity while stocks in which traders endogenously acquire information advantages (large stocks) are likely to benefit. This result is reversed in our analysis and so, overall, we uncover evidence that is inconsistent with both flavours of the AI hypothesis.[6]

Finally we show that the traders who benefit most from anonymity are those who trade repeatedly and trade the largest volumes. 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. We attribute this to reduced fear of predation. Price impacts for the aggressive executions of all other traders show no significant change with anonymity.

Thus, in our paper, even a very limited form of transparency in identity can degrade market quality, with dramatic effects for small stocks, stocks with high concentration in trading and most clearly for repeat traders. Further, our cross-sectional and trader level results are entirely consistent with predatory trading being the mechanism which degrades liquidity under transparency.

Predatory trading, also described as "order anticipation" or "liquidity detection", is currently a topic of heated debate among practitioners and is very much on the minds of regulators: the SEC recently called for evidence on order anticipation 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." The SEC further notes that "Order anticipation is not a new strategy. (...) Do commenters believe that order anticipation significantly detracts from market quality and harms institutional investors (...)?"[7] This is one of the research questions we address.

[6]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 paper is different to that in their paper.

[7]SEC Concept Release on Equity Market Structure (2010), pp. 54-56. Our Appendix A gives details of earlier policy debates. The implications of transparency for predatory trading were for example very clearly spelt out in the NASD's request to the SEC for a rule change to introduce post-trade anonymity to its quote 'SuperMontage' a few years ago. Our own discussions regarding the introduction of the CCP with LSE broker dealers bear this out. They quite clearly and categorically

We view this study as a contribution to four areas of research all of them currently active. First, it is a *transparency* study – as far as we are aware, the first paper to empirically evaluate how and why the introduction of bilateral and post-trade anonymity affects market quality and trader welfare. Second, our paper supplies evidence consistent with the existence of a specific type of *predatory trading*, facilitated by transparency. PT could, in the cross-section, generate *endogenous illiquidity* leading to higher required rates of returns (Pástor and Stambaugh, 2003) [8] Third, our study forms part of a much older research thread on *imperfect liquidity* and price pressure effects, a line of research generally credited to Shleifer (1986). Fourth and finally, our work is related to the topic of *imperfect competition* in financial markets, as we document the extent of concentration in order book activity. A related study is Biais, Hillion, and Spatt (2010), who state: "(...) our findings suggest one should not take for granted the hypothesis that there is an infinite number of agents perfectly competing to supply liquidity in financial markets." We verify this conjecture, in an equity market that is one of the largest and most mature in the world.

The rest of the paper is set out as follows. A discussion of the data and the anonymity event follows in the next section. Our results are presented in Section 2. Section 2.1 analyses how SETS market quality changed with anonymity. The remainder of Section 2 focusses on identifying the mechanism that generated the change in liquidity. We conclude in Section 3.

1 The market and the data

1.1 The trading environment and the anonymity event

SETS was introduced in 1997 and, at the time of our sample, was available for trade in around 200 of the most liquid stocks from the 1,500 on London's Daily Official

described non-anonymity as generating predatory trading and argued that it became much less prevalent post-CCP.

[8] The link between PT and required returns is made by Brunnermeier and Pedersen (2005).

List. These 200 stocks accounted for around 95% of UK equity market activity. SETS operates as a standard electronic order-driven system, opened and closed with batch auctions.[9] 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 26 February 2001, the London Stock Exchange, in conjunction with the London Clearing House and CrestCo, launched a central counter-party (CCP) service for SETS order book trades. Until then, trades had been settled bilaterally and the identity of each counter-party 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 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."[10]

[9] Standard limit and market orders made up over 99% of all order entries in the sample shares. Other types of orders that were available at the time of the 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.

[10] *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 "Central counterparties offer risk reduction and trading cost benefits," *Financial News*, 4 Sep 2000.

1.2 The dataset

The data we use includes all trades and full order submission data for SETS stocks, allowing us to rebuild the order book. Our data also contain a numeric identifier for each dealer that allows us to track their order submission and trading activity (though not to identify the firms by name). The dataset further 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*.[11]

1.2.1 Main sample construction

We define our sample period to include 6 months of activity either side of the date on which the CCP was introduced to SETS trading (February 26th 2001). We exclude the month of February during which live testing of the new trading arrangements took place for a number of LSE member firms. We expect at least part of the deep changes in order submission strategies under the new transparency environment to have occurred by the start of our second subsample. We also exclude the last 5 trading days of December 2000 during which activity was very low due to the Christmas holiday.[12] The two sub-samples we are left with comprise 125 trading days pre and post-event and run from the end of July 2000 till the end of January 2001, and from 1 March until 30 August 2001, respectively.[13]

[11] An advantage of our package variable compared to that used in all earlier analyses of dynamic institutional trading costs (Keim and Madhavan, 1995, 1997; Chan and Lakonishok, 1993, 1995, 1997; Conrad, Johnson, and Wahal, 2003; Chiyachantana, Jain, Jiang, and Wood, 2004) is that it 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. We are therefore able to track unsuccessful attempts to trade and situations where the first trade materialized long after the initial order submission. This allows us to compute the duration of a package of orders and trades and associated price drift more accurately than has previously been possible. Note that this variable was not made available to those involved in trading. Limitations of this variable are that we do not know the size that the firm originally intended to trade and, also, use of the field was discretionary. It was left blank in 2.5% of our observations.

[12] Our results below do not depend in any way on the omission of the final days of December 2000 or on the choice of a wider exclusion period.

[13] In terms of market direction, the mood was bearish over our sample period, with both the FTSE-100 and FT All-Share indices exhibiting a decline of about 15% between end July 2000 and

Our final sample comprises 134 ordinary shares of companies that were continuously traded on the London exchange's order book during the sample period and did not experience a major corporate action, exhibit unusual price movements or demonstrate excessive return volatility. 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 150m (British Biotech) to GBP 133bn (BP). The sample companies represented over 70% of the total market capitalisation of the London exchange, and over 90% of all trading interest by value in 2001. In the analysis that follows, we will often use each security's "Normal Market Size" (NMS). The NMS was a stock-specific measure of the number of shares in an average institutional execution, computed and regularly reviewed by the Exchange. It was, roughly speaking, a rounded version of 2.5% of recent average daily volume, and so a 1 NMS trade was very large.[14] Table 1 gives data on the cross-stock distribution of some key liquidity and trading activity variables for our main sample. The table makes clear that the stocks are a fairly diverse group. For example, in terms of daily trading activity, De Vere Group traded only 33 times a day on average during the sample, while BT traded around 3000 times a day. Similarly, spreads varied widely across stocks. HSBC had a mean spread of only 15 b.p., while Kewill, an IT firm, had a mean spread of about 360 b.p.

There were slightly more than 17.6 million trades on and off the order book in our main sample over the 12 months. Activity in transaction frequency terms was significantly higher in the second sub-sample, with 55.1% of the total number of trades occurring after introduction of the CCP. However, this imbalance does not hold in terms of NMS traded, with the proportion of total value traded in the second half of the dataset at 51.1%.

end August 2001.

[14]NMS is used for the purpose of expressing some of our variables in units that are comparable across stocks. Note that even though NMS values are reviewed and may be 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 be driven by changes in trading practices related to CCP introduction do not cause problems in our analysis.

The order data comprise slightly fewer than 60 million events, including 26 million order entries, about 20 million partial or complete fills, and 13.5 million cancellations or expiries. Almost 90% of events were related to limit orders, with the rest made up of market order events (9.3%) and orders for execution in the batch auctions (0.7%).

1.2.2 Control samples

To strengthen inference, we construct a control sample of British shares that saw no change in anonymity during our sample period. These companies were traded on the other system operated by the London equity market, the dealership system called SEAQ. To maximize comparability between the two samples, we include only those SEAQ stocks that were continuously members of the FTSE-250 index (to which around 40% of our main sample shares also belong). The control sample comprises 76 stocks, with market capitalizations ranging from GBP 274m to 1.975bn in February 2001. (42 stocks in our main sample had market values within this range at the same point in time.) About 474,000 trades were recorded in the control sample shares during the same period.

Finally, we also construct a control sample of Euronext order book securities. This sample comprises intra-day trading and order book data for 28 stocks, all members of the French CAC-40 index. The sample period is bounded at the end of April by Euronext's own anonymity change, the subject of the study by Foucault, Moinas, and Theissen (2007). For symmetry in the pre and post-CCP periods, we include two months either side of the London event, therefore the Euronext sample covers January to April 2001.

2 Results

2.1 Anonymity, liquidity and order submission strategies

We begin our empirical analysis by conducting some panel regressions to establish the precise effects of the introduction of anonymity on SETS liquidity and on order submission strategies. Variation in SETS liquidity is contrasted with that from the SEAQ and Euronext control samples discussed above. We proceed to 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.

2.1.1 Panel specification

Our opening empirical evidence is based on panel regressions for daily liquidity variables. The most general model we employ is for bid-ask spreads. There, as we have data both for the main sample and the SEAQ control sample, we can estimate a difference-in-differences specification.[15] The difference-in-differences estimation uses a control group to "difference out" confounding factors and isolate the effect of a treatment or event. The assumption implicit in the modelling is that the control and main samples may be different, but the difference between them would have remained constant in the absence of the treatment. Our difference-in-differences model for spreads ($S_{i,t}$) is as follows;

$$\begin{aligned} S_{i,t} &= \alpha_i + \beta_1 V_{i,t} + \beta_2 RVOL_{i,t} + \beta_3 MKTCAP_{i,t} + \gamma_1 D_i^{CTRL} + \gamma_2 D_t^{ANON} \\ &\quad + \gamma_3 D_{i,t}^{CTRL \times ANON} + \epsilon_{i,t} \end{aligned} \quad (1)$$

[15] 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).

where D_i^{CTRL} is an indicator variable that isolates the control sample stocks and D_t^{ANON} is an indicator for the post-CCP period. The final interaction term between control sample and 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 in the model to account for stock-day specific changes in trading conditions. The three regressors are (i) aggregate traded value (in NMS) for stock i on day t ($V_{i,t}$); (ii) daily stock-level realised volatility denoted $RVOL_{i,t}$, based on a 15-minute sampling of order book midquotes (Andersen, Bollerslev, Diebold, and Labys, 2001); (iii) monthly log market cap for each stock ($MKTCAP_{i,t}$). Note that the control variables have been demeaned prior to inclusion in the regression. Thus the intercept coefficient 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.

For our other liquidity variables and for our order placement data, we only have measurements from the main sample, therefore the difference-in-differences approach cannot be used. Hence we run standard panel specifications containing the same set of right-hand side controls as above plus a post-CCP dummy variable only. Denoting the dependent variable of interest by $y_{i,t}$, we estimate;

$$y_{i,t} = \alpha_i + \beta_1 V_{i,t} + \beta_2 RVOL_{i,t} + \beta_3 MKTCAP_{i,t} + \gamma D_t^{ANON} + \epsilon_{i,t} \tag{2}$$

We use the estimated coefficient on D_t^{ANON} to judge the effect of the CCP on order placement patterns and order book depth in the main sample.

The key econometric issues in the estimation of both models above are endogeneity of the right-hand side variables and the possibility of multi-way dependencies in the panel residuals, respectively. To address concerns regarding endogeneity, 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). We estimate all panel models using the robust covariance matrix estimators developed in the recent econometric literature on unobserved heterogeneity (Cameron, Gelbach, and Miller, 2011; Thompson, 2009). The procedure we adopt corrects standard errors for within (stock) and cross-panel (time) dependencies in the regressors and estimated residuals.

In several of our estimations we also include stock fixed-effects.[16] Intuition suggests that there may be "deep" stock-specific factors affecting our dependent variables that must be modelled as constant over the sample period.[17] 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.[18]

2.1.2 Anonymity and inside spreads in the main and control samples

We focus first on inside spreads. For the main sample, we compute the daily time-weighted inside spread, expressed in basis points. For control sample shares, we compute effective spreads, defined as the basis point deviation of traded prices from the midpoint, after correcting for the sign of sell trades. That is, $2 \times 10000 \times I_{i,t} \times (\tilde{P}_{i,t} - M_{i,t})/M_{i,t}$ where $I_{i,t}$ is an indicator of trade direction.[19] A priori, theory based on asymmetric information suggests that the effect of anonymity on liquidity is unclear, while the PT hypothesis points to greater liquidity with anonymity, and hence lower spreads (Bessembinder and Venkataraman, 2010).

The results from estimation of the difference-in-differences model, equation (1), are reported in Table 2. The coefficient on the anonymity indicator indicates that liquidity in the main sample has improved under anonymity, with an economically sizeable downward shift in inside spreads of close to 13 b.p. The coefficient on the interaction variable implies that spreads in the control sample have fallen by only 2.6 b.p. (=

[16]The reported constant is computed as the average of the individual fixed effects.

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

[18]The results are not sensitive to the inclusion of fixed effects though.

[19]Using simple quoted spreads made no difference to the results. We report the results based upon effective spreads here, since they are often considered a better reflection of market conditions than quoted spreads in a dealership structure (as the latter can be kept wide by market makers to protect themselves against informed traders)

13.33 b.p. – 10.73 b.p.) over the same sample period and our inference indicates that this reduction is not significantly different from zero. Thus the stocks for which post-trade anonymity was introduced saw a dramatic improvement in liquidity that was not shared by other stocks traded in London.

The coefficient on the control sample dummy shows that these firms have wider spreads, being smaller on average. In terms of the control variables, the estimates indicate that liquidity is consistently and significantly improved on high activity days (the volume regressor is always negative and significant) and increased volatility reduces liquidity, consistent with volatility proxying for information and/or inventory risk. Finally, larger firms have consistently lower spreads, perhaps because firm size is inversely related to information asymmetry.

2.1.3 Liquidity beyond the inside spread

As detailed above, we cannot use our difference-in-difference approach to analyse the effects of anonymity on depth as we have no reliable depth measures for our SEAQ control sample. Therefore, we estimate panel regressions on the main sample only, using the specification in equation (2).

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. We call these measures outside spreads. In most extant US empirical work, depth is measured using quantities available at the best quotes. Our outside spreads are thus *negatively* related to depth as usually defined. We construct outside spreads for $K = 0.2$, 0.4, 0.6, 0.8 and 1 NMS. Theory based on asymmetric information has no clear prediction for the effect of anonymity on depth, but the PT hypothesis suggests that depth should rise – under reduced fear of predation, agents expose their orders more patiently and contribute to depth.

Table 3 contains results fromour outside spread estimations. We include inside spreads in the Table to verify consistency of the results between this and our previous analysis. The results demonstrate greatly increased order book liquidity, both in tightness and depth terms, in the anonymous regime. The anonymity dummies are all significant and have the expected negative sign. Depth increases (i.e. outside spreads fall) most significantly closest to the best bid and offer, but it is still significantly increased at the 1 NMS level. Consistent with the difference-in-differences estimation, inside spreads are reduced by 13 b.p. on average, an economically large number given an average pre-CCP spread of 60 b.p. for these stocks. For depth, the spread between the implied price of the marginal unit in a 1 NMS buy and a 1 NMS sell drops from close to 500 b.p. to 420 b.p.

2.1.4 Control sample 2: Euronext stocks

As a final step in the analysis of liquidity and anonymity, we employ our control sample of Euronext stocks for the specific purpose of examining whether an industry-wide development in order book trading practices – namely an increase in algorithmic trading in blue chip stocks over the course of 2001 – might have contributed to the changes in liquidity evident in our main sample. Since increased reliance on algorithmic trading would likely have affected all liquid European securities symmetrically, any changes in liquidity and order placement driven by it should be detectable in other European order books.

We estimate equation (2) on the Euronext sample, with size, realised volatility and volume regressors defined as above.[20] For dependent variables, we estimate two measures of liquidity, namely inside book spreads and spreads at the fifth price limit. The spread at the fifth price limit is the difference between the fifth lowest offer price on the book and the fifth highest bid price. As such, it is similar to the outside spread measures constructed for main sample stocks above.

Results from estimating our fixed effects model for the Euronext sample are given in Panel (b) of Table 3 and indicate that there are small post-CCP increases in spreads

[20]The normalization re-expresses raw number of shares in terms of the "Taille Normale des Blocks" defined by Euronext, a concept very similar to the London exchange's NMS.

and reductions in depth on Euronext, but that these are not statistically significant.

2.1.5 Anonymity, order placement and execution

Using the same panel framework, we analyse a second group of variables related to the characteristics of individual limit orders, constructing daily average values for each of these. Specifically, we look at the duration of orders (i.e. the seconds elapsed between entry and deletion or full execution), their fill rate (defined as the mean proportion of an order that is executed) and order size in NMS.

We go on to examine a set of variables characterizing individual limit order placement. We classify each incoming buy or sell order according to how keenly it is priced relative to the best bid or offer (respectively). Orders are classified as marketable, spread-reducing, at-the-spread and outside-the-spread. A hypothetical limit buy is: *marketable* if entered at a price at or above the prevailing best offer; *spread-reducing* if not marketable but at a higher price than the previous best bid; *at-the-spread* if it matches the best bid; and *outside-the-spread* if its price is lower than that of the best bid. For each stock and each sample day, we compute the proportion of each of these order types.

Panel estimations of Equation (2) for our order characteristic and order placement variables appear in Panels (a) and (b) of Table 4, respectively.[21] The effects of post-trade anonymity are again clear in most cases. Anonymity generates a significant decrease in average limit order size and in limit order fill rates. The most spectacular effect occurs in order durations, which fall dramatically. Combining the evidence of unchanged fill rates with lower order durations implies that liquidity suppliers are modifying their order placement decisions more frequently.

The effects of anonymity on order placement are particularly interesting. There is no significant change in the proportion of marketable limit orders post-CCP, but a very marked reduction in the proportion of spread-improving orders. This is balanced by a

[21] We are unable to perform similar estimations for either of our control samples as the UK control sample is not an order driven system and we do not have order level data for the Euronext sample.

near 25% increase in order entry at the best bid and offer and a similarly sized increase in the proportion of orders placed outside the spread. Hence, perhaps unsurprisingly, in the anonymous trading world of tighter markets, liquidity suppliers are less willing to improve inside spreads. Rather, they place orders at and just outside the best prices in the hope that they will attract execution and earn the spread.

2.1.6 Transaction-level pricing issues: average market impact (main and control samples)

We now investigate how the introduction of anonymity has changed the impact that trades have on subsequent prices. If anonymity reduces the scope for PT we would expect impacts to be smaller. If it perpetuates 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 basis points) in event time. These are regressed on eight sets of signed trade indicator variables (plus a constant). Each set of trade indicators contains 5 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 NMS, 0.25 NMS, 0.5 NMS and 10 NMS) and for each size category we distinguish pre and post-anonymity regimes.[22]

We run this impact regression for a pooled sample of order book trades and use the estimated regression coefficients to compute cumulative post-trade returns after 5 trades, for each size category and anonymity regime. Panel (a) of Table 5 contains the results of these calculations. The second column in the Table shows that the difference between the cumulative impact in the pre and post-CCP samples is always positive, indicating that under anonymity, order book executions have smaller post-trade price

[22]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 in increased.

impacts. The third column in the Table shows a Wald test statistic, based on a heteroskedasticity robust covariance matrix, and relevant to the hypothesis that the price impacts are identical pre and post. 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 differences are considerable.

Panel (b) of Table 5 displays results from a similar set of estimations but for the control sample of UK stocks. These results display 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.

A picture of the price adjustment implied by our regressions for order book trades in each anonymity regime can be seen in Figure 1.[23] The plots tell the same story of a downward shift in price impact functions from one sample to the next. For example, the average 0.25 to 0.5 NMS trade would have had a final price impact of around 10.5 b.p. before CCP introduction, while the comparable number under anonymity is around 20% lower at approximately 8.5 b.p. For a smaller execution in the 0.1 to 0.25 NMS range, pre-CCP the final impact would have been around 8 b.p. while under anonymity it is around 6 b.p. – a 25% decrease. Finally, also note from these plots that at all sizes, trades receive much better *immediate* execution as the estimated contemporaneous price impact is smaller – consistent with our earlier evidence on reduced inside spreads. Figure 2 presents impact plots by trade size group for the control sample stocks. It is clear from these plots that there is no change in price impact in the control sample under anonymity.

[23]The plots are based upon the same indicator regression methodology, although this time we compute cross-stock mean parameters from 134 stock-level regressions so they are less dominated by large caps than the impact difference figures discussed above (though they are extremely close).

2.1.7 Trading costs: Package-level price drift

Finally for this section, we analyse the determination of execution costs for worked orders and how they change under anonymity. This analysis is directly relevant to the PT 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 2 or more separate executions.

Our analysis proceeds using a specification similar to earlier literature such as Conrad, Johnson, and Wahal (2003) or Chiyachantana, Jain, Jiang, and Wood (2004). The dependent variable in this regression is the price slippage over the duration of 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 defined as;

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

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.[24]

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

[24] 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.

on NMS executed. The size cutoffs are similar to those in Section 2.1.6 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.

The panel regression estimates are shown in Table 6. Most estimates are strongly statistically significant. The coefficients on the control variables accord, in the main, with intuition. 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.

2.1.8 Summary of results on the effects of anonymity on liquidity

So how did the introduction of post-trade anonymity change SETS liquidity? Overall the change greatly improved liquidity. The market became tighter and deeperand price impacts from single executions and worked orders both fell. Neither UK stocks that did not experience the anonymity change nor European stocks of similar liquidity to those in our main sample saw any liquidity improvement in the period after the introduction of a CCP to SETS trading. These results run directly 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 PT hypothesis and also with the argument that anonymity might increase the incentives of informed *liquidity suppliers* to participate in trading and to undertake costly research.

In Appendix B we provide some 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 the VAR approach of Hasbrouck (1991a) and Hasbrouck (1991b), 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 the theory of Huddart, Hughes, and Levine (2001) would predict. Nor is there evidence that anonymity improves informational efficiency as the endogenous information acquisition model of Rindi (2008) would indicate.

2.2 Empirical evidence on concentration in intermediation and predictability of broker order flows

The estimations above and the contents of Appendix B suggest that there was no change in the asymmetric information problem facing SETS liquidity suppliers with the introduction of anonymity. Thus we switch attention to the predatory trading hypothesis and attempt to evaluate its implications more fully. First, however, we demonstrate that two market features which PT requires, order flow concentration and correlated order flow from large traders, are apparent in our data.

2.2.1 Order flow concentration

The brokerage industry is concentrated. A June 2010 report finds that the top 5 brokers in the US command a market share of equity trading of close to 9% *each*.[25] Again for the U.S., *Wall Street and Technology* similarly reports that "Having consolidated their broker relationships in recent years, buy-side firms execute most of their

[25] "Greenwich Share and Quality Leaders: U.S. Equities", Greenwich Associates, June 2010.

trading with large core brokers. According to recent research from TABB Group, 13 core brokers currently receive 72 percent of the buy-side flow."[26]

The same firms dominate Wall Street and the City of London, so we would expect this concentration to be visible in SETS order book intermediation also. The data confirm this unambiguously. 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 5 firms in limit order submission and in executions across all stocks and months stands at 54% and 55%, respectively. For the top 10 firms, the global mean market share stands at 77.5% for both executions and order submission. This is very stable across the sample period.[27] Related academic evidence reveals similar concentration of order flow executed by Nasdaq market-makers (Chung, Chuwonganant, and McCormick, 2006; Ellis, Michaely, and O'Hara, 2002).

2.2.2 Time dependencies in trade direction

Another necessary ingredient for PT to be possible is that the trading of large dealers is characterised by positive serial correlation in direction. Previous work on the Euronext Paris limit order book, Biais, Hillion, and Spatt (1995, pp. 1686-87), found that market wide order-flow "(...) exhibits a large degree of positive serial correlation" which the authors interpret as caused by "order splitting and imitation".[28]

We measure autocorrelation in trade direction for individual dealers. For each stock, in every month we isolate the trades of the 5 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

[26] "Agency Brokers Are Predicted to Gain Market Share Away From Bulge-Bracket Firms," *Wall Street and Technology*, Dec 19, 2008

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

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

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.

Thus, combining the results from this and the preceding subsection, trading on SETS in 2000/2001 was characterised by high degrees of market concentration and the flows of individual large traders were strongly positively correlated.

2.3 Which stocks and which traders benefit most from anonymity?

To substantiate the evidence consistent with the PT hypothesis, we now evaluate its cross-stock implications as well as ask how the existence of PT might affect execution quality across traders.

2.3.1 Cross-sectional tests 1: small versus large stocks

Predation will be a greater concern for dynamic traders of small stocks than for those trading large stocks. This is due to the fact that the low natural trading interest in small cap stocks makes repeat traders easier to isolate and thus easier to prey upon. Thus, if the PT 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.[29] As large caps are likely to fall in the first camp and small stocks in the second (Easley, Kiefer, O'Hara, and Paperman, 1996), this predicts that the improvement in liquidity associated with anonymity for large caps should exceed that of small caps. Thus, in the cross-section of stocks, the PT and asymmetric information stories have contradictory implications.

[29]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.

To discriminate between these hypotheses, we separate our main sample of stocks into three market cap-based subsamples and create an indicator variable identifying the members of each subsample. Using these indicator variables, we estimate a specification, based on the difference-in-differences model of equation (1), which relates the bid-ask spread to volume and volatility controls, augmented by our three size indicator variables, plus three variables constructed from the interaction of the size indicators with an anonymity dummy. The model does not include a constant for obvious reasons. This model allows us to show how the change in liquidity associated with anonymity varies in the cross section.

Results from this estimation 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, pre–CCP of spreads on market cap – clearly larger stocks have lower pre-CCP spreads. More interesting, though, are the interactions. These show a much greater absolute and proportionate improvement in small cap liquidity than they do in large cap liquidity. In absolute terms, spreads fall by 25 b.p. in small caps and by 3 b.p. in large caps. These figures represent, respectively, 25% of the pre-CCP spread and 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 suggests that PT 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 which is the exact opposite of ours, suggesting that different forces may be at work in their data.

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

BP model an order book with finite depth, and price impact measured by Kyle's λ. In the model, the profitability of predatory strategies depends on price pressure effects that are much harder to generate in a naturally deep order book. This generates the cross-sectional prediction that is it the stocks exhibiting the least depth before the event that stand to benefit the most from the introduction of anonymity as they would be, other things equal, more susceptible to predatory strategies.

We therefore group stocks in terciles according to their average outside spread at 0.2 NMS estimated over the months preceding the event, and create an indicator variable identifying each group. We estimate a panel model similar to that in the last subsection but now, instead of using market cap dummies, we augment the model with our three outside spread indicator variables, plus three variables constructed from the interaction of these indicators with the anonymity dummy.

Table 8 shows the results from this estimation. The combination of each tercile's outside spread dummy with its post-CCP interaction indicates that for low depth stocks in tercile 3, an economically substantial reduction in spreads of about $\frac{1}{3}$ was brought about by anonymity. Stocks endowed with a naturally deep book before the event experienced a much more modest improvement in liquidity, in the order of 10%. The results therefore confirm priors stemming from the BP analysis.

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

The PT 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 predict that the improvement in a stock's liquidity should be positively related to its order flow concentration pre-CCP.

To test this prediction, we compute concentration cross-sectionally over the non-anonymous trading period, by measuring for each stock the proportion of total order flow submitted to the order book by the five largest brokers over the three months preceding the event.[30] We then separate the set of stocks into terciles reflecting low, medium and high order flow concentration and create dummy variables from these non-overlapping subsets. The standard panel model is then augmented with the concentration dummies and their interactions with the anonymity dummy.

The results are reported in Table 9. The estimates on the three concentration dummies reveal that inside spreads increase somewhat with order flow concentration. This may be interpreted as evidence that, pre-CCP, more concentration meant greater potential for PT and thus excessive demand for immediacy on the part of large traders. The estimated coefficients on the interaction variables are fully 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 35%. In the middle group it is 25% and the improvement in liquidity experienced by the least concentrated third of our securities is only 12%.

Thus, to summarise, the results of this and the preceding two subsections indicate that the changes in liquidity across the cross-section of stocks are entirely consistent with liquidity improvements being generated by reductions in PT. Furthermore, the results related to stock market cap are inconsistent with the most plausible version of the asymmetric information story.

2.3.4 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 behaviour or one's future execution strategy. The PT mechanism predicts that the agents who benefit from anonymity are the brokers holding large market share. This is because, due to their large market share and dynamic trading methods, their (uninformed) order flow is predictable. Their benefit should

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

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 sub-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 3 shows, month-by-month, the mean trade size of the large traders versus those of all others. Figure 4 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.

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 counter-party to the trade.

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

$$\tilde{I}_{i,j,t} = \alpha_0 + \beta_1 V_{i,t} + \beta_2 RVOL_{i,t} + \beta_3 MKTCAP_{i,j,t} + \gamma_1 D_{i,j}^{LARGE} + \gamma_2 D_t^{POST} + \gamma_3 D_{i,j}^{LARGE} D_t^{POST} + \epsilon_{i,j,t} \quad (3)$$

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 post-CCP. If large traders benefit most from anonymity we would expect to see $\gamma_3 < 0$.

Table 10 shows results from estimating equation (3) for k equal to 10.[31] In running these estimations we have further subsampled the trade data for each stock, day and trader type to place them into 4 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 coefficients in Table 10 are in line with intuition. 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, aside from in the largest trade category, pre-CCP the trades of more active agents moved prices significantly further 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 significant large/small trader impact differential.

Thus our estimations agree with the intuition above. Under transparency, the traders who lose out are those who trade most frequently. This may be because their identities

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

are correlated with the their information advantage such that identity revelation is akin to information leakage. Alternatively, and more plausibly given our earlier results, large traders may be forced to trade in serially correlated fashion (as their order size is much larger than size available at the touch) and revelation that a large trader is trading may allow a strategic counter-party to exploit this serial correlation through predatory trading or quote shading.

3 Conclusion

We empirically evaluate the effects of introducing post-trade anonymity to a pre-trade anonymous electronic order book. The market we study is dominated by a few large players and the trading direction of these players is strongly serially correlated.

We demonstrate that removal of post-trade, bilateral identification of trade participants significantly affects order submission patterns, has large positive effects on liquidity and significantly reduces the execution costs of single trades and worked orders. Control samples do not display similar shifts in liquidity. We also show that the introduction of anonymity was not associated with a change in the severity of the asymmetric information problem in this market and go on to present evidence that the improvements in market quality under anonymity are driven by reductions in predatory trading (Brunnermeier and Pedersen, 2005). In particular, we show that it is the set of repeat traders that benefits most from the imposition of anonymity.

Thus, whereas non-anonymity is considered a solution to the block trading problem facing uninformed parties in venues featuring trading relationships (Seppi, 1990; Madhavan and Cheng, 1997), our results suggest that it may make the problem of trading in large size worse once any disciplining mechanisms are removed. Based on this, the dealership result that agents commanding large market shares of order flow receive better execution may be reversed in an order book context.

We document very strong order flow concentration in brokerage among a few sell-side firms as a stylised fact of real-world order books, complementing existing evidence from structures that feature designated liquidity suppliers and evidence of concentration in the brokerage industry at large. Electronic markets are much farther from atomistic ideals than we are used to thinking. Given that the London equity market is the second largest national market in the world after the US, whether in terms of traded volumes or capitalisation, and the most active equity trading system outside the US, the reality of less mature markets may correspond to higher concentration still. This may affect future research agendas significantly.

The optimal degree of order flow transparency in trading systems remains controversial in theory and empirically (see e.g. Madhavan, Porter, and Weaver, 2005 and Boehmer, Saar, and Yu, 2005). Our findings add to the evidence that certain forms of transparency can be bad for liquidity. They also help understand the fragmentation of order flow towards "dark pools" and other trading systems designed solely for the purpose of allowing 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 (Seppi, 1990; Röell, 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.

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Table 1: Summary statistics for main sample stocks

Variable	Mean	Std Dev	Min	25^{th}	Median	75^{th}	Max
Mkt Cap (GBP bn.)	8.31	16.16	0.15	1.44	3.43	7.93	132.55
Spread (b.p.)	78.04	55.32	15.75	41.79	61.67	93.11	359.89
Trades	511.88	479.77	33.17	170.87	379.68	615.46	2859.20
Quantity Traded (NMS)	23.18	16.34	3.78	12.36	21.45	28.65	117.45

Notes: the table reports summary statistics for liquidity and trading variables for our main 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 trades per day and mean daily quantity traded in NMS. The tables reports, for each variable, the mean, standard deviation, minimum, maximum and 25th, 50th and 75th percentiles from the cross-stock distribution.

Table 2: Difference-in-differences analysis of inside spreads for the main and the control samples under both anonymity regimes

Variable	Coefficient	(t-stat)
Volume	-0.211***	(3.94)
Volatility	0.677***	(7.66)
Log market cap	-49.39***	(6.21)
Control sample dummy	39.41***	(4.37)
Anonymity dummy	-13.33***	(7.15)
Control sample × Anonymity interaction	10.73***	(3.16)
Constant	71.26***	(12.42)
R^2	0.68	

Notes: the table reports the results of panel estimation of variables measuring inside bid-offer spreads of the main and control sample shares against measures of activity, realised 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 2.1 and the regressors in Section 2.1.1. The estimator used is robust to clustering effects both within and across panels. The number of observations is 33,500 stock-days for the main sample and 19,000 for the control sample. (***) indicates 1% significance.

Table 3: Panel regression analysis of the effect of anonymity on liquidity

	Constant	t-stat	Volume ($\hat{\beta}_1$)	t-stat	Volatility ($\hat{\beta}_2$)	t-stat	Log cap ($\hat{\beta}_3$)	t-stat	Anonymity ($\hat{\gamma}$)	t-stat
(a) Main sample										
Spread	59.25***	(11.70)	-0.182***	(3.70)	0.684***	(8.55)	-38.91***	(3.74)	-13.40***	(7.09)
Outside Spread 0.2	95.58***	(16.79)	-0.445***	(4.50)	1.183***	(10.55)	-40.59***	(4.17)	-12.70***	(5.43)
Outside Spread 0.4	143.38***	(17.66)	-0.792***	(4.58)	1.857***	(10.16)	-40.23***	(3.64)	-14.70***	(3.86)
Outside Spread 0.6	209.81***	(17.56)	-1.284***	(4.59)	2.637***	(8.46)	-34.60***	(2.48)	-21.65***	(3.52)
Outside Spread 0.8	310.27***	(13.62)	-2.069***	(4.41)	3.613***	(5.87)	-29.30	(1.05)	-34.32***	(3.04)
Outside Spread 1	492.94***	(10.67)	-3.498***	(4.21)	4.563***	(4.22)	-8.30	(0.13)	-79.63***	(4.09)
(b) Control sample 2 (Euronext stocks)										
Spread	21.12***	(36.97)	-0.0404***	(5.93)	3.218***	(5.09)	-4.675***	(11.89)	0.344	(0.78)
Outside Spread	103.17***	(41.06)	-0.151***	(5.12)	11.57***	(4.14)	-13.34***	(6.52)	2.012	(1.12)

Notes: the table reports the results of panel estimation of variables measuring liquidity of the main and control sample shares against measures of activity, realised 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 2.1 and the regressors in Section 2.1.1. 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. The number of observations is 33,500 stock-days for the main sample and 2,238 for the control sample of Euronext stocks.

Table 4: Panel regression analysis of the effect of anonymity on order execution and order placement

	Constant	t-stat	Volume ($\hat{\beta}_1$)	t-stat	Volatility ($\hat{\beta}_2$)	t-stat	Log cap. ($\hat{\beta}_3$)	t-stat	Anonymity ($\hat{\gamma}$)	t-stat
(a) Order execution										
Order size	0.204***	(8.37)	0.00167***	(6.97)	0.000182	(0.27)	-0.12266**	(2.19)	-0.0323***	(2.81)
Order duration	2546.82***	(7.04)	-9.261***	(3.07)	-19.30***	(4.42)	-1424.18**	(2.44)	-878.47***	(5.77)
Order fill rate	0.426***	(56.09)	0.00148***	(5.83)	-0.00015	(1.26)	0.00853	(0.94)	-0.022***	(3.94)
(b) Order placement										
Marketable	0.170***	(39.30)	0.00044***	(6.25)	-0.00025***	(2.95)	0.0022	(0.40)	-0.0026	(0.90)
Inside	0.463***	(63.85)	-0.00031***	(4.70)	0.001021***	(6.55)	-0.0002	(0.02)	-0.087***	(13.21)
At Best	0.185***	(34.37)	-5.7×10^{-6}	(0.12)	-0.00032***	(4.33)	-0.020***	(2.91)	0.044***	(9.78)
Outside	0.167***	(29.41)	-0.00014***	(2.45)	-0.00036***	(5.39)	0.021***	(3.27)	0.045***	(8.31)

Notes: the table reports the results of panel estimation of variables measuring order execution, placement and use of worked order strategies against measures of activity, realised 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 2.1 and the regressors in Section 2.1.1. 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. Estimation is based on 33,500 stock-days.

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

Size cutoff (NMS)	Cum. return difference (b.p.)	Wald stat.
(a) Main sample		
0.1	0.93***	375.65
0.25	1.76***	175.11
0.5	2.03***	81.33
10	4.40***	104.30
(b) Control sample		
0.1	-0.78	0.931
0.25	-0.66	0.113
0.5	1.41	0.432
10	0.000889	3.09×10^{-7}

Notes: The Table presents the difference between and the 5-trade impact of single trades in the anonymous and the non-anonymous regimes. Estimates are based on pooled cross-sectional time-series regression estimation of single-trade returns, based on over 7 million trades. The second column in the Table shows the difference between the cumulative impact in the pre and post samples in basis points, the third column in the Table shows a Wald statistic based on a heteroskedasticity robust covariance matrix. *** indicates that the cumulative return difference is significant at 1%.

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

Variable	Coefficient	(t-stat)
Market Cap	$-2.5\times10^{-5***}$	6.18
Buy dummy	-0.73^{***}	3.11
Volatility	0.151^{***}	14.74
Momentum	-0.010	1.33
Size dummy 0.1	3.48^{***}	9.30
Size dummy 0.25	4.36^{***}	14.43
Size dummy 0.5	6.58^{***}	19.60
Size dummy > 0.5	10.62^{***}	26.22
(Size $\times$ Anonymity) dummy 0.1	-0.017	0.06
(Size $\times$ Anonymity) dummy 0.25	-0.68^{***}	3.71
(Size $\times$ Anonymity) dummy 0.5	-1.19^{***}	5.98
(Size $\times$ Anonymity) dummy > 0.5	-2.37^{***}	8.68
R^2	0.05	

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. *** indicates 1% significance.

Table 7: Difference-in-differences estimation of the relationship between liquidity improvement and stock size before anonymity

Variable	Coefficient	(t-stat)
Volume	-0.2010***	-3.86
Volatility	0.995***	8.69
Small stocks	102.30***	13.02
Medium stocks	46.33***	21.96
Large stocks	30.79***	35.74
Small × Anonymity interaction	-24.05***	-3.76
Medium × Anonymity interaction	-11.08***	-8.32
Large × Anonymity interaction	-2.808***	-4.02
R^2	0.65	

Notes: The table reports the results of panel estimation of variables measuring inside bid-offer spreads against measures of activity and realised volatility. This specification is augmented by a set of three indicator variables constructed by grouping the sample shares into three subsets based on stock level market cap over the 3 months preceding the introduction of anonymity. We add three further variables interacting the previously defined indicators of size and an anonymity time dummy. The dependent variables are defined in Section 2.1 and the regressors in Section 2.1.1. The estimator used is robust to clustering effects both within and across panels. Estimation is based on 33,500 stock-days. (***) indicates 1% significance.

Table 8: Difference-in-differences estimation of the relationship between liquidity, anonymity and pre-event order book depth

Variable	Coefficient	(t-stat)
Volume traded	-0.0613	1.18
Volatility	0.77***	8.21
Log market cap	-21.37***	5.54
Depth tercile 1	53.18***	12.49
Depth tercile 2	48.52***	28.91
Depth tercile 3	77.62***	14.82
Depth tercile 1 × Anonymity interaction	-4.849***	6.03
Depth tercile 2 × Anonymity interaction	-11.10***	10.44
Depth tercile 3 × Anonymity interaction	-27.21***	5.18
R^2	0.69	

Notes: the table reports the coefficients estimated from ... Absolute value of t-statistics in parentheses. A ** denotes that the coefficient is significantly different from zero at the 5% level for a two-tailed test and a *** denotes significance at the 1% level.

Table 9: Difference-in-differences estimation of the relationship between liquidity improvement and concentration in order book intermediation before anonymity

Variable	Coefficient	(t-stat)
Volume	-0.046	-0.76
Volatility	0.815***	9.48
Log market cap	-24.24***	-7.18
Low Concentration stocks	53.80***	15.36
Medium Concentration stocks	62.63***	13.43
High Concentration stocks	63.81***	15.38
Low Concentration × Anonymity interaction	-6.443***	-7.04
Medium Concentration × Anonymity interaction	-14.66***	-3.29
High Concentration × Anonymity interaction	-22.37***	-7.16
R^2	0.68	

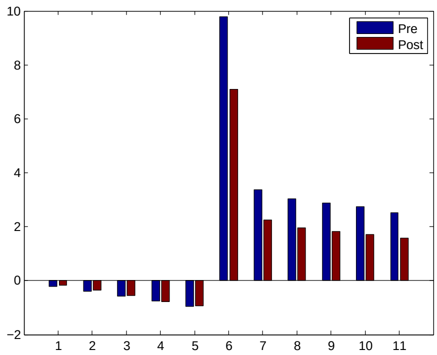
Notes: The table reports the results of panel estimation of variables measuring inside bid-offer spreads against measures of activity, realised volatility and stock size. This specification is augmented by a set of three indicator variables constructed by grouping the sample shares into three subsets based on the extent of concentration in their order book intermediation over the 3 month preceding the introduction of anonymity. This concentration is measured by the market share of order submissions held by the 5 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 2.1 and the regressors in Section 2.1.1. The estimator used is robust to clustering effects both within and across panels. Estimation is based on 33,500 stock-days. (***) indicates 1% significance.

Table 10: Price impacts by trader size: pre and post-CCP: 10 trade impact horizon

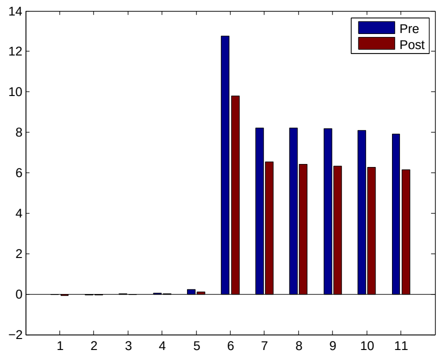
Variable	0-0.1 NMS	0.1-0.25 NMS	0.25-0.5 NMS	0.5-10 NMS
Constant	4.00***	8.38***	9.36***	7.89***
	(9.25)	(15.57)	(12.59)	(2.92)
Volume	-0.002	-0.010*	-0.007	0.005
	(-0.30)	(-1.80)	(-1.07)	(0.58)
Volatility	0.28***	0.38***	0.34***	0.34***
	(10.71)	(12.33)	(7.96)	(5.47)
Log market cap	-0.34***	-1.13***	-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 × Anonymity	-3.40***	-3.02***	-1.96**	-1.37
interaction	(-5.17)	(-4.38)	(-2.10)	(-0.40)
R^2	0.02	0.04	0.02	0.02

Notes: the table presents results from estimating equation (3). 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 below. (***) indicates 1% significance.

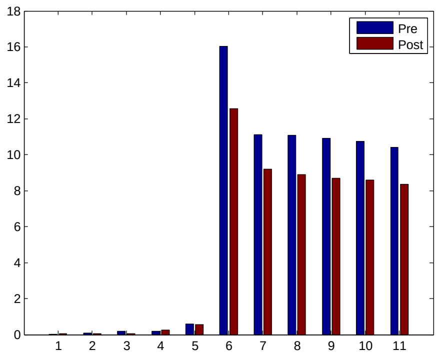
Figure 1: Cumulative tick-time pre and post-trade price impacts by trade size and anonymity regime



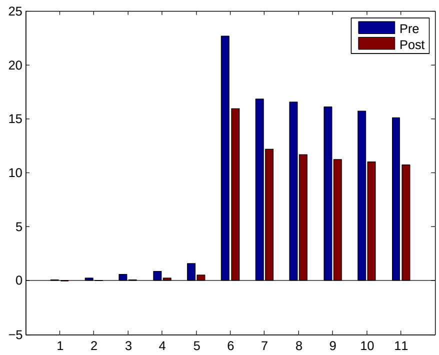
(a) 0 - 0.1 NMS



(b) 0.1 - 0.25 NMS



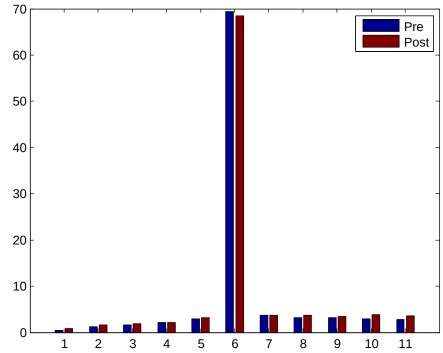
(c) 0.25 - 0.5 NMS



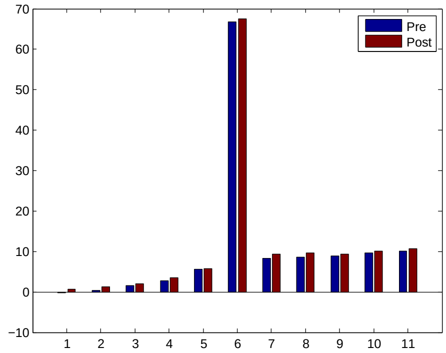
(d) 0.5 - 10 NMS

Notes: the blue bars show the price impact function in the non-anonymous regime and the red bars the same function under anonymity. The price impacts are estimated from regressions of transaction price changes on 8 sets of signed indicator variables to separate 4 trade size categories under each anonymity regime. The coefficients from the stock-specific regressions are averaged with equal weights. Estimation is based on over 7 million trades.

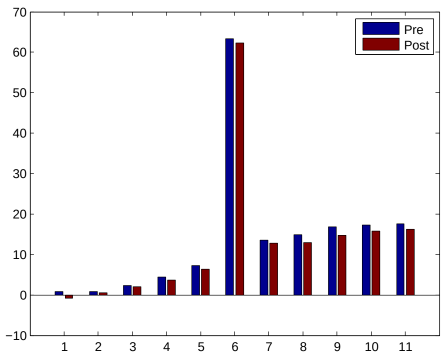
Figure 2: Cumulative tick-time pre and post-trade price impacts by size and anonymity regime – control sample



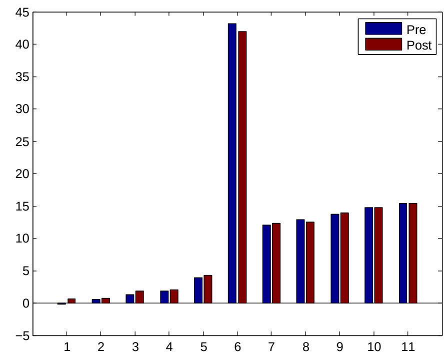
(a) 0 - 0.1 NMS



(b) 0.1 - 0.25 NMS



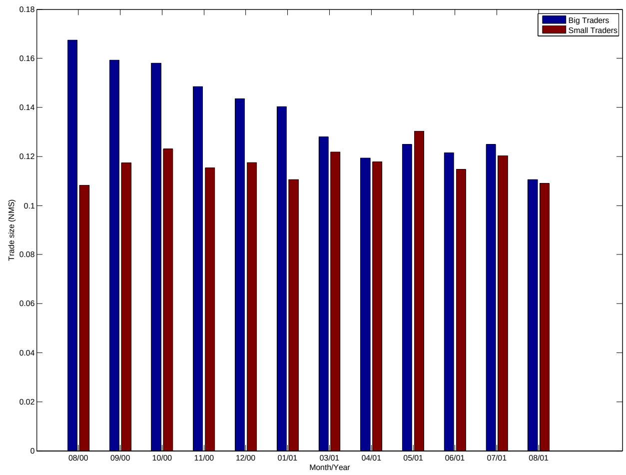
(c) 0.25 - 0.5 NMS



(d) 0.5 - 10 NMS

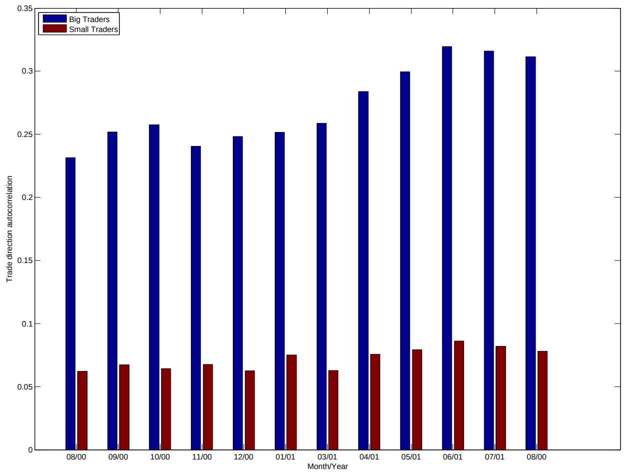
Notes: for control sample stocks that did not experience a change in anonymity, the blue bars show the price impact function in the SETS non-anonymous regime and the red bars the same function when SETS trading was anonymous. The price impacts are estimated from regressions of transaction price changes on 8 sets of signed indicator variables to separate 4 trade size categories under each anonymity regime. The coefficients from the stock-specific regressions are averaged with equal weights. Estimation is based on over 400,000 trades.

Figure 3: 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 4: 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 – US 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.[32] 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:[33]

> 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 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

[32]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."

[33]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.

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."[34][35]

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 UK 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 US investment managers by Economides and Schwartz (1995) indicates that majorities of both active and passive managers are "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 asymmetic information. Huddart, Hughes, and Levine (2001), for example, present a

[34] 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.

[35] 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.

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 2.1.6. 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.