The Causal Effect of Stop-Loss and Take-Gain Orders on the Disposition Effect

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The causal effect of stop-loss and take-gain orders on the disposition effect

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Abstract

The disposition effect, i.e., the tendency to sell winning stocks too early and losing stocks too late is one of the most frequently observed and discussed biases of financial investors. We investigate in a laboratory experiment whether the option of automatic selling devices causally reduces investors’ disposition effect. Our investors can actively buy and sell assets, and, in the treatment group, additionally use stop-loss and take-gain options to automatically sell assets. Investors who had access to the automatic selling devices had significantly smaller disposition effects. The reduction was driven by a significant increase in realized losses. The proportion of winners realized was similar in both treatments. Additionally, our setup provides new evidence on which reference prices investors relate to when choosing limits for automatic sales.

Keywords: disposition effect, stop-loss orders, limit sales, experiment, finance

JEL-Classification: C91, G02, G11

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1 Introduction

The disposition effect, i.e., the tendency to sell winning stocks too early and losing stocks too late, is one of the most frequently observed and discussed biases of financial investors. Since the seminal work by Shefrin and Statman (1985) the disposition effect has been studied in a vast number of theoretical and empirical investigations.\(^1\) While research focused mainly on explanations for the existence of the disposition effect and how the socio-economic background of investors affects its propensity,\(^2\) explicit measures to reduce the disposition effect have received less attention. In particular, so far, no experimental study has investigated whether the possibility to use stop-loss and take-gain orders can causally reduce the disposition effect. The aim of this paper is to fill this research gap.

We investigate whether traders who have access to automatic selling devices use them in a way that allows realizing losses earlier and gains later. Building on the experimental work by Weber and Camerer (1998), who have shown that forced sales of all shares in stocks at the beginning of every period (with the possibility to repurchase them in the same period for the same price) lowers the disposition effect, we create an experimental environment in which participants themselves can choose if and at what price limit to automatically sell their assets.

In order that limit orders appear natural, our experiment mimics the situation of individual investors who cannot always access the stock market. We implement two experimental treatments. In the control treatment, investors can buy and sell goods only in periods in which they can access the stock market. In the limit treatment investors can

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\(^1\) See for example Odean (1998), who was the first to identify the “disposition effect” in the field using data from a U.S. broker and Weber and Camerer (1998), who first identified the effect in a laboratory experiment as well as Ferris et al. (1988), Grinblatt and Keloharju (2001), Shapira and Venezia (2001), Locke and Mann (2005), Kumar (2009), Kaustia (2010a), Seru et al. (2010), Jin and Scherbina (2011) and Ben-David and Hirshleifer (2012), who document the disposition effect in various trading environments.

\(^2\) For instance, Dhar and Zhu (2006) show that wealthier individuals, individuals employed in professional occupations and people who trade frequently tend to have a lower disposition effect. Further, Kumar and Lim (2008) show that traders who execute more clustered trades exhibit lower disposition effects and hold better diversified portfolios.
additionally use automatic selling devices, namely stop-loss and take-gain orders, to realize their losses or gains automatically.\textsuperscript{3} To do so investors specify a lower and upper price limit when purchasing an asset (with the possibility of later adjustments). Because investors are randomly allocated to the two treatments our experiment allows us to identify the causal effect of the possibility to automatically sell losing and winning assets on the disposition effect.

To measure the disposition effect (DE) we build on the method suggested by Odean (1998). He defines the DE as the difference between the proportion of winners realized (PWR) and the proportion of losers realized (PLR). Our results show that investors’ disposition effects are significantly lower in the limit treatment than in the control treatment. Lower disposition effects are caused mainly by an increase in the PLR whereas PWRs in both treatments do not differ significantly. These results are also qualitatively robust to different definitions of reference prices such as highest, lowest, first, last and weighted purchase price.\textsuperscript{4}

Our experiment complements studies that use non-experimental data to investigate how stop-loss and take-gain orders relate to the disposition effect. For instance, Linnainmaa (2010) shows that Finnish investors who use take-gain orders for selling stocks have a higher disposition effect than those using market orders and further, that in particular less sophisticated and uninformed investors use take-gain orders. However, stop-loss orders are not included in the analysis and the data indicates that investors realize only a low proportion of losers. Richards et al. (2011) document a significant negative effect of ordinary stop-loss orders and trailing stop-loss orders on UK investors’ disposition effect. Nolte (2012) analyzes the effects of stop-loss and take-gain orders, using a panel survival approach on data from an internet trading platform for currencies. He finds evidence that the positive disposition effect for trades

\textsuperscript{3} Introducing, both, the stop-loss and take-gain option is relevant because both options are existent in online brokerages. Allowing for both types of limits results also in a rather conservative test for the effectiveness of automatic selling devices, because, in particular, take-gain orders may also increase the disposition effect.

\textsuperscript{4} For reasons of comparability (for instance with Weber and Welfens, 2009) we use the weighted average purchase price as the reference price throughout the paper. However, our findings are qualitatively robust with respect to other reference price specifications.
with larger profits and losses is less pronounced for sales executed by stop-loss or take-gain orders. In addition, he finds an inverse disposition effects for trades with small profits and losses which occurs mainly if stop-loss or take-gain orders are used. In contrast to the studies that use field data, our study allows us to exogenously manipulate whether investors may or may not use stop-loss and take-gain orders. Thus we can avoids problems of self-selection apparent in real world trading data and hence study the causal effect of the possibility to use automatic selling devices individual disposition effects. In addition, we can track revisions of automatic selling orders as well as control the information investors have about each asset and elicit investors risk preferences, overconfidence and socio-economic background in a post-experimental questionnaire.

In addition to studying the causal effect of automatic selling devices on the disposition effect our setup allows us to complement the literature on reference point formation in price developments. Baucells et al. (2011) provide a first investigation on what investors may perceive as the reference point in the context of price developments. The authors confront their participants with 60 different price developments and ask them “at what selling price [they] would […] feel neutral about the sale of the stock, i.e., be neither happy nor unhappy about the sale” (Baucells et al., 2011, p. 508). The authors conclude that investors’ reference price is determined mostly by the first and last price of a time series of prices, whereas the average of intermediate prices has a weaker impact. Our setup allows us to investigate which reference prices explain most of the variation in the choice of lower and upper bounds for automatic selling devices. We find that the lowest, weighted average and first purchase price explain most of the variation of lower limits. For the variation of upper limits highest, intermediate, last and current prices turn out to explain variation best. This indicates that investors refer to different reference prices when deciding ex ante about selling gains and losses. In particular, since variation in lower limits is best explained by the lowest purchase price investors are willing to realize losses ex ante particularly if all units of an asset trade at a loss.
The rest of this paper is organized as follows. In the next section we present the experimental design in detail. Then we explain how we measure the disposition effect in the limit and no-limit treatment. In Section 4 we derive theoretical predictions and benchmarks for investors’ trading behavior. We present the results in section 5 and conclude in section 6.

2 Experimental Design

The general design of our experiment is similar to the stock market treatment in the experiment of Weber and Welfens (2007). Investors receive an initial endowment of 2000 experimental currency units to make portfolio decisions. Investors can trade six different goods labeled 1-6. The game consists of 34 rounds, starting in round -3 and ending in round 30. In rounds -3 to -1 investors cannot trade but observe the price changes for the six goods. In Round 0 all investors can buy goods for the first time. In the following rounds 1-30 it is randomly determined with a probability of 1/3 whether investors may trade goods such that investors may trade in 10 rounds on average. This restriction is introduced to generate an environment in which investors perceive stop-loss and take-gain orders as natural trading devices. It also reflects the fact that individual investors may not always personally monitor their portfolio and cannot actively react to price changes in person at all times. For the rest of the paper we call rounds in which investors can access the market “trading rounds”. In trading rounds investors can purchase and remove goods from their portfolio according to the amount of goods in their inventory and the amount of points left on their account. In non-trading rounds investors cannot actively trade but watch the price development (as long as they want) and then advance to the next round. In round 30 the game ends. All goods remaining in the portfolio are sold automatically and the final earnings in experimental currency units are displayed.

Prices are determined by a random process to ensure that investors’ trading behavior is not affected by strategic market manipulation or any other interdependence between traders. In round -3 the price of each good equals 100 experimental currency units and changes in each of
the following rounds. The price either rises by 6 percent or drops by 5 percent. The six goods exhibit constant probabilities of a price increase (decrease). Investors know the size of the price increase (6 percent) or decrease (5 percent) but are not aware of the exact individual probabilities of a price increase for each good. However, they know that each good has an individual probability of a price increase and that this probability is constant across all periods and independent of trading activities. The exact probabilities of price increases for the six goods in the experiment were 40, 45, 50, 50, 55 and 60 percent.

To find out whether the possibility to use of stop-loss and take-gain orders can reduce the disposition effect we employ a between-subjects design, in which one group of randomly selected investors can use a stop-loss and take-gain order device whereas the control group has no such device. To additionally allow for a within price development comparison, we form pairs of participants across treatments (one investor in the limit treatment and the other in the no-limit treatment) who are confronted with the same randomly determined price paths and can access the market in the same trading rounds.5

In the limit treatment, investors had to define a price below and a price above the current price at which all units of the respective goods would be automatically sold. It was mentioned in the instructions that non-binding limits of a price of 0 and 1000 were possible. The limits had to be chosen explicitly when investors bought a particular stock for the first time or if investors increased the amount of a good in later rounds again from zero to one. Limits could be adjusted in every trading round. If investors only sold a fraction of their holdings of a good or added some units of a good to their existing holdings, there was the possibility but no need to set new price limits. The automatic selling device triggered a sale of all units of a good as soon as one of the price limits was reached, in both, trading and non-trading rounds. If a good was automatically sold, it was possible to repurchase it in the same round if the round was a

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5 Whether the price of an asset increased or decreased was determined randomly by the computer (using the respective probability) at the point in time in which one of the two subjects advanced to the next trading round.
trading round (alternatively, in the next upcoming trading round). Thus, the stop-loss order and take-gain options can also be considered imperfect commitment devices to realize losses or gains.

During the experiment, each participant sat at a randomly assigned and separated PC terminal and received a copy of instructions. A set of control questions as well as a short on-screen trading example ensured the understanding of the game and the trading interface. If any participant repeatedly failed to answer control questions correctly, the experimenter provided an oral explanation. No form of communication was allowed during the experiment. We conducted all sessions in July 2011 at the LakeLab (University of Konstanz, Germany). The data was collected over 2 sessions, with 50 participants in total. In both sessions, treatments were run in parallel, with random allocation of investors to treatment condition. Participants received a show-up fee of 4 euros ($5.68 at that time). The experiment took about one hour and 15 minutes, average income (including the show-up fee) was about 17.65 euros ($25.06). The experiment was programmed and conducted using z-Tree (Fischbacher, 2007). We recruited participants using the online recruiting system ORSEE (Greiner, 2004). Participants were part of the LakeLab subject pool, including undergraduate and graduate students of all fields of study.

3 Measurement of the disposition effect

To calculate the disposition effect we build on the method by Odean (1998) which relates all sales to all possibilities to sell at a gain or a loss. A good is regarded as a winner (loser), if the current price lies above (below) a certain reference price. For reasons of comparability, we use the weighted average purchase price as the reference price throughout the paper. The

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6 A copy of translated instructions can be found in the appendix.
7 We also conducted analyses using the first, last, lowest and highest purchase price as reference prices. All qualitative results are robust.
The proportion of winners realized (PWR) and the proportion of loser realized (PLR) are defined as:

\[
PWR = \frac{\text{sales at a gain}}{\text{possibilities to sell at a gain}}
\]

\[
PLR = \frac{\text{sales at a loss}}{\text{possibilities to sell at a loss}}
\]

The disposition effect (DE) is defined as the difference between PWR and PLR:

\[
DE = PWR - PLR.
\]

The DE measure ranges between -1 and 1. A DE measure of 0 means that winners and losers are realized at the same rate relative to selling opportunities. For participants who generally refrain from selling goods that incur a loss but always realize gains, the DE equals 1, because the PLR equals 0 and the PWR equals 1. For the other extreme, i.e. if subjects show the strongest eagerness to sell goods that incur loss and never sell goods that yield a gain, the DE equals -1, because the PLR equals 1 and the PWR equals 0.

We measure sales and possibilities to sell at a gain or loss as follows. Sales at a gain (or loss) correspond to the number of goods actually sold at a gain or loss, irrespective of whether the good was sold automatically or manually. Possibilities to sell at a gain correspond to the number of goods in an investor’s inventory for which the price has increased above the reference price from one to the next trading round. Possibilities to sell at a loss correspond to the number of goods in an investor’s inventory for which the price has decreased below the reference price from one to the next trading round. We briefly illustrate the measure using two examples: first, assume an investor buys five units of a good in round 1 and can trade again in round 4. Assume further that the price increased according to the reference price in round 2 but then decreased according to the reference price in rounds 3 and 4 such that in round 4 the five units trade at a price below the reference price. Obviously, an investor in the no-limit treatment has zero possibilities to sell at a gain in round 2, round 3 (since round 2 and 3 are no trading
rounds) and round 4 but five possibilities to sell at a loss in round 4. However, an investor in the limit treatment had five possibilities to sell at a gain in round 2 by using the automatic selling device and - in case the investor did not sell the units automatically - also five possibilities to sell at a loss in round 4. Hence if the price of the five units in round 2 did not exceed the investors’ upper selling limit, we record five possibilities to sell at a gain and five possibilities to sell at a loss for the investor in the limit treatment in the second example. Assume now alternatively that the price of the five units has increased monotonously from round 1 to 4 and no automatic sale was caused in the limit treatment. Then, we count five possibilities to sell at a gain in round 4 in the no-limit treatment and ten in the limit treatment, because from the end of round 1 to the end of round 4, an investor in the limit treatment had 5 possibilities to sell at a gain using the automatic selling device during the non-trading periods and 5 possibilities to actively sell it in round 4.\footnote{Note that the possibility to use an automatic selling device naturally increases the number of possibilities to sell compared to the case in which no automatic selling is possible. In turn, the proportion of winners and losers realized may naturally be lower in the limit treatment. Our measurement thus creates a particularly conservative test for whether automatic selling devices can increase the PLR significantly.}

For the total number of selling possibilities at a gain or loss we consider all trading rounds from round 0 to 29. For comparability reasons, we do not evaluate whether an investor had the possibility to sell some goods at gains or losses after the last trading round in the limit treatment.\footnote{Further note that we cannot measure the disposition effect if an investor never has the possibility to sell at a gain (or loss), because the PWR (PLR) is not defined in this case. This exceptional case applies to two individuals in the no-limit treatment, who had no possibility to sell a loser. For comparability, we exclude the PLRs of the two investors who faced the same price developments as the dropouts. Including the two does not change the results.} For the total number of selling possibilities at a gain or loss we consider all trading rounds from round 0 to 29. For comparability reasons, we do not evaluate whether an investor had the possibility to sell some goods at gains or losses after the last trading round in the limit treatment. Further note that we cannot measure the disposition effect if an investor never has the possibility to sell at a gain (or loss), because the PWR (PLR) is not defined in this case. This exceptional case applies to two individuals in the no-limit treatment, who had no possibility to sell a loser. For comparability, we exclude the PLRs of the two investors who faced the same price developments as the dropouts. Including the two does not change the results.

\footnote{This way of measurement guarantees that the number of selling possibilities between two trading periods is not affected by the monotonicity of the price development.}

\footnote{Such cases are rare and do not change our results qualitatively.
4 Theoretical benchmarks

In this section we briefly discuss investors’ optimal strategies and the resulting benchmarks for our experiment. First note that for a risk neutral investor, it is optimal to invest as many points as possible in the market because the market has an upward moving price trend.\textsuperscript{10} If subjects invest points in goods, they will maximize their expected payoffs by investing in the good that has the highest probability of a price increase, i.e. the good(s) with the highest price(s) in the current period. Thus, a simple rule of thumb, namely “always invest all points in the good with the highest price” is optimal for risk neutral investors (given the information they received in the experiment).\textsuperscript{11} Employing this rule of thumb yields on average a negative DE measure of -0.619.\textsuperscript{12} However, risk neutrality and rationality are a strong assumption. Risk averse investors may diversify and thus their portfolio may include several goods, in particular, because investors only know that some of the probabilities of price increases differ but not whether all are different. Thus, we focus on the less “tough” benchmark, in which we assume that investors do not have a bias in their selling decision and are equally likely to realize winners and losers (or do not realize gains and losses at all). This implies that the disposition effect equals on average zero.\textsuperscript{13}

5 Results

We structure the results section in five parts. The first part focuses on the main research question, namely whether the disposition effect is reduced by having the possibility to use stop-

\textsuperscript{10} This is not straightforward for the participants at the beginning of the experiment because subjects do not know the underlying probabilities for the price increase of each good. Nevertheless, subjects know that the probabilities of price increases for each good are constant over time and thus may learn the upward moving trend within the first rounds.

\textsuperscript{11} Note that because subjects do not know any ex ante probability for a price increase of any good, Bayesian updating is not possible.

\textsuperscript{12} The DE measures resulting from the simple trading heuristic can be calculated for 19 price developments and ranges from -1 to 0.314. For 6 price developments the trading heuristic yields no possibility to sell at a loss and hence the PLR and consequently the DE cannot be calculated.

\textsuperscript{13} Both benchmarks have already been proposed by Weber and Welfens (2007), who also consider the second benchmark as more appropriate.
loss and take-gain options. The second section discusses portfolio choices of our investors in more detail. The third section focuses on limit use in the limit treatment. The fourth part discusses which reference prices explain most of the variation in lower and upper limit choices and the fifth part provides regression analyses controlling for risk preferences, overconfidence and socio-economic background variables.

5.1 Automatic selling devices and the disposition effect
Figure 1 shows the cumulative distribution (cdf) of the individual disposition effects measured with the weighted average purchase price as the reference point.\textsuperscript{14} The cdf in the limit treatment is clearly to the left of the cdf for the no limit treatment and almost all DE values are positive in the no-limit treatment. As a first test for the existence of the disposition effect, we relate our data to the theoretical benchmark of random trading which results in an equal distribution of DE measures above and below zero in the two treatments. To do so, we apply a binomial test for both treatments separately. In the no-limit treatment we observe 21 out of 23 individuals with a positive disposition effect and thus a binomial test rejects the null hypothesis at the 1 percent level. However, in the limit treatment we observe 12 out of 25 investors with a disposition effect larger than 0 and thus we cannot reject that the number of DE measures above and below zero is equal. On average, we observe a disposition effect of 0.289 in the no limit treatment and 0.037 in the limit treatment and the distributions of DEs across treatments are significantly different (Kolmogorov-Smirnov test, p=0.002). We summarize this finding in result 1:

**Result 1:** The disposition effect is significantly lower in the limit treatment than in the control treatment.

\textsuperscript{14} Two individuals in the no limit treatment had no possibility to sell a loser. Thus we cannot calculate their PLR nor DE. For comparability, we exclude the disposition effects (.13 and .23) of the two subjects from the limit treatment who faced the same price developments as the dropouts in Figure 1 and the PLRs of these subjects (0.08 and 0.23) in Figure 2a.
Figure 1: Cumulative distributions of individual DEs (n=46)

Figure 2a: Cumulative distributions of individual PLRs (n=46)

Figure 2b: Cumulative distributions of individual PWRs (n=50)
Separating the DE into its two components (PWR and PLR) allows us to disentangle whether the disposition effect is reduced by a higher proportion of realized losses or by a lower proportion of realized gains. Figure 2a shows the cumulative distributions for the PLR and Figure 2b for the PWR. The cdf of the PLR in the limit treatment first order stochastically dominates the cdf in the no-limit treatment and the distributions are significantly different (Kolmogorov-Smirnov test, two-sided, \( p=0.091 \)).\(^{15}\) However, we cannot reject the equality of distributions for the PWR (Kolmogorov-Smirnov test, two-sided, \( p=0.368 \)). We summarize our finding in result 2:

**Result 2:** _The limit treatment increases the proportion of losers realized but does not significantly affect the proportion of winners realized._

Next, we investigate whether the limit treatment yields lower DE values for different price developments. We can do so since in the experiment, we used randomly created price developments faced by one investor in each treatment. Figure 3 shows the DE for pairs of investors across treatments with the same price development. Each point reflects how large the DE was for a specific price development in the two treatments. It becomes clear from the figure that most observations are below or close to the 45° line. A Wilcoxon signed-rank test, one-sided, \( p=0.01 \) for DE rejects the equality of the population mean ranks. We summarize this finding in result 3:

**Result 3:** _The disposition effect is significantly lower in the limit treatment than in the control treatment also if we control for different price developments._

Finally, we investigate whether the treatment is only effective for investors with specific PLR/PWR ratios. To do so we plot the individual PLR and PWR across treatments in Figure 4. The figure shows that there is no type specific reaction to the limit treatment. In both treatments,

\(^{15}\) Note that this test result is statistically significant when using the first, last, lowest and weighted purchase price but fails the ten-percent significance level (\( p=0.181 \)) when using the highest purchase price as the reference price.
the correlation between PLR and PWR is small, insignificant and very similar (No limit treatment: Spearman’s \( \rho=0.140 \), p-value=0.525, Limit treatment: \( \rho=0.178 \), p-value=0.394). This is consistent with previous findings by Weber and Welfens (2007) who also observe no significant relationship between PLR and PWR. In addition the figure again confirms Result 2 which states that the limit treatment in particular increases PLRs.

Figure 3: DE for investors facing same price development (n=46)

Figure 4: Individual PLRs and PWRs across treatments (n=46)
5.2 Investors’ portfolio choices
We present investors’ relative portfolio choice according to price realizations in Figure 5.¹⁶ We rank the goods according to their relative price level from rank 1 (highest price) to rank 6 (lowest price) for each period. If two or more goods have the same price, they both receive the upper rank and the respective rank(s) below are empty (e.g. if two goods have the highest price both receive rank 1 and rank 2 is consequently empty). Note that this may lead to an overrepresentation of some ranks in some periods but does not systematically affect the treatment comparison because for each price development we have one investor in each treatment. We find that the relative quantity of the good with the highest price in the limit treatment is on average (round 0 to 29) 35.9 percent whereas it is only 19.4 percent in the no-limit treatment (signed rank test, p=0.03). The average relative quantity of the good with the lowest price is 12.1 percent in the limit treatment and 29.0 percent in the no-limit treatment (signed rank test, p=0.03). All other relative quantities do not differ significantly across treatments. Interestingly, we do not observe a strong time trend with respect to portfolio choice. Instead, from the beginning on traders in the limit treatment choose the asset with the highest

![Figure 5: Average relative quantities in no limit and limit treatment (n=46)](image)

¹⁶ We decided to use realized prices instead of the fixed probabilities of price increases to rank the six goods since price realizations were actually observed by our subjects whereas probabilities were not. Using probabilities does no change results qualitatively.
price more frequently. In turn, for the same price developments, traders in the limit treatment have significantly more possibilities to sell at gains whereas the number of possibilities to sell at losses across treatments only tends to be larger (signed-rank tests for the matched sample; \( \text{PSG}_{\text{nolimit}}=65.52 \) vs. \( \text{PSG}_{\text{limit}}=107.72, p<0.01 \) and \( \text{PSL}_{\text{nolimit}}=39.96 \) vs. \( \text{PSL}_{\text{limit}}=53.36, p=0.45 \)).

We conclude with result 4:

**Result 4:** The relative quantity of the good with the highest (lowest) price is higher (lower) in the limit than in the no limit treatment.

### 5.3 Limit use

Not all investors in the limit treatment make use of the limit option. Out of 25 investors three chose not to use the upper limit (i.e. they always choose an upper limit of “1000”), four chose not to use the lower limit (i.e. they always choose a lower limit of “0”) and three use no limit at all (i.e. they always choose an upper limit of “1000” and a lower limit of “0”).

Comparing these subgroups with investors from the no limit treatment, disposition effects are on average smaller for the 15 investors who make use of both limits (\( \text{DE}_{\text{both}} = -0.04 \) vs. \( \text{DE}_{\text{nolimit}} = 0.29 \), Kolmogorov-Smirnov-Test, \( p=0.008 \)) as well as for the three investors who only use the lower limit (\( \text{DE}_{\text{onlylower}} = -0.10 \) vs. \( \text{DE}_{\text{nolimit}} = 0.29 \), Kolmogorov-Smirnov-Test, \( p=0.014 \)) whereas the disposition effects of the four investors using only the upper limit tend to be higher than DEs in the no limit treatment (\( \text{DE}_{\text{onlyupper}} = 0.48 \) vs. \( \text{DE}_{\text{nolimit}} = 0.29 \), Kolmogorov-Smirnov-Test, \( p=0.491 \)).

On average, investors use the upper limit slightly less frequently in 57 percent of the cases) than the lower limit in about 60 percent of the cases. Average use of the lower limit is

\[ \text{Note (again), due to the automatic selling devices there are naturally more possibilities to sell (losses and gains) in the limit treatment. However, because investors hold on average more assets with a lower likelihood of a price increase in the no limit treatment, the natural increase in PSL turns out to be less strong in the no limit treatment. Using other reference prices, the differences above are also statistically significant for PSL.}\]

\[ \text{We interpret the choice of a lower limit of “0” as not using the lower limit and a choice of an upper limit of “1000” as not using the upper limit, because we explicitly explained to the participants that this guarantees that limits will not apply.}\]
significantly positively associated with the PLR but only insignificantly positively associated with PWR (Spearman’s ρ = 0.671 with p<0.001 for PLR, ρ = 0.274 with p= 0.185 for PWR and ρ = −0.261 with p= 0.208 for DE). The average use of the upper limit is significantly positively associated with the PWR and DE but insignificantly associated with the PLR (Spearman’s ρ = 0.134 with p= 0.524 for PLR, ρ = 0.423 with p= 0.035 for DE and ρ = 0.762, p< 0.001 for PWR.).

In a next step, we investigate whether investors use lower and upper limits in the same way. Is it that investors set lower limits closer to the purchase price than upper limits? Calculating the average relative distance of the current trading price to the limits of all trading rounds (i.e. all rounds in which investors can set or change a limit) we find for the 15 participants who used both limits that the average relative distance to the upper limit is larger (0.223) than the average relative distance of the current price to the lower limit (0.111) (Wilcoxon signed-rank-test, p=0.055). In turn, automatically realizing gains takes slightly longer than automatically realizing losses.

To investigate whether the possibility to sell assets automatically affects also the size of realized gains or losses we further compare the relative distances of the weighted purchase price to the selling price. For all unforced realizations of gains and losses, i.e. for all gains and losses realized before the last round, the average relative distance between the weighted purchase price and the selling price amounts in the no limit treatment to 0.09 for losses (n=14) and to 0.16 for gains (n=26). In the limit treatment it amounts to 0.11 for losses (n=21) and to 0.13 for gains (n=21). While these differences are not statistically different across treatments (Wilcoxon rank sum test, two-sided, for losses p=0.545, for gains p=0.630 the number of observations in parentheses emphasizes again that the possibility to realize sales automatically leads to more loss realizations. Relative distances for forced sales, i.e. distances calculated for the forced sales

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19 At this point it is also worthwhile to note that limit adjustments are rare (i.e. they occur in less than 3% of possible adjustments).
in the last round, amount to 0.15 for losses (n=18) and 0.42 for gains (n=20) in the no limit
treatment and to 0.16 for losses (n=6) and to 0.31 for gains (n=20) in the limit treatment. Again,
the size of realized losses and gains does not significantly differ across treatments (Wilcoxon
rank sum test, two-sided, for losses p=0.882 and for gains p=0.516). For the investors in the
limit treatment who make at least once use of automatic selling devices, relative distances to
losses (0.109) are significantly smaller than relative distances to gains (0.137, Wilcoxon signed-
rank tests, p-value=0.080) reflecting our finding on limit choices to current prices.\(^{20}\)

Our main result (Result 1) thus reflects two facts. First, investors who are offered the
possibility to sell gains and losses automatically are less reluctant to sell losers. Second,
investors in the limit treatment tend to use the upper limit less frequently than the lower limit
and set lower limits closer to current prices than upper limits. The latter leads to smaller realized
losses than gains for investors who make use automatic selling devices.

### 5.4 Limit choices and reference prices

In this section we shed light on the question about which reference prices investors refer to
when trading observing price developments of several assets. We do so by reporting on how
well the different reference price concepts (first, last, lowest, highest, weighted and intermediate
prices) explain limit choices. Our analysis is similar to the idea of Baucells et al. (2011) who
confronted their study participants with 60 different price paths asking them to state a selling
price that would make them feel neutral about the sale of the stock. In contrast to Baucells et
al. (2011) we do not focus on which price makes participants feel neutral about the sale of the
stock but on their limit choices. We regress the lower limit (Table 1) and upper limit (Table 2)
on the first, last, highest, lowest, weighted average purchase and the mean intermediate price.

\(^{20}\) Comparing relative distances to losses and gains within treatments for all investors who sell at, both, losses and
gains, we find that the relative distances of the weighted purchase price to the selling price tend to be smaller
for losses than gains in both, the no limit and the limit treatment, but do not significantly differ (Wilcoxon
signed-rank tests, p-values > 0.154).
Table 1: OLS Regression, Dep.Var.: Lower limit

<table>
<thead>
<tr>
<th>Dep. Var.</th>
<th>Lower limit for good in portfolio (in trading round)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td>First purchase price</td>
<td>0.865***</td>
</tr>
<tr>
<td>Last purchase price</td>
<td>0.570**</td>
</tr>
<tr>
<td>Highest purchase price</td>
<td></td>
</tr>
<tr>
<td>Lowest purchase price</td>
<td></td>
</tr>
<tr>
<td>Weighted purchase price</td>
<td></td>
</tr>
<tr>
<td>Current price</td>
<td></td>
</tr>
<tr>
<td>Mean intermediate price</td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>1.654</td>
</tr>
<tr>
<td>Observations</td>
<td>249</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.740</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses, clustered on 15 individuals, for which also a mean intermediate price could be calculated, *** p<0.01, ** p<0.05, * p<0.1

The regression models take into account all observations in which investors owned goods and could access the market (i.e. set or adjust their price limits). Because prices are highly correlated we cannot integrate all reference prices into one model. Instead we can run a regression for each reference price and compare the $R^2$ s across models because all models are translated into one parameter. Our results indicate that investors focus on different reference points when deciding on lower and upper limits. Lowest, weighted average and first purchase price explain the variation lower limits best ($R^2=0.756$, $R^2=0.748$, $R^2=0.740$). Mean intermediate price performs slightly worse ($R^2=0.654$) whereas the highest purchase price, the last purchase price and the current price explain clearly less of the variation of lower limit choices ($R^2=0.487$, $R^2=0.473$, $R^2=0.394$). Differences in $R^2$s for the regressions on upper limits are smaller. Current ($R^2=0.387$), last ($R^2=0.364$), mean intermediate ($R^2=0.353$) as well as the highest purchase
price (R^2=0.346) tend to be more predictive than weighted (R^2=0.318), lowest (R^2=0.303), and first purchase prices (R^2=0.287). Our findings thus indicate that investors’ reference points for lower limits differ from those for upper limits. Variation in lower limit choices can be best explained by prices that implicitly reflect the worst that has happened so far. For upper limits instead variation in investors’ choices are best explained by prices that almost guarantee that each unit of the respective good is sold at a gain. Hence, it seems as if investors are ex ante willing to realize losses if losses are “big enough” and gains, if selling prices outperform purchase prices for all units of an asset. Further, the regression that explains most of the variation of lower limits best (model (4)) shows that there is a high correlation between lowest

**Table 2: OLS Regression, Dep.Var.: Upper limit**

<table>
<thead>
<tr>
<th>Dep. Var.</th>
<th>Upper limit for good in portfolio (in trading round)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td>First purchase price</td>
<td>0.957***</td>
</tr>
<tr>
<td>Last purchase price</td>
<td></td>
</tr>
<tr>
<td>Highest purchase price</td>
<td></td>
</tr>
<tr>
<td>Lowest purchase price</td>
<td></td>
</tr>
<tr>
<td>Weighted purchase price</td>
<td></td>
</tr>
<tr>
<td>Current price</td>
<td></td>
</tr>
<tr>
<td>Mean intermediate price</td>
<td></td>
</tr>
<tr>
<td>R-squared</td>
<td>0.287</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses, clustered on 13 individuals, for which also a mean intermediate price could be calculated, *** p<0.01, ** p<0.05, * p<0.1
purchase price and lower limits whereas the constant in the regression is close to zero and insignificant. Instead, the regression explaining the variation in upper limits best (model (6)) models the upper limit as a combination of the purchase price with a fixed markup around 35 price units (the constant term is significantly different from zero). Hence, investors tend to use lower limits to avoid losses – at least for the cheapest units of each good - whereas they set upper limits to such that gains are realized only if gains are sufficiently large.

### 5.5 Individual characteristics and final payoffs

Finally, we provide a robustness check on our treatment effects by reporting results from OLS regressions including controls for individual differences between investors. Control variables were gathered in a questionnaire after the experiment. We control for the following self-stated variables: Investors’ average math grade in final secondary school examinations (Abitur), investors’ gender (using a male dummy), investors’ risk-taking behavior in monetary decisions (ranging from not at all risk seeking to very risk seeking), a dummy for investors’ stock market experience, and a relative measure of overconfidence.\(^{21}\) Model (1a) and (1b) confirm the non-parametric tests by showing that the limit treatment reduces the disposition effect significantly, even if we control for individual background variables (which play a minor role compared to the treatment effect).

In model (2) we study how the limit treatment affects the DE in more detail. Is it that just having the possibility to sell assets automatically reduces the DE or does the treatment effect depend on actual limit use? To answer this question we add the (standardized) individual percentages of lower and upper limit use as explanatory variables. Model (2) shows that just

---

\(^{21}\) The measure is taken from the German socio-economic panel (GSOEP) see Dohmen et al. (2005).

\(^{22}\) The overconfidence measure is a relative rank that ranges from 1 (low overconfidence) to 50 (high overconfidence). Subjects had to answer six questions requiring a numerical answer. They had to do so by giving confidence intervals. For each question, subjects were ranked by how far the true answer fell outside of their confidence interval. The sum of ranks for each question determined the overall rank reported in the regression models.
having the possibility to use automatic selling devices tends to reduce the DE (the limit treatment dummy is negative, and significant on the 10-percent level). However, the investors who use the lower limit frequently have significantly lower DEs than their counterparts. Using the upper limit frequently instead increases the DE significantly.\(^{23}\) Finally, in model (4) we relate the standardized disposition effects to the total number of points earned in the experiment.

The regression results show that on average, investors earn around 3,600 points within the 30

### Table 3: OLS Regression, Dep.Vars.: Individual disposition effects (1a-2), and total points earned (3).

<table>
<thead>
<tr>
<th>Dep. Var.</th>
<th>(1a)</th>
<th>(1b)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Treatment effect</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Limit treatment</td>
<td><strong>-0.252</strong></td>
<td><strong>-0.266</strong></td>
<td><strong>-0.212</strong></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0975)</td>
<td>(0.107)</td>
<td>(0.123)</td>
<td></td>
</tr>
<tr>
<td><strong>Limit use</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Percentage use of upper limit</td>
<td></td>
<td></td>
<td></td>
<td><strong>0.441</strong>*</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>(0.152)</td>
</tr>
<tr>
<td>Percentage use of lower limit</td>
<td></td>
<td></td>
<td><strong>-0.489</strong>*</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.153)</td>
<td></td>
</tr>
<tr>
<td>Disposition Effect (standardized)</td>
<td></td>
<td></td>
<td><strong>-571.41</strong>*</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(191.706)</td>
<td></td>
</tr>
<tr>
<td><strong>Controls</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Math grade</td>
<td><strong>-0.0477</strong></td>
<td><strong>-0.0231</strong></td>
<td></td>
<td><strong>567.936</strong>*</td>
</tr>
<tr>
<td></td>
<td>(0.0452)</td>
<td>(0.0512)</td>
<td></td>
<td>(193.046)</td>
</tr>
<tr>
<td>Male</td>
<td><strong>-0.122</strong></td>
<td><strong>-0.248</strong></td>
<td></td>
<td><strong>693.012</strong></td>
</tr>
<tr>
<td></td>
<td>(0.137)</td>
<td>(0.107)</td>
<td></td>
<td>(489.697)</td>
</tr>
<tr>
<td>Risk taking (money)</td>
<td></td>
<td></td>
<td><strong>-0.0305</strong></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0561)</td>
<td>(0.0535)</td>
<td></td>
<td>(197.279)</td>
</tr>
<tr>
<td>Overconfidence</td>
<td></td>
<td></td>
<td><strong>-0.0468</strong></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0305)</td>
<td>(0.0542)</td>
<td></td>
<td>(232.959)</td>
</tr>
<tr>
<td>Stock market experience</td>
<td></td>
<td></td>
<td><strong>0.134</strong></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.122)</td>
<td>(0.0917)</td>
<td></td>
<td>(477.977)</td>
</tr>
<tr>
<td>Constant</td>
<td><strong>0.289</strong>*</td>
<td>0.124</td>
<td>0.332</td>
<td><strong>3,603.747</strong>*</td>
</tr>
<tr>
<td></td>
<td>(0.0669)</td>
<td>(0.197)</td>
<td>(0.226)</td>
<td>(352.206)</td>
</tr>
</tbody>
</table>

Independent observations

<table>
<thead>
<tr>
<th>R-squared</th>
<th>46</th>
<th>46</th>
<th>46</th>
<th>46</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>0.132</td>
<td>0.207</td>
<td>0.444</td>
<td>0.479</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses, clustered on 23 between treatment pairs with different price developments, *** p<0.01, ** p<0.05, * p<0.1

\(^{23}\) Model (2) indicates also that males tend to have a lower disposition effect if we control for limit use. However, in general, evidence for gender differences is mixed. Shu et al. (2005) as well as Beckmann and Menkhoff (2008) provide results in line with our findings. However, Da Costa Jr et al. (2008) show that women exhibit a smaller disposition effect than men.
periods. An increase by one standard deviation of the individual DE decreases the total amount of points earned by around 571 points. Similarly, an improvement in investors’ math grade by one standard deviation yields around 567 additional points whereas all other controls are statistically insignificant.24

6 Discussion and Conclusion

We investigate the role of stop-loss and take-gain orders for the disposition to sell winning assets too early and losers too late. In a laboratory experiment investors can buy and sell goods. Investors can either only actively sell assets or have the additional possibility to sell assets by using automatic stop-loss and take-gain options. The experimental setup allows us to abstract from several reasons for the disposition effect such as explicit transaction costs, tax reasons, portfolio balancing and beliefs in mean reversion.25 In particular, it excludes the issue of self-selection that is always present in trading data from markets. Thus, our experiment allows identifying the causal effects of automatic selling devices on individual disposition effects. Further, our experimental setup provides new insights on to which reference prices investors refer when deciding about stop-loss and take-gain orders and to understand how stop-loss orders can help investors to reduce their disposition effect.

Our findings show that automatic selling devices can indeed reduce the disposition effect. The possibility to use automatic selling devices reduces the disposition effect significantly and, in particular, the propensity to sell losers is significantly higher when

24 It is worthwhile to note that although the limit treatment significantly reduces the disposition effect it does not significantly increase the total number of points earned in our experiment. The median number of total points for the 23 investors in the no limit treatment is 3569.7 whereas the median for the investors in the limit treatment is 4192.4 (Wilcoxon signed-rank test, p>0.10). However, in our trading environment, it is (by design) likely that significant differences in disposition effects may translate into significant differences in total points earned with a longer trading horizon.

25 Because of the fixed probabilities in price increases rational beliefs in mean reversion are excluded in our setting. Further Weber and Camerer (1998) have already shown in a similar environment that also irrational beliefs in mean reversion are unlikely to explain subjects’ trading behavior. In one of their treatments, they forced subjects to sell all assets at the beginning of every period but explicitly allowed subjects to re-buy the assets. However subjects did not do so, which is not in line with beliefs in mean reversion.
investors can sell automatically. A variety of theories have been proposed to explain the disposition effect. Some of them can also explain why automatic selling devices increase the proportion of losers realized and thereby reduce the disposition effect (or, vice-versa, why the proportion of losers realized is lower if automatic selling is not possible). For instance, self-control problems in combination with Kahneman and Tversky (1979)’s prospect theory or pride seeking and regret minimization can explain why automatic sales can help to reduce the disposition effect. If investors have a self-control problem, they may use the stop-loss order as an imperfect commitment device to decide on the realization of losses ex ante. The limit treatment could also reduce the disposition effect because, in the limit treatment, where assets can be sold automatically, realization utility from active trading decisions is less likely to occur and consequently investors make more deliberate trading decisions. Our findings shed some light on which of these reasons may explain the effectiveness of stop-loss orders. First, the analysis of limit choices indicates that variation in lower limits is best explained by the lowest purchase price. Consequently, our results are in line with the idea that investors use the stop-loss order as an imperfect commitment device (as already hypothesized by Shefrin and Statman (1985). However, our investors implement rather conservative measures for automatic sales at a loss: Lower limits are chosen such that an automatic sale at a loss will be triggered if (almost) all units of the asset trade at a loss. Second, we do not find large differences in the sizes of realized losses (or gains) across both treatments. The latter finding is not perfectly in line with a prospect theory functional form for utility (concave in gains and convex in losses).

26 See also Thaler and Shefrin (1981).
27 For a critical discussion see Barberis and Xiong (2009), Hens and Vleck (2011), Kaustia (2010b), Li and Yang (2011) and Henderson (2012).
28 See also Muermann and Volkman (2006), Fogel and Berry (2006) and Bleichrodt et al. (2010)
29 See Shefrin and Statman (1985); Barberis and Xiong (2009) and (Barberis and Xiong, 2012). The principle idea of realization utility is similar to mental accounting (Thaler, 1985). Both reflects the intuition that “a realized loss is more painful than a paper loss” (Thaler, 1999, p. 189). Realization utility describes the additional (dis)utility derived from an active trade. For experimental evidence in line with the realization hypothesis see Frydman et al. (2012), who provide evidence from an fMRI study that is in line with the idea that people derive utility directly from the act of realizing gains.
30 Remember, revisions of limits were allowed but occurred rarely (in less than 3% of possible adjustments).
Instead, our data seems to reflect a more general reluctance to realize losses which can be reduced by defining limits ex ante.

Usually investors can not only use stop-loss orders but also take-gain orders. This allows investors to also decide ex-ante on the size of gains they want to realize when trading a specific asset. On theoretical grounds the take-gain option may have (at least) two opposing effects for the individual disposition effect. On the one hand, the existence of a take-gain option may be interpreted as a signal that taking gains is a good trading strategy and thus increase the PWR (and consequently the DE). On the other hand, the take-gain option may also make investors think ex ante about a reasonable goal with respect to the selling price of their asset. Consequently, take-gain orders may reduce the impulsive inclination to realize gains immediately. In other words, investors may use the take-gain option as a reminder of what returns they expected when buying the asset and adapt their reference point such that selling at the expected price is not considered a gain anymore. Hence, being offered a take-gain option may on the one hand lead to more sales at a gain but also to less impulsive sales of winners.

Our data do not provide clear guidance with respect to these opposing effects. Neither PWRs nor the size of realized gains differ significantly across treatments and thus the two effects may either set-off each other (or do not exist). Future research may investigate whether there is an impulsive inclination to realize gains that may be reduced by ex-ante committing to a realization of gains.

From a business and policy perspective our results provide nevertheless a clear cut implication. Individual traders do benefit from being offered an automatic selling device that allows them to set lower price limits, at which the asset is sold automatically. Our experiment has shown that online brokerages may benefit not only from offering stop-loss orders to their customers but also from actively reminding traders to use the stop-loss when buying new assets. In turn, investors will have lower disposition effects, avoid severe losses and eventually be more satisfied with their trading platform.
Acknowledgements

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Appendix - Translated Instructions (from German)

We present a full translation of the instructions for the Limit treatment. Instructions for the no limit treatment are identical, except for the limit option, which is missing there. We indicate the respective differences for the no limit treatment by “[]”.

**General information**

Today you take part in an economic decision making experiment. If you read the following instructions carefully, you will be able to earn money. The actual amount you will earn depends on your decisions. Therefore it is important that you read the instructions completely.

For the entire duration of the experiment, it is not allowed to communicate with other participants. If you break the no communication rule we may exclude you from the experiment and payments.

If you have problems understanding the experiment, please have a second look at the instructions. If you still have questions, please give us a hand signal. We will come to your cubicle and answer your questions personally.

Today you receive 4€ for showing up and answering a questionnaire at the end of the experiment. You will receive some additional amount of money, which depends on your decisions in the experiment. In the experiment we do not talk about Euro; we talk about experimental monetary units. The numbers of experimental monetary units you earn in the experiment are converted into Euro with the following exchange rate.

\[
200 \text{ experimental monetary units} = 1€
\]

The following pages will explain the experiment in detail. At the end of the instructions we added some control questions helping you to understand the sequence of events. Please answer these questions. The experiment does not start until all participants solved the control questions and are completely familiar with the course of the experiment.

**The Experiment**

This experiment is about trading goods. You have the possibility to trade six different Goods (Good 1 to Good 6). In total, the experiment consists of 34 periods (Period -3 to 30). In period -3 you receive **2000 experimental currency units**, but no goods. You cannot trade in periods -3 to -1. Instead you observe the price development for the goods. From period 0 on you may buy and sell goods.
Price development
Each of the six goods has a starting price of 100 in period -3. Afterwards the price of each good changes. The price either increases by 6% or decreases by 5% per period. I.e. the price of each good changes in every period.

Each of the six goods has an individual probability of a price increase. This means the price development of each good is independent of the price development of any other good. The probability that the price of a specific good increases is the same for all periods. Nevertheless the price development is independent of the preceding period: In each period the computer randomly allots (with the product specific probability), whether the price of the good will increase or decrease. Neither you nor any other participant know the exact probabilities of price increases (or decreases) for the different goods. Also, your choices do not affect the price development.

Trading goods
You will not have the possibility to trade goods in every period. The probability that you can trade in a specific period is 1/3. This means you can trade on average every third period. In periods in which you cannot trade you can only observe price developments. Whether you may trade in a subsequent period is randomly selected in each period. If you are in a period in which you may trade, you can buy as many units of goods as long as the money on your account exceeds the price for one unit of the respective product.

[If you buy a good for the first time, you have to set two sale-price limits. This means you have to set a price below the current price and a value above the current price, at which the good will automatically be sold. The automatic sale will be executed in all periods (even in non-trading rounds) as soon as one of the two limits has been reached. The good will then be sold at the current price. If you enter “0“ as the lower limit and “1000“ as the upper limit the goods will not be sold automatically. ]

End of the experiment
The experiment ends after period 30. All goods you still hold in period 30 will be sold automatically at the price of period 30 and the experimental currency units on your account will be adjusted respectively.
In the following section we explain the experiment with a test run. Please switch on your monitors now. If you have questions with respect to the test run, give us a hand sign and we will come to your cubicle.

Test run

On the upper part of the screen you see that you are currently in the test run. Next to it you see the current period. As explained before, you start in period -3. Please hit the button “to next period“ until you are in period 0. The screen will then look similar to this:

The screen is separated in two parts. In the upper part, you see the price development of the six goods across the different periods as well as your purchase and selling decisions. The six rows stand for the six goods. In each column you see the past periods and the current period beginning with period -3.

In each cell you see the price of the good in the respective round. Below the price you see the quantity which you have bought (“+“) or sold (“-“). Since you cannot start trading before period 0 you find “---” below the prices for periods -3, -2 and -1. You can also see that the price for each good has changed in every period. The display of the first column of this table will disappear from Period 11 on. You may however retrieve the prices and quantities of the old periods by clicking on arrow buttons which will appear from period 11 on.
In the lower part of the screen you find a table. In the first column of this table you see how many units of which good are in your inventory. Since you have not bought anything, you will see a “0” in each cell. In the second column you see the current prices for the goods, which are also shown in the upper part of the screen. To the right you see two buttons “Good (+1)” and “Good (-1)” and at the bottom you find information on your experimental monetary units on your account. [Far to the right you see a table in which you see the sale-price limits. As long as you have not purchased anything the lower limit is set to “0” and the upper limit is set to “1000”.

Please make now a test purchase for Good 1 by clicking on the “Good (+1)” button. The number of units in your inventory increases by one unit, while your credit on your account (in experimental monetary units) is reduced by the price for one unit. (If you would like to sell some units of goods in the real experiment you can press the “Good (-1)” button, which reduces the units of the respective good in your inventory by one unit and increases the credit on your account (in experimental monetary units) by the price of the respective good.

[If the units of goods in your inventory increase for the first time from 0 to 1, you have to enter two sale-price limits. To do so, an extra field will appear, in which you have to enter two values. One value has to be below the current price and one above. For the test run please set the lower limit to the current price minus 1 and the upper limit to the current price plus 1.] Further, please by one unit of Good 2 [and set the lower limit to “0” and the upper limit to “1000”. After you are down, click the “OK” Button.] Please buy one additional unit of Good 2. [If you would like to change the limits (in the real experiment) you may click on “change limits”.
If you click on “to next period“, on the right, you will proceed to period 1. In the table at the upper part of the screen an additional column will appear including information on period 1. You will see for instance how the prices have changed. [Also, you see that Good 2 was not sold, because the lower limit was set to “0“ and the upper limit was set to “1000”. Good 1 was instead sold because the price of good 1 reached one of the two limits. This is emphasized also by a message in the lower part of the screen saying: “An automatic sale has taken place“.] Also you see that you cannot trade in the second period of the example but only observe the price developments. The “good (+1)” and “good (-1)” buttons are not available in non-trading rounds.

In the real experiment you would then click on “to next period“, in order to start the next period. You could then buy or sell goods in the following periods. After period 30 all goods in your
inventory will be sold at the price in period 30 and the return in experimental monetary units will be added to your credit on your account. The test run instead ends here. Please click now on “to next period”. 

Please solve the following control questions!
Control questions
In the following you find several control questions. Your answers to the control questions will have no influence on the amount of money you earn in this experiment. Please answer all questions. The experiment will not start before all subjects have answered the questions below. If something is unclear to you please raise your hand.

1. Does your trading activity affect the price developments?
   □ Yes
   □ No

2. Are the prices of the six goods independently determined?
   □ Yes
   □ No

3. Assume you buy one unit of good 1, 4 and 6 in period 0 for the price of 100 and you keep the good in your inventory until the end of the experiment.
   a) Does each good have the same probability of a price increase?
      □ Yes
      □ No
   b) If the price of good 3 in Period 4 is 108,2 is it possible that the price of good 4 is 108,2 in period 5?
      □ Yes
      □ No

4. a) By how much do prices increase if they increase?
   □ 6 %
   □ 5 %
   b) By how much do prices decrease if they decrease?
   □ 6 %
   □ 5 %

- Please also fill in the questions at the back of this paper-
5. Assume you buy one unit of good 3 for 95,7 and set the lower limit to 90 and the upper limit to 100.

a) What happens if the price falls to 90,9?
   □ Good 3 will be sold for 90
   □ Good 3 will be sold for 90,9
   □ Good 3 will not be sold
   □ Good 3 will be sold for 95,7
   □ Good 3 will be sold for 100

b) What happens if the price increases to 101,4?
   □ Good 3 will be sold for 90
   □ Good 3 will be sold for 95,7
   □ Good 3 will not be sold
   □ Good 3 will be sold for 100
   □ Good 3 will be sold for 101,4

You have answered all control questions or you still have some questions? – Please give a hand sign, we will come to your cubicle!
References


