Banning Short Sales and Market Quality: The UK's Experience

Abstract:

We study the effects that the ban on short sales of shares in financial firms introduced in late 2008 and removed early 2009 had on the microstructure and the quality of UK equity markets. We show that the ban did nothing to affect order flows: financial stocks were being more aggressively sold off than their peers pre-ban and this situation persisted through the ban period. Trading volume in financials was massively reduced, however. The ban also decimated order book liquidity for financials. The deterioration was symmetric, affecting the limit buy and limit sell side of the order book equally. Finally we show that, through the period of the ban, markets for financial stocks were substantially less efficient and that the role of the trading process in aiding price discovery was greatly reduced. The effects identified above were largely reversed once the ban was lifted. The persistence of the deterioration in market quality and liquidity though the relatively long-lasting UK ban on short selling suggest that other major market developments such as the TARP program were not responsible since these were concentrated in the early half of the ban. We thus argue that the short selling ban was responsible for detrimental effects on the quality of UK equity markets and that, far from being stabilising, the ban exacerbated problems in valuing UK financial stocks.

JEL classification: G14, G18

Keywords: short-selling, liquidity, market efficiency

1. Introduction

Short selling is the practice of selling a security that an agent does not own. Speculators short sell a security with the intention of buying it back at a later date at a lower price, so as to profit from a price decline.[1] While frequently attracting ire from executives of companies subjected to ~~short selling pressures~~the practice, some form of short~~ sell~~ing is ~~usually~~ permitted in most major stock markets since short sellers may add liquidity to the market and can contribute to price discovery. A large body of academic literature summarised below confirms that, on average, the presence of short sellers is beneficial for liquidity and price formation.

Amid the turmoil in financial markets as the banking crisis of 2008 intensified, however, the U.K.'s Financial Services Authority (FSA) took the step of banning short sales of the equities of a number of financial institutions. New provisions to the Code of Market Conduct were announced on Thursday 18th September 2008 effective 00:01am the following day. The provisions prohibited the creation or increase of net short positions, naked or covered, in publicly quoted financial companies and required daily disclosure (from 23rd September) of all net short positions in excess of 0.25 per cent of the ordinary share capital of the relevant companies, together with disclosure of net short positions held at close on 19th September. The ban included intraday trading and had a global reach such that shorting of U.K. financial shares outside of the U.K. was also banned. The ban extended to cover shorting through derivatives, contracts for differences and spread betting, but since only ordinary and preference shares were covered by the ban short positions in bonds and credit derivatives were still possible. Market makers were exempt. The announcement specified that the provisions would remain in force until 16th January 2009 but that they would be reviewed after 30 days. ~~Both naked and covered short selling were banned under the provisions.~~

[1] The seller might have arranged to borrow the security from a third party for delivery to the buyer at settlement (a covered short) or may simply promise to deliver (a naked short).

Stocks in 32 financial firms were covered by the FSA's ban at the time of announcement.[2] ~~The FSA defined financial companies as banks, insurers or the parent companies of banks or insurers. This meant that stocks of U.K. listed fund managers, brokers and the London Stock Exchange itself were not subject to the ban. Regulators around the world introduced similar restrictions at similar times, although the coverage and specifics of the restrictions on short selling differed across jurisdictions.~~

The motivation for the new provisions banning short selling was clarified in a speech by Sir Callum McCarthy, Chairman of the FSA, on the evening of 18th September 2008.[3]

> "We have been much concerned – as have many – at the volatility and what I would describe as incoherence in the trading of equities, particularly for financial institutions. There is a danger in a trading system which allows financial institutions to be targeted and subject to extreme short selling pressures, because movements in equity prices can be translated into uncertainty in the minds of those who place deposits with those institutions with consequent financial stability issues. We have seen acute examples of this phenomenon in both London and New York this week."

His speech echoed the statement of his Chief Executive, Hector Sants, who earlier in the day said: "While we still regard short selling as a legitimate investment technique in normal market conditions, the current extreme circumstances have given rise to disorderly markets."

The statements from senior executives at the FSA make it clear that the ban on short sellers was in response to exceptional market events~~s observed in the market place~~. Thus, to the extent that they were aware of it, regulators ignored existing academic research on short-selling in stable market conditions, almost all of which suggests that short-sellers have positive effects on market quality. As the regulatory response was predicated on short-sellers performing very different roles in stable versus turbulent markets, it seems worthwhile to analyse the quality of UK equity markets in Autumn 2009 and to evaluate the effects short-sellers might have had in those volatile ~~markets~~times. ~~This study seeks to do exactly that.~~ We study how banning short sellers from operating in UK equity market in

[2] Some stocks were added to the list after the initial announcement and some companies were taken over during the period of the ban, unfortunately, too few to analyse separately.
[3] Speech by Sir Callum McCarthy at the City Banquet, The Mansion House, London.

Autumn 2009 changed market quality (defined below). ~~Certain o~~Other studies seek to do similar work on US and other markets and t~~. T~~hese are surveyed in section 2.

The main innovation in our study is in the quality of the microstructural data we analyse.[4] We have access to full order level data and signed transaction information on all stocks traded on the London Stock Exchange. From the order book data we can compute separate measures of buy and sell liquidity and as the trade data sign~~s~~ every execution precisely, we can measure buy and sell volume, and thus net order flow. This allows us to go far beyond the study of prices, bid-ask spreads and volumes contained in prior work on emergency short sales bans (e.g. Beber and Pagano, 2009; Boehmer, Jones and Zhang, 2009; Harris, Namvar and Phillips, 2009). For example, we can study whether financial stocks were subject to sustained and unusual selling pressure relative to other stocks. Further, given that the FSA's policy intervention was explicitly designed to be asymmetric in its effects on traders, targeting short sellers but not long sellers or buyers, one might conjecture that it would affect trading and/or liquidity on the buy and sell sides of the market differently. Such asymmetries can only be detected using data such as that we employ.

We focus on the following ~~features~~ measures of UK market quality around the time of the short sales ban:

- Trading activity: we measure volumes and, more interestingly, order flows (i.e. net aggressive buying pressure) in financial stocks versus non-financials.
- Liquidity: we examine spreads and measures related to the depth of the limit order book. We can analyse buy and sell side depth separately and thus evaluate whether the ban on short-sales had an asymmetric effect on liquidity.
- Efficiency: via the techniques introduced by Hasbrouck (1991) we calculate the proportion of variation in returns that is driven by information, as opposed to noise. This has been used as a measure of market efficiency by, for example, Hendershott and Moulton (2009) in preference

[4] Clifton and Michayluk (2010) examine the U.K.'s ban using similar data to ours. Their paper confirms several key order book developments that we note but it does not analyse the justification of the FSA's move by considering developments in the pre-ban period. Further, it does not discuss the evolution of market efficiency and focuses instead on liquidity.

to more crude and far less ~~informative~~ precise measures such as the autocorrelation in returns.

- Price discovery: we evaluate the contribution of trades to the determination of the efficient market price, again using the Hasbrouck (1991) technology.

We use these ~~data~~ measures to address two main issues: Can we identify the "disorderly" conditions that prevailed in the period prior to the ban's introduction, and did the change in rules on short sales do anything to remedy the "incoherence" of stock markets at the time?

The answer to both of the questions above is "no". We struggle to identify any factors that would justify regulatory intervention specifically to support financial sector stocks. While prices were falling and there was strong negative order flow (i.e. selling pressure) before the ban, this was true for both financial and matched non-financial stocks. Further, efficiency and the role of trading in price discovery declined pre-ban by roughly the same amount for financials and non-financials. It is therefore not clear to us why the FSA felt it needed to intervene specifically to change the nature of trading in the equities of financial sector stocks. Any disorderly conditions appear to have been market-wide and not concentrated in ~~the~~ financial~~ sector~~s.

While we find few differences between the behaviour of financials and non-financials before the ban, once the ban was enacted differences become very apparent: liquidity drained~~s~~ from the order book for financials to a much larger extent than for non-financials; transactions costs for small and large trades increased much more dramatically and trading volumes fe~~a~~ll much more dramatically for financials than non-financials.[5] Finally, during the ban, efficiency and the information content of trading deteriorated much more for financials than non-financials. None of these moves would appear to be in line with the objectives of regulators.

[5] Research based on bid ask spreads at the top of the order book tell a limited albeit consistent story. Spreads for control group stocks during the ban were 79% higher than during the pre-crisis period while spreads for financials rose by 173%. The cost of executing a market sell order for 0.25% of the average daily volume of control group stocks rose by 137% while the cost for financial stocks jumped by over 600%. While spreads suggest that liquidity was generally lower during the ban and especially so for financials, our calculations of the costs of trading realistic numbers of shares show just how little liquidity was present for financial stocks during the ban.

Furthermore, we find no evidence that restrictions on one set of participants – short sellers – had asymmetric effects on the market. Liquidity drained more-or-less equally from both the bid and offer-sides of the order book, and while volume fell, aggressive sells and aggressive buys fell by similar amounts, leaving order flow unchanged (and thus still negative). If by removing short sellers the FSA had hoped to make buying financial stocks cheaper or more attractive their move failed. Trading in financial stocks, whether to buy or to sell, became much more expensive and less attractive. Finally, we also show that the ban resulted in a shift of trading *off* the limit order book towards *darker* bilateral trading between dealers. Again, it is unlikely that the FSA wished to shift the supply of liquidity towards less transparent segments of the market as this would likely contribute to the reduced efficiency and slower rates of price discovery that was observed for financials.

The continued bans on short selling in some jurisdictions, the 2011 re-introduction of restrictions on short selling in France, Spain, Italy and Belgium and published comments by regulators suggest that some policy-makers still think that such changes to trading rules can improve the quality of trading in equities and enhance financial stability. Our microstructure analysis of the market for U.K. financial stocks around the 2008/09 ban on short selling gives little support for such views. ~~We confirm the evidence from the U.S. which indicates that trading volumes were decimated and bid-ask spreads greatly increased for financials. We go on to provide novel evidence that the ban did nothing to affect order flows in banned stocks, that depth drained symmetrically from the buy and sell sides of the markets for financials and that both market efficiency and the role of trading in price discovery were greatly reduced for financial stocks.~~ Our results suggest that the positive contribution of short sellers to market quality in normal times found in ~~the~~ previous literature did not turn negative during the crisis.

One additional benefit of our study is that we can take advantage of the relatively long-lived FSA ban on short selling. Studies of the effect of the SEC's short sales ban are complicated by its very short duration and the multitude of other policy initiatives and news that were emerging at the same time. For example the announcement and introduction of the Troubled Asset Relief Program (TARP) were contemporaneous with the introduction and removal of the US ban respectively. We show that the detrimental effects on liquidity and market quality were stable and held perssitently~~held fairly consistently~~ throughout the relatively long-lasting U.K. ban on short selling, but largely disappeared once it was lifted. ~~The major events seen in the weeks around the introduction of the short selling ban might be expected to have relatively long lived effects on liquidity and efficiency. However the deteriorations we note were both persistent and stable. There is no evidence of gradual improvement in liquidity or efficiency through the months of the U.K. ban as would be expected as the effects of major shocks die away. Most tellingly, t~~The sharp ~~subsequent~~ improvements in liquidity and efficiency coinciding with lifting of the ban strongly suggest that the FSA's ban on short selling was to blame rather than other market developments.

The rest of the paper is structured as follows. Section 2 contains a summary of the key theoretical and empirical findings from the short selling literature. Section 3 details the data used in the analysis. Section 4 presents our empirical results and the paper closes with conclusions in Section 5.

2. A ~~Brief~~ Review of the Literature on Short Selling and Hypothesis Development

There is a large literature suggesting that short selling enhances market efficiency and price discovery beginning with Miller (1977) and Harrison and Kreps (1978). In Miller's work, short-sales constraints exclude pessimistic investors and result in an upward bias to stock prices. Diamond and Verrecchia (1987) extend this work to a~~a~~ rational expectations setting in which negative information is fully incorporated into prices, ~~framework in which the presence of options markets (or other related markets) allows pessimistic investors to establish appropriate positions. These positions allow negative information to be incorporated into stock prices, moving them towards 'fair value'~~ but ~~possibly~~ at a slower pace than when shorting is allowed.

On the empirical front, Geczy, Musto and Reid (2002), Ofek and Richardson (2003) and Reed (2007), among many others, suggest that stock prices do not fully incorporate information in the presence of short sales constraints. Bris, Goetzmann and Zhu (2007) ~~compare equity markets around the world and~~ demonstrate that negative information is incorporated into prices faster in markets where short sales are allowed~~, and~~. Daouk and Chaoenrook (2005) analyse changes in short-selling restrictions in 111 countries and ~~conclude~~ show that allowing short selling improves market quality~~, based on an analysis of changes in short selling restrictions in 111 countries.~~ .

Boehmer, Jones and Zhang (2008) analyse individual short selling trades using proprietary NYSE data and show that short sellers are, on average, better informed than other~~s~~ traders and thus contribute to efficient pricing. Fotak, Raman and Yadav (2009) concentrate on naked short sellers and ~~also conclude that their impact is, on average, positive since~~show that they ~~naked short sellers~~ function as liquidity providers and value arbitrageurs. Similarly, Diether, Lee and Werner (2009) argue that short sellers both predict future stock performance (in that heavily shorted firms have negative future returns) and can recognise and correct transient market overreactions. Many other papers suggest that, on average, the presence of short sellers increases market efficiency (see, for example, Saffi and Sigurdsson, 2011).

However, the FSA's ban on short selling and similar moves in other jurisdictions~~the SEC's separate moves on naked and covered short selling~~ were not justified on the basis of the average effect of the presence of short sellers. Indeed the FSA made it clear that they believed that short sellers provide valuable services in normal times. Rather, their actions were motivated by supposed predatory actions of short sellers in destabilised markets. Shkilko, Van Ness and Van Ness (2009) argue that short selling may cause excessive price pressure. They analyse days during which prices fall substantially and then quickly rebound and show that short selling at the beginning of the day is often aggressive and has a causal effect on the magnitude of declines, consistent with Brunnermeier and Pedersen's (2005) model of predatory trading. Focusing on U.S. financial stocks during the 2008 crisis, however, Fotak, Raman and Yadav (2009) find no evidence that that the sharpest price declines were caused by naked short selling.

Harris, Namvar and Phillips (2009) evaluate the effects of the SEC's ban on the price level of US stocks~~ and on the wealth transfer that resulted~~. Using a factor~~-analytic~~ model they conclude that the ban inflated financial sector stock values by 10-12% on average and that the subsequent reversal after the ban was lifted suggests the ban contributed to the temporary exclusion of negative value opinions from the market. Based on analysis at a higher sampling frequency, Boehmer, Jones and Zhang (2009) contest this conclusion. They argue that the sharp price increase in stocks subject to the ban was probably due to the effect of the Troubled Asset Relief Program (TARP) announced alongside the short sales ban. They also study some measures of liquidity (i.e. spreads and volume), showing that both deteriorated severely during the ban. Beber and Pagano (2009) consider the impacts of short selling restrictions globally, exploiting the differential timing and coverage of restrictions in different jurisdictions. They use end of day data to show that the restrictions were detrimental to liquidity and failed to lift stock prices (with the possible exception of financial stocks in the United States as analysed by Boehmer *et al*.).[6]

Testable hypotheses~~In the context of restrictions on short selling, considering liquidity on both sides of the order book separately also gives insight. At one extreme, in a Miller-type world, if short sellers simply supply liquidity by adding limit orders to the offer side of the book, their exclusion during the ban will reduce offer-side liquidity and so raise the cost of buying stocks, leaving the cost of selling stocks unaltered. However, the models of Diamond and Verrecchia (1987) and Bai, Chang and Wang (2006) suggest that a ban on short selling would reduce the speed of price discovery, making market prices less informative. This increases the risk to an uninformed participant which might be expected to result in decreased liquidity provision on both sides of the book.~~

~~The FSA's comments quoted above suggest, however, that short sellers were not acting as simple suppliers of liquidity in the run-up to the ban, and we might instead characterise them as actively consuming bid side liquidity through aggressive orders to sell. In this case, their exclusion will, other things kept equal, leave the supply of liquidity on the bid side higher than it would have otherwise been, and so keep the cost of selling low.[7]~~

~~There is no theoretical literature on the issue of limit order book liquidity provision under short sales constraints, to our knowledge, and only limited empirical evidence. Reed (2007) reports an~~

[6] Other papers examining the U.S. ban include Autore, Billingsley and Kovacs (2010) and Bailey and Zheng (2010). Gagnon and Witmer (2010) study the effect of the ban on Canadian cross-listed stocks, while Helmes, Henker and Henker (2009) consider the ban on Australian stocks. Bris (2008) examines the earlier U.S. ban on naked short selling.

~~[7] Again, the other side of the book might be expected to react to such developments, also reducing the cost of buying.~~

~~asymmetric price adjustment in response to information about earnings, and Bris *et al*. (2007) show that downward price moves are slower in markets where shorting is prohibited. Both papers are consistent with the Diamond and Verrecchia (1987) rational expectations model in which prices react more slowly to negative information, and suggestive of relatively higher liquidity on the bid side of the book. However, these results come from relatively normal times rather than crisis episodes and so below we let our data speak.~~

Based on this summary of the literature and our interpretation of the statements made by senior executives of the FSA, we address the following hypotheses in the remainder of the paper.

Hypothesis 1:

Market quality deteriorated for financial stocks to a greater extent than it did for non-financial stocks immediately prior to the start of the ban on short selling.

This hypothesis is based on the FSA's characterisation of markets as being "disorderly". ~~This~~We look for evidence of ~~-~~deterioration in market quality ~~would be evidenced by~~through (i) an increase in trading costs for seller-initiated orders, (ii) increasingly negative net order flow, both due to the liquidity-consuming actions of predatory short-sellers, (iii) a deterioration in the signal to noise ratio in the variance of returns and (iv) a decrease in the contribution of trades to the determination of efficient prices, due to the non-information-based trades of manipulative short-sellers.

However, in the ban period itself, one might formulate two sets of competing hypotheses, which essentially pit the view an interpretation of the likely regulatory view against a view based on academic analysis of short-selling and its effect on markets.

Hypothesis 2:

Based on our interpretation of the intentions of the FSA (and consistent, at least in part, with the Miller-type view of the world), a ban on short-sales should lead to~~we observe~~ a reversal of the effects set out under Hypothesis 1.

Hypothesis 3:

Based on the Diamond and Verrecchia/consensus academic view of short-sellers as ~~passive suppliers of liquidity~~agents who contribute both information and liquidity to markets, the short-sales ban should worsen market quality ~~worsened~~ significantly for financials relative to non-financials during the ban.

Under this view of the world, ~~Thus~~ we would expect to ~~would~~ observe decreased liquidity provision on both sides of the order book, an increase in the signal to noise ratio in returns variance (i.e. lower efficiency) and a lower contribution of trades to efficient pricing.

Under both hypotheses 2 and 3 we would likely expect trading activity to decrease.

3. Data

Our analysis is based on data sourced from the London Stock Exchange (LSE) that allows us to recreate the full limit order book entry by entry from start June 2008 through end February 2009 for stocks traded ~~on the main market of the LSE. Specifically we analyse all stocks traded~~ on either the SETS or SETSmm systems ~~(the 'main market')~~. 23 of the 32 stocks subject to the FSA ban on short selling were traded on ~~the LSE's main market~~ these systems and are analysed below. We ~~have~~ drop~~ped~~ two financial stocks ~~also traded on the main market~~ because of incomplete data during our sample due to mergers. The remaining seven stocks were traded on the Alternative Investment Market and are not part of our sample.

We reconstruct the limit order books in continuous time for the 313 LSE stocks that were continuously traded throughout our ~~sample~~ data period without major corporate actions. We take snapshots of these order books at one-minute intervals ~~and~~, record the key features and from these build daily time-weighted averages. ~~These features of the trading book are then aggregated into daily time-weighted averages.~~ ~~We also calculate other features such as daily trading volume which~~

~~incorporates transactions both on and off the order book.~~ We are interested in the following indicators:

Liquidity and Costs of Trading

Our key measures of liquidity are bid-ask *spreads* and *slippage* measures derived from the shape of the limit order book. We define buy (sell) slippage to be the difference between the current midquote and the volume weighted average price of a market buy (sell) of given size. Thus slippage is inversely related to order book depth. Slippage measures are defined for various notional market order sizes.

Transactions

~~We measure all transactions-related variables in terms of the numbers of shares traded rather than by value to avoid the effect of price level changes during the sample.~~ Volume is computed as the sum of shares bought and sold in a day.[8] As the LSE data provides information on whether trades are buyer or seller initiated we can also measure buy and sell volume separately. ~~From here, w~~We compute order flow as buyer initiated volume less seller initiated volume, scaled by total volume. For the three volume measures, we scale the daily measures by the mean value in the first 25 days of the sample. Finally, we also compute~~, for each day in the sample,~~ the proportion of each day's LSE volume traded on the order book, rather than traded off order book in the bilateral segment of the market.[9]

Returns

Daily returns for banned and matched stocks, ~~are also computed and are~~ measured in basis points.

Matching

In the regressions below we match each stock subject to the ban with ten stocks that do not fall under the ban (the control group stocks) according to market capitalisation. Specifically, we compute the average market capitalisation of each stock on the LSE over the first half of 2008. For each stock

[8] We measure all transactions-related variables in terms of the numbers of shares traded rather than by value to avoid the effect of price level changes during the sample.

[9] We do not have data for trades on other venues such as Chi-X and BATS or on trades reported to Boat.

subject to the ban we find ten stocks with the most similar average market caps.[10] The control group for each stock is then unaltered for the rest of the sample. The equally weighted average value of each indicator across these ten stocks is then used as a benchmark against which we compare the indicator for the banned stock. Table 1 details the 23 companies subject to the ban together with market cap information for them and their control group counterparts. The variation in market capitalisation across these stocks is apparent. While the average market capitalisation of the matching stocks is relatively close for the smaller stocks, the largest two companies are more difficult to match with a simple average control group.[11]

In the analysis below we report results based on the full set of 23 companies. In results available in the Internet Appendix we report separate results for the six largest stocks – the international commercial banks in the sample – and for the remaining 17 smaller stocks that are either investment banks, insurance companies or asset managers.[12] There are no great differences between behaviour of the 'Big 6' or the 'Small 17', but splitting them sometimes helps statistical inference by reducing cross-sectional heterogeneity.

~~< Table 1 >~~

~~< Descriptive stats of banned and matched samples >~~

4. Empirical Results

4.1 Difference-in-Difference Regressions

[10] Note that stocks can be in the control group for more than one firm subject to the ban.

[11] We have experimented with improving the match by using a smaller number of stocks in the control group or weighted average matching, and our results are robust to these alternatives. For our regression analysis below, we identify and exclude any dramatic outliers in the entire set of matched non-banned stocks. On each sample day, we eliminate data for any matched stock which is more than 10 standard deviations away from that day's cross-sectional mean. This ensures that no extreme data in the sample of matched stocks affect our results.

[12] The six large stocks that we analyse separately are HSBC, RBS, Lloyds, Barclays, HBOS and Standard Chartered.

We use a difference-in-difference regression to model the behaviour of the various indicators through our sample. This empirical approach is designed to measure the effect of a ‘treatment’ on a set of subjects through comparison of the behaviour of the treated group and a control sample pre and post treatment. It has been used extensively in studies of regulatory change in economics, law and finance.[13] Denote by $y_{i,t}$ our variable of interest. For each of the N financial stocks subject to the ban we have T observations on $y_{i,t}$ and T observations for the matched sample. The dependent variable then is a matrix with T rows and $2N$ columns.

The difference in difference regression is ~~nothing other than~~ a panel regression augmented with sets of dummy variables~~.~~. The right-hand side specification contains a constant (α) and a dummy variable to select observations for the financial stocks subject to the ban (D^F). We add dummies to pick out observations during the ten trading days immediately prior to the introduction of the ban (D^{Pre}), a dummy to pick out observations during the ban (D^{Ban}) and a dummy to pick out observations after the ban was removed (D^{Post}). The difference in difference terms which isolate the difference in behaviour of the control stocks and the banned stocks are ~~nothing other than~~ interactions of the three time dummies with the dummy that selects banned stocks (i.e. interactions of D^F with D^{Pre}, D^{Ban} and D^{Post} respectively). If the coefficients on these interactions are significantly different from zero it reveals a difference in the behaviour of banned and control group stocks for a particular period.

Finally, we add a volatility control variable to the right hand side, constructed as the equally weighted average daily volatility of all the stocks in our sample *except* the financial stocks (V_t). This controls for market wide changes in the information environment and Beber and Pagano (2009) use a similar control in their study of the effects of short-sales bans on spreads.[14] One could use previous work to add further control variables to the specification. For example, the typical time-series models for liquidity measures also control for traded volume and market cap. However, in the current context, the

[13] See Ashenfelter and Card (1985), Angrist and Pischke (2009) and Bertrand, Duflo and Mullainathan (2004) for examples and explanations of the difference-in-difference approach.
[14] We omit the volatility control variable when running our specification with returns as the dependent variable.

first of these is clearly endogenous and market cap differences between banned and control stocks have been controlled for via our matching process.~~.~~

Commented [s1]: This still feels a little weak.

The full regression specification is given below. Robust, double-clustered standard errors are reported which, as Bertrand, Duflo and Mullainathan (2004) point out, tend to be rather conservative.

$$\square_{\square,\square} = \square + \square_1 \square^{\square}_{\square,\square} + \square_2 \square^{\square\square\square}_{\square,\square} + \square_3 \square^{\square}_{\square,\square} \times \square^{\square\square\square}_{\square,\square} + \square_4 \square^{\square\square\square}_{\square,\square} + \square_5 \square^{\square}_{\square,\square} \times \square^{\square\square\square}_{\square,\square}$$
$$+ \square_6 \square^{\square\square\square\square}_{\square,\square} + \square_7 \square^{\square}_{\square,\square} \times \square^{\square\square\square\square}_{\square,\square} + \square_8 \square_{\square} + \square_{\square,\square} \qquad \textbf{(1)}$$

4.1.1 Headline Figures

Since the novelty in our work is in the detail of the microstructure data that we have, the main focus of our work is on characteristics derived from consideration of the full trading environment. However, we begin with a brief discussion of variables previously addressed in the literature.

~~< Table 2 >~~

~~< returns, spreads, volatility and volume >~~

Table 2 reports the results of our difference-in-difference analysis using returns, spreads, volatility and volume as dependent variables for the full set of 23 affected stocks plus matched control stocks.

While we do not expect our difference-in-difference approach to explain the behaviour of equity returns over this extraordinary period we can make some observations. Returns were (statistically insignificantly) negative over the benchmark period (2nd June 2008 – 4th September 2008), and the immediate pre-ban period saw a significant deterioration in prices for all stocks. Furthermore, returns were significantly worse for financials than control group stocks.[15] Clearly, these especially rapid falls in the stock prices of large financial institutions ~~were~~ would have likely been key factors behind the FSA's move to restrict short selling. There is no evidence of return differentials between financials and control group stocks after the ban came into effect or once the ban was lifted. ~~This is in~~

[15] This result is mainly driven by the Big-6 financial stocks rather than the smaller banned companies.

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~~contrast to the evidence for the United States in Harris *et al*. (2009) but lends credence to the suggestion in Boehmer *et al*. (2009) that the jump in stock prices of U.S. financials was driven by TARP-related announcements rather than the simultaneous ban on short sales.~~[16] ~~Taken as a whole, o~~Our ~~evidence suggests that U.K. financials significantly under-performed control group stocks pre-ban, but that this performance differential disappeared during and after the ban.~~ Thus, overall, ~~If~~ if the goal of the FSA was to arrest sharp declines in financials' stock prices relative to those of non-financials, our results may be interpreted as reflecting success for their policy of banning short sales. However, as the Introduction makes clear, the FSA's stated goal was not to prop up the prices of financial stocks but to calm 'disorderly markets'.

The literature has also established that volatility, volume and spreads all changed dramatically around the ban period, and our findings confirm these results. In the baseline period, spreads on the control stocks averaged 19.34bp, ~~while~~ with spreads on financials ~~were~~ insignificantly ~~narrower~~smaller. In the immediate pre-ban period spreads in *all* stocks widened by around 3bp. During the ban period, spreads rose ~~by 15bp from baseline levels for the control group~~ to 35bp for the control group, while ~~those~~ spreads for financial stocks jumped to 52bp. Spreads fell slightly for the control group once the ban was lifted, but the additional 17bp spread charged on banned stocks disappeared.

Trading volume shows perhaps the most dramatic behaviour. Volume during the benchmark period for all stocks was slightly down on the level seen during the first 25 days of the sample (used to normalise volume measures). It increased significantly in the immediate pre-ban period by 36% for control stocks, and while the point estimate for financial stocks is even higher this difference is not significant. Volume in the ban period for control stocks was still 17% higher than benchmark levels despite the wider spreads. Given the extremely high levels of stock return volatility at this time, for this group of stocks the usual positive relationship between volume and volatility is observed. However, trading volume in financial stocks during the ban was substantially lower, down by 23% from the benchmark period~~'s level~~, and 40% lower than volume in control stocks in the same period.

[16] This is in contrast to the evidence for the United States in Harris *et al*. (2009) but lends credence to the suggestion in Boehmer *et al*. (2009) that the jump in stock prices of U.S. financials was driven by TARP-related announcements rather than the simultaneous ban on short sales. Beber and Pagano (2009) also report an absence of stock price jumps for the U.K. as part of their global study on short sales restrictions.

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This is despite the fact, reported in the Internet Appendix, that volatility for financials was much higher than usual. Once the ban was lifted, control group volumes returned to benchmark levels (as volatility fell), while volumes in financials recovered. This negative correlation between volatility and volume is, again, unusual.

These findings, consistent with other studies, tell a simple story. The high levels of volatility and trading volume immediately before and during the ban for the control stocks suggest that this was a period of relatively high information revelation. Spreads widened throughout the period as liquidity suppliers acted less aggressively. Financial stocks were still at the heart of the crisis and so it is unreasonable to suggest that information revelation was low for them. They too saw an increase in volatility and trading costs. Trading volume for financials fell by 40% relative to the control group during the period the ban on short selling was effective, more for larger financial stocks. The short sales ban therefore raised the cost of trading at a time when more people wanted to trade and when the cost of trading was already relatively high.

4.1.2 Transactions and Transparency

The patterns in volumes outlined above suggest that the ban on short selling had a huge impact on the trading in financial stocks. ~~Trade volumes deteriorated dramatically, while volume in the control stocks remained above benchmark levels.~~ While the exact rationale behind the introduction of the ban has not been made public, simply reducing trading volume was probably not the aim~~ of the ban on short selling~~. Rather, it seems more reasonable to assume that the FSA wanted to reduce selling pressure on financial stocks. In other words, one goal of the ban might have been to increase order flow in financials by raising the amount of market buy orders relative to market sells.

We examine this by looking at order flow on the LSE book. As Table 2 shows, flow was negative for all stocks in the baseline period, consistent with a falling stock market, and flow for financial stocks was significantly worse. In the pre-ban period, flow deteriorated significantly for all stocks. Importantly, however, flow for financial stocks behaved no differently to flow for the control group during this pre-ban period, making it hard to justify intervention by the authorities designed specifically to affect financials only, a point we return to in Section 4.2 below.

~~<Figure 2>~~

~~<Flow plot>~~

~~Figure 2~~Figure 1 illustrates the behaviour of cumulated flows averaged across financials and control group stocks. Both show clear negative trends and while financial stocks suffer more, most of this is due to very high selling pressure well before the ban. Both series move very much in line during the pre-ban period [-20, 0], and the trends extend through the ban period. Clearly, the selling pressure on financial stocks during the ban could only be from aggressive long- sales.[17] The ban on short selling did not reverse the direction of trade flow, which on average remained out of financial stocks immediately before and during the ban.[18]

While the majority of transactions in our sample stocks take place on the LSE order book, dealers could and still can transact with each other and directly with customers off book. Communicating directly with a dealer allows a trader to negotiate over price, particularly if the dealer can be convinced the trader is uninformed, and to collect market intelligence. Prior research suggests that, faced with increased uncertainty and poor order book liquidity, impatient traders might be induced to seek liquidity off-book (Friederich and Payne, 2007).

Table 2 shows that in the benchmark period, on average, 76% of transactions reported to the LSE took place on the order book for both financials and control group stocks. This rose by around 2% in the two weeks prior to the ban for all companies, large or small, financial sector or control group. During the ban, however, while control group trading was unaltered, order book trading in financial stocks was 7-8% below benchmark levels. The response of traders to the sizeable withdrawal of liquidity on the order book for financials during the short sales ban was to transact instead with market makers. Thus, during the ban, almost 10% of trading activity in financial stocks migrated to a less transparent, in a pre-trade sense, segment of the market. The likely effect of such a migration is a smaller

[17] These sales could also have been due to market-maker activities but over a relatively long period one would expect marker maker flows to be balanced.

[18] Arguably, the figure suggests that selling pressures on financials grew stronger immediately after the ban was announced before slowing some 20 days after the ban was introduced. Nevertheless, the big picture remains that the ban did little to alter selling pressure on financial stocks over the period.

contribution to price discovery from the order book and reduced efficiency in order book prices. ~~The next s~~Section 4.2 provides some direct evidence to support this assertion.

~~The results in this section confirm the perhaps surprisingly symmetric effect that the ban had on trading in financial stocks. The ban explicitly removed one class of trade – short sales – and the inference from Sir Callum's comments quoted above is that the specific target was short sellers that were aggressively consuming liquidity on the bid side of the book. As such we might have expected to see a reduction in the number of aggressive sales relative to the number of aggressive buys. We do not. While the number of sell orders dramatically fell, the number of buy orders fell similarly, leaving the balance of order flow unaffected. We noted in section 4.1.2 that the drop in liquidity was also approximately equal on the bid and offer sides of the order book. Together, these findings suggest that the short sellers excluded by the ban were not aggressive consumers of liquidity from the bid side but were instead suppliers of liquidity, at least to the offer side of the book. As offer side liquidity was removed by the ban, bid side liquidity either fell directly, perhaps as market-neutral funds were forced to withdraw from both sides of the book, or indirectly as liquidity suppliers on the bid side reacted to the liquidity drain on the opposite side of the book.~~

4.1.3 Costs of Trading and Depth

Bid-ask spreads are an easy to measure indicator of liquidity, however, they are only completely relevant for extremely small deals. Further, existing studies of short-sales bans that measure the bid-ask spread using data from the end of the trading day risk contamination by time of day effects on spreads. Analysis of the full order book sampled at a high frequency can give a richer view of liquidity by revealing depths available for trade at all price levels throughout the day, rather than focusing on just the prices at the top of the book at one point in time.

~~In the context of restrictions on short selling, considering liquidity on both sides of the order book separately also gives insight. At one extreme, in a Miller type world, if short sellers simply supply liquidity by adding limit orders to the offer side of the book, their exclusion during the ban will reduce offer side liquidity and so raise the cost of buying stocks, leaving the cost of selling stocks unaltered.~~

~~However, the models of Diamond and Verrecchia (1987) and Bai, Chang and Wang (2006) suggest that a ban on short selling would reduce the speed of price discovery, making market prices less informative. This increases the risk to an uninformed participant which might be expected to result in decreased liquidity provision on both sides of the book.~~

~~The FSA's comments quoted above suggest, however, that short sellers were not acting as simple suppliers of liquidity in the run-up to the ban, and we might instead characterise them as actively consuming bid side liquidity through aggressive orders to sell. In this case, their exclusion will, other things kept equal, leave the supply of liquidity on the bid side higher than it would have otherwise been, and so keep the cost of selling low.[19]~~

~~There is no theoretical literature on the issue of limit order book liquidity provision under short sales constraints, to our knowledge, and only limited empirical evidence. Reed (2007) reports an asymmetric price adjustment in response to information about earnings, and Bris *et al.* (2007) show that downward price moves are slower in markets where shorting is prohibited. Both papers are consistent with the Diamond and Verrecchia (1987) rational expectations model in which prices react more slowly to negative information, and suggestive of relatively higher liquidity on the bid side of the book. However, these results come from relatively normal times rather than crisis episodes and so we let our data speak.~~

~~*< Figure 1 >*~~

~~*< order book plot >*~~

The final panel of Table 2 gives our difference-in-difference results for our order book slippage measures. It is immediately clear that the ban greatly degraded depth for financial stocks relative to stocks in other industries and that this effect disappeared once the ban was lifted. This result is perhaps easier to see in ~~Figure 1~~Figure 2 which plots the average shape of the order book for financial and control group stocks during the benchmark and ban periods. The figure shows the cost of the marginal share in a trade of given size (and is thus different from our slippage measures which are the

~~[19] Again, the other side of the book might be expected to react to such developments, also reducing the cost of buying.~~

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Formatted: Font: (Default) Times New Roman volume weighted average price of a share in a trade.)~~.~~ The book is slightly deeper (cheaper) for control group stocks than for financials during the benchmark period, but deteriorates during the ban, suggesting that general liquidity conditions were poor at this time. However, the change in the book for financial stocks is much more dramatic. The marginal share in a 0.5% of ADV market buy order, for example, cost over 1,000bp more than the mid price during the ban, up from around 100bp in the benchmark period. Depth was massively reduced at all prices and on both sides of the book. The deterioration is approximately symmetric at smaller depths (<0.003ADV), but the offer side of the book clearly suffers more at greater depths. Neither the incredible fall in liquidity on both sides of the book, nor the somewhat higher costs of executing large buy orders for financials could have been in line with the wishes of the regulators when they introduced the short sales ban.

The results in this section and the preceding one confirm the perhaps surprisingly symmetric effect that the ban had on trading in financial stocks. The ban explicitly removed one class of trade – short sales – and the inference from Sir Callum's comments quoted above is that the specific target was short sellers that were aggressively consuming liquidity on the bid side of the book. As such we might have expected to see a reduction in the number of seller-initiated deals relative to the number of buyer-initiated ones, and an increase in liquidity available on the bid side. Instead, we observe an equal fall in buyer- and seller-initiated deals, leaving net flow unchanged, and more-or-less equal drops in liquidity on both sides of the book.

In terms of the alternative scenarios sketched out above, our results suggest that the short sellers were, on balance at least, passive suppliers of offer-side liquidity rather than aggressive consumers of bid-side liquidity. Their exclusion through the ban hugely raised the cost of executing buy orders. The rational expectations class of models suggest that uninformed participants then ought to have perceived an increase in risk as market efficiency fell, leading to a reduction in bid-side liquidity at the same time. This is exactly what we observe in the data. The key step in this logic – that market efficiency was harmed by the ban – is considered in section 4.2 below.

An alternative and perhaps simpler explanation for the simultaneous and equal drop in bid and offer-side liquidity supply is that the primary impact of the ban on short selling was to force market-neutral equity hedge funds out of financial stocks. These funds take long and short positions in different but (statistically) related stocks, seeking to profit from short-term adjustments in relative prices. Since their positions are often industry neutral, either by design or because the correlated stocks they are trading are likely to be in the same sector, once unable to take short positions in financials these funds were also much less likely to take long positions. Their withdrawal from the financial sector therefore reduced liquidity on both sides of the book approximately equally.[20]

4.2 Hasbrouck VAR Decompositions

In this section we run standard Hasbrouck VARs on the sample of financial stocks and on the control group stocks and employ them to assess several dimensions of market quality~~, and the role played by trading activity in the determination of market quality around the short sales ban~~. Hendershott and Moulton (2009) use an identical technology to evaluate the effects of changes in the NYSE's trading rules on market efficiency and price discovery.

In Hasbrouck's (1991) framework price changes may be driven by private information, which enters the market through unexpected trading activity, or public information. This is modelled econometrically with a bivariate VAR containing two variables: mid-quote returns between trades at time t and t-1 (denoted by r_t and measured in basis points) and the signed trade at time t (denoted by q_t and taking the value +1 if the trade at time t was a market buy and -1 for a market sell). The VAR picks up order flow dependence out to p lags:

$$\square_{\square} = \sum_{\square=1}^{\square} \square_{\square}\square_{\square-\square} + \sum_{\square=0}^{\square} \square_{\square}\square_{\square-\square} + \square_{\square\square} \qquad \textbf{(3)}$$

$$\square_{\square} = \sum_{\square=1}^{\square} \square_{\square}\square_{\square-\square} + \sum_{\square=1}^{\square} \square_{\square}\square_{\square-\square} + \square_{\square\square} \qquad \textbf{(4)}$$

The standard VAR can be inverted to get the VMA representation:

$$\begin{bmatrix} \square_{\square} \\ \square_{\square} \end{bmatrix} = \begin{bmatrix} \square(\square) & \square(\square) \\ \square(\square) & \square(\square) \end{bmatrix} \begin{bmatrix} \square_{\square\square} \\ \square_{\square\square} \end{bmatrix}$$

[20] This is certainly the hypothesis expounded by some commentators in the financial press. See for example the article in the Wall Street Journal entitled "Hedge funds wrestle with short sales ban" on September 25th 2008.

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(5)

where $a(L)$, $b(L)$, $c(L)$, and $d(L)$ are lag polynomial operators, and

$$\square\square\square\begin{bmatrix}\square_{\square\square}\\ \square_{\square\square}\end{bmatrix} = \begin{pmatrix}\square^{2} & 0\\ 0 & \Omega\end{pmatrix}$$

(6)

We can also define

$$\square_{\square}^{2} = \left(\sum_{\square=0}^{\infty}\square_{\square}\right)\Omega\left(\sum_{\square=0}^{\infty}\square_{\square}'\right) + \left(1+\sum_{\square=0}^{\infty}\square_{\square}\right)^{2}\square^{2}$$

(7)

and

$$\square_{\square}^{2} = \left(\sum_{\square=0}^{\infty}\square_{\square}\right)\Omega\left(\sum_{\square=0}^{\infty}\square_{\square}'\right)$$

(8)

where (7) is the variance of the permanent component of returns and (8) is the variance of the trade-related component of returns. A ten-lag VAR is estimated for each stock for the four separate intervals and from these VARs three measures are computed

First, the price impact of a trade (PI) is measured by the sum of the $b(L)$ coefficients in the VMA representation. This is equivalent to the impulse response of prices to a trade innovation implied by the VAR and is another liquidity measure. Second, we compute the size of the permanent component of prices (PC)~~, measured by~~ as the ratio of the variance of the permanent component of returns defined in (7) to total return variance~~ is computed~~. This ~~gives us another market quality measure, namely~~measures how important information is versus noise in driving returns and is thus a measure of informational efficiency~~. The closer this number is to unity, the larger the information content. We interpret this as a measure of market efficiency as larger numbers suggest that returns are driven more by information production rather than noise. .~~ Last we calculate the size of the trade correlated component (TCC), measured as the ratio of the variance of the trade-correlated component [defined in equation (8)] to the variance of the permanent component [defined in equation (7)]. This tells us ~~how~~

Formatted: Don't adjust space between Latin and Asian text, Don't adjust space between Asian text and numbers what proportion~~much~~ of information was getting into ~~permanent~~ price changes through trading and thus measures trading related price discovery.

~~A number closer to unity suggests that trading conveyed more information, whereas a low number would suggest that prices adjusted without the need for trading.~~

~~*< Table 6 >*~~

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~~*< Hasbrouck results for all, buys and sells >*~~

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~~Table 6~~Table 3 reports the simple average of each measure across the financial stocks and the control group stocks in each period. Two main points stand out from the table. First, there is no economically meaningful difference between financial stocks and control group stocks for any of the three measures during the benchmark period. Further, while the price impact of trades rose and ~~market quality~~efficiency and price discovery indicators deteriorated between benchmark and immediate pre-ban periods, financial and control group stocks were affected equally. Though the difference is small, the deterioration in market efficiency (measured by PC) was, if anything, larger for the benchmark stocks than for financials. ~~This again questions the decision to single out financial sector stocks for special regulatory intervention.~~

Second, and conversely, financial and control group stocks behaved ~~noticeably~~ very differently for all three measures while the ban was in effect. The price impact of trades rose by 64% compared to the benchmark period for the control group stocks, but by more than one-hundred percent for financial stocks. This again reflects the much larger drain in liquidity for financials caused by the ban on short selling. The permanent component of prices fell by 30% for control stocks but by almost 46% for financials. ~~Put differently, the signal to noise ratio in the returns process dropped dramatically for financial stocks.~~ These figures suggest that there was a general fall in efficiency during the ban but that the fall was significantly larger for those stocks affected by the ban. Finally, the trade correlated component of trades fell just 17% for control group stocks yet almost halved during the period of the ban for financials. Trades conveyed much less information during the period of the ban for financials. This fall is perhaps partially due to the high level of public information revelation at this time, which may have been priced without the need for trading. However, once the ban was lifted, the TCC for financials returned to the same level seen prior to the ban (and to the level that prevailed for control group stocks throughout the sample). Together with the huge drop in PC for financials, this suggests that the ban on short selling made order book prices less informative and impeded the role of ~~the~~ trading ~~process~~ in the discovery of efficient prices. These findings echo those of Fotak *et al*. (2009) who conclude that SEC ban on naked short selling of financial securities during July and August 2008 had a negative impact on pricing efficiency for U.S. stocks.[21]

~~In summary, this set of results suggests that while market quality declined somewhat in the run-up to the ban financial and control group stocks were moving very much in line with each other. We noted above that selling pressure on stocks, while significantly stronger in the period just before the ban than in the benchmark period, was approximately equal for both financials and control group stocks. Together, these results lead us to question the decision to 'support' financial stocks with a ban on shorting. Having done so, it is apparent that the ban only served to significantly worsen market quality for financial stocks. Trades conveyed much less information to the market during the ban and the drop in liquidity doubled the price impact of trades. This could not have been in line with the goals of the FSA.~~

4.3 Persistence of the effects of the ban

Studies of the short selling ban in the U.S. have important shortcomings. First, several other events occurred at much the same time as the ban was introduced. On the day the U.S. short sales ban took effect, the U.S. Treasury announced the creation of what would become the Troubled Asset Relief Program.[22] Further, the U.S. ban on short selling was lifted just three business days after the enactment of TARP. The correlation in the timing of these events greatly complicates the interpretation of the US evidence on the short-sales ban. ~~Both the short sales ban and TARP could have been interpreted by the markets as signals of the U.S. governments' pessimistic views of the state of the (global) financial sector. If this is the case, the effects of these signals are difficult to~~

[21] Reed (2009) similarly concludes that stocks where short selling is costly (through standard demand and supply forces) are much less informationally efficient.

[22] The U.S. Treasury also announced a guarantee program for money market funds, and the Fed announced a program to lend against high-quality asset-backed commercial paper.

~~separate from the effect of restricted short selling, especially in the U.S. when the short sales ban was relatively short-lived and so well-aligned with developments in TARP.~~

Fortunately, the longer period of the U.K. ban helps in this regard. If the negative changes in liquidity at the beginning of the ban are due to the abovementioned government signals (rather than the ban itself), they ~~should~~ would likely recover through the ban period. Conversely, if the deterioration in liquidity was due to the short sales ban, the deterioration should be apparent for the full period of the ban. Further, since the end of the U.K. ban was free from confounding events, evidence of improvement in liquidity and trading volumes at this time strengthen the argument that the deteriorations can be ascribed to restrictions on short selling rather than other events.

In this sub-section we introduce sub-period trend variables into our basic regression model to capture possible gradual adjustment processes.

$$\begin{aligned}\square_{\square,\square} = \square + \square_1 \square^{\square}_{\square,\square} + \square_0 \square\square\square\square\square^{\square\square\square} + \square_1 \square^{\square}_{\square,\square} \times \square\square\square\square\square^{\square\square\square} + \\ \square_2 \square^{\square\square\square}_{\square,\square} + \square_3 \square^{\square}_{\square,\square} \times \square^{\square\square\square}_{\square,\square} + \square_2 \square^{\square\square\square}_{\square,\square} \times \square\square\square\square\square^{\square\square\square} + \square_3 \square^{\square}_{\square,\square} \times \square^{\square\square\square}_{\square,\square} \times \square\square\square\square\square^{\square\square\square} + \\ \square_4 \square^{\square\square\square}_{\square,\square} + \square_5 \square^{\square}_{\square,\square} \times \square^{\square\square\square}_{\square,\square} + \square_4 \square^{\square\square\square}_{\square,\square} \times \square\square\square\square\square^{\square\square\square} + \square_5 \square^{\square}_{\square,\square} \times \square^{\square\square\square}_{\square,\square} \times \square\square\square\square\square^{\square\square\square} + \\ \square_6 \square^{\square\square\square\square}_{\square,\square} + \square_7 \square^{\square}_{\square,\square} \times \square^{\square\square\square\square}_{\square,\square} + \square_6 \square^{\square\square\square\square}_{\square,\square} \times \square\square\square\square\square^{\square\square\square\square} + \square_7 \square^{\square}_{\square,\square} \times \square^{\square\square\square\square}_{\square,\square} \times \square\square\square\square\square^{\square\square\square\square} \\ + \square_8 \square_{\square} + \square_{\square,\square}\end{aligned} \quad (2)$$

This specification contains four trends, one each for the benchmark, pre, ban, and post periods and each trend variable is allowed to affect the banned and control group stocks differently. ~~Each trend is allowed to affect all firms equally during the relevant period and when interacted with the financial stock dummy captures trends specific toto affect just financial firms.~~ Significantly different trends for the financial firms subject to the ban would ~~then~~ be revealed by significant β_1, β_3, β_5, or β_7 coefficients. Results are reported in ~~Table 5~~Table 4.

~~< Table 5 >~~

~~< regressions with trends >~~

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~~The~~Our most important finding here is that there is no evidence of trends during the ban period for the cost of trading indicators (spreads, BSlip2 and OSlip2).[23] The significant rise in trading costs for benchmark firms and the much larger rise for financial firms appear to be constant ~~rather than changing during~~across the long U.K. ban period. Slippage costs were trending upwards immediately prior to the ban for all firms (and not significantly differently for financials). Once the ban was removed, while slippage costs remained on average higher for control group stocks and particularly high for financials, they were trending downwards for control group stocks and were trending downwards significantly faster for financials.

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The results for transactions indicators suggest there were significant trends during the ban for all firms. Volumes trended downwards significantly through the ban, but ~~those in financial firms trended downwards at the same rate as~~at the same rate for banned and control group stocks. The markedly different levels effects for these variables discussed above remain significant, however, even when these trends are taken into account. Finally, we note that order flow shows no evidence of either levels or trend effects at any time..

The results of the dynamic analysis are quite clear. Transactions costs rose significantly more for financial stocks during the ban period, and these higher trading costs remained relatively stable throughout the ban. Once the ban was lifted, transactions costs fell and continued to fall as the market adjusted to ~~the~~ new ~~regime~~trading conditions. Volumes dropped dramatically for financials during the ban relative to control group stocks. The evidence strongly suggests that the different behaviour of liquidity and trading indicators for financials was due to the ban on short selling rather than confounding effects.

4.4 Robustness Checks

We recognise that we have made some arbitrary choices in our empirical work, and that the robustness of our results might be a concern. We seek to allay those fears in this section, describing

[23] We report slippage costs only for 1/10th of ADV for parsimony. Other trade sizes give qualitatively similar results.

some variations to the analysis that we have performed and their effect on our results. All are more fully described in the Internet Appendix to the paper.

First, we do not know the pre-ban window during which the FSA observed what it described as disorderly trading conditions. In the results above we have used a two-week window immediately prior to the introduction of the ban. We have varied this two-week window in two ways. First we have looked at a one month pre-event window. Second, we have excluded the week immediately prior to the introduction of the ban and used the two weeks preceding that week (trading days [-15, -5]). Neither of these changes makes any material difference to our results.

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Second, we recognise that the first day for which the ban was effective (September 19th) saw large-scale adjustments of portfolios to reflect the new rules and as such may be deemed an outlier observation. We therefore exclude this day from our analysis to ensure that it does not bias our findings for the ban period. Again, our results do not change in any important fashion.

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We also study whether any of these changes to the definitions of the pre-ban period or whether we include the day of the ban has any effect on the Hasbrouck-VAR analysis. Again, the answer is ‘no’.

A final possible criticism that might be levelled at our VAR analysis is that it treats market buy and market sell orders symmetrically. It could be argued that the effects of sell orders in the run-up to the ban were particularly severe. To test this, we have run an extended version of the VAR system with returns, trades and a set of interactions that separate market buys and sells on the right hand side. The interactions suggest that the differences between buy and sell orders are, in general, economically minuscule and statistically insignificant. In particular, the sum of the coefficients on sell trades in the returns equation is smaller than the sum of the buy coefficients, suggesting that, if anything, sells had a smaller impact on prices than buys.[24] Given the tiny differences between coefficients on buys and sells, the simple specification reported in the paper seems entirely appropriate.

5. Conclusions

In this paper we have ~~compared~~ examined ~~several~~ the microstructur~~al~~e ~~indicators of conditions prevailing in~~of U.K. stock markets between June 2008 and February 2009. This period spans the introduction and subsequent removal of new provisions to the Code of Market Conduct issued by the Financial Services Authority that banned the creation or increase of net short positions in publicly quoted U.K. financial companies. We have attempted to answer two key questions in this paper.

First, was there any clear difference between the ~~microstructural~~ behaviour of financial stocks and set of control group stocks that might have motivated the FSA's move to ban short-selling? Since figures in the FSA spoke of "incoherence" in stock markets and stated that "disorderly" conditions prevailed in the period prior to the ban's introduction we might have expected to find evidence of abnormal conditions in the market for financial company stocks in the period before 18th September 2008.

Second, what were the effects of the ban on short selling on market conditions in general, and on liquidity, efficiency, trading activity and price discovery in particular? As the ban on short selling was motivated by the existence of abnormal market conditions, we investigate whether there was an improvement in market conditions once the ban was in force.

In short, we find no strong evidence that conditions in the market for financial stocks were any different to conditions for control group stocks ~~in other sectors~~ in the period prior to the ban. Market quality indicators were deteriorating in late August-early September, but they were deteriorating for all stocks and not just for financial companies. Trading costs were rising and ~~despite this~~ trading volumes were ~~also~~ increasing for all stocks. Of course, stock prices were falling at this time and order flow was significantly negative as traders aggressively sold stocks, presumably both ~~through~~ due to liquidation of long positions and ~~through~~ short sales. But again, conditions were similar for financial

[24] These results are not reported but are available on request.

and non-financial stocks making it hard to justify the intervention by the regulators designed specifically to affect only financial sector stocks.

The effect of the ban on market conditions is quite clear. Liquidity in the market for financial stocks drained away and trading costs rocketed. Trading volume on the order book fell noticeably at a time when volume in stocks not subject to the ban rose. Critically, we find that the cost of buy orders and sell orders increased approximately equally, and that the numbers of market buy and sell orders fell by similar amounts. In other words, the ban raised the cost of trading and reduced the volumes traded but did not alter the balance of buy and sell orders. ~~Order flow remained out of financial stocks despite the ban.~~ This suggests that long-sellers were the real drivers of negative sentiment towards financial stocks. Moreover, if high selling pressure on financials was the real reason behind the ban, its introduction did nothing to alleviate this pressure.

Other market quality indicators were significantly worse during the ban. The fall in liquidity resulted in higher price impacts following a trade, reduced market efficiency and a smaller price discovery role for trading.~~trades conveyed less information to the market which impeded traders' abilities to discover the true price of financial stocks.~~ The ban served to make the trading process *less* rather than more informative.

We demonstrate that the reduction in liquidity and market quality persisted though the relatively long-lasting ban on short selling in the U.K. Furthermore we observe strong reversals coincident with the lifting of the ban. Together these suggest that the effects we identify were indeed caused by the ban rather than other major market developments such as the introduction of TARP in the U.S. since the latter were concentrated in the early days of the U.K.'s ban.

We can also draw some inferences regarding the behaviour of short sellers from our findings. At the time of the ban, short sellers were often portrayed by commentators as predatory consumers of (bid-side) liquidity who aggressively sold financial stocks (presumably buying them back later on once their prices had dropped sufficiently). Their removal was thus justified in that it would allow stability back to markets as potential buyers of stocks faced reduced risks of being preyed upon. The academic literature, conversely, typically portrays short sellers as more passive (offer-side) liquidity suppliers, willing to sell an asset if its price rises "too far" and so helping to correct over-exuberant markets. Removing this type of market participant would only serve to reduce liquidity and worsen market quality. Our findings suggest strongly that the behaviour of short sellers is best captured by this second portrayal, even in the volatile last few months of 2008.

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Data Appendix

Measure	**Definition**
Spread	Bid-ask spread, measured in basis points.
Slippage	Measures as the cost of consuming liquidity with a market sell/buy order of given size. Slippage is defined as the difference, in basis points, between the mid price and the volume-weighted average price (VWAP) of a trade of given size. Various size levels are examined, ranging from one hundredth of one percent of average daily volume (ADV) to one percent of ADV. Separate slippage measures are computed for the buy and sell side of the order book.
Volume	Daily number of shares traded divided by the mean number of shares traded in the first 25 days of the sample.
Buys/Sells	Daily number of shares aggressively bought or sold divided by the mean number of shares aggressively bought/sold over the first 25 days of the sample. Aggressive orders are defined as orders that immediately consume liquidity.
Order flow	Signed trade imbalance as a proportion of total shares traded. That is, number of shares aggressively bought – number of shares aggressively sold)/(total number of shares traded)]×10000.
Order book share	Proportion of volume traded on the limit order book as a proportion of total volume traded on the London Stock Exchange

Table 1

This table lists the 23 stocks in our analysis subject to the short sales ban. The average market capitalisation of each stock calculated over the first half of 2008 is given in the second column. The third column gives the mean market capitalisation of the ten non-financial stocks most similar in size that form the control group. The final two columns give the highest and lowest average market capitalisation from the ten control stocks.

	Market Capitalisation (£m)			
	Financial Stock	Mean Matched	Min Matched	Max Matched
Rathbone Brothers	426	427	412	440
Brit Insurance	711	711	692	730
Aberdeen Asset Management	901	898	870	934
Provident Financial	1062	1053	1012	1096
Close Brothers	1139	1132	1081	1205
St. James' Place	1204	1212	1144	1262
Investec	1632	1660	1557	1738
Schroders	2260	2229	2082	2448
Alliance Trust	2274	2229	2082	2448
Admiral Group	2478	2506	2204	2748
Friends Provident Group	3221	3099	2748	3590
RSA Insurance Group	4299	4141	3590	4824
Standard Life	4901	4674	4045	5690
Old Mutual	6897	6692	5950	7484
Legal and General	7836	7104	6389	8212
Prudential	15766	15618	10976	19474
AVIVA	15789	15618	10976	19474
HBOS	23311	22947	16376	33420
Standard Chartered	23417	22947	16376	33420
Lloyds Banking Group	24476	22947	16376	33420
Barclays	30313	31019	19211	39166
Royal Bank of Scotland	37314	31019	19211	39166
HSBC	92378	50162	29510	103826

Table 2

The table reports results from OLS estimation of the following model with the dependent variable identified in the first column;

$$□_{□,□} = □ + □_1 □^{□}_{□,□} + □_2 □^{□□□}_{□,□} + □_3 □^{□}_{□,□} \times □^{□□□}_{□,□} + □_4 □^{□□□}_{□,□} + □_5 □^{□}_{□,□} \times □^{□□□}_{□,□} + □_6 □^{□□□□}_{□,□} + □_7 □^{□}_{□,□} \times □^{□□□□}_{□,□} + □_8 □_{□} + □_{□,□}$$

Coefficient estimates are given in each column with *t*-stats robust to dependence in the residuals both across stocks at a point in time and across time for a given stock given in parentheses. Panel A gives results from headline variables discussed in previous work, panel B presents results from analysis of order flows and order book market shares and panel C shows results related to order book depth via our slippage measure. For the slippage results, the percentage figure in the row header gives the size of the notional market order for which the slippage is calculated expressed as a percentage of daily average daily trading volume.

	Benchmark		Financial		Pre		Financial×Pre		Ban		Financial×Ban		Post		Financial×Post		Volatility		R2
Panel A: benchmark variables																			
Returns	-9.08	(0.52) (0.60)	7.99	(0.46) (0.65)	-116.55	(2.45) (0.01)	-87.47	(2.27) (0.02)	-18.08	(0.44) (0.66)	-8.71	(0.35) (0.73)	0.66	(0.02) (0.98)	-9.68	(0.18) (0.86)			0.01 (12.1)
Spread	19.34	(6.68) (0.00)	-0.20	(0.05) (0.96)	2.73	(2.26) (0.02)	0.91	(0.50) (0.62)	15.24	(4.68) (0.00)	17.80	(2.32) (0.02)	11.87	(4.06) (0.00)	7.03	(1.75) (0.08)	0.01	(1.98) (0.05)	0.12 (143.3)
Volatility	8.49	(12.07) (0.00)	0.84	(0.80) (0.42)	8.25	(3.32) (0.00)	23.76	(1.28) (0.20)	31.26	(3.45) (0.00)	14.56	(1.39) (0.16)	9.92	(5.09) (0.00)	24.08	(2.01) (0.04)			0.02 (24.5)
Volume	0.94	(34.75) (0.00)	0.06	(1.69) (0.09)	0.36	(3.06) (0.00)	0.13	(1.00) (0.32)	0.17	(2.91) (0.00)	-0.40	(3.89) (0.00)	-0.04	(0.86) (0.39)	-0.10	(1.17) (0.24)	0.00	(1.01) (0.31)	0.05 (55.3)
Panel B: trading and transparency																			
Flow	-98.43	(2.18) (0.03)	-125.13	(3.01) (0.00)	-209.09	(2.16) (0.03)	49.28	(0.69) (0.49)	32.01	(0.53) (0.60)	95.51	(1.52) (0.13)	-118.3	(1.75) (0.08)	212.34	(1.52) (0.13)	0.00	(0.08) (0.94)	0.01 (12.1)
OBShare	0.76	(105.7) (0.00)	0.00	(0.26) (0.79)	0.03	(3.46) (0.00)	0.01	(0.72) (0.47)	0.00	(0.36) (0.72)	-0.08	(7.08) (0.00)	-0.02	(2.28) (0.02)	-0.02	(1.78) (0.08)	0.00	(0.88) (0.38)	0.08 (104.5)
Panel C: order book liquidity																			
BSlip 0.01%	9.98	(6.98) (0.00)	0.01	(0.00) (1.00)	1.50	(2.51) (0.01)	0.61	(0.67) (0.50)	8.17	(4.95) (0.00)	10.64	(2.85) (0.00)	6.34	(4.21) (0.00)	4.13	(2.02) (0.04)	0.00	(2.04) (0.04)	0.15 (212.0)
BSlip 0.10%	13.01	(10.66) (0.00)	1.48	(0.83) (0.41)	4.01	(8.58) (0.00)	2.66	(3.16) (0.00)	13.99	(10.64) (0.00)	38.87	(5.84) (0.00)	9.25	(6.38) (0.00)	10.44	(4.21) (0.00)	0.01	(2.95) (0.00)	0.36 (675.8)
BSlip 1.0%	79.40	(10.32) (0.00)	40.22	(1.56) (0.12)	41.83	(5.39) (0.00)	17.70	(2.52) (0.01)	98.17	(10.53) (0.00)	214.16	(4.59) (0.00)	20.26	(4.31) (0.00)	86.50	(3.70) (0.00)	0.00	(0.08) (0.94)	0.25 (400.5)
OSlip 0.01%	9.99	(6.99) (0.00)	0.00	(0.00) (1.00)	1.48	(2.47) (0.01)	0.56	(0.61) (0.54)	8.14	(4.88) (0.00)	10.54	(2.81) (0.00)	6.33	(4.20) (0.00)	4.03	(1.99) (0.05)	0.00	(2.11) (0.03)	0.15 (212.0)
OSlip 0.10%	13.18	(10.73) (0.00)	1.45	(0.82) (0.41)	4.08	(8.51) (0.00)	2.91	(2.87) (0.00)	14.26	(9.87) (0.00)	38.74	(6.36) (0.00)	9.37	(6.48) (0.00)	10.62	(4.38) (0.00)	0.00	(0.97) (0.33)	0.35 (646.9)
OSlip 1.0%	96.16	(9.32) (0.00)	88.16	(2.11) (0.03)	56.72	(4.23) (0.00)	26.13	(0.90) (0.37)	131.50	(7.61) (0.00)	316.83	(3.89) (0.00)	10.86	(1.57) (0.12)	72.36	(1.91) (0.06)	0.05	(2.21) (0.03)	0.19 (281.8)

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~~Table 6~~Table 3

This table reports statistics based on the vector autoregression detailed in eqs (3) and (4). The price impact of a trade is the impulse response of a trade to a trade innovation implied by the VAR. The permanent component of prices, and the trade correlated component of prices are calculated as detailed in the text. Each variable is calculated separately for each period and for each stock in the sample. The figures in the columns represent the simple average of each measure across the relevant sample of stocks for the relevant time period. In Panel A, the full sample of 23 financial firms is included, followed by their matched stocks. In Panel B, only the six largest financial firms and matched stocks are included, while in Panel C the 17 smaller financial firms and their matched stocks are included.

	Benchmark	**Pre**	**Ban**	**Post**
Financial stocks				
Price Impact	0.47	0.57	0.95	0.81
Permanent Component	0.68	0.64	0.37	0.56
Trade Correlated Comp.	0.45	0.40	0.24	0.40
Matched stocks				
Price Impact	0.47	0.55	0.77	0.80
Permanent Component	0.70	0.63	0.54	0.70
Trade Correlated Comp.	0.46	0.43	0.38	0.47

~~Table 5~~Table 4

The table reports results from OLS estimation of the model below with dependent variable given in the column headings;

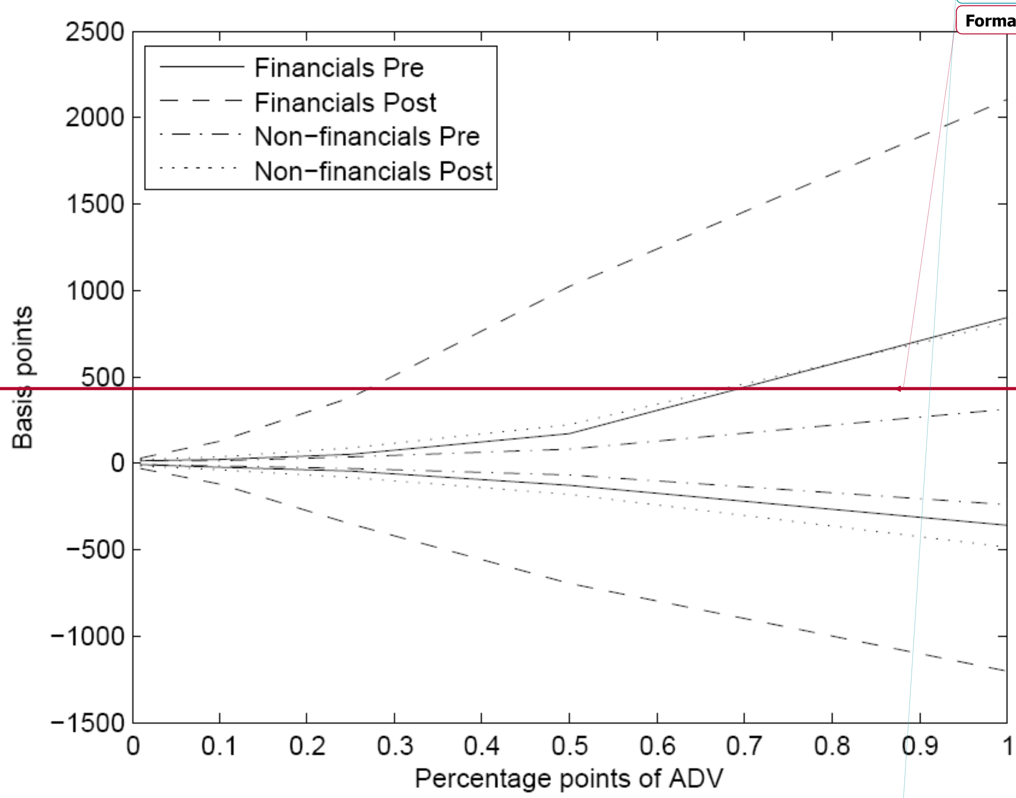
$$\square_{\square,\square} = \square + \square_1 \square^{\square}_{\square,\square} + \square_0 \square\square\square\square\square^{\square\square\square} + \square_1 \square^{\square}_{\square,\square} \times \square\square\square\square\square^{\square\square\square} + \square_2 \square^{\square\square\square}_{\square,\square} + \square_3 \square^{\square}_{\square,\square} \times \square^{\square\square\square}_{\square,\square} + \square_2 \square^{\square\square\square}_{\square,\square} \times \square\square\square\square\square^{\square\square\square} + \square_3 \square^{\square}_{\square,\square} \times \square^{\square\square\square}_{\square,\square} \times \square\square\square\square\square^{\square\square\square} + \square_4 \square^{\square\square\square}_{\square,\square} + \square_5 \square^{\square}_{\square,\square} \times \square^{\square\square\square}_{\square,\square} + \square_4 \square^{\square\square\square}_{\square,\square} \times \square\square\square\square\square^{\square\square\square} + \square_5 \square^{\square}_{\square,\square} \times \square^{\square\square\square}_{\square,\square} \times \square\square\square\square\square^{\square\square\square} + \square_6 \square^{\square\square\square\square}_{\square,\square} + \square_7 \square^{\square}_{\square,\square} \times \square^{\square\square\square\square}_{\square,\square} + \square_6 \square^{\square\square\square\square}_{\square,\square} \times \square\square\square\square\square^{\square\square\square\square} + \square_7 \square^{\square}_{\square,\square} \times \square^{\square\square\square\square}_{\square,\square} \times \square\square\square\square\square^{\square\square\square\square} + \square_8 \square_{\square} + \square_{\square,\square}$$

Coefficient estimates are given in each column with *t*-stats robust to dependence in the residuals both across stocks at a point in time and across time for a given stock given in parentheses. Each trend is set to zero at the beginning of the relevant interval and is incremented by one each trading day.

	Spreads		**BSlip: 0.1%**		**OSlip: 0.1%**		**Buys**		**Sells**		**Volume**		**Flow**	
Bench	18.86	(7.21)	16.75	(7.69)	18.12	(7.24)	1.06	(25.44)	1.07	(23.39)	0.06	(1.51)	-254.31	(2.65)
Bench×Financial	-0.74	(0.23)	9.16	(1.58)	23.18	(1.55)	0.05	(2.02)	0.05	(2.29)	0.05	(2.15)	-242.89	(2.60)
Bench×Trend bench	0.02	(0.85)	0.24	(9.00)	0.26	(10.10)	0.00	(1.52)	0.00	(2.20)	0.00	(1.94)	4.72	(2.13)
Bench×Trend bench×Financial	0.01	(1.00)	0.28	(2.16)	0.06	(0.67)	0.00	(0.51)	0.00	(1.22)	0.00	(0.90)	3.57	(1.75)
Pre	1.92	(0.87)	14.06	(4.27)	18.11	(4.76)	-0.13	(1.21)	-0.13	(1.31)	-0.13	(1.31)	-49.77	(0.25)
Pre×Financial	1.06	(0.33)	8.27	(3.77)	16.95	(3.30)	-0.23	(1.97)	-0.26	(2.57)	-0.24	(2.26)	205.15	(1.71)
Pre×Trend pre	0.40	(0.98)	3.36	(3.98)	3.23	(3.38)	0.10	(4.42)	0.10	(4.94)	0.10	(4.76)	-0.86	(0.03)
Pre×Trend pre×Financial	-0.03	(0.05)	1.96	(0.91)	0.39	(0.30)	0.08	(3.24)	0.08	(3.65)	0.08	(3.40)	-8.47	(0.61)
Ban	13.95	(4.81)	55.60	(8.35)	73.88	(7.07)	0.45	(3.87)	0.40	(3.55)	0.43	(3.75)	156.74	(1.24)
Ban×Financial	15.58	(2.57)	184.64	(3.23)	162.92	(3.62)	-0.55	(4.08)	-0.56	(4.78)	-0.56	(4.47)	100.92	(0.95)
Ban×Trend ban	0.13	(1.86)	-0.10	(1.25)	-0.26	(1.50)	-0.01	(5.06)	-0.01	(5.12)	-0.01	(5.25)	0.79	(0.45)
Ban×Trend ban×Financial	-0.01	(0.07)	-0.08	(0.09)	0.74	(1.45)	0.00	(0.30)	0.00	(0.21)	0.00	(0.31)	3.00	(1.50)
Post	20.30	(2.98)	24.37	(10.25)	24.39	(11.04)	-0.14	(1.76)	-0.12	(1.43)	-0.15	(1.96)	87.85	(0.64)
Post×Financial	4.21	(0.54)	64.17	(3.86)	60.32	(2.72)	-0.10	(0.85)	-0.14	(1.17)	-0.11	(0.93)	198.44	(0.98)
Post×Trend post	-0.33	(3.63)	-0.52	(4.36)	-0.52	(5.13)	0.00	(0.50)	0.00	(0.26)	0.00	(0.37)	-3.62	(0.55)
Post×Trend post×Financial	0.00	(0.04)	-0.98	(3.96)	-1.47	(3.07)	0.00	(0.54)	0.00	(0.90)	0.00	(0.79)	9.40	(1.65)
Volatility	0.01	(2.12)	0.00	(0.27)	0.01	(0.46)	0.00	(0.01)	0.00	(0.05)	0.00	(0.03)	0.02	(0.51)
R2	0.10		0.26		0.18		0.25		0.29		0.28		0.01	

~~**Figure 1**~~

~~Average Order Books~~



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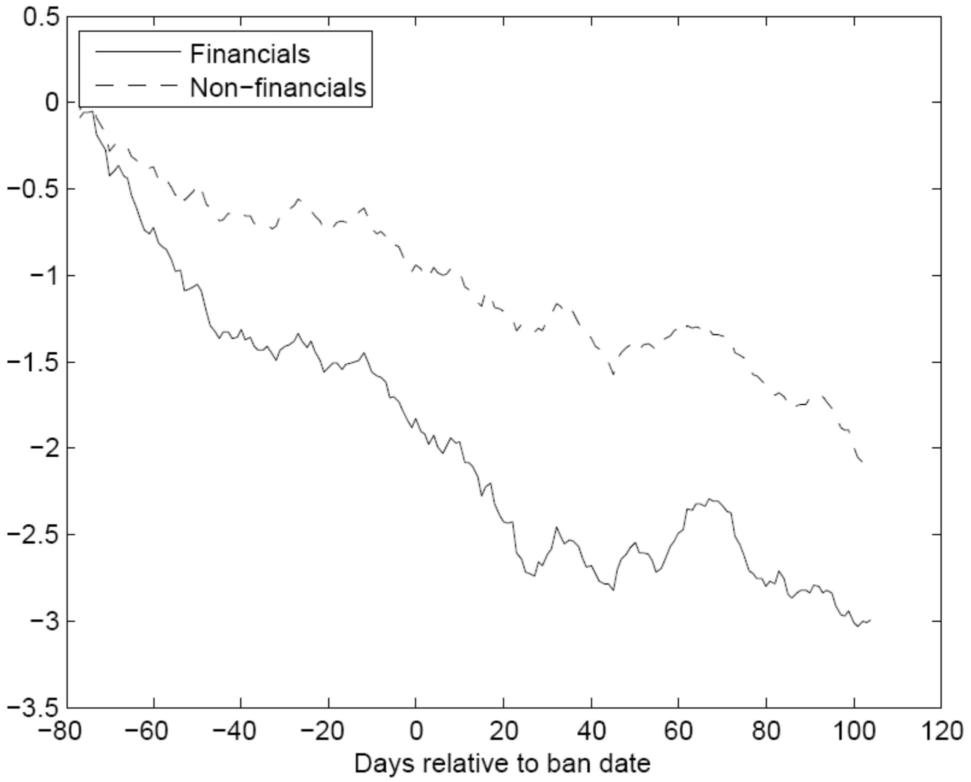
~~Figure 2~~Figure 1
Order Flows



Note: the graph plots cumulative daily order flows for financial firms and control group stocks over the entire sample period. For a particular stock on a particular day, order flow is defined as 1000 times the difference between the number of shares aggressively bought and the number of shares aggressively sold, divided by the total number of shares traded.

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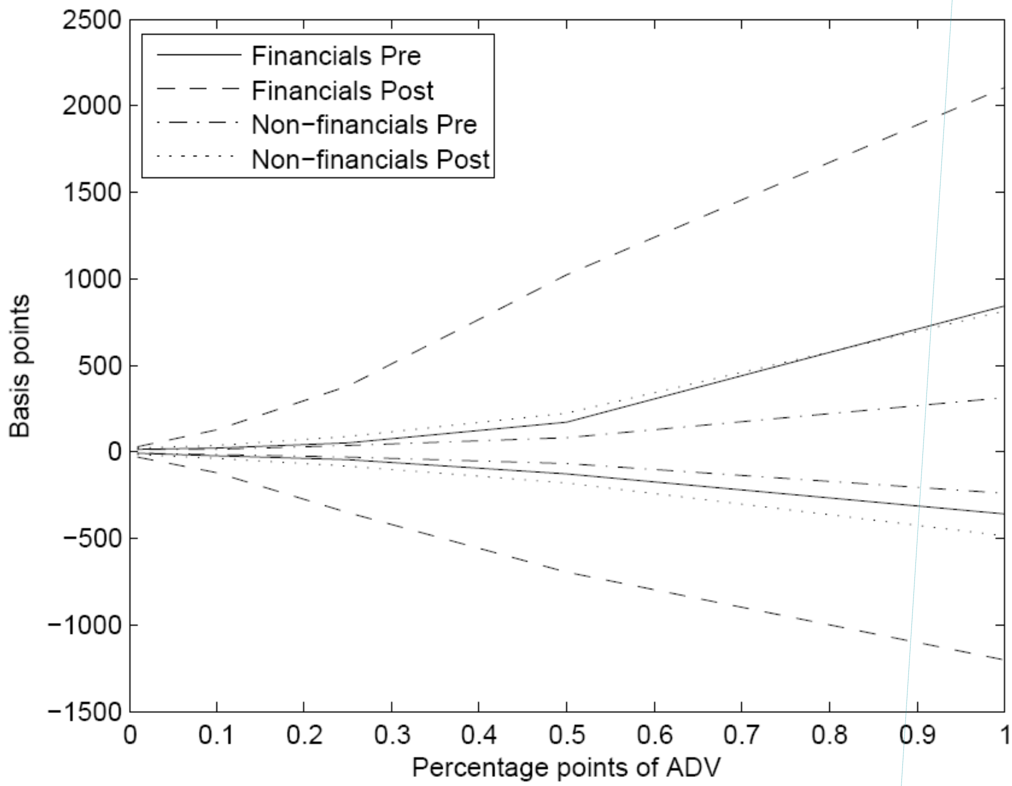
Figure 2

Average Order Books



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