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Working Paper 2014-03

September 16, 2014

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Palan, S., Stöckl, T., "When chasing the offender hurts the victim: The case of insider legislation", Journal of Financial Markets 35, 2017, 104-129, DOI: 10.1016/j.finmar.2016.07.002 .

Working Paper Series
Faculty of Social and Economic Sciences
Karl-Franzens-University Graz
ISSN 2304-7658
sowi.uni-graz.at/de/forschen/working-paper-series/
sowi-wp@uni-graz.at

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Working Paper Series
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Stefan Palan[†] Thomas Stöckl[‡]

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Backers and opponents argue over the pros and cons of legislation forbidding insider trading. At the same time, the lack of reliable empirical data caused by the currently prevailing legal systems inhibits an exhaustive scientific evaluation. We circumvent this problem by resorting to laboratory market data and show that insider legislation has significant *negative* effects on multiple market dimensions: Under insider legislation, (i) the liquidity and informational efficiency of markets are reduced, while spreads are unaffected, (ii) uninformed traders' losses (before redistribution) are higher due to deteriorating market quality, and (iii) overall welfare suffers due to the lower information content of prices and the cost of enforcement. In summary, insider legislation leads to welfare losses while failing to deliver the expected benefits for uninformed investors.

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*We thank Jason Shachat, Jürgen Huber, Michael Kirchler, seminar participants at the Universities of Zürich, Vienna, Trento, Graz, Mannheim, Sussex, BI Norway Business School, and conference participants at the Experimental Finance 2014 (Zürich) and 9th Nordic Conference on Behavioral and Experimental Economics 2014 (Aarhus) for helpful comments. Financial support by the University of Innsbruck (Nachwuchsförderung Stöckl), the Research Platform "Empirical and Experimental Economics" (eeecon.uibk.ac.at) and UniCredit (Modigliani Research Grant, 4th edition) is gratefully acknowledged.

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1 Introduction

Ever since the emergence of insider trading regulation (see Bhattacharya and Daouk (2002) for a historical review) backers and opponents have argued over the pros and cons and consequently, whether insider trading should be regulated or not.[1] In a recent review, Bainbridge (forthcoming 2013) notes that advocates of strict regulation highlight the negative effects of insider trading on investor confidence and the potential harm generated for the owners of the involved firm. Opponents of regulation argue that insider trading helps increase price efficiency because it moves prices in the direction of where the price would be if more information were public. In this view, earnings gained by corporate insiders are justified as legitimate compensation for generating and revealing relevant new information about the firm. Following this argument, insiders' profits are the price society pays for obtaining the beneficial effects of enhanced price efficiency.

From a scientific perspective, studies on insider trading legislation, its consequences on trader behavior, and the likely effects on markets are complicated by two major obstacles. First, most currently prevailing legal systems prohibit (illegal) insider trading and thus preclude empirical analyses due to a lack of data (see Meulbroek, 1992, for a rare counterexample). Second, the limit order markets in place at most major exchanges offer a large action space to the traders operating in them. Solving traders' problem of optimizing their actions in such a continuous time environment has so far proven impossible for the economics and finance disciplines. These features make it difficult to study limit order markets and their inherent dynamics using theoretical models.[2]

To avoid the caveats restricting empirical and theoretical research we study the effects of insider trading legislation using data from laboratory asset markets.[3] In a laboratory experimental setting, we are not only able to observe every aspect of traders' behavior, but we also control the market environment.

[1] The U.S. Securities and Exchange Commission defines illegal insider trading as trading in securities, in breach of a fiduciary duty or other relationship of trust and confidence, while in possession of material non-public information about the security.

[2] See Glosten (1994); Hollifield et al. (2006) on the advantages of, and Parlour and Seppi (2008); Gould et al. (2013) for recent surveys on, limit order markets. Plott and Sunder (1982); Smith (1982); Friedman et al. (1984) provide early experimental investigations.

[3] See Noussair and Tucker (2013) and Palan (2013) for recent reviews of experimental research on asset markets.

Varying only the variable of interest, i.e. whether or not there are rules against insider trading, we can isolate the effects of insider legislation on trader behavior, aggregate market outcomes and overall welfare.

Our analyses yield a rigorous evaluation of three research questions (RQ). In RQ 1 we explore whether and how informed traders adapt their trading behavior in response to insider trading legislation. There is ample evidence that insiders exhibit abnormally high trading activity (Easley and O'Hara, 1987; Meulbroek, 1992) and that they are active in limit and market orders alike (Chakravarty and Holden, 1995; Harris, 1998; Barner et al., 2005; Bloomfield et al., 2005; Kaniel and Liu, 2006; Goettler et al., 2009; Roşu, 2013; Stöckl, 2014). So far, however, there is no evidence on how these results change in markets with insider legislation. Imposing (potential) prosecution on insiders changes their economic environment, implying a behavioral response. Economically speaking, prosecution and fines increase insiders' cost of trading. (See Becker, 1968 for a theoretical analysis of crime and punishment and Cumming et al., 2013 for related empirical work.) This increase in marginal cost has at least two consequences. First, insiders will refrain from conducting transactions for which the marginal benefits are lower than the marginal costs, causing an overall decrease in market liquidity. Second, in order to camouflage their presence and avoid prosecution, insiders are expected to adapt their trading behavior (Medrano and Vives, 2001; Schnitzlein, 2002; Chakraborty and Yilmaz, 2004; Hornung et al., 2014).

Based on these considerations RQ 2 assesses the effects of insider trading legislation on two aspects of market quality, namely the informational efficiency of prices and bid-ask spreads. The analysis of price efficiency lies at the heart of economic reasoning against any sort of insider trading legislation. According to the argument, prosecution and punishment will hamper price adjustment processes toward the asset's fundamental or intrinsic value, as insiders cannot fully exploit their informational advantage. Consequently, insider trading legislation results in lower levels of price efficiency (Bhattacharya and Daouk, 2002, 2009; Bris, 2003; Beny, 2006; Fishman and Hagerty, 1992; Durnev and Nain, 2007; Stöckl, 2014; Holden and Subrahmanyam, 1992; Kyle, 1985; Leland, 1992; Cumming et al., 2011; Huber et al., 2011). The second aspect of markets assumed to be affected by insider trading legislation is bid-ask spreads. However, there is mixed evidence on the likely effects, with both greater and smaller spreads finding support in some papers in the literature (Cornell and Sirri, 1992; Easley et al., 1996; Cheng et al., 2006; Cumming et al., 2011).

In RQ 3 we elaborate on the effects of insider trading legislation on traders' profits. The complex interaction between insiders' behavioral adjustments and the resulting effects on market quality make it far from obvious how insider trading legislation will affect traders' profits. This analysis lies at the heart of economic reasoning in favor of insider trading legislation which states that it undermines investor confidence in the fairness and integrity of the securities markets (see e.g. Seyhun, 1986; Heilmann et al., 2001; Schredelseker, 2001; Jeng et al., 2003; Beny, 2005; Huber et al., 2008; Hauser and Schredelseker, 2014).

Our results show that insider trading legislation has significant *negative* effects on multiple market dimensions: Under insider legislation, (i) the liquidity and informational efficiency of markets are reduced, while spreads are unaffected, (ii) uninformed traders' losses (before redistribution) are higher due to deteriorating market quality, and (iii) overall welfare suffers due to the lower informational content of prices and the cost of enforcement.

2 The Experiment

In each experimental session, twelve subjects form a cohort and interact in a sequence of ten independent periods. Ten subjects are potential traders while two are market observers. Subjects' assignment to one of the two roles is randomly determined at the beginning of the session and is retained until its end.

2.1 Traders

Traders interact in a multi-unit continuous double auction with open order books, frequently referred to as a limit order market.[4] They are free to choose a trading channel, i.e. they can submit limit orders or alternatively market orders. The latter equates to accepting outstanding limit orders by other traders.[5]

[4]See Appendix A for a screenshot of the trading environment.

[5]These two order types - limit and market orders - have distinct features, and traders face the following trade-off: limit orders provide better conditions in terms of prices, yet execution

There are no restrictions on the size of limit orders, and the partial execution of limit orders is possible. The order book is empty at the beginning of trading and limit orders are executed according to price and then time priority. It provides information about prices and quantities of outstanding orders, yet does not allow market participants to identify the originator of orders other than themselves. The trading protocol and the experimental implementation guarantee full anonymity in this sense. Posted limit orders can be canceled for free at any time. Shorting stocks and borrowing money is not allowed. No interest is paid on Taler holdings (the experimental currency) and there are no transaction costs. Each market (trading period) lasts 240 seconds.

Markets are made up of n traders, where n is an integer in the range from 2 to 9 inclusive. The asset has a life of one period and is bought back by the experimenter at the end of the market at the buyback value (BBV). The BBV is drawn from a discrete uniform distribution over $U(30, 85)$ (with one decimal place) before the beginning of each period. Between 0 and n traders are (costlessly) informed of the asset's exact BBV in each period, while uninformed traders know only its ex-ante distribution.[6] Their best estimate for the BBV thus is its unconditional expected value of 57.5 Taler.[7]

The number and identity of uninformed and informed traders varies with every period. If possible, we conduct two concurrent but independent markets in a period to optimally utilize our 10 (potential) traders. For instance we pair a market populated by four with a market of six traders and conduct both markets within the same period. Table 1 gives an overview of the number of observations for each market type (defined as a specific combination of the number of uninformed and the number of informed traders). The sequences and combinations of markets within the sessions were randomly drawn prior to conducting the sessions in order to ensure the predetermined number of observations per cell listed in Table 1. The variation allows us to separate competition effects from

is uncertain as limit orders require acceptance by another trader to trigger a transaction. Market orders, on the other hand, offer immediate execution, yet at less favorable prices.

[6] Implementing a charge for receiving information would mimic potential information cost in real world settings. We nonetheless set information costs to zero as the main target of this paper is the analysis of insider trading legislation effects. See Huber et al. (2011) on the effects of information costs in an asymmetric information setting.

[7] Our information structure constitutes a minor deviation from those parts of the literature where insiders receive slightly different signals. However, the expected implication of noisy signals would simply be a reduction in effect size.

size effects in the results. The range of possible market sizes and insider numbers is public knowledge, but the probabilities associated with each potential combination of traders is not. Traders also do not receive explicit information regarding the actual market size during the experiment.[8] Each trader in a market is endowed with 60 assets and 4800 Taler. Taler and asset holdings are reset at the beginning of each period. The cash/asset-ratio is in the range of 0.94 to 2.67.[9]

Traders' earnings are based on the total Taler earnings generated from three sources. First, the most important source of traders' income is the sum of their final wealth positions determined at the end of each market (with asset holdings evaluated at the BBV):

$$Wealth_p = \# \text{ shares} * \text{BBV} + Taler \tag{1}$$

Second, we run an incentivized questionnaire immediately after the end of each trading period, eliciting traders' beliefs about the total number of traders in the market (trader question T1) and about the number of informed traders (trader question T2). Traders are awarded 500 Taler for each correct answer. Third, if it is not possible to assign all ten traders to a market, inactive traders are asked to solve as many multiplications of a two-digit number by a one-digit number as possible in the time during which the other subjects trade (example for a calculation: $37 * 3 = 111$; mean/maximum number of correct calculations: 21/45). They earn a fixed amount of 350 Taler for each correctly solved calculation. The calculation task is run to keep subjects busy who, for reasons of experimental control, do not participate in the market experiment.

Traders' payoffs in EUR are determined by their cumulative Taler earnings from all three sources, reduced by a fixed amount (66.000 Taler) and converted

[8]Subjects are informed of the market size and the number of insiders in two training periods we run prior to the start of the actual markets. Furthermore, they may be able to infer the number of market participants indirectly from observing market activity.

[9]These cash/asset-ratios ensure that traders are able to make transactions at reasonable frequencies and prices but they are also reasonably low in order to avoid biasing our results by cash endowment effects. See Kirchler et al. (2012), Noussair and Tucker (2014) and references therein on the effects of cash endowments on mispricing.

into Euros at an exchange rate of 900 Taler = 1 EUR:[10]

$$\text{Payoff in EUR} = \frac{\sum_{p=1}^{10}(Wealth_p) - 66,000}{900}$$

At the end of each period subjects see a table containing information about the previous periods including their earnings.

The trading screen provides traders with current information on their asset and Taler holdings and the current wealth (assets evaluated at the *latest* transaction price). A separate box, shown only to insiders, informs them about the BBV. Transaction prices over time are shown in a real-time chart on the left-hand side of the screen.

2.2 Market observers

Market observers monitor the market and do not actively participate in trading. They receive EUR 15 as a base compensation for their participation in the experiment. Their task is to answer three incentivized questions (observer questions O1-O3) at the end of each market. Earnings (losses) from the questionnaire are added (subtracted) from the base compensation.

The questionnaire asks observers to estimate the number of informed traders (observer question O1) and their identity (observer questions O2 and O3). Questions O1-O3 are incentivized as follows. In O1 observers receive 2 EUR for correct estimates (i.e., when the estimated number of insiders equals the true number). The second and third questions are identical to each other, varying only in the incentive scheme. For each correct answer on question O2 (O3) observers earn 0.5 (1) EUR, while they lose 0.5 (2) EUR for each incorrect guess. The questions thus convey the following incentives: Risk-, loss-. and ambiguity-averse observers should identify a trader as being an insider if their subjective probability estimate of the trader having inside information is greater than 50% in O2, or greater than 75% in O3. See Appendix B for details on the parameter derivation.

Observers provide answers to these questions based on publicly observable data (order book, price chart) and (costless) real-time aggregate information

[10] 66.000 equals the sum over 10 periods of the values of the initial endowment (Taler plus assets) assuming the lowest BBV realization.

about each traders' activity in the market (see Table 2 and the instructions in Appendix A for details). Traders are identified by a unique ID made up of a letter and a number (e.g. I8). IDs change from period to period and each ID is used only once in each session. In cases where we conduct two contemporaneous markets in a period, each observer is assigned to one of the markets. When there is only one market operating, both observers are assigned to it.[11]

2.3 Legal settings

Setting NOLEG is the baseline setting, which is modelled after markets with no insider trading legislation. It is characterized by no interaction between traders and observers. Setting LEG implements an insider trading legislation regime. The insiders identified in observer question O3 by the observer assigned to the market (or a randomly chosen observer in case both observe the same market) are targeted with sanctions. Note that in the remainder of the paper, we will always refer to observers' answers to question O3 when discussing the detection of insiders, since only this answer affects the traders.

Setting LEG comes close to the currently prevailing system in major capital markets, where a central authority monitors markets and punishes (illegal) insider trading. Our legal system is modeled after the U.S. Securities and Exchange Commission (SEC) procedure referred to as the "contemporaneous trader rule." It states that investors who trade in the same instruments and on the same day as an insider are usually entitled to compensation.[12] The insiders' trading profits are redistributed to investors following a pro-rata scheme based on the number of shares the investor bought (if the insider profited by selling) or sold (if the insider profited by buying) on this day, relative to the total number of shares bought or sold (same condition as above) by all claimants.

In our experiment the rule is implemented as follows. Correctly identified

[11] We decided to rely on human subjects in the observer role for two reasons. First, an alternative specification would be to rely on detection algorithms, a procedure for example employed by the SEC (besides being tipped by other traders or involved people). However, market authorities keep their detection algorithms under tight wraps, making it impossible to copy their exact procedures. At the same time, the development of proprietary algorithms would introduce an element of arbitrariness to our study. Second, another possibility would be to use fixed detection probabilities. This would however have made it impossible to investigate insiders' hiding strategies, as given constant probabilities, hiding would not have served any purpose.

[12] If it is "not economically practical or efficient to identify investor claimants and provide them with notice", the disgorged funds are paid to the US Treasury instead.

insiders lose their trading profits, which are redistributed to traders who engaged in unprofitable trades with informed traders. Trading profit in this context is defined as the positive difference between the trader's wealth at the end of the period and the wealth at the beginning of the period (in both cases, BBV is used to evaluate the value of the shares). The entire profit which is deducted from informed traders in this way is distributed to those traders who were not correctly selected as having been informed (even if, in reality, they had been informed). The distribution is based on the number of shares bought or sold at a loss.[13]

2.4 Implementation of the experiment

We conducted 16 sessions each in the NOLEG and in the LEG settings, yielding a total of 560 markets. The number of repetitions for each combination of informed/uninformed traders varies, as was outlined already in Table 1. We decided to put greater weight on markets with fewer traders as these are usually more prone to idiosyncratic variation, thus necessitating a greater sample size in order to derive robust results.

Our experiments were conducted in September, October and December 2013 at the University of Innsbruck with a total of 384 bachelor and master students from different fields. Most subjects had previously participated in other economic experiments but each participated in only one session of this study. The software was programmed with z-Tree 3.3.12 by Fischbacher (2007) and subjects were recruited using ORSEE by Greiner (2004).

At the beginning of each session subjects had 20 minutes to study the written instructions on their own. This was done to eliminate any possible experimenter bias. Afterwards, the trading mechanism and the most important screens were

[13]In line with our approach, civil fines in the US may amount to between one and three times the profit gained or the loss avoided. In addition to this, the SEC levies substantial fines, which are absent in our setting. We decided to limit the level of redistribution to one time the trading profit. Setting potential redistributions higher than the losses incurred could spur strategic behavior by the uninformed traders in an attempt to gain from redistributions. One could think of traders (informed as well as uninformed) deliberately engaging in unprofitable trades with suspected insiders, hoping that expected redistributions would more than outweigh the losses. Such strategic behavior is unlikely to motivate trades in real world markets, but might conceivably have biased our results. Furthermore, given the low fines, we expect our results to be conservative relative to the full possible effect of market authorities' enforcement activities. In other words, we would expect higher penalties to strengthen rather than weaken our results.

explained in detail, followed by two trial markets to allow subjects to become familiar with the experimental procedures and the payment schemes. Role assignment (trader or observer) was determined before the trial periods started and subjects kept their role for the entire session. In the trial periods all ten traders interacted in the same market. In the first trial period, five subjects were chosen as insiders, with the remaining participants remaining uninformed. These assignments were reversed in the second trial period, such that each subject was trained once as an informed and once as an uninformed trader. Every subject received the same instructions. In particular, both traders and observers received instructions for the trader and observer roles. They were informed of their role in the experiment only after they had read the instructions for both, but before commencing the training periods. In the latter they already participated in the role they would remain in for the remainder of the experiment. The information available to subjects in each role, and the incentives they faced, were thus common knowledge. See Appendix A for instructions and screenshots. Each session lasted approximately two hours. In NOLEG traders earned on average EUR 21.01 (s.d. 6.11) and observers earned EUR 21.36 (s.d. 12.10). In LEG traders earned on average EUR 20.03 (s.d. 3.99) and observers earned EUR 16.91 (s.d. 10.47).

3 Results

3.1 Overview of market activity

We start by taking a closer look at the overall level of activity in the markets. In general we observe lively trading in both settings. Still, in 34 (16) out of 280 markets of NOLEG (LEG), we do not observe any transactions. However, these markets are predominantly small, as measured by the number of traders, with 28 (11) in NOLEG (LEG) having a trader population of no more than three. Table 3 gives a descriptive overview about insiders' and uninformed traders' activities in terms of limit order submission and transactions as well as setting aggregate values. Three values are provided in each category. First, the number of LOs posted (trades executed); second, the volume of shares offered to trade (transacted); and third, the average size of the LO (transaction). Values in parentheses are relative shares for uninformed traders and insiders (number and volume) and relative deviations from the setting average (average LO and trade size). The upper panel presents data for setting NOLEG, while the lower panel provides data for setting LEG.

There is no notable difference between legal settings with respect to the total number of limit orders posted and the number of trades executed. However, considerable differences are evident in the volumes of limit orders and trades. A total of 95,931 shares are offered to trade in NOLEG, compared to 73,405 in LEG; a reduction of 23.5%. Remarkably, the reduction is mainly caused by insiders' reduced submission activity, as uninformed traders submit similar numbers in both settings. A similar picture emerges for trades. The volume of shares transacted in LEG is 24.0% lower than in NOLEG, where a total of 43,318 shares are transacted. Thus, on average, about 171 (131) shares are offered to trade, resulting in 39 (29) transacted shares in NOLEG (LEG) in each market. Furthermore, the average volume of a single limit order is smaller in markets of NOLEG compared to LEG. Similar figures are obtained for the average transaction size which is greater in markets of NOLEG than in LEG.

Focusing on the probability of insiders being spotted by observers, we find that an insider's probability of being selected is slightly less than 1 in 4. Interestingly, we do not detect a significant difference between the mean detection probabilities in our two settings (23.7% in NOLEG, 24.3% in LEG; ranging up to maxima of 62.5% and 53.1%, respectively). This suggests that the insiders' hiding strategies fail to affect the observers' ability to discriminate between informed and uninformed trading. We investigate this further by regressing (OLS) the detection probability on a dummy for the setting and on two variables containing the number of insiders and the number of uninformed traders in the market, respectively. We find a highly significant negative coefficient for the number of insiders, but no significant coefficients for the legal setting or the number of uninformed traders. Our interpretation is that it is easier for insiders to hide the more insiders are present in a market.

3.2 RQ 1: Trading behavior

We now turn to RQ 1 where we elaborate on the level of market activity, on traders' behavior in the markets, and on how they are influenced by insider trading legislation. Table 4 contains the list of measures we consider as well as details on how they are calculated.[14] We first consider order submission and trading activity in each market. Submission activity is measured by looking at the number of contracts submitted ($\#CON$) and limit order turnover (LOT). Trading activity is proxied by the number of completed transactions ($\#TRA$) and share turnover (ST). LOT and ST normalize limit order volume and trading volume, respectively, by the number of (active) shares outstanding (ASO) to yield comparable measures across markets of different size. To determine the influence of insider trading legislation we use these measures as dependent variables in multilevel regressions with robust standard errors. Table 5 provides a summary of the abbreviations and definitions for the variables which serve as our regressors. Apart from the dummy variable LEG, measuring the effect of our setting, we use several controls. $SHARE_{IN}$ controls for the relative share of insiders present. BBV controls for a potential influence of the BBV and $DISTANCE$ takes into account the extremity of the BBV realization. $NUMTRADERS$ measures market size, i.e. the number of traders in a market, and determines levels in our hierarchical regressions. Furthermore, we include dummy variables for the 32 cohorts of traders ($COHORT$ 1-31) to control for cohort-fixed effects. We choose multilevel regressions as they capture important features of our unique dataset. We stick to this estimation procedure throughout the paper, if not otherwise noted.

The regression results are reported in Table 6. Columns 2 and 3 present data on submission activity, and columns 4 and 5 on trading activity. We find that insider trading legislation has no significant effect on the number of offers submitted to the market. However, our other measures suffer significantly. Specifically, limit order turnover, the number of transactions and share turnover are significantly reduced. We can thus conclude that markets are less liquid in LEG. We formulate Result 1.

[14]We refrain from indicating the level of aggregation in the Table for better readability but report it in the respective analysis.

> **Result 1**: Markets in setting LEG are significantly less liquid in terms of (i) shares offered to trade (LOT), (ii) the number of transactions ($\#TRA$), and (iii) the number of shares traded (ST).

Our results are in line with Leland (1992), presenting theoretical evidence, and Heilmann et al. (2001), presenting experimental evidence, that liquidity in a market will be reduced when insider trading is permitted. More recently, Cheng et al. (2006) examine the impact of directors' dealings on firm liquidity, finding that depth falls on insider trading days as compared to non-insider trading days. However, our findings contrast with Cornell and Sirri (1992) who analyze insider trades using court records and find that the trades improved liquidity.

Next, we investigate the effect of insider trading legislation on the average sizes of limit orders ($\overline{LO}$) and transactions ($\overline{TRA}$). To get a more detailed picture, we calculate these measures per market and separately for informed and uninformed traders. We already noted considerable differences in these measures when discussing Table 3. The average volume of a single limit order is 14.22 (10.40) shares for insiders (uninformed traders) in NOLEG and 10.10 (9.03) in LEG, respectively. The average transaction size is 7.22 (5.86) for insiders (uninformed traders) in NOLEG and 5.26 (4.78) in LEG, respectively. These results contrast with Hornung et al. (2014) who report that insiders' orders are smaller than uninformed traders' orders in their experimental setting.

The setting differences are confirmed as being significant when $\overline{LO}$ and $\overline{TRA}$ are used as dependent variables in our regression framework. Table 6, columns 6 and 7 (insiders) and columns 8 and 9 (uninformed traders) contain details on the regressions in question. We find a significant negative legal setting effect in all four regressions. However, the reduction in the average order and transaction sizes attributable to setting LEG are more pronounced for insiders. More specifically, the average transaction size for informed traders (uninformed traders) is reduced by 20% (11%) compared to α in setting LEG. The combined results in Table 6 thus imply that the market's capability of executing large orders is reduced when insider legislation is in force. We formulate Result 2.

> **Result 2**: Average limit order sizes and trade volumes are significantly smaller in LEG. This effect is more pronounced for informed than for uninformed traders.

Table 3 also reveals differences in the relative shares of limit order submission and trading activity by informed and uninformed traders (see values in parentheses). These differences hint at changes in submission and trading strategies implemented by the different trader types. We focus our next analyses on this issue. In order to make more precise statements about the trading behavior of traders we have to discriminate between two distinct market situations (Stöckl, 2014). In the first situation ($\mathrm{SIT}_{\mathrm{within}}$) the bid-ask spread brackets the asset's BBV. In other words, the BBV lies *within* the bid-ask spread. This market situation effectively restricts profitable trading options to posting limit orders. Buy (sell) transactions based on market orders generate losses as they are executed at prices above (below) the BBV. In $\mathrm{SIT}_{\mathrm{within}}$ the choice between limit and market orders thus is crucial. In the second situation ($\mathrm{SIT}_{\mathrm{outside}}$) the best ask is below, or the best bid above, the BBV. The BBV thus lies *outside* the bid-ask spread. In this situation limit and market orders can both be profitable and traders are free to choose as long as they are buying (selling) at prices below (above) the BBV. Of course, knowing the asset's precise value, insiders are always able to assess the profitability of transactions and to avoid unprofitable trades. Recall, however, that the common trade-off between the order types in terms of execution risk and price improvement remains. Given these considerations, the prevailing market situation crucially influences the insiders' action space and trading strategies.

Table 7 provides an overview of the market shares of limit orders and trades by situations $\mathrm{SIT}_{\mathrm{within}}$ and $\mathrm{SIT}_{\mathrm{outside}}$, and by legal setting. Values are calculated by determining the number of shares offered to trade (transacted) in each situation at the market level. Market shares are then computed as the number of shares offered to trade (transacted) in a specific situation divided by the total number of LOs (shares transacted) in that market. Note that there are remarkable differences between settings NOLEG and LEG. In NOLEG, 52.92% of all limit orders are submitted in $\mathrm{SIT}_{\mathrm{within}}$, while this is the case in only 41.26% of all cases in setting LEG, a difference which is significant at the 1% level. The same pattern is visible in trades, where 38.77% of all trades occur in $\mathrm{SIT}_{\mathrm{within}}$ in setting NOLEG compared to only 28.18% in setting LEG. Again the difference is significant at the 1% level. The two settings thus differ in the distribution of limit orders submitted and trades executed in outside and within situations; a fact which influences the observed trading behavior and thus needs to be reflected in the analyses. By so doing we ensure that the reported results are not driven by market situations.

To quantify trading strategies we determine the total limit order volume (LO), the volume of trades originating from limit orders (LT), and market order-induced trades (MT) by insiders and uninformed traders conditional on the situation for each market. These figures are then put in relation to the total volume in LOs, LTs, and MTs by both trader groups to calculate the market share insiders and uninformed traders have in limit orders ($\%LO$), limit trades ($\%LO$), and market trades ($\%MT$). Finally, we analyze these measures in our regression framework. In addition to the independent variables implemented so far, we also control for the market situation using a dummy ($WITHIN$), and add an interaction for LEG and $WITHIN$. The regression results are provided in Table 8.

We find two changes in insider behavior caused by the legal setting which are of special interest. First, insiders significantly reduce their market share in submission activity in LEG generally, and even more pronounced in $\text{SIT}_{\text{within}}$ in setting LEG. Second, insiders significantly increase their market share of market orders in $\text{SIT}_{\text{within}}$ in setting LEG. Both changes can be interpreted in light of strategies implemented to avoid detection. The first change shows that insiders try to reduce their presence in the order book. The second change reveals unprofitable trading behavior by insiders, most likely performed in an attempt to hide their presence or mislead markets about the true BBV. This line of argument is supported by verbal comments made by the traders in a questionnaire elicited at the end of the experiment. Question two specifically asked them which strategies they applied in order to evade identification as informed traders by the observers. A large number of answers point at the insiders' implementation of unprofitable trades to camouflage their identity.[15] We formulate Result 3.

[15] 16.1% of all traders in the LEG setting explicitly mentioned engaging in unprofitable trades in an effort to hide their insider status when answering the open question 2. This was the most frequent hiding strategy mentioned. See Appendix C for details on the entire questionnaire.

Result 3: Facing legislation, insiders reduce their market share of limit orders and engage in unprofitable market order-induced trades.

There exists ample evidence that uninformed traders or market observers are able to infer the insiders' information (Plott and Sunder, 1988; Bruguier et al., 2010). So attempts to hide information might already be appropriate in NOLEG in order to avoid strategy copycats. Medrano and Vives (2001), Chakraborty and Yilmaz (2004), and Hornung et al. (2014) provide evidence on informed traders implementing contrarian strategies, i.e. trades in the wrong direction, to mislead other traders or to manipulate markets. Schnitzlein (2002) reports that insiders use the timing and size of trades to hide from dealers and each other. While there exist incentives for insiders to hide in any market, hiding becomes more important when insiders face monetary consequences from authorities as in our setting LEG. The reduction in market shares can be interpreted in this light and is in line with our findings of reduced order and contract size. This behavior is supported by Garfinkel and Nimalendran (2003) who find evidence that it is possible to make inferences on insider status from order size.

To summarize our results regarding RQ1, we find for markets in setting LEG that (i) these markets are significantly less liquid, (ii) average limit order and trade volumes are smaller, and (iii) facing legislation, insiders reduce their market share of limit orders and engage in unprofitable market orders.

3.3 RQ 2: Price performance measures

We next analyze how two price performance measures, namely absolute price deviation (AD), a proxy of informational efficiency, and bid-ask spreads ($SPREAD$), a proxy for transaction costs, are influenced by insider trading legislation. Table 4 outlines details about the calculation of the two measures under the heading "RQ2". To quantify the degree of convergence between prices and the asset's buy-back value (BBV), i.e. the informational efficiency of prices, we calculate AD, the absolute deviation between the (volume weighted) mean price ($\overline{P}$) and the BBV in a market. Note that AD is an inverse measure of price efficiency, as lower values of AD indicate smaller deviations and consequently higher levels of price efficiency. The specific setup of the experiment necessitates the calculation of a benchmark level of AD. We construct such a benchmark by simulating, for every level of the BBV (i.e., realizations between 30 and 85 Taler in steps of 0.1), the average absolute deviation if bids and asks were to be submitted randomly over the entire possible interval. We refer to this as the "random trading benchmark" (RTB) and use it to distinguish price efficiency levels driven by random trading activity from levels where prices reliably reflect inside information.

Our second price performance measure is $SPREAD$, calculated as the (volume weighted) average bid-ask spread as a percentage of the corresponding transaction prices in a market. The spread is determined as the difference between the prevailing best bid and best ask at the time of the transaction in question.[16]

We start the analyses by providing evidence on the development of AD and $SPREAD$ conditional on the share of insiders present in the market. Our results are depicted in Figure 1. The upper panel plots average AD values over the share of insiders present in the market, separately for NOLEG (red crosses) and LEG (black dots). The solid (red) and the dashed (black) lines represent the best fit model for each legal setting based on three different model specifications. We tested linear, quadratic and exponential models with one asymptote. The best fit model was selected based on AIC and BIC. For AD we find that its relationship to the share of insiders present is best characterized by the exponential model, which clearly (weakly) outperforms the linear (quadratic) specification. The figure reveals a strongly negative relationship between the share of insiders and price efficiency, confirming results from earlier work (Huber et al., 2011; Bossaerts et al., 2013; Stöckl, 2014).[17] It is no surprise that the highest values of AD are realized in markets without insider participation. They also constitute the only case where we observe realizations of AD above the random trading benchmark. Visual inspection alone already reveals a marked difference between NOLEG and LEG markets, with the latter exhibiting higher values of AD.

Using the same mode of presentation, the lower panel of Figure 1 plots average $SPREAD$ values over the share of insiders present in the market, separately

[16]Due to very unusual, erratic trading activity which made them clear outliers, we excluded 2 out of our 510 markets from the analyses regarding AD. In the analyses on $SPREAD$ we exclude values above 0.5 (above the 90% percentile, 44 markets) and markets where it was not possible to measure $SPREAD$ at the time of transaction (18 markets).

[17]In line with our results Medrano and Vives (2001) show that the market price approaches, but does not completely converge to, the asset's fundamental value over time.

for NOLEG (red crosses) and LEG (black dots). The solid (red) and the dashed (black) lines again represent the best fit model for each legal setting based on the three different model specifications. For both settings the relationship between $SPREAD$ and the share of insiders present is best characterized by the linear model, which clearly (weakly) outperforms the quadratic (exponential) specification. For NOLEG we observe a significantly (at the 1% level) negative trend as $SPREAD$ decreases with increasing competition among insiders. For LEG the coefficient turns out positive, although not significantly so. Our findings contradict the findings of Kyle (1985) and Glosten and Milgrom (1985) in a market maker environment, where more informed trading creates higher degrees of adverse selection, forcing market makers to increase spreads. In a related study Bacidore and Sofianos (2002) find that non-U.S. stocks exhibit larger spreads compared to U.S. stocks and that the differences are primarily due to higher informational asymmetry and increased adverse selection risk.[18] More recent evidence provided by Roşu (2013) (theoretical) and Hornung et al. (2014) (experimental) shows that a higher share of informed traders generates smaller bid-ask spreads. In our setting this positive effect is mitigated by introducing insider trading legislation.

To provide statistical evidence on the effects of insider legislation on price performance measures, we use AD and $SPREAD$ as dependent variables in our regression framework. Columns 2 and 3 of Table 9 present the results for AD and $SPREAD$, respectively. The figures from the econometric estimation largely support our visual observations. We document that insider trading legislation causes significantly higher values of AD, indicating a negative effect on the informational efficiency of prices. We formulate Result 4.

> **Result 4**: Informational price efficiency is significantly lower in LEG.

This result is in support of earlier work reporting a positive link between insider trading and price efficiency (Manne, 1966; Fishman and Hagerty, 1992; Leland, 1992; Meulbroek, 1992).

The results from our first regression analysis of $SPREAD$ reveal no significant setting effect. To account for the different developments of $SPREAD$

[18] See Dolgopolov (2004) for a review of the adverse selection literature.

conditional on the share of insiders present in both settings we run an additional regression model ($SPREAD$ 2) in which we interact the setting dummy LEG with $SHARE$. We find a small but marginally significant effect of LEG and a significantly positive coefficient on the interaction term, supporting the visual impression from Figure 1 of a significantly negative relationship between spreads and competition in NOLEG, which is absent in LEG. We formulate Result 5.

> **Result 5**: Bid-ask-spreads decrease with competition between insiders in NOLEG. They are constant in LEG.

Our result is supported by Cornell and Sirri (1992), who show that insider purchases did not lead to a widening of the bid-ask spread in Anheuser-Busch's 1982 tender offer for Campbell Taggart. Similarly, Easley et al. (1996) report that spreads for low- and medium-volume stocks do not differ by statistically significant amounts. However, our results contrast with Cheng et al. (2006) who examines the impacts of directors' dealings on firm liquidity. Consistent with the information asymmetry hypothesis, spread widens and depth falls on insider trading days as compared to non-insider trading days.

We test the robustness of our results on two sub-samples of our data. The corresponding results are provided in Appendix D. Both robustness checks support the qualitative conclusions from our main analysis.

3.4 RQ 3: Trader earnings

The complex interaction between insiders' behavioral adjustments and the resulting effects on market quality as detailed in the previous analyses make it difficult to estimate how insider trading legislation will affect traders' profits. However, this analysis lies at the heart of the economic reasoning in favor of insider trading legislation, which argues that insider trading undermines investor confidence in the fairness and integrity of the securities markets. While we are unable to address questions like trust in, or the perceived fairness of, markets, we can analyze the earnings of informed and uninformed traders in our two settings. To determine the effects of insider trading legislation on traders' earnings we calculate period EUR earnings for each trader. We then proceed by calculating average trader earnings in a market and determining individual traders' deviations from this average ($\Delta EARN$). So doing, we control for different levels of BBV which significantly influence period earnings. Finally, we compute the sum of $\Delta EARN$ for insiders and uninformed traders per market.[19] The calculation procedure is repeated twice, once considering EUR earnings before and once after redistribution of identified insiders' profits. The computed values are used as dependent variables in our regression framework. The results are presented in Table 10. We only present results from the insiders' perspective. Due to the zero-sum-property of our measure, the values for the uninformed traders are identical except for having the opposite sign.

We find that the summed deviations from average earnings for the insiders before redistribution are significantly higher in LEG markets than in NOLEG. This indicates that - ignoring redistribution - insiders *profit* from the implementation of the legislation regime. By construction, uninformed traders' losses are thus significantly larger, putting them at a disadvantage after legislation (but before the redistribution of the fines levied on insiders). We formulate Result 6.

> **Result 6**: Not considering redistribution, insiders profit from legislation, while uninformed traders' earnings suffer.

Once we account for the redistribution from insiders to uninformed traders, we no longer find any significant effect of legislation on the deviations from average earnings. We formulate Result 7.

> **Result 7**: There is no significant effect of insider trading legislation on earnings after redistribution.

Leland (1992) arrives at a similar result as liquidity traders in his model framework suffer welfare losses under regulation. The results can be interpreted in light of the economic model of crime (Becker, 1968). Applying the model to our market setting predicts that, since insiders' cost in case of detection increase, they will only be willing to engage in the more profitable transactions. This notion is supported by empirical findings of Cumming et al. (2013) who show that more detailed exchange trading rules (e.g. the presence of insider trading

[19] Note that, given that our markets are zero-sum, inactive traders earn the market average and do not bias the sum of $\Delta EARN$.

legislation) and surveillance over time and across markets significantly reduce the number of insider cases, but increase the profits per case.

Results 6 and 7 spur important implications for the welfare situation in our markets. Although we find that uninformed traders are not worse off after redistribution, note that there are significant economic costs involved with legislation and enforcement. Redistribution only takes place in the presence of an authority, which necessarily operates at a cost, which in turn reduces total welfare.

Leland (1992) and Dow and Rahi (2003) discuss welfare effects from different angles, which cannot be studied in our framework. In his model, Leland (1992) focuses on aspects of allocative efficiency given by the level of real investment. He finds that welfare will tend to increase when the amount of investment is highly responsive to the current stock price. If on the contrary investment is inflexible to current stock price, net welfare tends to be lower when insider trading is permitted. Dow and Rahi (2003) consider the effects of insider trading on risk-sharing opportunities and consequently on agents' welfare. They find that greater revelation of information that agents wish to insure against reduces their hedging opportunities while early revelation of information that is uncorrelated with hedging needs allows agents to construct better hedges.

4 Conclusion and discussion

We conduct experimental limit order markets to study the effects of insider trading legislation on trading activity, price performance measures, and traders' profits. By resorting to laboratory market data we circumvent the lack of reliable empirical data caused by the currently prevailing legal systems, which inhibits an exhaustive scientific evaluation. We show that insider legislation has significantly *negative* effects on multiple market dimensions: Under insider legislation, (i) the liquidity and informational efficiency of markets are reduced, while spreads are unaffected, (ii) uninformed traders' losses (before redistribution) are higher due to deteriorating market quality, and (iii) overall welfare suffers due to the lower information content of prices and the cost of enforcement. In summary our findings suggest that insider legislation leads to welfare losses while failing to deliver many of the expected benefits for uninformed investors.

The substantial negative impact of insider trading regulation documented by our results stands in stark contrast to the fact that most western capital markets employ such regulations. This discrepancy can be rationalized in several ways.

First, the existence of insider trading legislation is sometimes explained by public choice theory (Bainbridge, forthcoming 2013). This theory states that an (governmental) agency will try to increase its scope of operations and enhance its prestige in order to obtain more resources. Insider trading laws are highly popular among voters, such that a vigorous enforcement program is surely an effective means to attract political support for larger budgets. However, if the costs of enforcement outweigh the benefits (as in our experiments) this results in welfare losses.

Second, turning to the demand side, insider trading legislation appears to be supported and driven in large part by market professionals (fund managers, pension funds, hedge funds). This group of investors - cohesive and politically powerful - benefits from legislation as it effectively insulates them from insider trading risk. Once insiders are removed from the equation, these market professionals constitute the best-informed group in the market, which may enable them to capitalize on this advantage. This demand side argument is supported by Hauser and Schredelseker (2014) who show by means of simulations that this group of investors indeed profits most from legislation.

Third, outside of our laboratory, fairness concerns may change the relative performance of, and overall welfare induced by, markets with and without insider regulations. If investors are sufficiently exploitation averse, they may refrain from participating in the market entirely in an environment without insider trading regulation. Given a sufficiently large proportion of investors with such preferences in a market, the attendant loss in liquidity could outweight the drawbacks from insider trading legislation diagnosed in our study.

A potential criticism of the results presented in this study is the use of a student subject pool compared to a pool consisting of professional traders. One might expect different results from a subject pool of market professionals. We will discuss this argument focusing on two dimensions: (i) incentives and (ii) theoretical considerations.

Starting with incentives, we concede that a subject pool difference could for example occur if professional traders were highly motivated to perform well, while students were not. To counter this argument, we follow the norms generally applied in experimental economic studies. We design our parameters such that the average payment to our subjects approximately equals the average wage for student jobs in the city where the experiments were conducted. Furthermore, note that trader (observer) payoffs vary considerably, ranging from EUR 0.00 to EUR 38.04 (EUR 0.00 to EUR 46.50) in NOLEG (LEG). Thus, subjects actions in the experiment mattered, in the sense that they significantly impacted their final monetary payoffs. When asked whether the payment they were led to expect motivated them to give their best, our participants' average reply was 3.21 on a scale where 0 corresponded to "Not at all" and 4 corresponded to "Very much". Taken together, we thus assert that our subjects were incentivized to do well.

A related issue is that our subjects' payments is bound from below at zero, thus ruling out losses and introducing convexity into our payout function. However, both characteristic apply to traders' payoffs inside and outside of our lab. Professional traders usually are not liable for any losses they may generate (except for the case of fraud, which is not under consideration here). Furthermore, bonus incentive schemes, which are widely applied in the financial industry, similarly generate convex payoff functions. See Holmen et al. (2014) and Kleinlercher et al. (2014) and references therein for studies documenting this phenomenon.

The second dimension focuses on theoretical consideration of the issue. Apart from any effect on the traders, an alternative way to look at the issue is to focus on the impact of a possible subject pool effect among the observers. If there is an effect of not using professionals in the role of market observers, we would expect it to manifest itself in a lower insider detection probability. In other words, we would expect highly trained professionals at, for example, the SEC to have a greater chance of identifying suspicious behavior than our student subjects have. However, this would lead our study to underestimate the impact of insider legislation on traders' behavior compared to the impact on traders outside the lab. In other words, this argument suggests that our estimates of the size of the setting effect are likely to be conservative. In fact, our subjects' answers on our exit questionnaire corroborate this finding. When asked what strategy they used to identify insiders, 14.1% of all observers indicated that they found their task very difficult or that none of the information items provided helped them. Note that the next highest answer category (identifying insiders from observing which traders made offers or traded early on in the period) was chosen by only 9.4% of all observers, and only 35.9% provided any answer we were able to classify. (See Appendix C for the exit questionnaire and for more information on the questionnaire results.) Taking these argument together, we are confident that the use of a student subject pool instead of professional traders does not bias the reported results.

We believe that our findings provide new input into the debate regarding the desirability of insider trading regulation and hope that this study will spark new research into this highly relevant element of security market design.

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Figures

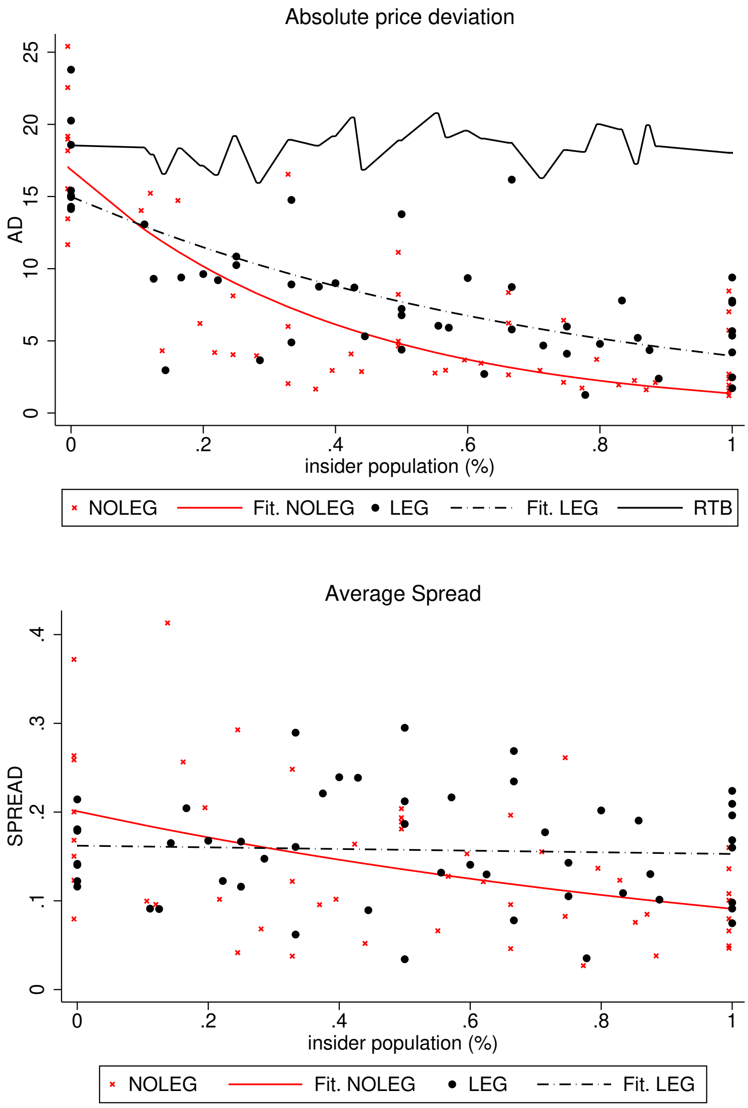


Figure 1: UPPER PANEL: Average values of absolute price deviation (AD) in settings NOLEG (red crosses) and LEG (black dots) conditional on the share of insiders in the market. AD is an inverse measure of price efficiency and is defined as the absolute difference between the (volume weighted) mean price in a market and the BBV. The random trading benchmark (RTB, bold, solid, black line) is an indicator for the expected level of AD assuming orders submitted at random prices. The solid (red) and dashed (black) lines represent the best exponential fit for settings NOLEG and LEG, respectively. LOWER PANEL: Average values of $SPREAD$, a measure of transaction costs, in settings NOLEG (red crosses) and LEG (black dots) conditional on the share of insiders in the market. The solid (red) and dashed (black) lines represent the best linear fit for settings NOLEG and LEG, respectively.

Tables

Table 1: Distribution sample size.

# informed traders	# uninformed traders 0	1	2	3	4	5	6	7	8	9
0			12	8	7	6	5	4	4	4
1		12	8	7	6	5	4	4	4	
2	12	8	7	6	4	4	4	4		
3	8	7	6	5	4	4	4			
4	7	6	4	4	4	4				
5	6	5	4	4	4					
6	5	4	4	4						
7	4	4	4							
8	4	4								
9	4									

Notes: The cells report the number of observations from the corresponding market composition.

Table 2: Accessible real time observer information about traders' activities in the market.

#	item	description
1	Offer volume limit	Total number of shares the trader offered to buy or sell.
2	Volume limit canceled	Number of shares the trader deleted from her offer volume limit.
3	Trading volume limit	Number of shares the trader traded through offers to buy and sell she made.
4	Trading volume market	Number of shares the trader traded by accepting existing offers by another trader in the market.
5	Volume bought	Number of shares the trader bought.
6	Volume sold	Number of shares the trader sold.
7	Volume bought - sold	Volume bought minus volume sold.
8	Average price	Average price the trader bought or sold shares for.
9	Average volume	Average number of shares the trader bought or sold in each transaction.

Table 3: Trading activity by informed and uninformed traders.

	LIMIT ORDERS			TRADES		
NOLEG	#	VOL	AV	# (2x)	VOL (2x)	AV
uninf.	3,225 (42%)	33,551 (35%)	10.40 (-17%)	3,288 (50%)	19,276 (44%)	5.86 (-10%)
insider	4,386 (58%)	62,380 (65%)	14.22 (13%)	3,332 (50%)	24,042 (56%)	7.22 (10%)
	7,611	95,931	12.60	6,620	43,318	6.54
LEG	#	VOL	AV	# (2x)	VOL (2x)	AV
uninf.	3,672 (48%)	33,146 (45%)	9.03 (-6%)	3,225 (49%)	15,415 (47%)	4.78 (-5%)
insider	3,988 (52%)	40,259 (55%)	10.10 (5%)	3,327 (51%)	17,505 (53%)	5.26 (5%)
	7,660	73,405	9.58	6,552	32,920	5.02

Notes: # the number of limit orders and trades; VOL volume of limit orders and trades; AV average size of a limit order or trade (VOL divided by #). Note that results for # and VOL of trades are double counts, since both an insider and an uninformed trader may be party to a trade. Values in parentheses are percentage shares for informed and uninformed traders (# and VOL) and percentage deviations from the average value (AV).

Table 4: Variable definitions for the set of dependent variables.

Variable		**definition**
RQ 1		
Number of contracts	$\#CON$	= number of contracts.
Limit order turnover	LOT	$= LO/ASO$.
Number of transactions	$\#TRA$	= number of transactions (N).
Share turnover	ST	$= VOL/ASO$.
Average order size	$\overline{LO}_i$	$= LO/\#CON$.
Average transaction size	$\overline{TRA}_i$	$= VOL_i/\#TRA_i$.
Market share of limit orders	$\%LO_i$	$= LO_i/LO$.
Market share of trades initiated by limit orders	$\%LT_i$	$= LT_i/LT$.
Market share of trades initiated by market orders	$\%MT_i$	$= MT_i/MT$.
RQ 2		
Absolute price deviation	AD	$= \left\|\overline{P} - \text{BBV}\right\|$.
Average spread	$SPREAD$	$= \frac{1}{VOL}\sum_{t=1}^{N}\frac{SPREAD_t \cdot VOL_t}{P_t}$.
RQ 3		
Deviation from av. earning	$S\Delta EARN_i$	$= \sum \Delta EARN_i$.

Notes: i indexes variables calculated for informed and uninformed traders separately; t indexes transactions; N total # transactions; LO = number of shares offered to trade; ASO = active shares outstanding, i.e., sum of asset holdings of all active traders; VOL = number of shares traded; $\overline{P}$ = (volume-weighted) mean price; BBV = buy-back value of the asset; P = individual transaction price; $SPREAD$ = bid-ask spread at time of transaction; $\Delta EARN$ = deviation from average earnings of (active) market participants.

Table 5: Definitions for the set of independent variables.

Variable	definition
LEG	Treatment dummy; 1 for market under LEG; 0 otherwise (NOLEG).
$SHARE_{IN}$	Control for the relative share of insiders present.
BBV	The assets' buy-back value.
$DISTANCE$	Absolute difference between the unconditional expected value of BBV and its realization.
$NUMTRADERS$	The market size, defines levels.
$COHORT1-31$	Dummy variables for individual cohorts of traders (totalling 32); included in the regression but not reported.
$WITHIN$	1 for within situations ($\mathrm{SIT}_{\mathrm{within}}$); 0 otherwise ($\mathrm{SIT}_{\mathrm{outside}}$).
$LEG*WITHIN$	interaction between LEG and $WITHIN$.

Table 6: Market activity and average limit order and transaction size by trader type.

	# CON	LOT	# TRA	ST	$\overline{LO}_{in}$	$\overline{TRA}_{in}$	$\overline{LO}_{un}$	$\overline{TRA}_{un}$
α	26.364***	0.988***	28.214***	0.221***	12.709***	6.336***	11.678***	4.187***
	(3.635)	(0.050)	(6.934)	(0.029)	(1.247)	(0.360)	(1.702)	(0.481)
LEG	0.296	-0.218***	-2.050***	-0.028***	-3.487***	-1.247***	-1.014**	-0.459*
	(0.335)	(0.022)	(0.726)	(0.009)	(0.591)	(0.220)	(0.396)	(0.255)
SHARE_{IN}	2.002***	0.385***	-4.197	0.025	0.839	0.967	3.181	2.387***
	(0.594)	(0.051)	(3.387)	(0.034)	(1.238)	(0.666)	(2.141)	(0.915)
DISTANCE	0.039***	-0.000	0.073	0.001**	0.029	0.030	-0.011	0.049***
	(0.015)	(0.001)	(0.069)	(0.001)	(0.029)	(0.024)	(0.035)	(0.016)
BBV	-0.046***	-0.003***	-0.079	-0.001**	-0.009	-0.018*	-0.048	-0.010
	(0.016)	(0.001)	(0.051)	(0.001)	(0.016)	(0.010)	(0.041)	(0.008)
N	558	558	508	508	452	421	441	413
χ^2	34.96	226.02	17.50	14.89	59.27	90.78	13.16	24.97
p	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.00

Notes: * p<0.1; ** p<0.05; *** p<0.01; standard errors in parentheses.

Table 7: Market shares of limit orders and trades by situations and legal setting.

	LIMIT ORDERS		TRADES	
	SIT_{within}	$SIT_{outside}$	SIT_{within}	$SIT_{outside}$
NOLEG	52.92%	47.08%	38.77%	61.23%
LEG	41.26%	58.74%	28.18%	71.82%
Difference	±11.58***		±10.60***	
N	560		510	

Notes: * p<0.1; ** p<0.05; *** p<0.01; p-values determined by t-tests. Treatment differences denominated in percentage points.

Table 8: Regression model for changes in the trading activity of informed and uninformed investors.

	%LO		%LT		%MT	
	Insider	Uninformed	Insider	Uninformed	Insider	Uninformed
α	0.114***	0.967***	0.066	0.960***	0.234***	0.867***
	(0.040)	(0.037)	(0.058)	(0.051)	(0.059)	(0.042)
LEG	-0.040**	0.034*	-0.004	0.009	-0.022	0.025
	(0.018)	(0.018)	(0.037)	(0.035)	(0.019)	(0.019)
WITHIN	0.112***	-0.122***	0.252***	-0.264***	-0.278***	0.285***
	(0.006)	(0.007)	(0.040)	(0.038)	(0.025)	(0.023)
LEG*WITHIN	-0.050***	0.061***	-0.043	0.040	0.075***	-0.076***
	(0.013)	(0.015)	(0.053)	(0.049)	(0.018)	(0.021)
SHARE_{IN}	0.878***	-1.040***	0.889***	-0.898***	0.845***	-0.996***
	(0.025)	(0.023)	(0.029)	(0.041)	(0.038)	(0.031)
DISTANCE	0.001	-0.001	-0.001	0.000	-0.001	0.000
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)
BBV	-0.000	0.000	-0.001	0.001	0.000	-0.000
	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.000)
N	876	844	686	677	686	677
χ^2	7702.69	7615.00	7931.85	1830.06	1094.70	1702.12
p	0.00	0.00	0.00	0.00	0.00	0.00

Notes: * p<0.1; ** p<0.05; *** p<0.01; standard errors in parentheses. In regression %LT for uninformed traders the iteration process of the gradient based estimation is stopped after iteration step 15.

Table 9: Regression analyses for price performance measures.

FULL SAMPLE	*AD*	*SPREAD*	*SPREAD* 2
α	4.532***	0.156***	0.181***
	(1.083)	(0.016)	(0.021)
LEG	1.656***	0.013	-0.026*
	(0.463)	(0.008)	(0.014)
SHARE_{IN}	-11.225***	-0.062***	-0.106***
	(0.863)	(0.018)	(0.019)
LEG*SHARE_{IN}			0.079***
			(0.025)
DISTANCE	0.270***	0.000	0.000
	(0.042)	(0.000)	(0.000)
BBV	0.081***	-0.000	-0.000
	(0.017)	(0.000)	(0.000)
N	508	448	448
χ^2	210.07	30.40	81.63
p	0.00	0.00	0.00

Notes: * p<0.1; ** p<0.05; *** p<0.01; standard errors in parentheses.

Table 10: Sum of deviations from average profits by insiders before and after redistribution.

$S\Delta EARN$ for insiders	before redistribution	after redistribution
α	-0.053	0.088
	(0.182)	(0.127)
LEG	0.088**	-0.006
	(0.036)	(0.055)
SHARE$_{IN}$	-0.150	-0.168
	(0.193)	(0.120)
DISTANCE	0.011**	0.008**
	(0.004)	(0.003)
BBV	0.002	0.000
	(0.002)	(0.002)
N	360	360
χ^2	38.93	6.89
p	0.00	0.14

Notes: * p<0.1; ** p<0.05; *** p<0.01; standard errors in parentheses. Markets either with no insiders (100) or all insiders (100) are excluded from the analyses. For figures of uninformed traders multiply by -1. No robust standard errors used in regression using values after redistribution.

Appendix

Appendix A - Experimental Instructions

Dear participant!

Welcome to this experimental session. Thank you for your participation!

Please note the following right away:

- Everything you are told in these instructions is the truth.
- Every participant gets the same instructions and information about the experiment.
- The decisions you make cannot be linked to your person either by other participants or by the experimenters. In other words, your anonymity is guaranteed throughout the entire experiment.
- Communication between participants is prohibited during the entire experiment. You are furthermore not allowed to use mobile phones or pocket calculators and you may use only those functions of the computer which are required for the experiment. If you do not follow these conditions, you will NOT be paid.
- If you have a question regarding the procedures or the instructions during the session, the experimenter will clear them up with you privately if you raise your hand.

Structure of the experimental session

Market experiment

- Instructions market experiment
- Explanation trading of shares, training periods (not payoff-relevant)
- Experiment

Payment

Instructions for the market experiment

Background of the experiment

The experiment replicates a stock market. You are a member of a group which consists of yourself and 11 other people. The composition of your group will remain unchanged throughout the entire experiment and you will only interact with people from your group. The experiment consists of 10 independent (trading) periods.

Your role during the experiment

At the beginning of the experiment, 10 participants will be randomly chosen to become active market participants (traders). As a trader, you participate in the trading of shares and you can buy and sell shares (of a fictitious company). The 2 remaining participants are passive market participants (obervers), who will follow the goings-on in the market, but will not actively participate in trading. This role assignment will remain unchanged throughout the entire experiment. You will now receive more information about the trader and observer roles.

Traders

Starting endowment

At the beginning of a trading period, each trader will receive 60 shares of stock and 4800 taler in order to be able to make transactions (purchases and sales of shares). Note that your balances of shares and taler cannot fall below zero and that you will NOT be able to carry over share and taler holdings into the next trading period.

Number of traders in the market

The number of traders in your group is 10. The number of traders who will be in a *market* together at any one time can vary between 2 and 9. In a given period, four traders from your group may for example trade in one, and another six traders may trade in a second market. For you, only the activities in your own market are of any relevance. You will not receive any information regarding the number of traders in your market.

Trading

Every trader can both buy and sell shares. Every trading period ends automatically after 240 seconds. In the market, the activities of the traders alone determine at what price the share will be traded. When you buy or sell shares, your taler and share balances will change accordingly.

Buyback value of the share

At the end of a trading period, your share holdings will be taken over by the experimenter in return for the buyback value. The latter will be randomly drawn by the computer at the beginning of the period. It will have a value of between 30.0 and 85.0 (with one digit behind the comma). Every value in this interval will be drawn with equal probability.

Information about the buyback value of the share

Before a trading period starts, an unknown (to you) number of traders will receive information about the exact buyback value of the share (informed traders). The number of traders who will receive this information can be 0, all, or any number in between. The remaining traders (uninformed traders) will not receive this information. They only know that the buyback value is uniformly distributed over 30.0 and 85.0.

Calculation of your payoff as a trader

Your payoff in euros is the sum of the income from the 10 periods, which consists of the income as a trader and an extra income from answering two estimation questions.
Your income as a trader is determined by your wealth at the end of a period:

your wealth = number of shares × buyback value + taler balance

[Start LEG only]Please note that your wealth may furthermore be influenced by the actions of the observers (you will learn more in the section “Observer”).[End LEG only] You can increase your wealth by estimating, at the end of each trading period, as precisely as possible two pieces of market data:

> *Estimate 1)   Estimate the total number of traders who were in your market (incl. yourself).*
>
> *Estimate 2)   Estimate the number of informed traders who were in your market (incl. yourself, in case that you were informed).*

For each correct estimate you will get another 500 taler added to your wealth.

At the end of the experiment, the wealth you accumulated will be added up over all periods, and a fixed amount of 66,000 taler will be deducted. The result will be converted into euros at an exchange rate of 900:1.

income in euros = (sum of all wealth positions from all periods – 66.000) / 900

Example: At the end of a period, you have 66 shares and 4,785 taler. The buyback value is 52.50. Your wealth in this case is 66 x 52.50 + 4,785 = 8,250. If we assume that you obtain the same result in all 10 periods (=82,500) and that you earn another 2,500 taler from your estimates, then the sum of your period incomes is 85,000. In this case, your income is (85,000 – 66,000) / 900 = 21.11 euros.

Inactive trader

In exceptional cases, not all 10 traders in your group will be able to participate in a market in a given period. In this case there will be inactive traders, who can earn money by solving calculation exercises. One such exercise consists of multiplying a two-digit number by a one-digit number. If the calculation is correct, the exercise will be considered solved. If the calculation is incorrect, you will receive an error message and will have to solve the exercise again. You will receive 350 taler for each exercise solved correctly. You will have 240 seconds to solve exercises. For your calculations, you may only use the scrap paper provided (no mobile phones or pocket calculators)!

Observers

In the observer role, you follow the market activity without participating in transactions yourself. As an observer, you will get a payment for participation of 15.- euros. Your payoff in euros will consist of the payment for participation and the sum of the 10 period earnings. As an observer, you can earn money by answering three estimation questions at the end of each period. To help you, you will be provided with information about the trading behavior of the traders in the market. Every trader will be identified by a randomly assigned, unique code, which will consist of a letter and a number, e.g. J8. This code will change for every trader from period to period, and every code will only be used once in the 10 trading periods. During the trading period, the following data will be continuously updated for each trader. You can display three of these at any one time.

Offer volume limit:	Total number of shares which the trader has offered to buy or sell.
Volume limit cancelled:	Number of shares which the trader has deleted from his offer volume limit.
Trading volume limit:	Number of shares which the trader has traded through offers to buy and sell he created.
Trading volume market:	Number of shares which the trader has traded by accepting existing offers by another trader in the market.
Volume bought:	Number of shared the trader has bought.
Volume sold:	Number of shared the trader has sold.
Volume bought - sold:	Volume bought minus volume sold.
Average price:	Average price the trader has bought or sold shares for.
Average volume:	Average number of shares the trader has bought or sold in each transaction.

As an observer, you will submit the following estimates at the end of a period:

Estimate and payoff 1)
Estimate the number of informed traders in the market. If your estimate equals the actual number of informed traders, you will receive 2 euros.

Estimate and payoff 2)
Identify, using their trader codes, those traders who you consider more likely than not to have had information about the buyback value. For each correctly identified trader, 0.50 euros will be added

to your payoff. For each incorrectly identified trader, 0.50 euros will be subtracted from your payoff.

Estimate and payoff 3)
Select, using their trader codes, those traders whom you think have had information about the buyback value and whom you want to select. For each correctly identified trader, 1 euro will be added to your payoff. For each incorrectly identified trader, 2 euros will be subtracted from your payoff.

[Start LEG only]**Effect of your answer on estimation question 3 for the traders**
Each trader who is selected by you as being informed (estimate 3) and who actually had received information about the buyback value will lose his trading profit for the period. The trading profit is the (positive) difference between the trader's wealth at the end of the period and the wealth at the beginning of the period (in both cases, the buyback value will be used to evaluate the value of the shares). The entire profits which are deducted from informed traders in this way will be distributed to those traders, who were not selected as having been informed (even if, in reality, they had been informed). The distribution is based on the number of shares bought or sold at a loss. If two observers observe a single market, one of them will be randomly selected to have his answers used in this way.

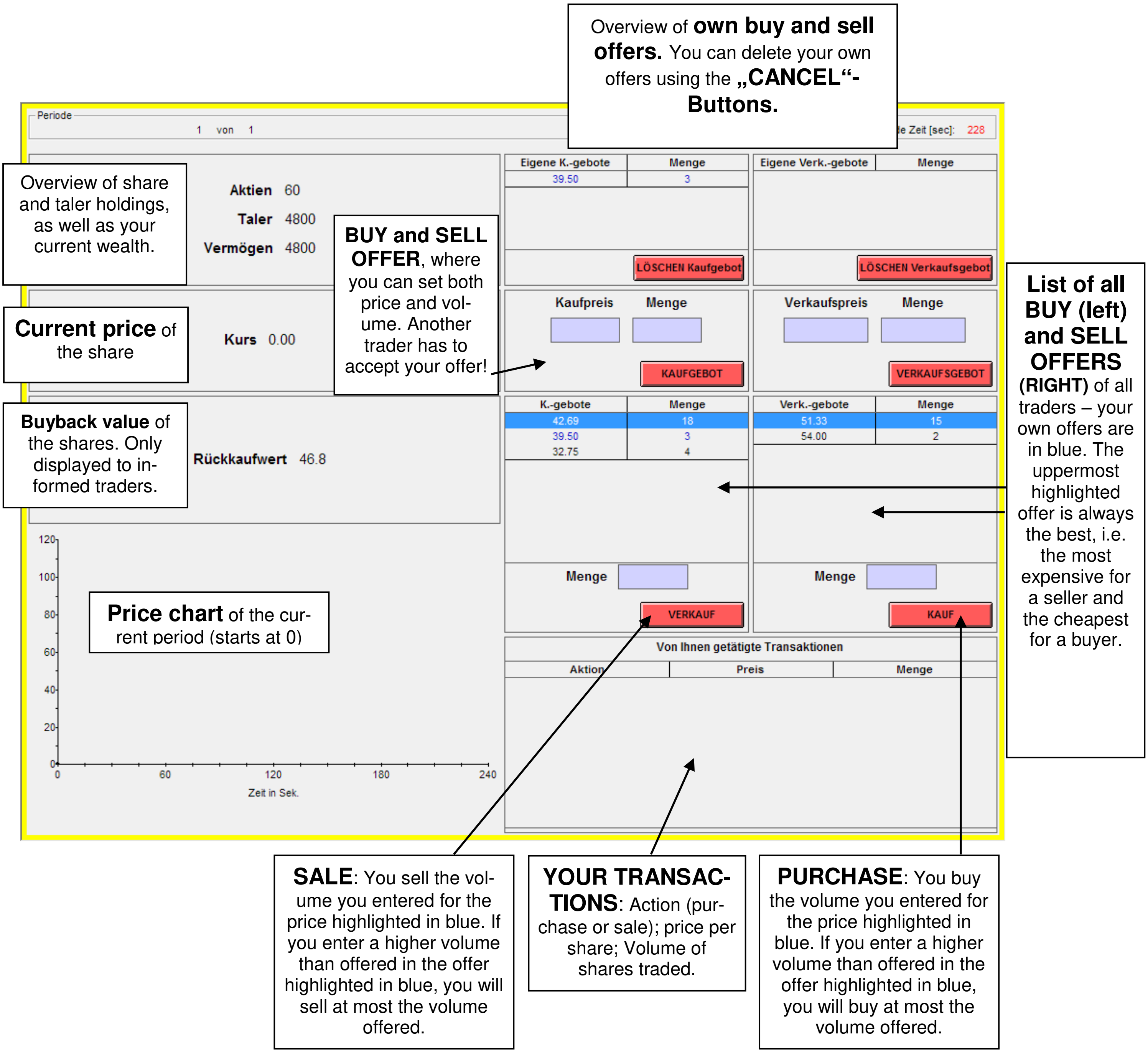
Example: The buyback value of the shares is 65. In a market with three traders, an informed trader buys a share for 40.00 from one uninformed trader and two shares for 47.50 each from a second uninformed trader.
At the end of the period, the informed trader is selected as being informed by the observer. His ending wealth consists of 63 shares with a buyback value of 65 taler and 4,665 taler in cash, for a total of 8,760 taler. The difference with regard to the beginning wealth of 60 x 65 + 4,800 = 8,700 taler is 60. This difference is the profit he will lose because of having been selected.
The uninformed traders have sold a total of 3 shares at a loss. Therefore, the first uninformed trader will receive 1/3 x 60=20 taler, and the second will receive 2/3 x 60=40 taler. [End LEG only]

Additional important information

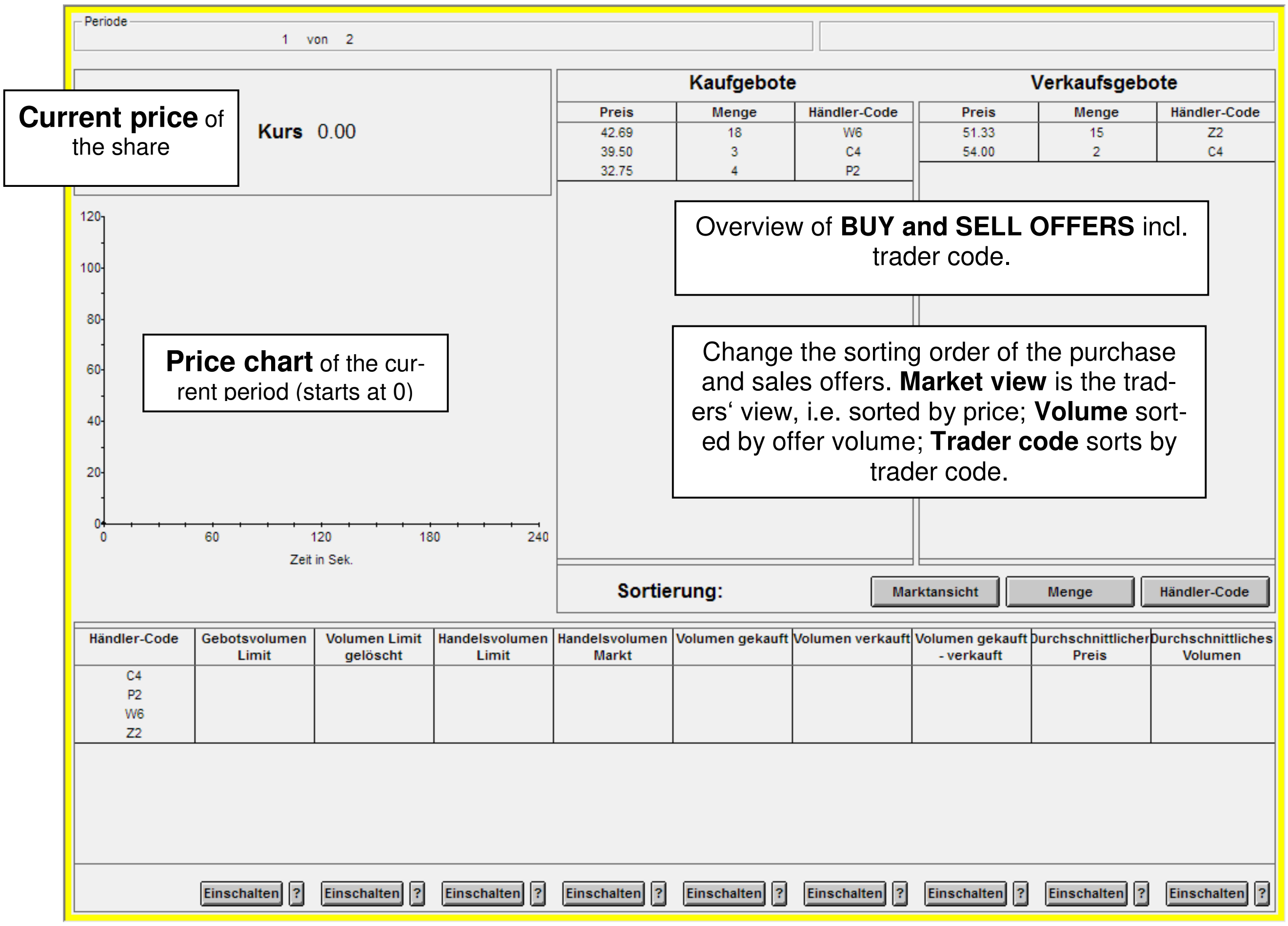
- Taler holdings do not bear interest
- The comma character is the full stop (.)
- There will be 2 training periods in which all 10 traders form one market. Each participant will in one period be an informed trader and in the other period an uninformed trader. Training periods do not count towards your payoff.

Trader screen

The process of buying and selling will be illustrated using the following illustration.



Observer screen



Information about the trading activity of the individual traders (trader code). You can display a maximum of **3 columns at any one time**. Just use the „**Activate**" button below. Activated information can be deactivated at any time to be replaced by different columns. The „**?**"-Buttons explain the values displayed in the columns above.

Please always click OK when you have read the information on the screen. On most screens, the information will remain on the screen and the experiment will continue when all participants have confirmed by clicking OK.

Appendix B - Incentives for observer questions O2 and O3

We design the payment scheme for observer questions O2 and O3 to ensure that subjects are incentivized to report their true beliefs, without incentives to for example randomize or to for example suspect/select all traders in order to maximize the expected payoff. Specifically, we start by designing question O2 such that an observer should *suspect* a trader of having been informed if the observer believes the probability of said trader having been informed to be greater than 50%. Note that, in order to derive our payment function, we assume observer subjects to be risk-, ambiguity- and loss-neutral. This is a conservative assumption, as risk-, ambiguity- and/or loss-averse observers are generally less likely to suspect or select a trader than are risk- and loss-neutral observers who hold the same beliefs about traders' probability of having been informed. Thus, we expect our results to yield a lower bound on the number of traders being suspected or selected by observer subjects. We start by defining the threshold probability p as the probability of a trader being informed above which an observer subject will decide to suspect the trader. In other words, if an observer estimates the probability of any specific trader being informed to be less than p, the observer will not suspect this trader. An observer's expected profit from suspecting any trader is:

$$E[\Pi_{susp}] = \quad p\pi^{+} + (1-p)\pi^{-} \tag{2}$$

where π^{+} (π^{-}) is the fixed payoff from correctly (incorrectly) suspecting a trader. The instructions inform all subjects that "The number of traders who will receive [information about the buyback value] can be 0, all, or any number in between." Without any further information, observers should thus expect any trader to be just as likely to be an insider as not. Remember that we want observers to suspect traders if their subjective probability estimate of the trader being informed is greater than 0.5. In other words, we want the right-hand side of equation (2) to be positive for $p > 0.5$ and negative for $p < 0.5$. This yields the following relationship of the two possible payoffs:

$$E[\Pi_{susp}] = 0.5\pi^+ + (1-0.5)\pi^- = 0 \qquad (3)$$
$$\Rightarrow \quad \pi^+ = -\pi^-$$

This implies that the cost of falsely suspecting a subject of being an insider should be equal to the profit from rightly identifying an insider if an observer should only suspect those traders she believes to more likely than not having been informed. However, note that this criterion can be expected to yield relatively unstable results, as the observer will rationally suspect any trader she deems even a little bit more likely than not of having been informed. Thus, we would expect our measure of suspected traders to carry a large margin of type I error (uninformed traders being suspected).

On the other hand, requiring a higher estimated probability of a trader having been informed raises the possibility that an observer will not accuse somebody she thinks is more likely than not to have been an insider. Thus, there is a tradeoff: a higher threshold probability p decreases the danger of a type II error (not accusing an insider), but increases the danger of a type I error (accusing a non-insider subject). For our second measure, *selecting* a trader (O3), we thus choose a probability we deem to be intermediate. We structure our incentives in such a way that an authority should be at least 75% sure that a given subject is an insider for her to accuse him. Again departing from equation (2) this implies:

$$E[\Pi_{sel}] = 0.75\pi^+ + (1-0.75)\pi^- = 0 \qquad (4)$$
$$\Rightarrow \quad \pi^+ = \pi^- - \tfrac{\pi^-}{0.75}$$
$$\Rightarrow \quad \pi^+ = -0.5\pi^-$$

In other words, the negative payoff for falsely selecting a non-insider should be twice as large as the positive payoff for selecting an insider.

Appendix C - Exit questionnaire

Exit Questionnaire

The exit questionnaire was elicited after the markets but before subject payment. While filling in the questionnaire, subjects cannot return to previous pages. For each entry, the variable name is listed in square brackets, while the captions and internal value coding are listed in parentheses. A horizontal line indicates a page break.

1. Please describe how the available information (limit order volume, limit order volume cancelled, limit trading volume, market trading volume, volume bought, volume sold, volume bought – volume sold, average price, average volume) may be used to identify informed traders! [IdentificationStrategy]
   (Open text) [Observers only]

---

2. Which strategies did you apply in order not to be identified by the observers as an informed trader? [HidingStrategy]
   (Open text) [Traders only]
3. How easy do you think is it for observers to identify informed traders? [EaseIdentification]
   (0 = "Very easy" …7 = "Very hard") [Traders only]

---

4. How do you see yourself: are you generally a person who is fully prepared to take risks or do you try to avoid taking risks? [RiskType][1]
   (0 = "Not at all willing to take risks" …10 = "Very willing to take risks")

---

5. How easy to understand were the instructions? [UnderstandingInstructions]
   (0 = "Not at all easy" …4 = "Very easy")

---

6. Did the payment you were expecting increase your motivation to give your best? [PayoffSalience]
   (0 = "Not at all" …4 = "Very much")

---

7. Which program are you currently studying in? [LevelOfStudies]
   ("None", "Bachelor", "Master/Diploma", "Doctorate")
8. How many years have you so far studied in total? [StudyYears]
   (Integer between 0 and 99)
9. Which department are you studying at? [Department]
   ("Medicine", "Law", "Business/Economics", "Humanities", "Natural sciences", "Theology", "Construction", "Other")

[1] This item is taken from Dohmen et al. (2011), with the specific wording stemming from Infratest Sozialforschung (2004).

10. If you chose “Other” – what department are you studying at? [MajorOther]
    (Open text)

11. Is this the first laboratory experiment you participate in? [FirstLaboratoryExperiment]
    (“Yes”, “No”)

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12. Your age? [Age]
    (Integer between 17 and 99)

13. Your gender? [Female]
    (1 = “Female”, 0 = “Male”)

14. Your nationality? [Nationality]
    (“Austrian”, “Other”)

15. What is your mother language? [Language]
    (“German”, “Other”)

16. If you chose “Other” – what is your mother language? [LanguageOther]
    (Open text)

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17. Room for your comments regarding the experiment: [GeneralComments]
    (Open text)

Appendix D - Robustness checks for $SPREAD$

We test the robustness of our results regarding $SPREAD$ on two sub-samples of our data. The results are provided in Table E1. Columns 2 to 4 contain robustness check 1, which only considers markets in which the share of insiders is below 25%. It is motivated by the fact that in real markets the number of insiders compared to the size of the market can be considered small. Columns 5 to 7 report our findings for robustness check 2. It focuses on larger markets - we exclude the smallest markets which are usually prone to higher idiosyncratic variation. We do so by running the regression only on markets with 4 or more traders. Both robustness checks support the qualitative conclusions from our main analysis.

Table E1: Regression analyses for price performance measures - Robustness checks.

	RC1: 0< SHARE$_{IN}$ <0.25			**RC2**: NUMTRADERS>3		
	AD	*SPREAD*	*SPREAD* 2	*AD*	*SPREAD*	*SPREAD* 2
α	5.403	0.119***	0.208***	4.545***	0.151***	0.168***
	(3.707)	(0.027)	(0.033)	(1.407)	(0.018)	(0.023)
LEG	1.178***	-0.008	-0.154***	1.465***	0.013	-0.014
	(0.451)	(0.019)	(0.053)	(0.512)	(0.009)	(0.013)
SHARE$_{IN}$	-22.685*	-0.072	-0.524***	-11.011***	-0.065***	-0.095***
	(12.867)	(0.072)	(0.147)	(1.103)	(0.010)	(0.017)
LEG*SHARE$_{IN}$			0.742***			0.056**
			(0.218)			(0.024)
DISTANCE	0.280***	0.000	0.000	0.235***	0.000	0.000
	(0.106)	(0.001)	(0.001)	(0.036)	(0.000)	(0.000)
BBV	0.065**	0.000	0.000	0.073***	-0.000	-0.000
	(0.029)	(0.001)	(0.000)	(0.020)	(0.000)	(0.000)
N	75	66	66	411	372	372
χ^2	294.24	13.90	187.10	134.62	97.18	896.26
p	0.00	0.01	0.00	0.00	0.00	0.00

Notes: * p<0.1; ** p<0.05; *** p<0.01; standard errors in parentheses. In regression *SPREAD* 2 of RC2 the iteration process of the gradient based estimation is stopped after iteration step 15.