HOW MALLEABLE ARE CHOICE BRACKETS? THE CASE OF MYOPIC LOSS AVERSION

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Abstract

We study whether a change in the framing of the decision problem affects how individuals bracket their choices in the context of myopic loss aversion. We find a systematic asymmetry in the malleability of choice brackets: Subjects who were previously exposed to a frame that encourages narrow bracketing can learn to bracket broadly when the framing of the decision problem changes. In contrast, subjects previously exposed to a frame encouraging broad bracketing continue to bracket broadly after a change in framing. Our results further suggest that narrow bracketing is a mistake rather than the outcome of preferences. **JEL classification**: C91, D14, D81, G11

**Keywords**: framing, feedback frequency, investment behavior, myopic loss aversion, narrow bracketing

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1 Introduction

A fundamental question in research on decision making is how people integrate over individual choices. Accumulating evidence suggests that in many situations people bracket narrowly, i.e., view each choice in isolation and not in the context of the other choices they face (Kahneman and Tversky 1984; Thaler 1985; Kahneman and Lovallo 1993). Narrow bracketing can explain important departures from the predictions of the neo-classical model in different domains such as consumption choices (Abeler and Marklein 2010) or investment decisions (Barberis et al. 2006; Benartzi and Thaler 1995; Gneezy and Potters 1997). Even in very simple decision problems, narrow bracketing can lead people to select strictly dominated choice alternatives (Rabin and Weizsäcker 2009).

In view of the potentially adverse consequences, understanding how individuals form their choice brackets is of crucial importance. A main finding of the literature is that the framing of a decision problem can influence whether or not people bracket narrowly (e.g., Abeler and Marklein 2010; Gneezy and Potters 1997; Thaler et al. 1997). A key open question is how malleable choice brackets are, i.e., whether and how individuals adapt their choice brackets in response to changes in the framing of a decision problem. In this paper, we study the malleability of choice brackets in the context of investment decisions. We implement a dynamic design in which we exogenously vary frames over time. We show that the impact of a given frame differs vastly depending on which frame an individual has previously been exposed to. More specifically, subjects previously exposed to a frame which encourages narrow bracketing can learn to bracket broadly after a frame change. In contrast, subjects previously exposed to a frame which encourages broad bracketing continue to bracket broadly after a frame change. Brackets are thus not completely malleable in every direction.
The specific concept we use to address our research question is myopic loss aversion (MLA), a combination of narrow bracketing and loss aversion, which has become famous as an explanation for the equity premium puzzle (Benartzi and Thaler 1995; Barberis et al. 2006). Here narrow bracketing implies that investors evaluate their portfolios frequently. A key prediction of MLA is that the willingness to invest in a risky asset varies with the frequency at which an individual evaluates financial outcomes. Loss-averse investors who evaluate the outcomes of their investments over shorter periods will find stocks, ceteris paribus, less attractive since these run a greater risk of short-run losses than bonds. They will therefore only be willing to hold stocks at a substantial premium.

Evidence for MLA comes from a substantial number of experiments pioneered by Gneezy and Potters (1997) and Thaler et al. (1997). These experiments study repeated investments into a risky asset with positive expected return. The key idea is to exogenously manipulate the evaluation period of investors via different feedback institutions. All subjects face the same underlying decision problem but are given feedback either at a high frequency (High treatment) to induce a short evaluation period, i.e., narrow bracketing, or at a low frequency (Low treatment) to induce a long evaluation period, i.e., broad bracketing. The different feedback institutions systematically affect investment as predicted by MLA: subjects in Low invest more into the risky asset and thus make higher profits than those in High. We refer to this difference as investment gap.

Myopic loss aversion is well suited to study the malleability of choice brackets in a dynamic setup for at least two reasons. First, the robustness of the investment gap has been demonstrated for an impressive range of contexts and decision makers (Bellemare et al. 2005; Eriksen and Kvaloy 2010a; Eriksen and Kvaloy 2010b; Fellner and Sutter 2009; Gneezy et al. 2003; Haigh and List 2005; Moher and Koehler 2010; Sutter 2007; van der Heijden et al. 2012). Based on these findings
for between-subject setups, various researchers suggest that changes in feedback institutions might be used for policy interventions, for example, to increase retirement savings (e.g., Fellner and Sutter 2009; Gneezy et al. 2003; Haigh and List 2005). However, the effectiveness of such measures, which depends on the malleability of choice brackets, has not been studied yet.

Second, the investment gap plays an important role in a recent debate about the nature of narrow bracketing. While behavior of the type observed in MLA experiments has typically been interpreted as a mistake (Benartzi and Thaler 1995), Kőszegei and Rabin (2009) present a model in which this type of behavior can be a manifestation of preferences over changes in beliefs about future consumption. In their model, an individual may dislike piecemeal interim information because this leads to unnecessary fluctuations in beliefs. Kőszegei and Rabin (2009) argue that the investment gap observed in the literature on MLA is consistent with such preferences because more interim information is given in High. To reduce the resulting fluctuations in beliefs, an individual with Kőszegei-Rabin preferences will invest less in High than in Low.

To investigate these issues we provide controlled evidence on the effects of changes in feedback institutions. Our experiment consists of two parts. In both parts, subjects play the standard investment task introduced by Gneezy and Potters (1997). In two treatments, subjects are exposed to a change in feedback institutions, either going from a high frequency of feedback in part 1 to a low frequency of feedback in part 2, or vice versa. Our design thus allows us to investigate whether previous exposure to a certain feedback institution affects subsequent behavior. In two additional control treatments, subjects are exposed to the same feedback institution in both parts of the experiment. We can therefore distinguish the behavioral effects of changes in feedback institutions from time trends, or wealth effects. In particular, we can check whether the usual gap in investment between High and Low persists
in part 2 after previous exposure either to a high, or a low frequency of feedback in part 1.

According to the preference interpretation of the investment gap only the feedback institution that is currently in place should matter, i.e., going from High to Low, subjects should increase their investment, and going from Low to High they should decrease it. If instead the investment gap is a mistake that arises because individuals passively accept a given framing of the decision problem (Kahneman 2003), the effects of a change depend on whether subjects also passively accept the new framing of the decision problem after the change. Three cases seem plausible. First, agents might always passively accept the feedback institution that is currently in place. Less frequent feedback should therefore lead to higher, and more frequent feedback to lower investment. Second, agents might “learn” not to make the mistake of narrow bracketing once they have been induced to bracket broadly by low frequency of feedback. This would lead to an asymmetric effect: investments should increase going from High to Low, but not decrease going from Low to High. Finally, subjects might simply ignore the new formulation of the decision problem after a change in feedback institution and continue to use the same decision brackets as before.

Our main finding is an asymmetric effect of a change in feedback institutions. Subjects who go from a high to a low frequency of feedback increase their investment, but those who change from low to high frequency of feedback do not lower their investment. We thus observe the usual investment gap between High and Low in part 2 after exposure to high frequency in part 1 of the experiment. However, exposure to low frequency of feedback in the first part completely eliminates the investment gap in the second part. This suggests that feedback institutions influence choice brackets after a high frequency of feedback, whereas this does not seem to be the case when starting with a low frequency of feedback. In other words, a broad
choice bracket induced by a low frequency of feedback cannot be narrowed again by high frequency feedback.

Our paper provides several novel insights. First, by studying behavior in a tightly controlled dynamic context, it moves beyond simply establishing a framing effect of feedback institutions. One previous study that investigates the potential effects of changes in feedback institutions uses market data on stock prices in Israel after switches in the trading frequency of securities from daily to weekly trading or vice versa (Kliger and Levit 2009). These switches are not exogenous but implemented by the stock exchange in response to market developments. It is therefore unclear to which degree the observed results are driven by MLA or confounding factors.\(^1\) In the laboratory, we are able to implement exogenous changes of feedback institutions and isolate their effects by using control treatments. In addition, we observe how each individual reacts to these changes and can investigate whether heterogeneity in these reactions can be traced back to differences in individual characteristics such as cognitive ability or impulsiveness.

Second, our findings raise a more general methodological point. Given that narrow bracketing may lead to suboptimal decisions (Rabin and Weizsäcker 2009), the question how a particular way of bracketing can be induced has received a lot of attention. Much of what we know about framing effects has been learned from static between-subject setups (e.g., Abeler and Marklein 2010). In many instances, policy advice is based on between-subject findings with the implicit assumption that these findings will also hold in a dynamic within-person setup, i.e., when the same person

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\(^1\)In Fellner and Sutter (2009), changes in feedback institutions also arise endogenously since individuals can decide to switch feedback institutions for a fee. Unfortunately, the number of subjects who switch is too small to make a statement about switches from *High* to *Low* (\(n = 1\)). For those subjects who switch from *Low* to *High* at some point during the experiment average investment before and after the switch does not differ significantly (\(n = 11\)). While this is qualitatively in line with our findings, it is unclear whether this is due to the feedback institutions, selection effects or the low number of observations.
is exposed to different formulations of the decision problem over time. Our results suggest that such policy changes may not always have the intended effects because choice brackets may only be malleable in certain directions. In the case of financial decision making this can be good news, since subjects do not seem to revert to narrow bracketing once they have been exposed to a framing of the decision problem that facilitates broad bracketing.

Third, we contribute to the recent discussion about the nature of narrow bracketing. The asymmetric effect we find is not in line with Kőszegi and Rabin (2009). Instead, our findings provide support for the interpretation of narrow bracketing as a mistake.

Finally, our results tie in with a larger literature which studies the path dependence of institutions (e.g., Bohnet and Huck 2004; Falk et al. 2006; Gneezy and Rustichini 2000). This literature shows that sometimes institutions cannot simply be switched on and off, but may have lasting effects, e.g., because previous exposure to a certain institution affects what people perceive as fair. We provide another rationale for why such lasting effects exist. In our setup, these effects occur even though the institutions we consider are quite subtle compared to, e.g., imposing a binding minimum wage as in Falk et al. (2006).

The rest of the paper is structured as follows. Section 2 describes the experimental design. Section 3 reports and discusses our results. Section 4 concludes.

2 Experimental Design and Predictions

2.1 Experimental Design

Our experiment is based on the investment task from Gneezy and Potters (1997), which has become the most widely used tool in research on myopic loss aversion. In this task participants receive an endowment of 100 experimental currency units
(ECU) in every round and decide which amount $X \in [0, 100]$ to invest in a risky lottery with positive expected value. The lottery yields a profit of 2.5 times the investment with probability 1/3; with probability 2/3 the amount invested is lost. The payoff for each round is thus:

$$\pi = \begin{cases} 
100 + 2.5X & \text{with probability } 1/3, \\
100 - X & \text{with probability } 2/3.
\end{cases}$$

Subjects play nine rounds of the investment task. The key idea in Gneezy and Potters (1997) is to vary the frequency at which participants receive feedback about the lottery outcomes, and the number of rounds for which they have to commit their investment.\(^2\) In the high frequency treatment (High), subjects have to make an investment in each round. After each round, they receive feedback about the outcome of the lottery. In the low frequency treatment (Low), subjects have to make a decision for blocks of three rounds. The chosen amount is invested in each of the next three rounds, and participants receive feedback about the outcome of the three lotteries at the end of each block.\(^3\)

In our experiment, we are interested in the effects of a change in feedback institutions. Subjects therefore play two parts with nine rounds each. In both parts,

\(^2\)Bellemare et al. (2005), Hopfensitz and Wranik (2008), and Fellner and Sutter (2009) study the effects of varying just one of these two factors in the original setup. Langer and Weber (2008) and Looney and Hardin (2009) study the same question in a multiplicative setup where subjects can invest their earnings from one round in the subsequent round. The results are mixed, suggesting that both factors contribute to the investment gap. Beshears et al. (2012) investigate the influence of feedback frequency in an online experiment. Subjects can invest a substantial amount in real mutual funds over the course of one year. The design varies among other things the frequency (weekly vs. biannual) with which subjects receive an email with a link to the performance of their investments. Subjects in both treatments are free to change their investment at any time. In contrast to the lab experiments, Beshears et al. (2012) only find a small and insignificant difference in initial allocation to equities between the two groups.

\(^3\)Screenshots of the feedback screens in our experiment (Figures A1 and A2) can be found in the appendix.
Table 1. Treatment Overview

<table>
<thead>
<tr>
<th>Type of Treatment</th>
<th>Treatment Feedback Institution</th>
<th>N</th>
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<tbody>
<tr>
<td>Change</td>
<td>High Low</td>
<td>43</td>
</tr>
<tr>
<td>Change</td>
<td>Low High</td>
<td>43</td>
</tr>
<tr>
<td>Control</td>
<td>High High</td>
<td>38</td>
</tr>
<tr>
<td>Control</td>
<td>Low Low</td>
<td>38</td>
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</tbody>
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subjects either play the original $High$ or $Low$ treatment described above. We study two treatments in which subjects experience a change in feedback institutions either going from high frequency of feedback in part 1 to low frequency of feedback in part 2 ($HighLow$) or vice versa ($LowHigh$). To ascertain that potential changes in behavior can really be ascribed to changes in feedback institutions instead of time trends or wealth effects, we include two control treatments in which subjects are exposed to the same feedback institution in both parts ($HighHigh$ and $LowLow$). Comparing $HighLow$ to $HighHigh$, and $LowHigh$ to $LowLow$ allows us to test whether the investment gap persists after previous exposure to $High$ or $Low$, respectively. Our $2 \times 2$ design is summarized in Table 1.

In total, 162 subjects, mainly students of the University of Bonn from various fields of study, participated in the experiment. Subjects were recruited from the database of the BonnEconLab via ORSEE (Greiner 2004), and the experiment was programmed with z-Tree (Fischbacher 2007). Each subject participated in only one of the treatments. The change treatments ($HighLow$ and $LowHigh$) were played with 43 participants each, the control treatments ($HighHigh$ and $LowLow$) with 38 participants each. Differences are due to variation in show-up rate at the laboratory. Upon arrival subjects were seated in separate cubicles closed off with curtains. The
instructions for part 1 were distributed and read aloud. Subjects knew ex ante that
the experiment had two parts but did not know what the second part was until they
had completed the first part. After completion of part 1, instructions for part 2 were
distributed and read aloud. An English version of the instructions for part 1 and
part 2 of LowHigh can be found in the appendix. After the experiment, participants
answered a questionnaire which included items on socio-demographic variables as
well as mathematical ability, risk attitudes, impulsiveness, regret, confidence in own
decisions, and interest in and experience with financial markets. This allows us to
check whether certain individuals are particularly vulnerable to frame manipula-
tions. At the end of each session, ECU were converted into euro at an exchange rate
of 100 ECU = 0.5 euros. Sessions lasted approximately 30 minutes, and subjects
earned 11.81 euros on average, including a show-up fee of 2 euros.

2.2 Behavioral Predictions

Predictions for the effect of changes in feedback institutions crucially depend on
whether the investment gap between High and Low observed in a static environ-
ment is the result of a mistake or driven by preferences, as suggested by Kőszegi
and Rabin (2009). An agent with such preferences dislikes interim piecemeal in-
formation, since it exposes him to possibly unnecessary fluctuations in beliefs.\(^4\) To
reduce these fluctuations, he will invest less in High than in Low. This will always be
the case independent from previous exposure to another feedback institution since
agents only care about the current timing of information. For changes in feedback
institutions, the model therefore predicts that more frequent feedback will reduce
investment, and less frequent feedback will increase it. If subjects do not adjust their
behavior after a change in feedback institution, or do so only after a change in one

\(^4\)See Zimmermann (forthcoming) for a test of this prediction in a context where subjects can
choose the timing of information.
direction but not the other, this would be strong evidence against the interpretation of the investment gap as the manifestation of Kőszegi–Rabin preferences.

These cases would, however, be in line with an interpretation of the investment gap as a mistake. According to this interpretation, the investment gap in the static setup is a classic framing effect\(^5\): subjects make decisions intuitively rather than through effortful reasoning and passively accept the framing of the decision problem given to them. Hence, they evaluate their investment at the frequency at which they receive feedback. Because the gambles are independent and have a positive expected value, integrating over three gambles (broad bracketing) reduces the probability of observing a loss compared to separate evaluation of the gambles (narrow bracketing). Loss-averse subjects therefore invest more when they receive feedback less frequently.

In this framework, the effects of a change in feedback institutions crucially depend on whether or not subjects passively accept the new framing of the decision problem after the change. Intuitively, three outcomes seem plausible:

1. **Bracketing is completely malleable.** If subjects always passively accept the feedback institution that is currently in place, we would expect subjects in LowHigh to decrease, and subjects in HighLow to increase their investment after the change. Predictions in this case are identical to those for individuals with Kőszegi–Rabin preferences.

2. **Subjects learn to bracket broadly.** Low frequency of feedback makes the fact that viewing several plays of the lottery in one bracket reduces the probability of observing a loss more accessible to subjects. Once they have realized and experienced this effect, it is possible that it will still be accessible when

\(^5\)See, for example, Kahneman (2003) for a discussion of framing effects and the underlying mechanisms.
feedback institutions change. This would lead to an asymmetric effect: Subjects might “learn” not to make the mistake of narrow bracketing once they have been induced to bracket broadly by a low frequency of feedback. In this case, investment should still increase in part 2 of HighLow, but it would not decrease in part 2 of LowHigh.

3. Subjects stick with initial bracketing. Subjects might simply ignore the new framing of the decision problem after a change in feedback institution and continue to use the same decision bracketing as in part 1, in particular given the temporal proximity of the two parts. This could, for example, be the case if subjects realize that, despite the different frequency of feedback, they face the same decision problem in part 1 and part 2 and continue to bracket the decision problem in the way they did in part 1. In this case, subjects would not change their investment after a change in feedback institution. As a result, investment in HighLow should be below investment in LowHigh in part 2.

By studying changes of feedback institutions in both directions we are able to distinguish between the three options described above, and we can potentially disentangle the preference from the mistake interpretation of the investment gap.

The different explanations for the investment gap in a static setup also make different predictions for the comparison between each change treatment and its respective control treatment in part 2. The key question is whether we will see an investment gap in part 2 after previous exposure to a certain feedback institution. If brackets are completely malleable or if individuals have Kőszegi–Rabin preferences we should observe the usual investment gap both after exposure to High and after exposure to Low in part 1. If subjects learn to bracket broadly, the investment gap should not occur anymore after previous exposure to Low, but still be there after previous exposure to High. Finally, if subjects stick to their initial way of decision
bracketing, there will be no investment gap in part 2, neither after previous exposure to *High* nor after previous exposure to *Low*.

### 3 Results

#### 3.1 Behavior in Part 1

Before we analyze the effects of changes in feedback institutions, we confirm that our data for part 1 replicate previous results. In addition, we check the validity of our control treatments by confirming that there is no difference in part 1 between those treatments which employ the same feedback institution in part 1, i.e., between each change treatment and its respective control treatment.

Panel A of Figure 1 shows investment in part 1 depending on the feedback institution. In line with the literature, we find the typical gap in investment between those participants who play *High* in part 1 (*HighHigh* and *HighLow*) and those who play *Low* (*LowHigh* and *LowLow*). While the former invest 41.3, the latter invest 51.9 (*U*-test *p* = 0.04).\(^6\) Also in line with previous findings, men invest more than women (*U*-test *p* < 0.01).\(^7\)

Figure 2 plots investment into the lottery for the different treatments over time. To make the data comparable across treatments, investment is aggregated into blocks of three periods. Since we want to study the effect of changes in feedback institutions against the background of control treatments, in which institutions do not change, we check that randomization into treatments worked by showing that behavior in those treatments which employ the same feedback institution in part 1 does not differ. *U*-tests confirm the impression from Figure 2 that there are no significant differences in behavior in part 1 between *HighHigh* and *HighLow* (*U*-test

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\(^6\) All *p*-values reported in this paper are for two-sided tests.

\(^7\) See Charness and Gneezy (2012) for an overview of similar findings. However, see also van der Heijden et al. (2012), who do not find a gender difference.
Figure 1. Investment in part 1 and part 2. Panel A shows investment in part 1 by feedback institution. High pools data from HighHigh and HighLow, Low pools data from LowHigh and LowLow. Panel B shows investment in part 2 for the change treatments LowHigh (left bar) and HighLow (right bar).
$p = 0.98$), and between LowHigh and LowLow (U-test $p = 0.62$). In addition, Figure 2 shows an increase in investment over time in part 1 in both High and Low, which has also been documented, e.g., in Moher and Koehler (2010). Random-effects regressions of investment in part 1 on a time variable (round in High, block in Low) confirm that this time trend is significant in both treatments (Columns 1 and 4 in Table A1 in the appendix). \(^8\)

### 3.2 Behavior in Part 2: The Effects of a Change in Feedback Institutions

Panel B of Figure 1 shows investment in part 2 for the two change treatments. If choice bracketing is completely malleable, investment under a certain feedback institution should be independent from previous exposure to other feedback institutions. In this case, the bars of the same color in panel A and panel B of Figure 1 should have the same height. The figure reveals that this is clearly not the case. Instead we see an asymmetric effect. While investment in part 2 of HighLow is similar to the investment in part 1 of those who were exposed to Low (black bars), subjects in part 2 of LowHigh do not seem to decrease their investment to the level observed in High in part 1 (grey bars).

Figure 2 presents our main result in more detail. Most importantly, investment in LowHigh does not decrease in part 2 but stays at the level observed in part 1 (52.7 vs. 55.4, Wilcoxon signed rank test $p = 0.77$). There is no statistically significant difference in investment in part 2 between LowHigh and the control treatment

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\(^8\)The time trend seems to be more pronounced in the Low treatments. In principle, a simple model of directional learning in which subjects increase their investment after a gain and reduce it after a loss could explain an increasing difference between the treatments, since it is more likely to observe a loss in a single lottery than in the aggregation of three lotteries. However, the data do not support this explanation. In columns 2–3 and 5–6 of Table A1, we include a dummy variable for a loss in the previous round (block). In contrast to the directional learning model, we find that, similar to the gambler’s fallacy, subjects do not decrease, but increase their investment after a loss in High. In Low we do not observe a significant reaction to observing a loss in the previous round.
Figure 2. Investment in part 1 and part 2 over time

LowLow (55.4 vs. 57.9, U-test $p = 0.87$). The usual investment gap thus vanishes completely after previous exposure to Low. This is remarkable in view of the robustness of the investment gap observed in the literature. Subjects in HighLow, on the other hand, increase their investment from 40.8 in part 1 to 53.9 in part 2 (Wilcoxon test $p < 0.01$). While this increase in investment could in principle be a continuation of the time trend observed in part 1, a comparison with the control treatment HighHigh shows that this is not the case. Subjects in HighHigh do not increase, but in fact reduce their investment in part 2, albeit not significantly so (41.8 vs. 33.1, Wilcoxon test $p = 0.18$). Overall, we still see an investment gap in part 2 after exposure to High in part 1 (33.1 vs. 53.9, U-test $p < 0.01$).
Figure 2 also shows that our main result can already be observed in the first block of part 2 (periods 10–12). After the change in feedback institution, subjects in $HighLow$ jump to about the level observed in the first block (1–3) of $Low$ in part 1 (U-test $p = 0.36$). The difference between $HighHigh$ and $HighLow$ is already (weakly) significant in the first block of part 2 ($p = 0.07$). On the other hand, the difference in the first block of part 2 between $HighLow$ and $LowLow$, i.e., those subjects who already played $Low$ in part 1, is not significant ($p = 0.80$).

Taken together, we find an asymmetric effect of changes in feedback institutions: A switch to less frequent feedback increases investment, but a switch to more frequent feedback does not reduce it. This implies that the impact of a given frame differs vastly depending on which frame an individual has previously been exposed to. In particular, $High$ does not seem to induce narrow bracketing after previous exposure to $Low$. Choice brackets are thus not completely malleable in every direction.

Our findings, which are not in line with a preference based explanation of the investment gap, support an interpretation of narrow bracketing as a mistake. In addition, they suggest that some form of learning is the mechanism which limits the malleability of choice brackets. Initially, many subjects seem to passively accept the given description of the decision problem. However, the asymmetric effect suggests that they “correct” their mistake ($HighLow$), or do not fall for it ($LowHigh$), once they have been exposed to a low frequency of feedback. Subjects seem to learn to integrate over gambles when guided by the right feedback institution. Interestingly, they do not seem to be able to figure this out from first principles or the experience of others: In Fellner and Sutter (2009), simply telling subjects that previous subjects in $Low$ made more money than those in $High$ did not induce subjects to
predominantly choose a low frequency of feedback when given the choice, nor did it increase investment of those subjects that chose a high frequency of feedback.\footnote{These findings are reminiscent of the literature on experience-based choice (Rakow and Newell 2010) which finds that individuals’ decisions differ as a function of whether probabilities are given or are experienced via observation and feedback.}

### 3.3 Individual Characteristics and Feedback Institutions

In a final step of our analysis, we turn to the data on individual characteristics. This data enables us to address two questions. First, who is most vulnerable to the investment gap in a static setup? The answer to this question can also provide further evidence for the mistake interpretation of our data given that this interpretation makes specific predictions regarding the size of the investment gap for certain individuals. Second, we ask whose brackets are most malleable, i.e., which individuals adjust their behavior most in response to a reduction in feedback frequency.

Kahneman (2003) proposes that framing effects do not arise because individuals reason poorly, but because they act intuitively. This implies that there should be systematic differences in narrow bracketing across individuals. More impulsive individuals should be more likely to accept the given framing of a decision problem without thinking. As a consequence, the investment gap in part 1 should be bigger for these individuals. Kahneman (2003) also conjectures that in situations in which the intuitively attractive action is in conflict with a rule that an individual would endorse, such as integrating over different gambles, this rule will more likely come to mind and override intuition in high cognitive skill individuals. Read et al. (1999) also suggest a negative correlation between cognitive skills and narrow bracketing. In our setup, this would imply a bigger investment gap for individuals with lower cognitive skills.\footnote{In line with this argument, Abeler and Marklein (2010) find that framing leads to stronger violations of fungibility of money for subjects with lower mathematical abilities. In the Gneezy and Potters (1997) myopic loss aversion paradigm, the influence of impulsiveness and cognitive ability}
To check whether the investment gap between High and Low is especially pronounced for impulsive individuals, we employ a direct measure of impulsiveness in the post-experimental questionnaire, the widely used Barratt Impulsiveness Scale (BIS-11, Patton et al. 1995). Regarding cognitive ability, we use the same proxy as Abeler and Marklein (2010), i.e., subjects’ math grade in their final high school exam. Table 2 shows a regression of average investment in part 1 on a treatment dummy “low”, a dummy “impulsive” for above median impulsiveness, and the interaction of the two (analogous for cognitive skills using a dummy “low_math” for below median cognitive skills).\(^{11}\) In this specification, the constant is the mean investment in the high frequency treatment for non-impulsive subjects. “Low” is the treatment effect for these individuals, while the interaction term states the difference in treatment effects between impulsive and less impulsive individuals.

Column 1 of the table reveals that the treatment effect is not significant for less impulsive individuals \((p = 0.85)\). However, the treatment effect for impulsive individuals \((= \text{low} + (\text{low} \times \text{impulsive}))\) is significant \((F\text{-test}, p < 0.01)\). In addition, the weakly significant interaction term \((p = 0.05)\) indicates that impulsive individuals are affected more strongly by the treatment manipulation. The difference in investment gaps for impulsive and non-impulsive individuals is almost 18 ECU. A less clear picture emerges for cognitive skills (column 2). Again, the treatment difference is not significant for individuals with high cognitive skills \((p = 0.37)\), but it is

\(^{11}\)We use the median split for ease of interpretation. Using the impulsiveness or math grade variable instead of the dummies produces similar or even stronger results. The same holds if we use the motor impulsiveness subscale of the BIS-11, which measures the tendency to act without thinking, instead of overall impulsiveness.
Table 2. Investment regressions for part 1. Impulsive and low_math are dummy variables created by median split. Standard errors are reported in parentheses.

<table>
<thead>
<tr>
<th>Dependent variable</th>
<th>(1)</th>
<th>(2)</th>
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<tbody>
<tr>
<td>constant</td>
<td>40.500***</td>
<td>43.331***</td>
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<tr>
<td></td>
<td>(4.39)</td>
<td>(4.60)</td>
</tr>
<tr>
<td>low</td>
<td>1.202</td>
<td>6.039</td>
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<tr>
<td></td>
<td>(6.45)</td>
<td>(6.73)</td>
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<td>impulsive</td>
<td>1.638</td>
<td></td>
</tr>
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<td></td>
<td>(6.49)</td>
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<td>low × impulsive</td>
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<td>low_math</td>
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</tr>
<tr>
<td>low × low_math</td>
<td></td>
<td>9.190</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(9.40)</td>
</tr>
<tr>
<td>Adjusted $R^2$</td>
<td>0.067</td>
<td>0.038</td>
</tr>
<tr>
<td>$N$</td>
<td>162</td>
<td>162</td>
</tr>
</tbody>
</table>

for those with low cognitive skills ($F$-test, $p = 0.02$). The difference in treatment effects is sizable and has the expected sign but fails to reach significance ($p = 0.33$). Overall, the analysis of behavior in part 1 suggests that there is systematic heterogeneity in the treatment effect in the conjectured direction.

In contrast to previous studies, our subjects were exposed to different feedback institutions over time. In a second step, we therefore also study whether the individual malleability of bracketing can be related to impulsiveness or cognitive ability.\textsuperscript{12} We have shown in section 3.2 that on average subjects increase their investment after a reduction of feedback frequency ($HighLow$). This increase should be particularly pronounced for impulsive individuals, i.e., those most likely to passively

\textsuperscript{12}Note that the power of this within-subject analysis is lower given that, in contrast to the cross-sectional analyses above ($N = 162$), we study only the $HighLow$ treatment ($N = 43$).
accept the given formulation of a decision problem. Using a median split for impulsiveness, we find that more impulsive individuals on average increase their investment by 17.7 in part 2, whereas less impulsive individuals only go up by 8.8. The difference is not statistically significant ($U$-test $p = 0.21$), but the numbers are sizable. For cognitive skills, we find an unexpected effect. Individuals with high cognitive skills react much stronger to a reduction in feedback frequency. They increase their average investment by 18.4, while individuals with low cognitive skills only raise theirs by 7.6. The difference is weakly significant ($U$-test, $p = 0.08$). One interpretation of this finding is that high cognitive skill individuals are better at learning to integrate over gambles when the formulation of the decision problem is conducive to such learning.

4 Discussion and Conclusion

The way individuals bracket their choices is an important determinant of behavior. Understanding how the decision environment influences how individuals form their choice brackets is therefore not only of great theoretical interest, but also important from a practical perspective. In this paper, we studied how malleable choice bracketing is in the context of investment decisions. We introduced a dynamic design and studied the effects of changes in feedback institutions on investment behavior. Our main finding is an asymmetric effect of such changes: Less frequent feedback increases investment, but more frequent feedback does not reduce it. As a result the investment gap between High and Low completely vanishes after previous exposure to a low frequency of feedback.

There are two main implications of this finding. First, the asymmetry of the effect supports the interpretation that the investment gap observed in static environments is really a mistake due to narrow bracketing rather than the outcome of preferences as suggested by Kőszegi and Rabin (2009). In line with this interpreta-
tion, we also find that the investment gap in part 1 of our experiment is especially pronounced for impulsive individuals. Second, our main result suggests that there are limits to the malleability of choice bracketing. In our setup, a narrow choice bracket can be broadened, but a broad choice bracket cannot be narrowed down again. Subjects seem to learn to integrate over gambles when guided by the “right” feedback institution and this learning cannot be reversed by subsequent exposure to a high frequency of feedback. This is good news from a policy perspective in a double way: the frequently suggested intervention of reducing feedback frequency works \((\text{HighLow})\), and previous exposure to the “right” feedback institution seems to have a lasting effect on investment \((\text{LowHigh})\). In this respect we complement previously suggested “cures” for myopic loss aversion such as exposing subjects to a low frequency of feedback as the default and imposing costs for switching to a high frequency of feedback (Fellner and Sutter 2009), or integrating choices for subjects by providing them with the explicit distribution of potential final outcomes (Benartzi and Thaler 1999).

Our findings also imply an important caveat for the literature on framing effects. Framing effects are usually established in between-subject designs. Typically these experiments demonstrate that giving one group a certain description of a decision problem, and a different description of the same decision problem to another group leads to different decisions. Our findings show that these between-subject findings need not hold when the same person is exposed to different formulations of the decision problem over time, i.e., in a within-person setup.

An open question for future research concerns in how far our findings extend to other situations in which narrow bracketing plays an important role. We speculate that an asymmetric effect will also appear in other settings in which there is a “rational” solution which can be learned by exposure to a certain formulation of the decision problem. A good example is the experiment in Abeler and Marklein
(2010) where labels attached to certain parts of their budget (“housing subsidy”) induce subjects to violate fungibility of money. Our simple experimental strategy can be readily applied to test the malleability of bracketing over time in this and other related setups.
References


## Appendix

### Figure A1. Feedback screen **High**

<table>
<thead>
<tr>
<th>Description</th>
<th>Value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Your amount (X) (i.e., the amount of Taler you have invested in the lottery in this round)</td>
<td>77</td>
</tr>
<tr>
<td>Random number in this round</td>
<td>1</td>
</tr>
<tr>
<td>You have won</td>
<td></td>
</tr>
<tr>
<td>Your earnings in this round</td>
<td>291</td>
</tr>
<tr>
<td>Your total earnings (in Taler) in all rounds up to now</td>
<td>291</td>
</tr>
</tbody>
</table>

### Figure A2. Feedback screen **Low**

<table>
<thead>
<tr>
<th>Description</th>
<th>Value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Your amount (X) (i.e., the amount of Taler you have invested in the lottery in each of the previous 3 rounds)</td>
<td>77</td>
</tr>
<tr>
<td>Random number in round 1</td>
<td>3</td>
</tr>
<tr>
<td>You have not won</td>
<td></td>
</tr>
<tr>
<td>Random number in round 2</td>
<td>2</td>
</tr>
<tr>
<td>You have not won</td>
<td></td>
</tr>
<tr>
<td>Random number in round 3</td>
<td>1</td>
</tr>
<tr>
<td>You have won</td>
<td></td>
</tr>
<tr>
<td>Your earnings from the previous 3 rounds</td>
<td>339</td>
</tr>
<tr>
<td>Your total earnings (in Taler) in all rounds up to now</td>
<td>339</td>
</tr>
<tr>
<td>Dep. var.</td>
<td>(1) Investment High</td>
</tr>
<tr>
<td>-----------</td>
<td>---------------------</td>
</tr>
<tr>
<td>period</td>
<td>1.015*** (0.36)</td>
</tr>
<tr>
<td>loss in previous</td>
<td>12.427*** (2.19)</td>
</tr>
<tr>
<td>constant</td>
<td>36.175*** (3.55)</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.005</td>
</tr>
<tr>
<td>$N$</td>
<td>729</td>
</tr>
</tbody>
</table>

Table A1. Time trend and effect of a loss in the previous round on investment for the High and the Low treatment in part 1. The data are pooled over HighHigh and HighLow, and over LowLow and LowHigh. The variable period refers to individual rounds in the High treatment and to blocks of 3 rounds in the Low treatment.
Appendix Instructions for *LowHigh*

In what follows, we present a translation of the instructions for players in the *LowHigh* treatment. Note that subjects received the instructions for part 2 after they had completed part 1.

**Instructions**

Thank you for your participation in this experiment. Please do not talk to any other participants of this experiment from now on!

The money you earn during this experiment, in addition to the show-up fee of 2 euro, will be paid out to you in cash at the end of the experiment. All your decisions within this experiment are anonymous. Today’s experiment consists of two parts. **The two parts are independent of each other**, i. e., your decisions in one part do not affect your chances and opportunities in the other part.

Now please read the instructions for the first part carefully. You will be provided with detailed instructions for the second part at the beginning of the second part.

**Part 1**

This part consists of a total of *nine rounds*. In each round, you are endowed with 100 Taler (100 Taler=50 Euro-Cent). You decide how many Taler (denoted by X) of your endowment (between 0 and 100 Taler) you would like to invest in each round in the following lottery:

If you decide to invest X Taler in the lottery, then
• with probability \( \frac{2}{3} \) (66.67%) you lose the X Taler you spent on the lottery, and, at the end of the round, will make a profit \( = 100 - X \).

• with probability \( \frac{1}{3} \) (33.33%) you gain the 2.5-fold of the X Taler invested in the lottery. You then make a profit \( = 100 + 2.5 \times X \).

The result of the lottery depends on a random number \( Z \), which is drawn anew each round. The random variable \( Z \) can have the values 1, 2, and 3; each value is drawn with the same probability.

You are a type 1-player, i. e.:

You **win** if the random variable is \( Z = 1 \).

You **lose** if the random variable is \( Z = 2 \) or \( Z = 3 \).

Hence, the probability of a profit of \( 2.5 \times X \) is \( \frac{1}{3} \) (33.33%). With probability \( \frac{2}{3} \) (66.67%) you lose X Taler.

**Determination of X for three rounds each**

At the beginning of the first, the fourth, and the seventh round, you decide the amount \( X \) you would like to invest in the lottery in each of the next three rounds (i. e. in rounds 1-3, 4-6, and 7-9, respectively). While the random variable \( Z \) is determined anew each round, you choose the same amount \( X \) for three rounds. After you have entered your decision on the amount \( X \), you will be told on the next screen the random numbers of the three rounds, and how many times you have won and lost in these three rounds. This determines the sum of your profits of each round. The total previous profit from earlier rounds will also be displayed on the screen. The profits of all rounds are added up for the final payout.

Please note that the profits of previous rounds cannot be reinvested in the lottery in the current round; i. e., in each round, the amount \( X \) you can invest in the lottery is limited to 100 Taler.
Instructions for Part 2

The second part also consists of a total of nine rounds. In each round, you are endowed with 100 Taler (100 Taler = 50 Euro-Cent). You decide how many Taler (denoted by X) of your endowment (between 0 and 100 Taler) you would like to invest in each round in the following lottery:

If you decide to invest X Taler in the lottery, then

- with probability \( \frac{2}{3} \) (66.67%) you lose the X Taler you spent on the lottery, and, at the end of the round, will make a profit = 100 – X.

- with probability \( \frac{1}{3} \) (33.33%) you gain the 2.5-fold of the X Taler invested in the lottery. You then make a profit = 100 + 2.5 \times X.

The result of the lottery depends on a random number Z, which is drawn anew each round. The random variable Z can have the values 1, 2, and 3; each value is drawn with the same probability.

You are a type 1-player, i.e.:

You win if the random variable is \( Z = 1 \).

You lose if the random variable is \( Z = 2 \) or \( Z = 3 \).

Hence, the probability of a profit of 2.5 \times X is \( \frac{1}{3} \) (33.33%). With probability \( \frac{2}{3} \) (66.67%) you lose X Taler.

After you have entered your decision on the amount X, you will be told on the next screen the random number for the respective round, and whether you have won or lost. This yields the profit of the respective round. The total previous profit from earlier rounds will also be displayed on the screen. The profits of all rounds are added up for the final payout.
Please bear in mind, that the profits of previous rounds cannot be reinvested in the lottery in the current round; i.e. in each round, the amount $X$ you can invest in the lottery is limited to 100 Taler.