The Causal Influence of Investment Goals on the Disposition Effect

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Abstract

The disposition effect describes investors' tendency to realize gains more frequently than losses. While it is one of the most robust findings in finance research and there is an extensive body of literature examining its theoretical foundation, relatively little attention so far has been paid to ways in which the disposition effect could be mitigated. This is surprising considering that the disposition effect entails negative wealth consequences. Therefore, we conduct an experiment that investigates the influence of investment goals on the disposition effect. We find that subjects who are provided with a specific investment goal exhibit a reversed disposition effect. This result stems from the fact that those subjects realize paper gains less frequently. Their behaviour with regards to losses, however, does not change significantly.

JEL Classification: D12, D14, G11, G41

Keywords: disposition effect, experimental finance, investment goal, behavioural biases

1 Introduction

The disposition effect describes investors' tendency to hold losing assets for too long while realizing paper gains too quickly (Odean (1998)). Since its first theoretical and empirical investigation by Shefrin and Statman (1985), a wide body of research has focused on the origins, as well as the economic and conceptual consequences of the phenomenon. As a result, the disposition effect has been found to be one of the most robust findings in finance research (Barber and Odean (2011)). It has been observed across various geographies (e.g., Bremer and Kato (1996), Brown et al. (2006), or Barber et al. (2009)), different asset classes (e.g., Genesove and Mayer (2001), Chang et al. (2016), or Heimer (2016)), and a variety of investor types with varying levels of investment sophistication (e.g., Grinblatt and Keloharju (2001), Barber et al. (2007), or Choe and Eom (2009)).

The fact that the disposition effect entails adverse consequences for investors' investment performance (see for example Garvey and Murphy (2004), Lee et al. (2008), or Roger (2009)), proves that it deserves exceptional attention. More specifically, Odean (1998) finds that the assets that investors sold too quickly as a result of the disposition effect continue to overperform over the subsequent periods, while the losing assets that these investors held on to for too long remain underperformers. This is particularly troubling considering the more recent developments where pension systems have shifted from defined benefit to defined contribution schemes in the United States (Lusardi and Mitchell (2014)) and where, as a consequence, investors increasingly make their own investment decisions (Rubaltelli et al. (2005)), especially via online trading platforms (Barber and Odean (2001)). While this is not a negative development in itself, cognitive biases have been found to be particularly pronounced in online environments (Barber and Odean (2002)). Hence, being exposed to the disposition effect in this context can incur significant costs, most notably for inexperienced private investors who are most prone to it (Shapira and Venezia (2001)).

While there is an extensive body of evidence on the the origins of the disposition effect (Kaustia (2010)), to date, there is limited literature that sets out to investigate potential ways to mitigate it (Ploner (2017)). In this regard, we agree with Döbrich et al. (2014), who argue that it is not sufficient to merely identify and investigate behavioural biases — rather, measures to prevent these biases from emerging in the first place should be explored in more detail. A few experiments (e.g., Frydman and Rangel (2014) or Fischbacher et al. (2017)) do, however, show that even relatively small interventions can have a significant impact on the reversal or complete elimination of the disposition effect. Considering both the theoretical and practical relevance of the issue, we therefore argue that it is worthwhile investigating additional proactive debiasing strategies (see Soman and Liu (2011)).

In light of these developments, we propose to investigate the influence of investment goals and their presentation format on the disposition effect in an experimental setting. Our motivation stems from the fact that goal theory can provide an unobtrusive and straightforward to implement debiasing mechanism. This is because goals make the investor focus on the overall performance of their investment instead of separating it into several mental accounts (Thaler (1985)) that are evaluated individually. Thereby, they also enhance investors' self-control. Hence, we expect to reduce or even completely eliminate the influence of two of the factors that are argued to cause the disposition effect — mental accounting and missing self-control — and consequently reverse the disposition effect.

We find that providing investors with a specific investment goal that they are primed to achieve in the experiment significantly reduces their disposition effect. In fact, enhanced self-control and the refraining from mental accounting seems to cause these subjects to hold on to paper gains for longer. While their behaviour with regards to loss realization does not change, they exhibit a reversed disposition effect overall. This finding is robust to two specifications used to measure the disposition effect. However, aggregating their portfolio's performance in a single visual graphical representation does not have any significant effect on their subjectivity to the disposition effect. Thereby, we contribute to the current state of research in the following ways: We show that goal theory can provide a simple and unobtrusive way in which the disposition effect can be debiased in a sophisticated experimental setting. Furthermore, we confirm the existence of the disposition effect among MTurk workers in a context where it has not been examined before.

This paper is hence structured as follows: Section 2 discusses the relevant literature on the disposition effect and goal theory. Subsequently, section 3 first describes the basic experimental design. Furthermore, it develops hypotheses that are derived directly from the present body of literature and describes the treatments administered to test said hypotheses. Also, the experimental procedure and the two measures of the disposition effect are explained in detail. Section 4 first reports the sample statistics and then carries out the main analysis. Robustness checks are reported in section 5, before section 6 concludes by providing a summary and presenting avenues for future research.

2 Literature Review

2.1 Disposition Effect

The term disposition effect was first coined by Shefrin and Statman (1985) and defined by the authors as investors' tendency to "sell winners too early and ride losers too long" (p. 777). The authors base their argument of why a disposition effect occurs on positive theory with four main components: (1) prospect theory, (2) mental accounting, (3) pride seeking and regret aversion, and (4) lacking self-control. Subsequently, they show that their theory is consistent with empirical results derived from mutual fund data and individual stock trading data from Schlarbaum et al. (1978). For both asset classes, they show that a disposition effect can clearly be observed.

Following the first analysis by Shefrin and Statman (1985) and responding to their call for more detailed investigations of the phenomenon, Odean (1998) provides the first large-scale empirical analysis of the disposition effect. He records — whenever a transaction is conducted — the number of realized gains (losses) and compares it to the number of paper gains (losses).[1] He finds that in his sample of 10,000 discount brokerage accounts (with transactions between 1987 and 1993) there is significant evidence of the disposition effect. In fact, his results show that investors on average realize 14.8% of paper gains, while merely 9.8% of paper losses are sold. In other words, investors were 50% more likely to sell winning stocks as compared to losing stocks — which was consistent with what Shefrin and Statman (1985) had predicted.

Considering that the dataset employed in the analysis stemmed from a US discount broker reduces the possibility that all investors were influenced by the same pieces of trading advice. Recalling that discount brokerages generally do not advise their clients on transactions, Odean (1998) concludes that it is unlikely that his results were driven by herding behaviour or private information. Furthermore, to investigate whether transactions were influenced by portfolio rebalancing motivations, he excludes partial sales and focuses exclusively on those sales where a stock was completely removed from the portfolio. However, the results remain essentially unchanged, wherefore Odean (1998) dismisses the possibility that the observed pattern is a result of portfolio rebalancing. Lastly, he tests the economic implications of the disposition effect and finds that, over a one-year period, the excess return over the CRSP value-weighted index is 3.4% higher for winners that were sold as

[1] Focusing on the number of realizations relative to sale opportunities makes the results robust to the influences of market developments in specific time periods.

compared to the losses that remained in investors' portfolios. Hence, investors' belief in mean-reversion seems to be unfounded and the disposition effect is clearly hazardous to their performance.

Weber and Camerer (1998) conduct the first laboratory experiment to shine light on the origins of the disposition effect. Subjects can trade 6 distinct stocks over 14 trading periods. Each stock is associated with a certain probability of a price increase that is constant over all trading periods. Thereby, the experiment is designed in a way such that a mean-reversion belief is clearly unfounded and the exhibition of a disposition effect cannot be explained by strictly rational behaviour. Still, the authors find that 59% of sales were attributed to winners, which makes subjects 50% more likely to realize gains as compared to losses, consistent with the disposition effect. However, when shares were automatically sold at the end of each trading period and could subsequently be bought back for the same price, the disposition effect diminished significantly and was almost non-existent.

Based on these three fundamental studies, an array of further research has set out to investigate the disposition effect in thorough detail. As a consequence, the phenomenon has been found to be surprisingly robust and ubiquitous (Barber and Odean (2011), Chang et al. (2016)). For example, Shapira and Venezia (2001) find the disposition effect among Israeli investors when investigating the duration of round trip trades for winning and losing assets. Feng and Seasholes (2005) use Chinese brokerage data from 1,511 accounts. By employing hazard rate models to estimate how long investors typically hold a position, they find that investors realize losses about 30% less quickly as compared to gains. Other geographies where the disposition effect has been found include Finland (Grinblatt and Keloharju (2001)), Australia (Brown et al. (2006)), Taiwan (Barber et al. (2007)), Korea (Choe and Eom (2009)), and Sweden (Calvet et al. (2009)). Thus, it seems evident that the disposition effect is clearly a global phenomenon.

With regards to asset classes, the disposition effect cannot exclusively be found in stock markets. Instead, research has identified the disposition effect in mutual funds (e.g., Frazzini (2006)), futures markets (e.g., Choe and Eom (2009)), and social trading (e.g., Heimer (2016)), in addition to stock markets (e.g., Kliger and Kudryavtsev (2008)). Even non-financial domains, such as the Boston real estate market in the 1960s, seem to exhibit the disposition effect (Genesove and Mayer (2001)).

Moreover, the disposition effect is present across a variety of investor groups (Amarnani (2010)) — from students in experimental settings (e.g., Oehler et al. (2002)) to professional traders (e.g., Shapira and Venezia (2001), Frino et al. (2004), or Locke and Onayev (2005)). While research generally agrees that professional as well as non-professional traders are subject to the disposition effect, there is contradictory evidence concerning the magnitude of the effect. Whereas Chen et al. (2007) report that institutions suffer from a less severe disposition effect as compared to individual investors, Grinblatt and Keloharju (2001) find that the difference across various investor types (i.e., non-financial corporations, finance and insurance institutions, general government, non-profit institutions, and households) is relatively small.

The question thus arises what constitutes the roots of this widespread anomaly. With regards to the foundation of the disposition effect, Chang et al. (2016) note that: "Empirical work has been much more successful in identifying problems with various proposed explanations than in finding positive evidence that points directly to a particular theory to the exclusion of all others" (p. 268). As mentioned previously, Shefrin and Statman (1985) initially base their theory underlying the disposition effect on four components, including prospect theory (Kahneman and Tversky (1979)). They argue that in a prospect theoretic setting, investors who have experienced a paper loss become more risk seeking and hence agree to hold the investment for a longer period of time. On the other hand, paper gains make investors more risk averse and therefore lead them to sell winning investments more quickly. However, the prospect theoretic explanation is not without its critics. Kaustia (2010) notes that, for reasonable prospect theory parameterizations, the propensity to sell an asset should actually decrease for gains and losses alike when the current price diverts from the original purchase price in either direction. This, however, is in contrast to his (and others') empirical findings and therefore leads him to conclude that prospect theory is unlikely to explain the disposition effect. Similarly, Barberis and Xiong (2009) and Hens and Vlcek (2011) argue that investors with prospect theoretic preferences would not choose to invest in risky stocks in the first place.

Secondly, Shefrin and Statman (1985) argue that, following the mental accounting framework (Thaler (1985)), investors tend to attribute different stocks to separate mental accounts. Hence, rather than evaluating their portfolio's performance as a whole, they only consider individual stocks in their financial decision-making. Thaler (1999) further argues that closing a position at a loss is "painful" (p. 189), wherefore people are reluctant to do it.

Thirdly, Shefrin and Statman (1985) assert that investors are hesitant to admit misjudgments. Hence, rather than admitting having made a mistake by closing a losing position, investors keep the investment and hope that it will recover. Concurrently, gains are realized more readily since they add to investors' pride and reinforce their belief of having made a good decision. This is in line with Chang et al. (2016), who propose that the disposition effect is founded in cognitive dissonance (Festinger (1957)).[2] They provide compelling evidence for their theory, showing that the disposition effect is reduced with increasing levels of investment delegation, where investors feel less responsible and hence experience a lesser degree of cognitive dissonance when selling a position at a loss.

Lastly, missing self-control constitutes the fourth component of the framework. Shefrin and Statman (1985) argue that augmented self-control explains the pattern found in empirical results that the disposition effect disappears in December, even though there seems to be no rational explanation for this observation. Rather, they propose that due to its "perceived deadline characteristic" (p. 784), investors become more open towards loss realization in the last month of the year.

A few experiments have thus focused on debiasing the disposition effect by considering these components. In a laboratory experiment, Frydman and Rangel (2014) find that the disposition effect can be substantially reduced by 25% when making a stock's purchase price less salient. A possible explanation for this could be that when purchase prices are omitted and the investment's value has depreciated, investors experience a lesser degree of regret for having made a bad investment. They are thus more willing to divest losing assets, whereby the disposition effect is greatly reduced. Fischbacher et al. (2017) show that when investors have the possibility to employ automatic selling mechanisms in the form of limit orders, they exhibit a significantly lower disposition effect. Their results can be explained by realizing that limit orders serve as an ex-ante self-control mechanism. Another debiasing example can be found in Döbrich et al. (2014). Here, the authors use a simulated stock market to find that the disposition effect can successfully be eliminated using their debiasing intervention. A rational or emotional warning message about the disposition effect is presented before trading decisions can be submitted. It hence seems that simply making investors aware of the disposition effect could be sufficient to eliminate it.

2.2 Goal Theory

We argue, however, that goal theory can provide a less intrusive and more practical debiasing strategy, especially considering that goals exert a significant influence on investors' behaviour (Antonides et al. (2011)). Goal theory traces back to Locke (1968), who posited that confronting people with a goal will make them strive to

[2] Cognitive dissonance describes the social psychology theory proposed by Festinger (1957), who argues that individuals experience substantial discomfort as a result of inconsistent choices or beliefs.

achieve said goal. Since then, it has been found in more than 500 empirical studies that setting specific, challenging goals is associated with better performance than setting unspecific or so-called "do-your-best" goals (Seijts et al. (2011)). The proposed relationship is of linear nature (Latham and Locke (2007)). Even goals that are impossible to achieve do not harm — but rather enhance — performance (Landers et al. (2017)). Also, the effect persists regardless of whether goals are self-set or assigned by others (Locke and Latham (2002)).

Psychologically, goal theory is founded in self-regulation. Latham and Locke (1991) argue that "goal setting facilitates self-regulation in that the goal defines what constitutes an acceptable level of performance" (p. 234). With regards to the disposition effect, we thus expect that exposing investors to an investment goal will enhance their self-control. Specifically, we expect them to be more inclined to realize losses more quickly and hold on to paper gains for longer in order not to miss their investment goal. This should result in a lower disposition effect. Moreover, instead of separating their investments into several mental accounts that are evaluated independently, setting a goal will force investors to deviate from this behaviour and instead take a more holistic approach to their portfolio management. Additionally, since Karlan et al. (2016) find evidence of the effectiveness of goal reminders, we expect that providing investors with a performance graph where their goal is clearly displayed will also help to reduce the disposition effect.

Aspara and Hoffmann (2015) show in a simplified experimental setup that the disposition effect can indeed be reversed if investors are primed towards investment goals. However, in their setup participants do not make active trading decisions but are rather confronted with a particular given investment scenario. Subsequently, merely their intention to sell (vs. hold) the two stocks in the scenario is used to calculate the disposition effect and draw conclusions to test the authors' hypotheses.

We argue that while their results seem promising, they should be regarded with caution. Subjects only provided their intention to sell (vs. hold) the stocks but did not conduct any actual trading decisions themselves. Also, they did not have to acquire the assets in the first place and hence could easily blame negative performance on external factors, wherefore regret aversion and cognitive dissonance might not have had an influence on their behaviour. This is especially troubling considering that prior research has shown that reference prices and cognitive dissonance (see Chang et al. (2016)) are important factors that contribute to the disposition effect. Lastly, the experiment in Aspara and Hoffmann (2015) only considers one trading period and participants are not adequately incentivized to behave in their best interest. As a result, their observations might suffer from limited external validity.

In order to address these issues, we thus propose to investigate the effect of investment goals on the disposition effect in thorough detail using an experimental setup adopted from Weber and Camerer (1998). Before the experimental treatments and specific hypotheses are summarized in section 3.2, we first elaborate on the basic underlying experimental design.

3 Data and Methodology

3.1 Experimental Design

In order to test for the debiasing effect of investment goals on the disposition effect, we build on an established experimental setup and methodology that closely follows Weber and Camerer (1998), who provided the first experimental study of the disposition effect. Since then, their procedure has been adapted by a wide variety of further research undertakings (see for example Weber and Welfens (2007), Döbrich et al. (2014), Kadous et al. (2014), Rau (2015), or Fischbacher et al. (2017)).

Building on this established methodological setup entails several advantages: First, considering that the underlying setup has been tested and employed before allows us to focus on the appropriate administration of treatments to the experiments' participants. Second, a comparison of the disposition effect that is expected to occur in the control condition with previous research enables us to conduct a first robustness check of our experimental procedure and results. Last, the disposition effect we expect to find in the control condition will serve as a baseline against which the debiasing effectiveness of our treatments will be tested.

Within the experimental framework of Weber and Camerer (1998), participants can trade six distinct assets over 14 trading periods. In our setup, these assets are labelled Stock A through Stock F and subjects are told that they will participate in an "experiment about stock market decision-making." The price developments of all six shares follow a distinct two-stage random process. In the first step, it is determined individually whether the price of each share will increase or decrease. Each share is associated with a certain probability of a price increase or decrease and, hence, prices cannot remain constant from one period to the next. The probabilities remain unchanged over the whole duration of the experiment and are designed in a way such that there are clearly favourable, neutral, and unfavourable stocks. The corresponding price change probabilities, which were directly taken from Weber and Camerer (1998), are summarized in Table 1.

Table 1: Probabilities of price increases and decreases for each stock

#	Stock	Probability of price increase	Probability of price decrease
1	B	65%	35%
2	D	55%	45%
3	F	50%	50%
4	A	50%	50%
5	E	45%	55%
6	C	35%	65%

Note: This table summarizes the probabilities of price increases and decreases for all six stocks in the experiment. The stocks were randomly labelled according to the first six characters of the alphabet (A – F) in the experiment to avoid order effects. Hence, Stock 1 listed in the table above does not correspond to Stock A in the experiment.

Subjects are told these probabilities in the instructions preceding the experiment. However, they do not know which probabilities are associated with which stock. Therefore, they will have to observe stock prices carefully in order to infer the more favourable stocks. In a subsequent step, the magnitude of the price change is determined. This process is completely independent from the first random process. Prices can either change by \$1, \$3, or \$5 with equal probability. Again, prices cannot stay constant from one period to the next. Additionally, subjects were told explicitly that their (or other participants') actions would not affect stock prices.

The way in which this experiment has been set up implies that the expected value of a price change for a randomly selected stock will always equal zero. Furthermore, since the probabilities above are communicated to participants, they can identify the most favourable stocks merely by counting the number of price increases. As Barber and Odean (2011) note, the stocks with the greatest number of past price increases are most likely to exhibit the most price increases in subsequent periods. Hence, participants should adopt a trading strategy that merely invests in these stocks for optimal performance. The occurrence of the disposition effect thereby cannot be explained by investors' belief in mean-reversion.

In order to give participants an idea about stock price developments before they can start trading, we also calculated stock prices for periods -3 to -1. Participants can then start trading only from period 0 onwards. In period -3, the initial stock prices are randomly set between \$60 and \$150. While the price determination process is indeed random, prices for all stocks were calculated in advance. This was done to ensure that all participants would make decisions based on the same available information and such that results can be compared across participants without having to consider the potential influence of individual price development paths as a confounding factor. This results in the stock price movements as illustrated in Appendix A.

Subjects are initially endowed with $10,000 of experimental currency, which they can invest over periods 0 to 14. At any time, they can own as many stocks as they want. However, they cannot borrow money to buy more stocks and transaction costs are not considered. Short-selling is also not allowed and subjects are not paid any interest on the amount they choose to hold in cash. At the end of the last trading period, all stocks are automatically sold and converted into cash, following Fischbacher et al. (2017).

3.2 Treatments and Hypotheses

All subjects are confronted with the exact same underlying setup as described above. However, in order to investigate the debiasing effect of investment goals on the disposition effect, three treatments are implemented in addition to a control condition. Screenshots of the respective trading interfaces for each of the four experimental conditions can be found in Appendix B. The control condition will serve as a benchmark against which the effectiveness of the debiasing strategy can be measured. Considering that the control condition in our setup is similar to that in Weber and Camerer (1998), we expect that there will be a positive disposition effect.

Hypothesis 1 (H1): *Subjects in the control condition (i.e., those who do not receive any treatment) will exhibit a positive disposition effect.*

To test the impact of providing subjects with a specific investment goal, a so-called *Goal Treatment* is implemented. Here, subjects are explicitly told that they have to invest their initial endowment of $10,000 such that they accumulate $11,000 by the end of period 14. This is similar to the *Explicit Close Goal Treatment* in Aspara and Hoffmann (2015). Because subjects in this condition will exhibit augmented self-control in order to reach the imposed goal, we expect that they will sell losing stocks more readily and hold winning stocks for longer periods of time, thereby manifesting a reversal of the disposition effect.

Hypothesis 2 (H2): *Subjects in the Goal Treatment (i.e., those who are provided with a specific investment goal) will exhibit a reversed disposition effect.*

Furthermore, by showing subjects in the *Graph Treatment* a line graph that depicts the development of their total assets (hence, their portfolio's aggregate performance) over all 14 periods, we expect that they will refrain from mental accounting practices. Instead of evaluating their performance on a per-stock basis, they will focus on the cumulative performance of their investment decisions. Thereby, they will be more inclined to sell losing assets and hold winning assets longer to improve their overall performance, resulting in a reversal of the disposition effect. This experimental condition also directly addresses the appeal in Döbrich et al. (2014) to include "a graphical illustration of the personal losses accumulated thus far" (p. 9).

Hypothesis 3 (H3): *Subjects in the Graph Treatment (i.e., those who are shown a performance graph) will exhibit a reversed disposition effect.*

Lastly, by combining the elements of the *Goal Treatment* and the *Graph Treatment*, we create the so-called *Goal & Graph Treatment*. Consistent with the previous two hypotheses, we expect that subjects in this condition will exhibit a reversed disposition effect. Explicitly including this treatment in the experimental setup will allow us to investigate the combined effect of goal setting and display format on the disposition effect.

Hypothesis 4 (H4): *Subjects in the Goal & Graph Treatment (i.e., those who are provided with a specific investment goal and are shown a performance graph) will exhibit a reversed disposition effect.*

3.3 Experimental Procedure

The general experimental procedure looks as follows: Subjects who choose to participate in the experiment are first greeted by a welcome screen. Said screen outlines the purpose of the experiment by informing them that they are about to take part in an "experiment about stock market decision-making." Furthermore, the compensation mechanism (as outlined below) is explained to them. By clicking on a "Start Experiment" button, they are randomly allocated to one of the four experimental conditions with equal probability. Hence, we employ a between-subjects experimental design that allows us to draw causal inference and conclusions from the results that follow. The next screen contains detailed instructions.[3] Participants are made aware of the two-stage price determination process as explained above. Furthermore, they are shown a screenshot of the trading interface. This is done such that they can familiarize themselves with the interface and in order to explain the buying and selling mechanism in more detail. Lastly, this page includes a section that explains participants what their goal is going to be. This section is fitted directly to the individual experimental conditions as outlined above.

[3] The full-text instructions can be found in Appendix C.

By clicking on a "Continue" button, subjects are redirected to the trading interface, as shown in Figure 1. The trading interface again includes a reminder of what the goal (depending on participants' experimental condition) comprises. On the top right-hand side, participants can click on a "Next Period" button, in order to receive next period's share prices. They can take as much time as they want to complete all stock sales and purchases before moving on to the next period. Warnings are displayed when participants try to (1) buy shares in periods -3 to -1, (2) sell shares they do not own, and (3) buy shares for which they do not hold the required amount of cash.

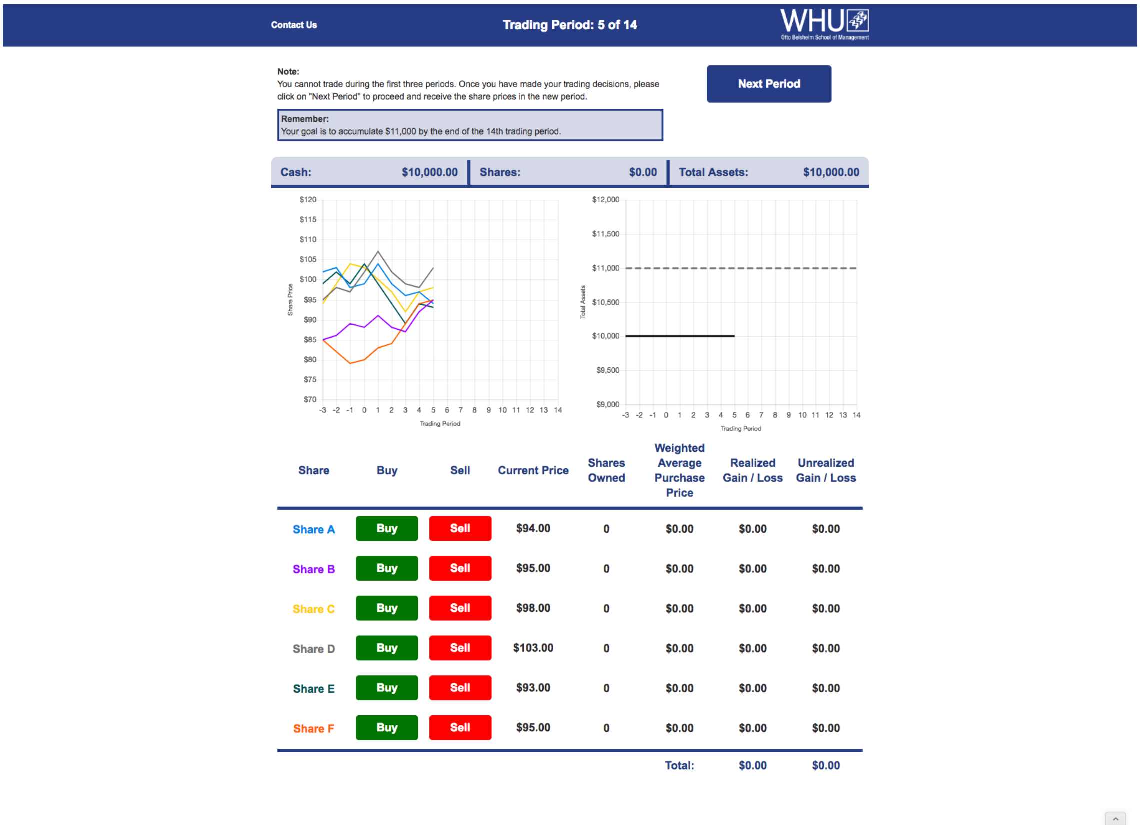


Figure 1: Screenshot of the experiment's trading interface

Note: The screenshot displays the trading interface that subjects in the Goal & Graph Treatment were shown. Therefore, it includes both, a performance graph on the right-hand side and a reference to the investment goal of $11,000 at the top of the screenshot.

After period 14, participants are redirected to the aforementioned questionnaire, which contains questions to measure: investor's expertise (Jordan and Kaas (2002)), financial literacy (Lusardi and Mitchell (2014)), self-regard (Kadous et al. (2014)), perception of regret and rejoice (Rau (2015)), and personal investment relevance (Hüsser and Wirth (2014)). Furthermore, participants' age, gender, and reason for participation are noted. Seeing that these factors, along with trading volume and the total number of trades (Kumar and Lim (2008)), can influence the disposition effect (Lukas et al. (2017)), we will be able to control for them in the data analysis stage. Lastly, we include a manipulation check to test whether subjects were attentive and could correctly remember their experimental stimuli. Subjects who fail said test will be excluded from the further analysis and also will not be paid in exchange for their participation.

3.4 Data Analysis

In the experimental literature, two measures of the disposition effect are commonly found. In line with Odean (1998), we first define the disposition effect as the difference between the proportion of realized gains and losses. At the end of each trading period[4] we calculate the proportion of realized gains (PRG) and the proportion of realized losses (PLR) as follows:

$$PGR = \frac{Gains_{realized}}{Gains_{realized} + Gains_{paper}} \tag{1}$$

$$PLR = \frac{Losses_{realized}}{Losses_{realized} + Losses_{paper}} \tag{2}$$

To determine whether a position counts as a (realized or paper) gain or loss, the current stock price is compared to its historic weighted average purchase price.[5] If the current price is higher (lower) than the weighted average purchase price, the position is counted as a gain (loss). A realized gain/loss is counted each time the investor decides to sell a share, while the remaining opportunities to sell shares are counted either as paper gains or paper losses. The disposition effect (DE) is then defined as:

$$DE = PGR - PLR \tag{3}$$

[4] For the following measures of the disposition effect, we only consider periods 0 to 13. The first three periods (-3 to -1) are excluded because participants cannot conduct transactions but merely observe price developments during this time. The last period is excluded from the analysis as a precautionary mechanism in order to avoid end-round effects (Kadous et al. (2014)), when subjects might trade obsessively to "lock-in" gains before the experiment concludes.

[5] Odean (1998) conducts the same analysis while also considering the highest purchase price, the first purchase price, and the most recent purchase price instead of the weighted average. He does not find any substantial deviations from the primary results. Feng and Seasholes (2005), Rau (2015), and Fischbacher et al. (2017) also report that their results do not depend on the method used to calculate the reference price, i.e., that their findings are robust. Therefore, and because subjects were explicitly shown the weighted average purchase price for each stock on the trading interface, we will only conduct the following analyses using the weighted average purchase price.

It is argued that a disposition effect is present if there is a large difference between PGR and PLR, i.e., if $DE \gg 0$. The disposition effect is measured at the level of the individual investor. At the two extremes, investors with $DE = 1$ solely and immediately realize gains, whereas they never realize losses. The opposite is true for investors who exhibit a disposition effect of $DE = -1$. In line with Weber and Welfens (2007) and Fischbacher et al. (2017), we assume that participants should be equally likely to realize winner and loser stocks. Hence, this implies that the individual-level disposition effect should — on average — amount to zero.

However, this measure of the disposition effect — while it is most frequently used in the literature — can be sensitive towards portfolio size and trading frequency (see for example Rau (2015)). Therefore, we also examine the disposition effect by calculating a so-called disposition coefficient, α, that Weber and Camerer (1998) define as the "difference in sales of winner ans loser stocks by one subject normalized by the total number of sales by that subject" (p. 177). More formally, we specify the disposition coefficient as:

$$\alpha = \frac{S_+ - S_-}{S_+ + S_-} \tag{4}$$

Here, S_+ is the number of sales after a price increase over the previous period and S_- is the number of sales after a price decrease. The disposition effect α will also range between -1 and 1. A value of 0 indicates that there is no disposition effect, while $+1$ (-1) indicates that the subject consistently sold after price increases (decreases).

3.5 Participants and Compensation

Participants are recruited on Amazon's Mechanical Turk ("MTurk") website.[6] In order to be able to do so, the experiment was programmed using HTML, CSS, and JavaScript web technologies and hosted on a web server. The succeeding questionnaire was implemented using the Unipark survey platform.[7] This implies that both parts of the experiment can be accessed remotely by MTurk workers from the United States of America.

The decision to focus exclusively on US MTurk workers was made deliberately because this demographic has been subject to previous research that found that those workers are comparable to the general US population (see for example Berinsky et al. (2012) or Goodman et al. (2013)). Hence, the results presented below will

[6] https://www.mturk.com/
[7] https://www.unipark.com/

prove more easily generalizable than those derived from, for example, university students or convenience samples. In other words, the MTurk sample is expected to exhibit greater external validity. More specifically, Goodman et al. (2013) report that, when compared to the overall US population, MTurk workers show a similar income distribution — albeit with a marginally smaller mean. Additionally, MTurk workers are only slightly younger than the US average. They also seem to exhibit the same decision-making biases — most importantly, risk aversion in the gains domain and risk-seeking in the loss domain — as the general population (Goodman et al. (2013)).

With regards to internal validity, Berinsky et al. (2012) find that MTurk workers are more attentive during experiments than comparable subjects. The authors argue that this is, at least in part, due to the incentive mechanism provided by Amazon on the MTurk platform: By default, when a new "HIT" (human intelligence task, the name for assignments on MTurk) is published, only those MTurk workers who have an approval rating of at least 95% can participate, which is a rather strict requirement. After workers have completed a HIT, the publisher can decide whether or not to accept the individual worker's submission. Acceptance will result in a higher approval rating and the payment of the pre-determined completion fee. Hence, if individual MTurk workers are inattentive to instructions or experimental stimuli, they face the risk of being excluded from future HITs. In summary, we agree with Goodman et al. (2013) who state that they "highly recommend MTurk to behavioural decision-making researchers because of its reliability, low cost, speed of data collection, and heterogeneity of participants" (p. 222).

In order to elicit realistic and truthful behaviour from all participants, their compensation depends on their individual performance in the experiment. Camerer and Hogarth (1999) note that incentives seem to be most effective in judgement and decision-making tasks. Hence, participants are incentivized to perform well and pay close attention to the experiment's instructions and stimuli. All participants receive a flat payoff that amounts to $4.50. Additionally, they can earn 0.25% of the amount they generated in experimental currency.[8] That is, if their total final assets amount to $11,000, they will receive a flat fee of $4.50 plus $2.50 ($\$11,000 - \$10,000 = \$1,000 * 0.25\% = \$2.50$). Hence, they would receive $7 in total.[9]

[8] This payoff scheme is similar to those used in Weber and Camerer (1998), Döbrich et al. (2014), Kadous et al. (2014), or Goulart et al. (2015).

[9] Please note that participants cannot earn less than $4.50 and do not have to pay a penalty for negative performance.

4 Results

Before the actual experiment was conducted to collect data for the successive analysis, several pretests were carried out to ensure that the instructions could not be misunderstood, to eliminate any technical complications, and to collect general feedback and remarks with regards to the research undertaking. These pretests were administered in two environments: The initial pretest took place during a research seminar,[10] where participants could provide verbal feedback to the authors, whereas succeeding tests were carried out directly via MTurk while making use of the exhaustive technical setup. Subsequently, technical obstacles were surmounted, the instructions were altered slightly to safeguard subjects' accurate understanding, and some questions in the questionnaire were altered to avoid any potential misunderstandings. Additionally, participants' performance was used to establish a realistic wealth target that could be set as a predefined goal in the *Goal* and *Goal & Graph* treatments.[11] Lastly, one pretest session was used to make certain that subjects understood the design of the price movements of the respective stocks.

Once pretests and the corresponding alterations of the setup had been completed, data were collected between August 18 and August 25, 2018. As a first step, participants' trading data and the answers they gave to the questionnaire were combined in a single dataset. The following data cleansing and trimming procedures were then applied: We excluded all subjects who did not trade at all during any of the 14 periods as we argue that they did not follow the basic instructions (i.e., to "perform as well as you can" or to "reach the predefined goal"). Furthermore, all subjects who did not pass our simple manipulation check at the end of the experiment were excluded from the further analysis as well.

Only those subjects who remained in our dataset after these two procedures had been applied were compensated for their participation. Furthermore, in case a subject only bought shares but never realized any profits or losses, the denominator of the disposition coefficient becomes zero and, hence, α is undefined (see equation 4). In order to be able to compare the results across disposition measures, we also excluded subjects for which α was undefined from the analysis that focuses on the difference in PGR and PLR. Note, however, that these subjects still received compensation for their participation since a buy-and-hold strategy could be reasonable in our setting if subjects were able to identify the more favourable stocks early on and then stuck with their decisions for the remaining periods.

[10] As part of the Burgenland seminar (Austria, July 11, 2018).

[11] However, it should be noted that, as stated before, even impossible goals should have the desired effect.

4.1 Sample Statistics

Before discussing the results of the disposition effect analysis, we first examine our basic sample composition and several descriptive statistics. Subjects' demographic characteristics are summarized in Table 2. Overall, 160 subjects remained in our dataset after the above-described data cleansing procedures had been applied. These procedures are also one reason as to why the number of participants per condition is not equal but instead ranges from 38 to 43. Furthermore, we did not impose any maximum on the number of participants per condition but rather employed pure random allocation.

This resulted in the fact that while the number of females (83) and males (77) in our sample is relatively balanced overall, there is a significant imbalance in the *Goal & Graph* treatment, with merely 15 females and 23 males. This imbalance is purely due to chance. Additionally, participants' age varied across conditions, with an overall average of 37.9 years. This is significantly older than the average age of student samples, making our sample more representative overall, which reinforces our motivation to use MTurk to acquire participants. In order to counteract the sample imbalances, we chose to run OLS regressions and include gender and age as control variables in a later part of our analysis to see if our results sustain and were, hence, not driven by differences in these demographic characteristics.

Table 2: Demographic characteristics by experimental condition

Condition	**Participants**	**Gender**		**Avg. age**
		Female	Male	
Control	40	22	18	40.0 (13.4)
Goal	43	24	19	39.7 (14.6)
Graph	39	22	17	36.9 (10.4)
Goal & Graph	38	15	23	34.5 (9.6)
Overall	160	83	77	37.9 (12.4)

Note: This table shows the number of participants per experimental condition and overall. Furthermore, it reports the gender composition and participants' average age. Standard deviation are reported in parentheses.

On average, subjects took 7 minutes and 15 seconds to complete the investment task (excluding reading the instructions and answering the questionnaire). Their investment-related characteristics are summarized in Table 3. We used the widely adopted measures from Lusardi and Mitchell (2014) to establish participants' financial literacy. Their three questions focus on the understanding of (i) numeracy/interest rates, (ii) inflation, and (iii) risk diversification, as the authors argue that these three dimensions capture the most important fundamental financial concepts. A person is classified as financially literate if they manage to answer all three questions correctly.

Table 3: Investor characteristics by experimental condition

Condition	**Literacy**	**Relevance**	**Avg. expertise**	**Leisure**
Control	67.5%	47.5%	2.02 (0.85)	27.5%
Goal	60.5%	48.8%	1.92 (0.70)	23.3%
Graph	79.5%	48.7%	2.34 (0.66)	23.1%
Goal & Graph	76.3%	47.4%	2.38 (0.88)	21.1%
Overall	70.6%	48.1%	2.15 (0.79)	23.8%

Note: This table reports investment-related characteristics of all participants per condition. Participants were classified as financially literate if they answered all three financial literacy questions correctly. As for investment relevance and the leisure vs. money motivation, we looked at the two extremes of the respective scales and classified investors according to a dummy variable of 1 if they were mostly motivated by leisure and if they claimed that investments are personally relevant for them. Average expertise measures the average of the four expertise items. Standard deviations are reported in parentheses.

What is particularly conspicuous is that, overall, 70.6% of our participants can be classified as financially literate. Compared to Mitchell and Lusardi (2011), who administer the same set of questions to a representative US sample and find that merely 30.2% of their respondents are financially literate, this is a strikingly high number. Even internationally, the picture persists: While in Romania only 3.8% of respondents are financially literate (Beckmann (2013)),[12] Bucher-Koenen and Lusardi (2011) report a financial literacy rate of 53.2% in Germany. Hence, it seems that our sample is composed of particularly (financially) informed participants.

One reason for this observation might be that our sample suffers from self-selection issues. The task was advertised as an "experiment about stock market decision-making" and consequently might have attracted particularly those MTurk workers who are more financially sophisticated, while repelling those who are not. Self-selection issues are not unique to our experiment, however. Because incentives are linked directly to subjects' performance, it is feasible to assume that experiments will always overproportionally attract those subjects who believe they can perform

[12] It should be noted, however, that Beckmann (2013) uses slightly altered wording to phrase the inflation-related question.

well on the task. In our particular case, this could have resulted in a significantly more sophisticated sample than the general US population.

About half of the experiment's subjects stated that financial investments are also relevant to them in their personal lives. Hence, we believe that our sample consists of subjects that either regard investments as more or less relevant, which is likely to be the case in the general population as well. With regards to their reason for participating in the experiment, merely 23.8% of participants stated that they were more motivated by the fun or leisure aspect of the task. On the other hand, this implies that more than three-quarters were mostly motivated by the monetary reward they expected to achieve. Hence, we are confident that our compensation was set at an attractive level and that subjects did in fact behave rationally and in a manner that would maximize their payoff at the end of the experiment.

Table 4: Trading statistics by experimental condition

Condition	**# purchases**	**# sales**	**# total trades**	**Avg. final wealth**
Control	112.5 (90.0)	58.2 (66.3)	170.6 (152.6)	$10,034 ($328)
Goal	103.7 (99.6)	52.6 (69.8)	156.3 (166.3)	$10,005 ($486)
Graph	112.9 (94.6)	62.6 (78.3)	175.5 (168.6)	$10,045 ($260)
Goal & Graph	137.6 (160.9)	75.3 (144.2)	212.9 (302.3)	$10,070 ($420)
Overall	116.2 (113.7)	61.8 (93.4)	178.0 (203.7)	$10,037 ($383)

Note: The table summarizes participants' investment behaviour. It reports the number of share purchases, sales, and the resulting number of total trades per experimental condition. Also, it shows the average wealth at the end of period 14. Standard deviations are reported in parentheses.

While the previous discussion has mostly focused on exogenously given sample characteristics, we also examined subjects' trading behaviour, which is summarized in Table 4. Firstly, it can be seen that subjects conducted on average 178 transactions (defined as the sum of purchases and sales). Hence, we argue that they were actively involved in the experimental task. Moreover, it can be observed that this number varies significantly across experimental conditions. We see this as compelling evidence that the experimental stimuli induced subjects to exhibit distinct behaviour. In other words, we deem our treatments effective in influencing trading behaviour, which implies that potential variations in the disposition effect can also be linked back to the treatments.

With regards to the average final wealth at the end of the experiment, we were surprised to find that there were no substantial differences across the conditions. On average, subjects ended the experiment with $10,037 in wealth, generating merely

$37. However, there were wide performance variations even within conditions, as can be seen in Figure 2.

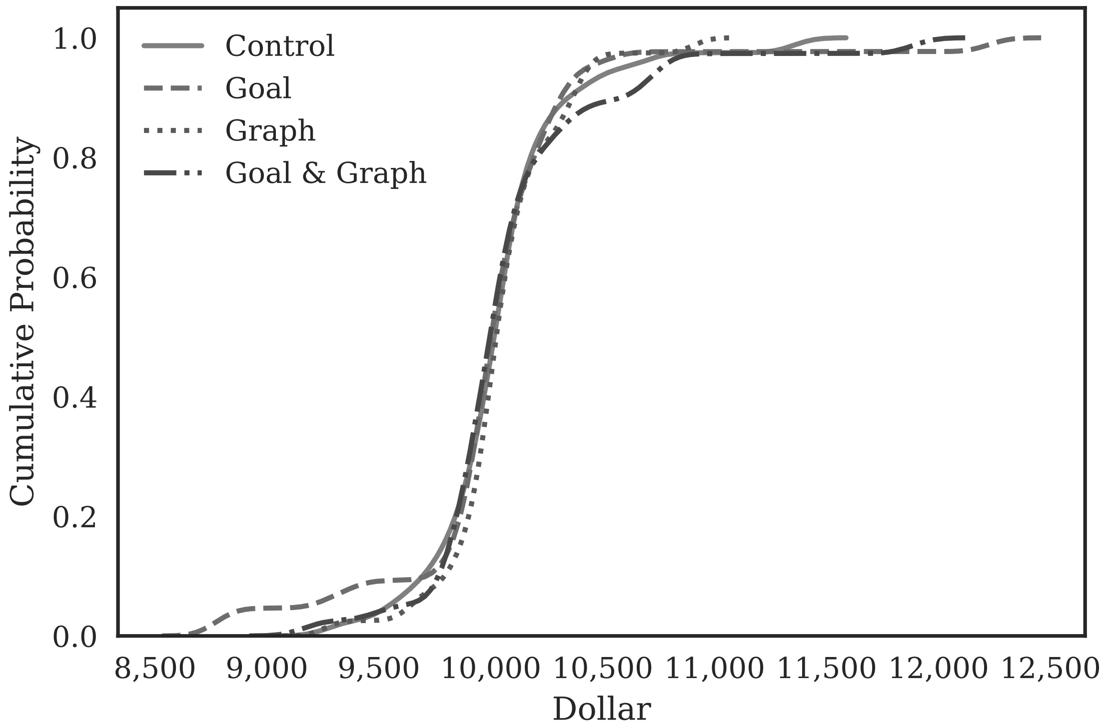


Figure 2: Cumulative distribution of total assets at the end of period 14

Note: This figure shows the cumulative probability distributions of subjects' final wealth at the end of period 14 by experimental condition.

What is particularly striking is that, on average, subjects in the *Goal* and *Goal & Graph* treatments were noticeably far away from the imposed goal of $11,000. Nevertheless, since some subjects did achieve these goals and because our pretests had also shown that it is possible to generate more than $11,000, we are confident that our treatments worked as intended nevertheless.[13]

4.2 Analysis of the Disposition Effect

Now that the sample statistics have been reported and we are confident that the treatments affected subjects' behaviour in the experiment, we will focus on the analysis of individual-level disposition effects. To do so, we will first limit the analysis to the disposition measure (DE) as proposed by Odean (1998), since it is the most widely adopted measure of the diposition effect. As a robustness check, we will, however, also consider the disposition coefficient (α) that was proposed by Weber and

[13] As stated above, even impossible to reach goals should have the desired effect.

Camerer (1998) in section 5. Section 6 will then discuss the aggregate findings from both measures and draw conclusions regarding the previously stated hypotheses.

As a first indicator, we focus on the mean DE by condition, as summarized in Table 5, where we also report $PGRs$ and $PLRs$. In the control condition, we find a positive disposition effect of 0.09. We test the statistical significance of this result by applying a two-tailed t-test and find that the result is significantly different from zero at the 5% level. Economically, our disposition measure is slightly lower than the ones reported of control groups in experiments that use a similar setup: Whereas Rau (2015) does not find a disposition effect in the control group, Döbrich et al. (2014) report a DE of 0.14, Fischbacher et al. (2017) of 0.29, Weber and Welfens (2007) of 0.24, and Goulart et al. (2015) of 0.11.

Table 5: Disposition measures (PGR, PLR, and DE) by experimental condition

Condition	**PGR**	**PLR**	**DE**	**Positive DE**	**Negative DE**
Control	0.19***	0.10***	0.09**	62.5%	32.5%
	(5.60)	(6.12)	(2.51)		
Goal	0.08***	0.11***	-0.03	41.9%	58.1%
	(4.90)	(5.22)	(-0.99)		
Graph	0.11***	0.12***	-0.01	43.6%	51.3%
	(4.81)	(4.36)	(-0.17)		
Goal & Graph	0.14***	0.12***	0.02	52.6%	47.4%
	(4.61)	(3.29)	(0.45)		

Note: This table summarizes subjects' PGR, PLR, and DE, as proposed by Odean (1998). The parentheses report the $t - statistics$ for the null hypothesis that the measures are equal to zero. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$. Also, the percentage of subjects with positive or negative disposition measures are reported. Values might not necessarily add up to 100% due to rounding and since some subjects exhibited a DE measure of exactly zero.

Considering the particularly high financial literacy rates found across our subjects, we argue that it is not countnerintuitive that our disposition measure is lower than the ones found in the studies citied above. Previous research has shown that the disposition effect decreases with investor sophistication (e.g., Feng and Seasholes (2005) or Dhar and Zhu (2006)). While investor sophistication is typically measured in terms of the number of trades executed or the years of investment experience, it seems likely that financial literacy is another proxy of investor sophistication. Hence, seeing that our sample is particularly literate, it is not surprising to find that the disposition effect, while it is still present and significant, is lower than in comparable studies.

Looking at this first result more closely, it can be seen that while subjects in the control condition realized around 19% of their available paper gains, only about 10% of available paper losses were realized. In other words, gains were realized more readily, while losses were held on to for too long, resulting in a positive disposition effect measure. Moreover, there is mentionable heterogeneity in our sample. While 62.5% of participants in the control group did in fact exhibit a positive DE, 32.5% of participants are associated with a negative, reversed DE. This result is in line with prior research. Weber and Welfens (2007), for example, also report that around one-third of their subjects sell losses more often than or equally as often as gains. Furthermore, subjects with a positive disposition effect accumulated lower final assets ($10,022) than subjects with a negative disposition effect ($10,077). Hence, the disposition effect was associated with worse trading performance, which reiterates our motivation that debiasing the disposition effect can have positive welfare implications for investors. Overall, this leads us to conclude that there is a positive and significant disposition effect in the control group which we expected considering the existing literature. Also, the first result reinforces our confidence that the experimental setup was properly understood by participants and that they behaved according to our expectations.

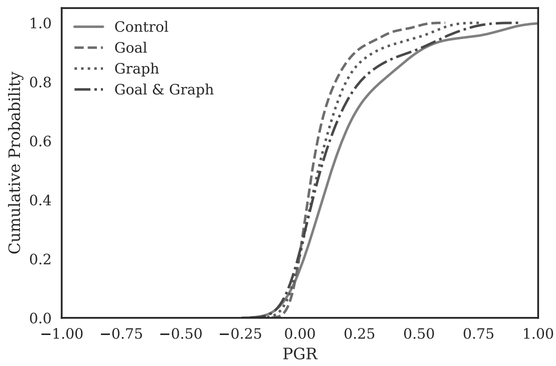
Next, we investigate the DE measures in all three treatment conditions. As can be seen in Table 5, none of the conditions show a disposition measure that is significantly different from zero. Recalling our hypotheses from section 3.2, these results are unexpected since we anticipated to find negative disposition measures (i.e., reversed disposition effects). Consequently, we look at these results in more detail. A mere comparison of $PGRs$ and $PLRs$ of treatments with the control condition indicates that while subjects did not alter their behaviour in terms of loss realization, they significantly reduced their propensity to realize paper gains.

This conjecture is analysed by conducting Kolmogorov-Smirnov (KS) tests on the distributions of PGR, PLR, and DE. The values in parentheses in Table 6 report the $p-values$ for the hypothesis that any given combination of two distributions are the same. It can be concluded from this analysis that none of the PLR distributions are significantly different from the PLR distribution of our control group. Hence, this implies that subjects did not change their loss realization behaviour as a result of any of our administered treatments. On the other hand, the distributions of PGR in the control group and the *Goal* treatment are significantly different from each other ($p = 0.0108$). Overall, this picture persists when looking at the resulting disposition measure. While the distributions of DE in the control group and the *Goal* treatment are significantly different from each other, there is no significant difference when considering the distributions in the *Graph* or *Goal & Graph* treatments. Graphically, these results are also evident from Figure 3, where we plot the cumulative probability distributions of $PGRs$, $PLRs$, and DEs.

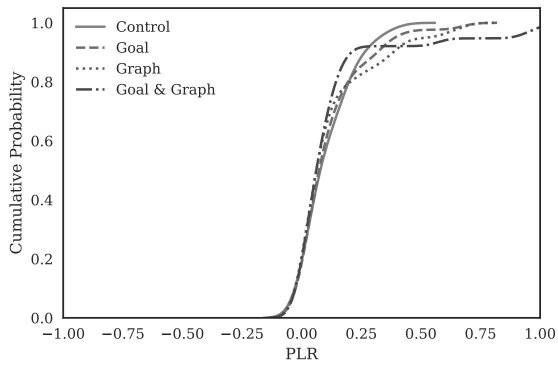
Table 6: KS-statistics for PGR, PLR, and DE by experimental condition

Condition	**Control**	**Goal**	**Graph**
	PGR		
Goal	0.3441		
	(0.0108)		
Graph	0.2154	0.1640	
	(0.2828)	(0.6016)	
Goal & Graph	0.2500	0.1885	0.1107
	(0.1487)	(0.4298)	(0.9630)
	PLR		
Goal	0.1192		
	(0.9127)		
Graph	0.1442	0.179	
	(0.7735)	(0.9615)	
Goal & Graph	0.2197	0.1273	0.1262
	(0.2680)	(0.8771)	(0.8992)
	DE		
Goal	0.2959		
	(0.0420)		
Graph	0.2417	0.2224	
	(0.1709)	(0.2315)	
Goal & Graph	0.2079	0.1603	0.1417
	(0.3301)	(0.6384)	(0.8040)

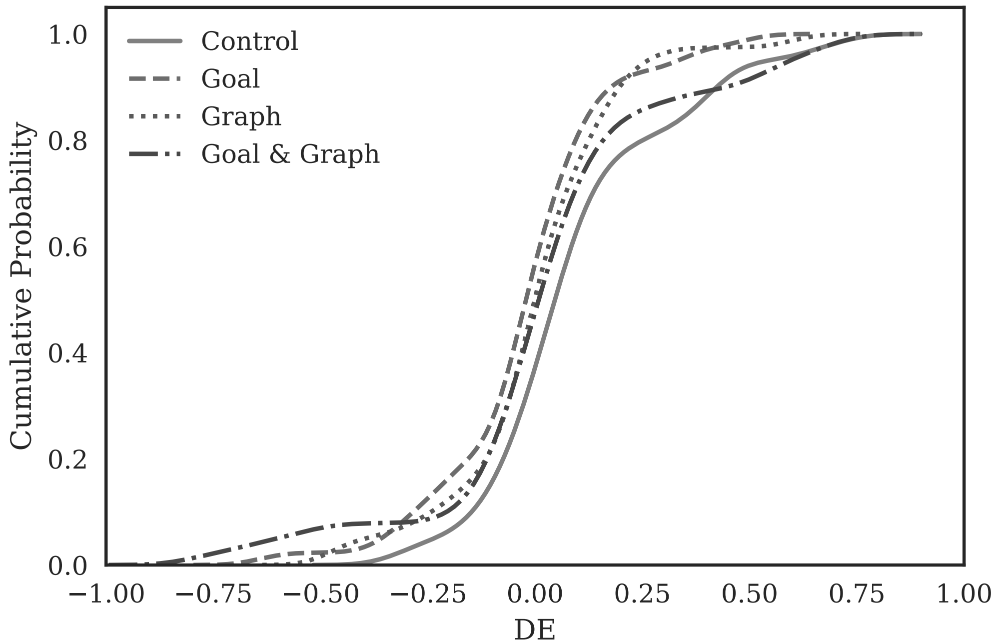
Note: This table shows Kolmogorov-Smirnov test statistics for PGR, PLR, and DE for all possible combinations of experimental conditions. The parentheses report the $p-values$ for the hypothesis that the two distributions are the same.



(a) *PGR*



(b) *PLR*



(c) *DE*

Figure 3: Cumulative distributions of individual (a) $PGRs$, (b) $PLRs$, and (c) DEs

Note: We show the cumulative probability distributions of (a) $PGRs$, (b) $PLRs$, and (c) DEs separately. Each plot includes the distributions for all four experimental conditions.

It thus seems as if at least the *Goal* treatment was in fact effective in reducing the disposition effect. In order to be able to make more robust conclusions in relation to all our hypotheses, we run several ordinary least squares (OLS) regressions on PLR, PGR, and DE. There, we include a constant that represents the control condition and dummy variables for each of the three treatment conditions. This implies that the coefficients of these dummy variables represent the change in DE relative to the control condition. In other words, they represent the magnitude of the debiasing mechanism.

Furthermore, since we have seen substantial imbalances with regards to age and gender across the conditions in section 4.1, we also run regressions where we include age and gender as control variables. Gender was included as a dummy variable equal to 1 for $Females$ and Age is a continuous variable. Age was mean-centred around the grand mean across conditions to make its coefficient more readily interpretable. The resulting regressions are reported in Table 7.

Table 7: OLS regressions on PGR, PLR, and DE

	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)
Variable	**PGR**	**PGR**	**PLR**	**PLR**	**DE**	**DE**
Constant	0.1894***	0.2262***	0.1011***	0.1177***	0.0882**	0.1085**
	(0.034)	(0.038)	(0.017)	(0.022)	(0.035)	(0.042)
Goal	-0.1073***	-0.1064***	0.0105	0.0110	-0.1177**	-0.1174**
	(0.038)	(0.037)	(0.027)	(0.027)	(0.046)	(0.046)
Graph	-0.0762*	-0.0724*	0.0171	0.0205	-0.0933**	-0.0929*
	(0.041)	(0.041)	(0.032)	(0.032)	(0.047)	(0.048)
Goal & Graph	-0.0484	-0.0542	0.0188	0.0189	-0.0672	-0.0731
	(0.046)	(0.046)	(0.040)	(0.038)	(0.059)	(0.059)
Age		0.0009		0.0009		-0.0000
		(0.001)		(0.001)		(0.001)
Female		-0.0702**		-0.0335		-0.0367
		(0.027)		(0.024)		(0.035)
Observations	160	160	160	160	160	160
Adj. R-squared	0.036	0.065	-0.017	0.008	0.019	0.013

Note: This table summarizes various linear ordinary least squares regressions on PGR, PLR, and DE. Age is a continuous, mean-centered variable, whereas $Female$ is binary. Heteroskedasticity-robust standard errors are reported in parentheses. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Focussing on regression 1b confirms our prior statement with regards to subjects' $PGRs$. Even when controlling for age and gender, subjects in the *Goal* condition realize their paper gains significantly less frequently ($p < 0.01$) as compared to the control condition. The same conclusion can be drawn for subjects in the *Graph* treatment ($p < 0.10$), even though the effect is slightly less pronounced, wherefore it is particularly surprising that there is no significant effect with regards to gain realization in the *Goal & Graph* treatment. It is furthermore worth mentioning that women generally realize significantly less paper gains than men ($p < 0.05$).

With regards to $PLRs$, there are no significant differences across the experimental conditions. This conclusion can be drawn from regressions 2a and 2b. In other words, the treatments did not affect subjects' behaviour with regards to their propensity to hold versus realize paper losses and the behaviour seems to be independent from gender and age.

Lastly, regression 3b confirms a positive and significant disposition effect (DE) in the control condition ($p < 0.05$) even when controlling for age and gender. By providing subjects with a specific investment goal, their disposition effect could be significantly reduced ($p < 0.05$) and in fact turned slightly negative. These two observations provide evidence in favour of our first two hypotheses. Showing subjects a graph that depicts their aggregate portfolio performance graphically also has a significant negative effect on the DE measure ($p < 0.10$). Surprisingly, however, the combination of the two debiasing mechanisms (i.e., the *Goal & Graph* treatment) does not significantly affect the DE measure in either direction. The reason for this lies in the fact that subjects did not significantly and substantially reduce their propensity to realize gains. This is a startling result that requires further examination in section 5.

To summarize, we can conclude that we have found a positive disposition effect in the control condition, thereby providing evidence the first hypothesis. Furthermore, the *Goal* and *Graph* treatments taken separately significantly reduced the DE measure, even though we did not find evidence of a reversed disposition effect in both cases. Hence, hypotheses two and three can only partially be supported. Lastly, the combined *Goal & Graph* treatment did not have any significant effect.

5 Robustness Checks

To test whether our results are robust, we use the disposition coefficient (α) as defined by Weber and Camerer (1998) and check if our results sustain in this alternative specification of the disposition effect measure. To recall, the disposition coefficient considers whether subjects sold more shares after price increases or after price decreases. Thereby, it adopts the previous period's share price as the reference price for the disposition effect. Since it only considers sales transactions, it is also less sensitive to portfolio size. Analogously to the previous analysis, we first consider the mean disposition coefficients for each experimental condition. The results of this analysis are summarized in Table 8.

Table 8: Disposition coefficients (Alpha) by experimental condition

Condition	Alpha	Positive Alpha	Negative Alpha
Control	0.15*	60.0%	35.0%
	(1.75)		
Goal	-0.25**	32.6%	65.1%
	(-2.34)		
Graph	-0.08	38.5%	59.0%
	(-0.69)		
Goal & Graph	0.12	55.3%	44.7%
	(1.01)		

Note: This table summarizes subjects' disposition coefficients (Alpha), as proposed by Weber and Camerer (1998). The parentheses report the $t-statistics$ for the null hypothesis that the measures are equal to zero. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$. Also, the percentage of subjects with positive or negative disposition measures are reported. Values might not necessarily add up to 100% due to rounding and since some subjects exhibited an Alpha measure of exactly zero.

Starting with the results of the control condition, it can be seen that subjects on average exhibited a positive disposition coefficient of 0.15 ($p < 0.10$). Again, this result is of slightly smaller magnitude than results of previous experiments: While Rau (2015) reports a disposition coefficient of 0.22, Döbrich et al. (2014) find a coefficient of 0.27 in their control group. Analogously to the previous investigation, we argue that our Alpha is potentially smaller because our subjects are more financially literate and, hence, more sophisticated. Also in line with the previous results, there is significant heterogeneity across participants. Merely 60% of subjects exhibited a positive disposition coefficient, while the measure was negative for 35% of respondents. Furthermore, subjects with a positive disposition coefficient performed significantly worse. Their average final wealth amounted to merely $9,967, indicating that they exhibited negative trading performance. On the other hand, subjects with negative disposition coefficients performed considerably better, ending up with an average of $10,152 in total wealth at the end of period 14.

With regards to the implemented treatment conditions, it seems that the *Goal* treatment led subjects to exhibit a significantly negative disposition coefficient of -0.25 ($p < 0.05$). This result again provides evidence for our second hypothesis. While the Alpha measure is also negative in the *Graph* condition, it is not significantly different from zero and we find a positive, though insignificant, disposition coefficient in the *Goal & Graph* treatment. This is contrary to what we had expected based on our hypotheses.

Kolmogorov-Smirnov test statistics (see Table 9) show that the distributions of disposition coefficients in the *Goal* and in the *Graph* treatment are, in fact, significantly different from their distribution in the control condition. This conclusion cannot be drawn for the *Goal & Graph* treatment, however ($p = 0.42$).

Table 9: KS-statistics for disposition coefficients (α) by experimental condition

Condition	**Control**	**Goal**	**Graph**
Goal	0.40		
	(0.00)		
Graph	0.34	0.18	
	(0.02)	(0.49)	
Goal & Graph	0.19	0.25	0.19
	(0.42)	(0.14)	(0.43)

Note: This table reports Kolmogorov-Smirnov test statistics for individual disposition coefficients (α) for all possible combinations of experimental conditions. The parentheses report the $p-values$ for the hypothesis that the two distributions are the same.

As a last step, we therefore run OLS regressions on disposition coefficients. The specifications here are the same as before and are reported in Table 10. We initially run a regression of all treatments on Alpha (1a), and subsequently include gender and age as control variables (1b) to account for imbalances in our sample composition.[14] Since the results are essentially identical, we focus our discussion on regression 1b.

First, the regression reports a positive and highly significant ($p < 0.01$) constant, which represents the disposition effect in the control group. The constant (0.2802) is now also in the range of the previously reported disposition coefficients of Döbrich et al. (2014) or Rau (2015). The coefficient of the *Goal* treatment is negative and also highly statistically significant ($p < 0.01$). This provides evidence that equipping subjects with a specific investment goal will help reduce their disposition effect. In fact, the regression allows us to conclude that subjects in the *Goal* treatment will, on average, exhibit a reverse disposition effect, which is in line with our second hypothesis.

As for the coefficients representing the *Graph* and *Goal & Graph* treatments, we find that they are not statistically significantly different from zero. This leads us to conclude that the stimuli presented in these conditions do not help reduce or even reverse the disposition effect. It is worth mentioning, however, that females seem to exhibit a significantly ($p < 0.05$) lower disposition effect than males. While the

[14] The control variables are coded in the same way as before.

magnitude of the gender effect seems startling at first, the fact that gender affects the disposition effect is not new and hence our finding is in line with prior research (e.g., Da Costa et al. (2008)).

Table 10: OLS regressions on disposition coefficients (Alphas)

	(1a)	(1b)
Variable	**Alpha**	**Alpha**
Constant	0.1479*	0.2802***
	(0.085)	(0.100)
Goal	-0.4009***	-0.3985***
	(0.137)	(0.134)
Graph	-0.2235	-0.2154
	(0.139)	(0.140)
Goal & Graph	-0.0286	-0.0585
	(0.145)	(0.147)
Age		0.0015
		(0.004)
Female		-0.2458**
		(0.109)
Observations	160	160
Adj. R-squared	0.040	0.059

Note: This table summarizes linear ordinary least squares regressions on disposition coefficients (Alphas). *Age* is a continuous, mean-centered variable, whereas *Female* is binary. Heteroskedasticity-robust standard errors are reported in parentheses. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Lastly, after conducting the experiments and proceeding with the data analysis, we noticed that the share price developments seemed to not always match their exogenously set return characteristics. Consequently, we investigated our suspicion more closely and found that the stock with the lowest probability of showing a price increase (Stock C) in fact exhibited a positive average return of 0.49% per period. This is a result of the fact that, purely due to chance, the stock's price increased 8 times, while it decreased only 9 times over the 17 periods for which prices were made available. In order to check for the gravity of this issue, we rerun the whole analysis while excluding Stock C. Seeing that the results essentially remain the unchanged, we are confident that this unexpected development did not affect participants' behaviour or our results in any significant manner.

6 Conclusion

6.1 Summary and Implications

The aim of this research paper was to investigate whether providing investors with a specific investment goal or displaying their portfolio's performance on an aggregated level (or the combination of theses two mechanisms) could reverse their susceptibility to the disposition effect. Having conducted an in-depth analysis of two measures of the disposition effect in sections 4 and 5, we will now aggregate the findings and relate them to our hypotheses from section 3.2.

In the control condition, we find a positive and significant disposition effect. This result is robust to both measures of the disposition effect (i.e., the disposition measure as proposed by Odean (1998) and the disposition coefficient as proposed by Weber and Camerer (1998)). The magnitude of the effect is, depending on the specification used, slightly lower than the disposition effect in comparable experiments. We suspect that this could be due to the fact that our sample is considerable more financially literate, i.e., more sophisticated. Overall, we find evidence in favour of our first hypothesis, which we summarize as follows:

Result 1 (R1): *Subjects in the control condition (i.e., those who did not receive any treatment) exhibited a positive and significant disposition effect under both specifications.*

Next, we hypothesized that providing subjects with a specific investment goal would reverse their disposition effect because these subjects should exhibit augmented self-control. Our results show that our subjects did report a lower DE measure as compared to the control group. In fact, their DE measure turned slightly negative when controlling for demographic differences in age and gender. This result is due to the fact that while subjects did not alter their behaviour with regards to loss realization, they realized their paper gains less frequently. It thus seems that investment goals cause investors to focus more on the long-term, augmenting their self-control, wherefore they hold on to gains for longer periods of time, foregoing early gain realization. Under the alternative specification, we find that subjects in the *Goal* condition also exhibited a reversed disposition effect, wherefore we overall find compelling evidence in favour of the second hypothesis, in line with Aspara and Hoffmann (2015):

Result 2 (R2): *Subjects in the Goal Treatment (i.e., those who were provided with a specific investment goal) exhibited a significant reversed disposition effect because they realized paper gains less frequently.*

In the *Graph* condition, subjects were provided with a graphical illustration of their overall portfolio performance. This treatment was implemented as a direct response to the call in Döbrich et al. (2014) to display portfolio performance graphically. We expected that this treatment would lead subjects to refrain from mental accounting practices and instead focus on their overall portfolio, leading them to exhibit a reversed disposition effect. The previously discussed regression on DE shows that subjects in this treatment condition did, on average, have a lower disposition effect. However, the resulting DE was still positive. Also, when adopting the alternative specification, we found that the disposition coefficient α was not significantly reduced in the *Graph* treatment. Hence, while there is some evidence for a debiasing effect, we cannot accept the third hypothesis in any case:

Result 3 (R3): *Subjects in the Graph Treatment (i.e., those who were shown a performance graph) did not show a reversed disposition effect.*

Lastly, we also expected subjects in the *Goal & Graph* treatment to exhibit a reversed disposition effect. However, our results show that this is not the case under either specification. The coefficients are statistically insignificant in both cases. By decomposing the DE measure, we find that subjects did not alter their behaviour with regards to gain or loss realization. Hence, this treatment is deemed ineffective and we subsequently reject the fourth hypothesis:

Result 4 (R4): *Subjects in the Goal & Graph Treatment (i.e., those who were provided with a specific investment goal and were shown a performance graph) did not exhibit a reversed disposition effect. Their behaviour with regards to gain and loss realization did not change as compared to the experimental control group.*

6.2 Future Research

While the present research evidently shows that goals can be effective in reversing the disposition effect, it also raises several questions that should be subject to further detailed investigation in potential follow-up work. First, we were surprised to find no substantial differences in subjects' investment performance across the four experimental conditions. Building on prior research alone, we had expected to find that subjects in the *Goal* treatment would perform significantly better than those in the control group. Adding to this, the fact that these subjects exhibited a reversed disposition effect but the same level of performance is another startling fact that we cannot explain with the data available from our experiment. Hence, future research might look at this phenomenon in more detail.

Furthermore, we were surprised to find that the *Goal & Graph* treatment did not have any significant effect on the disposition effect overall. While the *Goal* treatment was effective and the *Graph* treatment did not have any significant effect, we had expected to find that the *Goal & Graph* treatment would also reverse the disposition effect, considering that it is merely a combination of the other two treatments. Again, the data available from the present experiment unfortunately does not allow us to investigate this observation more thoroughly, wherefore an additional, dedicated experiment might be considered as part of future research.

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A Stock Price Developments

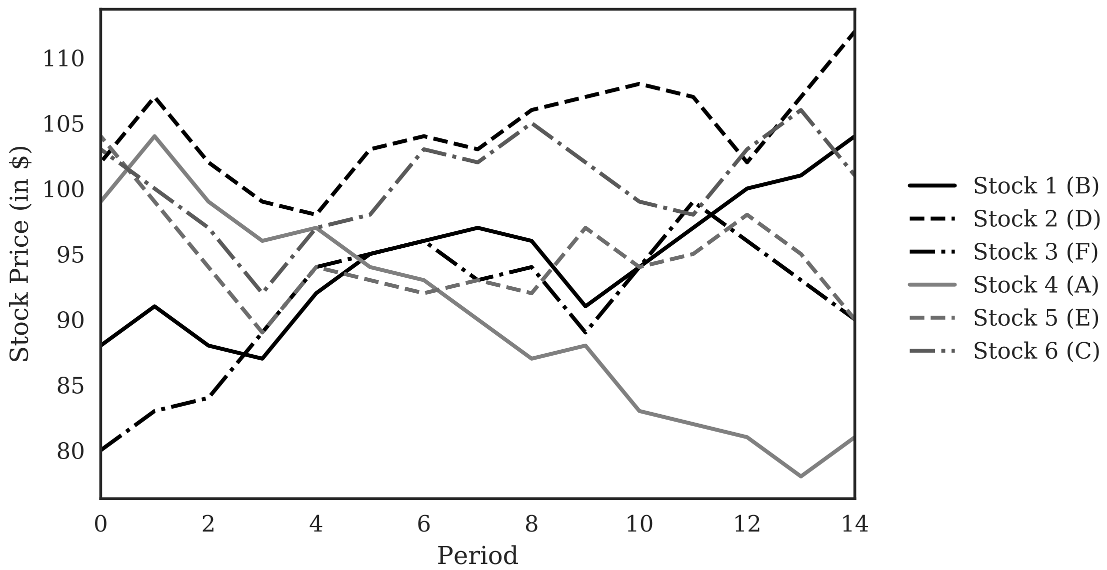


Figure I: Simulated stock price development over time

Note: The graph shows the simulated price development of all six stocks. The prices were determined randomly once before the start of the data collection period according to the process described in section 3.1. Consequently, all participants were shown the same price developments.

B Trading Interface Screenshots

Figure II: Screenshot of all trading interfaces by condition

Note: This figure shows screenshots of the trading interface for all four experimental conditions. Note that, depending on the specific condition, subjects are reminded about their investment goal and are provided with a graph that depicts their aggregate portfolio performance by period.

C Instructions

[Page 1]

Welcome!

About this experiment

Welcome to this experiment about stock market decision-making. If you decide to participate, you will find detailed instructions on the next screen. At the end of the experiment, you will also be asked to answer a few questions. Please answer all questions truthfully — there is no right or wrong answer.

Compensation

Your compensation will depend on the quality of decisions you make during the experiment. You will initially be endowed with $10,000, which you can invest over 14 periods.
At the end of the experiment, your payoff will amount to $4.50 plus 0.25% of your increase in total assets. I.e., if your total final assets amount to $11,000, you will receive a flat fee of $4.50 plus $2.50 ($11,000 - $10,000 = $1,000 * 0.25% = $2.50). Hence, you would receive $7 in total.

Thank you for your participation!

[Page 2]

Instructions

Description

This experiment consists of 14 periods. At the start of the experiment, you will be endowed with $10,000. You will have the possibility to trade six distinct shares (A-F) starting in period 0. In periods -3 to -1, you are asked to observe prices — but you cannot trade any shares yet.

Share price development

The starting prices in period -3 are determined randomly. Prices subsequently change according to the following two processes in each period:

- First, it will be determined whether a stock will exhibit a price in- or decrease. The probabilities of in- or decreases are distinct for each share and do not change over time. You do not know which share (A-F) is associated with which of the following probabilities:

Number of shares on the market	**Probability of price increase**	**Probability of price decrease**
1	65%	35%
1	55%	45%
2	50%	50%
1	45%	55%
1	35%	65%

- Second, the magnitude of the price in- or decrease will be determined. Prices either change by \$1, \$3, or \$5 with the same probability. Again, the magnitude of price changes is completely independent.

In other words, price changes do not depend on your or others' trading decisions. Instead, they follow a random process.

Trading

On the next screen, you will see a trading interface that will look similar to this:

[Trading interface adopted according to experimental condition]

At the top of the page, the current trading period will be displayed. Remember, you cannot trade during the first three periods. The bar below shows how much of your total assets you currently hold in cash and in shares. No interest will be paid on the amount you hold in cash.
The graph entitled "Share prices" displays the historic price development of all shares (A-F). Using your mouse cursor, you can investigate these developments in more detail.

Buying shares

Lastly, you will see a row for each share. If you click on the "buy" button, you will receive one of the respective shares and its current price will be deducted from your cash balance. You cannot buy any shares if you do not have the appropriate cash balance — i.e., borrowing money is not possible in this experiment.

Selling shares

If you click on "sell," you will sell one share at the current price and the proceedings will be added to your cash balance. If the current price is above (below) the weighted average purchase price, the sale will be counted as a realized gain (loss). You cannot sell shares you do not own — i.e., short-selling is not allowed in this experiment.

When you are content with all your trading decisions for the current period, you can click on the "Next period" button to receive new prices. In period 14, the button's label will change to "Continue." If you click on it you will be redirected to a short questionnaire.

Goal

[Control Condition]:

The goal of this experiment is to invest in the shares such that you maximize your wealth over the 14 trading periods. Remember, your compensation will depend on your performance in the experiment as well.

[Goal Condition]:

Please imagine that your goal is to invest in the shares such that you reach your investment goal of at least $11,000 by the end of the 14th period. Remember, your compensation will depend on your performance in the experiment as well.

[Graph Condition]:

The goal of this experiment is to invest in the shares such that you maximize your wealth over the 14 trading periods. Remember, your compensation will depend on your performance in the experiment as well.

[Goal & Graph Condition]:

Please imagine that your goal is to invest in the shares such that you reach your investment goal of at least $11,000 by the end of the 14th period. Remember, your compensation will depend on your performance in the experiment as well.

D Questionnaire

[Part 1]

Otto Beisheim School of Management

Question 1

Please evaluate the following statements:

	1	2	3	4	5
How do you assess your investment knowledge? (1 = I have very small knowledge; 5 = I have a lot of knowledge)	○	○	○	○	○
Please evaluate to what extent you have already gained investment experience (1 = I am very inexperienced; 5 = I am very experienced)	○	○	○	○	○
I know a lot about investing money (1 = I fully disagree; 5 = I fully agree)	○	○	○	○	○
I am very inexperienced in investing money (1 = I fully agree; 5 = I fully disagree)	○	○	○	○	○

Question 2

Suppose you had $100 in a savings account and the interest rate was 2 percent per year. After 5 years, how much do you think you would have in the account if you left the money to grow:

- ○ more than $102
- ○ exactly $102
- ○ do not know
- ○ less than $102
- ○ refuse to answer

Imagine that the interest rate on your savings account was 1 percent per year and inflation was 2 percent per year. After 1 year, would you be able to buy:

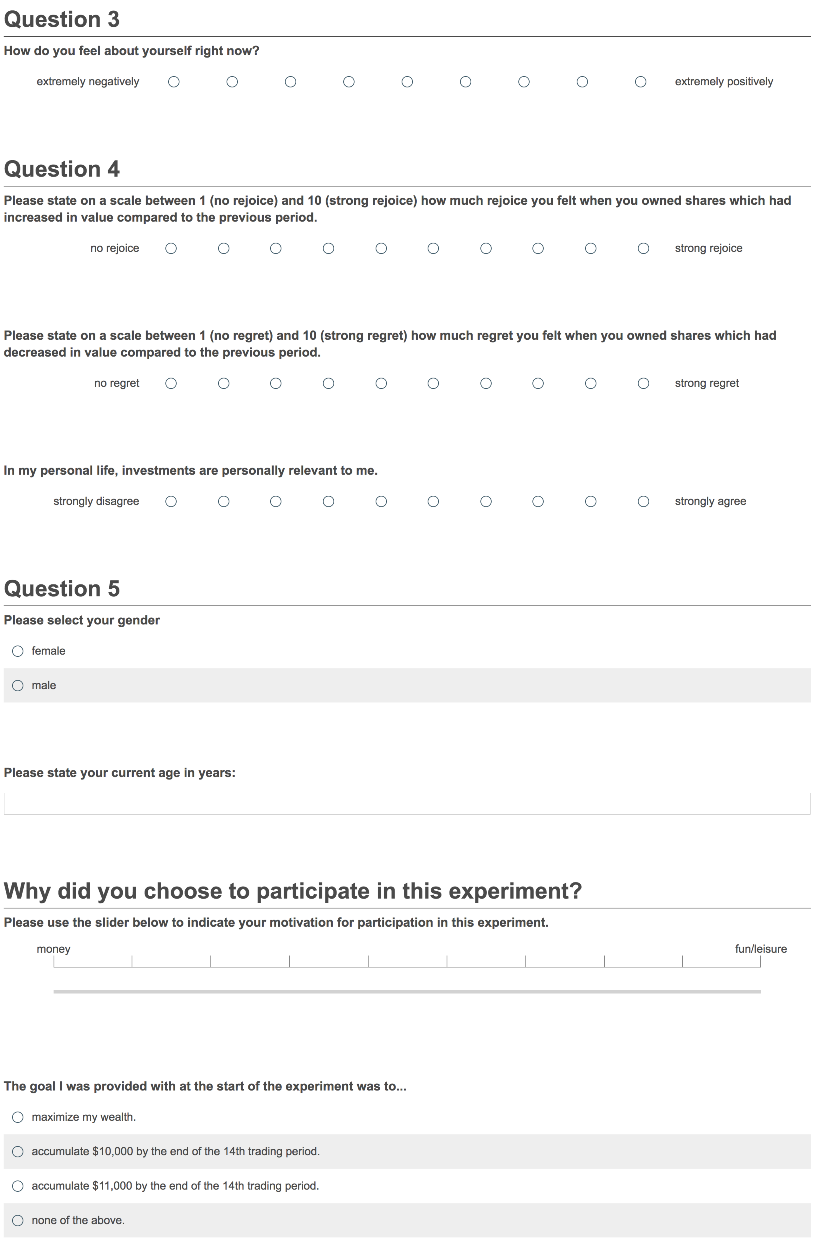
- ○ refuse to answer
- ○ do not know
- ○ exactly the same as today with the money in this account
- ○ less than today with the money in this account
- ○ more than today with the money in this account

Do you think that the following statement is true or false?
"Buying a single company stock usually provides a safer return than a stock mutual fund."

- ○ false
- ○ do not know
- ○ true

Figure III: Screenshot of the questionnaire that succeeded the experiment (Part 1 of 2)

[Part 2]



Question 3

How do you feel about yourself right now?

extremely negatively ○ ○ ○ ○ ○ ○ ○ ○ ○ extremely positively

Question 4

Please state on a scale between 1 (no rejoice) and 10 (strong rejoice) how much rejoice you felt when you owned shares which had increased in value compared to the previous period.

no rejoice ○ ○ ○ ○ ○ ○ ○ ○ ○ ○ strong rejoice

Please state on a scale between 1 (no regret) and 10 (strong regret) how much regret you felt when you owned shares which had decreased in value compared to the previous period.

no regret ○ ○ ○ ○ ○ ○ ○ ○ ○ ○ strong regret

In my personal life, investments are personally relevant to me.

strongly disagree ○ ○ ○ ○ ○ ○ ○ ○ ○ ○ strongly agree

Question 5

Please select your gender

○ female

○ male

Please state your current age in years:

Why did you choose to participate in this experiment?

Please use the slider below to indicate your motivation for participation in this experiment.

money fun/leisure

The goal I was provided with at the start of the experiment was to...

○ maximize my wealth.

○ accumulate $10,000 by the end of the 14th trading period.

○ accumulate $11,000 by the end of the 14th trading period.

○ none of the above.

Figure IV: Screenshot of the questionnaire that succeeded the experiment (Part 2 of 2)