Pricing accuracy, liquidity and trader behavior with closing price manipulation

Carole Comerton-Forde and Tālis J. Putniņš*

Discipline of Finance, Faculty of Economics and Business, University of Sydney, NSW 2006, Australia

18 March 2009

Abstract

We study closing price manipulation in an experimental market in order to evaluate its social harm. We find that manipulators, given incentives similar to many actual manipulation cases, decrease price accuracy and liquidity. The mere possibility of manipulation alters market participants' behavior causing reduced liquidity. We find some evidence that ordinary traders attempt to profitably counteract manipulation. This study provides examples of the strategies employed by manipulators, illustrates how these strategies change in the presence of regulatory scrutiny and assesses the ability of market participants to identify manipulation.

JEL classification: G14, C90

Keywords: manipulation, closing price, high-closing, experimental market

---

∗ Corresponding author: Tālis Putniņš Email: T.Putnins@econ.usyd.edu.au; Phone: +61 2 9351 3915; Fax: +61 2 9351 6461. We thank the Australian Stock Exchange, the Australian Research Council and Securities Industry Research Centre of Asia-Pacific for funding (ARC Linkage Project LP0455536). We are grateful for the comments of Henk Berkman, Robin Hanson, David Johnstone, Terrence Odean, Ryan Oprea and seminar participants at the University of Western Australia.

1. Introduction

Two fundamentally important aspects of financial market quality are pricing accuracy and liquidity. Pricing accuracy, the precision with which market prices reflect the underlying value of an asset, determines the informativeness of prices and their ability to encourage efficient resource allocation.[1] Liquidity allows efficient transfer of risk. The presence of traders with incentives to manipulate prices is a feature of markets that may limit their informational and transactional efficiency.

The central aim of this paper is to identify how closing price manipulation affects pricing accuracy and liquidity in order to evaluate manipulation's social harm. In their discussion of how to define illegal market manipulation, Kyle and Viswanathan (2008) argue that forms of manipulation should only be illegal if they are detrimental to *both* pricing accuracy and liquidity. Their argument is based on the premise that if a manipulator distorts pricing accuracy but brings about greater liquidity, or vice versa, depending on the relative social value of these two externalities, it may be economically efficient to allow such forms of manipulation.

The small body of existing evidence on the effects of manipulation is mixed and inconclusive, largely due to the difficulties in empirically studying manipulation. There is little doubt that manipulators are able to influence prices.[2] However, it is not clear how consistently and to what extent manipulators distort prices. Rational expectations theory predicts that if market participants are able to recognize manipulation they should profitably counteract it, thereby offsetting any price distortion. This intuition is central to the microstructure model in Hanson and Oprea (2008) where manipulation causes prices to be *more* accurate due to increased liquidity from rational profit seeking investors. Further evidence of manipulation attempts that do not impair pricing accuracy are found

---

[1] Kyle and Viswanathan (2008) point out that "pricing accuracy" does not mean the same thing as "market efficiency".

[2] There are many examples in the litigation releases of the US and Canadian regulators (see www.sec.gov/litigation/litreleases.shtm and www.osc.gov.on.ca/Enforcement/Proceedings/ep_index.jsp), direct empirical evidence in Aggarwal and Wu (2006), indirect empirical evidence in Carhart et al. (2002), Hillion and Suominen (2004), Khwaja and Mian (2005), Ni et al. (2005) and evidence from theoretical analyses in Allen and Gale (1992) and Kumar and Seppi (1992).

in experimental and field studies. In an experimental market involving asset trading via an electronic limit order book, Hanson et al. (2006) find no evidence that manipulators are able to distort prices. In a field experiment involving attempts to manipulate horse racing odds, Camerer (1998) reports that manipulation failed to distort prices.

On the second important aspect of market quality, liquidity, Hanson and Oprea (2008) show that in their microstructure model the possibility of manipulation increases liquidity due to rational traders' attempts to profitably counteract manipulation. Other studies, on the other hand, argue that manipulation reduces participation in markets resulting in lower liquidity, higher trading costs and higher costs of capital (see, for example, Prichard (2003)).

A further issue is how regulation affects manipulators' strategies, pricing accuracy and liquidity. In an inter-jurisdiction study, Cumming and Johan (2007) find that more detailed market manipulation rules increase trading activity through enhanced investor confidence. Bhattacharya and Daouk (2002) find in a sample of 103 countries that the *enforcement* of laws governing financial conduct, rather than simply their presence, affects markets in a positive way. Little is known about how manipulation strategies change in response to regulation.

Empirical examination of these issues is difficult. In order to provide direct evidence a researcher must be able to observe manipulation. In practice, regulators only observe the non-random subset of manipulation that they detect. Researchers, due to the opaqueness of regulation, are generally only able to observe the fraction of detected manipulation that gets prosecuted. Anecdotal evidence suggests this is a small proportion of manipulation and because it is systematically different to undetected or not prosecuted manipulation leads to significant biases in empirical analyses. The nature of this partial observability problem is such that conventional approaches to overcoming endogeneity or sample selection issues, such as Heckman two-stage procedures or instrumental variables, can not be applied to correct the bias. Further, key variables such as true asset values, incentives and information sets, as well as important counterfactuals such as manipulation free markets, are generally not observable. In order to control incentives and information, observe true asset values and counterfactual manipulation free markets, and to avoid the significant partial observability or endogeneity biases, we study closing price manipulation in an experimental market.

We find that manipulators, given incentives similar to many actual manipulation cases, decrease price accuracy (ex-post) and liquidity (ex-post and ex-ante). The mere possibility of manipulation alters market participants' behavior causing reduced liquidity. We find some evidence that ordinary traders attempt to profitably counteract manipulation, but that their effect is not strong enough to prevent the harm caused by manipulation. Finally, this study provides examples of the strategies employed by manipulators, illustrates how these strategies change in the presence of regulatory scrutiny and assesses the ability of market participants to identify manipulation.

Hanson et al. (2006) conduct the first laboratory work on price manipulation in asset markets. Their main result is that manipulators are unable to distort price accuracy throughout trading sessions because other traders counteract the actions of the manipulator. We extend Hanson et al. (2006) in several important ways. First, we consider not only pricing accuracy, but also the effect of manipulation on liquidity – the second externality that must be understood to draw conclusions about manipulation's social harm or benefit. Second, by making the presence of manipulators uncertain, we create a more realistic setting and are able to examine how the possibility of manipulation alters trading characteristics, i.e. ex-ante effects. Third, we examine how regulation affects manipulators' strategies and other traders' reactions. Finally, and perhaps most importantly, we examine a different form of manipulation - closing price manipulation - by giving manipulators incentive to realize high *closing* prices as opposed to high prices throughout a trading session. We demonstrate that this last difference is critical in determining how successfully manipulators influence prices. This is because closing price manipulators can concentrate their resources in a very short period of time.

A large number of parties stand to gain from a high or low closing price and consequently have incentive to engage in closing price manipulation. Examples include a fund manager at the end of a reporting period that benefits from inflating reported performance[3], a market participant with a large position in cash settled derivatives at expiry[4], a substantial shareholder during the pricing period of a corporate acquisition or a trader about to receive a margin call. Closing prices have also been manipulated by brokers attempting to alter their customers' inference of their execution ability[5], during pricing periods for seasoned equity issues, to maintain a stock's listing on an exchange with minimum price requirements and on stock index rebalancing days for a stock to gain inclusion in an index. Closing price manipulation is relatively easy and can be conducted by an individual without much planning or capital. Although price distortions created by closing price manipulation are generally short-lived, their effect is substantial because of the importance of closing prices. We design our experiment to allow comparison with Hanson et al. (2006). The comparison highlights important differences between intra-day and day-end price manipulation.

2. Experiment design and procedure

Our experiment design consists of three treatments: a control with no manipulators, a treatment to examine the ex-ante and ex-post effects of manipulation and a treatment to examine the effects of regulation. In all treatments 12 subjects trade shares of a common asset in an electronic continuous double auction market. Each experimental session consists of 16 trading periods of 200 seconds each, under one of the treatments.

Treatment 1 replicates a variation of a classic design developed by Plott and Sunder (1988) to study information aggregation and is similar to the control treatment used by Hanson et al. (2006). The fundamental value of the asset, V, is unknown to individual subjects during the course of trading and is revealed at the end of each period. However, it is made common knowledge among subjects that $V \in \{20,40,80\}$ with an equal probability of each value occurring. At the start of each trading period subjects are

[3] See, for example, Carhart et al. (2002) and Bernhardt and Davies (2005).
[4] See, for example, Kumar and Seppi (1992) and Ni et al. (2005).
[5] See, for example, Hillion and Suominen (2004).

endowed with four shares of the common asset, 200 experimental currency units (ECU) and a clue about *V*. The clue is knowledge of one of the three possible values that *V* will certainly not take in that period. For example, if $V = 40$, half the traders (chosen at random) are told $V \neq 80$ and the other half are told $V \neq 20$. Although no individual knows the true fundamental value, *V*, in aggregate subjects have enough information to determine *V*.

At the end of each period the shares owned by each trader are converted to cash at their fundamental value, *V*, and, together with any remaining cash, added to the trader's payoff pool. The traders' payoff pools determine how much they are paid for participating in the experiment as explained later. Traders' endowments are reset to the original amount of four shares and 200 ECU at the beginning of each period.

Treatment 2 introduces the possibility of manipulation by giving some subjects incentives to manipulate the closing price. In a randomly selected half of the trading periods a trader drawn at random is informed that they will assume the role of manipulator for that period. The remaining traders are only aware that a manipulator may have been selected and the probability of manipulation is not provided. Manipulators receive the same initial endowment as other traders but different payoffs. A manipulator's payoff at the end of a trading period is $15(P_{closing} - P_{median}) + 250$, where $P_{closing}$ and P_{median} are the closing price (the last traded price) and median price respectively. This payoff provides incentive for manipulators to try and increase the last trade price irrespective of *V* and is consistent with many real examples of closing price manipulation.[6] Unlike several other forms of market manipulation, closing price manipulators often profit from sources external to the market, e.g. from overstated fund performance. This is simulated by the payoff we provide to manipulators. Periods with a manipulator allow us to examine ex-post effects of manipulation and periods without a

[6] Although in practice manipulation conducted with the intent of decreasing the closing price also exists, is considerably less common than increasing closing prices. In all of the closing price manipulation cases prosecuted by the US and Canadian regulators between 1996 and 2007 none involve attempts at decreasing closing prices. We believe downward closing price manipulation has similar effects on markets but leave the examination of this to future research.

manipulator provide evidence on the ex-ante effects of manipulation (the effect of possible manipulation).

At the end of each period ordinary traders submit guesses as to whether or not a manipulator was present. Correct (incorrect) guesses earn (cost) the subject 50 ECU. Manipulators guess how many of the other 11 traders will have guessed that a manipulator was present and also earn (lose) 50 ECU for correct (incorrect) guesses.

Treatment 3 simulates possible manipulation with a regulator by introducing a penalty for manipulators that are "detected" by the other traders. In each period a randomly selected trader assumes the role of manipulator. Manipulators start with the same endowment as other traders and choose whether or not to trade given knowledge of the following payoffs. A manipulator that chooses to trade is "detected" if eight or more of the other 11 traders (approximately three quarters) guess that the manipulator traded, and evades "detection" otherwise. Undetected manipulators receive a manipulation profit of $15(P_{closing} - P_{median})$ and detected manipulators receive a detection penalty of negative the manipulation profit. In addition to the manipulation profit or detection penalty (which is zero if the manipulator does not trade) manipulators also receive 250 ECU to make their average payoffs close to those of the ordinary traders. This payoff structure and the choice offered to the manipulator allow us to investigate how regulation affects manipulation.

At the end of each period ordinary traders submit guesses as to whether or not the manipulator traded and are paid for correct and incorrect guesses as in Treatment 2. These guesses determine if a manipulator that chooses to trade is "detected". At the end of each period the manipulator guesses how many of the other 11 traders will have guessed that the manipulator traded. Table 1 contains a summary of the payoffs from trading and guessing in each of the treatments.

< TABLE 1 HERE >

Subjects trade using computer terminals running a trading simulator (Rotman Interactive Trader) that allows them to place market and limit orders.[7] Subjects, on their terminals, are able to see the full order book, a list and chart of trade prices and volumes and a countdown of the time remaining to the end of the period. Conversion between stocks and cash occurs instantaneously after a trade and there are no brokerage costs, short selling or margin buying. To avoid price biases from the prohibition of short selling and margin buying, we set the initial endowments of stock and cash such that buying and selling power are on average approximately equal. Subjects are not allowed to communicate with one another and are aware of the payoffs that each type of participant faces. The asset values, *V*, clues and the manipulator allocations are randomly drawn prior to the study and the ordering kept the same for each session as detailed in Table 2.[8] The instructions provided to subjects consist of a core set common to all treatments with additional instructions added for Treatments 2 and 3.[9]

< TABLE 2 HERE >

Eight sessions are conducted with two sessions in Treatments 1 and 3 and four sessions in Treatment 2. Twice as many sessions are run in Treatment 2 than the other two treatments because there are two sub-treatments in Treatment 2 (periods that have a manipulator and periods that do not). With 16 trading periods in each experimental session we have 32 trading periods in Treatments 1, Treatment 3 and each of the sub-treatments of Treatment 2. We collect data on all trades and orders including prices, volumes, trade/order direction, trade initiator, trader IDs and timestamps as well as snapshots of the full order book at five-second intervals. Each session takes approximately two hours and subjects receive an average payment of $30.[10] Subjects are

[7] A screenshot of the trading interface is available from the authors upon request.

[8] In Treatment 2 the periods in which a manipulator is present are drawn at random subject to the conditions that for each of the asset values and each of the halves of the experimental session (periods 1-8 and 9-16) there are an equal number of periods with and without a manipulator. This condition allows a more reasonable comparison of the sub-treatments (manipulator and no manipulator) in Treatment 2.

[9] The instructions are available from the authors upon request.

[10] At the end of an experimental session subjects are ranked in descending order by their total payoff pools. The highest payoff earning subject receives $45, the second and third ranked subjects receive $40 each, the next two receive $35 each and so on down to subjects ranked 10 and 11 who receive $20 each and the lowest ranked subject who receives $15. This payout method, which has some similarities to the method

not allowed to participate in more than one session so in total 96 subjects are recruited. The subjects are undergraduate and graduate students at a university business school.

3. Analysis

As a starting point, we replicate part of the analysis in Hanson et al. (2006) to examine the effect of manipulation on closing price accuracy. We extend this analysis to examine intra-period effects and then apply it to liquidity variables. Next, we characterize the trading strategies used by manipulators with and without a regulator and examine how manipulation affects the behavior of ordinary traders. Finally, we assess the ability of market participants to identify manipulation and conduct some robustness tests. Throughout most of our analysis we split Treatment 2 into its two sub-treatments, 2a and 2b, according to whether or not a manipulator was present. We refer to Treatments 1, 2a, 2b and 3 as the control treatment, possible manipulation, manipulation and possible manipulation with a regulator respectively.

3.1 Effects on price accuracy

We begin our analysis of the effect of manipulation on price accuracy by replicating the tests conducted by Hanson et al. (2006). Figure 1 shows the prices of the last trade in each period (equivalent to the closing price in many stock exchanges), the averages of these prices by treatment, and the fundamental asset value, V, in each period. Similar to Hanson et al. (2006), prices are attracted towards V in each period but display "stickiness" to a value around 40. From Figure 1 it appears price convergence, i.e. the degree to which market prices track V, is stronger in our experiment than in that of Hanson et al. (2006).

< FIGURE 1 HERE >

We quantify the price convergence properties and test for the effect of manipulation on the ability of prices to track V using the following linear mixed effects models (replicating Hanson et al. (2006)):

used by Bloomfield and O'Hara (1999), ensures that average payoffs are equal across the three treatments and guarantees that the subjects receive at least $15.

$$\begin{aligned} price_{ij} = (\alpha + \alpha_i) + (\beta_1 + \beta_{1i}) manipulation_{ij} + (\beta_2 + \beta_{2i}) V_j \\ + (\beta_3 + \beta_{3i}) manipulation_{ij} \times V_j + \varepsilon_{ij} \end{aligned} \tag{1}$$

$$(price_{ij} - V_j)^2 = (\alpha + \alpha_i) + (\beta_1 + \beta_{1i}) manipulation_{ij} + \varepsilon_{ij} \tag{2}$$

$Price_{ij}$ is the average of the last three trade prices in period j of session i. $Manipulation_{ij}$ is an indicator variable that takes the value of 1 if the trading period is under Treatment 2a, i.e. a manipulator is present. V_j is the fundamental asset value in period j. Parameters α_i, β_{1i} and β_{2i} are random effects for session i. All random effects and the error term, ε_{ij}, are assumed to be distributed independently and normally with a mean of zero. Consequently, this model allows composite errors to be heteroscedastic and correlated between trading periods within an experimental session, but assumes sessions are independent of one another. If prices were to converge perfectly to V, α (in equation 1) would be zero and β_2 would be one. If manipulation had no effect on prices or price accuracy β_1 and β_3 would be zero.

Estimation results are reported in Table 3.[11] As expected price convergence is not perfect; in equation 1, α (25.84) is significantly larger than zero and β_2 (0.36) is just over a third of what it would be under perfect convergence. However, price convergence is better than in the experimental markets of Hanson et al. (2006) where α and β_2 are estimated as 48.58 and 0.2 respectively. There are a number of design modifications that may explain this difference. In our experimental market $V \in \{20,40,80\}$ as opposed to $V \in \{0,40,100\}$, our instructions are more explicit in explaining how to profit when market prices are away from V, our initial endowment makes buying and selling power more equal on average and we use a different trading interface.

< TABLE 3 HERE >

In contrast to Hanson et al. (2006), Table 3 shows that closing price manipulation has a large and detrimental ex-post effect on prices and their accuracy. Estimates from the first mixed effects model suggest that end of period prices in the presence of a closing price

[11] We estimate all models using Restricted Maximum Likelihood (REML).

manipulator are on average approximately 20 ECU higher than when there is no manipulator. This, in the second model, translates to a large increase in squared price error (a large decrease in price accuracy) in the presence of manipulation. The increase in squared price error attributable to manipulation (283 ECU^2) is very large relative to the underlying level (374 ECU^2), although this result is not statistically significant.[12]

Next, we extend the Hanson et al. (2006) models to examine the effects of our two additional treatments (possible manipulation with and without a regulator). To do this we add the variables $possible_{ij}$, and $regulator_i$ (and their interactions with $value_j$) to equations 1 and 2. $Possible_{ij}$, and $regulator_i$, similar to $manipulation_{ij}$, are indicator variables that take the value of 1 if the trading period is under Treatment 2a or Treatment 3 respectively. The results reported in Table 3 show that possible manipulation, i.e. when there is no manipulator but traders are under the belief that there may be a manipulator, does not have a significant effect of on prices or their accuracy. We conclude that closing price manipulation does not have a significant ex-ante effect on prices, but does have significantly detrimental ex-post effects. This is consistent with the main theoretical prediction in Hanson and Oprea (2008). Table 3 also shows that possible manipulation in the presence of a regulator, i.e. when potential manipulators face a penalty if detected, does not have a significant effect on prices. This could be because the risk of incurring a penalty deters manipulation or simply that manipulators distort prices less to avoid detection. As shown in the following subsections, both effects are at play.

Our finding that closing price manipulation has a large and detrimental effect on prices and price accuracy is not contradictory to Hanson et al. (2006), but rather, complimentary. Given the similarity in the experiment designs used in this study and in Hanson et al. (2006), our findings demonstrate that the manipulators' incentives, defined by the payoff structure, are critical in determining the effect of manipulation on prices. Manipulators in our experimental market are given less market power in that there is one

[12] When interpreting levels of statistical significance in this study, one should take into account that we have fewer observations per treatment than typical in empirical research.

manipulator trading against 11 other traders compared to six manipulators trading against six other traders in Hanson et al. (2006). However, of critical importance is that the manipulator in our experimental market is concerned about influencing only the last trade price, not prices throughout the entire period (as in Hanson et al. (2006)) and for this reason our manipulators are detrimental to price accuracy. The difference we have highlighted is of particular concern given the many real examples of market participants with incentives to realize high closing prices and the numerous important uses of closing prices.

Given that our manipulation incentive is focused at the end of a trading period rather than throughout, we also analyze price accuracy within a trading period. Figure 2 shows the average absolute price error (the absolute of the difference between trade price and fundamental asset value) for each treatment in ten-second intervals within a trading period. Average price error decreases through the course of a trading period as a result of price discovery. Our experimental market appears to gradually incorporate information into the price – a feature consistent with behavior observed on equity markets and existing literature. Price error appears to increase sharply in the last 20 seconds of the trading period in the presence of manipulation (Treatment 2b), but does not increase in any of the other treatments.

< FIGURE 2 HERE >

We quantify manipulation's effects on price accuracy within a trading period using a linear mixed effects model as previously:

$$\begin{aligned}\left|price_{ijk} - V_j\right| &= (\alpha + \alpha_i + \alpha_{ij}) + (\beta_1 + \beta_{1i})possible_{ij} + (\beta_2 + \beta_{2i})manipulation_{ij} \\ &+ (\beta_3 + \beta_{3i})regulator_i + (\beta_4 + \beta_{4i})V20_j + (\beta_5 + \beta_{5i})V80_j + (\beta_6 + \beta_{6i})period_j \\ &+ (\beta_7 + \beta_{7i})interval_k + (\beta_8 + \beta_{8i})interval_k^2 + (\beta_9 + \beta_{9i})last_k + (\beta_{10} + \beta_{10i})last_k \times possible_{ij} \\ &+ (\beta_{11} + \beta_{11i})last_k \times manipulation_{ij} + (\beta_{12} + \beta_{12i})last_k \times regulator_i + \varepsilon_{ijk}\end{aligned} \tag{3}$$

Price$_{ijk}$ represents the price of the trade immediately prior to the end of the k^{th} ten-second interval in period j of session i. *Possible*$_{ij}$, *manipulation*$_{ij}$ and *regulator*$_i$ are indicator variables that take the value of 1 if the trading period is under Treatment 2a, 2b or 3 respectively. *V20*$_j$ and *V80*$_j$ are indicator variables that take the value of 1 if $V = 20$ and

$V = 80$ respectively. $Period_j$ is the trading period number within the experimental session, which takes values from 1 to 16. $Interval_k$ is the number of the ten-second interval within a trading period, which takes values from 0 to 19. $Last_k$ is an indicator variable which takes the value of 1 for the last interval of the trading period. Parameters α_i and β_{1i} to β_{12i} are random effects for session *i* and α_{ij} is a random effect for period *j* of session *i*. All random effects and the error term, ε_{ijk}, are assumed to be distributed independently and normally with a mean of zero. Consequently, this model allows composite errors to be heteroscedastic and correlated between trading periods within an experimental session and between intervals within a trading period, but assumes sessions are independent of one another.[13]

Estimation results are reported in Table 4. Manipulation (Treatment 2b) causes prices to be less accurate on average throughout a trading period (by 4.2 ECU) and even less accurate in the last ten seconds of the trading period (an increase of 5.7 or total of 9.9 ECU). Although only the former effect is statistically significant, the estimated magnitude of both effects is very economically meaningful. The end-of-period increase in absolute trade price error that is attributable to manipulation is, as a percentage of *V*, between 12% and 50% (for $V = 80$ and $V = 20$ respectively). The other treatments do not appear to have a significant effect on price accuracy, consistent with the previous analysis. The coefficients of $interval_k$ and $interval_k^2$ suggests price accuracy improves (at a decreasing rate) through the course of a trading period, consistent with the pattern shown in Figure 2. Price accuracy also tends to improve through the course of an experimental session as participants learn to aggregate information more accurately. Prices are significantly less accurate for $V = 20$ and $V = 80$ than when $V = 40$, consistent with the previously observed "stickiness" of prices to a value around 40.

< TABLE 4 HERE >

[13] A covariance structure that allows the correlations between intervals within a period to decline with time-separation (for example, a first-order autoregressive process) may seem more appropriate than constant correlation if random price shocks take several intervals to dissipate. However, if price shocks are random, because the data are from repeated measures the effects of gradual adjustment to price shocks will average out leaving the estimates unbiased.

3.2 Effects on liquidity

The previous subsection shows manipulation has a significant detrimental ex-post effect on price accuracy. In order to evaluate manipulation's overall social harm we now examine its effects on the second important market externality – liquidity. We use three alternate measures of liquidity: bid-ask spread, depth and volume. Figure 3 shows the evolution of these variables through the course of a trading period. The patterns exhibited by these variables are generally consistent with behavior observed in equity markets (see, for example, Cai et al. (2004)) and other experimental markets (see, for example, Bloomfield et al. (2005)). Bid-ask spreads decline through the trading period but increase at the end of the period, depth tends to increase through the trading period at a decreasing rate and volume increases sharply at the end of the trading period. The most apparent difference between the treatments is that spreads tend to be smaller in the control treatment than in the other treatments.

< FIGURE 3 HERE >

We quantify manipulation's effects on liquidity with a linear mixed effects model, similar to the models used to examine price accuracy:

$$\begin{aligned} Y_{ij} = {} & (\alpha + \alpha_i) + (\beta_1 + \beta_{1i}) possible_{ij} + (\beta_2 + \beta_{2i}) manipulation_{ij} + (\beta_3 + \beta_{3i}) regulator_i \\ & + (\beta_4 + \beta_{4i}) V20_j + (\beta_5 + \beta_{5i}) V80_j + (\beta_6 + \beta_{6i}) period_j + \varepsilon_{ij} \end{aligned} \tag{4}$$

Y_{ij} represents the liquidity variable in period j of session i. Bid-ask spreads and depth values are averaged across the ten-second intervals within a period, similar to a time-weighted average. Volume is measured the total number of shares traded in the period. $Possible_{ij}$, $manipulation_{ij}$ and $regulator_i$ are indicator variables that take the value of 1 if the trading period is under Treatment 2a, 2b or 3 respectively. $V20_j$ and $V80_j$ are indicator variables that take the value of 1 if $V = 20$ and $V = 80$ respectively. $Period_j$ is the trading period number within the experimental session, which takes values from 1 to 16. Random effects parameters α_i and β_{1i} to β_{6i}, as well as the error term, ε_{ij}, are assumed to be distributed independently and normally with a mean of zero. Consequently, this model allows composite errors to be heteroscedastic and correlated between trading periods within an experimental session, but assumes sessions are independent of one another.

Estimation results are reported in Table 5. Bid-ask spreads are approximately nine to ten percent wider in Treatment 2 relative to the control treatment regardless of whether a manipulator is actually present or not. Similarly, spreads are approximately ten percent wider when manipulation is possible in the presence of a regulator (Treatment 3) than in the control treatment. These effects are statistically significant at the 5% level and meaningful relative to the grand mean spread of approximately 21% corresponding to the control treatment. Spreads are also wider for $V = 20$ and $V = 80$ than $V = 40$ and tend to decrease through the course of an experimental session. These results are consistent with notion that spreads are wider when there is greater uncertainty about V and that manipulation, or even the mere possibility of manipulation, causes greater uncertainty.

< TABLE 5 HERE >

$V = 20$ and $V = 80$ cause greater uncertainty than $V = 40$ due to the nature of the clues provided to traders. An obvious initial strategy for traders with the clue $V \neq 20$ is to buy the asset at prices below 40 knowing that either $V = 40$ or $V = 80$. Similarly, for the clue $V \neq 80$ an obvious initial strategy is to sell the asset at prices above 40. Consequently, when $V = 40$ and the set of clues is $\{V \neq 20, V \neq 80\}$ there tends to be no shortage of buyers at prices up to 40 and sellers at prices down to 40, so prices converge quickly and accurately with little uncertainty. As a secondary strategy, after having inferred the clues of other traders by observing order flow, a trader may choose to post limit orders above and below V, thereby acting as a market maker and earning the spread for supplying liquidity. On the other hand, when $V = 80$, only the traders with the clue $V \neq 20$ have an obvious initial strategy – to buy at prices up to 40. The other half, with the clue $V \neq 40$, only know with certainty that either $V = 20$ or $V = 80$ and therefore have to infer which of these possibilities is true by observing other traders' order flow. Consequently, states $V = 20$ and $V = 80$ induce greater uncertainty and cause traders to set wider spreads.

The presence of manipulators that have no regard for the fundamental asset value, V, increases the probability of observing a false signal in order flow and therefore increases the chance of incorrectly inferring V. As a result, price uncertainty is greater and traders set wider spreads.

Volume is significantly lower when manipulation is possible (Treatment 2a) relative to the control treatment, suggesting that the possibility of manipulation creates a greater reluctance to trade. This effect may stem from the fact that, all else being equal, manipulation and the possibility of manipulation increase spreads and therefore increase trading costs. This explanation is supported by the finding of Barclay et al. (1998) that wider spreads lead to reduced volume. The effect on trading volume is not as strong (and not statistically significant) when manipulation actually occurs (Treatments 2b and 3) because the manipulators, in trading to manipulate prices, offset the reduced trading levels of others. However, despite increased spreads, when $V = 20$ and $V = 80$ trading volume is significantly higher. Depth does not appear to be significantly affected by manipulation.

The results in this subsection on spreads and volumes suggest that manipulation, and even the mere possibility of manipulation, has a significant detrimental effect on market liquidity.

3.3 Manipulation strategy

We now turn our focus to the trading strategies employed by closing price manipulators. We characterize manipulators' order types and the timing of their trades in the presence and absence of a regulator. To do this, we classify orders into four categories of increasing aggressiveness: market orders (and marketable limit orders, i.e. limit orders that cause immediate execution) that execute all of the depth at the best quote and at least some of the depth at the next best quote; market orders that execute at the best quote; limit orders that are at least part filled and limit orders that are not at all filled. Figure 4 shows a breakdown of order types submitted by manipulators and other traders in each treatment. Panel A compares the orders used by manipulators to those used by other traders in the absence of a regulator (Treatment 2b). Panel B makes the same comparison, but in the presence of a regulator (Treatment 3). The most striking difference is the large number of very aggressive buy orders used by manipulators in the absence of a regulator (1.65 multiple-price market orders per period per manipulator compared to 0.14 for ordinary traders). This difference is statistically significant at the 1% level using a paired t-test (t-statistic of 4.87). In the presence of a regulator, manipulators tend to use considerably less aggressive orders. It appears that manipulators in such circumstances use more of the second most aggressive order type (1.40 single-price market orders per period per manipulator compared to 0.88 for ordinary traders), although the difference is not statistically significant.

It also appears that manipulators in Treatment 2 tend to use more aggressive sell orders than other traders, although this effect is not statistically significant and is smaller in magnitude than for buy orders. This effect can be explained by the fact that manipulators, unlike other traders, are not concerned about selling at above fundamental asset values. They merely sell stock to increase their purchasing power allowing them to aggressively buy at the end of the trading period.

< FIGURE 4 HERE >

Figure 5 shows the timing of buy and sell trades initiated by manipulators. In the absence of a regulator, manipulators tend to sell stock around the middle of a trading period to increase their buying power and then buy heavily in the last ten seconds of trading. In the presence of a regulator, however, the buying activity of manipulators is less intense and tends to peak earlier. Buying activity is highest in the second to last ten-second interval, as opposed to the last interval, and involves less than a quarter of the amount of trades that a manipulator uses when there is no regulator.

< FIGURE 5 HERE >

To test the differences in trading times between manipulators and ordinary traders we calculate a measure of how late in the trading period most trading takes place - the volume weighted average trade time (VWATT) measured in seconds from the start of the trading period. Paired t-tests comparing the VWATT of manipulators' buy and sell volume with that of ordinary traders confirm that manipulators in both Treatments 2 and 3 tend to buy later than ordinary traders (significant at the 5% level). There is no significant difference in the timing of sell orders for manipulators compared to ordinary traders.

The results reported in this subsection suggest that in our experimental setting the introduction of a regulator, i.e. imposing a penalty on detected manipulators, is successful in reducing the intensity of manipulation. This helps explain why price accuracy is not significantly worse when a manipulator accompanied by a regulator is present in our experimental market. However, there is a second factor at play here. The penalty we impose on detected manipulation in Treatment 3 also reduces the frequency of manipulation. Twenty-two percent of the subjects given the opportunity to manipulate the market in Treatment 3 choose not to manipulate. This fraction roughly corresponds to the perceived detection probability. Twenty-four percent of manipulators in Treatment 2 (no regulator) guess that at least eight out of the other 11 traders would guess that a manipulator was present, i.e. the equivalent of being detected in Treatment 3. The perceived detection probability in Treatment 3 is likely to be somewhat less than 24% because manipulators choose to act in a more subtle manner than in Treatment 2. Of the 78% that do attempt manipulation in Treatment 3, 40% are detected and receive a penalty and 60% avoid detection. Therefore the actual detection probability given the decision to manipulate (40%) is higher than the perceived probability of detection (less than 24%).

We conclude that in our experiment the imposition of a penalty on detected manipulators helps restore price accuracy, both by deterring manipulation and by reducing the intensity of the remaining manipulation. Of course, the ability of regulation to reduce the harm caused by manipulation is likely to depend on the credibility of the regulator, the size of the penalty and the probability of being caught. We have only simulated a specific instance of these parameters, which could be viewed as that of a successful regulator.

3.4 Effects on ordinary traders' behavior

Previously we showed that ordinary traders set wider spreads in the presence of manipulators in what appears to be a reaction to increased price uncertainty. In this subsection, we examine how manipulators affect other traders' order submission strategies and test some specific predictions about trader reactions to manipulation. Figure 4 Panel C compares the order types submitted by ordinary traders under the control treatment and possible manipulation (Treatment 2a). There are no obvious differences in the aggressiveness of orders and none of the paired t-tests by order type show any significant differences in order submission strategy between the two treatments.

Hanson and Oprea (2008) show that in their microstructure model the possibility of manipulation increases liquidity due to the desire of rational traders to profitably counteract manipulation attempts. In the context of closing price manipulation, one might expect a rational trader to increase depth on the ask side to profit from a manipulator's aggressive buying at prices above fundamental value. We test for this specific prediction using the mixed effects model in equation 3 replacing the dependant variable with depth at the best ask price and an alternate measure: the average depth at the best three ask prices. If ordinary traders do increase depth on the ask side throughout the trading period (at the end of the trading period) to try and profit from manipulation we would expect a significant positive coefficient on $possible_{ij}$ ($last_k$ x $possible_{ij}$). Estimating this model we find that possible manipulation causes an increase in depth of 1.44 shares at the best ask price in the last ten-second interval of a trading period. This increase is meaningful compared to the grand mean, α, of 2.71 shares and is statistically significant at the 10% level. However, we do not find evidence of an increase in depth at the ask throughout a trading period nor does this effect hold for average depth at the best three ask quotes. We conclude that there is some evidence of ordinary traders attempting to profitably counteract manipulation by offering more shares at the best ask and that these traders believe the manipulator, if present, is likely to trade in the last ten-second interval. However, the effect of this behavior is not strong enough to prevent manipulators from distorting prices, nor is it strong enough to restore the bid-ask spread to the level in the control treatment.

3.5 Ability of market participants to recognize manipulation

In this final part of our analysis, we assess the accuracy with which market participants are able to identify manipulation through observing the limit order book, a real-time list of trades and a chart of trade prices and volumes. The ability for market participants to identify manipulation is important in facilitating trading strategies that exploit manipulators and help restore price accuracy. It is also important for the efficient functioning of the allocative role of prices because if market participants are unable to recognize when prices have been distorted, biased signals will be used in resource allocation.

Table 6 shows two-way frequencies of the guesses submitted by ordinary traders to the question of whether or not a manipulator was present in the market, as well as the percentage of correct guesses. We test the null hypothesis that the percentages of correct guesses are equal to 50%, i.e. guessing ability is only as good as chance. Despite having shown that manipulation has a substantial impact on prices, surprisingly, market participants have poor ability in accurately identifying manipulation. In Treatment 2, overall only 53.2% of guesses are correct, only marginally better than chance. When a manipulator is present, market participants correctly identify this with an accuracy of 49.0% - no better than chance. In Treatment 3, the accuracy of guesses is higher: 59.8% overall and 64.9% when manipulation takes place.

< TABLE 6 HERE >

The difference in guessing accuracy between Treatments 2 and 3 may in part be explained by the different perceived prior probabilities of manipulation. In Treatment 2, a manipulator, with no reason not to manipulate, is selected in a randomly chosen 50% of trading periods. However, participants are not aware of the proportion of periods with a manipulator. On the other hand, in every period of Treatment 3, a manipulator is given the choice of whether to manipulate or not. Because participants are aware of all payoffs that are relevant to deciding whether or not to manipulate, arguably, participants are able to better estimate the prior probability of manipulation and therefore guess more accurately whether or not a manipulator was present. The generally poor accuracy with which market participants identify manipulation is concerning because, among other things, it makes profitably counteracting manipulation difficult.

3.6 Robustness tests

We check the robustness of our results to using alternative measures of price accuracy and liquidity, disregarding the first four trading periods in each session to allow participants learning time and simplification of our mixed effects regression models to random intercept models by dropping the random slopes. We find that our main results are robust to these tests.

4. Discussion and conclusions

Understanding how trading strategies commonly labeled as "manipulation" affect price accuracy and market liquidity is critical in determining whether such strategies are harmful to markets and should be illegal (Kyle and Viswanathan (2008)). However, the limited evidence that exists regarding the effects of manipulation on markets is mixed and inconclusive. This is largely because of the significant variation in manipulation strategies, the general lack of data on manipulation and the inability to observe key variables, such as true asset values, and counterfactuals, such as manipulation free markets. By studying manipulation in an experimental market we overcome these limitations and shed important insight into the effects of a particular and common form of manipulation – manipulation of the closing price.

Our first key result comes from contrasting the particular incentives given to manipulators in our experimental market with those in the closely related study by Hanson et al. (2006). We find that the manipulators' incentives are critical in determining the degree of harm caused by a particular type of manipulation. Consequently, different types of manipulation should be considered separately in formulating policy decisions or in conducting academic research.

Our second key finding is that closing price manipulation harms both price accuracy and liquidity. In fact, even the mere possibility of manipulation decreases liquidity and increases trading costs through increased price uncertainty. Therefore, in line with the argument put forward by Kyle and Viswanathan (2008), closing price manipulation creates social harm and should be prohibited. Our findings, specifically about closing price manipulation, are particularly concerning given the many examples of market participants with incentives to manipulate closing prices and their numerous important uses.

A third important result is that price accuracy can be restored by imposing a credible mechanism that monitors the market and issues penalties to detected manipulators. However, the restoration of liquidity through the imposition of penalties for manipulation is more difficult. The decrease in price accuracy caused by manipulation is largely an ex-post effect resulting directly from the manipulators' actions, whereas the decrease in liquidity is an ex-ante effect caused by ordinary traders' reactions to the perceived probability of manipulation. Whilst regulation may have an immediate impact on the behavior of manipulators and therefore help restore price accuracy, changing the behavior of ordinary traders to restore liquidity requires that market participants believe regulation will eliminate manipulation. This was not the case in our experimental markets; regulation restored price accuracy but not liquidity. Our conclusion is consistent with Bhattacharya and Daouk (2002) who find that the perception of credibility gained by a regulator through the enforcement of laws governing financial conduct, rather than simply their presence, affects markets in a positive way.

Our last significant contribution is in characterizing a typical closing price manipulation strategy and the reactions of ordinary traders. Manipulators of a stock with a reasonable level of liquidity, in the absence of a credible regulator, submit many highly aggressive buy orders in the last seconds of trading. In the presence of a regulator, manipulators trade less aggressively and earlier in a trading period, trading off some of the benefits they stand to gain from manipulation, against the probability of being caught. We find some evidence that ordinary traders attempt to profit from manipulators by offering more shares for sale shortly before the close when they perceive manipulation to be likely. Such a strategy, motivated by self-interest, offers hope to markets for attenuating the detrimental effects of manipulation and minimizing the need for regulatory intervention. However, in order for ordinary traders to successfully counter manipulation, they must first be capable of identifying manipulation. In our experimental market, despite the fact that manipulators have a substantial impact on prices, market participants have great difficulty in identifying manipulation. This concerning result suggests the need for regulatory intervention, as opposed to leaving markets to their own devices, particularly in light of the finding that closing price manipulation imposes a social cost. Further, this also suggests that regulators need more advanced monitoring mechanisms than human judgment in order to detect a meaningful fraction of manipulation.

References

Aggarwal, Rajesh K., and Guojun Wu, 2006, Stock market manipulations, *Journal of Business* 79, 1915-1953.

Allen, Franklin, and Douglas Gale, 1992, Stock price manipulation, *The Review of Financial Studies* 5, 503-529.

Barclay, Michael J., Eugene Kandel, and Leslie M. Marx, 1998, The effects of transaction costs on stock prices and trading volume, *Journal of Financial Intermediation* 7, 130-150.

Bernhardt, Dan, and Ryan J. Davies, 2005, Painting the tape: Aggregate evidence, *Economics Letters* 89, 306-311.

Bhattacharya, Utpal, and Hazem Daouk, 2002, The world price of insider trading, *Journal of Finance* 57, 75–108.

Bloomfield, Robert, and Maureen O'Hara, 1999, Market transparency: Who wins and who loses?, *Review of Financial Studies* 12, 5-35.

Bloomfield, Robert, Maureen O'Hara, and Gideon Saar, 2005, The "make or take" decision in an electronic market: Evidence on the evolution of liquidity, *Journal of Financial Economics* 75, 165–199.

Cai, Charlie X., Robert Hudson, and Kevin Keasey, 2004, Intra day bid-ask spreads, trading volume and volatility: Recent empirical evidence on the London Stock Exchange, *Journal of Business Finance & Accounting* 31, 647–676.

Camerer, Colin F., 1998, Can asset markets be manipulated? A field experiment with racetrack betting, *Journal of Political Economy* 106, 457–481.

Carhart, Mark, Ron Kaniel, David Musto, and Adam Reed, 2002, Leaning for the tape: Evidence of gaming behavior in equity mutual funds, *Journal of Finance* 57, 661–693.

Cumming, Douglas, and Sofia Johan, 2007, Exchange surveillance index, Unpublished working paper.

Hanson, Robin, and Ryan Oprea, 2008, Manipulators increase information market accuracy, *Economica* (forthcoming).

Hanson, Robin, Ryan Oprea, and David Porter, 2006, Information aggregation in an experimental market, *Journal of Economic Behavior & Organization* 60, 449-459.

Hillion, Pierre, and Matti Suominen, 2004, The manipulation of closing prices, *Journal of Financial Markets* 7, 351–375.

Kumar, Praveen, and Duane J. Seppi, 1992, Futures manipulation with cash settlement, *Journal of Finance* 47, 1485–1502.

Khwaja, Asim I., and Atif Mian, 2005, Unchecked intermediaries: Price manipulation in an emerging stock market, *Journal of Financial Economics* 78, 203-241.

Kumar, Praveen, and Duane J. Seppi, 1992, Futures manipulation with cash settlement, *Journal of Finance* 47, 1485–1502.

Kyle, Albert S., and S. Viswanathan, 2008, How to define illegal price manipulation, *American Economic Review Papers and Proceedings* (forthcoming).

Ni, Sophie X., Neil D. Pearson, and Allen M. Poteshman, 2005, Stock price clustering on option expiration dates, *Journal of Financial Economics* 78, 49-87.

Plott, Charles R., and Shyam Sunder, 1988, Rational expectations and the aggregation of diverse information in laboratory security markets, *Econometrica* 56, 1085-1118.

Table 1
Summary of trader payoffs at the end of each period by treatment

This table summarizes the payoffs earned by manipulators and ordinary traders (all other traders) at the end of each trading period. N and C are the number of shares and amount of cash respectively, owned at the end of the period. $V \in \{20,40,80\}$ is the payoff of each share at the end of a period. $P_{closing}$ and P_{median} are the last and median trade prices respectively in a trading period. In Treatment 3 manipulation (defined as a manipulator choosing to trade) is "detected" if at least eight of the other 11 traders guess that the manipulator traded and "not detected" otherwise. Ordinary traders guess whether or not a manipulator was present and manipulators guess how many of the ordinary traders will guess that a manipulator was present. All amounts are denominated in experimental currency units.

Treatment	Trader type	Trading payoff	Guessing payoff
1	Ordinary	$NV + C$	
2	Ordinary	$NV + C$	+50 if correct, -50 if incorrect
	Manipulator	$15(P_{closing} - P_{median}) + 250$	+50 if correct, -50 if incorrect
3	Ordinary	$NV + C$	+50 if correct, -50 if incorrect
	Manipulator	$\left\{ \begin{array}{rl} 15(P_{closing} - P_{median}) + 250 & \text{if not detected} \\ -15(P_{closing} - P_{median}) + 250 & \text{if detected} \\ 250 & \text{if no trade} \end{array} \right\}$	+50 if correct,-50 if incorrect

Table 2

Asset values, clues and manipulator allocations

V is the payoff in experimental currency for each share of the asset at the end of a trading period. The clue given to each subject is knowledge of one of the three possible values that *V* will certainly not take in that period. For example, Subject 1 in Period 1 is told $V \neq 20$. For each period of each treatment Panel B describes which subject, if any, is assigned the role of manipulator (given a different payoff schedule as described in Table 1).

Panel A: Asset values and clues

	Practice	Period 1	Period 2	Period 3	Period 4	Period 5	Period 6	Period 7	Period 8	Period 9	Period 10	Period 11	Period 12	Period 13	Period 14	Period 15	Period 16
V	40	40	20	80	80	40	20	40	80	20	80	40	20	20	40	80	20
Subject 1 clue	20	20	80	40	20	80	40	20	20	80	40	80	40	80	20	40	80
Subject 2 clue	80	80	40	20	40	80	80	20	40	80	20	80	80	40	80	20	40
Subject 3 clue	20	80	80	40	40	80	40	80	20	40	40	20	40	80	80	20	80
Subject 4 clue	80	20	80	20	20	80	80	80	40	40	20	20	80	40	20	40	40
Subject 5 clue	80	20	40	40	20	80	40	80	20	80	20	20	80	40	20	40	80
Subject 6 clue	80	80	40	20	40	20	80	20	40	40	20	80	40	80	80	20	40
Subject 7 clue	20	20	80	20	20	20	80	20	40	40	40	80	40	40	80	20	80
Subject 8 clue	80	80	80	20	40	20	80	80	20	40	40	80	40	80	80	40	40
Subject 9 clue	20	80	40	40	20	80	40	20	40	40	40	20	40	80	20	20	80
Subject 10 clue	20	80	40	40	20	20	40	80	20	80	40	20	80	40	20	40	40
Subject 11 clue	20	20	40	40	40	20	40	80	20	80	20	20	80	80	20	20	80
Subject 12 clue	80	20	80	20	40	20	80	20	40	80	20	80	80	40	80	40	40

Panel B: Manipulator allocations

Treatment	Practice	Period 1	Period 2	Period 3	Period 4	Period 5	Period 6	Period 7	Period 8	Period 9	Period 10	Period 11	Period 12	Period 13	Period 14	Period 15	Period 16
1	None	None	None	None	None	None	None	None	None	None	None	None	None	None	None	None	None
2	None	None	Subject 5	Subject 2	None	Subject 7	None	None	Subject 4	Subject 1	None	Subject 6	None	None	Subject 8	None	Subject 3
3	None	Subject 10	Subject 4	Subject 7	Subject 9	Subject 1	Subject 11	Subject 2	Subject 6	Subject 8	Subject 3	Subject 12	Subject 5	Subject 1	Subject 3	Subject 2	Subject 4

Table 3
Effect of manipulation on end of period price accuracy

Estimates from a linear mixed effects model with random intercepts and slopes. *Price* and *Squared error* are the dependent variables. *Price* is the average of the last three trade prices in a trading period. *Squared error* is the square of the difference between *Price* and the fundamental asset value. *Possible*, *Manipulation* and *Regulator* are indicator variables that take the value of 1 if the trading period is under Treatment 2a, 2b or 3 respectively. $V \in \{20,40,80\}$ is the fundamental asset value. Significance at the 10%, 5% and 1% levels is indicated by *, ** and *** respectively and t-statistics are shown in parentheses.

Covariate	Price	Price	Squared error	Squared error
Intercept	25.84*** (3.98)	25.84*** (7.46)	374.43** (2.02)	374.43*** (3.23)
Manipulation	19.67* (1.77)	19.98** (2.12)	283.43 (0.76)	273.52 (0.80)
Possible		-0.27 (-0.06)		-106.50 (-0.67)
Regulator		-1.50 (-0.31)		-81.53 (-0.50)
V	0.36* (1.84)	0.36*** (3.97)		
Manipulation x V	-0.33 (-1.29)	-0.33* (-1.89)		
Possible x V		0.14 (1.21)		
Regulator x V		0.05 (0.43)		

Table 4

Effect of manipulation on price accuracy within a trading period

Estimates from a linear mixed effects model with random intercepts and slopes. The dependent variable is the absolute difference between the price of the last trade and the fundamental asset value at the end of each ten-second interval within a trading period. *Possible*, *Manipulation* and *Regulator* are indicator variables that take the value of 1 if the trading period is under Treatment 2a, 2b or 3 respectively. *V20* and *V80* are indicator variables that take the value of 1 if the fundamental asset value in that trading period is 20 or 80 respectively and *Period* is the trading period number within the experimental session, which takes values from 1 to 16. *Interval* is the number of the ten-second interval within a trading period, which takes values from 0 to 19. *Last* is an indicator variable which takes the value of 1 for the last interval of the trading period. Significance at the 10%, 5% and 1% levels is indicated by *, ** and *** respectively.

Covariate	Estimate	t-statistic
Intercept	9.64***	4.73
Possible	1.84	0.85
Manipulation	4.2*	1.88
Regulator	0.99	0.47
V20	14.2***	8.86
V80	20.59***	9.59
Period	-0.3**	-2.06
Interval	-0.85***	-5.02
Interval^2	0.02***	3.27
Last	-0.11	-0.06
Last x Possible	-1.82	-0.81
Last x Manipulation	5.67	1.62
Last x Regulator	-0.4	-0.16

Table 5
Effect of manipulation on liquidity

Estimates from a linear mixed effects model with random intercepts and slopes. *Bid-ask spread*, *Depth* and *Volume* are the dependent variables. *Bid-ask spread* is the difference between the best ask and best bid prices divided by the midquote (average of the best bid and best ask) expressed as a percentage and averaged across the ten-second intervals within a trading period. *Depth* is the average of the number of shares demanded or offered at the best three bid and ask quotes averaged across the ten-second intervals within a trading period. *Volume* is the number of shares traded in a trading period. *Possible*, *Manipulation* and *Regulator* are indicator variables that take the value of 1 if the trading period is under Treatment 2a, 2b or 3 respectively. *V20* and *V80* are indicator variables that take the value of 1 if the fundamental asset value in that trading period is 20 or 80 respectively and *Period* is the period number within the experimental session, which takes values from 1 to 16. Significance at the 10%, 5% and 1% levels is indicated by *, ** and *** respectively and t-statistics are shown in parentheses.

Covariate	Bid-ask spread	Depth	Volume
Intercept	20.54***	2.84***	31.03***
	(5.26)	(4.56)	(7.36)
Possible	8.83**	0.02	-12.22**
	(2.36)	(0.03)	(-2.45)
Manipulation	9.61**	0.29	-2.92
	(2.34)	(0.38)	(-0.58)
Regulator	9.60***	-0.29	-5.34
	(2.68)	(-0.33)	(-0.49)
V20	18.90***	0.09	9.62***
	(5.67)	(0.54)	(3.47)
V80	14.74***	-0.15	12.68***
	(4.39)	(-0.75)	(4.21)
Period	-1.39***	-0.01	0.20
	(-4.68)	(-0.60)	(0.51)

Table 6
Ability of traders to identify manipulation

Two-way frequency tables of state (whether a manipulator was present in the market or not) and traders' guesses of whether a manipulator was present or not. *% Correct* is the percentage of correct guesses. Significance at the 10%, 5% and 1% levels is indicated by *, ** and *** respectively for two-sided binomial proportion tests with the null hypothesis that *% Correct* equals 0.5, i.e. the accuracy of guesses is not different from chance.

Panel A: Without regulator (Treatment 2)

	Guess			
State	No manipulator	Manipulator	Total	% Correct
No manipulator	214	161	375	57.1***
Manipulator	175	168	343	49.0
Total	389	329	718	
% Correct	55.0**	51.1		**53.2***

Panel B: With regulator (Treatment 3)

	Guess			
State	No manipulator	Manipulator	Total	% Correct
No manipulator	30	42	72	41.7
Manipulator	92	169	261	64.8***
Total	122	211	333	
% Correct	24.6***	80.1***		**59.8*****

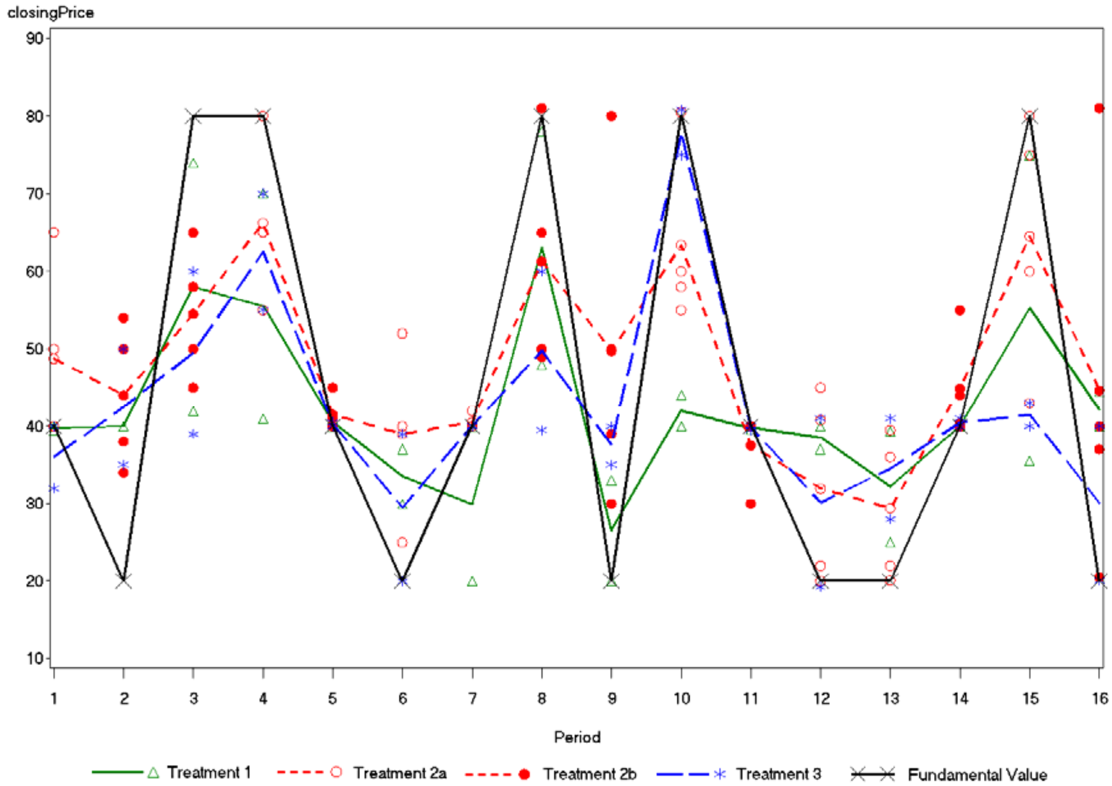


Figure 1. End of period prices by period. This figure shows the prices of the last trade in each period of each experimental session (the various shaped and colored points) as well as the average of these prices in each period by treatment (lines). The solid black line shows the fundamental asset value in each period.

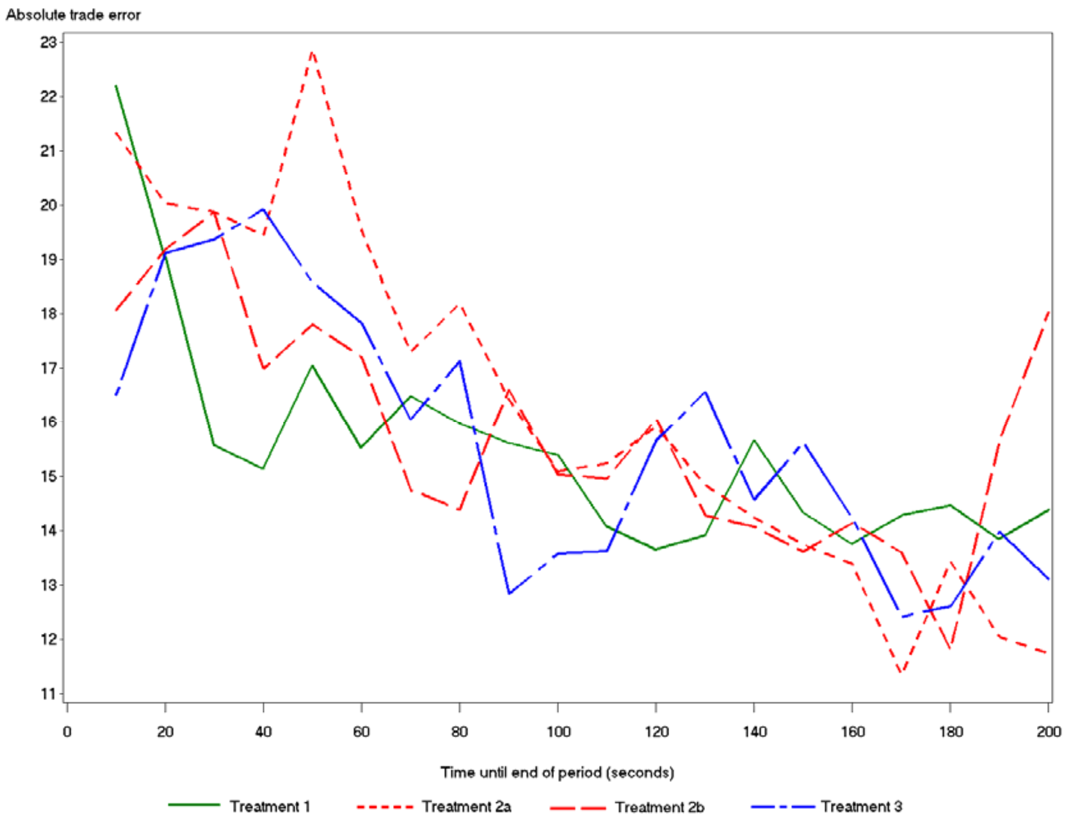
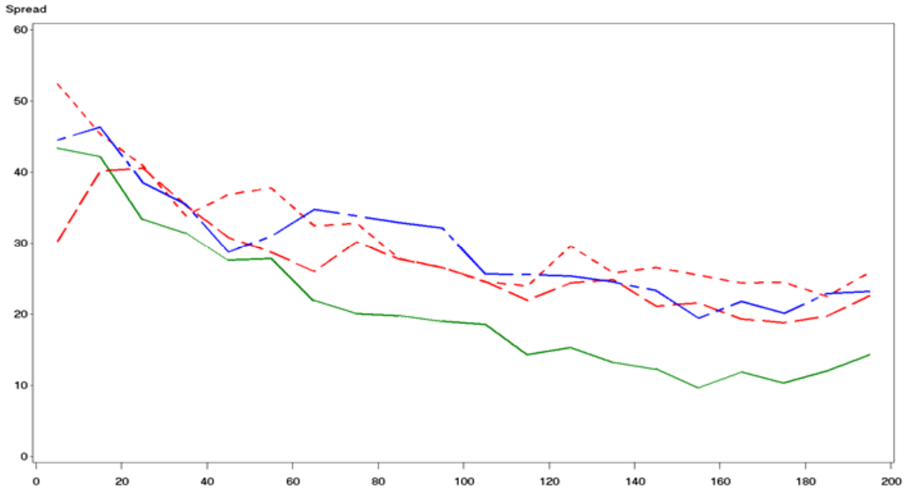
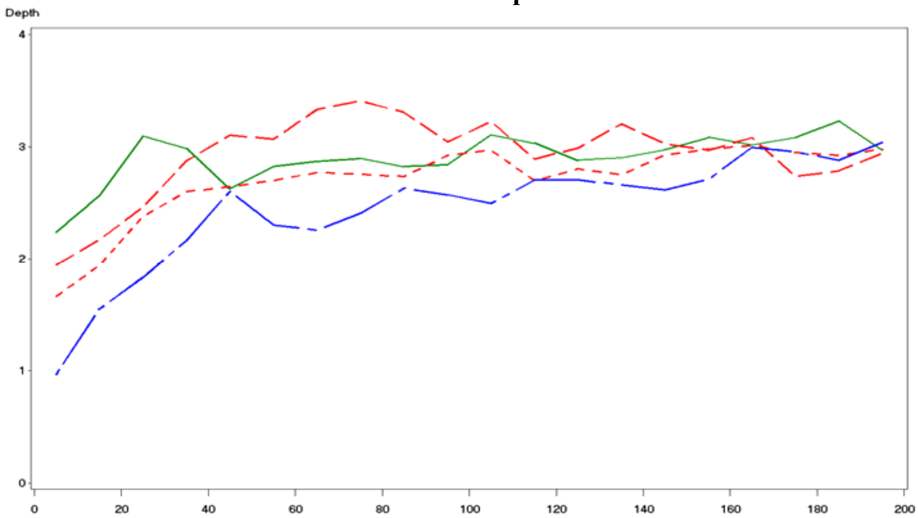


Figure 2. Average absolute pricing errors within a trading period. This figure shows the average (by treatment) of the absolute pricing error at the end of each ten-second interval within a trading period. Absolute pricing error is calculated as the absolute difference between the price of the trade immediately prior to the end of a ten-second interval and the fundamental asset value.

Panel A: Bid-ask spread



Panel B: Depth



Panel C: Volume

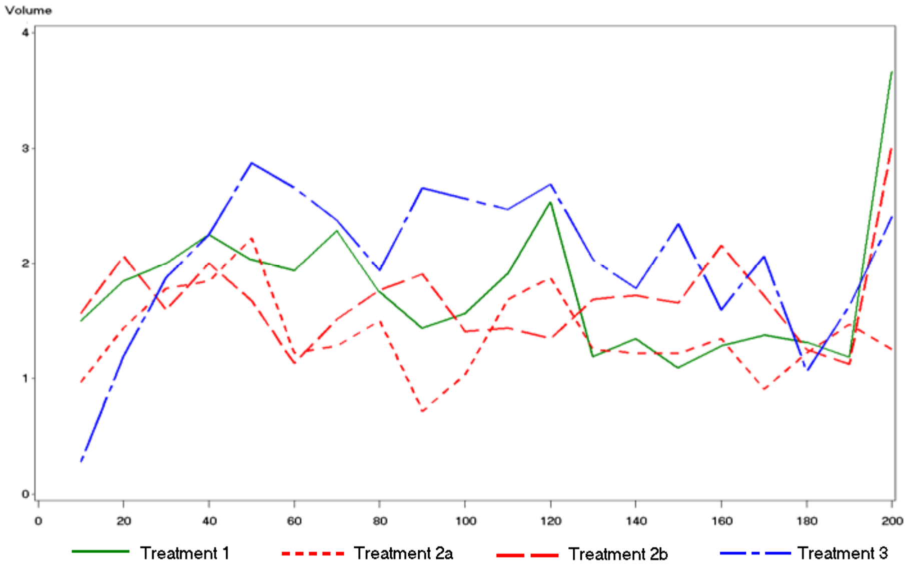


Figure 3. Evolution of liquidity variables. This figure shows average bid-ask spread (difference between the best bid and best ask as a percentage of the midquote), depth (average the number of shares demanded or offered at the best three bid and ask prices) and volume (number of shares traded in each ten-second interval) within a trading period for each of the treatments. The horizontal axis measures time (in seconds).

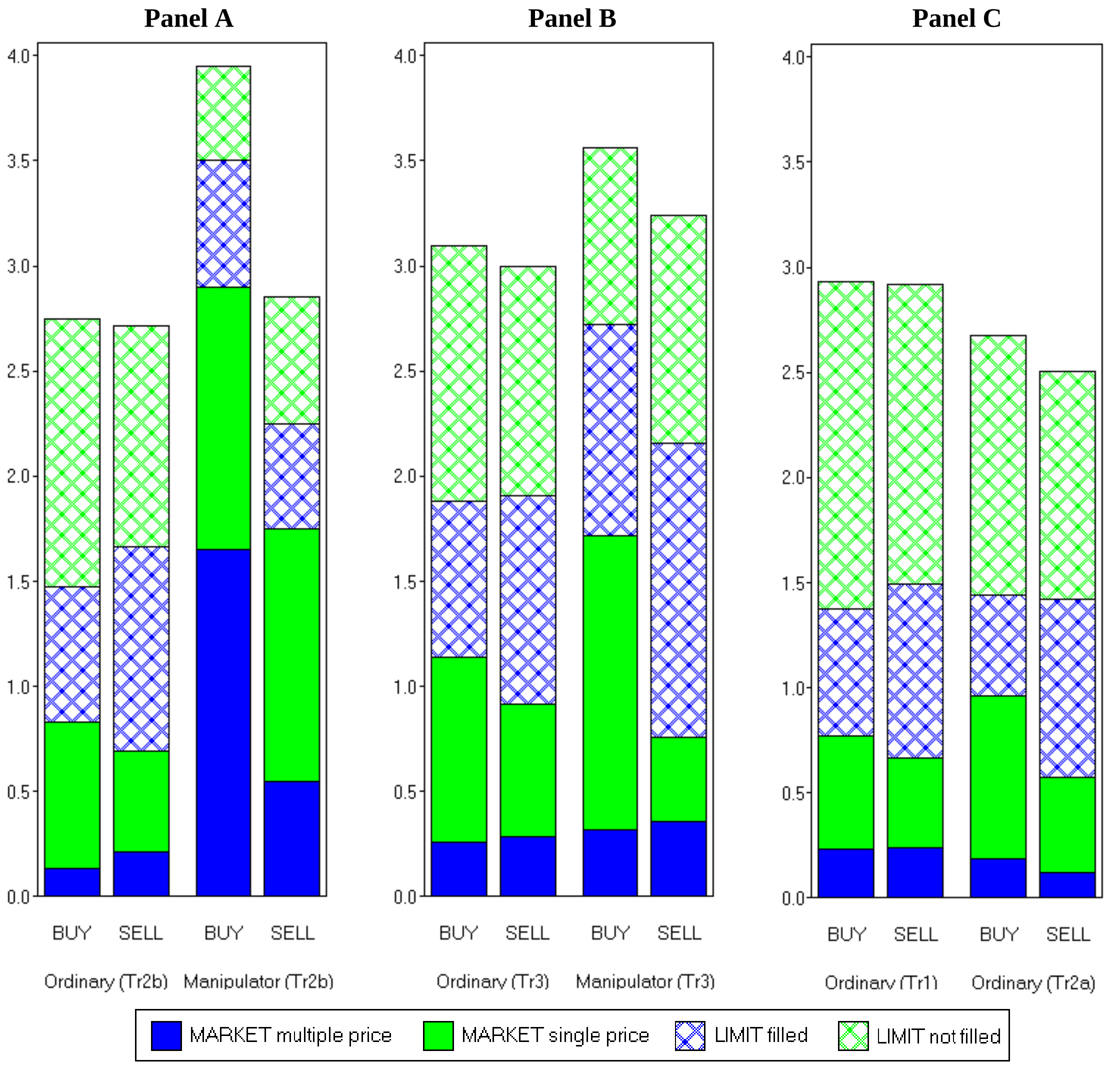
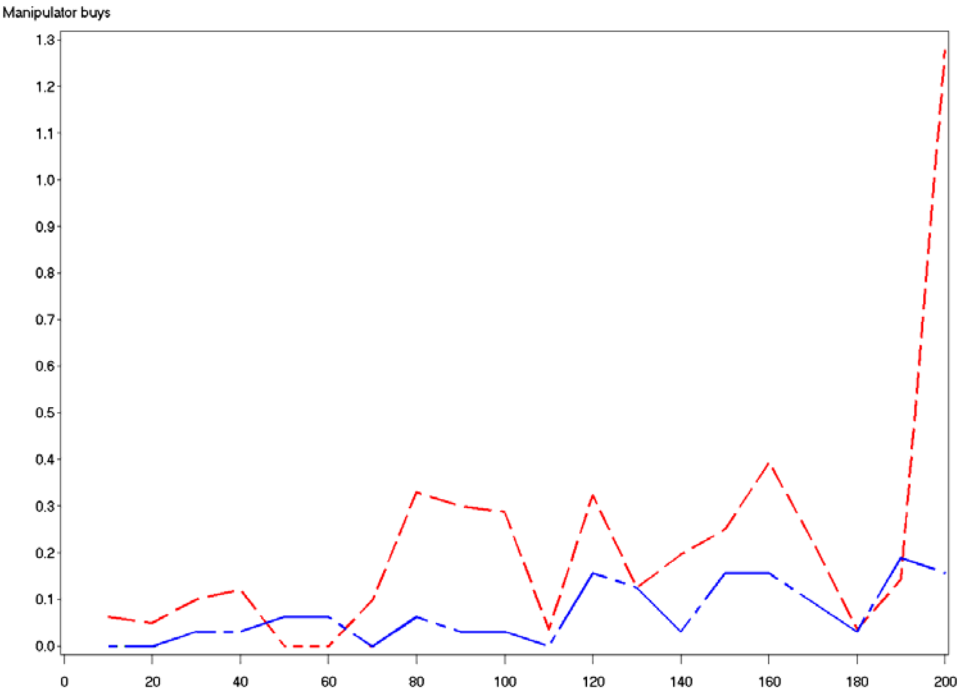


Figure 4. Order types used by manipulators and ordinary traders by treatment. This figure shows the average number of various types of order per trader per trading period. Panel A compares the orders of non-manipulators (*Ordinary*) with those of manipulators (*Manipulator*) in Treatment 2b (manipulation without a regulator). Panel B compares the orders of non-manipulators with those of manipulators in Treatment 3 (possible manipulation with a regulator). Panel C compares the orders of non-manipulators in Treatments 1 and 2a (control and possible manipulation). *MARKET multiple price* and *MARKET single price* are orders that execute instantaneously (either market orders or marketable limit orders) at more than one price level (cause price impact) and only one price level respectively. *LIMIT filled* and *LIMIT not filled* are limit orders that are at least part filled and not at all filled respectively. For Treatment 3 we have only included trading periods in which the manipulator chose to trade to allow fair comparison between manipulators and other traders.

Panel A: Buys



Panel B: Sells

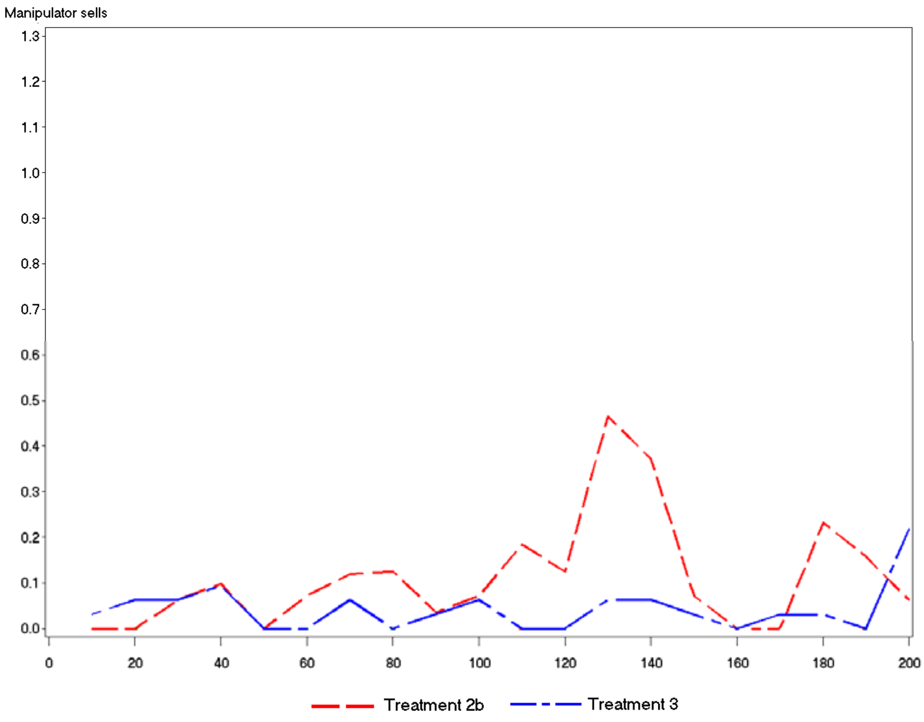


Figure 5. Manipulator buying and selling activity within a trading period. This figure shows the average number (by treatment) of buys (Panel A) and sells (Panel B) initiated by the manipulator in each ten-second interval within a trading period. The horizontal axis measures time (in seconds). For Treatment 3 we have only included trading periods in which the manipulator chose to manipulate to allow fair comparison across the two treatments.