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Voting for insider trading regulation. An experimental study of informed and uninformed traders' preferences[☆]

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ABSTRACT

Capital markets often regulate insider trading, but whether such regulation aligns with traders' preferences is an open question. This study examined traders' regulation preferences conditional on their prospects of becoming informed. Of 64 referenda, traders decided 41 (64%) against regulation. Moreover, traders' prospects of becoming informed significantly impacted the outcomes of the referenda. In markets in which a group of traders has no chance of receiving inside information, 47% of the referenda are decided against regulation. When all traders could get such information, 81% are. Individual votes reveal that traders who know they will remain uninformed support regulation in 69.27% of the cases, while informed traders do so only 8.33% of the time. Traders who may or may not become informed support regulation 33.33% of the time.

1. Introduction

Ever since regulation of insider trading was first proposed, backers and opponents have engaged in a debate over its pros and cons. Advocates of regulation argue that allowing insider trading reduces public confidence in a securities market's fairness and integrity when insider trading is allowed. They cite this as an argument for regulation against insider trading (Securities and Exchange Commission, 2021; European Union, 2014, preamble, para. 23). These advocates argue that a lack of confidence in unregulated securities markets would prevent investors from participating in such markets and highlight the potential harm that this reduced participation would cause the overall economy. Specifically, the lower participation reduces liquidity (Chen and Hao,

2011; Halling et al., 2007) and price efficiency (Fernandes and Ferreira, 2008; Fishman and Hagerty, 1992), and increases spreads (Bagehot, 1971; Frijns et al., 2008). In contrast, opponents of regulation highlight the contributions insider trading makes to price efficiency. They argue that informed trading allows prices to incorporate more private information (see, e.g., Arshadi, 1998; Cziraki and Gider, 2021; Palan and Stöckl, 2017).

The reality in financial markets shows that the backers of strict insider trading regulation have carried the day. Today most developed capital markets have implemented such regulation. The adoption process across countries, however, was slow. In their historical review, Bhattacharya and Daouk (2002) show that countries only

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gradually implemented laws regulating insider trading. The United States first established insider trading laws in 1934. Further countries followed the U.S. example slowly, and even by 1977, only 15 percent of the countries with stock exchanges had passed insider trading regulation. Over the next 30 years, the rate of increase accelerated significantly and by 2006, almost 83 percent of the countries with stock exchanges had such regulation (Bach and Newman, 2010). One particularly prominent late adopter was Germany, which did not pass insider trading laws until 1994 (Bhattacharya and Daouk, 2002).

In the adoption process of insider trading regulation, many stakeholders (political parties, regulatory authorities, financial interest groups, and others) communicate their interests. Moreover, Bach and Newman (2010) and Raustiala (2002) highlight the importance of transgovernmental networks for policy convergence. Given these considerations, it is not clear whether the diverse interests of these many stakeholders are in line with the interests of traders in financial markets. In fact, accumulating evidence raises doubts as to the effectiveness of insider trading regulation. If, however, its effectiveness is not assured, questioning the dominant prevailing practice may be justified.

In this paper, we contribute to this literature by studying traders' endogenous regulation choices. Conventional arguments suggest that uninformed traders would favor markets with a stricter legal environment, while informed traders would prefer markets with no regulation restricting informed trading.¹ Contrary to this assertion, various studies argue that uninformed traders benefit from competition between insiders because the insiders' activity in the market causes liquidity to increase and leads information to become publicly available. The resulting increase in price efficiency means that uninformed traders face less adverse selection risk when trading (Arshadi, 1998; Bainbridge, 2023; Cziraki and Gider, 2021; Palan and Stöckl, 2017). Moreover, even informed traders may prefer regulation, since it prevents competitors from impounding their information into prices too quickly, reducing insider profits in the process (Kim et al., 2019). The same argument applies to market professionals, who have an informational advantage over retail investors, but an informational disadvantage against insiders. Haddock and Macey (1987) develop this argument in a private-interest model based on the economic theory of regulation (see, e.g., Peltzman, 1976, 1989; Posner, 1974; Stigler, 1971, for historical contributions on this theory). Their model distinguishes between the preferences of insiders, of stock market professionals, and of outsiders. While outsiders are not well-organized in communicating their preferences to regulators (Haddock and Macey, 1987) and have opaque preferences (Beny, 2008), the first two groups typically express strong preferences regarding insider trading laws. In particular, corporate insiders' preferences typically are rival to those of market professionals because insiders gain earlier access to material, non-public information. Corporate insiders' unregulated actions thus diminish professionals' profits while professionals' efforts to obtain an informational advantage yield no return. We take this mixed evidence regarding traders' preferences for regulation as motivation to run incentivized experimental asset markets to evaluate whether current legal practices reflect traders' preferences for regulation. We do so to answer two research questions:

Research question 1. *Do traders prefer markets with no insider trading regulation over markets with insider trading regulation?*

Research question 2. *Do traders' prospects of becoming informed influence their preferences for insider trading regulation?*

To answer our research questions, we design an experimental asset market environment. In this environment, traders can vote in referenda which determine the legal rules that then govern traders' activity in the

experimental markets. We study traders' regulation choices conditional on these traders' prospects of obtaining superior information, and we can distinguish between decisions in two incentivized referenda. We use this design to control for traders' historical voting record, their past experience with different regulatory regimes, their expectations of what a regime change could look like, and their prior actions and performances in the market. Our approach thus relates to the experimental study of Bodoff et al. (2006), who employ a secret paper ballot to determine majority preferences. Their results suggest that traders with equal chances of receiving information prefer markets in which every second trader receives information over markets in which nobody receives information, and prefer the latter over markets in which everyone receives information. However, Bodoff et al. (2006) study preferences regarding insider trading from the aspect of information asymmetry and do not elicit preferences for insider trading regulation itself.

Evaluating the outcomes of 64 incentivized referenda collected in the 32 sessions of our experiment, we find that traders decide 23 (36%) in favor of regulation and 41 (64%) against. While these numbers suggest that traders prefer unregulated markets, these preferences vary with traders' prospects of becoming informed. In particular, in the 16 sessions in which two thirds of the traders have no chance of receiving inside information, traders decide 15 referenda (47%) in favor of regulation and 17 (53%) against. This result significantly contrasts with the results of the 16 sessions in which all traders have a chance of receiving inside information. In these sessions, traders decide six (19%) referenda in favor of regulation and 26 (81%) against. This treatment difference is driven by traders who are predetermined to remain uninformed in future periods. Analyzing individual voting behavior, we find that these traders vote in favor of regulation in 69.27% of all cases, while traders predetermined to receive information do so in only 8.33% of all cases. Traders without a predetermined type, who have a chance but are not guaranteed to receive information, vote in favor of regulation 33.33% of the time. Finally, we find no significant or substantial impact of traders' distributional (i.e., social) preferences on their voting behavior.

2. Method

In our laboratory setting, ten participants form a cohort and make decisions in two parts. In Part 1 (Section 2.1), participants go through a test procedure revealing their distributional (i.e., social) preferences. In Part 2 (Section 2.2), they participate in an asset market and policy choice experiment.

2.1. Experiment part 1: Equality equivalence test

Studying insider trading regulation requires considering market participants' equality and fairness concerns (Bainbridge, 2023). We thus elicited participants' distributional preferences by means of the *Equality Equivalence Test* (EET, see Kerschbamer, 2015) in Part 1 of the experiment. Based on participants' incentivized decisions, the EET provides $\langle x, y \rangle$ -scores for all participants with monotone preferences. In the domain of disadvantageous inequality (i.e., when another individual's payoff is greater than one's own), a positive (negative) x -score corresponds to benevolent (malevolent) behavior, while in the realm of advantageous inequality (i.e., when one's own payoff is greater than another's), a positive (negative) y -score corresponds to benevolent (malevolent) behavior (Holzmeister and Kerschbamer, 2019). See Section 4.1 of the Online Appendix for detailed information and the parameterization.

2.2. Experiment part 2: Asset market and policy choice experiment

The ten participants of a cohort interact in a market over a sequence of twelve independent periods, each containing a trading phase of 180 s. Nine of the ten participants are *traders* (Section 2.2.1) and the remaining participant is a market observer (Section 2.2.2).

¹ We use the terms 'informed trading' and 'insider trading' interchangeably to abstract from specific legal definitions of what constitutes insider trading in different countries.

2.2.1. Traders

Participants trade in a continuous double auction with open order books (i.e., a limit order market). At the beginning of each period, traders receive positive amounts of assets and talers. The exact numbers that they receive are drawn anew in each period, independently of previous periods' draws. The procedure ensures that uninformed traders cannot deduce useful information about the asset's fundamental value from their initial endowments. Moreover, it ensures that the initial ratio of outstanding talers to the value of outstanding assets (cash-to-asset ratio), equals 1 in each period. We further allow traders to engage in short sales and margin purchases, as is common in most equity markets outside of crisis periods. Specifically, our traders may sell short or purchase on margin up to 100% of their initial endowments in assets and talers, respectively.² We split the nine traders into two different types. At the beginning of a period, three traders receive information about the asset's exact buyback value (BBV). The remaining six traders only learn the common information, i.e., that the buyback is drawn from a uniform distribution between 30.0 and 85.0 talers.³ We provide all information free of charge.⁴ We refer to the first type as *informed traders* and to the second as *uninformed traders*. The ratio between informed and uninformed traders remains undisclosed for all participants, but we inform all participants that the number of informed traders is constant, no less than two, and less than the number of uninformed traders.

Our incentive scheme emphasizes traders' ability to create trading profits. (Kleinlercher and Stöckl, 2018 discusses incentive schemes in experimental asset markets.) Eq. (1) describes the incentive scheme used to determine trader i 's income in period p .

$$income_{i,p} = \max(BC + (PD_{i,p} \cdot MP), 0) \quad (1)$$

$BC = \text{€}30$ is the basic compensation that participants receive for the time spent in the experiment. In addition, it serves as a cushion for negative individual trading profits.⁵ PD measures the percentage difference between an evaluation value (EV) and a reference value (RV), such that

$$PD_{i,p} = \frac{EV_{i,p} - RV_{i,p}}{RV_{i,p}} \quad (2)$$

We choose EV and RV to ensure that a trader's payout depends solely on their individual actions and not on random parameter realizations. In the experiment, RV equals the trader's initial wealth in each period, i.e., the sum of the initial taler holdings and the asset endowment, evaluated at BBV . The trader's EV equals their wealth at the end of the period. To increase the salience of the incentive scheme, we leverage PD by a multiplier of $MP = 90$. We choose MP such that a change of plus (minus) 33.33% in PD doubles (wipes out) BC . One desirable

² We present the detailed procedures regarding the trading mechanism and the determination of traders' endowments in Section 4.2 of the Online Appendix and provide detailed analyses of traders' market activity, limit order submissions, trading volume, short selling, and margin buying activity in Section 3.1 of the Online Appendix.

³ In our information structure, informed traders do not receive noisy signals, as they frequently do in related strands of the literature. This design choice mitigates the risk of informed traders making mistakes in assessing BBV , based on misinterpretations of the noisy information or its generation process. A noisy signal would furthermore increase heterogeneity between sessions and reduce the statistical power of our tests. At the same time, uninformed traders would have the same incentives to participate in the market in our own setting and in a setting where the informed traders receive dispersed or imprecise information about BBV . (Uninformed traders face an informational disadvantage whenever the signal is at least minimally informative Lipsey and Christal, 2015, p. 970.)

⁴ See Huber et al. (2011) for a study offering costly information in a setting with multiple information levels.

⁵ We designed an experimental session to last 2 h and to yield an average hourly compensation of no less than $\text{€}15$.

characteristic of this payment scheme is that inactive traders have a PD equaling 0 and thus receive an income of exactly BC . The basic compensation is the same for informed and for uninformed traders.⁶ We determine the period used to calculate the traders' final payouts through a public card draw (performed by one of the participants) at the end of the experiment. A trader's payout in the experiment (incl. the payout of Part 1) is bounded from below by $\text{€}5$.

2.2.2. Market observers

In addition to traders, one participant in each cohort acts as a *market observer* who does not trade but whose task it is to detect informed traders by observing all traders' actions during the trading phase.⁷ At the end of each period, we ask the observer to mark those traders who they believe were informed in the period just ended. For their participation, observers receive a BC of $\text{€}30$ plus a performance-based compensation. For each *informed* trader that they mark as having been informed, observers earn $\text{€}4$. For each *uninformed* trader that they falsely mark as having been informed, they lose $\text{€}8$. There is no penalty for failing to mark an informed trader. Risk-, loss-, and ambiguity-neutral observers thus should mark a trader as informed if the observer believes to be correct with a probability of greater than $2/3$. Earnings (losses) from the observer's task are added to (subtracted from) their base compensation.⁸ Observers perform their task using publicly observable data (order book, price chart) and real-time aggregate information about each trader's activity in the market (provided at no cost). To mimic the situation outside of the lab, observers are not informed about the asset's BBV . We associate each trader with a unique ID consisting of a letter and a number (e.g., "K2") on the observer's screen. These IDs change from period to period and each ID is used only once in each session. This procedure ensures that observers can follow each trader's actions within a period, but cannot link them between periods.

2.2.3. Regulatory regimes and treatment manipulations

We use two regulatory regimes to study traders' preferences for insider trading regulation. In regime *NOREG*, informed traders do not face any consequences when identified by the observer. Thus, observers' decisions do not affect traders in the market. On the contrary, setting *REG* models an insider trading regulation regime in which informed traders who are marked by the observer ("taken to court by the market supervisory authority") are targeted with sanctions. Correctly identified, informed traders pay back their trading profits, which are then redistributed to the traders who engaged in unprofitable trades. Additionally, identified informed traders pay a fine equal to their trading profits. In total, correctly identified informed traders thus have

⁶ One could argue that this payment scheme gives preferential treatment to informed traders. Yet in one of the two market settings we describe in Section 2.2.3, insiders face the threat of punishment. Furthermore Palan and Stöckl (2017) show in a similar setting that competition between informed traders renders prices relatively efficient and eliminates most of the profit a monopolistic insider could have enjoyed.

⁷ We rely on human participants in the observer role for two reasons. First, one alternative specification would be to rely on detection algorithms, a procedure for example employed by the SEC. However, market authorities do not publish their detection algorithms, making it impossible to replicate their exact procedures and the development of proprietary algorithms would introduce an element of arbitrariness to our study. Second, we could use fixed detection probabilities, but this would make it impossible to investigate informed traders' behavioral responses to regulation, like hiding or dissimulation strategies.

⁸ Since observers receive a BC of $\text{€}30$, and the payout cannot become smaller than $\text{€}5$, the payment scheme allows observers to mark as many traders as they wish, with the maximum loss amounting to the $\text{€}25$ of BC . These parameters imply a small incentive to encourage observers to mark traders.

	Phase 1	Phase 2	Referendum 1	Phase 3	Referendum 2	Phase 4	Referendum 3	
NR	NOREG	REG		Determined by Referendum 1		Determined by Referendum 2		
RN	REG	NOREG						
	0	3	6	9	12			
	Period							

Fig. 1. Regulatory regimes by starting regime and phase.

Notes: REG indicates markets with insider trading regulation while NOREG indicates markets without such regulation. NR and RN indicate the order of the predetermined regulatory regimes in Phases 1 and 2. NR cohorts start with no regulation in Phase 1 and face regulation in Phase 2. RN reverses this order.

to give up twice their trading profits, i.e., $2 \cdot PD \cdot MP$. In both regimes, observers face the same task, yet their decisions affect traders in regime REG only. Setting REG approximates the existing situation in most major capital markets in which a central authority monitors market activity and punishes illegal insider trading. The traders do not receive information about the number of correctly or incorrectly identified informed traders. They only get information when they are affected by the detection (informed traders about the related costs to themselves, uninformed traders about redistribution payments).

We split the 12 periods of the market experiment into four phases of three periods each. Graphically, these phases are presented in Fig. 1. In each phase, insider trading laws are either being enforced or not, according to the following procedure: Phase 1 and Phase 2 are predefined, with the regulatory regime switching between them. To control for order effects, half of the cohorts begin with no regulation and switch to regulation (NR) while the other half begin with regulation and switch to no regulation of insider trading (RN). The regulatory regimes in Phases 3 and 4 are then determined by traders' majority votes. Thus, traders experience both regulatory regimes before they can vote on the regulation for the three periods of Phase 3 in Referendum 1. After Phase 3, Referendum 2 determines the regulatory regime of Phase 4. The majority vote ensures strategy-proofness of traders' choices, since the preference revelation cannot result in any gains (see Dasgupta and Maskin, 2020; Jehle and Reny, 2011). Furthermore, all that any participant learns about the votes cast is the resulting regulatory regime in the subsequent phase. This mitigates the risk that traders believe their vote to be irrelevant even in the presence of an overwhelming majority.⁹

Our treatment manipulation targets traders' expectations of receiving inside information. To ensure that differences in traders' voting behavior can be traced to their prospect of becoming informed, we randomly assigned participants to trader types. In treatment FIXED, the computer conducts a random draw at the beginning of the experiment to predetermine traders' types as being either informed or uninformed. All traders in treatment FIXED thus know their respective types throughout all phases, i.e., even before Referenda 1 and 2 and Phases 3 and 4. In treatment FLUCT, in contrast, traders' types are randomly reassigned at the beginning of each period (in periods 7 and 10 this happens after the referenda). Trader types thus may change from period to period and traders do not know their future types in advance. However, we use the two initial phases to ensure that each trader experiences each type in each regulatory regime. In particular, each trader is informed exactly

⁹ Note that after period 12, we ask traders to again indicate their preferences for regulation in a final Referendum 3. This referendum, however, does not affect future regulatory regimes. Thus, Referenda 1 and 2 are incentivized in that the outcome of these referenda affects traders in the following three periods. Referendum 3 is not incentivized, since it is held after the last trading period. We present analyses and results for Referendum 3 in Sections 2.1 and 2.2 of the Online Appendix.

once in Phase 1 and once in Phase 2. This procedure guarantees that traders have the same historical ratio of periods in which they receive information and periods in which they do not receive information prior to Referendum 1. We do not inform traders about this procedure and leave the determination of types in Phases 3 and 4 to a random mechanism.

Note also that our design choice of having three informed and six uninformed traders is of course not innocuous. It is likely to affect referendum outcomes and does not necessarily reflect typical market settings outside of the lab, limiting external validity. We designed the composition of the trader groups such that a material share of the uninformed population (more than one trader, i.e., more than one third of the uninformed trader population) is required to vote against regulation alongside the informed traders to prevent insider trading regulation from getting implemented. We furthermore chose to have a large enough number of informed traders (3) to induce competition for information rents and a certain adverse selection risk for uninformed traders (for evidence on the effect of competition among informed traders in experimental asset markets, see Palan and Stöckl, 2017). At the same time, the share of informed traders (insiders) is likely to be relatively low in most markets outside of the lab. We therefore kept the majority of the traders in our markets uninformed. We thus collect data in a carefully calibrated, albeit somewhat artificial setting that gives uninformed traders less weight than they might hold outside of the lab.

2.3. Experimental implementation

We collected data from 16 cohorts in each of two treatments (FIXED and FLUCT). We thus possess data from 64 incentivized referenda (and 32 non-incentivized referenda) across 32 independent cohorts and a total of 384 market observations (12 trading periods per cohort). Our experiments were conducted in October and November 2019 at a large research and teaching university with a total of 320 undergraduate and graduate students from different fields. Participants may have previously participated in other economic experiments but each participated only once in this study.^{10,11}

¹⁰ See Section 4.3 of the Online Appendix for details of the participant pool and questionnaire analyses.

¹¹ The software was programmed in z-Tree 3.6.7 (Fischbacher, 2007) and participants were recruited using hroot (Bock et al., 2014). We analyze our results using R (R Core Team, 2020), reading it in using package ztree (Kirchkamp, 2019). We calculate confidence intervals for binomial proportions via the function binconf from package Hmisc (Harrell Jr, 2021) and we estimate mixed effects models via the functions lmer and glmer from package lme4 (Bates et al., 2015). We use the package nnet (Venables and Ripley, 2002) when estimating multinomial logit regressions. We generate the regression tables using texreg (Leifeld, 2013), the figures using ggplot2 (Wickham, 2016) and furthermore use the package ggalluvial (Brunson, 2020; Brunson and Read, 2020) to generate Figure OA3 in the Online Appendix.

We implemented strict protocols to maintain consistency across cohorts, minimizing confounding variables that could influence the observed relationships. At the beginning of each session, every participant received the same written instructions, containing information on Part 1 (EET; reproduced in Section 5.1 of the Online Appendix). The EET began when all participants had confirmed to have read and understood the instructions and any remaining questions had been addressed. Then, after the final round of the EET, every participant received new written instructions. These instructions were the same for all participants and contained information on Part 2 (asset market) for both the trader and observer roles (reproduced in Section 5.2 of the Online Appendix). Participants had time to study these instructions on their own until they confirmed to have read and understood them. Each participant then had to answer a set of control questions. In case that a participant needed help, the experimenter pointed them to the relevant place in the instructions. The experiment proceeded only once every participant had answered all control questions correctly. Afterwards, the trading mechanism and the most important screens were explained in detail, followed by two trial periods that allowed the participants to familiarize themselves with the experimental procedures and the payment schemes. We informed participants about the role assignment (trader or observer) after the trading mechanism had been explained but before the trial periods started, and participants remained in their roles for the entire session. The information available to participants in each role, and the incentives they faced, were thus common knowledge. In the trial periods, all nine traders interacted in the same market. In treatment *FIXED*, trader types were allocated before the first trial period. In treatment *FLUCT*, traders were trained in both trader types, once as an informed and once as an uninformed trader. Specifically, in the first trial period, five participants were informed, while the remaining participants remained uninformed. These assignments were reversed in the second trial period. In the implementation of the experiment we took great care to label everything neutrally to minimize experimenter demand effects (Zizzo, 2010), and avoid a potential interplay with participants' moral values. Specifically, we used no terms like "insider", "authority" or "illegal" in our instructions or on screen. Each session lasted approximately two hours. In *FIXED*, traders earned an average of €34.32 (s.d. 7.54) and observers earned €28.69 (s.d. 8.25). In *FLUCT*, traders earned an average of €34.75 (s.d. 4.27) and observers earned €29.75 (s.d. 7.61).

2.4. Analysis approach

We analyze the outcomes of the referenda in Section 3.1 and traders' individual voting behavior in Section 3.2.¹² Both sections start with a descriptive overview, before we estimate mixed effects logit models or mixed effects Poisson models as described in Bates et al. (2015). Mixed models mitigate omitted variable biases as described by Heckman (1979), which can be a result of unobserved heterogeneity between cohorts or trader types (McFadden and Train, 2000). To account for the possible heterogeneity, we decompose the variance components into an iid error component ε and a random cohort-specific parameter θ (Greene, 2020, pp. 885–887). The mixed logit model, for instance, is particularly useful for analyzing diverse preferences between different types in a sample (Train, 2009, pp. 134–137). Fokkema et al. (2018)

¹² While it is not the focus of this paper, we present further analyses regarding the data generated in the experiment in the Online Appendix: Section 2.1 and 2.2 present additional results on referendum outcomes, individual voting behavior, and trader migration; Section 3.1 presents results on trading behavior; Section 3.2 reports on various market quality measures; Section 3.3 presents data on traders' profits; and Section 3.4 analyzes observer performance. Section 4.1 discusses the results from the EET and its relation to voting behavior; and Section 4.3 presents details on participant pool characteristics and a questionnaire analysis.

outline various applications of mixed effects models and we use one of their specifications in our analyses:

$$g(y_i) = x_i^T \beta + z_i^T \theta + \varepsilon_i$$

$$\mu_i = E[y_i | x_i] = g^{-1}(x_i^T \beta)$$

where $g(\cdot)$ describes the model's link function, $x_i^T \beta$ is the linear predictor, z_i a vector of length M , and θ the vector of random intercepts (see Fokkema et al., 2018). In our model, we use a cluster-specific random intercept only. Thus, each entry in z_i is zero except for the m -th element, which is 1 since m corresponds to the cohort of observation i . Meanwhile, the expected predictor μ_i is defined by the inverse link function at the linear predictor given the regressor x_i .

As the dependent variables in our logit regressions, we use either (i) the outcome of the majority vote (1 in favor of regulation, 0 against; Section 3.1), or (ii) the traders' individual votes (1 if the trader votes in favor of regulation, 0 if they vote against; Section 3.2). Additionally, we use (iii) the aggregate number of individual votes in favor of regulation as an alternative specification of the dependent variable when analyzing referendum outcomes in Section 3.1. This variable ranges from 0 (no trader in favor of regulation) to 9 (all traders in favor of regulation) and, thus, captures the strength of the preferences for regulation on a finer scale compared to (i). We estimate means in count data via a Poisson model as described in Zeileis et al. (2008). In our data, we observe only a single cohort in which no trader favors REG. Thus, the data need not be considered zero-inflated.

The set of independent variables includes a treatment dummy ('*FIXED*'), a dummy for the regime order in Phases 1 and 2 ('*Starting regime RN*'), which is 1 for participants who experienced regime REG in Phase 1 and regime NOREG in Phase 2, and 0 for participants who experienced the treatments in the opposite order, as well as a dummy for the referendum ('*Referendum 2*'), which is 1 for the referendum after the 9th period and 0 otherwise. In Section 3.2, '*PD^{Before}*' captures traders' ability to generate profits. We calculate this variable as the mean percentage difference before punishment and redistribution in Phases 1 and 2 as defined in Eq. (2). In treatment *FLUCT*, we uniformly calculate the average of *PD* over the four periods a trader is uninformed and over the two periods a trader is informed.

Analyzing individual votes (Section 3.2), we furthermore use variables reflecting participants' EET information collected in Part 1 of the experiment. In particular, we include traders' $\langle x, y \rangle$ -scores, their interaction, and a dummy variable equaling 1 for participants with non-monotonic preferences and 0 otherwise. For traders with non-monotonic distributional preferences, we replace the $\langle x, y \rangle$ -scores from the EET with zero. Given that the original $\langle x, y \rangle$ -scores do not include any values at zero, and since we include fixed effects for non-monotonic traders, the coefficients' point estimates are not affected by this approach. There may, however, be an impact on $\langle x, y \rangle$ -scores' variances from zero-inflated regressors.

When analyzing individual votes (Section 3.2), we also introduce interactions of the independent variables with the treatments and with trader types. These interactions are labeled (*I*) for interactions with predeterminedly informed and (*U*) for interactions with predeterminedly uninformed traders in treatment *FIXED*. Consequently, variables without an interaction represent the common estimates for all participants and describe, in particular, estimates regarding the reference traders in treatment *FLUCT*. Meanwhile, the interactions describe the differences in variable estimates of the respective trader types compared to traders in treatment *FLUCT*.

To support the interpretation of the regression results, we present the results in two columns. The first column presents model estimates, while the second column reports the average of the sample marginal effects (AME), i.e., marginal effects based on the average prediction of the sample (calculated according to Greene, 2020, pp. 774, 925). The marginal effects in the logit models specify the predicted marginal change in probability for a unit change of the independent variable. In the Poisson model, marginal effects describe the predicted marginal change in REG votes for a unit change of the independent variable.

Table 1
Mixed effects logit and Poisson regressions of (majority) votes in favor of regulation by referendum.

	(1) Logit majority vote REG		(2) Logit majority vote REG		(3) Poisson # of REG votes		(4) Poisson # of REG votes	
	Estimates	AME	Estimates	AME	Estimates	AME	Estimates	AME
<i>Intercept</i>	-2.41*** (0.71)		-1.76* (0.73)		1.00*** (0.14)		1.06*** (0.16)	
FIXED	1.72** (0.61)	39.69%	0.69 (0.84)	15.81%	0.38** (0.13)	1.42	0.28 (0.19)	1.05
<i>Starting regime RN</i>	1.15 (0.60)	26.41%	0.00 (0.91)	0.00%	0.08 (0.13)	0.28	-0.04 (0.20)	-0.15
FIXED × RN			1.92 (1.21)	44.17%			0.20 (0.27)	0.73
<i>Referendum 2</i>	0.50 (0.58)	11.58%	0.54 (0.60)	12.35%	0.11 (0.13)	0.41	0.11 (0.13)	0.41
$\mu(\text{Intercept})$	8.23%		14.72%		2.73		2.89	
Avg. prediction	35.94%		35.94%		3.70		3.70	
McFadden R ²	0.15		0.18		0.04		0.04	
Num. obs.	64		64		64		64	
Num. cohorts	32		32		32		32	

Notes: Model 1 is a mixed effects logit regression for the cohort’s majority vote as the dependent variable, where 1 corresponds to cohorts with a majority (at least five votes) in favor of regulation. Model 2 is a mixed effects Poisson regression for the cohort’s number of votes in favor of regulation. Standard errors are provided in parentheses. The independent variables include the treatment (‘FIXED’), which is 1 for cohorts in treatment FIXED and 0 otherwise. ‘Starting regime RN’ indicates the order of the predetermined regulatory regimes in Phases 1 and 2, which is 1 when cohorts enforce regulation in Phase 1 and are without regulation in Phase 2, and 0 otherwise. ‘FIXED × RN’ describes the interaction of ‘FIXED’ and ‘Starting regime RN’, which is 1 when both are 1, and 0 otherwise. ‘Referendum 2’ is 1 for observations stemming from the referendum after the 9th period and 0 for the referendum after the 6th period. For each model, the first column represents model estimates, while the second column reports averages of the sample marginal effects (AME), i.e., marginal effects based on the average prediction of the sample, calculated according to Greene (2020, pp. 774, 925).

* $p < 0.05$.
** $p < 0.01$.
*** $p < 0.001$.

3. Results

3.1. Referendum outcomes

Result 1. *The majority of referenda turn out against regulation.*

Support. Across both treatments, a majority of the traders vote for regulation in 23 of 64 referenda (36%). This low preference for regulation in the experiment runs counter to the current practice in markets outside of the lab, where the majority of countries with stock markets have regulation in place (Bhattacharya and Daouk, 2002).

Result 2. *In FLUCT, where traders have the chance to become informed, they decide referenda in favor of regulation significantly less often than they do in FIXED.*

Support. In treatment FLUCT, in which every trader has a chance to receive inside information, traders decide 26 referenda (81%) against and 6 (19%) in favor of regulation. When, however, a group of traders has no chance of receiving inside information (FIXED), the referenda are decided in favor of regulation 17 times (53%) and against it 15 times (47%). This difference in referenda outcomes between the treatments is statistically significant at the 1%-level (equal proportions test, $\chi^2(1) = 6.79, p = .0092, n = 64$). Despite this treatment difference in the ultimate outcomes, the results of FIXED indicate that even in a situation in which the majority of traders never has a chance of profiting from an informational advantage, there is no strong majority for regulation.

We explore our data further in Table 1. We use logit regressions to study the outcomes of the majority votes (1 in favor of regulation, 0 against) as the dependent variable in Models 1 and 2. In line with Result 2, Model 1 yields a positive and significant coefficient for FIXED, indicating a higher probability of a referendum outcome in favor of REG in treatment FIXED. Considering the regression results, there is one aspect that we would like to further elaborate on. The (insignificant) coefficient of ‘Starting regime RN’ in Model 1 suggests a higher probability of voting in favor of regulation for cohorts who experienced regulation in Phase 1. Thus, traders seem to prefer regulation after they have experienced – or have become habituated to – regulation over the

course of the first three periods. To better understand this observation, we estimate Model 2, where we control for treatment differences in the starting regime effects by interacting treatment and starting regime. This approach yields a predicted probability for a majority vote in favor of regulation for an intercept cohort (i.e., for a referendum after period 6 in treatment FLUCT) of 14.72% when Phase 1 was unregulated (NR) and equally so when Phase 1 was regulated (RN). The corresponding probabilities in treatment FIXED are substantially different. Specifically, in cohorts that are unregulated in Phase 1 (NR), we predict REG outcomes with a probability of 25.54%, while in cohorts that enforce regulation in Phase 1 (RN), we predict REG outcomes with a probability of 70.03%. Although the differences between treatment FLUCT and cohorts in treatment FIXED that enforce regulation in Phase 1 (RN) remain significant in Model 2 (Wald’s $W(1) = 8.92, p = .0028$), the coefficients of both ‘FIXED’ and ‘Starting regime RN’ are not significant in Model 2. This result indicates that cohorts in treatment FIXED that enforce regulation in Phase 1 (RN) drive the observed treatment difference in referendum outcomes. When traders in treatment FIXED are exposed to regulated markets early in the experiment, they later rarely change their voting behavior towards unregulated markets. See Figure OA1 in the Online Appendix for a graphical illustration.

In Models 3 and 4, meanwhile, we analyze the aggregated number of votes in favor of regulation within a referendum via Poisson regressions. The model assumes that the dependent variable follows a Poisson distribution that ranges from 0 to 9 votes in favor of regulation. On average, we observe 6.00 votes against and 3.00 votes in favor of regulation in treatment FLUCT, and 4.59 votes against and 4.41 votes in favor of regulation in treatment FIXED. We again find supportive evidence for Result 2 in the significantly positive coefficient for FIXED in Model 3. Comparable to Model 2, this significance vanishes in Model 4, in which we control for interactions between treatments and the starting regime. Using Model 4, we estimate the predicted number of votes ($\hat{\lambda}$) in favor of regulation for an intercept cohort in treatment FLUCT to be $\exp(1.06) = 2.89$ in NR and 2.78 in RN cohorts. In treatment FIXED, we predict 3.84 votes in favor of regulation in NR cohorts and 4.49 in RN cohorts.

To better understand the driving forces behind the difference in referendum outcomes between treatments and starting regimes, the

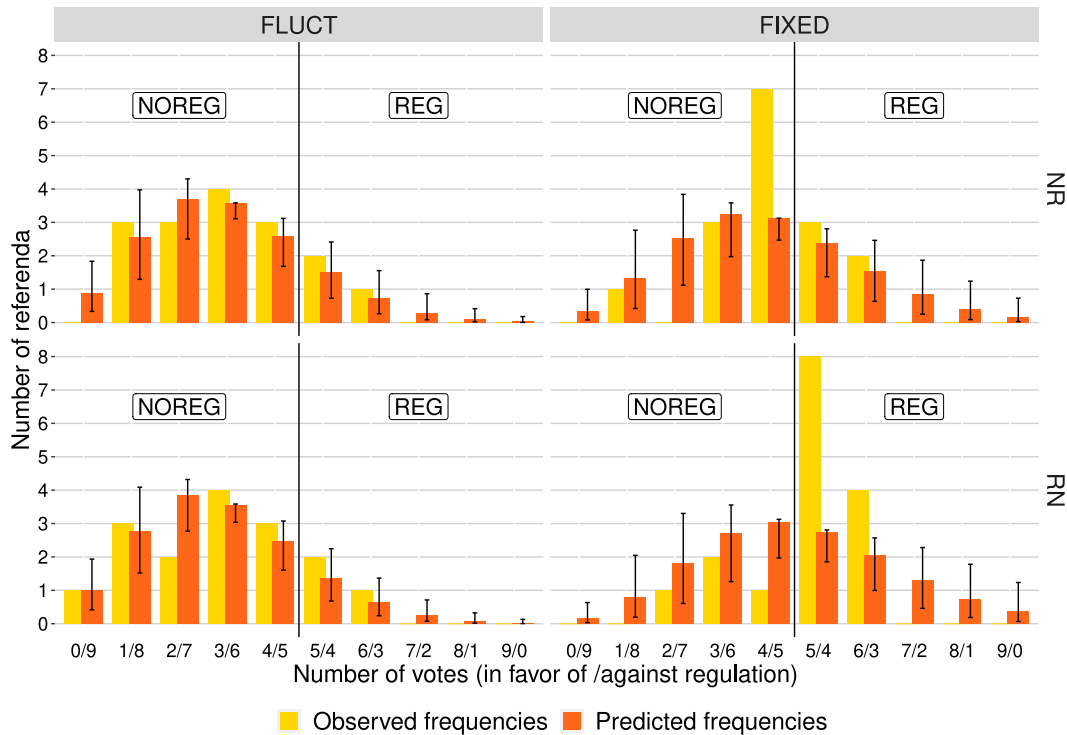


Fig. 2. Distribution of votes by treatment and starting regime.

Notes: Distribution of trader votes in number of referenda by treatment, starting regime, and order of regimes in Phases 1 and 2. Yellow bars represent the actual distribution of votes while orange bars represent predictions of Model 4 in Table 1. Rows labeled NR represent cohorts that started with no regulation, while rows labeled RN contain cohorts that started with regulation. Error bars indicate extrema of projections within the 95% confidence intervals for the regression estimates of λ , i.e., error bar limits describe $Poi_{\hat{\lambda}}(k)$ and $Poi_{\bar{\lambda}}(k)$, taking into account the extrema within the confidence interval of $\hat{\lambda}$, denominated $\underline{\lambda}$ and $\bar{\lambda}$.

descriptive analysis shown in Fig. 2 presents the distribution of votes within the referenda. The panels on the left present distributions for referenda in treatment FLUCT while the panels on the right present referenda in treatment FIXED. The panels in the upper half of the figure represent cohorts that started with unregulated markets (NR), while the panels in the lower half present cohorts that started with regulated markets (RN). Each distribution is split into two halves by a solid line; bars on the left describe majorities against regulation (NOREG), bars on the right describe majorities in favor of REG. Yellow bars represent observed frequencies of votes within a referendum, while orange bars represent the predictions of Model 4 in Table 1. We calculate predicted frequencies via the Poisson probability mass function, $Poi_{\hat{\lambda}=\exp(x^T \hat{\beta})}(k)$, which we multiply by 16 for comparison with the 16 referenda in each combination of treatment and starting regime. The projected confidence intervals represent the extremes of projections within the 95% confidence intervals of the regression estimates of $\hat{\lambda}$; for example, in Model 4 in Table 1, we estimate λ for an intercept cohort to be $\exp(1.06) = 2.89$, with limits of [2.17, 3.87]. Therefore, in the upper left-hand panel, i.e., in cohorts starting with no regulation (NR) in treatment FLUCT, projections of confidence intervals for 2/7 votes are given by $16 \cdot Poi_{2.17}(2) \simeq 4.30$ and $16 \cdot Poi_{3.89}(2) \simeq 2.50$. For almost all votes, these limits specify the confidence intervals, however, for 3/6 votes, the upper limit is found via $Poi_3(3)$.

The left-hand panel, confirms our previous insights. We observe no effect of the starting condition on referendum outcomes in treatment FLUCT, in which all traders have a chance of becoming informed traders. In contrast, the right-hand panel of Fig. 2 reveals clear patterns. First, we observe that the cohorts that start with regulation (RN) in treatment FIXED decide 75% of the referenda in favor of regulation, whereas of the cohorts that start with no regulation (NR) decide only 31% of the referenda in favor of regulation. Second, we observe peaks of the yellow bars close to the threshold value that decides a referendum (solid line) that are far greater than the predicted frequencies.

In cohorts that started with NOREG markets in Phase 1 (NR), the peak is to the left of the threshold value, implying that REG outcomes are opposed by a 4 to 5 vote. In cohorts that started with REG markets in Phase 1 (RN), the peak is to the right of the threshold value, implying that REG outcomes are favored by a 5 to 4 vote. The starting regime difference thus seems to be driven by the change in the voting behavior of the median voter, behavior that could result from the existence of undecided ‘swing’ voters, who do not feel strongly tied to any of the regulatory regimes and who switch their vote towards the regime they have become habituated to in Phase 1.¹³

These observations indicate a certain level of status quo bias in policy choice. (See Hanke et al., 2010 and Merl et al., 2023 for related discussions indicating that the effects of regulatory changes, once implemented, are unlikely to be completely reversed once the change is reversed.) Since many developed countries have insider trading regulation in place and traders might have become habituated to these regulations, our results imply that the currently prevailing practice may possess a certain stickiness. Additionally, Raustiala (2002) argues that asymmetry reinforces policy convergence towards the policy prevailing in the most dominant jurisdiction, suggesting that changes in a regulatory regime are less likely when they go against the example of the lead regulator.

3.2. Individual voting behavior

Result 3. Traders in treatment FIXED who are predetermined to remain uninformed in future periods vote in favor of regulation more frequently than

¹³ While there may be path-dependencies between Referenda 1 and 2 in response to referenda outcomes, an analysis of such patterns would be statistically unreliable due to the limited sample size. What analyses we run to study patterns of voting behavior are reported in Section 3.2.

both traders predetermined to become informed and traders in treatment FLUCT.

Support. The individual voting data shows that a majority of traders' votes (51.6%) are in favor of regulation in treatment FIXED. As expected, at the individual trader level, this preference for regulation strongly depends on trader type. Uninformed traders vote in favor of regulation in 69.27% of all cases, while informed traders do so in 8.33%. A remarkable one-third of the uninformed traders thus oppose regulation, while – even if the proportion is small – some informed traders express a preference for regulated over unregulated markets. The results of treatment FIXED contrast with those of FLUCT in which we find 33.33% of traders' votes in favor of regulation. Thus, the majority of traders in an asset market where every trader has the same chance of becoming informed (i.e., on a level playing field) do not desire regulation. Taken together, these descriptive results indicate a strong influence of traders' prospects of becoming informed on their preferences for regulation.

We further explore the individual voting data through the mixed-effects logit regressions reported in Table 2. Controlling for additional relevant variables in the regressions, we find a persistent effect of traders' prospects of becoming informed on their revealed preferences for regulation, in line with Result 3. Specifically, the coefficient of 'FIXED - Uninformed traders (U)' in Model 1 indicates that uninformed traders in treatment FIXED are significantly more likely to vote in favor of regulation than are traders in treatment FLUCT. Meanwhile, informed traders in treatment FIXED are (not significantly) less likely to vote in favor of regulation than traders in treatment FLUCT. The coefficient 'FIXED - Informed traders (I)' in Model 1 is negative, but not significant. In treatment FIXED, uninformed traders are significantly more likely to vote in favor of regulation than are informed traders (Wald's $W(1) = 10.81$, $p = .0010$).

Model 1 predicts that the intercept trader, i.e., a trader in treatment FLUCT, without a wealth change, whose session started without regulation, will vote in favor of regulation in Referendum 1 with a probability of 32.92%. Given that regulation is intended to restrict or prevent informed trading, this result relates to the findings of Bodoff et al. (2006). They report that traders ex ante prefer markets with asymmetric information in which traders have an equal chance of receiving information over markets in which no one receives information. Consequently, the results in Bodoff et al. (2006) and our Results 2 and 3 support the argument that the presence of regulation in most markets may result from the absence of such a level playing field. Most market participants outside of our lab do not have realistic chances of gaining access to private/inside information and thus resemble the uninformed participants in our FIXED treatment. For traders in treatment FIXED, we find that a predeterminedly uninformed trader in the same situation votes for regulation with a probability of 55.65%, while a predeterminedly informed trader votes in favor of regulation with a probability of 12.55% (see Figure OA2 in the Online Appendix for a visualization of the predictions). While the probability is low, it is still interesting to observe that some informed traders vote for regulation. One argument rationalizing this behavior is presented by Kim et al. (2019), who suggest that regulation prevents competitors for information rents from impounding their information into prices too quickly. Our data, however, does not allow us to clearly identify whether this motive drives informed traders' voting behavior.

The predictions for intercept traders (i.e., traders without a wealth change in Referendum 1) in Model 1 of Table 2 show that the probabilities for voting in favor of regulation are smaller than 50% both for traders in treatment FLUCT (32.92%) and for informed traders in treatment FIXED (12.55%; Wald's $W(1) = 8.61$, $p = .0033$, for traders in treatment FLUCT; Wald's $W(1) = 10.05$, $p = .0015$, for informed traders in FIXED). These numbers reveal a common tendency in treatment FLUCT and for informed traders in FIXED to vote against regulation. We find less pronounced effects for uninformed traders in FIXED. Specifically, the sum of the coefficients of the intercept and of 'FIXED - Uninformed traders

(U)' result in a prediction of 55.65%, indicating a non-significant difference to a probability of 50% for an uninformed trader without a wealth change in Referendum 1 (Wald's $W(1) = 0.63$, $p = .4249$). Thus, controlling for multiple influential factors, our findings document no clear preference of predeterminedly uninformed traders towards either of the regulatory regimes. These traders might thus be less concerned with insider trading regulation than expected.

A trader's ability to generate profits and its influence on voting behavior is represented by the coefficient of 'PD^{Before}' in Table 2. In Model 1 we do not find a significant effect of traders' ability to generate profits, but we find a significantly greater effect for uninformed traders in treatment FIXED than for traders in treatment FLUCT. However, the uninformed traders' coefficient is not significantly different from zero when we add the corresponding interaction term to the variable's main coefficient (Wald's $W(1) = 1.00$, $p = .3170$). In other words, there is a significant difference between traders in different treatments, but we do not observe a significant effect of 'PD^{Before}' on voting behavior.¹⁴

When we investigate the potential influence of the starting regime on voting behavior, we thus find no significant coefficient for 'Starting regime_{RN}' for any trader type or treatment. The largest (though not significant) coefficient regarding order effects is the interaction with uninformed traders in treatment FIXED. Indeed, the marginal effects of Model 1 show almost twice the effect on uninformed traders in treatment FIXED than the starting regime has on any other trader type. Nevertheless, we find no evidence of a relevant effect of starting regime on voting behavior.

Model 2 in Table 2 augments the model by adding traders' $\langle x, y \rangle$ -scores, which we collect in the EET in Part 1 of the experiment and use to control for the impact of participants' distributional preferences. The $\langle x, y \rangle$ -scores range from -2.5 to 2.5 , where someone who always chooses the malevolent right (left) option on the x -list (y -list) receives a score of -2.5 for the respective list. Since not all (potential) traders in practice and in our treatment FIXED are equally likely to obtain superior information, fairness concerns, as described in Bainbridge (2023), are likely to drive both international market regulations and the treatment differences in our results. Trautmann (2009) for example, based on the model by Fehr and Schmidt (1999), argues that fairness concerns are affected by properties of the outcome generating process, such as the probability of receiving information.

Model 2 shows that adding the EET-related variables does not change the previously reported results, and that traders' distributional preferences do not substantially impact their voting behavior. Specifically, the significantly positive coefficient 'FIXED - Uninformed traders (U)' indicates a persistent impact of traders' prospects of becoming informed on their revealed preferences for regulation. Regarding the coefficients that relate to distributional preferences, we do not find evidence of statistical significance when we jointly test coefficients related to $\langle x, y \rangle$ -scores (Wald's $W(9) = 13.77$, $p = .1306$ for nine coefficients 'x-score', 'y-score', 'x * y-score', and the corresponding six interaction terms with informed (I) and uninformed traders (U) in treatment FIXED). Meanwhile, those traders in treatment FLUCT who revealed non-monotonic distributional preferences are more likely to vote in favor of regulation. Note, however, that these results should be interpreted with caution due to the low number of observations and the potential of noisy behavior induced by these traders.

Model 3 meanwhile reports a mixed effects logit regression of voting behavior for a reduced sample. Specifically, we restrict our sample

¹⁴ Note that the absolute values of the marginal effects for 'PD^{Before}' are greater for a unit change; however, the observed 'PD^{Before}' values range between a minimum of -0.34 and a maximum of 0.45 . To put that into perspective, only 10.12% of the predeterminedly informed traders in treatment FIXED achieve 'PD^{Before} > 10%', only 10.38% of the predeterminedly uninformed traders achieve 'PD^{Before} < 10%', and the majority of traders in treatment FLUCT stay within an even narrower range; the effect size of 'PD^{Before}' lies between -28% and 34% for 99% of the sample.

Table 2
Mixed effects regressions of voting behavior on regulation.

	(1) Logit indiv. vote _{REG}		(2) Logit indiv. vote _{REG} incl. EET data		(3) Logit indiv. vote _{REG} consistent & active traders	
	Estimates	AME	Estimates	AME	Estimates	AME
<i>Intercept</i>	-0.71** (0.24)		-1.04*** (0.30)		-0.97** (0.33)	
FIXED - Informed traders (<i>I</i>)	-1.23 (0.66)	-29.77%	-1.03 (0.98)	-25.05%	-1.61 (1.30)	-38.62 %
FIXED - Uninformed traders (<i>U</i>)	0.94* (0.37)	22.73%	1.69*** (0.47)	40.84%	1.72*** (0.50)	41.20%
\overline{PD}^{Before}	-4.48 (2.30)	-108.45%	-4.57 (2.36)	-110.64%	-4.24 (2.37)	-101.39 %
$(I) \times \overline{PD}^{Before}$	-3.46 (4.24)	-83.68%	-5.37 (4.84)	-129.97%	-8.97 (5.92)	-214.63 %
$(U) \times \overline{PD}^{Before}$	6.69* (3.19)	161.87%	6.70* (3.33)	162.17%	7.49* (3.47)	179.11%
<i>Starting regime</i> RN	-0.07 (0.29)	-1.67%	-0.01 (0.29)	-0.23%	-0.12 (0.34)	-2.96 %
$(I) \times \textit{Starting regime}$ RN	-0.37 (0.86)	-9.00%	-0.32 (0.97)	-7.77%	0.51 (1.23)	12.30%
$(U) \times \textit{Starting regime}$ RN	0.76 (0.46)	18.45%	0.67 (0.47)	16.29%	0.61 (0.55)	14.63%
<i>Referendum 2</i>	0.06 (0.25)	1.56%	0.07 (0.26)	1.66%	0.04 (0.28)	0.90%
$(I) \times \textit{Referendum 2}$	-0.69 (0.84)	-16.59%	-0.70 (0.85)	-16.29%	-2.08 (1.28)	-50.77 %
$(U) \times \textit{Referendum 2}$	0.61 (0.41)	14.74%	0.63 (0.42)	16.95%	0.49 (0.46)	11.83%
<i>x</i> -score			-0.10 (0.20)	-2.30 %	-0.05 (0.21)	-1.30 %
<i>y</i> -score			0.13 (0.14)	3.22%	0.15 (0.15)	3.66
<i>x</i> * <i>y</i> -score			0.17 (0.10)	4.03%	0.14 (0.10)	3.36
$EET_{Non-monotonic}$			1.98*** (0.58)	47.89%		
$(I) \times \textit{x}-score$			0.22 (0.56)	5.29%	0.59 (0.67)	14.11%
$(I) \times \textit{y}-score$			-0.34 (0.53)	-8.15 %	-0.22 (0.58)	-5.27 %
$(I) \times \textit{x} * \textit{y}-score$			-0.56 (0.36)	-13.60 %	-0.81 (0.44)	-19.31 %
$(I) \times EET_{Non-monotonic}$			-1.93 (1.48)	-46.77 %		
$(U) \times \textit{x}-score$			0.57 (0.30)	5.29%	0.51 (0.31)	12.19%
$(U) \times \textit{y}-score$			-0.35 (0.25)	-8.15 %	-0.32 (0.26)	-7.69 %
$(U) \times \textit{x} * \textit{y}-score$			-0.58** (0.19)	-13.98 %	-0.55** (0.20)	-13.04 %
$(U) \times EET_{Non-monotonic}$			-2.60*** (0.77)	-63.06 %		
$\mu(\textit{Intercept})$	32.92%		26.16%		27.50%	
Avg. predicted probability	41.10%		41.12%		39.58%	
McFadden R ²	0.18		0.21		0.21	
Num. obs.	576		576		492	
Num. cohorts	32		32		32	

Notes: Mixed effects regressions for individual votes as the dependent variable, which is 1 for votes in favor of regulation. Model 1 is a mixed effects logit regressions for individual votes in Referenda 1 and 2. Model 2 controls for traders' distributional preferences in the mixed effects logit regressions for individual votes in Referenda 1 and 2. Model 3 excludes traders with non-monotonic distributional preferences and traders who did not place any limit orders and accepted no market orders in more than 3 periods. Standard errors are provided in parentheses. The independent variables include the mean \overline{PD} before redistribution in Phases 1 and 2, the order of regimes in Phases 1 and 2 (*Starting regime* RN), and whether we observe Referendum 2 or Referendum 3 instead of Referendum 1. Standard errors are provided in parentheses. For each model, the first column represents model estimates, while the second column reports averages of the sample marginal effects (AME), i.e., marginal effects based on the average predicted probability of the sample calculated according to Greene (2020, p. 774).

* $p < 0.05$.

** $p < 0.01$.

*** $p < 0.001$.

along two dimensions: First, we only include active traders, i.e., traders who placed a limit order or accepted a market order in at least three of the six periods in Phases 1 and 2, and thus before the first vote. Activity after the first vote is likely to be endogenous to voting outcomes. The

effects of having or not having regulation is likely more relevant for those who trade frequently, since these traders are more strongly exposed to information risk, affected by larger spreads, and by inefficient prices. Second, we only include traders with monotonic distributional

preferences, assuming that traders answering inconsistently in the EET may also be a potential source of noise in Part 2 of the experiment. Applying these conditions, the sample shrinks to 492 votes, meaning that we exclude 84 votes, by 42 traders (10 for inactivity: 7 in *FLUCT*; 1 informed and 2 uninformed in *FIXED*; and 32 for inconsistency: 9 in *FLUCT*, 7 informed and 16 uninformed in *FIXED*). The regression output reveals no material differences in the results of Model 3 compared to Models 1 and 2. That is, restricting the analysis of the voting behavior to active traders who participated in Phases 1 and 2 and answered consistently in the EET does not impact our regression results. While we thus conclude that our results are robust to these two specific dimensions, we cannot rule out that other sources of noise might have affected our findings.

3.3. Limitations

While we design our experiment to optimally address our research questions, no approach is without limitations. We use this section to transparently discuss these.

First, we implemented several design features with the aim of supporting the internal validity of the results. For instance, our design takes particular care to include substantial training and to expose traders to both regimes before these traders make their policy choices. Moreover, as each referendum aggregates the preferences of nine traders, it required a substantial effort to collect the reported data of 64 incentivized referenda (and from 32 non-incentivized referenda) across 32 independent cohorts (16 cohorts in each treatment, 320 participants in total). While the study is larger than the majority of experimental asset market experiments, the sample size can nevertheless be considered small, calling for a cautious interpretation of results and for further research to validate our insights into referenda outcomes.

Second, compared to other experimental settings, our experimental design can be considered complex and, thus, likely induces a certain level of noise in the observed behavior, which should be kept in mind when interpreting the results and drawing conclusions. Anticipating this issue, we took several steps to train participants to minimize such noise. For instance, we allowed participants to ask clarifying questions (in private), we used control questions, and we conducted trial periods. Moreover, we chose to standardize the information among informed traders and to not allow informed traders to disclose their information, such that informed trading is the only channel for information dissemination in the markets. Future research may target this issue by, for example, allowing informed traders to tip others, or to mislead by sharing incorrect information. In addition to these ex-ante measures, we use a regression model to eliminate potential sources of noise by only considering observations of traders with consistent EET data and a certain level of trading activity.

The level of price efficiency observed in both settings of our market experiment, *REG* and *NOREG*, could be considered another potential indicator for the level of noise in participants' behavior. In particular, given the substantial number of informed traders (3 out of 9) and thus a considerable level of competition for information rents (Kyle, 1985), price inefficiencies could be interpreted as a lack of understanding by participants. Note, however, that informed traders may have a motive to hide their informed status in both settings: in *REG* because they want to evade punishment, and in *NOREG* because this allows them to exploit their informational advantage to a greater extent (see Huddart et al., 2001, for a discussion of dissimulation strategies). Besides these considerations, our price efficiency results are comparable to those in other publications (e.g. Merl et al., 2023) and Page and Siemroth (2020) estimate the share of private information used by the market to be up to 30%. (See Section 3.2 in the Online Appendix for a detailed discussion of market quality measures.)

Third, our single market design does not allow us to study the effects of regulation on market participation. While participants in our experiment can of course refrain from trading in markets, they have no alternative investment opportunities like other asset markets, savings

accounts, real estate, or commodities. We decided against adding such features to the experimental design to limit complexity and to focus participants' attention on the main market. We refer the reader to contributions like Cumming et al. (2018) and Merl et al. (2023), which test whether market participation suffers under regulation. Merl et al. (2023) for example find that informed traders reduce their market participation under regulation, whereas uninformed traders do not significantly adjust their activity in response to a regulatory regime.

Fourth, in markets outside of the lab, there are several challenges to implementing a voting mechanism that reliably aggregates traders' interests. For example, policy makers would have to decide on who would be eligible to vote in such a referendum (e.g., traders residing in the jurisdiction, or those trading in the respective market) and whether all voters should get equal weight in the voting process (e.g., small vs. large, or institutional vs. private investors). While we cannot speak to such questions of practical implementation, our results offer evidence to support the discussion regarding the favorability of insider trading regulation.

Fifth and finally, our participants were mainly economics students, who (while frequently being active in financial markets) are likely to rarely possess experience trading based on privileged (i.e., inside) information. Although Holmén et al. (2021), Schwaiger et al. (2020), and Weitzel et al. (2019) provide evidence that student participants and professional traders behave similarly in related contexts, and that results from asset market experiments tend to be relatively free from bias induced by participant pool effects, we cannot entirely rule out participant-induced idiosyncrasies.

4. Conclusion

Today, most developed capital markets operate under regulations restricting insider trading. In the adoption process of insider trading regulation, many stakeholders contribute their interests and often it is not clear whether these interests are in line with the interests of the different individuals and firms that are active in financial markets. This assertion is supported by accumulating evidence raising doubts as to the effectiveness of insider trading regulation and, consequently, questioning the currently prevailing practice. In this paper, we contribute to this discussion by studying traders' regulation choices in an experimental market setting, conditional on traders' prospects of obtaining superior information.

Evaluating the outcomes of 64 incentivized referenda, we find that traders decide 23 (36%) referenda in favor of regulation and 41 (64%) against. While these numbers indicate a clear preference for unregulated markets, the proportions vary across traders with different prospects of becoming informed. In particular, in markets in which uninformed traders have no chance of receiving inside information, referenda are decided against regulation 15 times (47%) and in favor 17 times (53%). This result contrasts with the results of markets in which all traders have a chance of receiving inside information, in which the traders decide 26 (81%) of the referenda against and 6 (19%) in favor of regulation. Evaluating individual voting behavior, we observe that this treatment difference is driven by traders who know that they are guaranteed to remain uninformed in future periods. These traders are more frequently in favor of regulation than both, traders with no predetermined type and informed traders. Nevertheless, given the disadvantaged position of uninformed traders, we find it surprising that we do not observe stronger support for regulation in this group. Furthermore, we do not find a significant or substantial impact of traders' elicited distributional preferences on their voting behavior.

These results have several implications. First, our observations challenge the common assertion that insider trading regulations are necessary to promote participation and public confidence in the integrity of capital markets (see European Union, 2014, preamble, para. 23). Our findings do not support a clear preference of uninformed traders towards regulated markets, indicating that this trader group may be

less concerned about regulation than expected. Second, the limited evidence supporting the regulation of informed trading may spark discussions among regulatory agencies regarding the most efficient use of their resources. By reducing their focus on informed trading, regulators could more vigorously pursue other, potentially more costly (in terms of welfare losses) forms of market misconduct, such as price manipulation, securities fraud, front-running, or the illegitimate application of algorithmic and high-frequency trading technologies. Third, our findings contribute to debates about promoting measures that establish a level playing field among the various market participants. Deregulation in markets with a well-established level playing field may not only be favored by traders but may also improve market efficiency (see Section 3.2 in the Online Appendix, as well as, e.g., Easley et al., 1996; Cornell and Sirri, 1992; Du and Wei, 2004; Cumming et al., 2011; Palan and Stöckl, 2017; Merl et al., 2023). To conclude, we hope that this study and its findings will spark further investigation into the factors driving uninformed and informed traders' regulation preferences and into the effects of such regulation on market participation and informational efficiency.

CRedit authorship contribution statement

Dominik Schmidt: Writing – review & editing, Writing – original draft, Visualization, Validation, Software, Resources, Project administration, Methodology, Investigation, Formal analysis, Data curation, Conceptualization. **Thomas Stöckl:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Resources, Project administration, Methodology, Investigation, Funding acquisition, Conceptualization. **Stefan Palan:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Conceptualization.

Data availability

We have made data and code publicly available on Zenodo at <https://zenodo.org/records/13378858>. In particular, it includes the instructions in German language, the software files (z-tree), the experimental data, and the analysis code.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at <https://doi.org/10.1016/j.jbankfin.2024.107295>.

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