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

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Abstract

We study explicit measures to reduce the disposition effect (DE) and thereby provide new insights into the underlying causes of the DE. In a laboratory experiment, we first investigate whether the option of automatic selling devices can causally reduce investors' DEs. Investors can actively buy and sell assets, and in the treatment group additionally use stop-loss and take-gain options to automatically sell assets. We find that investors who have access to the automatic selling devices have significantly smaller DEs; as these investors realize significantly more losses. Second, we introduce a measure that – as automatic selling devices - requires investors to explicitly state price limits but only reminds investors about their selling plan if a limit is hit (instead of automatically selling the assets). Results show that the reminder is not enough; it is the opportunity to ex ante commit to automatically selling at a loss that causally reduces the disposition effect. Hence, we provide evidence for a preference-based explanation of the DE which takes time-inconsistent behavior into account.

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

JEL-Classification: C91, G02, G11

1 Introduction

The disposition effect, i.e. the tendency to sell winning stocks more frequently than losing stocks, 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. A series of empirical studies proved its existence in different trading environments and a variety of theories have been proposed to explain the disposition effect. However, the underlying causes of the disposition effect still remain unclear.[1]

On the one hand, prospect theory may – under certain assumptions - explain the disposition effect (see e.g. Barberis and Xiong, 2009; Henderson, 2012; Hens and Vlcek, 2011; Kaustia, 2010b; for a discussion Li and Yang, 2013). On the other hand, pride seeking and regret minimization (see Bleichrodt et al., 2010; Muermann and Volkman, 2006; O'Curry Fogel and Berry, 2006), models of realization utility (see e.g. Shefrin and Statman, 1985; Barberis and Xiong, 2009; 2012 and Frydman et al., 2013) as well as self-control problems (in combination with prospect theory or realization utility) may explain the existence of the disposition effect. For instance, Barberis (2012, p. 50) suggests that “a naive investor […] may exhibit a ‘disposition effect’ in his trading even though he planned to exhibit the opposite of the disposition effect.” The current study uses an experimental approach that provides novel insights on the underlying reasons for the disposition effect and highlights the important role of time-inconsistent behavior for the disposition effect. Further, our results shed new light on the recent discussion about whether the disposition effect is driven by preferences or beliefs (see e.g. Ben-David and Hirshleifer, 2012) as well as on the idea that the disposition effect is caused by cognitive dissonance (Chang et al., 2016).

We study the causal effects of automatic selling devices and pre-specified selling plans with reminders (but without automatic trade realizations) on investors’ disposition effects. Doing so allows us not only to identify which of these measures can causally reduce the disposition effect but, more

[1] See for example Odean (1998), who uses data from a U.S. broker to identify the “disposition effect” in the field and Weber and Camerer (1998), who first identified the disposition effect in a laboratory experiment, Ferris et al. (1988), Grinblatt and Keloharju (2001), Shapira and Venezia (2001), Locke and Mann (2005), Dhar and Zhu (2006), Kumar and Lim (2008), Kumar (2009), Kaustia (2010a), Seru et al. (2010), Jin and Scherbina (2011), Ben-David and Hirshleifer (2012), Dhar and Zhu (2006).

importantly, to draw conclusions for the theoretical reasons behind the disposition effect. Our experiment builds on the work of Weber and Camerer (1998). Weber and Camerer (1998) have shown that forced sales of all stocks at the beginning of every trading period significantly reduce the disposition effect.[2] In contrast to Weber and Camerer's (1998) work, we create an experimental environment in which investors have the opportunity to decide themselves if and at what price limit their assets are automatically sold. This novel design feature allows us to highlight the important role of time-inconsistent behavior for the disposition effect. In our experiment, automatic selling devices do not allow for a better implementation of certain trading strategies due to better market access.[3] Consequently, rational traders' disposition effects should not differ across treatments that include or exclude automatic selling devices. However, if investors have self-control problems, automatic selling devices may help reduce their disposition effect. Investors with self-control problems may plan to realize losses but are reluctant to do so when losses actually occur. If such investors are aware of their self-control problem (i.e. if they are *sophisticated*) they can commit to realizing losses through automatic selling devices and thereby reduce their disposition effects.

We implement three treatments in our laboratory experiment: a control treatment (from now on *no limit* treatment), a *limit* treatment and a *reminder* treatment. In the *no limit* treatment investors can (only) actively buy and sell assets. In the *limit* treatment investors can additionally use automatic selling devices (stop-loss and take-gain orders). To do so, investors specify a lower and upper price limit when purchasing an asset (with the possibility of later adjustments). As soon as the price hits one of these limits the automatic selling device sells all units of the respective asset. In the *reminder* treatment investors, as in the *limit* treatment, state price limits for which they plan to sell the assets and are informed if one of these limits is hit, but investors still have to decide on whether or not to realize the trade. As investors are randomly allocated to the different treatments, our design allows us

[2] The authors conducted this treatment to show that irrational beliefs in mean reversion are not causing the disposition effect in their trading environment. This conclusion follows from the observation that investors do not re-buy assets that were sold at a loss.

[3] As this could be the case in the real world we also conducted additional treatments in which market access was restricted (also to make the use of automatic selling devices more natural). Results are qualitatively similar and available upon request. We thank an anonymous referee for pointing out the advantages of the environment with unrestricted access.

to study whether automatic selling devices can causally reduce the disposition effect as well as which elements of the automatic selling device (making plans vs. automatic selling) are crucial. In turn, we are able to identify whether committing to realize a loss before the loss occurs is of value to (sophisticated) investors.

Using the measure proposed by Odean (1998), who defines the DE as the difference between the proportion of winners realized (PWR) and the proportion of losers realized (PLR), we find that investors' disposition effects are significantly lower in the *limit* treatment than in the *no limit* and *reminder* treatment. Lower disposition effects in the *limit* treatment result from an increase in the PLRs whereas PWRs do not differ significantly across treatments. These results are qualitatively robust to different definitions of reference prices such as highest, lowest, first, last and weighted purchase price. The *reminder* treatment allows us to identify which elements of the *limit* treatment causally reduce the disposition effect. As the disposition effects in *reminder* are as large as in the *no limit* treatment, the causal reduction of the disposition effect in the *limit* treatments stems from the fact that investors have the opportunity to commit to realizing losses ex ante, before these losses occur; favoring a preference-based explanation of the disposition effect that includes time-inconsistent behavior.

2 Related Literature

Our results provide important insights for the theoretical understanding of the disposition effect and complement a nascent literature studying explicit measures to reduce the disposition effect. Frydman and Rangel (2014) show that subjects' tendency to exhibit a disposition effect is less likely if information about a stock's purchase price is less salient. Their findings are in line with models of realization utility (Barberis and Xiong, 2012; Ingersoll and Jin, 2013; Shefrin and Statman, 1985) in which investors receive a utility burst when selling a risky asset and the size of the burst depends on the gain or loss relative to the purchase price. In turn, making purchase price information less salient reduces the size of the disposition effect (by 25% in their experiments). In contrast to Frydman and Rangel's (2014) findings, models of realization utility alone cannot fully explain our results. At first sight, the limit treatment could (similar to lowering the salience of purchase prices) reduce the psychological (dis)utility of an active trading decision (since in our setting investors can decide on the sale before prices become known). However, a rational time-consistent individual will anticipate the realization utility loss caused by the automated sale and therefore will be reluctant to commit to it. Further, many realization utility models assume that (realization) utility boosts only occur if the investor does not immediately repurchase the asset. In our trading environment investors who experience an automatic sale of a loss could avoid the negative utility boost by repurchasing the asset for the same price, but they do not do so. In a similar vein as realization utility, "mental accounting" reflects the intuition that "a realized loss is more painful than a paper loss" (Thaler, 1999, p. 189). However, mental accounting cannot explain our treatment effects without taking time-inconsistent behavior (explicitly or implicitly) into account.

Further, treatment differences may arise due to the fact that active trading after a price change may refer to an immediate reaction to an event (i.e. to affectively hot decision making, see Loewenstein, 2005) whereas choosing limits that eventually cause an automatic sale may refer to decision making in a "cold" phase. This could lead to more deliberative choices when using automatic selling devices.[4] However, if lower affect was the only reason for the reduction of the disposition effect, we should expect the *reminder* treatment to reduce the disposition effect (as investors can remind themselves about their deliberative choice). In a similar way, the *reminder* treatment may help investors to avoid cognitive dissonance. If investors are reluctant to sell losses because doing so means acknowledging personal purchase failures (see e.g. Chang et al., 2016) being reminded about the ex-ante plan to sell an asset at that loss should offset the cognitive dissonance. As the *reminder* treatment does not significantly reduce the disposition effect (but the automatic selling device does) we conclude that neither cognitive dissonance nor affect can explain our results.

Concerning the broader question of whether a preference-based or belief-based explanation for the disposition effect is favorable, we provide at least two pieces of evidence in favor of a

[4] In recent work, Ploner (2014) studies differences in behavior of investors in a simple risk task using with a focus on the hot-cold approach. Comparing conditional selling decisions in the strategy method (cold approach) with selling decisions after experiencing a loss (in the hot protocol) he finds that people are more likely to hold onto the risky asset in the hot approach (see also Imas, (forthcoming) for a comprehensive discussion of such realization effects).

preference-based explanation. First, it is hard to see how the ability to pre-commit to selling at a specific price in the future should affect beliefs. Second, using a belief-based explanation, it is difficult to explain the fact that the *limit* treatment increases the proportion of losses realized, but the proportion of gains realized remains unchanged (unless there is an exogenous asymmetry in the belief updating process, see e.g. Kuhnen, 2015 and the discussion in Ben-David and Hirshleifer, 2012, pp. 2517-2518). However, the observed asymmetry is in line with a preference-based explanation that includes time-inconsistent behavior.[5]

Apart from providing theoretical insights into the underlying reasons of the disposition effect we provide causal evidence on the effectiveness of automatic selling devices to reduce investors' disposition effects. Thereby, we complement studies that use non-experimental data to investigate how stop-loss and take-gain orders relate to the disposition effect. For instance, Richards et al. (forthcoming) document a significantly 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 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 effect for trades with small profits and losses which occurs mainly if stop-loss or take-gain orders are used. Further, 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 that in particular less sophisticated and uninformed investors use these take-gain orders. However, he does not include stop-loss orders in his analysis and his data indicates that investors realize only a low proportion of losers.

3 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

[5] We thank an anonymous referee for pointing this out.

units to make portfolio decisions. Investors can trade six different assets 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 assets. In Round 0 all investors can buy assets for the first time. In the following rounds 1-30 investors may buy and sell assets in all rounds 0-30, which guarantees that investors can implement exactly the same trading strategies in the *limit,* the *no limit* and the *reminder* treatment. For the rest of the paper we call rounds in which investors can access the market (0 to 30) "*trading rounds*". In trading rounds investors can purchase and sell assets from their portfolio according to the quantity of assets in their inventory and the number of points left on their account. In non-trading rounds (-3 to -1) 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 assets remaining in the portfolio are sold automatically and the final earnings in experimental currency units are displayed.

A random process determines prices 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 asset 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 assets exhibit constant probabilities of a price increase (decrease). Investors know the size of the price increase (6 percent) or decrease (5 percent) but not the probabilities for a price increase. However, investors know that each asset has an individual probability for a price increase and that this probability is constant across all rounds and independent of their trading activities. The exact probabilities of price increases for the six assets in the experiment were 40, 45, 50, 50, 55 and 60 percent.

To find out whether the possibility to use 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 orders whereas the control group has no such device. To 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.[6]

In the *limit* treatments, investors had to decide on a price limit below and a price limit above the current price. Whenever the price reached or exceeded these prices, the automatic selling device triggered a sale of all units of the respective asset. Investors had to choose limits when they bought a particular stock for the first time or they increased the units of an asset in later rounds again from zero to a positive number. Non-binding limits of a price of 0 and 1000 were possible and investors could adjust limits in every trading round. If investors only sold a fraction of their holdings of an asset or added some units of an asset 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 an asset as soon as one of the price limits was reached and sold the assets at the realized price. The automatic selling device informed investors if a price limit was hit and an asset was automatically sold, and it was possible to repurchase the asset in the same round at the same round's price (with no bid-ask spread).

To isolate which elements of the automatic selling device causally reduce the disposition effect and thereby shed more light on the underlying theoretical reasons for the disposition effect, we conducted an additional treatment which we call *reminder*. In this treatment, investors faced the same decision making frame as investors in the *limit* treatment. Investors in *reminder* had to define a price below and a price above the current price, at which they plan to sell an asset. If investors wished not to state such a target, they could also enter a target price of 0 and 1000. The choice and revision options of the price limits were the same as in the limit treatment. If a price limit was hit, the investor was informed (as in the *limit* treatment). In contrast to the *limit* treatment, no automatic selling device triggered a sale. Instead the investor herself had to decide whether or not to sell (or buy) assets actively in the given trading round.

During the experiment, each participant sat at a randomly assigned and separated PC terminal and received a copy of instructions.[7] A set of control questions as well as a short on-screen trading

[6] 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. For the treatment *reminder*, which is described below, we used the same random prices as in the *limit* and *no limit* treatments.

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 the experimental sessions in February and April 2015 at the laboratory of the University of Konstanz, Germany (Lakelab). We collected the data over 3 sessions, with 72 participants in total. We ran the treatments *limit* and *no limit* in parallel within a session, with random allocation of investors to the treatment condition. The treatment *reminder* was run in a single session using the random price developments from the earlier sessions. Participants received a show-up fee of 4 euros which was added to the earnings from the trading experiment (where 200 points corresponded to 1 Euro). The experiment took about one hour and 15 minutes, average income (including the show-up fee) was about 17 Euros. We programmed and conducted the experiment using z-Tree (Fischbacher, 2007). We recruited participants using the online recruiting system ORSEE (Greiner, 2004). Participants were part of the local subject pool, including undergraduate and graduate students of all fields of study.

4 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. An asset is regarded as a winner (loser), if the current price lies above (below) a certain reference price. For reasons of comparability (for instance with Weber and Welfens, 2009), we use the (quantity) weighted average purchase price as the reference price throughout the paper.[8] The proportion of winners realized (PWR) and the proportion of losers realized (PLR) are defined as:

$$\text{PWR} = \frac{\textit{units sold at a gain}}{\textit{possible number of units to sell at a gain}}$$

$$\text{PLR} = \frac{\textit{units sold at a loss}}{\textit{possible number of units to sell at a loss}}$$

The disposition effect (DE) is defined as the difference between PWR and PLR (DE = PWR-PLR).

[7] A copy of translated instructions can be found in the appendix.

[8] We also conducted analyses using the first, last, lowest and highest purchase price as reference prices. Qualitative results are robust if not reported otherwise.

The DE measure ranges between -1 and 1. A DE measure of 0 means that the investor realizes winners and losers at the same rate relative to selling opportunities. For investors who generally refrain from selling assets 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 investors show the strongest eagerness to sell assets that incur a loss and never sell assets 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 assets actually sold at a gain or loss, irrespective of whether the asset was sold automatically or manually. Possibilities to sell at a gain (loss) correspond to the units of assets an investor can sell, for which current price is higher (lower) than the reference price. Investors can trade assets in rounds 0 to 29 (in round 30 all remaining assets are sold and the game ends). Thus, the possibilities to sell at a gain (loss) correspond to the units of assets an investor owns in each round (1 to 29) for which the current price is higher (lower) than the reference price. We briefly illustrate this measure with an example. Let us assume an investor buys five units of an asset in round 1 and sells them in round 5. Let us further assume that the price decreased according to the reference price in round 2 but then increased according to the reference price in rounds 3, 4 and 5 such that in rounds 3, 4 and 5 the five units trade at a price above the reference price. In all treatments, we will then count 5 possibilities to sell at a loss in round 2 and 5 possibilities to sell at a gain in each of rounds 3, 4 and 5, which gives a total of 5 possibilities to sell at a loss and 15 possibilities to sell at a gain.

Finally, 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 one individual in the *no limit* and one in the *limit* treatment who both had no possibility to sell at a loss.

5 Theoretical and empirical benchmarks

Let us briefly discuss investors' optimal trading strategies and the resulting theoretical benchmarks for our experiment. Subjects know that the probabilities of price increases for each asset are constant over time but they do not know the actual probabilities. Assume a rational and risk neutral subject that is willing to invest and has the same priors for the probabilities of a price increase for each asset. For such a subject it is optimal to invest in the asset with the highest price increase since the start. Because all assets start at the same price, the asset with the highest price increase is the asset with the highest current price. Thus, a rational and risk neutral subject with identical priors will either not invest or invest all her points in the asset(s) with the highest price. Thus, a simple rule of thumb, namely "always invest all points in the asset with the highest price or do not invest" is optimal for risk neutral investors (given the information they received in the experiment). Employing this rule of thumb yields on average a negative DE measure.[9]

Risk neutrality and rationality are strong assumptions. Risk-averse investors may diversify and thus their portfolio may include several assets. Note however, for risk-averse investors who are willing to invest in the market it is optimal to diversify among the assets with the highest prices, as these assets have the highest probability of future price increases.[10] Also this trading strategy yields on average non-positive values for the disposition effect, as it results in holding on to those assets which are more likely to increase in the price. A third theoretical benchmark is random trading behavior which would result in individual disposition effects being symmetrically distributed around zero. While these benchmarks will help to interpret the obtained values of DEs in general, our main interest lies on the comparison of DEs across experimental treatments which should not differ significantly if investors are trading rationally (or randomly). Consequently we will focus on the empirical benchmark, i.e. the distribution of DEs in the *no limit* treatment.

6 Results

We structure the results section in four parts. The first part focuses on the main research question: whether the possibility to use stop-loss and take-gain options causally reduces the disposition effect and if so, why. In the subsection 6.2, we discuss the portfolio choices of our investors in more detail. Section 6.3 presents limit use in the limit treatments and in subsection 6.4 we provide regression

[9] The DE measures resulting from the simple trading heuristic can be calculated for 18 out of the 22 price (the trading heuristic yields no possibility to sell at a loss for 4 price developments) and ranges from -1 to -.625.

[10] Again, this holds true as long as investors have homogeneous priors about the six goods, which is reasonable to assume in our setting.

analyses on DEs and PLRs across treatments controlling for risk preferences, overconfidence and socio-economic background variables.

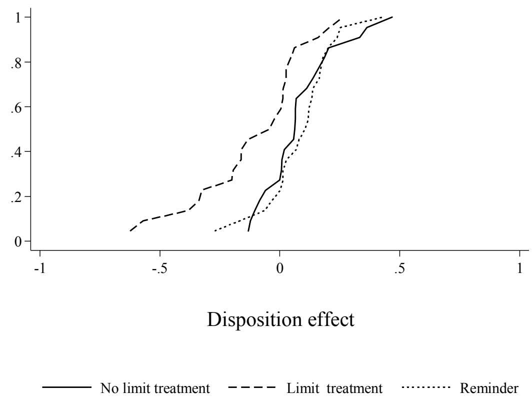
6.1 Automatic selling devices and the disposition effect

We first report whether the *limit* treatment causally reduces the disposition effect. Then we show whether the reduction is due to an increase in realized losses (i.e. through PLR) or due to a decrease in realized gains (i.e. through PWR). Finally, we report the results from the *reminder* treatment. To investigate whether the *limit* treatment is effective, we compare the distribution of DEs across the treatments (Figure 1) as well as how the DEs differ across treatments for given price developments (Figure 2). Figure 1 shows the cumulative distribution (cdf) of the individual disposition effects using the weighted average purchase price as the reference price.[11] The cdf in the *limit* treatment is clearly to the left of the cdf for the *no limit* treatment, i.e. DE values are significantly lower in the *limit* treatment (Kolmogorov-Smirnov test, p=0.002). Further, a binomial test rejects the H_0 that investors are equally likely to have a positive or negative DE in the *no limit* (p=0.052) and *reminder* treatment (p=0.004) whereas it fails to reject this H_0 in the *limit* treatment (p=0.832).

Figure 2 shows the DE for pairs of investors across treatments facing the same price development. Each point reflects how large the DE was for a specific price development in the *no limit* and *limit* treatment. It becomes clear from the figure that more observations are below or close to the 45° line. That is, for most price developments, DEs are larger in the *no limit* treatment. A Wilcoxon signed-rank test rejects the equality of the population mean ranks (p=0.008). We summarize these findings in Result 1.

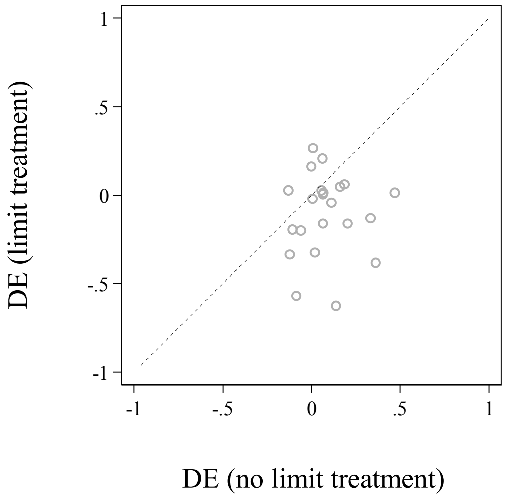
Result 1: *The disposition effect is significantly lower in the limit treatment than in the no limit treatment.*

[11] As we could not calculate the DEs for one individual in the *no limit* treatment and one individual in the *limit* treatment (as they had no possibility to sell any asset at a loss) we exclude these individuals as well as the DEs/PLRs of those subjects, who experienced the same price developments from the figures and the non-parametric analyses.



($n_{no\ limit}$=22, n_{limit}=22, $n_{Reminder}$=22)

Figure 1: Cumulative distributions of individual DEs



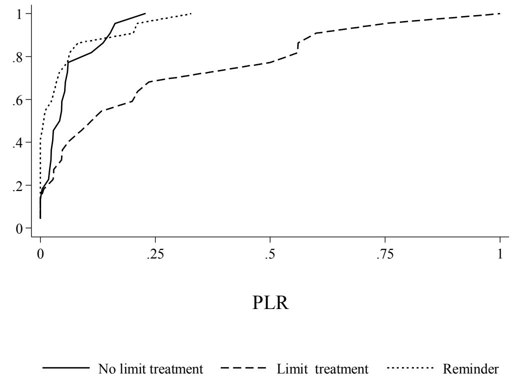
($n_{no\ limit}$ = 22, n_{limit} = 22)

Figure 2: DE for investors facing same price development

Separating the DE into its two components (PWR and PLR) makes it possible to disentangle whether the *limit* treatment reduces the disposition effect due to a higher proportion of realized losses or due to a lower proportion of realized gains.[12] Figure 3 shows the cumulative distributions for the PLR and PWR across treatments. The cdf of the PLR in the limit treatment first order stochastically dominates the cdf in the no-limit treatment (Kolmogorov-Smirnov test, two-sided, p=0.049). However, we cannot reject the equality of distributions for the PWR (Kolmogorov-Smirnov test, two-sided, p=0.632). We summarize our finding in Result 2:

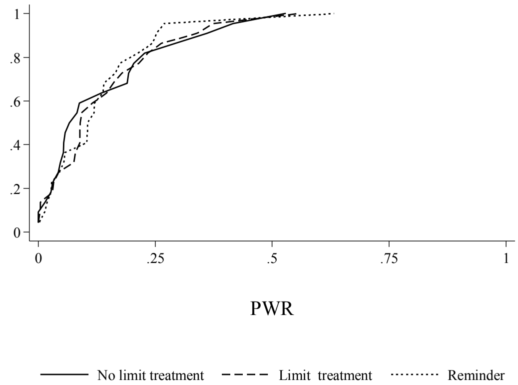
[12] Note that, consistent with previous findings by Weber and Welfens (2007), the correlation between the PLR and PWR in the *no limit* and *limit* treatment is small and insignificant (*no limit* treatment: Spearman's ρ=-0.09, p-value=0.685, *limit* treatment: ρ=0.23, p-value=0.305, *reminder*: ρ=-0.01, p-value=0.965).

(a)



($n_{no\ limit} = 22$, $n_{limit} = 22$, $n_{Reminder} = 22$)

(b)



($n_{No\ limit} = 22$, $n_{Limit} = 22$, $n_{Reminder} = 22$)

Figure 3: PLR (a) and PWR (b) across treatments.

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

Results 1 and 2 show that the possibility to use automatic selling devices causally reduces the disposition effect by significantly increasing the proportion of losses realized. As the limit treatment does not provide better market access we can exclude the notion that the reduction in DEs is caused because automatic selling devices make rational traders more able to implement optimal trading strategies. Instead, the *limit* treatment reduces the disposition effect either because it changes the way investors trade (as they have to think about price limits and enter upper and lower price limits when purchasing assets) or because investors can use the automatic selling device as a commitment to sell assets at a loss *before* they experience the loss. The *reminder* treatment lets us disentangle these two possibilities. As in the *limit* treatment, in *reminder* investors also had to enter lower and upper price limits for which they planned to sell their assets and received a message if one of their price limits had been hit.[13] However, in *reminder* no automatic selling device triggered a sale. Figures 1 and 3 show that the cumulative distribution functions of DEs and PLRs in *reminder* differ significantly from those in the *limit* treatment (Kolmogorov-Smirnov test, *reminder vs. limit* treatment, DE: p=0.007, PLR: p=0.020, PWR: p=0.872) and overlap with those in the *no limit* treatment (Kolmogorov-Smirnov test, *reminder* treatment *vs. no limit* treatment, DE: p=0.394, PLR: p=0.109, PWR: p=0.632). Consequently, we conclude that it is not enough to make people think about price limits and remind them about these limits when they are hit to reduce the reluctance to realize losses. Instead, the limit treatment is effective, because it provides an opportunity to commit to selling at a loss before the loss actually occurs. We summarize this finding in Result 3:

Result 3*: Reminders about selling plans are ineffective.*

6.2 Investors' portfolio choices

In this section we briefly shed light on investors' portfolio choices. At the beginning of the experiment investors hold on average about 50 percent of their wealth in assets (see Figure 4). This share is stable over time in the *limit* treatment and *reminder*. However, investors in the *no limit* treatment increase the share of wealth invested in assets over time to up to 75 percent (as they reduce the absolute number of points on their account over time). To understand this development better, we further present investors' relative portfolio choice according to price realizations. We rank the assets according to their relative price level from rank 1 (highest price) to rank 6 (lowest price) for each round. If two or more assets have the same price, they both receive the upper rank and the respective rank(s) below are empty (e.g. if two assets have the highest price both receive rank 1, and rank 2 is

[13] Note that limit choices (measured as the individual average difference between trading prices and the limit, excluding those who did not set a limit) do not differ significantly between the *reminder* and *limit* treatment (Kolmogorov-Smirnov test, *reminder* treatment vs. *limit* treatment, lower limits: p=0.157, upper limits: p=0.434). Also, the number of subjects who never set a lower limit (upper limit) does not different significantly (Fisher-exact test, for lower limits: p=0.281, for upper limits: p=0.185).

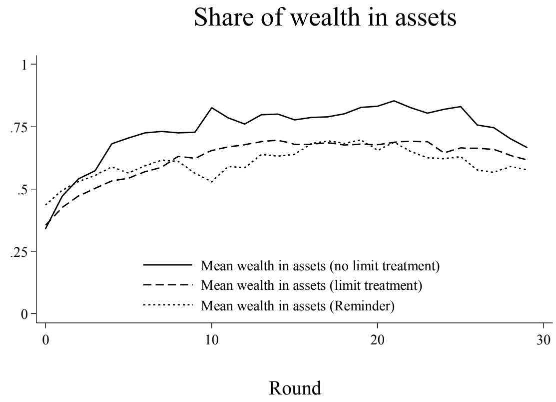


Figure 4: Share of wealth in assets across treatments

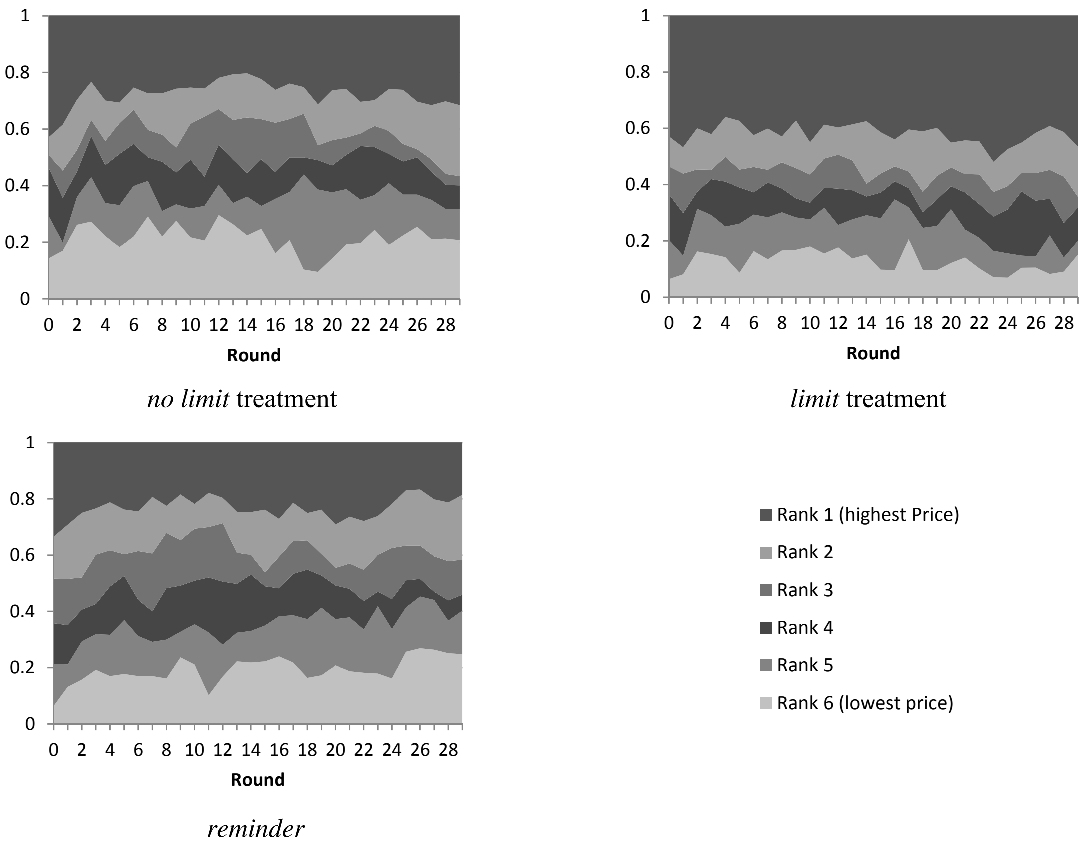


Figure 5: Average relative quantities in no limit and limit treatment

consequently empty).[14] In a next step we calculate the relative quantities of each asset for each round.[15] Figure 5 shows the average relative quantity by treatment across rounds. The relative quantity of the asset with the highest price in the limit treatment is on average (round 0 to 29) 38.2 percent

[14] Note that this may lead to an overrepresentation of some ranks in some rounds but does not systematically affect the treatment comparison because for each price development we have one investor in each treatment.

[15] Relative quantity $= \frac{\#\ of\ units\ held\ in\ risky\ good\ with\ price\ rank\ i}{\sum_{j=1}^{6} \#\ of\ units\ held\ in\ risky\ good\ with\ price\ rank\ j}$.

whereas it is only 25.9 percent in the *no-limit* treatment (Wilcoxon signed-rank test, p= 0.133). The average relative quantity of the asset with the lowest price is 12.8 percent in the *limit* treatment and 19.9 percent in the *no-limit* treatment (Wilcoxon signed-rank test, p= 0.184). In *reminder* average relative quantity of the asset with the highest price amounts to 24.4 percent (Wilcoxon signed-rank test, *reminder* vs. *limit* treatment p= 0.09, *reminder* vs. no *limit* treatment p= 0.787) and the average relative quantity of the asset with the lowest price to 19.4 percent (Wilcoxon signed-rank test, *reminder* vs. *limit* treatment p= 0.379 *reminder* vs. no *limit* treatment p= 0.865).

Interestingly, we do not observe a general time trend with respect to portfolio choice. Instead, after few rounds investors in the *limit* treatment tend to hold the asset with the highest price more frequently than in the *no limit* and *reminder* treatment. Consequently, for the same price developments, traders in the *limit* treatment have significantly fewer possibilities to sell at a loss ($PSL_{nolimit} = 165.05$ vs. $PSL_{limit} = 74.09$, $p<0.01$) and tend to have (insignificantly) more possibilities to sell at a gain ($PSG_{nolimit} = 218.09$ vs. $PSG_{limit} = 224.09$, $p=0.906$).

6.3 Limit use

Table 1 provides an overview on DEs across treatments and the use of limits. First note that not all investors in the *limit treatment* make use of the limit option. Thirteen out of 22 investors (for whom we can calculate the DE) use both limits. Two investors use only the lower limit and no investor uses only the upper limit. The investors who use both limits use the upper limit in about 44 percent of the cases and the lower limit in about 50 percent of the cases (Wilcoxon signed-rank test, p= 0.47). Disposition effects of the investors who make use of both limits are significantly smaller than disposition effects of investors in the *no limit* treatment ($DE_{limit\ treatment\_\mathrm{usedlimit}} = -0.124$ vs. $DE_{no\ limit\ treatment} = 0.082$, Kolmogorov-Smirnov-Test, p= 0.036). Differences between the DEs of the seven investors who do not use any limits and the DEs in the *no limit* treatment are statistically insignificant ($DE_{limittreatment\_\mathrm{usednolimit}} = -0.078$ vs. $DE_{no\ limit\ treatment} = 0.082$, Kolmogorov-Smirnov-Test, p= 0.218); reflecting that these investors may either be individuals with perfect self-control or naïve investors who underestimate their reluctance to actively sell assets trading at a loss.

Table 1: DE, PLR, PWR and Limit use

	Treatment		
	No Limit	*Limit*	*Reminder*
Main outcomes (all investors)			
Disposition effect	0.082	-0.106	0.088
Share of investors with DE >0	0.727	0.455	0.818
PLR	0.059	0.252	0.050
PWR	0.141	0.146	0.138
Investors limit use			
Investors using both limits	-	13 out of 22	17 out of 22
Investors using only upper limit	-	2 out of 22	1 out of 22
Investors using only lower limit	-	0 out of 22	2 out of 22
Investors using no limit	-	7 out of 22	2 out of 22
Among investors using both limits			
Limit choices			
Δ Upper limit to weighted purchase price	-	0.329	0.206
Δ Lower limit to weighted purchase price	-	0.234	0.151
Unforced sales (sales in all but last round)			
Δ Selling price to weighted purchase price (gains)	0.116	0.098	0.118
Δ Selling price to weighted purchase price (losses)	0.138	0.104	0.123
Sales in last round (forced)			
Δ Selling price to weighted purchase price (gains)	0.219	0.222	0.168
Δ Selling price to weighted purchase price (losses)	0.251	0.136	0.166

Note: Table reports means for the 22 price developments for which DEs could be calculated in all treatments.

In *reminder*, slightly more investors use both limits (χ^2 test, p=0.195) but reminders are ineffective, also for investors using them ($DE_{reminder\_usedlimit}$ = 0.099 vs. $DE_{no\ limit\ treatment}$ = 0.082, Kolmogorov-Smirnov-Test, p= 0.163).

To investigate whether investors use lower and upper limits in different ways we calculate the average relative distance of limits to the current trading price (for all trading rounds). We find that, in the *limit* treatment, the average relative distance to the upper limit (0.329) tends to be larger than the average relative distance to the lower limit but differences are statistically insignificant (0.234, Wilcoxon signed-rank-test, p=0.167). In the *reminder* treatment, the upper distance amounts to 0.206 and is significantly larger than the distance to the lower limit (0.151, Wilcoxon signed-rank-test, p=0.057).

Finally we report the size of realized gains or losses, i.e. the relative distances of the weighted purchase price to the selling price across treatments. For all unforced realizations i.e. for all gains and losses realized before the last round, losses and gains of investors using both limits tend to be smaller in the *limit* than in the *no limit* treatment but the differences are statistically insignificant (Wilcoxon rank sum test, two-sided, using individual averages, for losses p=0.627, for gains p= 0.556). Relative distances for forced sales in the last round amount to 0.251 for losses and 0.219 for gains in the *no limit* treatment and to 0.136 for losses and 0.222 for gains in the *limit* treatment (Wilcoxon rank sum test, two-sided, for losses p=0.521 and for gains p=0.962). While realized losses in the last round tend to be smaller than gains (0.136 vs. 0.222) in the *limit* treatment*,* gains and losses investors realize in the last round are of similar size in the *no limit* treatment (as well as in the *reminder* treatment).

Our main result (Result 1) thus reflects three behavioral aspects. First, and most importantly, investors who are offered the possibility to sell gains and losses automatically are less reluctant to sell losses in general (see Section 6.1). Second, investors in the *limit* treatment are also more likely to hold assets with higher prices (see Section 6.2). Third, investors in the limit treatment tend to set the lower limits closer to current prices than upper limits and tend to use the upper limit slightly less frequently than the lower limit (see Section 6.3). In turn, investors in the *limit* treatment realize more losses than gains than in the *no limit* and *reminder* treatments.

6.4 Individual characteristics and final payoffs

Finally, we provide a robustness test on our treatment effects by reporting results from OLS regressions including controls for individual characteristics (see Table 2). We elicited the additional control variables in a post-experimental questionnaire. Our specifications control for the following self-stated variables: Investors' average *math grade* in final secondary school examinations (standardized with higher values corresponding to better grades), investors' gender (using a *male* dummy), investors' *risk-taking* behavior in monetary decisions (ranging from not at all risk seeking to very risk seeking, see Dohmen et al., 2011), a dummy for investors' *stock market experience,* and a standardized measure of investors' *overconfidence.*[16]

[16] The overconfidence measure is calculated as follows: participants 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. In the regression analysis we use the standardized sum of ranks.

Table 2: OLS Regression on DEs(1a-1c), PLRs (2a-2c) and Total Points earned (3)

Dep. Var.	(1a) **DE**	(1b) **DE**	(1c) **DE**	(2a) **PLR**	(2b) **PLR**	(2c) **PLR**	(3) **Total Points**
Treatment effect							
Limit treatment	**-0.221***** (0.0682)	**-0.226***** (0.0771)	**-0.224*** (0.109)	**0.185***** (0.0596)	**0.197***** (0.0584)	**0.131*** (0.0632)	
Reminder	-0.0397 (0.0635)	-0.0401 (0.0696)	-0.0403 (0.115)	-0.00176 (0.0225)	0.00593 (0.0321)	-0.0996 (0.0932)	
Limit use							
Use of upper limit (share)			0.0553 (0.0648)			0.00503 (0.0800)	
Use of lower limit (share)			-0.0519 (0.0963)			0.116 (0.0843)	
Disposition Effect (standardized)							**-210.778***** (63.038)
Controls							
Math grade (standardized)		0.0130 (0.0274)	0.0150 (0.0287)		-0.0255 (0.0204)	-0.0291 (0.0221)	66.057 (51.647)
Male		0.0428 (0.0434)	0.0448 (0.0422)		-0.0332 (0.0385)	-0.00793 (0.0473)	**338.351***** (112.722)
Risk taking (money) (standardized)		0.0370 (0.0223)	0.0337 (0.0248)		**-0.0371*** (0.0193)	-0.0290 (0.0192)	58.489 (72.369)
Overconfidence (standardized)		-0.00769 (0.0343)	-0.00911 (0.0364)		0.0131 (0.0215)	0.00975 (0.0215)	**-151.408***** (51.364)
Stock market Experience (std.)		-0.0459 (0.0777)	-0.0428 (0.0804)		**0.135***** (0.0418)	**0.124***** (0.0407)	-133.286 (164.715)
Constant	**0.119**** (0.0497)	**0.105*** (0.0565)	**0.103*** (0.0557)	**0.0564***** (0.0129)	0.0451 (0.0323)	0.0335 (0.0331)	**2390.100***** (69.775)
Independent obs.	24	24	24	24	24	24	24
R-squared	0.184	0.234	0.238	0.206	0.320	0.353	0.354

Robust standard errors in parentheses, clustered on 24 different price developments, *** p<0.01, ** p<0.05, * p<0.1 Share use of lower and upper limits equals zero in the *no limit* treatment.

Models (1a) and (1b) confirm the non-parametric tests: 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 (1c) we include how frequently individuals use the upper and lower limit on average as additional explanatory variables. Model (1c) shows again that the limit treatment, i.e. having the possibility to use the automatic selling device, reduces the DE significantly.[17] Within the limit treatment, there is a tendency for investors who use the lower limit more frequently to also have lower DEs and for investors using the upper limit more frequently to have higher DEs. However, the coefficients for limit use are small and statistically insignificant. Models (2a-2c) repeat the analysis for investors' PLRs. In line with the non-parametric results, the

[17] Note that limit use in these regressions is defined as "0" for the *no limit* treatment and corresponds to the average limit use of an investor in the *limit* and *reminder* treatment.

limit treatment significantly increases investors' PLRs. Finally, model (3) relates the standardized disposition effects to the total number of points earned in the experiment. On average, an increase by one standard deviation of the individual DE decreases the total number of points earned by around 211 points.[18]

7 Discussion and Conclusion

We investigated the role of stop-loss and take-gain orders for the disposition effect in a trading environment that abstracts from several explanations for the disposition effect (such as explicit transaction costs, tax reasons, portfolio balancing and beliefs in mean reversion). In the experimental treatments, investors can either (i) only actively sell assets (*no limit* treatment), (ii) have the additional possibility to sell assets by using automatic stop-loss and take-gain options (*limit* treatment) or (iii) can pre-specify selling plans and are reminded about the selling plans if a price limit is hit (*reminder* treatment). Our results provide evidence that automatic selling devices causally reduce the disposition effect by helping investors to realize their losses whereas reminders about selling plans are not enough to reduce investors' disposition effects.

The findings from our experiment highlight the role of time-inconsistent behavior for the disposition effect. The *limit* treatment allows our investors to commit to selling an asset that trades at a loss before the loss actually occurs, and results in relatively low disposition effect measures. Neither the *reminder* treatment nor the *no limit* treatment offer this opportunity and both yield significantly higher disposition effects. Rational behavior as well as alternative utility models that do not (explicitly or implicitly) include time-inconsistencies cannot explain these treatment effects (i.e. the reduction of investors' disposition effect through automatic selling devices). Explanations for the disposition effect such as prospect theory, pride seeking and regret minimization, or theories of realization utility need to be augmented by time-inconsistent behavior to be able to explain why automatic sales may help to reduce the disposition effect. Further, as disposition effects in the *reminder* treatment are not lower

[18] 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 is 2561.9 whereas the median for the investors in the limit treatment is 2584.45 (Wilcoxon signed-rank test, p>0.10). However, in our trading environment, it is, by design, likely that a significant difference in disposition effects translates into significant differences in total points earned with a longer trading horizon.

than in the *no limit* treatment, it is unlikely that lower affect (hot. vs. cold effects, see also Loewenstein, 2005) in decision making or theories of cognitive dissonance, such as investors' avoidance of acknowledging personal purchase failures (see e.g.Chang et al., 2016) alone can explain the reduction of investors' disposition effects.

In our experiment, automatic selling devices helped investors to increase the proportion of losers realized but did not affect the proportion of winners realized. This asymmetric impact of the *limit* treatment on the disposition effect suggests that time-inconsistent behavior matters indeed for the disposition effect. In contrast to stop-loss orders, take-gain orders do not allow investors to commit postponing (impulsive) trades at a gain. Instead, if at all, take-gain orders may remind investors about the size of gains they planned to realize ex-ante. Future research may thus investigate whether investors who have an impulsive inclination to realize small short term gains may benefit from a trading device that allows them to ex-ante commit to realizing gains later (at a larger pre-specified size).

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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 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 have the possibility to trade goods in every period (0 to 30). You can buy additional units of goods as long as the money on your account exceeds the price for one unit of the respective good. Also, you see the price change of each good in each period.

[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. **]**

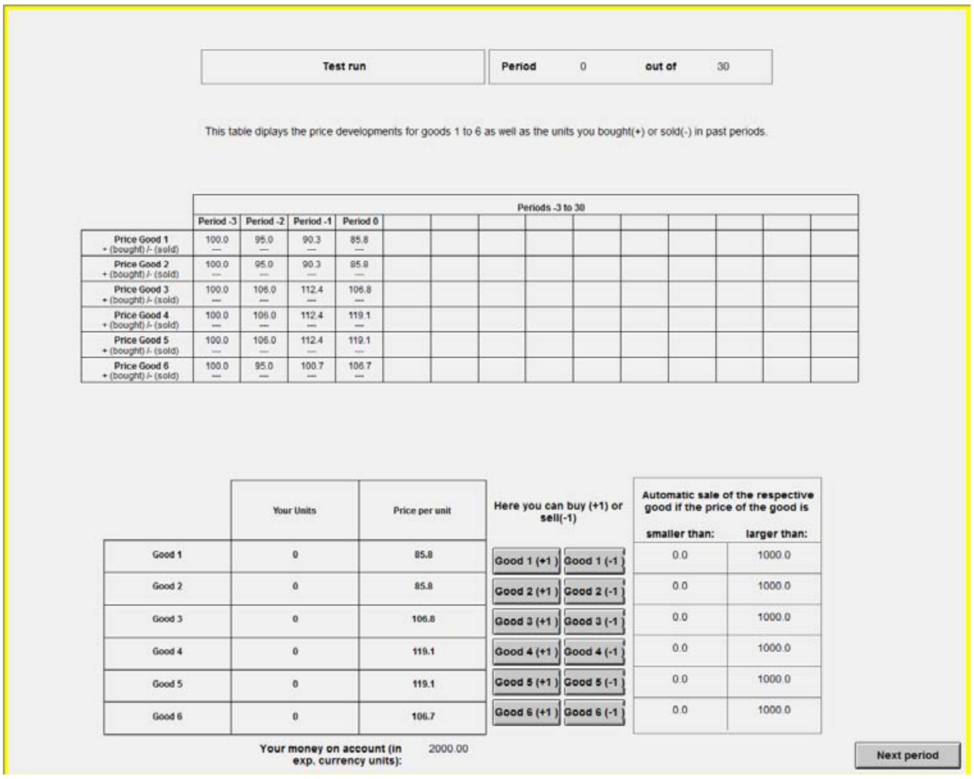
End of the experiment

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 any 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 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 buy one unit of Good 2 [and set the lower limit to “0” and the upper limit to “1000”. After you are done, 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 sold because the price of Good 1 reached on 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“.]

In the real experiment in order to start the next period you would click on “to 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, however, ends here. Please click now on “to next period“.

Please solve now the following control questions!

Control questions [correct answers in parentheses]

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 [correct answer]

2. Are the prices of the six goods independently determined?

□ Yes [correct answer]

□ 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 [correct answer]

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 [correct answer]

4. a) By how much do prices increase if they increase?

□ 6 % [correct answer]

□ 5 %

b) By how much do prices decrease if they decrease?

□ 6 %

□ 5 % [correct answer]

- 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 [correct answer]

□ 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 [correct answer]

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

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