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Faculty of Social and Economic Sciences

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Can human observers identify insiders in asset markets?

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Working Paper 2018-01

April 3, 2018

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Abstract

Securities regulators around the globe face the challenge of identifying trades based on inside information. We study human observers' ability to identify informed traders and investigate which trading patterns are indicative of informed trading using experimental asset markets. We furthermore test how the behavioral response of informed traders to the threat of detection and punishment impacts observers' detection abilities. We find that market trading data carries information which correlates with informed trading activity. Observers partly succeed in recognizing and using this information to identify in-formed traders.

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JEL: C92, D82, G12, G14

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Catch me if you can. Can human observers identify insiders in asset markets?

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ABSTRACT

Securities regulators around the globe face the challenge of identifying trades based on inside information. We study human observers' ability to identify informed traders and investigate which trading patterns are indicative of informed trading using experimental asset markets. We furthermore test how the behavioral response of informed traders to the threat of detection and punishment impacts observers' detection abilities. We find that market trading data carries information which correlates with informed trading activity. Observers partly succeed in recognizing and using this information to identify informed traders.

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Working paper, 2018-04-03

Trading based on material, non-public information – i.e., inside information – is regulated in most capital markets. Such trading is usually forbidden, as is passing on such information ("tipping"). Arshadi (1998) shows that the prevailing regulation might be reasonably effective in deterring illegal insider trading by registered and temporary insiders but that it may fail to deter illegal insider trading by persons from outside the firm whose securities are being traded. Therefore, securities regulators face the challenge of identifying violations of these rules, which they attempt by monitoring the market and by trying to spot suspicious market movements. Whether they are successful in this respect or not is not clear. Biggerstaff, Cicero and Wintoki (2017) and Kacperczyk and Pagnotta (2017) provide supportive evidence while Collin-Dufresne and Fos (2015) cast doubt on the possibility of detecting insiders based on price data. Specifically, Biggerstaff, Cicero and Wintoki (2017) show that corporate insiders trade over longer periods of time when they have a longer-lived informational advantage. Collin-Dufresne and Fos (2015) study how measures of adverse selection respond to informed trading and conclude that these measures may fail to capture the presence of informed trading when insiders can select when and how to trade. Kacperczyk and Pagnotta (2017) challenge these results and show that the information signals are impacted by trades based on private information. However, these authors report that the ability of these signals to detect private information is weaker when experienced traders or top executives trade.

In addition to this inconclusive evidence, there is ample evidence that market participants (e.g., uninformed traders) are capable of inferring the presence and the information informed traders (insiders) possess. This capability is quite robust and persists despite anonymity of the trading process, a lack of structural knowledge of the situation, and the absence of long histories from which traders can learn the market's statistical regularities (see Plott and Sunder, 1988; Nöth and Weber, 1996; Schnitzlein, 2002; or Bruguier, Quartz and Bossaerts, 2010). These papers, however, provide only limited evidence about the kind of information that allows observers to recognize the presence of informed traders or what type of information would allow them to identify informed traders in the order and trade flow of markets. Bruguier, Quartz and Bossaerts (2010) attempt to identify whether subjects who are better at theory-of-mind thinking, i.e., being able to understand and infer other people's plans, thoughts, and points of view, succeed at recognizing the presence of informed traders from observation of the trade flow. They report that this is the case and suggest that subjects may use GARCH-like persistence in transaction price changes to identify informed trading.

Since trading on inside information is prohibited by law in most jurisdictions, it is difficult to study this issue using empirical data. It is also not possible to study the counterfactual of a market without insider trading regulation, since essentially all developed capital markets have such regulations. In the present paper, we therefore turn to laboratory experiments to study informed trading and its detection. In a laboratory setting, the experimenter controls the environment, can implement custom regulations and can observe all outcomes. It is for this reason that we believe experiments to be ideally suited for studying insider trading and its regulation. We employ this approach to investigate which trading patterns are indicative of informed trading and to explore the ability of market observers to recognize informed trading and to distinguish it from uninformed trading. Finally, we test how informed traders react to the threat of detection and punishment and how their behavioral response impacts observers' detection abilities.

We find that traders who observers suspect of being informed are significantly more likely to have been informed than traders observers do not suspect. We also find that human observers are more successful in identifying informed traders than are zero-intelligence observers. Observers try to identify informed traders using the volume of shares offered for trade using limit orders, the volume of shares bought and sold, and average trading prices. In the cases of information on the volume of limit orders submitted by a trader, and on the volume of shares purchased (irrespective of order type), this reliance is justified, since informed traders indeed submit significantly higher volumes via limit orders, and buy more shares. However, observers also believe that informed traders trade more shares via limit orders. In this they are mistaken, as the opposite is the case. Moreover, observers fail to capitalize on the fact that informed traders' transactions are on average characterized by higher prices and volumes per trade. Comparing the sessions without and with the threat of detection and punishment we find that there are no differences in an informed trader's probability of being suspected by the observers. Finally, we do not find support for widespread manipulation attempts by informed traders. Specifically, there is no difference between informed and uninformed traders in cancelled limit order volume or limit order volume remaining unexecuted at the close of the market.

1 Experimental design

Twelve subjects form a cohort in our experiment. Ten out of the twelve subjects are (potential) traders, while two are market observers. Subjects are randomly assigned to one of the two groups after having jointly received the instructions for both. Once assigned, they retain their respective role throughout the session. Following two trial periods (not payoff-relevant), there are ten independent, payoff-relevant periods in each session.

1.1 Traders

Market participants trade a homogeneous asset in a multi-unit continuous double auction with an open order book. The asset has a life of one period and is bought back by the experimenter at the end of the period for a random buyback value (BBV) uniformly distributed over all multiples of 0.1 in the range between 30.0 and 85.0 taler (the experimental currency). Traders can either post limit orders or accept outstanding limit orders in what we will refer to as a market order or market trade.

There are no restrictions regarding the size of limit orders other than those imposed by subjects' budget constraints. The partial execution of limit orders is possible, while short sales and margin purchases are not. Order books are empty at the beginning of each period and limit orders are executed according to price and then time priority. The order books provide full information about the prices and quantities of unfilled orders. Traders cannot link orders to individual subjects. Posted limit orders, including the best bid and ask, can be canceled freely and at any time. Taler holdings do not pay interest and there are no transaction costs. The trading screen contains information about current asset and taler holdings as well as about current wealth (assets evaluated at the latest transaction price). It also displays a list of transaction prices with the corresponding trading times as well as a real-time price chart. Each market (trading period) lasts for 240 seconds. Figure 1 is a screenshot of a trader's trading screen who receives information on the asset's BBV (informed trader). An uninformed trader in this situation would see the same screen except for the box containing "Buyback value 46.8".

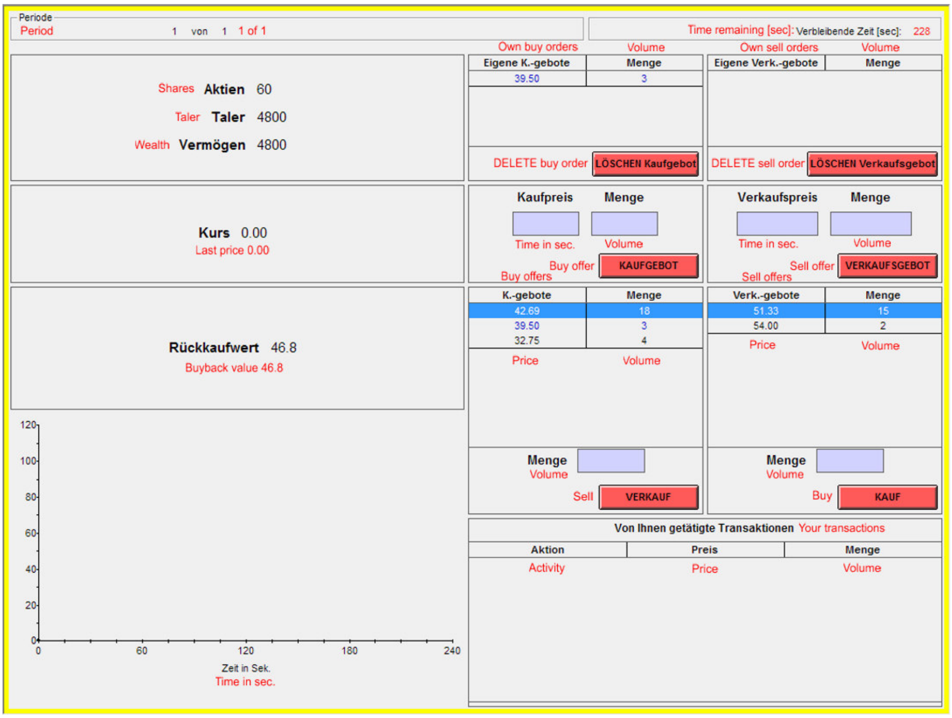

Figure 1 Screenshot Trader. The figure shows a sample trading screen. The red captions are translations of the German original.

In order to account for the impact of market size on our results, we run markets with different numbers of traders. In any of the ten periods, there may be either one or two markets operating in parallel. In other words, there is either one market in a given period, or there are two independent markets which do not interact and which run simultaneously. Each market is populated by $n \in \{2,3,\ldots,9\}$ traders. In each period, $i \in \{0,1,\ldots,n\}$ traders are exogenously informed of the exact BBV. Uninformed traders know only the range and distribution of BBV, implying an unconditional expected BBV of 57.5 taler.[1] We do not levy a charge for receiving inside information.[2] The number and identity of uninformed and informed traders varies with each period. This structure is illustrated in Figure 2.

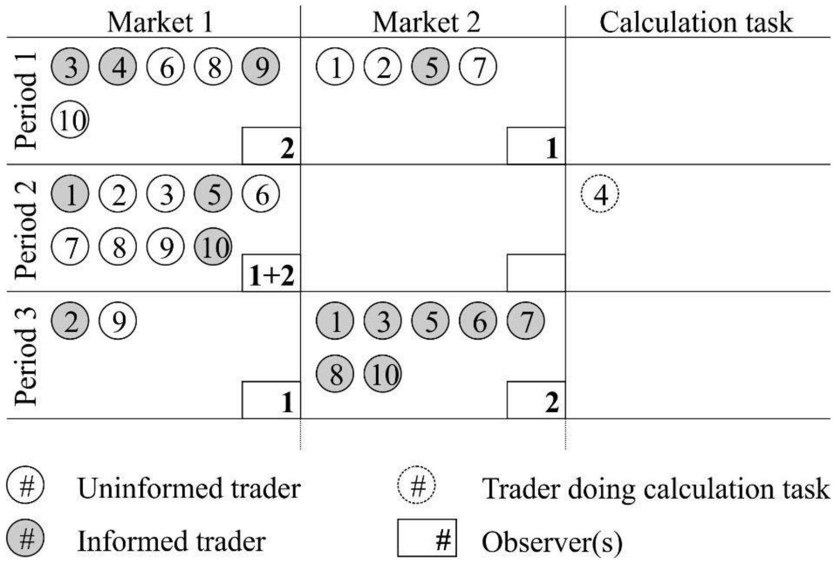

Figure 2 Period structure. The figure illustrates the period structure.

Table 1 gives an overview of the number of observations of each market makeup, defined as a specific combination of number of uninformed and number of informed traders. The sequences and combinations of market makeups within sessions were randomly drawn prior to conducting the sessions to ensure the predetermined number of observations per cell listed in Table 1. The variation is designed to allow for separating competition (between informed traders) and market size effects in the results. The range of possible market sizes and numbers of informed traders is common knowledge, but the probabilities associated with each market makeup is not. Traders also do not receive any explicit information regarding actual market size or number of informed traders during the experiment.[3]

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[1] Note that we chose to provide informed traders with a precise instead of a noisy signal of BBV since we expect that the latter would mainly have led to a reduction in the size of the treatment effect.

[2] Doing so would simply increase informed traders' profitability requirement for trades; see Huber, Angerer and Kirchler (2011) on the effects of information costs in an asymmetric information setting.

[3] Subjects were informed of the market size and the number of informed traders in the two training periods. They may also have been able to infer the number of market participants indirectly from observing market activity.

Table 1 – Overview Market Structure

The table lists the number of observations of each market makeup (specific combination of number of uninformed and informed traders). This table is the same for both treatments. There are 52 unique combinations and a total of 280 observations in each treatment.

		No. informed									
		0	1	2	3	4	5	6	7	8	9
No. uninformed	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									

Traders receive homogeneous endowments of 60 assets and 4800 taler. Due to the variation in BBV, the cash/asset-ratio ranges between 0.94 and 2.67. Each trader's end-of-period wealth is calculated by summing the cash balance and the value of the assets in the trader's inventory, valued at BBV.

In addition to their earnings from trading, traders can obtain income from two other sources. First, once each trading period has ended, traders are required to state their beliefs regarding the number of traders in total and the number of informed traders in the market just ended. For each instance where their answer matches the true number exactly, they earn an additional 500 taler (the maximum additional income from answering these questions is: 2 questions · 10 periods · 500 taler = 10000). Finally, in some periods it is not possible for all traders to participate in a market. Inactive traders are asked to solve as many multiplications of a two-digit number by a one-digit number as possible within the time limits of the trading period. They earn a fixed amount of 350 taler for each correctly solved calculation and can only progress to the next calculation after having correctly solved the current one. This calculation task is run to keep inactive subjects busy and to equalize earnings between active and inactive trader subjects. A trader's final earnings are calculated by summing all end-of-period wealth positions, adding the questionnaire and calculation task incomes, subtracting the minimum period buy-and-hold return of 66000 taler (4800 taler in cash, plus 60 assets evaluated at the minimum possible BBV of 30, for 10 periods) and converting this amount into euros at an exchange rate of 900 taler per euro. At the end of each period subjects receive information about their earnings in all periods to date.

1.2 Market observers

There are two subjects in each session who observe the markets without actively participating in trading themselves. In periods where there are two markets operating concurrently, each is observed by one observer subject. When there is only one market operating in a period, both observers view the same market.

Observers have access to a substantial amount of information to help them identify informed traders. Figure 3 shows a screenshot of their screen during the trading phase. Observers have access to publicly available information, augmented by additional pieces of information. Specifically, observers can observe the order book, the last price and the price chart. In addition to this information, which is also available to traders, observers see a trader code next to each entry in the order book. This code is a randomly chosen letter-and-number combination and is unique for each subject and within each period (in other words, trader codes change from period to period and each code is used only once during a session). The code allows observers to track an individual trader's actions during a trading period, while conserving trader anonymity and precluding the possibility of tracking traders between periods.

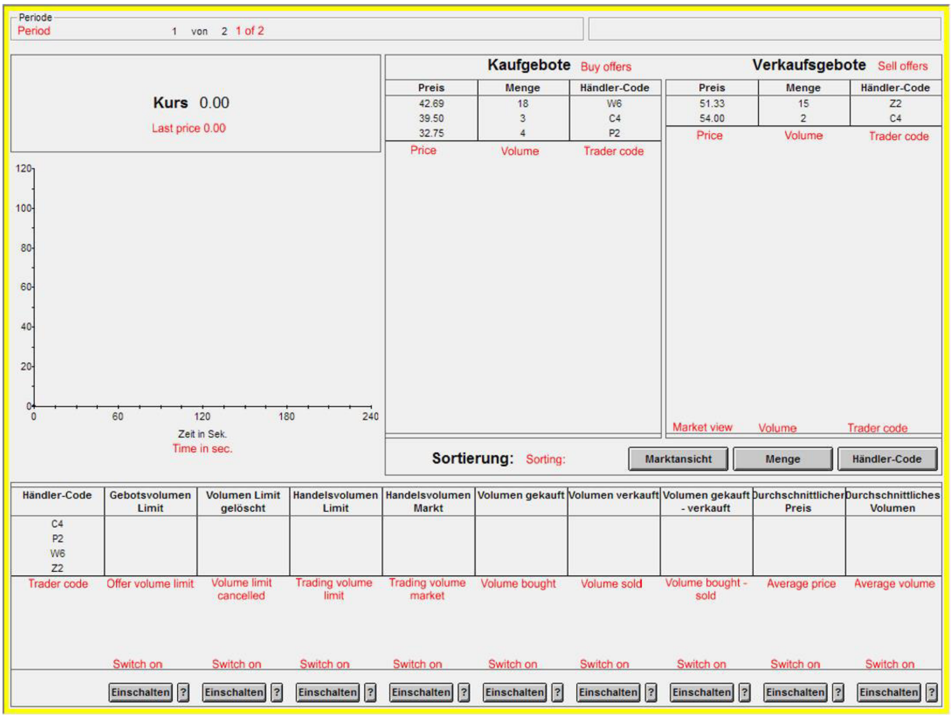

Figure 3 Screenshot Observer. The figure shows a sample observer screen. The red captions are translations of the German original.

In addition to the real-time order book data, observer subjects can view summary real-time information about traders' actions in the period at the bottoms of their screens. This summary information consists of nine items, presented in real time and listed separately for each trader, identified by the trader code. Observers can only access three out of the nine information items simultaneously, however. Viewing the items is free and they can switch between different ones as often as they like. The rationale underlying the three-item limitation is that it allows us to obtain data regarding which items observers consult to help them with their task.

Observer subjects never interact with each other. They receive a base compensation of 15 euros for their participation in the experiment. Based on their observations during each trading period, they are tasked with answering three questions after trading has concluded for the period. First, they are asked to state their belief regarding the number of informed traders who were present in the market just ended. They receive 2 euros for a correct answer. Second, they are asked to identify which traders in the market they *suspect* of having been informed. They receive 0.5 euros for each informed trader they suspect, while they lose 0.5 euros for each uninformed trader they suspect. Third, they are asked which traders they wish to *select* as having been informed. They receive 1 euro for each trader they correctly select as having been informed, while they lose 2 euros for each trader incorrectly selected (i.e., selected even though the trader was not informed).

Note that the first observer question is the same as the second question asked of the traders, while observer questions two and three are new. The difference between questions two and three lies in the compensation scheme only. While an incorrect answer on question two leads to a loss of the same magnitude as the gain in the case of a correct answer (±0.5 euros), the potential loss is twice as large as the potential gain in question three (-2 euros vs. +1 euro). As we show in appendix chapter 5.2, risk-, ambiguity- and loss-neutral observers maximize their expected utility by *suspecting* a trader of having been informed if they estimate the likelihood of the trader having been informed to be greater than ½. They maximize their expected utility by *selecting* only traders whose likelihood of having been informed they estimate to exceed ⅔.[4] Risk-, ambiguity- or loss-averse subjects would tend to err on the side of caution, i.e., to require higher threshold probabilities to be willing to suspect or select.

1.3 Treatments

Apart from the different market compositions, we run two treatments in our experiment. Treatment NOLEG is the baseline. It is characterized by no interaction be-

[4] The motivation for this compensations scheme, which punishes erroneous selections of subjects as being informed, is that regulators outside of the lab likely also have strong incentives not to wrongly accuse traders of being insiders. Put differently, we expect regulators like the SEC to only charge traders with insider trading which they have a solid case against – a case which they judge likely to have a substantial chance of leading to a conviction in court.

tween traders and observers. Treatment LEG implements insider trading legislation by tying fines for insider trading to the observers' answers on question three. Our legal system is modeled after the U.S. Securities and Exchange Commission's (SEC) procedure referred to as the "contemporaneous trader rule" (15 U.S. Code § 78t–1). It rules that investors who trade in the same instruments as an insider are usually entitled to compensation. In particular, insiders' trading profits are redistributed to investors following a pro rata scheme based on the number of shares they bought (if the insider profited by selling) or sold (if the insider profited by buying), relative to the total number of shares bought or sold by all claimants, on the same day.

In our experiment the rule is implemented as follows: Informed traders who are selected by the observer in the LEG treatment lose their trading profits, which are defined as the positive difference between the informed trader's wealth at the end minus the wealth at the beginning of the period. We refer to the wealth lost by any individual informed and selected trader as a fine. All fines in a market are redistributed to those traders who were either not, or incorrectly selected as having been informed. In other words, to those traders who were had not been "proven" to have been informed and who were thus considered to be uninformed.[5] The amount redistributed to each trader considered to be uninformed was proportional to the number of shares bought or sold at a loss by this trader.

1.4 Implementation

We conducted 16 sessions per treatment, yielding 280 market observations per treatment (560 in total). The number of repetitions for each combination of informed/uninformed traders varies as reported in Table 1. We decided to put higher weight on markets with fewer traders as these are usually more prone to idiosyncratic risk. The sessions were conducted in September, October and December 2013 in the Innsbruck-EconLab at the University of Innsbruck and totaled 384 students (bachelor and master students from different fields).[6] Most subjects had experience in other economic experiments but each participated in only one session of this study. The software was programmed in z-Tree 3.3.12 (Fischbacher, 2007) and subjects were recruited using ORSEE (Greiner, 2004).

At the beginning of each session subjects had about 20 minutes to study the written instructions on their own. This was done to minimize any possible experimenter bias. Afterwards, the trading mechanism and the most important screens were explained in detail, followed by two trial periods to allow subjects to become familiar

[5] In markets where all traders were informed and all were selected as having been informed, the fines were levied, but not redistributed. To some degree this mimics the SEC's fallback plan of sending funds from fines to the U.S. Treasury when it is "not economically practical or efficient to identify investor claimants and provide them with notice" (Flynn, 1992, 121).

[6] Palan and Stöckl (2017) use data from the same experiments, but report mainly on the trading behavior, market characteristics, and profitability of informed and uninformed subjects.

with the experimental procedure and the payment schemes.[7] In these trial periods all ten traders interacted in a single market, each once in the role of an informed and once in the role of an uninformed trader.

Each session lasted approximately two hours. In NOLEG, traders earned on average EUR 21.01 (standard deviation 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).

2 Hypotheses

We use the data gathered in the experiment to test several hypotheses which we structure into those pertaining to the behavior of the observers and those pertaining to the behavior of informed traders.

2.1 Observer behavior

Our first hypothesis focuses on whether observers succeed in their task of identifying informed traders. Given the information observers have regarding traders' activity in the market, we hypothesize first that the traders, observers suspect of being informed, are actually more likely to have been informed than traders that have not been suspected of being informed. Note that we implicitly assume (1) that informed traders make use of their information, (2) that informed traders behave differently compared to uninformed traders when using their information, and (3) that observers succeed in recognizing the differences in behavior.[8]

H1a Traders *suspected* of being informed are more likely to have been informed than traders not suspected.

We hypothesize second that a trader *selected* as being informed is more likely to actually be informed than a trader only *suspected* of being informed.

H1b Traders *selected* as being informed are more likely to have been informed than traders *suspected* of being informed.

Finally, we expect that informed traders will try to conceal their information status in treatment LEG in order to avoid losing their trading profits. If successful, such

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[7] Note that we took care to frame our instructions neutrally in order to (1) minimize experimenter demand effects and (2) avoid a potential interplay with subjects' moral values. Specifically, we refrained from using terms like "insider", "suing" or "illegal". See the online supplementary material for translated instructions.

[8] Palan and Stöckl (2017) show that price efficiency in these markets increases with the share of informed traders. Together with the observation that markets without informed traders exhibit levels of price efficiency close to a random trading benchmark this implies that informed traders indeed make use of their information. Moreover, the authors document some differences in trading behavior. Therefore, we assume (1) and (2) to hold and we investigate (3) in this paper.

concealment would decrease observers' probability of correctly identifying informed traders.

H2 Observers are more successful in detecting informed traders in NOLEG than in LEG.

2.2 Informed traders' behavior

Hornung et al. (2015) describe a possible channel of informed traders manipulating the market for their own benefit. They suggest that insiders use the open order book in double-sided call auction markets with open order books to submit prices far from the asset's real value in order to lure uninformed traders to adjust their own offer prices accordingly. They argue that insiders then cancel their own misleading orders in the last minute before the end of order submission and the start of call auction price determination, and profit from the indirect price impact their actions induced by moving the prices of uninformed traders. If, for example, they succeed in manipulating uninformed traders into posting buy offers with prices exceeding the asset's value, they themselves post sell offers to profit from the overvaluation. A market observer could infer the identity of an insider following this strategy from the number of limit orders cancelled late in the order submission phase.

This strategy is not directly transferable to our setting, as we do not employ a double-sided call auction market with an open order book. In our continuous double auction institution, traders who try to manipulate the market by posting misleading orders run the risk of these orders being accepted and executed before they have the chance to cancel them. Nonetheless, there are strategies by which informed traders may try to move prices in the direction they consider favorable even in a continuous double auction. If informed traders for example know BBV to be greater than the current market price, they may submit buy orders for very low prices in the hope that uninformed traders will view such orders as indicative of information that BBV is relatively low. If prices consequently go down, informed traders could buy more cheaply. Informed traders could safely leave such low-price buy orders in the order book indefinitely, which suggests that they may show up in the order book as open orders at market close. Alternatively, and still assuming that informed traders would like to depress the market price, they could submit sell orders with prices incrementally higher than the current best bid, which they quickly cancel again before they get executed. This would suggest to the market that there is some supply at relatively low prices, which may induce uninformed traders to lower their prices.

How would the two strategies just described show up in market data? One of them implies that informed traders cancel a greater proportion of their orders than uninformed traders. The other implies that a greater proportion of informed traders' orders remain open at the close of trading than would be the case for uninformed traders' orders. To detect these strategies, we hence propose the following two hypotheses:

H3 Informed traders submit more limit order volume which they later cancel than do uninformed traders.

H4 Informed traders submit more limit order volume which remains unexecuted at the close of the market than do uninformed traders.

Note that finding support for H3 constitutes relatively strong evidence for manipulative behavior by informed traders. If informed traders did not try to manipulate prices, we would expect them to cancel fewer orders than do uninformed traders. Since they know the exact BBV, they are at all times able to submit limit orders certain to be profitable when executed. In other words, they would tend to make fewer mistakes, thus necessitating fewer order cancellations. Consequently, a significantly higher share of order cancellations among informed traders would constitute strong evidence of manipulative attempts. Note, however, that there are two arguments suggesting a low likelihood of informed traders manipulating the market. First, informed traders are uninformed about the number of other informed traders and thus are limited to estimating the level of competition. Therefore, informed traders engaging in manipulative attempts run the risk of earnings potentials being "arbitraged away" by competing informed traders. Indeed, Palan and Stöckl (2017) document that price efficiency increases with the level of competition. Second, manipulative attempts increase the risk of being detected by observers, as manipulation requires "suspicious" (i.e., non-standard) behavior. This is pertinent in Treatment LEG.

3 Results

3.1 Observer behavior

We start our analyses by evaluating how many traders observers suspect and select. If they react correctly to the incentives provided, we would expect that they suspect a greater number of traders than they select. We consider this a first robustness check of our incentivization and of observers' understanding of the instructions. We find that, averaged over all market types, observers *suspect* 33.0%, and *select* 18.7%, of all traders as having been informed. These values are significantly different from each other (paired $t(103) = 16.717$, $p = 0.0000$), supporting the implications intended by our incentive scheme.

We next turn to our first hypothesis and find mixed evidence. Concerning H1a (Traders *suspected* of being informed are more likely to have been informed than traders not suspected) we find that traders suspected of being informed are actually informed with a probability of 55.8%. This is significantly higher than the unconditional probability of any trader being informed, which is 50.0% (test of the share of informed traders suspected among all traders suspected vs. the share of informed traders among all traders, by market makeup, paired $t(103) = 4.887$, $p = 0.0000$), supporting H1a.

Result 1: Traders *suspected* of being informed are significantly more likely to have been informed than traders not suspected.

However, concerning H1b (Traders *selected* as being informed are more likely to have been informed than traders *suspected* of so being) we find that traders selected as being informed are actually informed with a probability of 56.3%, which is not significantly higher than the 55.8% probability for suspected traders (test of the share of informed traders selected among all traders selected vs. the share of informed traders among all traders, by market makeup, paired $t(103) = 0.373$, $p = 0.7102$). This result indicates that observers only partly succeed in selecting those traders among the suspected that exhibit a higher probability of being informed. We thus fail to find support for H1b.

Result 2: Traders *selected* as being informed are not significantly more likely to have been informed than traders *suspected* of being informed.

Next, we conduct an in-depth analysis investigating observers' ability in identifying informed subjects as being informed. Table 2 provides an overview of *informed* subjects' probability of being identified as being informed by an observer. The numbers in the cells are the probabilities (in %) of being suspected of (panels a and b) or selected as (panels c and d) being informed, conditional on being informed. Panels (a) and (c) depict results for the NOLEG treatment while panels (b) and (d) depict data from the LEG treatment.

Table 2, panel (a) documents that the probability of being *suspected* is considerably higher for informed traders who are either alone or share their market with at most one additional informed trader in treatment NOLEG. The average normalized probability of being suspected is 52.2% for the first two columns, while it is 35.4% for the other columns. In the LEG treatment, depicted in panel (b), no such effect obtains. However, the effect extends to the probability of being *selected*. The average normalized probability of being selected is 43.8% for the first two columns, while it is 35.6% for the other columns in panel (c), which depicts NOLEG data (the differences mentioned for both suspecting and selecting are significant at the 0.001 level in both *t*- and ranksum tests). Again, there is no such effect in the LEG treatment, shown in panel (d).

Table 2 – Probability of Being Correctly Suspected or Selected

In all panels of the table, the horizontal (vertical) axis contains the number of informed (uninformed) traders in a market. The values in the cells are the probability of an informed trader's being suspected in %. Panel (a) shows the probabilities of an informed trader's being suspected in the NOLEG treatment. Panel (b) shows the same information for the LEG treatment. Panels (c) and (d) show the same data for the probabilities of being selected. The shading reflects the values in the cells (higher value = darker shade).

(a) Probability (%) of being suspected given informed status, NOLEG

		Informed									
		1	2	3	4	5	6	7	8	9	Avg.
Uninformed	0		54.2	45.8	67.9	43.3	56.7	39.3	12.5	29.2	43.6
	1	25.0	43.8	52.4	29.2	48.0	20.8	21.4	37.5		34.8
	2	62.5	64.3	11.1	12.5	25.0	45.8	39.3			37.2
	3	57.1	50.0	20.0	25.0	40.0	45.8				39.7
	4	50.0	75.0	41.7	31.3	37.5					47.1
	5	60.0	37.5	33.3	37.5						42.1
	6	75.0	50.0	41.7							55.6
	7	50.0	43.7								46.9
	8	37.5									37.5
Avg.		52.1	52.3	35.1	33.9	38.8	42.3	33.3	25.0	29.2	41.5

(b) Probability (%) of being suspected given informed status, LEG

		Informed									
		1	2	3	4	5	6	7	8	9	Avg.
Uninformed	0		50.0	37.5	42.9	33.3	53.3	28.6	59.4	27.8	41.6
	1	41.7	68.7	42.9	41.7	48.0	33.3	32.1	28.1		42.1
	2	12.5	71.4	16.7	68.7	25.0	25.0	26.8			35.2
	3	57.1	50.0	33.3	56.3	30.0	35.4				43.7
	4	66.7	37.5	41.7	37.5	35.0					43.7
	5	20.0	62.5	41.7	15.6						34.9
	6	50.0	12.5	20.8							27.8
	7	25.0	37.5								31.2
	8	37.5									37.5
Avg.		38.8	48.8	33.5	43.8	34.3	36.8	29.2	43.8	27.8	39.1

(c) Probability (%) of being selected given informed status, NOLEG

	Informed									
Uninformed	1	2	3	4	5	6	7	8	9	Avg.
0		37.5	16.7	32.1	16.7	10.0	10.7	6.3	20.8	18.8
1	25.0	31.2	28.6	12.5	16.0	4.2	7.1	23.4		18.5
2	62.5	42.9	11.1	12.5	20.0	29.2	32.1			30.0
3	28.6	50.0	13.3	25.0	25.0	22.9				27.5
4	16.7	25.0	41.7	6.3	30.0					23.9
5	20.0	25.0	0.0	6.3						12.8
6	50.0	50.0	12.5							37.5
7	50.0	25.0								37.5
8	12.5									12.5
Avg.	33.2	35.8	17.7	15.8	21.5	16.6	16.7	14.8	20.8	23.7

(d) Probability (%) of being selected given informed status, LEG

	Informed									
Uninformed	1	2	3	4	5	6	7	8	9	Avg.
0		45.8	4.2	35.7	16.7	13.3	14.3	53.1	19.4	25.3
1	16.7	50.0	23.8	12.5	32.0	12.5	21.4	21.9		23.8
2	0.0	42.9	11.1	37.5	20.0	16.7	21.4			21.4
3	42.9	16.7	33.3	43.8	5.0	27.1				28.1
4	33.3	25.0	16.7	25.0	32.5					26.5
5	20.0	25.0	33.3	9.4						21.9
6	50.0	12.5	12.5							25.0
7	25.0	12.5								18.8
8	25.0									25.0
Avg.	26.6	28.8	19.3	27.3	21.2	17.4	19.0	37.5	19.4	24.3

To investigate the impact of the level of competition among informed traders in greater detail, we run the regressions documented in Table 3. The regressands are the probabilities of informed traders being suspected (column 1) or selected (column 2). We control for the treatment condition (LEG) and use dummy variables for the number of informed traders in the market, using 5 (i.e., the middle between 1 and 9) as the reference level.

Table 3 – Detection Probability Regressions

The table presents OLS regressions of the probability of informed traders being suspected or selected by observers on a number of dummy variables. LEG is a dummy equaling 1 in the LEG treatment. X Informed is a dummy variable equaling 1 when the number of informed traders in the market equals X.

Regressors	Suspected	Selected
LEG	-0.023	-0.004
	(0.061)	(0.045)
1 Informed	0.068	0.074
	(0.065)	(0.070)
2 Informed	0.151	0.141
	(0.063)**	(0.059)**
3 Informed	-0.023	-0.031
	(0.042)	(0.052)
4 Informed	0.031	0.011
	(0.058)	(0.050)
6 Informed	0.033	-0.044
	(0.073)	(0.066)
7 Informed	-0.059	-0.032
	(0.082)	(0.068)
8 Informed	-0.028	0.051
	(0.048)	(0.053)
9 Informed	-0.087	-0.009
	(0.097)	(0.066)
Constant	0.383	0.213
	(0.038)***	(0.032)***
R^2	*0.04*	*0.04*
Adj. R2	*0.02*	*0.02*
F	*6.65*	*7.44*
p	*0.0007*	*0.0004*
N	*460*	*460*

* $p<0.1$; ** $p<0.05$; *** $p<0.01$

Standard errors clustered at the level of 16 cohorts each in parentheses.

The regression results find a significant effect only for markets with two informed traders, with the coefficient of the dummy variable for two informed traders being considerably larger than those of the other dummies for the number of informed traders. This confirms the impression from Table 2 that observers have a harder time identifying informed traders in the presence of more than two informed traders. At the same time, it is mild evidence that a minimum of competition between informed traders may help observers identify informed traders. One argument explaining this result is that it becomes more difficult for informed traders to coordinate on collusive behavior with an increasing number of competitors. Consequently, when informed traders perceive it to be likely that they are facing a high level of competition, they may pursue strategies that lead to a quick erosion of earnings potential. This behavior reduces potentially profitable trading strategies and may make it easier for uninformed traders to copy informed traders' strategies. Therefore, increasing competition complicates the identification of informed traders.

To gain more insights into observers' skills in identifying informed traders, we now compare their detection ability to what we will dub "zero-intelligence observers", in allusion to Gode and Sunder (1993). We do so only for observers' ability in *selecting* informed traders in order to gain insights into the ability of the latter to hide their information status. First, we assign, separately for each period, one point to every observer for each informed trader the observer correctly selected as having being informed. Second, we divide an observer's score of points by the total number of informed traders in the market. We thus obtain the likelihood of a randomly chosen informed trader to have been selected by the observer. Third, we calculate the average of this likelihood by observer and market for each market makeup (Table 1 shows market makeups).

We use the average likelihood obtained in this manner as our measure of human observer ability. Using computer simulation, we calculate a zero-intelligence observer score for each market type as follows. First, we take the average number of *traders* human observers selected in the market type. Second, we divide this number by the total number of traders in this market makeup. Doing so yields the average proportion of traders human observers selected in this market makeup. Third, we multiply this proportion by the share of informed traders in a market. This procedure yields our zero-intelligence trader "ability" score. Conceptually, this means that, for each market type, each informed trader is selected by zero-intelligence observers with a probability equal to the overall fraction of traders selected out of all traders by the human observers in the same market makeup. Finally, we subtract the zero-intelligence observer score from the human observer score to obtain a measure of human outperformance of zero-intelligence observers.

We find that this outperformance measure is strictly greater than zero – implying that humans outperformed zero-intelligence observers – in 28 (21) out of 36 market makeups in treatment NOLEG (LEG). We interpret this as further evidence supporting H1a. At the same time, the treatment difference indicates that human observers are significantly better at identifying informed traders than zero-intelligence observers in NOLEG ($t(35) = 4.116$, $p = 0.0002$; binomial test $p = 0.0012$), but only marginally so in LEG ($t(35) = 1.896$, $p = 0.0662$; binomial test $p = 0.4050$). Comparing these proportions (28/36 vs. 21/36) using Fisher's exact test does not find the difference to be significant.

Result 3: Human observers are more successful in identifying informed traders than are zero-intelligence observers.

Additional evidence is documented in Table 2, showing that there are little treatment differences in informed traders' probabilities of being suspected (41.5% in

NOLEG vs. 39.1% in LEG) or selected (23.7% in NOLEG vs. 24.3% in LEG). At the same time, the observers react to the differences in incentives and select significantly fewer subjects than they suspect. Specifically, they *suspect* 31.8% of all (i.e., informed and uninformed) subjects in NOLEG and 34.2% in LEG (n.s.), while they *select* only 16.3% in NOLEG and 21.2% in LEG ($t(102) = 2.7006$, $p = 0.0081$).

In a final analysis, we study observers' use of available information. Figure 4 shows the total times the observers' screen showed each of the nine trading activity summary items during the continuous double auction phase in the market.[9] Five items appear to carry the greatest relevance for observers. These are the limit order volume (LOV), volume bought (VB), volume sold (VS), the difference between the latter two (VD), and average price (AP). Discounting the redundant information in VD, we thus conclude that observers believe that they can identify informed traders – if at all – from the volume of shares they offer to trade using limit orders, from the actual volume of shares they buy and sell, and from the average prices they trade at. Conversely, they exhibit little interest in the split between limit and market trades (LTV, MTV), the average trading volume (AV), and the number of limit orders which subjects cancelled prior to execution (CO). This observation indicates that observers do not believe, as conjectured in H3, that informed traders would submit more limit orders which they would later cancel.

Result 4: Observers try to identify informed traders using (1) the volume of shares offered for trade using limit orders, (2) the volume of shares bought and sold, and (3) average trading prices. Observers do not rely on information regarding the volume of limit orders a trader cancelled.

[9] Figure A1 in Appendix 5.1 illustrates observers' viewing decisions separately for the market trading phase, for the stage when observers estimate the number of informed traders in the market, for the stage when observers indicate which traders they suspect of having been informed, and for the stage when observers indicate which traders they wish to select as being informed. The results are qualitatively comparable.

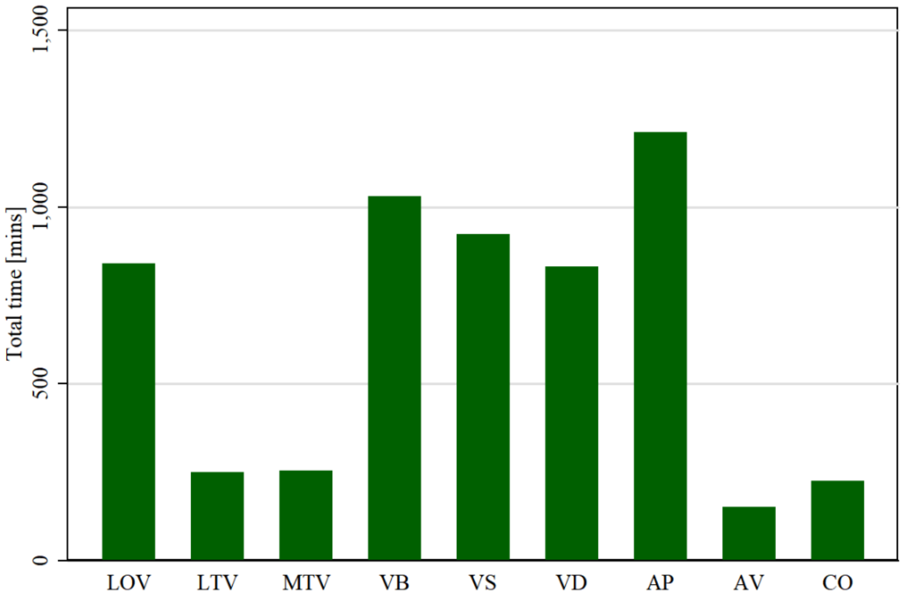

Figure 4 Viewing times of summary trading data. The figure shows the total time (in minutes) the observer's screens displayed the nine trading data items during the market trading phase. LOV stands for limit order volume, LTV is limit transaction volume, MTV is market transaction volume, VB is volume bought, VS is volume sold, VD is the difference between VB and VS, AP is a subject's average trading price, AV is a subject's average trading volume, and CO is the volume of limit orders cancelled by a subject.

3.2 Informed traders' behavior

In our first analysis of informed traders' behavior we follow up on the impressions from Figure 4 by relating trader subjects' trading data summary characteristics to being informed or not. We furthermore analyze observers' beliefs about these statuses. Table 4 presents the results from five regressions investigating which characteristics of subjects' trading behavior identified them as informed or as uninformed traders. We omit the difference between the volume bought and sold as well as market order trading volume, as they would introduce multicollinearity.[10] The first two results columns reveal which elements of subjects' behavior rendered them more likely to be suspected (column 1, Suspected) or selected (column 2, Selected) as being informed by an observer. The third column (IsInformed) reveals which behaviors were really correlated with subjects' being informed. The final two columns present the determinants of observers' payoffs in EUR per trader suspected (0.5 or -0.5, ProfSusp), selected (1 or -2, ProfitSel), or neither suspected nor selected (0). As the significance patterns make clear, several variables play a role.

[10] With volume bought and volume sold, and with the limit trading volume and volume bought and volume sold, respectively.

First, we do not detect a treatment difference in the profitability of observers' actions. Neither coefficient of LEG in the rightmost two columns is significantly different from zero. In fact, in the case of the final column, the coefficient even bears the wrong sign. These results contrast with H2 suggesting that observers are more successful in detecting informed traders in NOLEG than in LEG. Thus, we find no evidence of a treatment difference in observer profitability.[11]

Result 5: Contrary to H2, there is no evidence that observers earn higher profits from detecting informed traders in NOLEG than in LEG.

Second, the volume of limit orders submitted by a subject (LimitVol) is highly significantly positively correlated with their being suspected of, and selected as, informed by observers. Furthermore, observers rightly heed this variable, since its coefficient in the regression of subjects' actually being informed is also highly significantly positive. This relationship is also reflected in ProfitSusp, if not in ProfitSel. ProfitSusp shows that subjects are right in suspecting high LimitVol subjects. ProfitSel paints a more nuanced picture. While columns 1 through 3 show that high LimitVol traders are more likely than low LimitVol traders to be, and to be selected as being, informed, the observers seem to insufficiently account for the large difference in absolute payoffs for correct and incorrect selections to yield a coefficient of ProfitSel significantly greater than zero. This failure of observers to sufficiently account for the cost of incorrectly selecting high LimitVol traders may be caused by overconfidence in the form of miscalibration. Our data unfortunately does not allow us to test this conjecture.

Third, informed traders appear to trade significantly fewer shares using limit orders. However, this pattern is not picked up by the observers. On the contrary, there is mild evidence that observers are more likely to select subjects as being informed when they have a high volume in limit trades. The positive coefficients in the case of Suspected and Selected and the negative coefficient in the case of IsInformed are aggregated in ProfitSusp and ProfitSel, which show that the observers significantly reduce their profits by suspecting and selecting high TradeVolLimit traders.

Fourth, PurchasedVol exhibits a similar picture as LimitVol, albeit with slightly lower significance levels. Informed traders buy more than uninformed traders, and observers correctly identify them based on this characteristic.

Fifth, observers suspect traders with low SoldVol to be informed. However, this impression is not well-founded, as there is no significant relationship between subjects' being informed and the volume of shares they sold. This results in a ProfitSusp value indistinguishable from zero, with a weakly significant negative coefficient in the regression of ProfitSel.

[11] As pointed out in the analysis leading to Result 3, we also find no significant treatment difference in the outperformance of zero-intelligence observers by humans.

Table 4 - Identifying Behavioral Characteristics

The table presents probit and OLS regressions of observer behavior and informed status on summary variables of trader behavior. The regressands Suspected and Selected are binary variables indicating whether a trader was suspected of, or selected as, being informed by an observer. The regressand IsInformed is a binary variable indicating whether the subject was in fact informed in the period in question. ProfitSusp and ProfitSel are the observer's profits in euros from suspecting or selecting (or not) a subject as being informed. LEG is a binary variable indicating whether the observation stems from a legislation treatment session and NumTraders controls for market size. The remaining regressors contain the number of shares a subject has offered to buy or sell using a limit order (LimitVol), the number of shares stemming from such a limit order which a subject subsequently cancelled (CancelledVol), the number of shares a subject traded as a result of limit orders (TradedVol), the number of shares a subject bought and sold (PurchasedVol and SoldVol), and the average price and volume of the subject's trades (AvgPrice and AvgVol). The Suspected, Selected and IsInformed columns display results from probit regressions, while the ProfitSusp and ProfitSel columns contain OLS estimates.

Regressors	Supected (Probit)	Selected (Probit)	IsInformed (Probit)	ProfitSusp (OLS)	ProfitSel (OLS)
LEG	0.112	0.244	0.095	0.001	-0.028
	(0.120)	(0.145)*	(0.123)	(0.018)	(0.038)
Num	-0.038	-0.028	0.011	-0.002	0.008
Traders	(0.016)**	(0.017)	(0.019)	(0.003)	(0.007)
LimitVol	0.004	0.004	0.005	0.001	0.001
	(0.001)***	(0.001)***	(0.001)***	(0.000)**	(0.000)
Cancelled	-0.000	-0.001	0.001	0.000	0.000
Vol	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
TradeVol	0.004	0.003	-0.008	-0.001	-0.002
Limit	(0.002)*	(0.002)*	(0.002)***	(0.000)**	(0.001)***
Purchased	0.005	0.004	0.003	0.001	0.001
Vol	(0.002)**	(0.002)*	(0.002)*	(0.000)**	(0.001)*
SoldVol	-0.004	-0.001	-0.001	-0.000	-0.001
	(0.002)**	(0.002)	(0.002)	(0.000)	(0.001)*
AvgPrice	0.002	0.003	0.005	0.001	0.002
	(0.002)	(0.002)	(0.002)**	(0.000)*	(0.001)*
AvgVol	0.006	0.002	0.027	0.003	0.004
	(0.007)	(0.007)	(0.008)***	(0.002)*	(0.002)
Constant	-0.691	-1.285	-0.757	-0.051	-0.222
	(0.163)***	(0.180)***	(0.209)***	(0.037)	(0.090)**
(Pseudo) R^2	*0.05*	*0.05*	*0.04*	*0.02*	*0.01*
Adj. R^2				*0.02*	*0.01*
p	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*
N	*3,142*	*3,142*	*3,142*	*3,142*	*3,142*

* p<0.1; ** p<0.05; *** p<0.01. Standard errors clustered at the level of 32 cohorts in parentheses.

Sixth, AvgPrice and AvgVol are both positively and significantly related to subjects' being informed. Similar to the case of TradeVolLimit, observers do not recognize this sufficiently, such that while positive, the coefficients for the cases of Suspected and Selected are insignificant. Nonetheless, the coefficients in the regressions of subjects' profits are weakly significantly positive.

Seventh, the results in Table 4 allow us to evaluate H3, suggesting that informed traders submit more limit order volume which they later cancel than do uninformed traders.[12] The results presented in the table, however, contain no evidence that informed traders are more likely to cancel even though, considering aggregate data, informed traders cancel 27.29% of the total order volume they submit, while uninformed traders cancel 20.06%. This difference is significant in Mann-Whitney ranksum tests ($z = 7.603$, $p = 0.000$). To substantiate these preliminary findings, we run probit regressions to investigate the determinants of the probability of an order's being cancelled by the close of trading. The results are reported in the first content column of Table 5. We find that informed traders seem to cancel a somewhat higher proportion of limit orders, but that the coefficient is only marginally significant. Furthermore, all interaction terms of TraderInformed with LEG and LimitVol are insignificant. Overall, we consider the evidence regarding hypothesis H3 to be mixed.

Result 6: There is no clear evidence either supporting or rejecting H3 that informed traders submit more limit order volume which they later cancel than do uninformed traders.

Figure 4 and Table 4 suggest that – whether informed traders in fact cancel more or not – observers do not base their decisions on order cancellations. Moreover, we find that the treatment does not significantly impact the probability of cancellations, while a higher number of traders in the market reduces the probabilities of orders being cancelled. There furthermore is weakly significant evidence that the probability of order cancellations increases over time (Table 5).

In H4 we conjectured that a greater proportion of informed traders' orders would remain open at the close of trading than would be the case for uninformed traders' orders. On the aggregate, we find that a total of 40.54% of total limit order volume submitted by informed traders remain open at market close, while the corresponding figure for uninformed traders is 37.00%. This difference is again significant in Mann-Whitney ranksum tests (orders remaining open: $z = 3.535$, $p = 0.0004$). To substantiate this preliminary finding, we run probit regressions to investigate the determinants of the probability of an order remaining open by the close of trading. The results are reported in the second content column of Table 5. We find that informed traders indeed leave a somewhat higher proportion of limit orders open at the end of trading, but the coefficient value is insignificant.

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12 Informed (uninformed) traders offer 62,308 (33,551) shares to trade in NOLEG and 40,259 (33,146) in LEG, respectively. See Palan and Stöckl (2017), section 3.1 for more details on trading behavior.

Result 7: There is no clear support for H4 that informed traders submit more limit order volume which remains unexecuted at the close of the market than do uninformed traders.

Table 5 – Probit Regression of Probability of Orders being Cancelled and Remaining Open

The table presents probit regressions of the probability of orders being cancelled prior to execution, and of the probability of orders remaining open at the close of trading. TraderInformed is a dummy variable equal to 1 if the trader submitting the limit order was informed.

Regressors	% Cancelled	% Open
TraderInformed	0.202	0.068
	(0.108)*	(0.088)
LEG	-0.235	0.072
	(0.116)**	(0.105)
TraderInformed × LEG	0.050	-0.105
	(0.133)	(0.155)
NumInformed	-0.055	-0.042
	(0.013)***	(0.010)***
NumUninformed	-0.038	-0.062
	(0.009)***	(0.009)***
Period	0.013	0.002
	(0.008)*	(0.006)
LimitVol	0.001	-0.003
	(0.001)	(0.001)***
TraderInformed × LimitVol	0.000	-0.000
	(0.001)	(0.001)
LEG × LimitVol	0.003	-0.001
	(0.001)***	(0.001)
TraderInformed × LEG × LimitVol	-0.001	0.001
	(0.001)	(0.001)
Constant	-0.595	0.188
	(0.076)***	(0.065)***
Pseudo R^2	*0.02*	*0.03*
p	*0.00*	*0.00*
N	*18,024*	*18,024*

* p<0.1; ** p<0.05; *** p<0.01
Standard errors clustered at the level of 32 cohorts (in parentheses).

The treatment does not significantly impact the probability of orders remaining open. Interestingly, the coefficient for the interaction between TraderInformed and LEG, while also insignificant, is negative and larger than that of TraderInformed, thus counteracting the effect of being informed alone at least in treatment LEG.

Finally, a higher number of traders in the market reduces the probability of orders remaining open significantly.[13]

4 Conclusion

We explore the ability of market observers to recognize trading based on inside information (informed trading) and to distinguish it from trading not based on such information (uninformed trading). We investigate which trading patterns are indicative of informed trading. Finally, we test how informed traders react to the threat of detection and punishment and how their behavioral response impacts observers' detection abilities.

We find that traders who observers suspect of being informed are significantly more likely to have been informed than traders observers do not suspect. We also find that human observers are more successful in identifying informed traders than are zero-intelligence observers. Observers try to identify informed traders using the volume of shares offered for trade using limit orders, the volume of shares bought and sold, and average trading prices. In the cases of information on the volume of limit orders submitted by a trader, and on the volume of shares purchased (irrespective of order type), this reliance is justified, since informed traders indeed submit significantly higher volumes via limit orders, and buy more shares. However, observers also believe that informed traders trade more shares via limit orders. In this they are mistaken, as the opposite is the case. Moreover, observers fail to capitalize on the fact that informed traders' transactions are on average characterized by higher prices and volumes per trade. Comparing the sessions without and with the threat of detection and punishment we find that there are no differences in an informed trader's probability of being suspected by the observers. Finally, we do not find support for widespread manipulation attempts by informed traders. Specifically, there is no difference between informed and uninformed traders in cancelled limit order volume or limit order volume remaining unexecuted at the close of the market. We conjecture that this may be due to the facts that attempts at manipulation increase the risk of (1) missing profitable opportunities due to competition from other informed traders, and of (2) being detected by observers.

Our study is the first to investigate differences in informed and uninformed market participants' trading behavior which lend themselves to identifying informed traders. Given many securities regulators' stated goal of deterring and detecting informed trading, gaining insights into such identification strategies is of considerable practical importance. We believe that we contribute a first piece of evidence on this question and hope that our results will spark further investigation into this topic. We furthermore establish the experimental method as a promising approach for gaining

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[13] OLS regressions of the share of orders being cancelled or remaining unfilled on the same regressors yield qualitatively similar results, except that the LEG coefficient becomes insignificant in the first model and weakly significant in the second. This suggests that this coefficient is borderline significant overall. Data available upon request.

insights into market activities which are hard to observe and identify outside the lab. Insider trading is forbidden in almost all modern capital markets and is thus exceedingly hard to investigate using empirical data. Nonetheless, the existence of insider trading is undisputed and is proven time and again by insider trading convictions. The experimental lab allows researchers to investigate the characteristics of the phenomenon under controlled conditions, where a trader's information status is fully observable and indeed, under the control of the experimenter. Of course, the experimental method is not without drawbacks. The advantage of laboratory experiments in terms of control and observability of information status and individual behavior comes at the cost of some abstraction from context that is present in capital markets outside of the lab. Experiments can thus only be one tool in finance researcher's toolkit. It is up to the careful scientist to make the best use of all tools he or she can bring to bear on a given problem.

5 Appendix

5.1 Additional Figures

Figure A1 contains four panels with information on observers' viewing behavior. Panel a) contains information on observers' viewing behavior during the continuous double auction phase in the market and is identical to Figure 4 in the body of the paper. Observers spend 240 seconds in this phase, observing the traders' actions in the market in real time. Once a market phase has concluded, observers are asked to estimate the number of informed traders in the market (Prediction General); the data from this phase are shown in panel b). They are then asked to indicate which traders they suspect of having been informed (Prediction Suspected); the data from this phase are shown in panel c). Finally, they are asked to indicate which traders they wish to select as having been informed (Prediction Selected); the data from this phase are shown in panel d). During the latter three tasks, observers can view the final results of the trading data summary items which they already observed during the trading phase. They thus receive no new information during the latter three tasks.

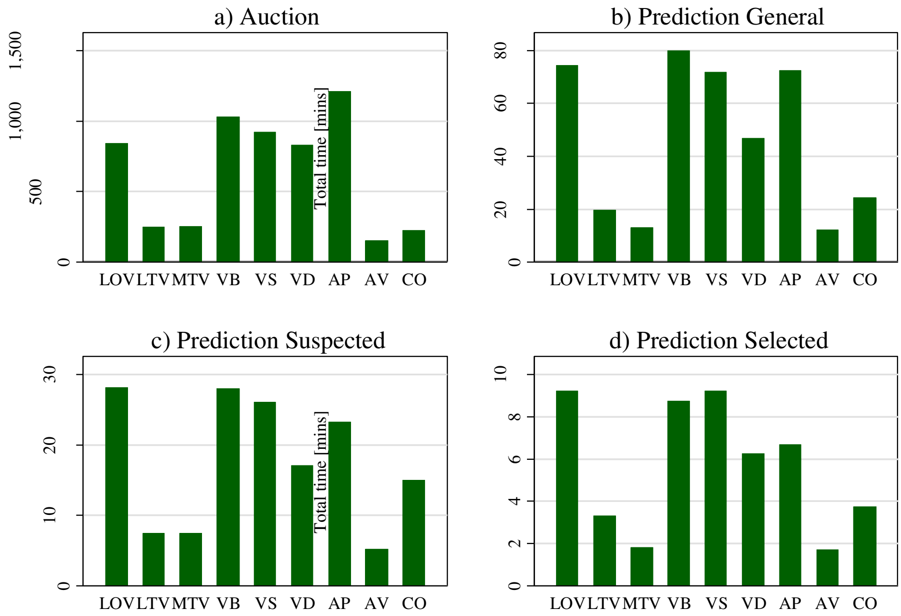

Figure A1 Viewing times of summary trading data. The figure shows the total time (in minutes) observer screens displayed the nine trading data items. LOV stands for limit order volume, LTV is limit transaction volume, MTV is market transaction volume, VB is volume bought, VS is volume sold, VD is the difference between VB and VS, AP is a subject's average trading price, AV is a subject's average trading volume, and CO is the volume of limit orders cancelled by a subject. Observers' viewing decisions in panel a) pertain to the market trading phase, in panel b) to when observers estimate the number of informed traders in the market, in panel c) to when observers indicate which traders they suspect of having been informed, and panel c) to when observers indicate which traders they wish to select as being informed.

5.2 Observer question parameterization

We design the payment scheme for observer questions two and three to ensure that subjects are incentivized to report their true beliefs, without incentives to for example randomize or to suspect/select e.g. all traders in order to maximize the expected payoff. Specifically, we start by designing question two 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 1/2.

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:

$$\mathrm{E}\left[\Pi_{susp}\right] = \pi_+ \cdot p + \pi_- \cdot (1-p) \tag{1}$$

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 informed as not.[14] 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 (1) to be positive for $p > 0.5$ and negative for $p < 0.5$. This yields the following relationship of the two possible payoffs:

$$\begin{aligned} &\mathrm{E}\left[\Pi_{susp}\right] = \pi_+ \cdot \tfrac{1}{2} + \pi_- \cdot \left(1 - \tfrac{1}{2}\right) = 0 \\ &\Rightarrow\ \pi_+ = -\pi_- \end{aligned} \tag{2}$$

This implies that the cost of falsely suspecting a subject of being informed should be equal to the profit from rightly identifying an informed trader 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 informed. Thus, there is a tradeoff: a higher threshold probability p decreases the danger of a type II error (not accusing an informed trader), but increases the danger of a type I error (accusing an uninformed trader). For our second measure (selecting a trader), 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 2/3 sure that a given subject is informed for her to accuse him. Again departing from equation (1) this implies:

$$\mathrm{E}[\Pi_{sel}] = \pi_+ \cdot \tfrac{2}{3} + \pi_- \cdot \left(1 - \tfrac{2}{3}\right) = 0$$

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[14] As can be calculated from Table 1, the actual ratio is a total of 724 uninformed to 736 informed traders, or 49.62% vs. 50.38%.

$$\Rightarrow \pi_+ \cdot \frac{2}{3} = \frac{2}{3} \cdot \pi_- - \pi_-$$
$$\Rightarrow \pi_+ = \pi_- - \frac{3}{2} \cdot \pi_-$$
$$\Rightarrow \pi_+ = -\frac{1}{2} \cdot \pi_-$$

In other words, the negative payoff for falsely selecting an uninformed trader should be twice as large as the positive payoff for selecting an informed trader.

5.3 Exit questionnaire

This section lays out the exit questionnaire 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]

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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]

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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][15]
   (0 = “Not at all willing to take risks” …10 = “Very willing to take risks”)

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

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

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

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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")
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)

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