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ABSTRACT

This paper presents an experiment on the effect of retroactive price-reduction schemes on buyers' repeated purchase decisions. Such schemes promise buyers a reduced price for all units that are bought in a certain time frame if the total quantity that is purchased passes a given threshold. This study finds a loyalty-enhancing effect of retroactive price-reduction schemes only if the buyers ex-ante expected that entering into the scheme would maximize their monetary gain, but later learn that they should leave the scheme. Furthermore, the effect crucially hinges on the framing of the price reduction.

Keywords: rebate and discount, buyer behavior, risk aversion, loss aversion, regulation of dominant firms, experiment
JEL Codes: C91, D03, D81, L42

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1 Introduction

This paper studies price-reduction schemes that condition on quantity. More specifically, it considers *retroactive* price-reduction schemes, where the price reduction is granted only after a certain absolute or relative quantity threshold has been reached but then the reduced price is also applied to all units below the threshold.[1] Such retroactive price reductions are frequently used in business-to-business relationships, which implies that the buyer often is a retailer.[2]

Retroactive price reductions are often said to induce buyer loyalty. The economic reason is a so-called "suction effect:" Once a buyer starts ordering some units, he will not buy from alternative sellers anymore because close to the quantity threshold the marginal price for the remaining units up to the threshold is very low in comparison to the alternative offers.

On top of such rational drawing power, everyday life provides us with a considerable evidence that price-reduction schemes also *behaviorally* influence buyers: As an example, think of the members of frequent flier programs[3] or the use of point cards that are stamped when purchasing a cup of coffee. Professional retailers may tend to such judgmental biases[4] too – in particular, if they own a relatively small business unit such as, for example, a travel agency.[5]

This paper presents results from an experiment that is designed to address the research question of whether buyers have a behavioral tendency towards participating in retroactive price-reduction schemes, even if these schemes do not maximize their expected profit. This question has implications for market entry or exclusion models in economic theory, because

[1]The European Commission (2009) sometimes names such schemes "conditional" rebates. Also the terms "loyalty," "all-units," or "cliff" discount are frequently used in similar contexts – see, e.g., Salinger (2017).

[2]For lists of examples, where such schemes are used – both in business-to-business relationships as well as in market interactions with final consumers – see Genchev and Mortimer (2017) or Morell et al. (2009). Steuer (2017) discusses the different effects of various conditional pricing schemes on retailers and final consumers.

[3]A nice description of the behavior of a frequent flier addict is given in "The time is short, so he's gotta fly," *Los Angeles Times*, December 27, 2008. Carlsson and Loefgren (2006) attribute such behavior to habit formation.

[4]For an overview of biases from standard economic predictions in human behavior, see, e.g., Rabin (1998).

[5]*British Airways* (ECJ, Case C-95/04) used a retroactive price-reduction scheme when selling airline tickets via travel agencies.

buyers form the "other side" of the market, which is often neglected in formal models that represent their decisions in some standard demand function. A loyalty-enhancing effect of retroactive price-reduction schemes may serve the incumbent firm as an additional instrument to exclude (potential) rivals from competition.

The experiment in this paper studies three repetitions of a decision problem with 20 rounds in which participants in the role of buyers decide whether to buy a fictitious product either from a large incumbent firm ("seller 1") or from competing firms in the market (which are represented by "seller 2"). Seller 1 offers a retroactive price-reduction scheme. Seller 2 represents the spot market, where the product is offered randomly at a high or at a very low price. Treatments vary the framing of the price-reduction scheme as an immediate discount or a final refund. Another treatment variation determines whether the pricing scheme of seller 1 or the price offer of seller 2 maximize the expected profits of the buyer.

The experiment reveals two main findings: First, retroactive price-reduction schemes bias behavior toward the incumbent (seller 1) only when the buyer initially believes that buying from seller 1 is optimal but subsequent events cause buying from seller 2 to become optimal. Second, this effect is stronger if seller 1's price-reduction scheme is framed as a discount. Loss aversion can explain this result.

2 Related Court Decisions and Literature

The potential exclusionary effect of retroactive price reductions is the reason why antitrust law in the European Union considers them unlawful if applied by dominant firms. In 2009, *Intel*[6] was fined 1.06 billion euros for using such conditional price-reduction schemes (and also direct payments) to prevent computer manufacturers from buying its competitors' central processing units. In the case of *Michelin*[7] selling truck tires using sophisticated bonus schemes

[6]EC, Case COMP/C-3/37.990

[7]CFI, Case T-203/01

or *Tomra*,[8] a company that produced reverse vending machines, the European Commission argued that retroactive price-reduction schemes that include an absolute or relative quantity threshold qualify as unlawful loyalty rebates, which artificially increase the costs of choosing an alternative seller. In the *British Airways* case (ECJ, Case C-95/04), the European Court took the position that a predatory intent is sufficient to consider bonus schemes unlawful, irrespective of their final effect.

US jurisdictions instead tend to presume that price reductions – including retroactive schemes – are generally desirable because they are good for competition and, thus, for customers (see *Concord Boat v. Brunswick*[9] or *Virgin Atlantic Airways v. British Airways*[10]). As long as prices are not predatory – i.e., below marginal costs – they are generally not seen as anti-competitive (see also *Eisai v. Sanofi-Aventis*[11]). However, in *LePage's v. 3M*,[12] similar bundled price-reduction schemes for adhesive tape were seen as unlawful.

The discussion of courts about the treatment of retroactive price-reduction schemes with respect to their exclusionary effect has been taken up in the academic literature. Zenger (2012) supports the view that these schemes' occurrence generally indicates an active competitive process. Laskowska (2016) also supports the view of US jurisdictions that loyalty discounts are generally legal. Jacobson and Weick (2017) defend a similar position but emphasize that the competitors' ability to mitigate the exclusionary effect of any potentially exclusive pricing scheme has to be taken into account, too. Crane (2015) provides an overview of this debate in the US.

However, most authors argue in favor of some structured and economically grounded case-by-case analysis (Faella, 2008; Geradin, 2015; Durkin, 2017; Salinger, 2017; Fumagalli and Motta, 2017). These studies also provide comprehensive overviews of the antitrust assessment

[8] ECJ, Case C-549/10

[9] *Concord Boat Corp.* v. *Brunswick Corp.*, 207 F.3d 1039, 1061 (8th Cir. 2000)

[10] *Virgin Atlantic Airways Ltd.* v. *British Airways PLC*, 257 F.3d 256, 265 (2nd Cir. 2001)

[11] *Eisai, Inc.* v. *Sanofi Aventis U.S., LLC*, 821 F.3d 394, 409 (3d Cir. 2016)

[12] *LePage's Inc.* v. *3M*, 324 F. 3d 141 (3d Cir. 2003)

of several variants of price-reduction schemes. Maier-Rigaud (2005, 2006) provides an early theoretical framework to compute exclusionary effects. Some general criteria supporting a structured judgment are developed by Klein and Lerner (2016).

Another strand of the literature compares different variants of loyalty contracts with respect to their potential exclusionary effect. Feess and Wohlschlegel (2010) show in a two-stage model that retroactive all-unit discounts can lead to stronger efficiency distortions than exclusive dealing contracts. In the differentiated products model of Calzolari and Denicolò (2013), the dividing line is between entirely exclusive contracts and market-share discounts. Elhauge and Wickelgren (2015) particularly emphasize the importance of a precise distinction between loyalty contracts with and without buyer commitment by showing that the former type is far more likely to have severe anti-competitive effects.

The experimental literature so far includes only a few approaches to analyze the effect of price-reduction schemes on buyer behavior. Beckenkamp and Maier-Rigaud (2006) induce a suction effect by placing buyers in a situation in which they have already bought a predetermined quantity from the incumbent that is close to the threshold. This essentially makes the situation in the experiment a one-shot decision.

Morell et al. (2009) study buyers' decisions between entering and staying in a retroactive price-reduction scheme and a safe outside option in a neutrally framed setup with ten rounds. A suction effect comes into play by having two random draws in rounds 5 and 10, which potentially cause these rounds to be omitted. As sales in nine rounds are required to reach the quantity threshold for receiving the price reduction, canceling round 5 makes a switch from the price-reduction scheme to the outside option optimal for rational and risk-neutral buyers. They find that a substantial share of buyers keep trying to reach the price-reduction scheme's quantity threshold even when it becomes suboptimal during the process of repeated decisions.

The present study goes beyond those of Beckenkamp and Maier-Rigaud (2006) and Morell et al. (2009) in several aspects. Most importantly, this is the first study that can compare the behavioral drawing power of a price-reduction scheme to the stickiness that is induced by the initial plan *not* to enter the price-reduction scheme. It can thus disentangle a suction effect of the price-reduction scheme from mere inertia in a series of purchasing decisions.

Loosely related to the experiment in the current paper are the experimental studies by Normann et al. (2007) on price reductions under different marginal costs of the producer, by Davis and Millner (2005) on the effect of different price-reduction schemes on demand, and by Davis and Holt (1994, 1998) on secret price reductions in posted-offer markets and collusion. They all are concerned with the effects of price-reduction schemes on behavior, but do not specifically focus on the suction effect. Buyers' decisions as to whether to try to reach a quantity threshold depend on their risk attitude (see, for example, Holt and Laury, 2002; Dohmen et al., 2011). For risk-averse buyers, the price reduction becomes increasingly attractive with the increasing variance of alternative offers.

In the dynamic framework of our experiment, risk preferences further potentially interfere with time preferences; this is a topic that is dealt with in the studies of Andersen et al. (2008) and Anderhub et al. (2001). Finally, the experiment relates to experimental studies on stochastic demand and the newsvendor problem (Schweitzer and Cachon, 2000; Benzion et al., 2008; Bolton and Katok, 2008; Ockenfels and Selten, 2014). Though not considering price reductions at all, the decision problems are related as the newsvendor also faces uncertainty (arising from demand) about whether he will reach a certain sales level.

3 Design and Procedures

In each round, the buyer B buys a fictitious product from one of two computerized sellers and automatically resells it at a fixed price of $v_B = 60$ in the end of a round. Thus, the buyer is the only real player, which is known to the participants. The buyer always buys 10 units per round. He can decide only whether to order these units from seller 1 (the incumbent) or from a competing firm (seller 2).[13]

The experiment lasts three times 20 rounds, where a chain of 20 rounds will be called a

[13] In the experimental instructions, the role of the buyer was framed as the one of a retailer that is buying the product from one of two sellers and reselling it to final consumers. This reflects that retroactive price-reduction schemes mainly occur in business-to-business relationships.

"trade phase." Given the trade volume of 10 units per round, the total demand per trade phase amounts to 200 units.

Seller 1 supplies the good in each round and offers the following retroactive price-reduction scheme: $p_1^{reduced} = 50$ if $Q_1 \geq 180$: if the total quantity that is bought within a trade phase amounts to at least 180 units. Otherwise, $p_1 = 60$. Seller 2 represents the spot market, where the buyer may also get the product, but with more variation in prices. Prices of seller 2 are randomly determined in each round as follows: With probability $\alpha = 0.4$, the price is low, with $p_2 = p_2^{low} \in \{25, 35\}$, depending on the treatment. With the remaining probability $1 - \alpha = 0.6$, $p_2 = p_2^{high} = 60$. The buyer knows the probability α and is always informed about the prices in the current round before deciding where to buy.

There are two reasonable strategies for the buyer in our setup: The first strategy ("strategy 1") ensures that the buyer reaches the quantity threshold and receives the price reduction of seller 1. As the threshold is at 180 units, the buyer maximizes profit by buying exactly these 180 units from seller 1 and the remaining 20 units from seller 2 – ideally in the two rounds when the low price of seller 2 first becomes available.

The second strategy ("strategy 2") is adopted by a buyer who buys from seller 2 whenever p_2^{low} is offered. As the high price of seller 2 equals the non-reduced price of seller 1, this strategy is indifferent as to where to buy when the price of seller 2 is high. However, before buying more than two units from seller 2, it is weakly dominant to buy from seller 1 in such high-price rounds, because there is a small probability that seller 2 will not offer the cheap price more than twice.

Exogenous treatment variations occur at two instances: First, the experiment varies the frame of the price reduction as Discount or Refund. In the Refund frame, the price paid to seller 1 is 60 with a refund of 10 per unit if the threshold of 180 units is reached. In the Discount frame, the price of seller 1 is 50 and the price retroactively increases to 60 should the threshold level not be reached.

Second, there is the variation in the price of seller 2, $p_2^{low} \in \{25, 35\}$. The calibration of the parameters in the experiment is such that ex-ante strategy 1 maximizes the expected profit within a trade phase in treatments with $p_2^{low} = 35$, while strategy 2 is profit-maximizing when $p_2^{low} = 25$. Ex-ante, the expected profit of strategy 1 is[14]

$$E[\pi|1] = 10 \cdot (2 \cdot (v_B - p_2^{low}) + 18 \cdot (v_B - p_1^{reduced})),$$

and the expected profit of strategy 2 is

$$E[\pi|2] = 10 \cdot (20 \cdot \alpha \cdot (v_B - p_2^{low})).$$

For $p_2^{low} = 35$, we obtain $E[\pi|1] = 2300$ and $E[\pi|2] = 2000$, so that strategy 1 on expectation yields a 300 points higher profit. For $p_2^{low} = 25$, $E[\pi|1] = 2500$ and $E[\pi|2] = 2800$, so that now strategy 2 yields a 300 points higher expected profit. Choosing the wrong strategy therefore implies an expect payoff loss of 11-13 percent. Playing anything else but one of the two main strategies further decreases the buyer's profit.

A third variation evolves endogenously depending on the realizations of the random α. When the realized availability of low price offers by seller 2 differs from their expected frequency during the first few rounds of a trade phase, it can happen that a switch to the other strategy becomes optimal. Recall that the buyer can buy twice from seller 2 while still being eligible for seller 1's price-reduction scheme. This feature allows switching to the other strategy, because the buyer has to decide irrevocably for or against entering into the incumbent's price-reduction scheme only when $p_2 = p_2^{low}$ for the third time. Let us name a strategy, which turns out to be optimal at the point in time, when seller 2 offers the low price for the third time, a "later" optimal strategy.[15]

Depending on the round t, in which seller 2 offers p_2^{low} for the third time, the expected total profit of strategy 2 **at this point in time** is

$$E_t[\pi|2] = 10 \cdot (3 \cdot (v_B - p_2^{low}) + (20 - t) \cdot \alpha \cdot (v_B - p_2^{low})).$$

[14]For simplicity, the calculation ignores the extremely small probability that seller 2 offers p_2^{low} less than twice during the 20 rounds.

[15]Note that this does not necessarily imply that this strategy will finally turn out to be optimal after the price realizations of all 20 rounds are known.

Comparing $E_t[\pi|2]$ with the (almost) safe payoff when playing strategy 1, $E[\pi|1]$, we find that $E_t[\pi|2] > E[\pi|1]$ when $t < 4.5$ for $p_2^{low} = 35$ and $E_t[\pi|2] > E[\pi|1]$ when $t < 9.6$ for $p_2^{low} = 25$. Thus, a switch from strategy 1 to strategy 2 is optimal when $p_2^{low} = 35$ realizes at least three times during the first 4 rounds of a trade phase. Conversely, a switch from strategy 2 to strategy 1 is optimal when $p_2^{low} = 25$ realizes less than three times during the first 9 rounds of a trade phase.

The switchpoints t are relatively robust to assuming constant relative risk aversion of the buyers. Using the CRRA utility function $u(x) = \frac{x^{1-r}}{1-r}$, we can calculate the expected utilities of both strategies. Comparing expected utilities instead of expected profits of both strategies, we find that for reasonable degrees of risk aversion the optimal switchpoints do not change. To obtain other than the above calculated values for the switchpoints t, values of r larger than 0.9 would be required.[16] In the study of Holt and Laury (2002), the choices of only about 5 percent of the participants reveal such a strong risk aversion.[17]

Intuitively, there are two reasons why the switchpoints are so robust to assuming risk aversion: First, there is a lot of probability mass on outcomes that are close to the expected one. Second, payoffs (and, thus, utilities) for both strategies vary only by a relatively small amount if one of these very likely events realizes. The analysis in this paper will therefore use the prediction for risk-neutral up to moderately risk-averse agents as the benchmark.

The two exogenous treatment variations are conducted in a 2x2 design, which results in four different treatment conditions. For each participant in the experiment, the frame as Refund or Discount as well as the value of $p_2^{low} \in \{25, 35\}$ was the same in all three trade phases. Table 1 summarizes the treatments and the number of observations that were collected for each treatment.

In addition to the four main treatments, there are two control treatments for the Refund frame. In these two treatments, the prices of seller 2 are perfectly predictable in the sense that

[16] More precisely, for $r > 1.36$ buyers switch from strategy 2 to strategy 1 already in round 8 if seller 2 offered the low price less than three times so far; for $r > 0.94$ they switch from strategy 1 to strategy 2 only if seller 2 offers the low price three times in the first three rounds.

[17] See also the literature that Holt and Laury (2002, fn. 9) cite.

the low price is available in exactly 8 predetermined but unknown rounds. Thus, the expected frequency of low-price offers of seller 2 is the same as in the main treatments, but there is no variance around the mean in the control treatments.

These two control treatments serve as a benchmark for how well participants are able to find their optimal strategy when risk plays no role for their decision. In general, both a systematic bias or erratic play may explain evidence of suboptimal behavior. To verify that erratic play is not the driver of suboptimal behavior, the control treatments provide a test whether participants are able to make optimal decisions in a setting where no systematic bias should occur.

Frame	p_2^{low}	ex-ante optimal	# observations
Refund	35	Seller 1	80
Refund	25	Seller 2	40
Discount	35	Seller 1	80
Discount	25	Seller 2	40
Refund	35	Seller 1	20
Refund	25	Seller 2	20

Table 1: Treatments and number of observations.

For each of the four main treatments, the data are further separated according to the later optimal strategy. The later optimal strategy differed across trade phases and participants because of the independent random draws of α. Therefore, the number of observations in these two conditions also varies across trade phases, as can be seen in Table 2.

After the main experiment, participants have to make one simple lottery decision. The lottery payoffs differ between treatments and provide a measure of the extent to which the different risk involved in strategies 1 and 2 explains differences in behavior in the specific situation in the experiment. Note first that the variance of the profit of strategy 2 is larger, because ex ante the buyer does not know how often the low price will be offered by seller 2. Therefore, strategy 2 is riskier than strategy 1. To capture the effect of the different riskiness of the two strategies on behavior, the lotteries have the same mean and variance as the expected payoffs in euros of strategies 1 and 2 in the corresponding treatment.

In treatments with $p_2^{low} = 35$, this implies a choice between 2.87 euros with certainty versus

Name	Frame	Optimal strategy* ex-ante	later	# observations Phase 1	Phase 2	Phase 3
R1–1	Refund	Seller 1	Seller 1	66	62	57
R1–2	Refund	Seller 1	Seller 2	14	18	23
R2–1	Refund	Seller 2	Seller 1	6	9	8
R2–2	Refund	Seller 2	Seller 2	34	31	32
D1–1	Discount	Seller 1	Seller 1	71	59	60
D1–2	Discount	Seller 1	Seller 2	9	21	20
D2–1	Discount	Seller 2	Seller 1	15	10	7
D2–2	Discount	Seller 2	Seller 2	25	30	33
R1–f	Refund	Seller 1	Seller 1	20	20	20
R2–f	Refund	Seller 2	Seller 2	20	20	20

Table 2: Treatment conditions and number of observations, separated by the later optimal strategy. The treatment names contain R or D as abbreviation for the Refund or Discount frame. The number before the hyphen indicates the ex-ante optimal strategy; the second number indicates the later optimal strategy. The letter f marks the control treatments with a fixed number of low-price offers by seller 2. *ex-ante, strategy 1 is optimal when $p_2^{low} = 35$ and strategy 2 is optimal when $p_2^{low} = 25$. "later" refers to the optimal strategy at the point in time t when p_2 takes the low value for the third time.

1.83 euros or 3.19 euros with 50% probability each. In treatments with $p_2^{low} = 25$, the lottery choices are 3.31 euros with certainty versus 2.56 euros or 4.45 euros with 50% probability each. As the safe strategy 1 is ex-ante optimal in treatments with $p_2^{low} = 35$, the safe payoff of 2.87 euros in the first lottery is also larger than the expected payoff of the risky choice in the first lottery. In the other lottery, the safe payoff of 3.31 euros corresponds to the expected payoff of strategy 1 in treatments with $p_2^{low} = 25$, which is smaller than the expected payoff of strategy 2 in these treatments.

Instructions for the experiment were shown on screen and included an interactive guided tour through the screens of the experiment. A general calculator box was available while reading the instructions and during the experiment, but no explicit profit calculator was provided. At the end of each round, participants received detailed feedback about the full history of the current trade phase. The experiment was computerized using z-Tree (Fischbacher 2007). A total of 280 students from various disciplines took part in the experiment. They were recruited via ORSEE (Greiner 2015).

The experiment took place in the *Lakelab*, the laboratory for experimental economics at the University of Konstanz, in 2011, and in the *PLEx* at the University of Potsdam, in 2016.[18] The experimental currency was points. 800 points were converted into 1 euro after the experiment. On average, participants earned approximately 12 euros in the experiment, which lasted for about one hour.

The protocol during the experiment was as follows: After welcoming participants and explaining the main rules for participation in the experiment, they were randomly assigned seats in the laboratory. Participants received instructions[19] on their computer screen and were given the possibility to familiarize themselves with the computer screens. Then the experiment started. At the end of a session, they were asked to complete a short questionnaire.

4 Hypotheses

The previous literature on price reductions (Beckenkamp and Maier-Rigaud, 2006; Morell et al. 2009) has shown that retroactive price-reduction schemes exhibit attraction to buyers beyond the rational maximization of expected payoffs. The present study goes beyond previous ones, because it can compare the frequency of suboptimal "reduction seeking" to its counterfactual: a reluctance to enter into the reduction scheme when it would be optimal. Only if the former effect is larger than the latter, then "reduction seeking" would constitute a judgmental bias in itself. If both effects are present, they would instead result from well-known biases such as loss aversion or status quo bias.

Hypothesis 1 *Buyers make more optimal decisions when strategy 1 is optimal than when strategy 2 is optimal.*

Payoff-maximizing buyers in the experiment will determine the expected payoffs of the two strategies before they start making their decisions. Given the result of their calculations,

[18] In Potsdam, half of the data for the main treatments with $p_2^{low} = 35$ was collected. The behavior of the participants at the two different places was very similar.

[19] An English translation of the instructions can be found in the Appendix.

they may develop a preliminary plan to play one of the two above strategies, which forms the reference point. At the point in time when seller 2 offers p_2^{low} for the third time, they compare the possible gains and losses of a strategy switch to this reference point. If buyers in the experiment exhibit a status quo bias (Kahneman et al., 1991), they may switch too infrequently on average.

Prospect theory provides the standard tool to analyze such situations formally. Using the value function $v(x) = x^\alpha$ for $x \geq 0$ and $v(x) = -\lambda(-x)^\alpha$ for $x < 0$ over gains and losses x relative to the reference point, the probability weights $w(P) = P^\delta/(P^\delta + (1-P)^\delta)^{1/\delta}$, and the standard parameter values of Tversky and Kahneman (1992) – $\alpha = 0.88$, $\lambda = 2.25$ and $\delta = 0.65$ – prospect theory predicts that buyers will always stick to their ex-ante optimal strategy. Strategy switches are not predicted in this theoretical framework, neither from strategy 1 to strategy 2 nor vice versa.

Hypothesis 2 *There is more optimal play when the ex-ante and the later optimal strategy coincide than when a strategy switch becomes optimal during a trade phase.*

Furthermore, framing effects may occur: In the Discount frame, buyers pay a price of 50 when buying from seller 1 and immediately see a profit of $(60-50)\cdot 10 = 100$ points per round. In rounds, in which seller 2's price is equal to 60, it seems thus likely that buyers buy from seller 1 who appears to make a better offer. Most buyers will therefore buy some units from seller 1 in such rounds. Once having bought these units, they anticipate they would have to pay back the price reduction to seller 1 if they do not reach the quantity threshold, which now may feel like a loss.

For loss-averse buyers, the Discount frame therefore distorts behavior towards strategy 1. In other words, loss aversion amplifies the price-reduction scheme's attraction in the Discount frame compared to the Refund frame. This effect pushes buyers in the right direction if strategy 1 is optimal anyway, but it is detrimental if strategy 2 would be optimal.

The Refund frame, in contrast, may cause a distortion towards strategy 2. In this frame, buyers see a price of $p_1 = 60$ by seller 1 in all rounds, which is equal to the valuation v_B, so that their profit is zero until they receive the refund in the last round of a trade phase. As previous research has shown that individuals associate delay with uncertainty and that they dislike this uncertainty (see Frederick et al., 2002), the Refund frame tends to make the price-reduction scheme of seller 1 less attractive.

Hypothesis 3 *(i) The* Refund *frame leads to less optimal behavior when strategy 1 is later optimal. (ii) The* Discount *frame leads to less optimal behavior when strategy 2 is later optimal.*

5 Results

Table 3 presents the share of participants playing strategy 1 and strategy 2 in the different treatments in all three trade phases. Before comparing strategy choices across treatments, let us first check whether participants in the experiment understood their task sufficiently well to make meaningful decisions. The two control treatments – R1–f and R2–f – provide a measure for the degree to which erratic play matters.

	1st trade phase		2nd trade phase		3rd trade phase	
Treatment	Strategy 1	Strategy 2	Strategy 1	Strategy 2	Strategy 1	Strategy 2
R1–1	**0.53**	0.36	**0.76**	0.21	**0.70**	0.25
R1–2	0.36	**0.57**	0.39	**0.61**	0.35	**0.61**
R2–1	**0.50**	0.50	**0.78**	0.22	**0.75**	0.25
R2–2	0.15	**0.82**	0.35	**0.65**	0.13	**0.88**
D1–1	**0.80**	0.14	**0.69**	0.22	**0.83**	0.15
D1–2	0.22	**0.44**	0.43	**0.48**	0.85	**0.10**
D2–1	**0.40**	0.47	**0.80**	0.20	**0.86**	0.14
D2–2	0.36	**0.56**	0.37	**0.63**	0.24	**0.70**
R1–f	**0.70**	0.25	**0.80**	0.15	**0.95**	0.05
R2–f	0.15	**0.75**	0.10	**0.90**	0.10	**0.90**

Table 3: Distribution of strategies played by the participants. Optimal play is highlighted in bold. When the numbers for seller 1 and seller 2 do not add up to one, the remaining participants played neither of the two strategies.

The share of buyers playing their optimal strategy in these two control treatments increases from 70% ($p_2^{low} = 35$) and 75% ($p_2^{low} = 25$) in the first trade phase to 95% and 90% in the third trade phase. This implies that participants are well able to calculate the ex-ante optimal strategies – at least with some experience.

Suboptimal behavior in the third trade phase is thus not likely to be explained by errors or confusion. In the first and second trade phase, however, some suboptimal behavior may indeed be attributed to erratic play. The evaluation of the data in the main treatments will therefore put a special focus on the third trade phase.

Behavior with respect to the identification of strategies is very clear, and the share of participants playing one of the two predicted strategy patterns increases over time. In the first trade phase, 257 out of 280 participants across all treatments play one of the two patterns; in the second and third trade phase, this number increases to 270 and 272, respectively.

In the first trade phase, two entries in Table 3 are substantially larger than the others: 82% of the buyers optimally play strategy 2 in treatment R2–2, and 80% optimally play strategy 1 in treatment D1–1. In these two treatments, not only ex-ante and later optimal strategies coincide, but also the framing effects predicted in Hypothesis 3 pushes buyers into the right direction.

In the second trade phase, there is a noticeable increase in optimal play in the two treatments where strategy 1 is optimal only later: R2–1 and D2–1. At the same time, there is a little less optimal play in the R2–2 and D1–1 treatments, as compared to the high initial level reported above.

In the third trade phase, the share of optimal play in the four treatments, in which ex-ante and later optimal strategy coincide, is higher than in the first trade phase. The level of optimal play in these treatments is also relatively close to the corresponding level in the two control treatments. The same holds when the required strategy switch is from strategy 2 to strategy 1. However, when a switch in the opposite direction – towards strategy 2 – would be optimal, the share of optimal play does not improve in R1–2 and *declines* strongly in D1–2.

Thus, from the description of the data, it seems that buyers quickly learn to overcome most biases in this setting, with one exception: the DISCOUNT frame strongly impedes convergence towards optimal behavior, in particular in situations, in which a switch from strategy 1 to strategy 2 would be optimal.

We estimate probit regressions in Table 5 to test to what extent these findings are statistically significant in a multivariable setting. The dependent variable in the regressions is a dummy variable that takes the value 1 if the strategy that is played by the participant in this supergame is later optimal and 0 if it is not. Thus, the regression uses three observations per participant. Standard errors are clustered accordingly. Table 4 identifies the explanatory variables that are used in the regression.

Name	Explanation
Strategy 1 later optimal	1 if strategy 1 is later optimal, 0 otherwise
Switch optimal	1 if a strategy switch is optimal, 0 otherwise
REFUND·(Strategy 1 later optimal)[a]	1 if REFUND frame and strategy 1 later optimal, 0 otherwise
DISCOUNT·(Strategy 2 later optimal)[a]	1 if DISCOUNT frame and strategy 2 later optimal, 0 otherwise
Male	1 if male, 0 if female
(Risk seeking)·(Strategy 1 ex-ante optimal)[b]	1 if strongly risk seeking, 0 otherwise
(Risk averse)·(Strategy 2 ex-ante optimal)[b]	1 if strongly risk averse, 0 otherwise

Table 4: Explanatory variables. [a]"REFUND · (Strategy 1 later optimal)" and "DISCOUNT · (Strategy 2 later optimal)" are coded parallel to the wording of Hypothesis 3 in order to facilitate their interpretation. [b]"(Risk seeking) · (Strategy 1 ex-ante optimal)" and "(Risk averse) · (Strategy 2 ex-ante optimal)" control by how much suboptimal behavior (into both possible directions) is explained by extreme risk attitude. These variables take the value 1 if the participant did not make the expected profit-maximizing choice. They are set to zero for participants who either made the profit-maximizing choice or did not make this lottery decision by design of their treatment.

The regressions do not use the data from the control treatments. By design, these treatments exhibit neither variation in the later optimal strategy, nor should participants' risk attitude play a role here. Thus, a systematic bias towards the price-reduction scheme would not appear in these treatments; and, hence, they are not part of the analysis of treatment differences.

Table 5 presents four variants of the same regression, using slightly different datasets. In the first two columns, all observations from the main treatments are used, including those, where we cannot identify a clear-cut strategy. In order to include such observations, the classification of observed behavior into the two strategies was modified in the following simple way: Whenever a participant reaches the quantity threshold of seller 1 – including observations where the buyer buys more than 180 units from seller 1 – the observation counts as strategy 1;[20] otherwise, the participant is assumed to play strategy 2. The regressions in column three and four exclude these unclear observations.

The regressions in the first and third columns use the specification of later optimal strategies that assumes risk neutrality or moderate risk aversion. For very strong risk aversion, the switchpoints t move forward by one round. A switch from strategy 1 to strategy 2 is optimal when $p_2^{low} = 35$ realizes at least three times during the first three (instead of four) rounds of a trade phase. A switch from strategy 2 to strategy 1 is optimal when $p_2^{low} = 25$ realizes less than three times during the first eight (instead of nine) rounds of a trade phase. The regressions in the second and fourth columns use this alternative specification of later optimal strategies.

The control treatment data suggested that there is a substantial amount of learning across trade phases. To account for such learning in the regression analysis, the regressions interact all variables with dummy variables for the three trade phases. More specifically, there are three dummy variables: one for each of the three trade phases. Each of them takes the value 1 in exactly one trade phase and 0 in the other two trade phases. These dummy variables are interacted with all other explanatory variables, resulting in Table 5 presenting three values for each variable: one for the interaction with trade phase 1, one for the interaction with trade phase 2, and one for the interaction with trade phase 3.

Chow-like F-tests indicate that the influence of the explanatory variables on optimal play indeed varies over time (p-values for all variants of the dataset < 0.05). Where appropriate, the analysis will therefore discuss behavior in the three trade phases separately, with a focus on the last phase, when learning has taken place.

Hypothesis 1 predicted more optimal decisions when strategy 1 is optimal, which would

[20] In all price sequences that occurred during the experiments, seller 2 offered p_2^{low} at least two times during each trade phase.

	Switchpoints for	Including all observations risk neutral	risk averse	Excluding unclear strategies risk neutral	risk averse
Trade phase 1	Strategy 1 later optimal	-0.245	-0.382	-0.181	-0.188
		(0.294)	(0.295)	(0.312)	(0.301)
	Switch optimal	-0.560	-0.402	-0.487	-0.425
		(0.345)	(0.293)	(0.364)	(0.307)
	(Switch optimal) · (Strategy 2 later optimal)	0.193	-1.047***	-0.139	-0.878***
		(0.506)	(0.313)	(0.538)	(0.337)
	Refund · (Strategy 1 later optimal)	-0.632***	-0.554***	-0.735***	-0.697***
		(0.216)	(0.198)	(0.230)	(0.215)
	Discount · (Strategy 2 later optimal)	-0.527*	-0.522	-0.593*	-0.434
		(0.304)	(0.380)	(0.322)	(0.387)
	Male	-0.0703	0.102	-0.0689	0.130
		(0.178)	(0.179)	(0.188)	(0.189)
	(Risk seeking) · (Strategy 1 ex-ante optimal)	0.192	0.140	0.433	0.333
		(0.317)	(0.304)	(0.366)	(0.334)
	(Risk averse) · (Strategy 2 ex-ante optimal)	-0.880**	-0.843**	-0.991***	-0.903**
		(0.359)	(0.360)	(0.378)	(0.378)
	Constant	1.113***	1.102***	1.167***	1.048***
		(0.253)	(0.269)	(0.265)	(0.266)
Trade phase 2	Strategy 1 later optimal	0.203	-0.0270	0.309	0.0941
		(0.281)	(0.282)	(0.290)	(0.288)
	Switch optimal	0.128	0.0880	0.0455	0.0243
		(0.362)	(0.246)	(0.364)	(0.248)
	(Switch optimal) · (Strategy 2 later optimal)	-0.235	-0.776***	-0.220	-0.735***
		(0.473)	(0.240)	(0.477)	(0.249)
	Refund · (Strategy 1 later optimal)	0.160	0.186	0.0515	0.0767
		(0.232)	(0.203)	(0.243)	(0.211)
	Discount · (Strategy 2 later optimal)	-0.0583	-0.147	-0.103	-0.138
		(0.255)	(0.321)	(0.257)	(0.322)
	Male	0.0943	0.0679	0.125	0.113
		(0.172)	(0.171)	(0.177)	(0.176)
	(Risk seeking) · (Strategy 1 ex-ante optimal)	0.0617	-0.0887	0.175	-0.0491
		(0.339)	(0.322)	(0.362)	(0.331)
	(Risk averse) · (Strategy 2 ex-ante optimal)	0.147	0.243	0.157	0.242
		(0.351)	(0.351)	(0.349)	(0.350)
	Constant	0.320	0.490*	0.329	0.473*
		(0.233)	(0.268)	(0.235)	(0.269)
Trade phase 3	Strategy 1 later optimal	-0.489	-0.394	-0.450	-0.283
		(0.354)	(0.344)	(0.362)	(0.344)
	Switch optimal	0.203	-0.332	0.102	-0.408
		(0.446)	(0.258)	(0.445)	(0.259)
	(Switch optimal) · (Strategy 2 later optimal)	-1.469***	-0.897***	-1.452**	-0.899***
		(0.566)	(0.235)	(0.568)	(0.236)
	Refund · (Strategy 1 later optimal)	-0.444*	-0.537**	-0.395	-0.539**
		(0.250)	(0.226)	(0.259)	(0.236)
	Discount · (Strategy 2 later optimal)	-0.902***	-0.883**	-0.970***	-0.882**
		(0.290)	(0.350)	(0.297)	(0.349)
	Male	0.105	0.0777	0.0340	0.0540
		(0.185)	(0.191)	(0.188)	(0.195)
	(Risk seeking) · (Strategy 1 ex-ante optimal)	-0.00316	-0.223	0.199	-0.0698
		(0.358)	(0.345)	(0.411)	(0.386)
	(Risk averse) · (Strategy 2 ex-ante optimal)	-0.136	0.0977	-0.108	0.119
		(0.411)	(0.378)	(0.415)	(0.377)
	Constant	1.409***	1.488***	1.443***	1.459***
		(0.309)	(0.324)	(0.316)	(0.318)
Observations		720	720	683	683

Table 5: Probit regressions that explain optimal play in the four main treatments. Data from the control treatments are excluded. All variables are interacted with the trade phase dummy variable that is named in the first column. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered by participant.

translate into a positive impact of the variable "Strategy 1 later optimal." The data clearly reject this prediction. The variable is insignificant in all four specifications and all three trade phases, in most instances the estimate has a negative rather than the expected positive sign. We thus find no evidence for "reduction seeking" as a judgmental bias on its own.

According to Hypothesis 2, the variable "Switch optimal" should have a negative influence on the frequency of optimal play. This is not the case, it is insignificant in all specifications. However, the interaction term "(Switch optimal) · (Strategy 2 later optimal)" is negative and of substantial size in almost all specifications. Optimal switches out of the price-reduction scheme are less frequent than those into the scheme, which is in line with Hypothesis 2, and also provides some support for a limited part of Hypothesis 1, which in its general form was rejected above.

In the first two trade phases, the negative effect of "(Switch optimal) · (Strategy 2 later optimal)" on optimal play is significant only in the specifications that assume extreme risk aversion for the calculation of optimal switchpoints. In the third trade phase, the detrimental effect of a switch towards strategy 2's being required gets stronger – both in terms of magnitude and statistical significance. This indicates that experience does not improve decisions here, but rather hinders such improvements.

With respect to the framing of the price-reduction scheme as a Refund or as a Discount, the regressions provide evidence that supports both parts of Hypothesis 3: When strategy 1 is later optimal, the Refund frame leads to less optimal behavior. The Discount frame leads to less optimal behavior when strategy 2 is later optimal. Interestingly, these framing effects occur in the first and third trade phases, but not in the second phase.

In the lottery decision, 14 percent of the participants choose the risky option when the safe option would maximize expected profit (i.e. in treatments with $p_2^{low} = 35$, in which strategy 1 is ex-ante optimal). The variable "(Risk seeking) · (Strategy 1 ex-ante optimal)" identifies these participants. When the safe payoff is smaller than the expected payoff of the risky option (i.e., when $p_2^{low} = 25$ and strategy 2 is ex-ante optimal), 21 percent of the participants – those who are identified by "(Risk averse) · (Strategy 2 ex-ante optimal)" – choose it nevertheless. This decision identifies participants with strong risk aversion. The regressions indicate that strong risk aversion contributes to explaining suboptimal play in the first trade phase, but the effect of risk aversion disappears quickly.

6 Discussion and Conclusion

This paper studied an experiment on buyers' decisions to enter into a retroactive price-reduction scheme and to switch between such a scheme and an uncertain outside alternative. A novelty of the current paper is to compare behavior in the situation where it is optimal for the buyers to stay out of the scheme to those where they should enter. Moreover, the design allows decisions when a strategy switch becomes optimal over time.

The experimental results in this paper suggest no behavioral bias towards a retroactive price-reduction scheme per se. However, the price reduction does have such an effect, when the buyers initially believe that entering the scheme would maximize their payoff and later events indicate that they should leave the scheme. Loss aversion amplifies this effect, because such suboptimal behavior is particularly frequent in the DISCOUNT frame, where the price reduction is offered immediately but subject to later payment if the quantity threshold is missed.

The framing effect found in this paper is in contrast to the results in Beckenkamp and Maier-Rigaud (2006), who report no difference in behavior in different frames. A likely explanation for the different results in their paper and in this paper is that their experiment involves a one-shot decision where the behavioral impact of current gains and future losses is less strong than in the longer horizon of 20 rounds in the present study.

The findings contribute to the analysis of retailers' decision-making in their role as a buyer from a manufacturer. The findings suggest that retroactive price-reduction schemes could manipulate retailers' judgment of different available offers, if the price reduction is framed as a discount. With respect to competition law, this implies that the question of whether a price-reduction scheme that is applied by a dominant firm is likely to have an anti-competitive effect could depend on the frame of the price reduction.

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Appendix: Instructions

(These are the instructions for the Refund *frame and for $p_2^{low} = 25$. The text variant for the* Discount *frame is shown in square brackets. In treatments with $p_2^{low} = 35$, this price was displayed.)*

Welcome to the *Lakelab*.

Today you will participate in an experiment. If you read the following instructions carefully, you can earn money. The amount you will get depends on your decisions and on chance, but not on the decisions of other participants.

During the experiment, it is not allowed to communicate with other participants. Therefore, we ask you not to talk with each other. A violation of this rule leads to exclusion from the experiment and from any payment.

Please read these instructions carefully. If you have any questions or if anything is unclear, please raise your hand. We will then come to your place.

After the main part of the experiment, you will participate in a short lottery experiment. You will find the instructions for the second part of the experiment being displayed on your computer screen after the first part.

This experiment consists of 3 times 20 rounds, i.e. after 20 rounds there will be a restart. At the end of the 60 rounds in total, we will add up all your earnings.

During the experiment, we will count your earnings in points. The points you obtain during the experiment will be converted into euros at the following exchange rate: 800 points = 1 euro.

At the end of today's experiment, you will receive your achieved points converted into euros in cash.

In the following, we will describe the procedure of the experiment in detail. First, we will explain the general procedure. After that, you will be given the opportunity to familiarize yourself with the procedure at the computer screen. Before we start the experiment, we will ask you to answer some control questions on screen, which will help you to understand the procedure. The experiment does not start until all participants are familiar with the procedures of the experiment and answered all control questions correctly.

In this experiment you make decisions in the role of a retailer who buys a fictitious product and resells it. In each round, you can buy a predetermined amount of units of the fictitious product from one out of two suppliers and resell them to a buyer. Both the two suppliers and the buyer are not real participants in the experiment, but are simulated by the computer.

The two suppliers have different supply strategies and a different price design:

Supplier 1 offers the product at a price of 60. If you buy at least 180 units in total from supplier 1 during the 20 rounds, you get a rebate of 10 points for each unit you bought from supplier 1. The rebate is paid back at the end of the 20 rounds. This means, you pay a final unit price of 50 to supplier 1 in this case.

[*Text in the* DISCOUNT *frame: Supplier 1 offers the product at a price of 50 points, but only if you buy at least 180 units from supplier 1 during the 20 rounds. If you bought less than 180 units in total after 20 rounds, you retroactively have to pay a unit price of 60 for all units you bought from supplier 1.*]

Supplier 2 offers the product at different prices. In some rounds you can buy the product from supplier 2 at a price of 25 points. In each round, supplier 2 will offer a price of 25 with a probability of 40%. With a probability of 60%, supplier 2 will set a price of 60. You will be informed about supplier 2's current price at the beginning of each round before you make your purchasing decision.

You will resell the units you bought from suppliers 1 and 2 to a buyer in each round. This buyer pays you a unit price of 60 points each. The buyer is ready to buy 10 units of the product for a price of 60.

During the experiment a calculator program of the computer is available to you if you want to calculate the consequences of your purchasing decision in advance. You can open the calculator by clicking on the symbol next to the "OK" or "next" button.

At the end of each round, you will again be informed about the prices and the quantities you bought in this round and in all previous rounds. Moreover, your profit from all rounds will be shown, and you receive information about the number of units you bought at each supplier so far.