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Information Management against Excessive Stock Trading: More or Less? Or Both?

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Abstract

This paper investigates tools to counter excessive stock trading and increase profits for private households participating in the stock market. Creating a stylised hold or trade-scenario in a computer laboratory experiment, I find that by solely changing the information the participants receive, trading activity can be reduced by roughly 30%, increasing trading profits by more than 0.55 percentage points on monthly net returns. In particular, I consider two information treatments. First, I provide the participants with additional information by giving detailed feedback on their actions and outcomes at every turn. Second, when considering whether to hold a given stock or trade it for another one, I restrict participants' information on the recent performance of their allocated stock. Both interventions lead to significant changes in behaviour. Additionally, the 2×2 experimental design reveals that the effects stack.

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1 Introduction

Private households that participate in the stock market trade a lot. Excessive trading, through its various associated costs such as commission fees and bid-ask spreads, incurs sizeable losses. Barber et al. (2008) estimate that in Taiwan such losses are equivalent to 2.2% of gross domestic product. Similar behaviour has been documented in the U.S. (Barber and Odean, 2000), Canada (Linnainmaa et al., 2018) and Sweden (Dahlquist et al., 2016).¹ Despite this being a prevalent and well-documented phenomenon, little research has been done on how to counter this behaviour. Some scholars (see Huber et al., 2012; Hanke et al., 2010) have explored the impact of 'Tobin-like' taxes on market outcomes and individual behaviour in various settings. To the best of my knowledge, this article is the first to examine information management as a tool to curb excessive trading.

In particular, I present evidence from a computer laboratory experiment. In the experiment, participants engage in speculative trading tasks on the basis of real-life stock data where they are incentivised to realise the highest possible returns. In each instance, they can either choose to keep an allocated stock and receive its returns or pay a commission fee in order to swap their stock for another one if they fear that the stock will incur large losses. To inform this choice, participants may receive the return performance history of their allocated stock.

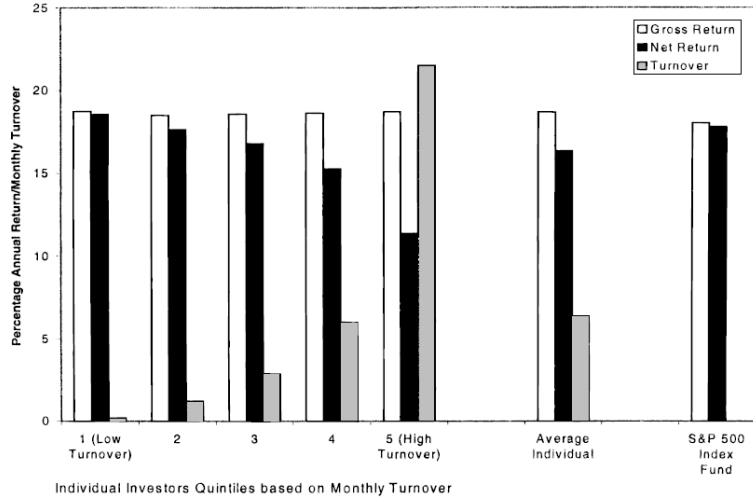
Figure 1 shows that in this context the same qualitative phenomenon noted in earlier work emerges: when ordering investors into quintiles according to trading activity, no clear pattern emerges regarding the gross returns earned. However there is a clear monotonic trend of decreasing net returns (gross returns minus trading costs) with increasing trading activity. The aim of this paper is to explore the propensity of two pure information interventions in curbing excessive trading and increasing net returns.

In a first treatment, I provide participants with detailed feedback on how their actions impact the outcomes of interest at every stage, what their outcome would have been if they had chosen differently as well as a prompt of whether they have chosen the most profitable option in a given instance. Such feedback interventions have been used to positive effect in diverse areas such as energy conservation (Allcott and Mullainathan, 2010; Tiefenbeck et al., 2018), personal savings (Karlan et al., 2016) and the enhancement of professional medical practice (for a review, see Ivers et al., 2012). The enrichment of the participants' information sets facilitates learning about the decision-making environment and increases the salience

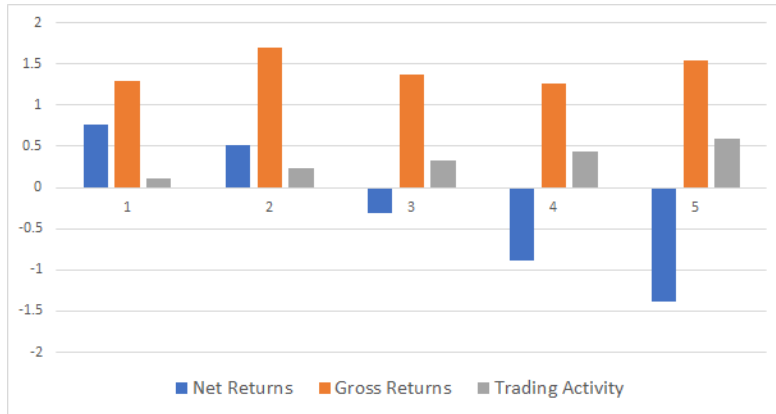
¹The authors examine individual investors that trading the umbrella of a Swedish Pensions Agency, where all transaction costs are collectively borne by the fund. Although highly active investors individually outperform inactive ones, the overall impact of such active trading on the fund's net returns is negative and large.

Figure 1: Stylized replication of Barber and Odean (2000).

(a) Evidence from Barber and Odean (2000)



(b) Experimental evidence.



of possible mistakes.²

In a second treatment, I reduce the participants' access to information on their allocated stock's previous performance. Although common wisdom dictates that, in stock-trading, more information will exclusively facilitate higher returns, this need not hold true when people incorrectly incorporate information or pursue more complex objectives. In fact, Mosenhauer (2019) shows theoretically that managerial decision-making in a firm may be improved by providing less information if CEOs overreact to salient outcomes. If a stock's performance history can 'tempt'

²It should be noted that in many feedback interventions, provided information is paired with social or normative comparisons, making it difficult to identify effect channels with certainty.

traders away from making the correct choice, for example by triggering panic-sales if falling prices are observed, providing the participants with less information may reduce excessive trading and increase net returns.

Both interventions show large effects. The Detailed Feedback and Information Filter Treatment lead to more than 13% and 17% reductions in trading activity, respectively. Accordingly, exposure to the treatments increased participants' net returns by sizeable 0.42 and 0.33 percentage points. Importantly, the experiment's overlapping treatment-design reveals that these effects stack. Participants who receive both treatments trade the least and receive the highest return.

These results provide new insights regarding the value of information in individual stock-trading. In an interactive market setup, Huber (2007) finds that average-informed traders perform the most poorly, while trading profits significantly increase with decreasing as well as increasing levels of information. The returns on information in such a setting may thus be non-monotonic. My results corroborate the view that traders may do better when receiving richer as well as poorer information. Crucially, however, the two interventions concern different *types* of information. Considering that their effects stack, my results suggest that distinct informational stimuli affect decision-making via independent channels rather than people acting on their overall informedness.

Moreover, I use personal information from the experiment's follow-up questionnaire to investigate the channels of overtrading. My analysis confirms previous studies (Biais et al., 2005; Grinblatt and Keloharju, 2009) that identify overconfidence and gambling activity as major drivers. On the other hand, contrary to Barber and Odean (2001) I do not find any link between a participant's gender and their level of overconfidence. In fact, male participants tend to trade less than women. Also, the treatment effects on participants' trading profits do not appear to arise from encouraging more risky portfolios as in Gneezy and Potters (1997). Instead, my results seem to stem from a novel channel of information management on trading outcomes.

At a practical level, my findings may be of interest to private individuals and professionals alike. Since the discussed experimental interventions solely concern which information should be presented when, they can easily be imitated on home computers with limited technical proficiency. These interventions could be implemented decentrally by households themselves or even offered as tools and applications by trading platforms or financial regulatory bodies acting in the public interest. Linnainmaa et al. (2018) further show that professional financial advisors may be subject to the same biases and mistakes as laymen, leading to high-cost overtrading both on behalf of their clients as well as within their personal portfolios, leaving room for my proposed interventions to exert benefits.

This article is organised as follows. The remainder of this section reviews

related literature. Section 2 discusses the experimental design, provides a descriptive analysis of the data and verifies the success of the experiment's randomisation. Section 3 presents the experiment's main results, while Section 4 discusses possible channels. Section 5 concludes.

1.1 Related Literature

Tobin (1978) famously proposed to counter excessive trading volumes by introducing a financial transaction tax. Although his recommendation was targeted towards international money markets, Keynes (1936) argued for the same in the domain of stock trading, receiving further theoretical foundation from Palley (1999).

This paper, on the other hand, attempts to achieve the same effect solely by deliberately choosing which information to supply to and which information to withhold from the trader. In doing so, it aims at averting losses incurred from overtrading and thus increase profits. Fama (1970)'s hypotheses on information efficiency provide a benchmark for the impact of information on stock trading outcomes. He states that prices 'fully reflect all available information at all times' (p. 385). Therefore, all variations of additional information should not have an effect on trading behaviour.

Findings from numerous studies dispute this hypothesis. In zero-sum market games, better-informed individuals can profit by exploiting worse-informed individuals, showing that gathering information can, indeed, incur benefits (Copeland and Friedman, 1992). Importantly, such gains need not be monotonic in the level of information. In an experimental setup with multiple levels of informedness, Huber et al. (2008) show that only the best-informed traders can profit from their informational advantage, while in Huber (2007) average-informed traders obtain significantly lower returns than the worst-informed. On financial markets, less information can be better.

However, as these studies were carried out in zero-sum settings where trading gains are shifted across traders, their predictions have no bite in contexts where individuals trade in a non-interactive manner as price-takers on financial markets. Blackwell (1951) provides a well-known theoretical argument for individual decision-making, claiming generally that a rational prediction procedure can never perform better with less information. But these results need not hold true if individuals incorporate information incorrectly into their decision-making. Recent studies have accumulated rich evidence suggesting that this may indeed be true for individuals on the stock market. Frydman and Wang (2019), making use of a natural experiment, show that changes in stock price salience, while leaving available information unchanged, increases investors' tendencies to sell winning stocks and hold on to losers. Biais et al. (2005) provide evidence from a computer laboratory experiment, linking participants' overconfidence with their trading behaviour.

Given these insights, surprisingly little research has been done on how systematic changes in information management may affect trading behaviour in non-interactive contexts. As an early example one may consider Benartzi and Thaler (1995), showing theoretically that if stock traders are both myopic and loss-averse, they tend to select excessively non-risky portfolios with detrimental effects to their trading profits. Conducting a direct experimental test of the theory, Gneezy and Potters (1997) show that this tendency can be reversed by decreasing the frequency in which subjects receive performance updates on their allotted portfolios. Through the channel of more risky portfolios, restricted access to information leads to significant increases in returns. Andries and Haddad (2019) further argue theoretically that individuals may benefit from deliberately restricting their access to information for purely affective reasons. Loss averse traders, despite losing out on returns, may enjoy net-utility benefits from less frequent updates on their portfolio performances. With this article, I hope to contribute to this discussion.

2 Experimental Design

In order to study the impact of varying information sets on trading outcomes in stock markets, I conduct a computer laboratory experiment. All experimental treatments will merely vary the information which participants receive while leaving everything else unchanged. I will therefore begin by explaining the commonalities throughout all of the experiment's iterations and then describe how the individual treatments differ in design.

Participants trade on the basis of real-life stock data, in particular monthly returns from stocks listed on Standard and Poor's 500 Index from January 1960 to July 2017 (all data taken from the Bloomberg Terminal). At the beginning of each round, participants are randomly allotted a random stock in a random month from the basket. For each of these rounds, they must then make a binary stylised speculative trading decision: they either hold their stock or trade it.

If a participant decides to hold the stock, she will simply receive the returns of her allotted stock for the allotted month. If, on the other hand, she decides to trade, she will receive the returns for another random stock at a random month, while a fixed commission fee of five percentage points will be deducted from her earnings for the round. The net profits, denominated in percent of return, are then added to (or subtracted from) the participant's total earnings at the end of each round. At the end of the experiment, these earnings are converted and paid out at a rate of $1\% = \pounds 0.08$. Participants receive an additional fix reward of $\pounds 6$ for completing an experiment. Overall earnings are capped from below at $\pounds 6$ and from above at $\pounds 20$.

In order to make this decision, participants in the control group receive in-

formation on the past performance of their allocated stock. Specifically, they are shown a graph of the returns the stock has yielded in each of the past 10 months (see Figure 9 in the appendix for an example). There are 30 rounds in the experiment and, up to the accumulation of earnings across periods, all rounds are technically independent from each other so that nothing the participant does in any given round may affect what will happen in another. Also, all participants trade independently of another, so that they may not affect each other.

As illustrated in Figure 2, there are two treatments in the experiment implemented in a 2×2 design. All experimental groups are played in each session, where participants are randomly allocated according to the four quadrants in Figure 2 and remain in their given treatment for the entirety of the session. Following the main experiment, all participants are asked to answer a short, non-incentivised follow-up questionnaire. Personal characteristics elicited from the survey will be used to supplement the empirical analysis (see Section 4).

In total, I conducted 18 sessions with a total of 352 participants. All sessions were held in the University of Glasgow, with 15 sessions from October 2017 to February 2018 and 3 further sessions in February 2019. All sessions lasted around 60 minutes. The experiment was fully computerised using z-Tree of Fischbacher (2007).

Figure 2: 2×2 experimental design.

Control	Detailed Feedback (Treatment I)
Information-Filter (Treatment II)	Treatment I & II

2.1 Experimental Treatments

Treatment I: Detailed Feedback

In this treatment, participants receive more information than the control group. In particular, participants will receive detailed feedback after each round regarding their performance (see Figure 10 for an exemplary screen). Investors receive information on the gross performance of their chosen option and the resulting net profits. Also, they are told how much they would have earned if they would have

chosen differently, including a split of net profit posts. Finally, they are prompted with an explicit evaluation whether, in the given instance, they have chosen the most profitable option or not.

Treatment II: Information Filter

In this treatment, participants receive less information than in the control group. In particular, participants are subject to an algorithmic information filter which determines whether they will receive information on their allotted stock's performance history.

The discrimination on whether the participants receive this information is implemented as follows: Each round, a random natural number z from 0 to 9 is drawn with uniform probabilities. The algorithm then counts the successive run of either positive or negative returns of the participant's allotted stock for the given round, starting with the most recent return. Let θ denote this number. For example, if the stock yielded positive returns for the last three months preceded by a negative return, $\theta = 3$. If conversely the stock yielded negative returns successively for the last three months preceded by a positive return, then also $\theta = 3$. If the stock yielded positive returns for the last month preceded by nine negative returns, then $\theta = 1$.

The algorithm then compares z and θ . If $\theta > z$, then the participant will indeed receive information on the stock's past performance and the participant will follow perfectly the protocol of the control group for the round. If the converse holds true, with $\theta \leq z$, then the participant will not receive any information on the allotted stock's performance, but will instead be told that information for the round has been restricted (see Figure 10 for a screen display). The algorithm therefore tends to flag exceptions and suppress relatively moderate outcomes, subject to the random draw of z .

2.2 Data and Randomisation Check

Table 1 provides the means, standard deviation and minimum and maximum realisations of variables elicited in the experiment. Panel A lists variables on trading outcomes elicited during the main experiment. Net Returns denotes the participants' returns earned in a given period from stocks minus commission fees subject to trading activity. Gross Returns denotes the same outcome without deducting potential commission fees. Trading denotes the participant's choice in any given period to trade (coded with 1) or not (coded with 0). As can be seen, net returns are highly volatile, featuring a low absolute mean and a high standard deviation. This will make it difficult, although not impossible, to find statistically significant effects on this outcome due to the substantial noise.

Table 1: Descriptive Statistics

	Mean	Std. Dev.	Min.	Max.
	(1)	(2)	(3)	(4)
Panel A: Trading Outcomes				
Net Returns	-0.24	10.32	-79.97	171.78
Gross Returns	1.42	10.05	-79.97	176.78
Trading	0.33	0.47	0	1
Panel B: Individual Characteristics				
Male	0.40	0.49	0	1
Age	24.77	7.54	17	69
Riskseek. Index	0.50	0.17	0	1
Overconf. Index	0.72	0.16	0.1	1
Gambling Index	0.17	0.26	0	1
Stock Exper. Basic	0.20	0.40	0	1
Stock Exper. Adv.	0.13	0.34	0	1
Low Self Est. Index	0.63	0.17	0.06	1
Irregular BMI	0.26	0.44	0	1

Notes: This table provides descriptive statistics within the sample for some key variables. ‘Trading Outcomes’ are noted at the decision-level (30 per participant), while ‘Individual Characteristics’ are stated at the participant-level.

Panel B in Table 1 lists select variables related to participants’ personal characteristics, based on information in the experiment’s follow-up questionnaire. I will now provide an account of how these measures are constructed, top to bottom. Screenshots of all the underlying questions are available in the appendix (see Appendix A). The variable Male is coded with 1 if the participant reported to be male and 0 otherwise. Age simply repeats the stated age. I follow the methodology of Holt and Laury (2002) to elicit participants’ risk sentiment. Participants have to choose ten times between two lotteries. For the Riskseeking Index, I count the number of risky choices and divide it by ten. For the Overconfidence Index, inspired by Biais et al. (2005), I use a version of the survey tool introduced in Alpert and Raiffa (1982). Participants are given ten questions such as: ‘What is the height of the Mt. Everest (in meters)’. They are then instructed to give both a low and a high estimate, as close as possible to what they believe the true answer is but such that they are 90% certain that the true answer lies in the stated interval. To arrive at the index, I then count the number of times that the true answer did, in fact, lie outside the stated interval and divide this number by the

Table 2: Randomisation Check, Part 1: Means by
Randomisation Groups

	Top Left (1)	Top Right (2)	Bottom Left (3)	Bottom Right (4)
Male	0.43 (0.50)	0.39 (0.49)	0.37 (0.49)	0.44 (0.50)
Age	24.25 (5.09)	25.44 (9.86)	24.84 (7.07)	24.55 (7.44)
RiskSeek. Index	0.47 (0.17)	0.50 (0.18)	0.52 (0.17)	0.50 (0.16)
Gambling Index	0.19 (0.28)	0.11 (0.22)	0.17 (0.28)	0.18 (0.26)
Overconf. Index	0.70 (0.17)	0.73 (0.16)	0.73 (0.14)	0.73 (0.17)
Stock Exper. Basic	0.21 (0.41)	0.22 (0.41)	0.22 (0.41)	0.16 (0.37)
Stock Exper. Adv.	0.16 (0.37)	0.12 (0.33)	0.14 (0.35)	0.09 (0.29)
Low Self Est. Index	0.65 (0.15)	0.63 (0.18)	0.62 (0.17)	0.62 (0.17)
Irregular BMI	0.27 (0.44)	0.25 (0.43)	0.22 (0.41)	0.30 (0.46)

Notes: This table provides the means (with standard deviations in parentheses) of some key personal characteristics for each of the experiment's four randomisation groups (see Figure 2).

total amount of questions. For the Gambling Index, participants can state four levels of intensity of gambling activity in their weekly routines. I code these four levels in ascending intensity with 0, 0.33, 0.66 and 1. Both Stock Experience Basic and Stock Experience Advanced are dummy variables aimed at eliciting the level of participants' experience in trading stocks, based on simple 'yes' or 'no' questions. For the Irregular BMI variable, I first use stated height and weight in order to

calculate the Body Mass Index according to the standard formula $\frac{\text{Weight(kg)}}{\text{Height(m)}^2}$. I then compare this number to the standardised benchmarks 18.5 and 25 to check whether a person classifies as underweight or overweight, respectively.

Table 3: Randomisation Check, Part 2: Differences across Randomisation Groups

	Col. 1-2 (1)	Col. 1-3 (2)	Col. 1-4 (3)	Col. 2-3 (4)	Col. 2-4 (5)	Col. 3-4 (6)
Male	0.033 (0.074)	0.056 (0.073)	-0.008 (0.075)	0.022 (0.073)	-0.042 (0.075)	-0.064 (0.074)
Age	-1.191 (1.176)	-0.584 (0.924)	-0.294 (0.963)	0.606 (1.286)	0.896 (1.329)	0.289 (1.100)
Riskseek. Index	-0.029 (0.027)	-0.041 (0.026)	-0.023 (0.025)	-0.012 (0.026)	0.005 (0.026)	0.017 (0.025)
Gambling Index	0.074* (0.038)	0.014 (0.042)	0.006 (0.042)	-0.059 (0.038)	-0.068* (0.037)	-0.008 (0.042)
Overconf. Index	-0.034 (0.025)	-0.038 (0.024)	-0.035 (0.026)	-0.003 (0.023)	-0.000 (0.026)	0.002 (0.024)
Stock Exper. Basic	-0.011 (0.062)	-0.011 (0.062)	0.048 (0.059)	0 (0.062)	0.060 (0.060)	0.060 (0.060)
Stock Exper. Adv.	0.044 (0.053)	0.022 (0.054)	0.074 (0.051)	-0.022 (0.051)	0.029 (0.047)	0.051 (0.049)
Low Self Est. Index	0.020 (0.025)	0.033 (0.025)	0.031 (0.025)	0.012 (0.027)	0.011 (0.026)	-0.001 (0.026)
Irregular BMI	0.016 (0.067)	0.051 (0.065)	-0.027 (0.070)	0.034 (0.034)	-0.044 (0.069)	-0.079 (0.068)

Notes: This table provides a randomisation check for the experiment. It shows the differences between the means of participant characteristics denoted in Table 2 (standard errors in parantheses). Alternative hypothesis is the two-sided inequality. Statistical significance denoted by: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

I use information regarding the participants' personal characteristics to check

whether there are significant differences between the samples of the randomisation groups (the four quadrants in Figure 2). This verifies the success of the experiment's randomisation. Table 2 displays the means and standard deviations of some personal characteristics for the four subsamples. Based on these, Table 3 checks for significant differences across all combinations of the subgroups. I find that out of 60 comparisons, only two are significant at the 10% level, which is consistent with chance.

3 Results

In this section I will examine the effectiveness of the experimental treatments in curbing excessive trading. I choose the most granular data as the unit of observation, meaning trading choice per participant per period. Letting i index participants, j sessions and t experimental periods, the regression equation can thus be written as follows:

$$\begin{aligned} TRADING_{ijt} = & \beta_0 + \beta_1 \cdot FEEDBACK_{ij} + \beta_2 \cdot FILTER_{ij} \\ & + \beta_3 \cdot INTERACT_{ij} + \phi_j + \tau_t + \varepsilon_{ijt} \end{aligned} \quad (1)$$

where $TRADING_{ijt}$ denotes a participant's choice for a given period, coded 0 if the participant decided to hold and 1 if the participant decided to trade. $FEEDBACK_{ij}$ and $FILTER_{ij}$ are dummy variables for the respective experimental treatments and $INTERACT_{ij}$ is a dummy variable capturing potential interaction effects for participants who receive both experimental treatments. ϕ_j and τ_t are dummy vectors controlling for potential session- and round-fixed effects which will be added and omitted from the analysis for robustness. The error term is ε_{ijt} .

Table 4 shows the regression coefficients from a pooled OLS-regression with robust standard errors. As can be seen, both treatments lead to a large and highly significant reduction in the participants' trading activity in all model specifications. Compared to the average trading activity of participants receiving neither treatment, the effects of the *FEEDBACK* and *FILTER* treatments represent cuts of more than 13% and 17%, respectively. Importantly, the 2×2 experimental design (see Figure 2) allows for an examination of the combined effects of both treatments, represented by $\beta_1 + \beta_2 + \beta_3$ in Equation (1). As the coefficient of the *INTERACTION* term is very close to 0, the evidence suggests that the effects from both treatments stack very well. Indeed, trading activity for those participants who receive both treatments is more than 30% lower than for those who receive neither.

Table 4: Effect of Experimental Treatments on Trading

	(1) Trading	(2) Trading	(3) Trading	(4) Trading
<i>FEEDBACK</i>	-0.0554*** (-4.20)	-0.0550*** (-4.16)	-0.0554*** (-4.21)	-0.0550*** (-4.16)
<i>FILTER</i>	-0.0734*** (-5.60)	-0.0717*** (-5.47)	-0.0734*** (-5.60)	-0.0717*** (-5.47)
<i>INTERACT</i>	0.00438 (0.24)	0.00470 (0.26)	0.00438 (0.24)	0.00470 (0.26)
Fixed Effects				
Session		✓		✓
Period			✓	✓
<i>N</i>	10560	10560	10560	10560

Notes: This table provides the coefficient estimates of the experimental treatments on the participant's trading choices. *t* statistics in parentheses. Statistical significance is denoted as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5, derived from the same regression exercise as Equation (1), with Net Returns (calculated as Gross Returns minus potential transaction costs) as the dependent variable, reveals that these changes in trading behaviour lead to the expected changes in trading profits. As monthly stock returns are by nature highly volatile, I exclude realisations beyond the 1st- and 99th-percentiles of the net return-distribution from the analysis in order to ensure that it is not driven by a few outliers. As can be seen, participants receiving the Feedback Treatment achieve 0.42 percentage points higher net returns from trading, an increase significant at the 10%-level.

The Filter Treatment shows qualitatively similar results, although attenuated both in effect strength as well as statistical significance. A stronger picture crystallises, if focus is restricted on rounds where the historic price information has been indeed suppressed. In Table 6, INFO RESTRICTION is a dummy variable switching on for any period in which participants did not receive such information. As can be seen, Net Returns in such periods is then about 0.30 percentage points higher compared to all other rounds, marking a significant increase at the 10%-level.

Table 5: Treatment Effects on Net Returns

	(1)	(2)	(3)	(4)
	Net Returns	Net Returns	Net Returns	Net Returns
<i>FEEDBACK</i>	0.448* (1.92)	0.432* (1.84)	0.447* (1.92)	0.432* (1.84)
<i>FILTER</i>	0.357 (1.55)	0.338 (1.34)	0.357 (1.55)	0.337 (1.46)
<i>INTERACT</i>	-0.232 (-0.71)	-0.205 (-0.63)	-0.230 (-0.70)	-0.204 (-0.62)
Fixed Effects				
Session		✓		✓
Period			✓	✓
<i>N</i>	10350	10350	10350	10350

Notes: This table provides the coefficient estimates of the experimental treatments on net returns. Outliers beyond the 1st- and 99th-percentile in the net return-distribution have been dropped. *t* statistics in parentheses. Statistical significance is denoted as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Effect of Information Restriction on Net Returns

	(1)	(2)	(3)	(4)
	Net Returns	Net Returns	Net Returns	Net Returns
INFO RESTRICTION	0.302* (1.81)	0.291* (1.74)	0.304* (1.82)	0.293* (1.75)
Fixed Effects				
Session		✓		✓
Period			✓	✓
<i>N</i>	10320	10320	10320	10320

Notes: This table provides the coefficient estimates of information restriction on net returns. The independent variable is a dummy, switching on if information on the historic returns (subject to the algorithm in the information filter treatment) has indeed been restricted. Unit of observation is the decision. Outliers beyond the 1st- and 99th-percentile in the net return-distribution have been dropped. *t* statistics in parentheses. Statistical significance is denoted as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

It should be stressed that, apart from their significance, these effects are large. For comparison, the mean net returns for participants receiving none of the treatments, excluding outliers beyond the 1st- and 99th-percentile, lay at -0.69% . Moreover, the effects from both treatments stack reasonably well with a combined effect of a 0.56 percentage point increase (combined effect significant at the 5%-level with p-value 0.0174). Both interventions together are thus able to reverse more than 80% of the losses incurred from overtrading.

4 Discussion

4.1 Overconfidence and Gambling

Although the phenomenon of overtrading amongst private individuals, *i.e.* high trading volumes leading to subpar net returns, is empirically well-established, its causes are subject to debate. A strand of literature contends that the observed behaviour is the result of mistakes in decision-making, stemming from psychological biases, rather than a rational process. This hypothesis dates back at least to Bondt and Thaler (1985), who show that actors in the stock market overreact to price movements. Biais et al. (2005) support this view with experimental evidence, showing that overtrading is linked to a particular of a psychological disposition towards overreacting dubbed ‘overconfidence’: when predicting future events, people do not sufficiently account for randomness and overweigh their private information.

While Grinblatt and Keloharju (2009) verify this result with real-life data, they propose a second, independent driver for overtrading. The authors find that trading activity correlates at the individual level with the amount of speeding tickets received. As an explanation, they argue that both overtrading and speeding may be the result of *sensation seeking*. Stock trading, in this case, assumes the role of a thrilling gambling activity. Gao and Lin (2014) further corroborate this hypothesis with findings from natural experiments, showing that individuals partially substitute stock trading with playing the lottery. In this case, people may ‘consume’ overtrading, trading off its leisure value with pecuniary drawbacks.

In this section, I use information from the experiment’s follow-up questionnaire to contribute to this discussion. Table 7 depicts the extent to which personal characteristics determines the participants’ trading behaviour (see Section 2.2 for a detailed description of the construction of all variables). The results support both strands of literature. In all specifications, an individual’s gambling frequency and overconfidence are both the most consistent (significant at the 1%-level) as well as the strongest (evaluated at a change of 1 standard deviation in the all characteristics) predictors of overtrading.

Table 7: Effects of Personal Characteristics
on Trading Choices.

	(1)	(2)	(3)	(4)
	Trading	Trading	Trading	Trading
Male	-0.0200** (-2.00)	-0.0243** (-2.38)	-0.0200** (-2.00)	-0.0243** (-2.38)
Age	0.00121* (1.80)	0.00165** (2.29)	0.00121* (1.80)	0.00165** (2.29)
Riskseek. Index	-0.0325 (-1.20)	-0.0337 (-1.22)	-0.0325 (-1.21)	-0.0337 (-1.22)
Gambling Index	0.0560*** (3.08)	0.0607*** (3.23)	0.0560*** (3.08)	0.0607*** (3.23)
Stock Exper. Basic	-0.0253 (-1.50)	-0.0236 (-1.37)	-0.0253 (-1.50)	-0.0236 (-1.37)
Stock Exper. Adv.	-0.0117 (-0.61)	-0.00850 (-0.43)	-0.0117 (-0.61)	-0.00850 (-0.43)
Low Self Est. Index	0.00557 (0.20)	0.0220 (0.77)	0.00557 (0.20)	0.0220 (0.77)
Overconf. Index	0.106*** (3.77)	0.104*** (3.54)	0.106*** (3.77)	0.104*** (3.54)
Irregular BMI	0.0147 (1.34)	0.0122 (1.10)	0.0147 (1.34)	0.0122 (1.10)
Fixed Effects				
Session		✓		✓
Period			✓	✓
Observations	10200	10200	10200	10200

Notes: This table provides the coefficient estimates of participants' personal characteristics on their trading choices. t statistics in parentheses. Statistical significance is denoted as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Effects of Personal Characteristics on Net Returns

	(1)	(2)	(3)	(4)
	Net Returns	Net Returns	Net Returns	Net Returns
Male	0.0852 (0.47)	0.0902 (0.49)	0.0878 (0.49)	0.0929 (0.51)
Age	-0.00682 (-0.57)	-0.00922 (-0.71)	-0.00681 (-0.57)	-0.00917 (-0.71)
Riskseek. Index	0.721 (1.47)	0.871* (1.72)	0.723 (1.48)	0.873* (1.73)
Gambling Index	-0.561* (-1.77)	-0.588* (-1.77)	-0.562* (-1.77)	-0.589* (-1.78)
Stock Exper. Basic	0.116 (0.38)	0.0852 (0.27)	0.119 (0.39)	0.0878 (0.28)
Stock Exper. Adv.	0.367 (1.07)	0.308 (0.88)	0.361 (1.05)	0.301 (0.86)
Low Self Est. Index	-0.258 (-0.51)	-0.281 (-0.55)	-0.254 (-0.51)	-0.277 (-0.54)
Overconf. Index	0.123 (0.24)	0.120 (0.22)	0.115 (0.22)	0.112 (0.21)
Irregular BMI	-0.296 (-1.52)	-0.260 (-1.30)	-0.293 (-1.50)	-0.256 (-1.28)
Fixed Effects				
Session		✓		✓
Period			✓	✓
<i>N</i>	9992	9992	9992	9992

Notes: This table provides the coefficient estimates of participants' personal characteristics on their net returns. Outliers beyond the 1st- and 99th-percentile in the net return-distribution have been dropped. *t* statistics in parentheses. Statistical significance is denoted as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The results, however, contrast with findings from Barber and Odean (2001).

Leaning on previous psychological evidence claiming that in the financial domain men are more overconfident than women, the authors adopt a trader's gender as a proxy for the tendency to act overconfidently. They then show that men more than women tend to trade excessively. My results do not show any systematic differences between men and women with respect to overconfidence (p-value for two-sided t -test is 0.42). Indeed, as seen in Table 7, male participants trade less than female ones.

It should be mentioned, though, that of the discussed personal characteristics only gambling activity translates to a statistically significant effect on trading profits (see Table 8).

4.2 Robustness Towards Risk as a Channel

This article support the argument that information management can curb excessive trading in a non-interactive stock trading task as well as increasing the profits gained from it. These results could, however, simply be generated by a shift in the participants' risk adjustments. For example, traders could have a desire to hold low-risk portfolios and be willing to give up some expected returns in order to meet this goal. Therefore, if they observe that their allocated stock's returns have been highly volatile over the past months, they may rationally wish to swap this stock for an (expectedly) lower-risk, lower-payoff one. In turn, restricting access to such information would then implicitly foster risk-taking, decrease trading activity and increase expected returns.

Indeed, Gneezy and Potters (1997) find in a lab experimental setting that risk-taking and trading profits increase if feedback on performance information is supplied less frequently, while Larson et al. (2016) find in a natural experiment that informing traders less frequently on prices leads to the same qualitative behaviour. An unsuccessful replication by Beshears et al. (2016), on the other hand, calls this effect into question.

Risk considerations, however, do not appear to drive this paper's findings. In Table 9, Net Return Std. measures the volatility of participants' period-by-period net returns through their standard deviation (column (2) displays the same for gross returns). If the decreased trading volume and increased returns in the experimental treatments were a result of increased willingness to hold risky stocks, one would expect greater volatility in the associated payoff stream. However, for both specifications, allocation to any treatment group actually, if anything, lowers payoff stream volatility. Thus, in terms of a risk-reward trade-off, the trading performance assisted by the experiment's proposed information management dominates the alternative.

Table 9: Effects of Experimental Treatments on Portfolio Risk

	(1) Net Return Std.	(2) Gross Return Std.
<i>FEEDBACK</i>	-0.685 (-1.40)	-0.600 (-1.20)
<i>FILTER</i>	-0.836* (-1.67)	-0.703 (-1.37)
<i>INTERACTION</i>	1.078* (1.70)	0.926 (1.43)
Fixed Effects		
Session	✓	✓
<i>N</i>	352	352

Notes: This table provides the coefficient estimates of the experimental treatments on portfolio risk. Unit of observation is the participant. Independent variables denote the standard deviation of the participants' returns earned throughout all periods; in col. (1) potential commission fees are deducted, in col. (2) not. Treatment variables are dummies, switching on if a participant belongs to the respective treatment. t statistics in parentheses. Statistical significance is denoted as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Also, there is no evidence that participants attempt to trade out of risky stock allocations. In Table 10, the variable Hist Ret. Std. captures the volatility of the historical return of the participant's allocated stock. Naturally, the sample is restricted to periods where this information was not suppressed by the FILTER treatment and participants could indeed observe the stock's return history. Again, it can be seen that in both specifications higher observed volatility rather leads to less trading activity than more, although the effect size is very small. I therefore argue that the benefits from the experiment's information interventions arise from novel channels which are independent from those put forward by Gneezy and Potters (1997).

Table 10: Effect of Allocated Stock Risk on Trading Activity

	(1)	(2)	(3)	(4)
	Trading	Trading	Trading	Trading
Hist. Ret. Std.	-0.000244*	-0.000234*	-0.000227	-0.000216
	(-1.73)	(-1.72)	(-1.50)	(-1.48)
Fixed Effects				
Session		✓		✓
Period			✓	✓
<i>N</i>	6346	6346	6346	6346

Notes: This table provides the coefficient estimates of historic risk on trading activity. Unit of observation is the decision. "Hist. Ret. Std." denotes the standard deviation of the historic returns of the allocated stock. The sample is restricted to decisions in which the participant could see this information. *t* statistics in parentheses. Statistical significance is denoted as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5 Conclusion

Excessive trading is a robust real-life phenomenon leading to economically large losses. In this paper, I present experimental evidence on functional counter-strategies. Both proposed measures lead to sizeable reductions in trading activity as well as increases in trading profits. Moreover, as the interventions are purely informational, these findings reveal new insights into the value of information in non-interactive stock trading by private individuals. Although I find evidence that providing performance feedback increases trading profits, perhaps even more surprising is that also restricting information on noisy predictors reduces trading activity.

The analysis further suggests that the results are not generated through increased risk-taking, a channel proposed in a similar setting by Gneezy and Potters (1997). I favour the explanation of Mosenhauer (2019) who shows theoretically that if decision-makers overreact to certain information, overall decision-making may be improved by reducing access to this information. This hypothesis is corroborated by evidence suggesting that overtrading is at least partly driven by an individual's overconfidence and tendency to gamble.

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Appendix A Materials from the Experiment

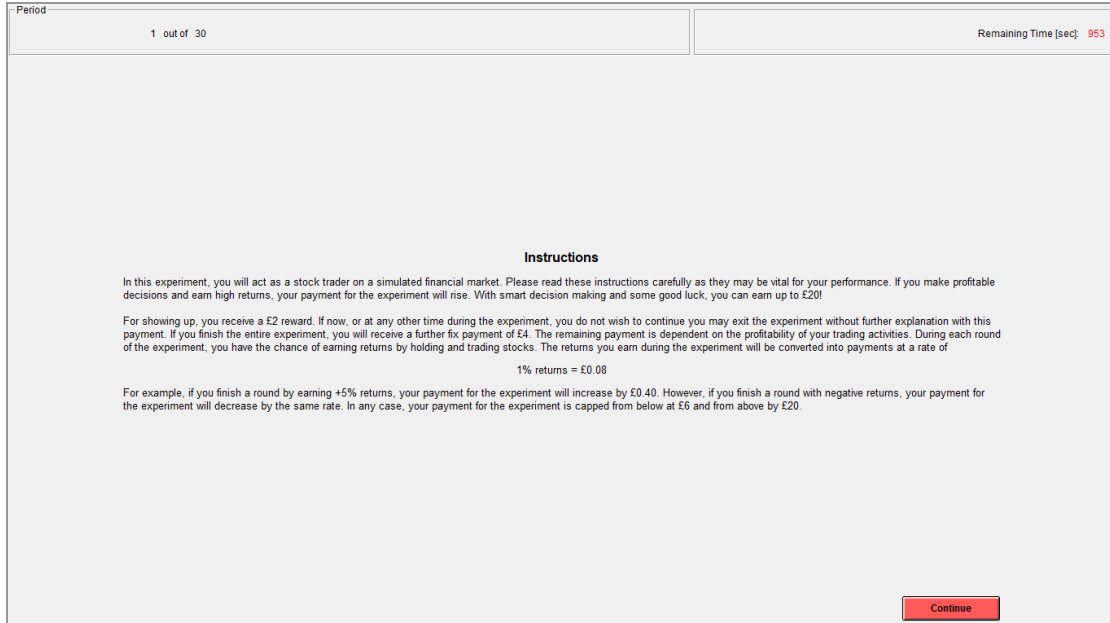


Figure 3: First instructions screen, common for all participants.

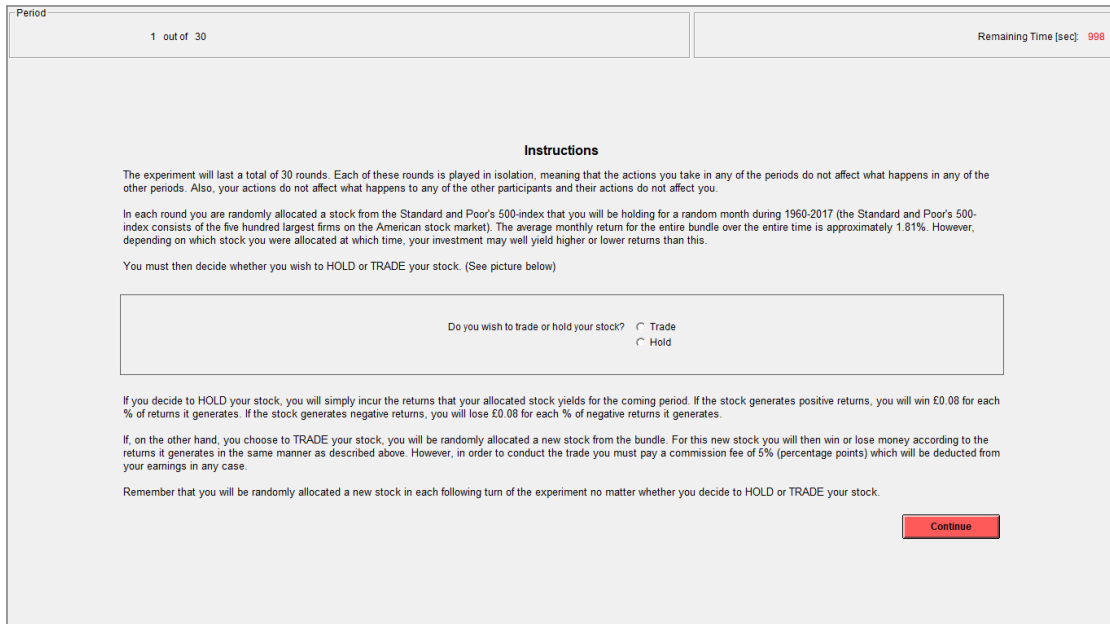


Figure 4: Second instructions screen, common for all participants.

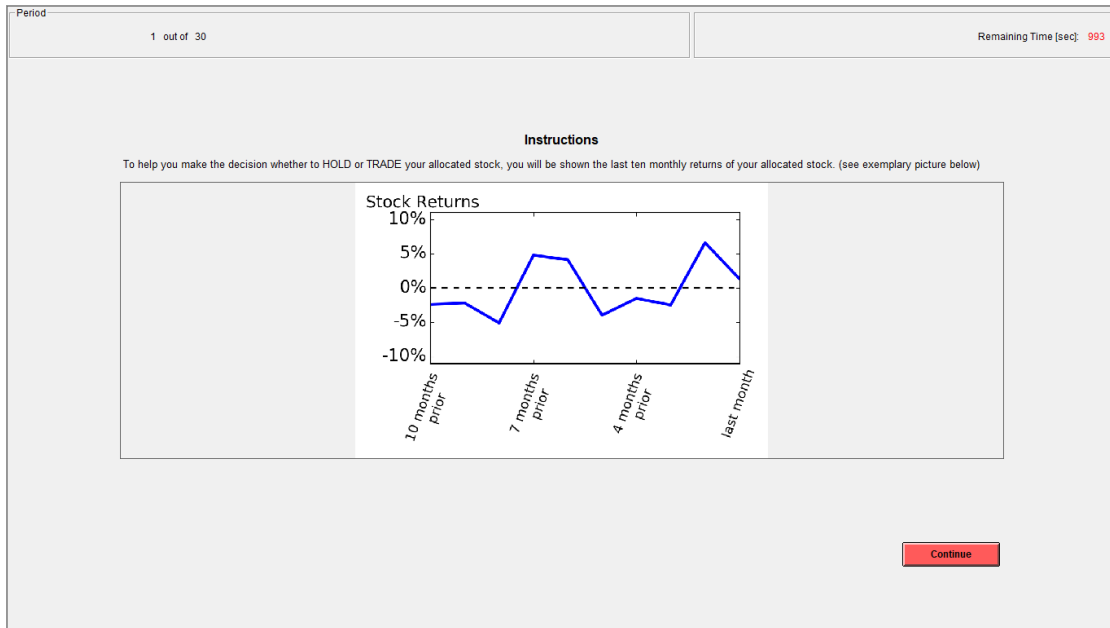


Figure 5: Third instructions screen for Control Group.

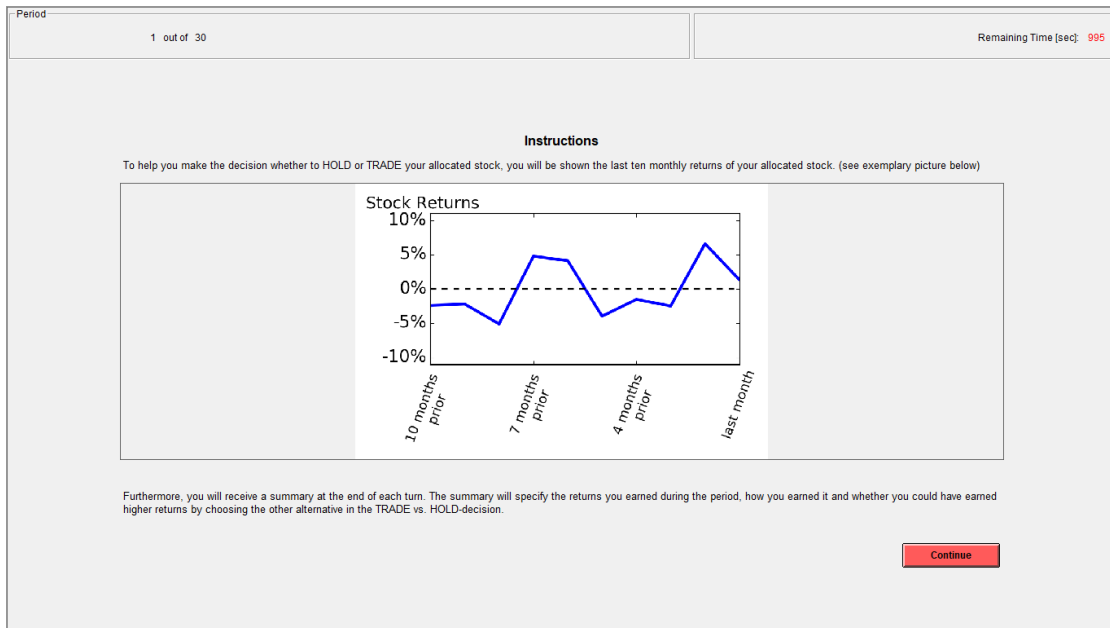


Figure 6: Third instructions screen for Detailed Feedback Treatment.

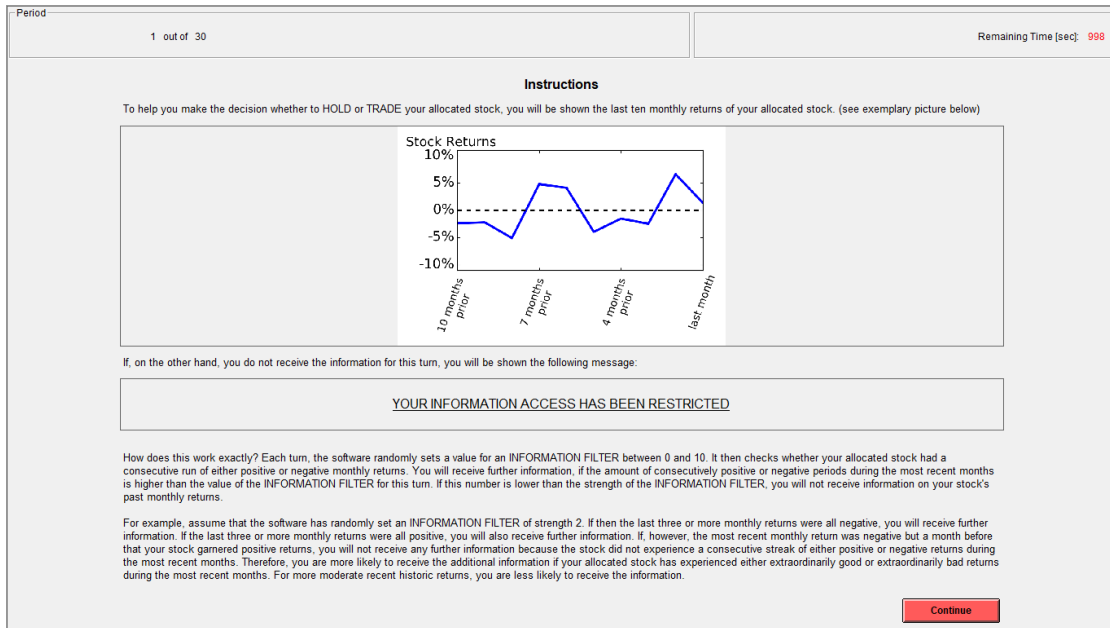


Figure 7: Third instructions screen for Information Filter Treatment.

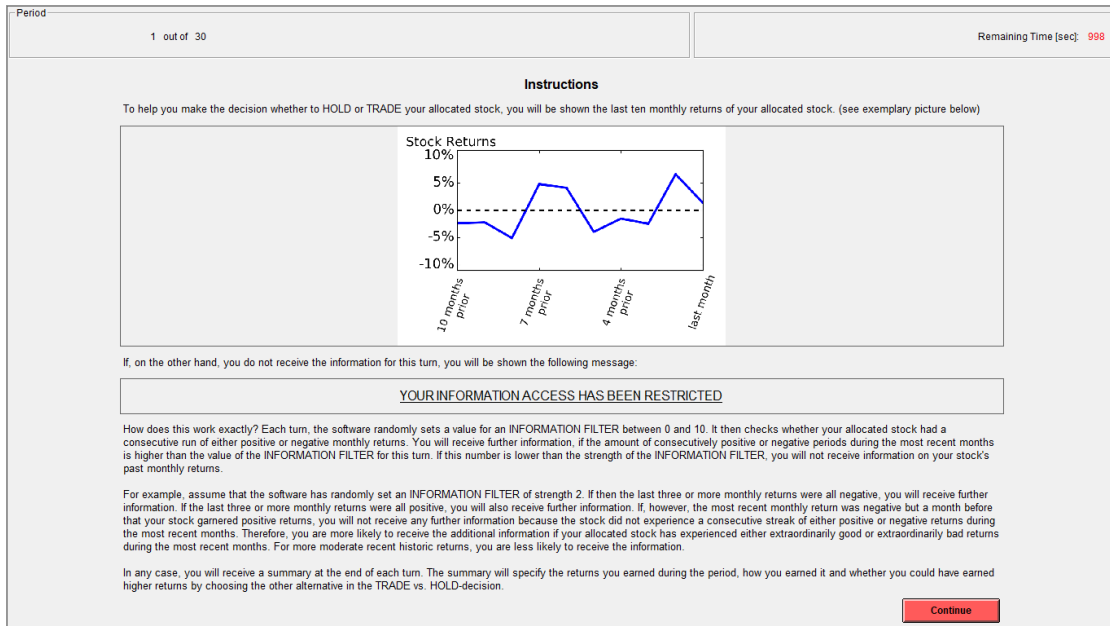


Figure 8: Third instructions screen for Treatment Interaction Group.

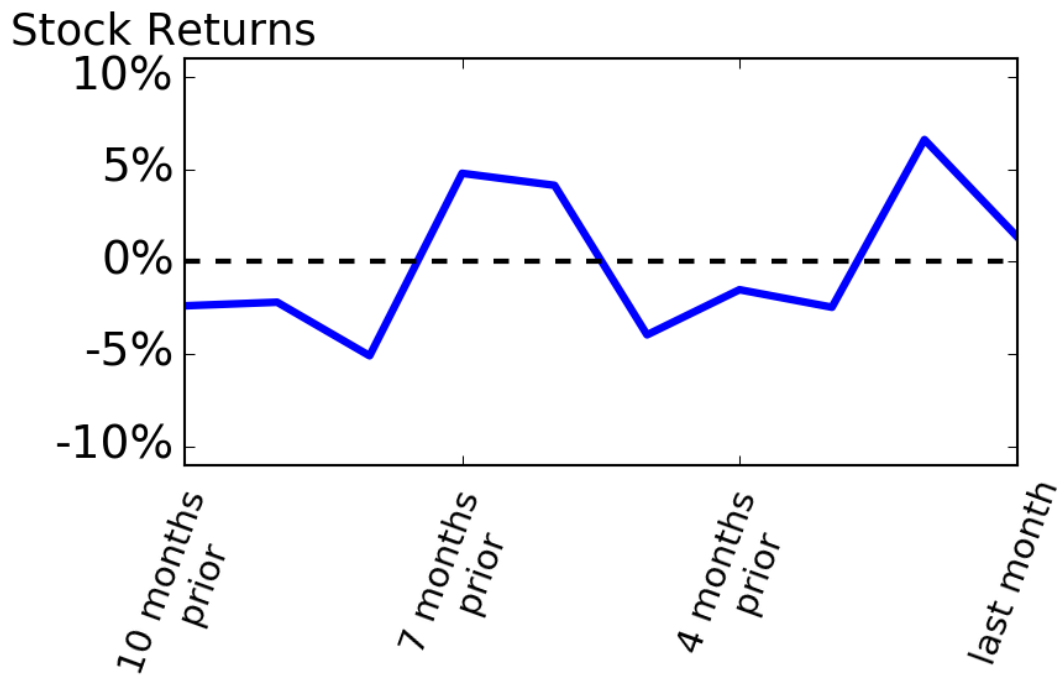


Figure 9: Exemplary information of allotted stock's performance history in baseline group.

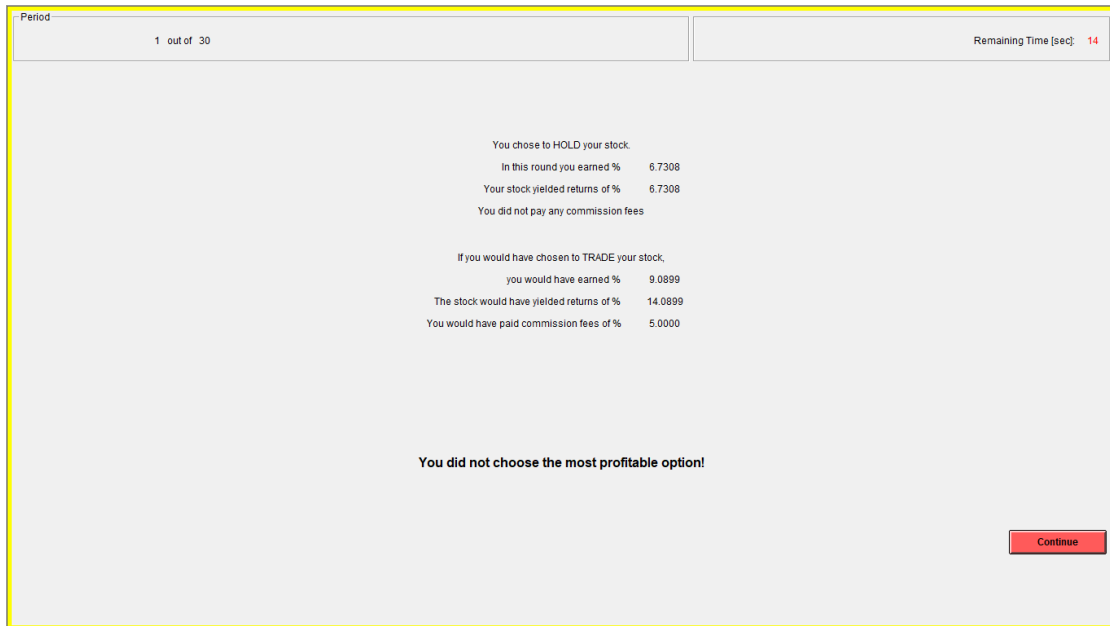


Figure 10: Exemplary screen display for the Detailed Feedback Treatment.

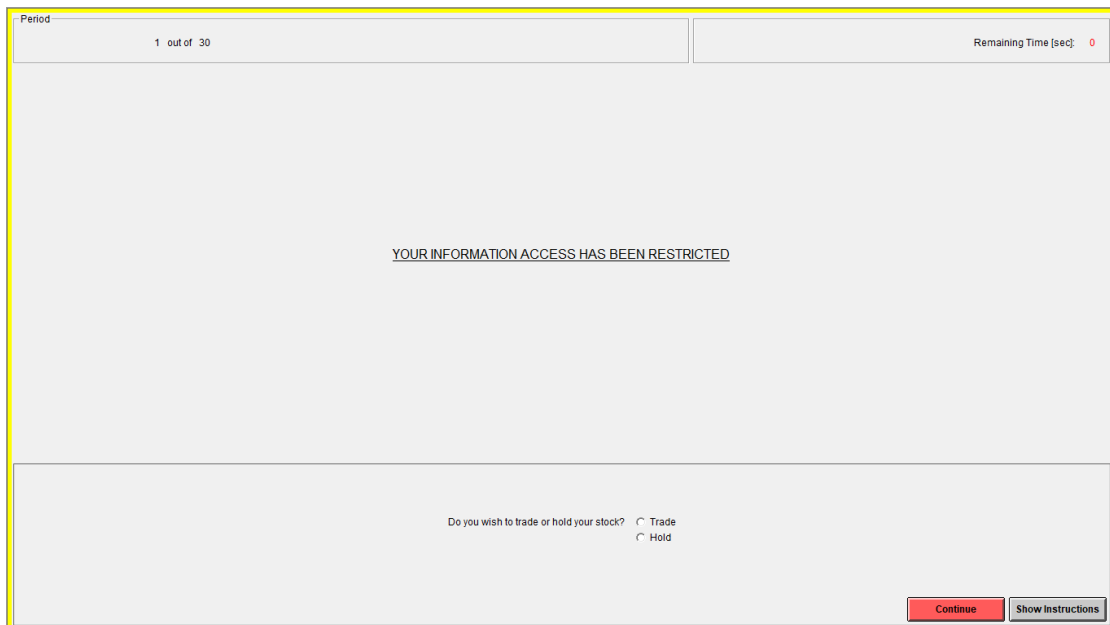


Figure 11: Exemplary screen display for the Information Filter Treatment when information is restricted.

Questionnaire

I am male
 female
 other

My age is (in years)

My height is (in cm)

My weight is (in kg)

The total amount of wealth (e.g. money in current and savings accounts, financial assets, real estate etc.) that I personally own is approximately (in pounds)

Figure 12: Follow-up questionnaire, first screen display

Questionnaire

At some point in my life, I held shares of some stock. yes
 no

I have personally bought or sold shares of some stock during the last three years. yes
 no

The total amount of speeding tickets I received in my life is

During the last twelve months, I have invested money in gambling activities (e.g. playing lottery, card games, betting on sports, roulette etc.) never
 1-5 times
 6 - 10 times
 more than 10 times

I have been diagnosed with dyscalculia. (r/r \i (Dyscalculia is difficulty in learning or comprehending arithmetic, such as difficulty in understanding numbers, learning how to manipulate numbers and learning facts in mathematics.)) yes
 no

Figure 13: Follow-up questionnaire, second screen display

Questionnaire

For the following question, please make ten choices. For each choice, decide whether you would rather play lottery A or lottery B and tick the circle next to your preferred option. In making these decisions, consider the possible payoffs and the chances of winning them for both alternatives.

EXAMPLE: Consider CHOICE 1. In lottery A, you will win £2 with a 10% chance and £1.60 with a 90% chance. In lottery B, you will win £3.85 with a 10% chance and £0.10 with a 90% chance. For CHOICE 10, in lottery A you will win £2 for sure and in lottery B you will win £3.85 for sure.

	Lottery A	Lottery B
CHOICE 1:	1/10 of £2, 9/10 of £1.60 <input type="radio"/>	<input type="radio"/> 1/10 of £3.85, 9/10 of £0.10
CHOICE 2:	2/10 of £2, 8/10 of £1.60 <input type="radio"/>	<input type="radio"/> 2/10 of £3.85, 8/10 of £0.10
CHOICE 3:	3/10 of £2, 7/10 of £1.60 <input type="radio"/>	<input type="radio"/> 3/10 of £3.85, 7/10 of £0.10
CHOICE 4:	4/10 of £2, 6/10 of £1.60 <input type="radio"/>	<input type="radio"/> 4/10 of £3.85, 6/10 of £0.10
CHOICE 5:	5/10 of £2, 5/10 of £1.60 <input type="radio"/>	<input type="radio"/> 5/10 of £3.85, 5/10 of £0.10
CHOICE 6:	6/10 of £2, 4/10 of £1.60 <input type="radio"/>	<input type="radio"/> 6/10 of £3.85, 4/10 of £0.10
CHOICE 7:	7/10 of £2, 3/10 of £1.60 <input type="radio"/>	<input type="radio"/> 7/10 of £3.85, 3/10 of £0.10
CHOICE 8:	8/10 of £2, 2/10 of £1.60 <input type="radio"/>	<input type="radio"/> 8/10 of £3.85, 2/10 of £0.10
CHOICE 9:	9/10 of £2, 1/10 of £1.60 <input type="radio"/>	<input type="radio"/> 9/10 of £3.85, 1/10 of £0.10
CHOICE 10:	10/10 of £2, 0/10 of £1.60 <input type="radio"/>	<input type="radio"/> 10/10 of £3.85, 0/10 of £0.10

Figure 14: Follow-up questionnaire, eliciting risk aversion

Abraham Lincoln's age at death.		Year in which W. A. Mozart was born.	
Low	<input type="text"/>	Low	<input type="text"/>
High	<input type="text"/>	High	<input type="text"/>
Length of the Nile River (in km).		Average duration of pregnancy of an Asian Elephant (in days).	
Low	<input type="text"/>	Low	<input type="text"/>
High	<input type="text"/>	High	<input type="text"/>
Number of countries that are members of the United Nations.		Diameter of the moon (in km).	
Low	<input type="text"/>	Low	<input type="text"/>
High	<input type="text"/>	High	<input type="text"/>
Number of categories in the Academy Awards (<i>The Oscars</i>).		Air distance from London to Tokyo (in km).	
Low	<input type="text"/>	Low	<input type="text"/>
High	<input type="text"/>	High	<input type="text"/>
Weight of an empty Boeing 747 (in kg).		Deepest known point in the oceans (in meters).	
Low	<input type="text"/>	Low	<input type="text"/>
High	<input type="text"/>	High	<input type="text"/>

Figure 15: Follow-up questionnaire, eliciting measure for overconfidence

Questionnaire

Below is a list of statements dealing with your general feelings about yourself. Please indicate how strongly you agree or disagree with each statement.

	Strongly Agree	Agree	Disagree	Strongly Disagree
On the whole, I am satisfied with myself.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
At times I think I am no good at all.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
I feel that I have a number of good qualities.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
I am able to do things as well as most other people.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
I feel I do not have much to be proud of.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
I certainly feel useless at times.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
I feel that I'm a person of worth, at least on an equal plane with others.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
I wish I could have more respect for myself.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
All in all, I am inclined to feel that I am a failure.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>
I take a positive attitude toward myself.	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>	<input type="radio"/>

[Continue](#)

Figure 16: Follow-up questionnaire, eliciting measure for self-esteem