The effect of choice on beliefs and markets

by

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Disclosure of coauthor contribution

How does choice affect beliefs?

Co-author: Gergely Hajdu

Balázs Krusper came up with the idea to analyze the effect of choice on beliefs using a lab experiment. Then, both authors contributed substantially to formulate the exact research question and to build the experimental design. Balázs Krusper implemented the empirical analysis.

How does choice affect learning?

Co-author: Gergely Hajdu

Working on the experiment on the effect of choice on beliefs, Balázs Krusper and Gergely Hajdu together came up with the idea to extend the research question to learning after the choice. Both authors contributed substantially to build the experimental design. Balázs Krusper implemented the empirical analysis.

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Abstract

The thesis consists of three chapters on the effect of choice on beliefs and markets. The first two chapters, co-authored with Gergely Hajdu, employ online experiments to study how choosing a product affects current beliefs and subsequent learning about the value of the products in the choice set. The third, single authored chapter analyzes the effect of optimistic belief distortions on competition between firms.

Chapter 1

People tend to think more favourably about a product when they own it compared to when they do not own it. Going beyond the effect of ownership, we study in the lab how choosing a product affects beliefs about the values of products in the choice set. Using a between-subject design, we compare a person who chooses a product from a binary choice set to a person who receives the same product exogenously. To deal with the endogeneity in choices, we construct information that is both sufficiently clear to make choices predictable and sufficiently unclear to leave room for belief distortion. We find that making a choice increases the difference in beliefs between the two alternatives, and the effect is driven by pessimism about non-chosen products: participants who do not choose a product believe it is worse than participants who do not receive it, while beliefs about chosen and received products are similar. When participants choose a product but their attention is shifted towards product evaluation, pessimism disappears, suggesting that the effect of choice is driven by attention. As choices are often made under uncertainty, the mechanism we identify may play a role in a potentially wide range of settings. Our findings also have policy implications: active choice policies may be more effective tools than opt-out defaults.

Chapter 2

After purchasing a product, people usually receive information and update their beliefs about both chosen and non-chosen products. This, in turn, can affect future buying and selling decisions. In this paper, we

study how choosing a product – as opposed to simply receiving it – affect learning about products after the choice has been made. We design an experiment where participants learn about the fundamental quality of financial investments by observing price changes in multiple rounds. Using a between-subject design, we compare beliefs of participants who choose some of the investments themselves (Choice condition) to participants who receive investments exogenously (Allocation condition). We find that learning is stickier after making a choice: participants respond less to price changes in the Choice condition than in the Allocation condition. This result holds for both own and non-owned investments and for both good news and bad news. We also show that participants in the Choice condition do not pay more attention to the investments; neither when they choose, nor after they have made the choice. We estimate a structural model and demonstrate that learning is not significantly different from the Bayesian benchmark after exogenous product allocation, while it is too sticky after making a choice.

Chapter 3

People tend to hold optimistic beliefs about their own future outcomes. In this paper I analyze the effect of optimistic belief distortions on competition between firms. I extend the standard Hotelling model by allowing consumers to distort their beliefs about the quality of products. As consumers subjective utility is affected only by their beliefs about the product they end up purchasing, belief distortion is asymmetric: consumers become more optimistic about products that they are more likely to buy. The asymmetry in beliefs increases perceived product differentiation and results in weaker competition and higher equilibrium prices. This result is consistent with the observation that high prices and markups can persist in markets with many sellers and fairly homogeneous products. The model identifies a novel channel through which product heterogeneity influences competition. If the products are similar then mistakes in product choice are less costly. Thus, consumers can pick any of the products and become very optimistic about its quality. As a result, belief distortion is more asymmetric that leads to weaker competition. The model predicts prices to be high when the products are similar (because belief distortion is asymmetric) or when products

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are highly differentiated (standard effect), while prices are lowest for intermediate levels of product heterogeneity.

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How does choice affect beliefs?

joint with Gergely Hajdu

1.1 Introduction

People tend to be optimistic about their own future in a variety of domains. Students estimate their own chances to be above average for positive events and below average for negative events (Weinstein, 1980) or entrepreneurs think their business is far more likely to succeed than a typical similar business (Cooper et al., 1988). In line with these observations, previous research has documented that owning a product leads to more optimistic beliefs about its value. For example, investors overestimate their portfolio returns compared to both realized values and market performance (Merkle, 2017). However, in many important economic contexts, people choose the products for themselves (e.g. they decide to invest in a particular fund). We propose and test the hypothesis that making a choice itself can lead to additional belief distortions beyond the effect of ownership.

In this paper we design an experiment to study the effect of making a choice on beliefs. Even though the experiment is set up with financial products, it is generalizable to any goods where people experience uncertainty about the fundamental quality of the goods. Using a between-subject design, we compare a person who chooses a product to a person who receives the same product exogenously. This comparison controls for belief distortions due to owning a product and allows us to focus on the effect of choice. The key identification concern is endogenous choice: beliefs about chosen products may be optimistic because people choose products they believe to have high value to begin with. To address this concern, we vary the choice set and provide information (i.e. a noisy signal) about the *relative ranking* of product values in the choice set. That is, we create predictable variation in choice.

In the experiment, we construct 12 products that we refer to as portfolios. In each of four consecutive rounds, participants observe a set of two portfolios and their main task is to estimate the payoff probabilities. We use a between-subject design where participants – for all four rounds – either receive one of the portfolios (*Allocation* condition) or choose one of the portfolios (*Choice* condition). Participants know they will get a reward if the portfolio they own – chosen or received – pays off. The key challenge is that we can not impose a choice on participants directly. Instead, they should make a non-trivial choice that we are still able to predict. Our solution is to provide a signal about the *relative ranking* of the portfolios to participants in both conditions. Thus, we can use the signal to predict choice with high accuracy. At the same time the *absolute level* of payoff probabilities remain sufficiently uncertain, so participants can form beliefs. Finally, we add two treatment conditions, *Delayed Choice* and *Ego Choice*, to learn about the mechanism. In the *Delayed Choice* condition participants choose a portfolio but we shift their attention from choosing to estimating the payoff probabilities. In particular, the choice buttons appear on the screen only when they record the estimates. Importantly, participants know ahead of time that they have to choose, hence, the intervention only affects the order of reporting beliefs and choice. In the *Ego Choice* condition, participants — before making a choice — read an excerpt about the relationship between high IQ and better asset choice. The rationale behind this manipulation is to make the choice potentially more ego relevant.

We start building the empirical strategy by comparing beliefs about the same portfolio when it is chosen compared to when it is not chosen. This comparison has two main concerns. The first one is reverse causality: participants choose a product because they think it is the better one. Hence, it biases the estimate of the effect of choice on beliefs upwards. We address this concern by instrumenting choice with optimal choice based on the objective payoff probabilities. Indeed, 90% of our participants managed to choose the better portfolio. Second, there are multiple effects contributing to the belief difference between chosen and non-chosen portfolios: a *contrast effect*, an *ownership effect* and a *choice effect*. The *contrast effect* predicts higher beliefs for portfolios when they are compared to inferior ones[1]. As participants, on average, choose the better portfolio, the *contrast effect* positively contributes to the belief difference between chosen and non-chosen portfolios. We can measure this effect in our *Allocation* condition, that allows to control for ownership while having an exogenous variation in the consideration set. The *ownership effect* predicts that participants are more optimistic about owned portfolios even when ownership is not determined by the participants' own choice. Thus, this effect is also expected to positively contribute to the belief difference between chosen and non-chosen portfolios. We can measure this effect as well, as the *Allocation* condition allows to control for the consideration set while having an exogenous variation in ownership. Finally, the *Allocation* condition and *Choice* condition comparison allows to measure the *choice effect* and decompose it into optimism — by comparing beliefs about chosen and received portfolios — and pessimism — by comparing beliefs about non-chosen and non-received portfolios.

We find a sizeable *contrast effect*: beliefs are 6.3 pp higher when the portfolio is compared to a worse as opposed to a better alternative, controlling for ownership. The effect size corresponds to 24% of the standard deviation of beliefs within portfolio types. Interestingly, our data does not feature the *ownership effect*, we do not find statistically significant difference between beliefs about received and non-received portfolios. The total *choice effect* is 2 pp (10% of the standard deviation of beliefs within portfolio types) and it is fully explained by *pessimism* about non-chosen portfolios. Furthermore, we find that the *choice effect* disappears when the participants record their estimates first and indicate their choice in the subsequent screen (i.e. *Delayed Choice* condition). When an excerpt about the relationship between high IQ and asset choice is provided (i.e. *Ego Choice* condition), the *choice effect* is slightly larger (3 pp). It comes – almost equally – from *pessimism* about non-chosen and *optimism* about

[1] The contrast effect is a cognitive bias that enhances the difference between things when we make a comparison between them. For example, the same color is perceived to be lighter when it is surrounded by darker background.

chosen products. The total *choice effect*, however, is not significantly different from the *choice effect* in the baseline *Choice* condition. A natural conjecture is that making a choice increases the stakes, induces higher cognitive effort and leads to more accurate beliefs. However, we find that participants who make a choice do not produce more accurate estimates of the payoff probabilities.

This paper builds on recent research that explores different drivers of optimistic belief distortions (Mayraz, 2013; Coutts, 2019b).[2] We contribute to this research in two ways: First, we find that after controlling for ownership, making a choice leads to additional belief distortions. Second, we show evidence that choice can affect beliefs about a portfolio which is not even in one's possession. Namely, there is *pessimism* about non-chosen portfolios compared to having the same portfolios not received.

The mechanism we identify helps explaining empirical observations in a number of domains. First, recent work in behavioral finance has shown that investors are more likely to hold on to their losing assets if they chose the assets themselves. Evidence comes both from observational data (Chang et al., 2016; Calvet et al., 2009; Ivković and Weisbenner, 2009; Jin and Scherbina, 2010) and experiments (Lehenkari, 2012; Summers and Duxbury, 2012). While this pattern is robustly documented, the mechanism behind it is not yet well understood. Our contribution is to provide clean evidence on a belief-based mechanism that can explain this observation. Our results suggest that investors who made the choice themselves become more pessimistic about the fundamentals of other assets and given these beliefs they will be less willing to switch. On the other hand, our results are not explaining a related finding that investors sell winning assets too early.

Second, consumers often fail to switch to better offers even in markets where products are similar. Examples include credit cards (Ausubel, 1991; Stango, 2000), mutual funds (Hortaçsu and Syverson, 2004) or so-

[2] People have also been found to be overoptimistic in ego-related settings. For example, they tend to overestimate their performance in IQ tests (Eil and Rao, 2011; Möbius et al., 2014; Exley and Kessler, 2018; Zimmermann, 2019), or underestimate how selfish their behavior is (Di Tella et al., 2015; Exley, 2016; Exley and Kessler, 2018; Dezső and Loewenstein, 2019). Others, however, focusing on the effect of ownership, find no significant asymmetry in belief updating in the financial domain (Barron, 2021; Hartzmark et al., 2021). Literature in psychology investigates the effect of choice on how preferences are constructed after the choice (Simon et al., 2004).

cial security insurance (Hastings et al., 2017). Our results suggest the novel explanation that consumers stick to their chosen products because they became more pessimistic about the non-chosen alternatives. We expect this effect to be especially prevalent in markets in which the choice set contains a few items that do not change over time. Moreover, even if there are many options, consumers may start by choosing between groups of products. For example, when purchasing a new smartphone, consumers may decide first which producer to buy from before picking the specific product.

The paper proceeds as follows. First, we describe the experimental design in section 1.2. Then we build the empirical strategy and present the results in section 1.3. Finally, we conclude in section 1.4.

1.2 Experimental design

1.2.1 Setup

First, we describe how we constructed the financial products. There is an imaginary economy, populated by firms. Each firm makes either profit or loss. Firms are divided into two industries, which we denote here for simplicity as A and B. Each industry contains the same number of firms but they differ in the share of profitable firms. In the experiment these shares are set to $p_A = 0.5$ and $p_B = 0.3$. While participants don't observe the shares, they can learn that $p_A > p_B$.

A financial product in this economy is a portfolio that contains shares of N firms. Let N_A and N_B denote the number of firms from industry A and B, respectively. Each firm is randomly selected from its industry. A portfolio pays a fixed amount if the number of profitable firm is at least K and pays nothing otherwise.

In the experiment, participants complete four rounds. In each round, they observe a pair of portfolios with the same N and K but different N_A. The key feature is that the payoff probability is increasing in N_A if $p_A > p_B$ (holding N and K constant). Participants only need to understand this relationship to figure out the *relative ranking* of the portfolios and to make a good choice. However, there remains significant uncertainty

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about the *absolute level* of the payoff probabilities. First, the required calculations are difficult both conceptually and numerically. The payoff probability of a portfolio is given by the following formula:

$$P(\lambda \geq K) = \sum_{k=K}^{N} \sum_{i=0}^{k} \binom{N_A}{k-i} \binom{N_B}{i} {p_A}^{k-i} (1-p_A)^{N_A-(k-i)} {p_B}^{i} (1-p_B)^{N_B-i}, \tag{1.1}$$

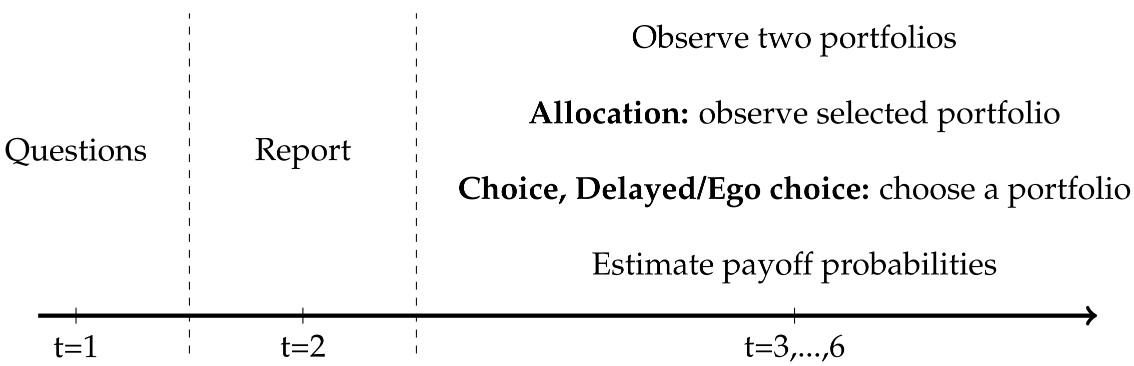
where λ denotes the number of profitable firms. Second, participants do not have all the necessary information as they do not observe p_A and p_B. As a result, participants have a large room to form beliefs.

We vary N and K across rounds and construct three portfolios by varying N_A within rounds[3]. We label the resulting portfolios as Low, Medium and High in increasing order of N_A. In each round, participants observe either a {Low, Medium} or a {Medium, High} pair. As a result, participants should choose the Medium portfolio in some cases (when it is compared to the Low portfolio), while they should not choose it in other cases (when it is compared to the High portfolio).

1.2.2 Timeline

We use a between-subject design where we randomly assign participants into the *Allocation*, *Choice*, *Delayed Choice* or *Ego Choice* conditions. The experiment has three stages. Figure 1.1 shows the timeline.

Figure 1.1: Timeline



After participants are familiarized with the instructions through exam-

[3]Table A.1 in the appendix provides a detailed description of the portfolios.

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ples and control questions[4], in the *Questions* stage, they have to answer three economics-related questions. They are told that they will see a report containing the name of one of the two industries, however, the informativeness of this report depends on their performance in the following way: if they give at least two correct answers, then they will receive a perfectly informative report. That is, the report contains the name of the industry that has the higher share of profitable firms[5]. If they fail to give at least two correct answers, then they might receive a less informative report. Specifically, participants are told the report doesn't necessarily select the industry with the higher share of profitable firms. The reason for including the *Questions* stage is to strengthen the link between knowledge and the ability of making a good choice. As a result, participants do not make a blind or absolutely trivial choice: they know which portfolio to choose because they were smart enough to give correct answers. We believe that an important distinction between real choice and blind choice is the reason why one can be proud of him/herself in case of good outcomes: luck or knowledge. On the one hand, if a blind choice turns out to be a good choice (a randomly picked product has high quality) then one can be proud of being lucky. On the other hand if a real choice turns out to be a good choice (an intentionally selected product has high quality) then one can be proud of being smart[6].

In the *Report* stage, participants observe the content of the report. Importantly, they don't know whether they receive the fully informative or the potentially less informative report. Hence, the portfolio with more of the industries mentioned in the report is weakly more likely to be the better one. As a result, participants can easily infer the *relative ranking* of the portfolios in the choice set. While figuring out the *relative ranking* is an easy task, it is not possible to determine the true payoff probabilities from the information provided. That is, there is room for belief formation. We elicit the beliefs of participants about the likelihood that

[4]They have to answer all control questions correctly to proceed in the experiment. If they give an incorrect answer, they have to try again.

[5]We label industries with the openly made-up names of Eclipse and Rosepaw. We randomize these labels.

[6]In a recent paper, Hartzmark et al. (2021) study the effect of ownership on learning. They report no difference between exogenous product allocation and blind choice (when participants make a choice from a set of identical products). Besides focusing on instantaneous beliefs instead of learning, our design is different in that participants make a real choice.

the report correctly identifies the industry that has the higher share of profitable firms. We emphasize that the experiment is identical across conditions until the end of the *Report* stage. It ensures that participants in the *Choice* condition do not put more effort in answering the three economics-related questions to increase the chance of receiving the informative report.

The *Portfolio evaluation* stage consists of four rounds. In each round, participants observe two portfolios with different industry composition (N and K are the same within rounds). We also give them information about the magnitude of the payoff probabilities: the payoff probability of a benchmark portfolio where each firm is randomly selected regardless of its industry. In the *Allocation* condition, participants randomly receive one of the two portfolios. In the *Choice* condition, participants have to choose between one of the two portfolios. To learn about the mechanism of belief distortion due to choice, there are two additional treatment conditions. The third treatment condition, called *Delayed Choice* condition, serves the purpose of diverting participants' attention from the act of choosing to estimating the payoff probabilities. Participants know from the beginning that they have to choose a portfolio, the only difference compared to the *Choice* condition is that participants can indicate their choice only after they estimated the payoff probabilities. The fourth, and last, treatment condition is the *Ego choice* condition. It only differs from the *Choice* condition by providing subjects the information that people with higher IQ tend to choose assets that are more likely to provide high payoffs. Additionally, participants are asked to remember this information till the end of the experiment. The rational behind this treatment condition is to potentially increase participants' perception of the ego relevance of their choice. Hence, it allows us to test whether ego relevance contributes to the size of the *choice effect*.

Importantly, participants are informed that they will earn a £3 bonus[7] at the end of the experiment if their own portfolio pays off in a randomly selected round. For all participants, we elicit incentivized beliefs about the payoff probabilities for both portfolios[8]. Figure 1.2 and 1.3 show the

[7] It is equivalent to $4.25.

[8] We use the Becker-DeGroot-Marschak method adapted to elicit probabilities (Karni, 2009) and set the reward to £0.5. Participants are told that we are incentivizing them to tell the truth. They can also click on a link that explains the procedure in detail.

portfolio evaluation screen in the *Allocation* and *Choice* condition respectively.

Figure 1.2: Portfolio evaluation screen in the *Allocation* condition

Information

- Each portfolio **contains 6 firms** and **pays off if at least 3 firms make a profit.**
- Recall, the report says that **the Eclipse industry has better outlook.**
- The computer **randomly selected Portfolio 1** for you. Remember, you can earn **£3.00** if this portfolio pays off.

Questions

For each portfolio, estimate the chance that it pays off.

- Hint: A hypothetical portfolio, where each firm is **randomly selected regardless of its industry**, would pay off with **46%** chance.

	Firm 1	Firm 2	Firm 3	Firm 4	Firm 5	Firm 6	Chance of paying off (%)
Portfolio 1	Eclipse	Eclipse	Rosepaw	Rosepaw	Rosepaw	Rosepaw	
Portfolio 2	Eclipse	Rosepaw	Rosepaw	Rosepaw	Rosepaw	Rosepaw	

Figure 1.3: Portfolio evaluation screen in the *Choice* condition

Information

- Each portfolio **contains 5 firms** and **pays off if at least 2 firms make a profit.**
- Recall, the report says that **the Eclipse industry has better outlook.**

Questions

1. **Indicate which portfolio you would like to choose** by using the buttons in the table. Remember, you can earn **£3.00** if the portfolio you choose pays off.
2. For each portfolio, estimate the chance that it pays off.
   - Hint: A hypothetical portfolio, where each firm is **randomly selected regardless of its industry**, would pay off with **66%** chance.

	Firm 1	Firm 2	Firm 3	Firm 4	Firm 5	Chance of paying off (%)
Portfolio 1	Eclipse	Eclipse	Eclipse	Rosepaw	Rosepaw	
Portfolio 2	Eclipse	Eclipse	Eclipse	Eclipse	Rosepaw	

1.2.3 Implementation

Data collection

We pre-registered the experimental design, the hypotheses and the empirical strategy in the American Economic Association's Randomized Control Trials Registry (ID: AEARCTR-0005974). The experiment was run using the experimental software oTree (Chen et al., 2016). We recruited participants through Prolific, a crowd sourcing platform designed specifically for academic studies. A very useful feature of Prolific is that it allows the researcher to pre-screen participants on various dimensions. We made two sets of restrictions. First, we required participants to be located in the US and to speak English as a first language in order to minimize language barriers. Second, we considered only participants who answered basic demographic questions when they registered on Prolific. We had access to these answers and did not have to include basic demographic questions in the experiment.

We posted the study on July 9, 2020. The participation fee was set to £2. On average, participants completed the experiment in 16 minutes and earned £4 (including bonuses). The relevant number of participants who completed the experiment is 993.

Treatment assignment

We assigned treatment status in two steps. First, each participant was assigned to one of the four conditions. Table 1.1 shows that 362 participants ended up in the *Allocation* condition and 340 participants ended up in the *Choice* condition. The *Delayed Choice* condition had 143 participants while the *Ego Choice* condition had 148 participants.

Second, in each of the four rounds in the *Allocation* condition one of the two portfolios was selected for each participant. In order to increase statistical power by making the *Allocation* condition similar to the other conditions, we set the probability of receiving the better portfolio to 80%.[9] It is important that we randomized whether Portfolio 1 or Portfo-

[9]Of course, the exact fraction doesn't affect the empirical strategy. However, for reasons of statistical power, we wanted to get close to the fraction in the *Choice* condition, therefore based this number on our pilot.

lio 2 is the better one. Participants were only informed that they could have received Portfolio 1 or Portfolio 2 with equal chances. Therefore, observing the received portfolio did not contain information about its likelihood to pay off. Table 1.1 shows that participants in the *Allocation* condition received the better portfolio in 79% of the cases.

Table 1.1: Treatment assignment

	Allocation	Choice	Delayed Choice	Ego Choice
Number of participants	362	340	143	148
Better portfolio is owned	79%	90%	92%	94%
Choice is consistent with beliefs		95%	98%	96%

In Table 1.1 we also reports statistics about choices. Participants in the *Choice* condition chose the better portfolio in 90% of the cases and slightly even more frequently in the *Delayed Choice Ego Choice* conditions. Participants' choices are mostly consistent with their stated beliefs. In the *Choice* condition, participants choose the portfolio that they estimate to have (weakly) higher payoff probability in 95% of the cases and slightly more often in the other two treatment conditions involving a choice[10].

Balance tests

During the instructions, participants had to answer 10 control questions in total. They could proceed to the next screen only if the answer was correct. While participants could complete the control questions by random guessing, we observe very few incorrect submissions (less than one on average). This indicates that those who completed the experiment understood the setup well and quickly. In the *Report* stage, 93% of the participants managed to answer at least two of the three questions correctly. On average, they estimated that the reported industry is the good industry with 79% probability. As both the *Questions* stage and the *Report* stage preceded the treatment assignment, we expect no difference across treatment conditions neither between these variables nor in personal characteristics and this is indeed the case.[11]

[10]Similarly to comparison between the *Allocation* and *Choice* conditions, the exact fraction of correct choices does not affect the empirical strategy.

[11]The only significant difference is that participants in the *Ego Choice* condition report slightly lower income than participants in the *Allocation* condition. See Table A.2 in the Appendix.

1.3 Results

1.3.1 Empirical strategy

Before getting to the regression results, we start by looking at the raw belief difference between chosen and non-chosen portfolios. To do this, we restrict the sample to the Medium portfolios and plot the average payoff probability estimates for when this portfolio was chosen and when this portfolio was not chosen (Figure 1.4). Since Medium portfolios are paired either with Low or High portfolios with equal chances and participants mostly choose the better portfolio, the composition of chosen and non-chosen portfolios are almost identical. Therefore, the plot indicates that beliefs are higher on average when the portfolio is chosen than when the same portfolio is not chosen. This in itself, however, is only suggestive evidence of a *choice effect* as this comparison has two main concerns. The first one is reverse causality: participants choose a portfolio because they think it is the better one. Second, there are other effects — *contrast effect* and *ownership effect* — contributing to this belief difference. Of course, our design is set up exactly to address both concerns.

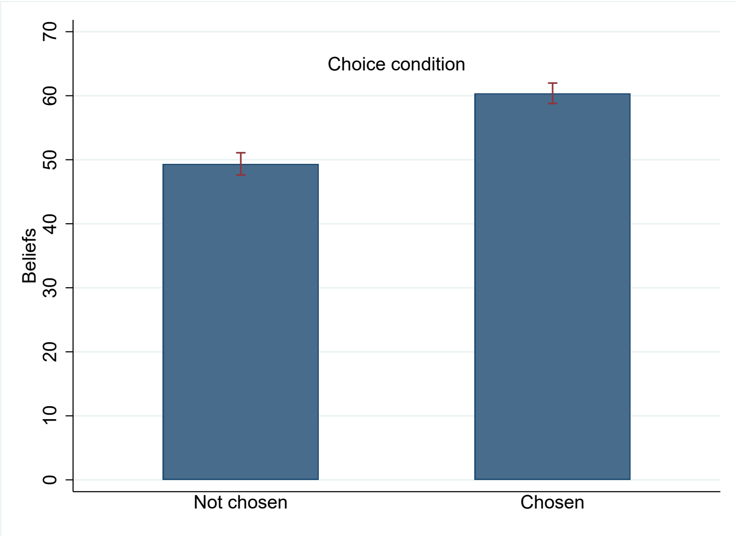
In this section we present our empirical strategy. We start with a simpler case by assuming that choice is not endogenous and show how we can measure the *contrast effect* and *ownership effect* using random variation in the *Allocation* condition. Then, we relax the assumption that beliefs do not affect choice. We use optimal choice to instrument actual choice because it is randomly determined and predicts actual choice with high accuracy.

Under the assumption that choice is exogenous, we can directly compare the effect of ownership with allocation (i.e. *ownership effect*) and with choice. We divide the data by treatment condition and ownership (Table 1.2).

The following difference-in-differences regression compares the averages of these categories:

$$Belief_{ij} = \beta_0 + \beta_1 Own_{ij} + \beta_2 Own_{ij} \times Choice_i + \beta_3 Choice_i + \varepsilon_{ij}, \quad (1.2)$$

Figure 1.4: Average beliefs for non-chosen and chosen portfolios.



Notes: This figure uses observations for Medium portfolios in the Choice condition and reports average beliefs about the payoff probabilities when the portfolios are chosen and not chosen. As the sample is restricted to Medium portfolios, the portfolio composition is expected to be the same between the two categories. The error bars represent 95% confidence intervals.

Table 1.2: Categories by condition and ownership

	Own	**Other**
Allocation	received	non-received
Choice	chosen	non-chosen

where $Belief_{ij}$ is participant i's belief about the payoff probability of portfolio j, Own_{ij} is a dummy variable for own portfolios and $Choice_j$ is a dummy variable for the *Choice* condition.

However, the simple difference-in-difference comparison is confounded by two factors. First, portfolio composition varies across categories. For example, the better portfolio is received with 80% probability in the *Allocation* condition and it is chosen even more often in the *Choice* condition. We can control for this heterogeneity by including portfolio fixed effects (α_j). Second, recall that Medium portfolios are compared to Low portfolios in some cases and to High portfolios in other cases. The same portfolio may seem more likely to pay off if it is compared to a worse portfolio than to a better portfolio due to the *contrast effect*. This is a potential confound, because Medium portfolios are more likely to be received or chosen if they are compared to Low portfolios. We can control for the *contrast effect* by including a dummy for the better portfolio within the set ($Better_{j,-j}$).

With these modifications, we arrive at the following equation:

$$\begin{aligned} Belief_{ij} = \beta_0 + \beta_1 Own_{ij} + \beta_2 Own_{ij} \times Choice_i + \beta_3 Choice_i + \beta_4 Better_{j,-j} + \\ + \alpha_j + \varepsilon_{ij}, \end{aligned} \tag{1.3}$$

The effect of ownership through allocation on beliefs (*ownership effect*) is measured by β_1. β_2 measures the difference between the effect of ownership with choice and allocation (*choice effect*). We can decompose the total effect into *pessimism* about non-chosen portfolios compared to non-received portfolios (β_3) and *optimism* about chosen portfolios compared to received portfolios ($\beta_2 + \beta_3$). Finally, β_4 measures the *contrast effect*.

As the last step, we relax the assumption that beliefs do not affect choice. In this case, Own_{ij} is endogenous in Equation 1.3. Recall that ownership is determined randomly in the *Allocation* condition, therefore endogeneity comes entirely from the *Choice* condition. Our solution is to instrument ownership with optimal choice in the *Choice* condition. It is random which portfolio is the better one and participants indeed choose the better portfolio in most of the cases. Combining these considerations, we use the following instrument for Own_{ij}:

$$Own^*_{ij} = Own_{ij} \times (1 - Choice_i) + Better_{j,-j} \times Choice_i \tag{1.4}$$

Interacting Own^*_{ij} with the $Choice_i$ directly gives us the instrument for $Own_{ij} \times Choice_i$:

$$Own^*_{ij} \times Choice_i = Better_{j,-j} \times Choice_i \tag{1.5}$$

In the baseline regression, we estimate Equation 1.3 by using instruments (1.4) and (1.5) for Own_{ij} and $Own_{ij} \times Choice_i$, respectively. This strategy relies on two assumptions. First, the effect of pure ownership should be the same across treatment conditions. Note that payoffs depend only on which portfolios consumers own regardless whether they received chose the portfolios. Second, the contrast effect should be identical across treatment conditions. In the experiment, participants face the same portfolio pairs thus they make the same comparisons.

1.3.2 Regression results

We estimate Equation 1.3 by OLS and IV and report the results in Table 1.3. The standard errors are clustered at the individual level. We use the IV (Column 2) as the baseline specification.

We find a large and significant *contrast effect* showing that participants' beliefs about a portfolio are 6.4 pp higher when the portfolio is paired with a worse as opposed to a better alternative. This makes up for the total belief difference between received and non-received portfolios resulting in a small and non-significant *ownership effect*. There is a *choice effect*, that is, choosing a portfolio increases the difference between beliefs by 2 pp about the same portfolio when the portfolio is owned compared to when it is not. The entire *choice effect* comes from *pessimism* about non-chosen portfolios compared to having the same portfolio not received. Namely, beliefs are 2.5 pp lower when the portfolio is not chosen than having the same portfolio not received. Additionally, both the *pessimism* effect and the interaction become large and significant when estimated only on Medium portfolios where participants had more space to distort beliefs (see Table 1.3 Column 3 and Column 4).

As a next step, we estimate the effect for both delaying the choice and making the choice more ego relevant by including observations from all treatment conditions. In the extended specification, we have separate

Table 1.3: Main results

	(1)	(2)	(3)	(4)
Dependent variable: Belief	OLS	IV	OLS	IV
Better	3.804***	6.359***	4.511***	5.104***
	(0.897)	(0.986)	(1.088)	(1.246)
Own	2.037**	0.556	0.974	0.630
	(0.736)	(0.717)	(1.218)	(1.263)
Own × Choice	5.935***	1.951	5.991***	5.065**
	(1.105)	(1.394)	(1.614)	(1.879)
Choice	-4.450***	-2.458*	-4.543***	-4.079**
	(1.021)	(1.095)	(1.329)	(1.397)
Observations	5616	5616	2808	2808
R^2	0.377	0.373	0.218	0.217
Portfolio FE	Yes	Yes	Yes	Yes
Sample			Medium	Medium

Notes: This table reports the coefficient estimates for Equation 1.3. The unit of observation is a participant × portfolio. The baseline is non-received portfolios, hence, the coefficients are percentage point differences showing the estimates of contrast effect, ownership effect, choice effect and pessimism, respectively. Column 1 and Column 2 use the full sample while Column 3 and Column 4 restrict the sample to only Medium portfolios for which participants had more space to distort beliefs. Column 2 and Column 4 present the IV estimates. Standard errors are in parenthesis and clustered at the individual level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

dummy variables for the *Choice*, *Delayed Choice* and *Ego Choice* conditions and we construct the instruments analogously to Equation 1.4 and 1.5. As a result, the identification assumptions are also similar: we assume that the *ownership effect* and the *contrast effect* are identical across all treatment conditions.

We report the estimates in Table 1.4. Observe that the previous estimates are robust to including observations from the *Delayed Choice* and *Ego Choice* conditions. The coefficient on Own $\times$ Delayed Choice shows the *choice effect* separately for the *Delayed Choice* condition. It is small and not significant in most specifications indicating that delaying the choice counteracts the baseline *choice effect*. We included a manipulation check question at the end of the experiment to assess the results from the *Delayed Choice* condition. Specifically, we asked participants how much they focused on comparing the portfolios rather than estimating the payoff probabilities separately. Table A.4 reports the results. Participants in the *Choice* condition paid more attention to comparing the portfolios than participants in the *Allocation* condition. Participants in the *Delayed Choice* condition are in between these two groups, but the difference form the *Choice* condition is not statistically significant (p-value = 0.16).

Table 1.4: *Choice effect* with all four treatment conditions

	(1)	(2)	(3)	(4)
Dependent variable: Belief	OLS	IV	OLS	IV
Better	3.261***	6.785***	5.060***	5.136***
	(0.820)	(0.958)	(0.978)	(1.248)
Own	2.551***	0.510	0.644	0.600
	(0.741)	(0.718)	(1.189)	(1.265)
Own × Choice	6.173***	1.944	5.903***	5.061***
	(1.092)	(1.395)	(1.606)	(1.879)
Choice	-4.573***	-2.459**	-4.499***	-4.077***
	(1.017)	(1.095)	(1.326)	(1.397)
Own × Delayed Choice	3.944***	-1.285	1.470	1.801
	(1.244)	(1.681)	(1.850)	(2.224)
Delayed Choice	-1.835	0.780	-0.668	-0.830
	(1.380)	(1.392)	(1.550)	(1.640)
Own × Ego Choice	4.803***	2.960*	4.597**	5.717**
	(1.486)	(1.751)	(2.108)	(2.448)
Ego Choice	-1.873	-0.952	-2.071	-2.625
	(1.222)	(1.329)	(1.599)	(1.711)
Observations	7944	7944	3972	3972
R^2	0.377	0.372	0.215	0.215
Portfolio FE	Yes	Yes	Yes	Yes
Sample			Medium	Medium

Notes: This table reports the coefficient estimates for a regression analogous to Equation 1.3, but this time including the observations from the *Delayed Choice* and *Ego Choice* conditions as well. The unit of observation is a participant × portfolio. The baseline is non-received portfolios, hence, the coefficients are percentage point differences showing the estimates of contrast effect, ownership effect, choice effect and pessimism, respectively. The estimates of the interactions of *Own* and the different choice dummies show the choice effect separately in the three choice conditions. Column 1 and Column 2 use the full sample while Column 3 and Column 4 restrict the sample to only Medium portfolios for which participants had more space to distort beliefs. Column 2 and Column 4 present the IV estimates. Standard errors are in parenthesis and clustered at the individual level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

The coefficient on Own $\times$ Ego Choice shows the *choice effect* for the *Ego Choice* condition. As expected, it is slightly larger (3 pp) than in the baseline *Choice* condition and comes — almost equally — from *pessimism* about non-chosen and *optimism* about chosen products. The total *choice effect*, however, is not significantly different from the *choice effect* in the baseline *Choice* condition. We included a manipulation check for the *Ego Choice* treatment as well. We asked participants how proud they were of themselves because they could choose portfolios that were more likely to pay off. For this question, we find no statistically significant difference between the *Choice* and *Ego* choice conditions (Table A.4).

We also look at whether making a choice leads to more accurate beliefs. It is possible that having to make a choice increases the stakes, hence, induces higher cognitive effort. This, in turn, might lead to more accurate beliefs. We define several variables to measure accuracy.

- *Squared error* is the negative of the squared difference between the reported belief and the true payoff probability.
- *Seconds eval* measures the time in seconds spent on the task.
- *Set ranking* measures whether participants get the ranking between the portfolios right. That is, whether the reported belief is higher for the portfolio that contains more good industry firms.
- *Relative to benchmark* measures whether participants get the ranking between the portfolio and the random benchmark right. That is, whether the reported belief is higher than the benchmark probability if and only if the portfolio contains more good industry firms than bad industry firms.
- *Rank correlation* is the Spearman's rank correlation coefficient for the reported beliefs and the true payoff probabilities.

Table 1.5: Accuracy of beliefs across conditions

	(1)	(2)	(3)	(4)	(5)
Dependent variable:	Squared error	Seconds eval	Set ranking	Rel. to benchmark	Rank correlation
Choice Baseline	-23.92	3.140	0.0369*	-0.0256*	0.00449
	(36.93)	(2.784)	(0.0210)	(0.0133)	(0.0276)
Delayed Choice	-6.424	5.961	0.0274	-0.0218	0.0115
	(48.04)	(4.663)	(0.0288)	(0.0176)	(0.0367)
Ego Choice	18.58	7.251**	0.0797***	-0.0169	0.0379
	(46.19)	(3.395)	(0.0234)	(0.0167)	(0.0369)
Constant					0.435***
					(0.0199)
Observations	7944	3972	3972	7944	980
R^2	0.132	0.149	0.014	0.187	0.001
Control variables:					
Set FE	No	Yes	Yes	No	No
Portfolio FE	Yes	No	No	Yes	No
Better portfolio	Yes	No	No	Yes	No
Round FE	No	Yes	No	No	No

Notes: The table compares different belief accuracy measures across the three treatment conditions. The baseline group is the *Allocation* condition, hence, the estimates of the different choice dummy variables show the differences from the *Allocation* condition. In Column 1 and Column 4 the unit of observation is participant $\times$ portfolio and we include portfolio fixed effects and the Better dummy as controls. In Column 2 and Column 3 the unit of observation is participant $\times$ portfolio pair and we control for portfolio pair fixed effects. In Column 2 we also include round fixed effects, because time spent on the portfolio evaluation screens decreases substantially over time as participants are getting more familiar with the task. In Column 5 the unit of observation is a participant. Standard errors are in parenthesis and clustered at the individual level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

We regress each accuracy variable on the treatment assignment and a set of control variables depending on the unit of observation. Table 1.5 reports the estimates. The dependent variable measures how well participants did, therefore higher accuracy is indicated by a positive coefficient. Participants in the *Choice* condition rank the portfolios correctly a bit more times, however, they are less correct in ranking the portfolio relative to the benchmark than participants in the *Allocation* condition. Interestingly, participants in the *Ego Choice* condition spent slightly more time on the task and had more accurate beliefs. [12] Considering the baseline *Choice* and *Allocation* comparison, if anything, participants in the *Choice* condition reported slightly less accurate beliefs.

[12] Together with the slight increase in the *choice effect* and *optimism* compared to the baseline *Choice* condition, this is consistent with the findings of Hartzmark et al. (2021).

1.4 Discussion and conclusion

1.4.1 Choice and the endowment effect

It has been documented that people attach additional value to things they own simply as a result of ownership (i.e. endowment effect).[13] Our finding of the *choice effect* implies that ownership — when happens through choice — changes beliefs not only about products in one's possession, but about alternative options as well. Additionally, our results on *contrast effect* shows that having alternative options can in itself increase the wedge between beliefs about the two observed options. Contrary to a random assignment, facing a consideration set is always an inherent part of choice. Consequently, *contrast effect* plays a role in all active choices. In the absence of an active choice (e.g. default or random assignment), this is not necessarily the case.

Ownership could potentially inflate valuations through two channels: first, people might inflate beliefs about the quality of the product. Second, people might change their preferences even when their beliefs are the same. We show that choice has an effect on beliefs beyond the effect of ownership. Since choosing a product doesn't provide any instrumental information about alternative products, the consequence that people are less likely to switch to alternatives is suboptimal from the consumers' point of view. This line of argument wouldn't hold for experiments documenting changes in valuations, as a change in valuation might be solely due to preference change.

1.4.2 Choice versus default

Our findings also have interesting policy implications. Recent research documents that default policies are significantly less effective in the long run than in the short run. For example, Beshears et al. (2018) study the effect of automatic enrollment on retirement savings over a horizon of eight years. They find that withdrawals and borrowing against savings offset approximately 40% of the positive effect of automatic enrollment.

[13]See Marzilli Ericson and Fuster (2014) for an overview of the literature.

Our results suggest that active choice policies may be more effective policy tools than opt-out defaults. Rather than setting an option as a default, policy makers could try to make people choose that option instead. Of course, the benefit of using an active choice policy — as opposed to a default — depends on whether policy makers can set up the decision environment such that people choose the default option by themselves.

1.4.3 Summary and future research

In this paper we design an experiment to study the effect of choice on beliefs. We show that making a choice considerably increases the difference between beliefs about owned and non-owned products. This effect comes mostly from participants forming pessimistic beliefs about products that are not chosen compared to beliefs about products that are not received. The effect of pessimism disappears when participants attention is diverted from choice to having accurate beliefs. This suggests that pessimism is mostly driven by attention. While facing a choice situation may induce higher cognitive effort, participants who make a choice do not form more accurate beliefs. As choices are often made under uncertainty, the mechanism we identify may play a role in a potentially wide range of settings.

While our findings are about beliefs at the time of choice, in many contexts, people receive information after their choice. In a follow-up study, we investigate the effect of choice on learning in a similar environment where optimal choice is a cognitively challenging task.

Chapter 2

How does choice affect learning?

joint with Gergely Hajdu

2.1 Introduction

After purchasing a product, people usually receive information and learn about both chosen and non-chosen products. Their updated beliefs, in turn, influence future buying and selling decisions. For example, investors monitor developments of both their own assets and alternative investment opportunities. Based on what they learn, they may decide to reallocate their portfolio. Previous research has documented that learning is influenced by ownership, people respond more to information about own products compared to information about non-owned products (Hartzmark et al., 2021). However, in many important economic contexts, people choose the products for themselves (e.g. they decide to invest in a particular fund). In this paper, we study how making a choice influences learning beyond the effect of ownership.

We design an experiment where participants learn about the fundamental quality of financial investments by observing price changes. Using a between-subject design, we compare participants who choose the investments themselves to participants who receive the investments exogenously. This comparison allows us to isolate the effect of choice from the effect of ownership. We find that learning becomes stickier after making a choice: participants who choose the investments respond less to new information than those who do not choose the investments themselves. We also document that learning is not statistically significant from the Bayesian benchmark after exogenous product allocation, while it is too sticky after choice.

In the experiment, the participants observe price realizations of financial investments in multiple rounds. Each investment has a fixed but unobserved underlying quality that determines its price evolution. Specifically, the higher the quality, the more likely that the price will increase in each period. We ask participants to estimate the underlying qualities by observing the price realizations. Participants are randomly assigned into one of two treatment conditions. After four rounds, they either choose some of the investments for themselves (*Choice* condition) or receive some of the investments exogenously (*Allocation* condition).

This design overcomes four important challenges in studying the effect of choice on learning. First, we are interested in the effect of a real choice when people face different options. In our experiment, participants choose from investments with different price histories. The choice needs deliberation, participants need to figure out how past and current prices predict future prices. While Hartzmark et al. (2021) find no difference between random allocation and blind choice (when participants choose from a set of identical options), we show that deliberate choice does have an effect on belief updating. Second, making a choice is endogenous, as current beliefs affect both choices and subsequent beliefs. While the choice should be non-trivial for participants, it should be predictable for the researcher in order to make causal inference. Most participants in the experiment figure out that the optimal strategy is to choose the investments with the highest current prices. Thus, we can predict their choices with high accuracy using current prices. Third, choosing a product might affect learning because ownership itself influences belief updating. We can isolate the effect of choice from the effect of ownership by comparing beliefs between the *Choice* condition and the *Allocation* condition. Finally, in Chapter 1, we document that making a choice has an instantaneous effect on beliefs. Our design attenuates this effect because choice is unexpected and we elicit participants' beliefs right before choosing.

We use data from 525 participants who completed the experiment and met a preregistered consistency requirement. On average, participants update their beliefs in the correct direction: beliefs are increasing after price increases and decreasing after price decreases. In the *Allocation* condition, participants change their beliefs by 4.1 percentage points on average in the direction of the current price change. We find that learning is stickier after making a choice compared to exogenous product allocation: participants change their beliefs only by 3.2 percentage points in the *Choice* condition. The difference is near statistically significant at the 5 percent level ($p = 0.059$). We observe the same pattern for both own and non-owned investments and for both good news and bad news with differences between the *Choice* condition and the *Allocation* condition ranging from -1.1 to -0.8 percentage points. That is, we do not find asymmetric effect on belief updating ('good news, bad news' effect). Instead, inference is uniformly weaker after making a choice.

A candidate explanation for stickier learning is that participants pay more attention to the investments before or during the choice and thus it is rational to respond less to new information. Our design rules out more attention before the choice because choice and product allocation is unexpected. In addition, participants do not spend more time on the choice screen than on the allocation screen, suggesting that they do not study the investments more carefully when they make the choice. It is possible though that making a choice and paying attention is associated strongly in people's mind. Thus they may (mistakenly) believe that they must have paid a lot of attention because they made a choice and update their beliefs accordingly.

Our data provides suggestive evidence that making a choice has a stronger effect on learning for less educated participants. After making a choice, participants without a degree respond 1.8 percentage points less to price changes ($p = 0.037$), while participants with at least undergraduate degree respond only 0.4 percentage points less ($p = 0.506$). However, the two estimates are not statistically significantly different from each other ($p = 0.189$). We do not find heterogeneous effects based on gender or age.

We examine whether making a choice has an impact on how much attention participants pay to the prediction tasks after the choice. We consider various measures of attention (accuracy in recalling previous price changes and beliefs, time spent on prediction screens, consistency of reported beliefs) and do not find a difference between the treatment conditions for any of them. We also find zero effects when we split the sample by gender, age or education.

Finally, we estimate a structural model to compare belief updating in the different treatment conditions to the Bayesian benchmark. In addition, the model will allow us to conduct counterfactual analysis in future work. We extend Bayesian updating by introducing a weight $1 + x$ on previous beliefs. Our model incorporates Bayesian learning ($x = 0$), overlearning ($x < 0$) and sticky learning ($x > 0$). We find that x is virtually zero in the *Allocation* condition: learning is not significantly different from the Bayesian benchmark after exogenous product allocation. However, we estimate $x = 0.03$ in the *Choice* condition ($p = 0.000$), implying that learning is too sticky after making a choice.

There is a large empirical literature in economics and psychology studying how people actually update their beliefs. Prior research has identified several biases such as base rate neglect (Kahneman and Tversky, 1973; Holt and Smith, 2009), representativeness bias (Grether, 1978, 1980, 1992), confirmation bias (Pitz et al., 1967; Charness and Dave, 2017), correlation neglect (Enke and Zimmermann, 2017) or selection neglect (Esponda and Vespa, 2018; Enke, 2020). Benjamin (2019) provides a comprehensive review of evidence and theory on behavioral biases in belief updating.

Within this broad research area, our findings contribute to the literature on preference-based inference. According to preference based inference, people may update beliefs differently when they receive 'good news' (information that increases expected utility) compared to when they receive 'bad news'. The evidence for the 'good-news, bad-news' effect is so far mixed, some papers find stronger inference from good news, some papers find stronger inference from bad news and some papers find symmetric inference (Benjamin, 2019). A candidate explanation for the heterogeneous findings is domain-specific learning: people may form beliefs differently about themselves, about others' behavior or about states with different monetary payments. Indeed, studies generally find that introducing financial stake does not lead to asymmetric belief updating (Gotthard-Real, 2017; Coutts, 2019a; Hartzmark et al., 2021; Barron, 2021). In line with these papers we also find a symmetric effect: making a choice leads to weaker inference from both good news and bad news for both own investments and non-owned investments.

Our paper is closest to Hartzmark et al. (2021) who employ a similar setting to study the effect of ownership on belief updating. The crucial difference is that they allocate investments randomly or ask participants to choose investments before they observe any price changes. They show that ownership increases the sensitivity of beliefs to information: participants respond stronger to both good news and bad news for own investments compared to non-owned investments. While Hartzmark et al. (2021) report no difference between exogenous allocation and blind choice, we show that a deliberate choice makes belief updating stickier compared to exogenous allocation. An important distinction between a blind choice and a real choice is the reason why one can be proud of him/herself in case of good outcomes: luck or knowledge. On the one hand, if a blind choice turns out to be a good choice (a randomly picked product has high quality) then one can be proud of being lucky. On the other hand, if a real choice turns out to be a good choice (an intentionally selected product has high quality) then one can be proud of being smart. As a result, motives related to one's ego may play a more important role for a real choice. While the current experiment does not provide evidence of which aspect of the choice process matters, the results can motivate future research about this question.

Our results can also explain an important observation about consumer behavior. There is overwhelming evidence that consumers often fail to switch to more favourable contracts or suppliers[1]. Consider a consumer who chooses a product based on her current beliefs. Then, she receives information about both the chosen and non-chosen product and updates her beliefs. She will switch to the other product if it becomes better according to the posterior beliefs. How does the fact that the consumer had chosen the product influence the likelihood of switching? In Chapter 1, we show that making a choice leads to more pessimistic instantaneous beliefs about the non-chosen product. Hence, even if the consumer ob-

[1]Examples include markets for energy (Hortaçsu et al., 2017; Ito et al., 2017; Competition & Markets Authority, 2016), health insurance (Handel, 2013; Handel and Kolstad, 2015; Polyakova, 2016), credit cards (Shui and Ausubel, 2005; Stango and Zinman, 2015; Galenianos and Gavazza, 2021), paid TV (Shcherbakov, 2016), mobilephone services (Shy, 2002), auto insurance (Kiss, 2019), and mortgages (Keys et al., 2016; Andersen et al., 2020).

serves good news about the non-chosen product, she will combine it with a more pessimistic initial belief and will be less likely to switch. In this paper we add a second channel: consumers respond less to new information after making a choice. Hence, even if the consumer observes bad news about the chosen product and/or good news about the non-chosen product, she will put higher weight on her initial beliefs and will be less likely to switch.

The paper is organized as follows. We start by outlining the experimental design in section 2.2. We compare learning between treatment conditions in section 2.3. Then, we estimate a structural model to compare learning to the Bayesian benchmark in section 2.4. Finally, we conclude in section 2.5.

2.2 Experimental design

Our experiment builds on a design scheme that is often used in experimental finance to study trading behavior since Weber and Camerer (1998). The closest version to ours was employed by Hartzmark et al. (2021).

2.2.1 Setup and timeline

Participants observe the price evolution of six investments for 20 periods. Each investment has a starting price of 100 in period 0. Price movements between period 1 and 20 are governed by an investment-specific underlying quality measure s_i. In particular, investment prices increase by 6% with probability s_i or decrease by 5% with probability $1 - s_i$. The quality is constant over time and price changes are *iid* across investments and periods. We interpret s_i as quality because higher s_i implies higher expected investment price. Participants do not know anything about how s_i-s were generated. We used the following values: 0.3, 0.41, 0.49, 0.56, 0.62, 0.74. To improve statistical power, we generated six price paths before the experiment. Thus, all participants observe the

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exact same price realizations, we only randomized the order of the investments on the screen[2].

In each period t, participants observe investment price histories up to t and their task is to estimate investment qualities. This setting provides a fairly simple learning environment where participants observe *iid* signals. A price increase is a good signal about the investment quality, while a price decrease is a bad signal. Belief elicitation is incentivized: we tell participants that they receive a £1 bonus if a randomly selected estimate is within 10 percentage points of the true value.

The experiment consists of three stages, the timeline is summarized in Figure 2.1. After reading the instructions and answering comprehension questions, participants start by predicting investment qualities for four periods (Stage 1). In each period, the screen displays the entire price histories and highlights the current prices. We emphasize that this stage is the same for all participants regardless of the treatment condition.

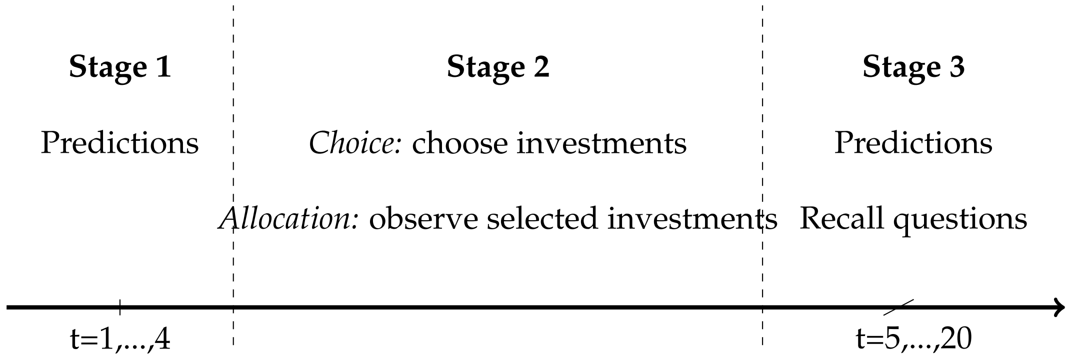


Figure 2.1: Timeline

Participants enter Stage 2 unexpectedly after period 4. We explain that they will own three of the six investments and receive additional payments based on the final price of their own investments. Specifically, total own investment value is exchanged at the rate of £1 per 400 experimental points. We also emphasize that there will be no trading, they will keep their investments until the end of the experiment. Participants in the *Choice* condition have to select the investments for themselves, while we tell participants in the *Allocation* condition that they receive the three investments with highest current price. We expected that participants in the *Choice* condition will tend to select these investments, thus this allocation facilitates the comparison between the two treatment conditions.

[2]Figure B.1 presents the price paths.

After ownership is determined, participants enter Stage 3 and continue with the predictions from period 5 to 20. Similarly to Stage 1, the screen displays the entire price histories and highlights the current prices. In addition, participants are also reminded which investments they own[3]. In Stage 3, we also add two recall questions. First, we ask participants about price changes of a randomly selected *High* and *Low* investment in the previous period. They receive this question in a randomly selected period between 14 and 16. Second, we ask participants about their predictions of a randomly selected *High* and *Low* investment in the previous period. They receive this question three periods after the price recall task. For each recall task, participants get a £0.50 bonus if they answer both questions correctly.

It is worth reviewing how this design helps us to overcome challenges in studying the effect of choice on learning and ruling out potential confounds.

First, we are interested in the effect of a real choice when people face different options and the decision needs deliberation. In our experiment, the timing of the choice ensures that there is dispersion in current investment prices. In addition, participants have to understand how investment prices evolve and work out how to predict final prices using observations on current prices. We believe that this task is by far not trivial for the vast majority of the participants and they have to think about their decision. This choice context is fundamentally different from the setting employed by Hartzmark et al. (2021). They ask participants to choose before they observe any price changes, thus they make a blind choice from identical investments.

Second, making a choice is endogenous: current beliefs affect both choices and subsequent learning (omitted variable problem). In order to make causal inference, we need to predict choices with high accuracy. While investment choice is not trivial in our design, we expected that most of the participants would figure out the correct strategy and buy the investments with highest current price. Indeed, participants in the *Choice* condition chose correctly in more than 80% of the cases. That is, we can use current prices to predict actual choice.

Third, choosing a product might affect learning because ownership it-

[3] Figure B.2 present an example prediction screen.

self influences belief formation. By comparing beliefs between the *Choice* condition and the *Allocation* condition, we can isolate the effect of choice from the effect of ownership. Specifically, we can compare beliefs about chosen investments to received investments and beliefs about non-chosen investments to not received investments.

Fourth, in Chapter 1, we document that making a choice has an instantaneous effect on beliefs. Consequently, learning may be different after making a choice because beliefs have already changed before the first signal. Our design attenuates this effect because we elicit participants' beliefs right before choosing. This design choice is also supported by our previous finding. In Chapter 1 we show that even when participants know they will have to choose, restricting them to indicate their choice only after product evaluation eliminates the instantaneous effect of choice.

2.2.2 Hypotheses

Prior to running the experiment, we had three (preregistered) hypotheses in mind about the effect of choice on learning: motivated learning, overlearning and sticky learning.

Hypothesis 2.1. *(Motivated learning) After making a choice, learning is more optimistic for own investments and more pessimistic for non-owned investments. That is, participants respond more to price increases and less to price decreases for own investments while they respond less to price increases and more to price decreases for non-owned investments.*

Hypothesis 2.2. *(Overlearning) After making a choice, participants respond more to both price increases and price decreases for both own investments and non-owned investments.*

Hypothesis 2.3. *(Sticky learning) After making a choice, participants respond less to both price increases and price decreases for both own investments and non-owned investments.*

2.2.3 Implementation

The experiment was run using the experimental software oTree Chen et al. (2016). We recruited participants through Prolific, a crowd sourcing platform designed specifically for academic studies. A very useful feature of Prolific is that it allows the researcher to pre-screen participants on various dimensions. We made two set of restrictions. First, we required participants to have US nationality, to be located in the US and to speak English as a first language in order to minimize language barriers. Second, we considered only participants who answered basic demographic questions when they registered on Prolific. We had access to these answers and did not have to include basic demographic questions in the experiment. We posted the study in four separate sessions between April 21 and 26, 2021. The participation fee was set to £2.5. On average, participants completed the experiment in 30 minutes and earned £4 (including bonuses).

754 participants completed the experiment. As responses about beliefs are typically noisy, we restricted the sample for analysis based on a preregistered condition. Specifically, we compared the sign of belief changes and price changes before treatment assignment (period 1-4). We calculated the share of correct changes (that is, when the beliefs changed in the same direction as prices) and excluded participants who did not reach 50%. This procedure leaves us with a final sample of 525 participants[4], 272 in the *Choice* condition and 253 in the *Allocation* condition.

We report balance checks in Table 2.1. We expect no difference in learning in the first four periods because participants were assigned to treatment conditions after period 4. We compare belief changes normalized by the sign of the corresponding price change[5] and find no significant difference. In addition, the samples are balanced according to personal characteristics such as age, gender, education and income.

[4]Similar restrictions are common in other belief updating experiments. We exclude 30% of the sample, which is of similar magnitude to other studies (for example, 32% in Hartzmark et al. (2021)). We emphasize that the restriction is based on beliefs *before* treatment assignment, therefore it does not bias the inference about the effect of choice on beliefs.

[5]We can interpret this variable as how much participants changed their beliefs in the direction of the price change. This normalization allows us to pool observations for different price changes.

Table 2.1: Balance table

	Choice		Allocation		T-test	
Variable	N/[Clusters]	Mean/SE	N/[Clusters]	Mean/SE	Difference	p-value
Normalized belief change	4896	8.158	4554	8.596	-0.438	0.363
(period 1-4)	[272]	(0.335)	[253]	(0.345)		
Age	269	34.944	247	35.850	-0.906	0.401
		(0.723)		(0.799)		
Female	271	0.517	247	0.575	-0.058	0.184
		(0.030)		(0.032)		
Any degree	270	0.670	247	0.623	0.047	0.266
		(0.029)		(0.031)		
High income	251	0.450	224	0.504	-0.054	0.238
		(0.031)		(0.033)		

2.3 Reduced form results

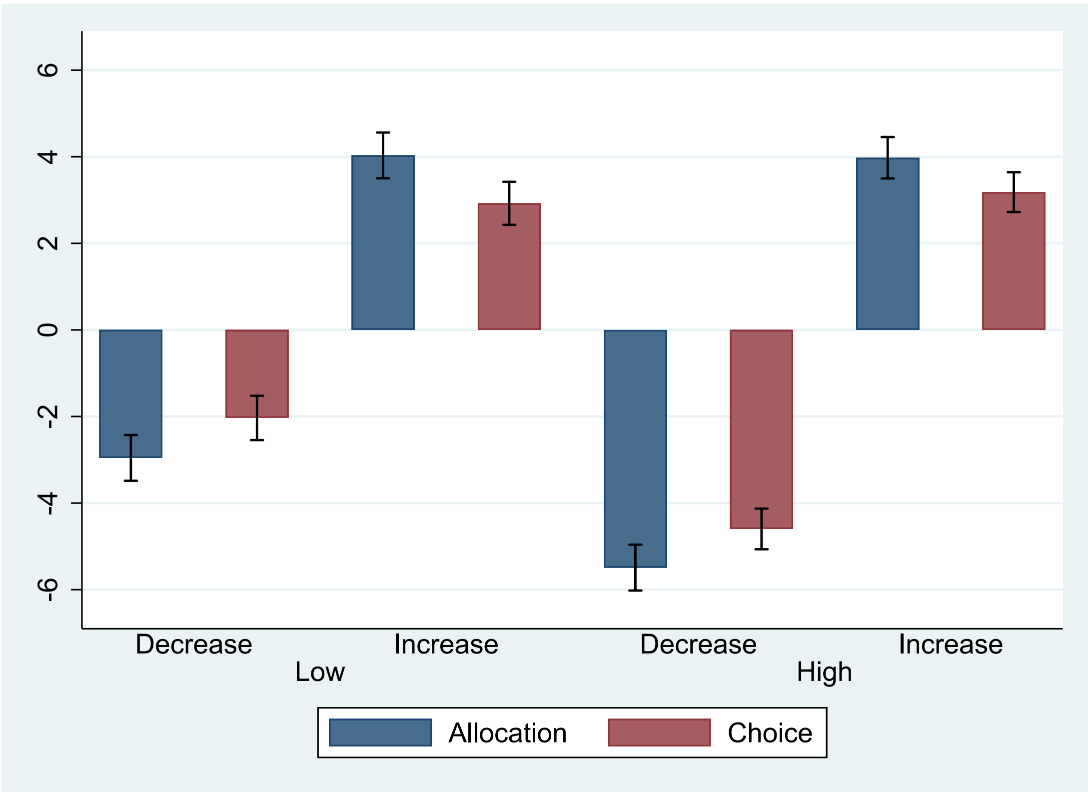
2.3.1 Learning

Our design allows us to distinguish between the above stated hypotheses by looking at how participants change their beliefs about own and non-owned investments after observing positive and negative signals. As investment choice is endogenous, we compare beliefs about *High* and *Low* investments instead of own and non-owned investments. Participants in the *Allocation* condition always receive the *High* investments, while participants in the *Choice* condition choose the *High* investments in most of the cases (82%). Figure 2.2 plots average belief changes by treatment conditions (*Choice* and *Allocation*), investment types (*High* and *Low*) and signals (price increases and price decreases). We include observations after treatment assignment (period 5-20).

We can make a couple of observations based on Figure 2.2. First, on average, participants update in the correct direction in both conditions. Beliefs are increasing after prices go up and decreasing after prices go down.

Second, our main result is that participants respond less in the *Choice* condition to both price increases and price decreases for both *High* investments and *Low* investments. That is, learning becomes stickier after making a choice compared to exogenous product allocation which

Figure 2.2: Average belief changes.



Notes: This figure reports average belief changes in percentage points by treatment condition (Allocation, Choice), investment type (Low, High) and price change (increase, decrease). We restrict the sample to period 5-20, thus we only use observations after participants received or chose the investments. The error bars correspond to 95% confidence intervals.

is consistent with Hypothesis 2.3. We can rule out motivated learning (Hypothesis 2.1) because the effect of choice on belief updating is not asymmetric. The observed pattern is also inconsistent with overlearning (Hypothesis 2.2), as inference from news is not stronger after making a choice. In addition, our results can not be explained by an ownership effect with differences in investment composition. Recall that participants in the *Allocation* condition always receive the *High* investments but participants in the *Choice* condition sometimes choose the *Low* investments. According to Hartzmark et al. (2021), exogenous ownership increases response to news. As a result, reaction for *High* investments is indeed expected to be smaller in the *Choice* condition, because some of these investments are not chosen. However, exogenous ownership also predicts stronger reaction for *Low* investments in the *Choice* condition because some of these investments are chosen. This prediction is not consistent with our data.

Finally, we can get a sense of the effect of exogenous ownership by comparing belief changes within the *Allocation* condition. After exogenous allocation, participants respond stronger to price decreases for own investments compared to non-owned investments, while responses to price increases are similar. This is broadly consistent with Hartzmark et al. (2021) who find stronger response to information for own investments than for non-owned investments. However, we emphasize that own and non-owned investments are different in our setting, therefore our finding cannot be viewed as a direct replication of their study.

We start the regression analysis by assessing the average difference in belief updating between the treatment conditions regardless of the investment type and signal. To do this, we normalize the change in beliefs by the sign of the corresponding price change. Thus, a positive value means that the belief is changed in the same direction as the price changed, while a negative value means a change in the opposite direction. We use the the normalized belief change as the dependent variable and pool observations by investment type and signal. The results are presented in Table 2.2 (similarly to Figure 2.2, we include observations between period 5-20). In Column 1, we estimate a regression with a constant and a dummy variable for the *Choice* condition. We find that participants change their beliefs by 4.1 percentage points on average in the *Allocation* condition and by 0.9 percentage points less in the *Choice* condition. The difference is near statistically significant at the 5 percent level ($p = 0.059$). The *Choice* coefficient is robust to adding fixed effects for lag belief intervals of 0-10, 11-20, ..., 91-100 (Column 2) and to round $\times$ investments fixed effects (Column 3).

Table 2.2: Difference in belief updating across treatment conditions

	(1)	(2)	(3)
Dependent variable: Normalized belief change			
Choice	-0.929*	-0.938*	-0.928*
	(0.491)	(0.489)	(0.490)
Constant	4.098***		
	(0.387)		
Observations	50400	50400	50400
R^2	0.001	0.001	0.020
Lag belief FE		Yes	Yes
Round $\times$ Investment FE			Yes

Notes: The unit of observation is participant $\times$ period $\times$ investment. The dependent variable belief change in percentage points normalized by the sign of the price change. We restrict the sample to period 5-20, thus we only use observations after participants received or chose the investments. In Column 1 we only include a dummy variable for the Choice treatment. In Column 2 we add fixed effects for a categorical variable splitting beliefs in the previous round into intervals of 0-10, 11-20, ..., 91-100. In Column 3 we also include round $\times$ investment fixed effects. Standard errors are in parenthesis and clustered at the individual level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Then, we expand the regression to estimate the difference between treatment conditions for all combinations of investment types and signals. Estimates lie between -0.79 to -1.11, indicating that learning is generally stickier in the *Choice* condition than in the *Allocation* condition regardless of the investment type and the current signal (see Table B.1 in the Appendix). We repeat the analysis by splitting investments based on ownership. Besides estimating the regressions by OLS, we also instrumenting ownership with *High* investments: participants receive *High* in the *Allocation* condition and mostly choose them in the *Choice* condition. Similarly to the the specification in which we compare learning about *High* and *Low* investments, the coefficients are negative and lie between -0.02 and -1.81 (see Table B.2 in the Appendix).

So far we have documented that participants respond less to information after making a choice in terms of average belief changes. Where does this difference come from? Participants in the *Choice* condition may change their beliefs less consistently with the direction of price changes or they may change their beliefs by less controlling for the direction of the belief change. We use an Oaxaca-decomposition to assess the importance of these two factors. Let Δy_c^1 denote the average consistent belief change in condition c, that is, when beliefs change in the same direction as prices. Then let Δy_c^0 denote the average inconsistent belief change in condition c, that is, when beliefs either change in the opposite direction than prices or remain the same. Finally, let ω_c denote the share of consistent belief changes in condition c. Then we can decompose the difference between conditions using the following formula:

$$\Delta y_C - \Delta y_A = \underbrace{\omega_C(\Delta y_C^1 - \Delta y_A^1) + (1-\omega_C)(\Delta y_C^0 - \Delta y_A^0)}_{Magnitude} + \tag{2.1}$$

$$+ \underbrace{(\omega_C - \omega_A)(\Delta y_A^1) + ((1-\omega_C) - (1-\omega_A))\Delta y_A^0)}_{Direction} \tag{2.2}$$

Table 2.3 presents the decomposition of the differences in normalized belief changes between treatment conditions by investment type and signal. Similarly to the regression above, negative values for the total difference indicate that participants in the *Choice* condition respond less to information. In all four cases, the difference is largely coming from differences in magnitude. Participants in the *Choice* condition update their beliefs with the same level of consistency as participants in the *Allocation* condition, but they change their beliefs less.

Table 2.3: Decomposition of the normalized belief change differences

Investment type	Low		High	
Price change	Decrease	Increase	Decrease	Increase
Magnitude	-1.01	-0.92	-0.76	-0.99
Direction	0.09	-0.18	-0.13	0.19
Total difference	-0.92	-1.11	-0.89	-0.79

2.3.2 Heterogeneous effects

To check whether the effect of choice on learning is different in different demographic groups, we split our sample by gender, age and education[6]. For age, we compare participants below and above the median age (33 years). For education, we compare participants who do not have any degree to participants with at least undergraduate degree. Figure 2.3 presents estimates for the differences in belief updating between treatment conditions[7]. We find that making a choice has somewhat larger impact on learning for females and for younger participants, but the differences in the effect sizes are not statistically significant ($p = 0.574$ and 0.448, respectively). We observe stronger heterogeneity when we divide the sample based on education. After making a choice, participants without a degree respond 1.8 percentage points less to price changes after making a choice ($p = 0.037$), while participants with at least undergraduate degree respond only 0.4 percentage points less ($p = 0.506$). However, the two estimates are not statistically significantly different from each other ($p = 0.189$).

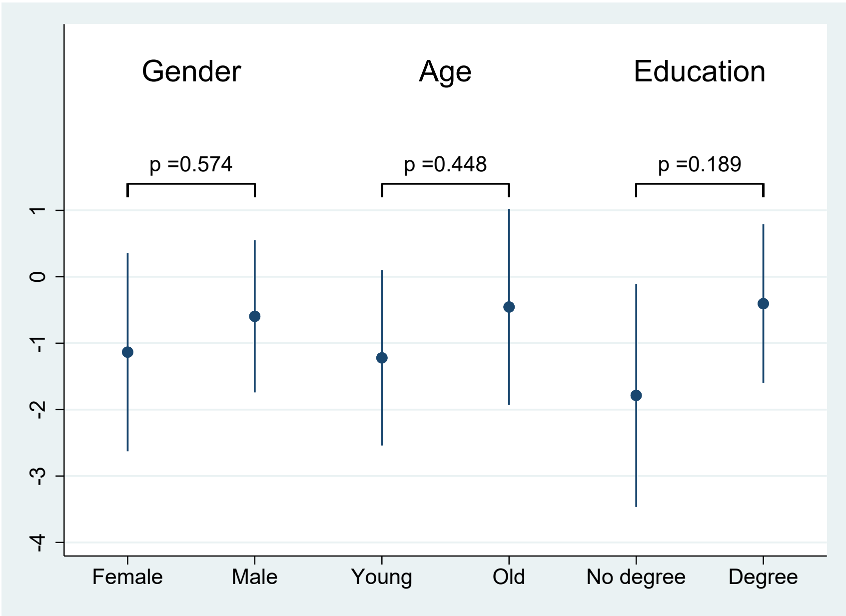
2.3.3 Attention

In this section we look at how much attention participants paid to the prediction tasks. Table 2.4 presents results using variables that aim to measure attention directly. In Column 1 and 2 the dependent variable is squared errors in the price and belief recall tasks, respectively. The coefficient estimates of the *Choice* dummy are virtually equal to zero. A potential concern with the recall questions is that they may have low power to detect any difference for various reasons. First, the questions concerned non-instrumental information. Second, participants' memory may have been overloaded (they performed 120 prediction tasks in total), especially because the questions appeared towards the end of the experiment. In fact, the constant term in Column 1 is only slightly below 0.5, indicating that participants recalled previous price changes only

[6] Prior to collecting the data, we did not have strong views about differences in learning between these groups. Rather, we ran these tests to see if they can guide future research questions. We pre-registered testing for gender differences.

[7] We report the corresponding regression outputs in the first columns of Table B.3-B.5 in the Appendix.

Figure 2.3: Choice effect estimates for different demographic groups



Notes: This figure presents results from regressions estimated on sub-samples based on gender, age and education. The unit of observation is participant × period × investment. The dependent variable is belief change in percentage points normalized by the sign of the price change. We restrict the sample to period 5-20, thus we only use observations after participants received or chose the investments. For each regression, we only include a constant and a dummy variable for the Choice treatment and plot the estimates for the Choice dummy with the 95% confidence intervals. The p-values refer to testing whether these estimates are equal to each other in the splitted samples.

slightly better than random guessing with equal chances[8]. In Column 3 and 4, we look at how much time participants spent on the choice/allocation and prediction screens, respectively. We find that making a choice did not require extra time and that it did not lead to more careful considerations in the prediction task.

Besides recall accuracy and time spent on screens, we also look at the reported beliefs to judge the consistency of predictions. We use several measures and find no difference in consistency (see Table B.6 in the Appendix), providing further support to the finding that participants paid the same amount of attention to the prediction tasks in the two conditions.

Similarly to the effect of choice on learning, we also examine whether making a choice has a heterogeneous effect on attention by gender, age and education. We present the regression outputs in Table B.3-B.5 in the Appendix. Using measures of recall accuracy and time spent on screens, we do not find an effect of choice on attention for any of the groups.

Table 2.4: Attention between treatment conditions

	Squared recall errors		Seconds spent on screen	
	(1)	(2)	(3)	(4)
	Price	Belief	Ownership	Predictions
Choice	0.00161	-1.501	-3.755	-3.515
	(0.0314)	(62.40)	(8.814)	(3.903)
Constant	0.451***	368.8***	43.44***	60.76***
	(0.0224)	(47.16)	(8.457)	(2.937)
Observations	1050	1050	525	8400
R^2	0.000	0.000	0.000	0.000

Notes: The unit of observation is participant × investment in Column 1 and Column 2, participant in Column 3 and participant × round in Column 4. We restrict the sample to period 5-20 in Column 4, thus we only use observations after participants received or chose the investments. Standard errors are in parenthesis and clustered at the individual level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

[8]The price recall question asked about a binary variable (whether the price went up or down in the previous period). Hence, we can interpret the constant as the share of incorrect answers in the *Allocation* condition. The belief recall question referred to a continuous variable, the share of exactly correct answers is approximately one quarter.

Recent research has found experimental evidence for selective memory. Gödker et al. (2021) show that individuals over-remember good news and under-remember bad news about their chosen asset. In contrast, individuals who received the same asset exogenously or only observed the outcomes, did not have this bias. We can also check if there is any systematic bias recalling previous signals and beliefs in our experiment. Table 2.5 reports the results. In Column 1, the dependent variable is a dummy variable which is equal to 1 if the participants reported that the price increased in the previous period and 0 otherwise. On the right hand side, we include a dummy for *High* investments and a dummy variable which is equal to 1 if the price increased in the current period and 0 otherwise. The coefficient of *High* is positive and significant, indicating that participants recall that previous signals are more likely to be positive about own investments than non-owned investments. Memory is also shaped by current signals, participants are more likely to report a previous price increase if the price has just increased in the current period. In Column 2, we interact the independent variables with the *Choice* dummy and find no difference between choice and allocation. In Column 3 and 4, we run the same regressions with the recalled beliefs as the dependent variable. We find that recalled beliefs are very close to actual previous beliefs on average, they are not affected by ownership or current price changes and there is no difference between choice and allocation.

In contrast to Gödker et al. (2021), we do not find evidence for selective memory about previous beliefs, nor when participants made a choice neither when they received the investments exogenously. An important difference from Gödker et al. (2021) is that they asked participants to remember signals and beliefs from a week before, while our participants had to recall signals and beliefs from the previous screen. Thus, there is much less room for distorting memory in our setting. We rather use these results to motivate the structural model in section 2.4 in which we assume that participants' recall of their previous beliefs is unbiased.

Table 2.5: Recalling previous signals and beliefs

	(1) Price	(2) Price	(3) Belief	(4) Belief
High	0.101**	0.137**	0.107	0.593
	(0.0432)	(0.0630)	(1.115)	(1.722)
High × Choice		-0.0679		-0.934
		(0.0866)		(2.247)
Increase	0.299***	0.283***	1.254	1.465
	(0.0440)	(0.0610)	(1.168)	(1.675)
Increase × Choice		0.0294		-0.421
		(0.0879)		(2.343)
Choice		0.0158		1.108
		(0.0855)		(2.287)
Constant	-0.235***	-0.244***	-0.298	-0.867
	(0.0427)	(0.0607)	(1.139)	(1.639)
Observations	1050	1050	1050	1050
R^2	0.048	0.048	0.001	0.001

Notes: The unit of observation is participant × investment in all four columns. The dependent variable is a dummy indicating that the previous price change is recalled to be a price increase in Column 1 and Column 2 and recalled previous belief in Column 3 and Column 4. Standard errors are in parenthesis and clustered at the individual level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

2.3.4 Discussion

Stickier learning without more attention

How can we reconcile that making a choice leads to stickier learning while there is no effect on attention? If participants pay more attention to the investments before the choice and/or at the time of the choice then they will be more confident and thus should respond less to new information. Note that according to this explanation, participants in the *Choice* condition behave rationally as long as they actually paid more attention before and/or during the choice. However, our design rules out differences in attention before the choice because choice is unexpected. In addition, participants do not spend more time on the *Choice* screen than on the *Allocation* screen, suggesting that they do not pay more attention to the investments during the choice. Overall, our results are not consistent with the explanation that participants respond less to information after the choice because they studied the products carefully before the choice. However, it is possible that making a choice and paying attention is associated strongly in people's mind. Thus they may (mistakenly) believe that they must have paid a lot of attention because they made a choice.

Real choice vs blind choice

An important feature of our design is that investments have different prices at the time of the choice, therefore participants make real, non-trivial decisions. On the contrary, in Hartzmark et al. (2021), participants pick investments before observing any price changes. That is, they make a blind choice from a set of identical elements. Hartzmark et al. (2021) argue that this procedure can be viewed as equivalent to exogenous random allocation. In fact, they run a version of the experiment where investments are randomly allocated to participants and they find no difference from the blind choice version. Combining these results indicate that in this context the mere act of choosing does not make a difference in learning. However, choices that are made after deliberation, do have an effect on belief updating.

An important distinction between real choice and blind choice is the reason why one can be proud of him/herself in case of good outcomes: luck or knowledge. On the one hand, if a blind choice turns out to be a good choice (a randomly picked product has high quality) then one can be proud of being lucky. On the other hand if a real choice turns out to be a good choice (an intentionally selected product has high quality) then one can be proud of being smart. As a result, motives related to one's ego may play a more important role for a real choice. While the current experiment does not provide evidence of which aspect of the choice process matters, the results can motivate future research about this question.

Preference-based inference

The evidence for the 'good-news, bad-news' effect is so far mixed: some papers find stronger inference from good news, some papers find stronger inference from bad news and some papers find symmetric inference (Benjamin, 2019). A candidate explanation for the heterogeneous findings is domain-specific learning: people may form beliefs differently about themselves, about others' behavior or about states with different monetary payments. Indeed, studies generally find that introducing financial stake does not lead to asymmetric belief updating (Gotthard-Real, 2017; Coutts, 2019a; Hartzmark et al., 2021; Barron, 2021). In line with these papers we also find a symmetric effect: making a choice leads to weaker inference from both good news and bad news for both own investments and non-owned investments.

2.4 Structural results

2.4.1 Model and estimation

In this section we build and estimate a structural model. Our main goal is to establish a Bayesian benchmark and investigate whether making a choice leads learning closer or further away from rational learning. In addition, the model will allow us to conduct counterfactual analysis in future work.

Suppose an individual's belief about the probability in period t follows a Beta(α_t, β_t) distribution. The Beta distribution seems to be the natural modeling choice because it is a distribution defined over probabilities. According to Bayesian updating, α_t and β_t track the number of good and bad signal realizations, respectively:

$$\alpha_t = \alpha_{t-1} + s_t \tag{2.3}$$
$$\beta_t = \beta_{t-1} + (1 - s_t), \tag{2.4}$$

where s_t is a dummy variable for a good signal realization.

In any period t, the mean belief is determined by prior beliefs α_0, β_0 and the number of good and bad signal realizations:

$$\hat{y}_t = \frac{\alpha_t}{\alpha_t + \beta_t} = \frac{\alpha_0 + \sum_{\tau=1}^{t} s_\tau}{\alpha_0 + \beta_0 + t} \tag{2.5}$$

We relax the Bayesian model by allowing for different weights on prior beliefs and current signals. Specifically, we model the law of motion of α_t and β_t as the following:

$$\tilde{\alpha}_t = (1 + x)\tilde{\alpha}_{t-1} + s_t \tag{2.6}$$
$$\tilde{\beta}_t = (1 + x)\tilde{\beta}_{t-1} + (1 - s_t) \tag{2.7}$$

The model reduces to Bayesian updating if $x = 0$. Learning is stickier than Bayesian if $x > 0$, as individuals put more weight on their previous belief and relatively less on current information. Finally, the model implies overlearning compared to Bayesian updating if $x < 0$, because individuals put less weight on their previous belief and relatively more on current information. We emphasize two important features of our model. First, this formulation assumes that the weight on previous beliefs applies equally to α_t and β_t. Second, in our model individuals recall previous beliefs in an unbiased way because they apply the same weighting scheme for the history of good news and bad news. However, they make a mistake in recalling how confident they were in their previous beliefs, overestimating their confidence if $x > 0$ and underestimating it if $x < 0$.

In this model, the mean belief is determined by the *discounted* sum of priors and signals. In the sticky learning case ($x > 0$) priors and early signals are more important, while in the overlearning case ($x < 0$) recent signals have a stronger influence.

$$\tilde{y}_t = \frac{\tilde{\alpha}_t}{\tilde{\alpha}_t + \tilde{\beta}_t} = \frac{(1+x)^t \alpha_0 + \sum_{\tau=1}^{t} (1+x)^{t-\tau} s_\tau}{(1+x)^t(\alpha_0 + \beta_0) + \sum_{\tau=1}^{t} (1+x)^{t-\tau}} \tag{2.8}$$

We estimate Equation 2.8 with Nonlinear Least Squares, using reported beliefs as the left hand side variable. As before, we include observations after ownership is formed (period 5-20). We estimate the coefficients separately for the two conditions. Table 2.6 reports estimates for the *Allocation* condition and the difference in the coefficients between the two conditions.

In Column 1, we estimate both x and the priors. We find that prior beliefs in the *Allocation* condition are not symmetric: participants are slightly more pessimistic ($\beta_0^A > \alpha_0^A$), the implied mean prior belief is 39.4%. As prior beliefs are formed before treatment assignment, it is reassuring that we find no significant difference in priors between the two conditions. x^A is estimated to be zero, indicating that learning in the *Allocation* condition is well described by Bayes-rule on average. However, x is significantly larger in the *Choice* condition: learning is *too sticky* after making a choice compared to the Bayesian benchmark. In the next two Columns we show that the results are robust to alternative assumptions on prior beliefs. We impose uniform priors in Column 2 ($\alpha_0 = \beta_0 = 1$) and substitute in priors estimated by Hartzmark et al. (2021) in Column 3 ($\alpha_0 = \beta_0 = 2.6$). We find the same results in both specifications: learning is Bayesian in the *Allocation* condition while it is stickier than Bayesian in the *Choice* condition.

2.4.2 Discussion

Instantaneous effect of choice on beliefs does not diminish with information

We show in our previous experiment that making a choice does affect beliefs at the time of the choice: participants who do not choose a product are more pessimistic about its value than participants who do not receive the same product. Our results in this paper imply that this difference is unlikely to disappear over time because participants are less willing to change their mind after making a choice.

Table 2.6: Belief updating relative to the Bayesian benchmark by treatment conditions

	(1)	(2)	(3)
Dependent variable: Belief			
Stickiness (x^A)	0.000760	-0.00108	-0.00855
	(0.00543)	(0.00631)	(0.00724)
Δ Stickiness ($x^C - x^A$)	0.0299***	0.0225**	0.0232**
	(0.00845)	(0.00887)	(0.0103)
Good news prior (α_0^A)	1.495***	1	2.62
	(0.254)	(.)	(.)
Bad news prior (β_0^A)	2.295***	1	2.62
	(0.249)	(.)	(.)
Δ Good news prior ($\alpha_0^C - \alpha_0^A$)	0.0438	0	0
	(0.356)	(.)	(.)
Δ Bad news prior $\beta_0^C - \beta_0^A$	-0.124	0	0
	(0.342)	(.)	(.)
N	50400	50400	50400
R^2	0.836	0.832	0.833
Prior	Estimated	Uniform	Hartzmark et al. (2021)

Notes: This table reports results from estimating Equation 2.8 with Nonlinear Least Squares. The unit of observation is participant $\times$ period $\times$ investment. The sample includes observations after treatment is assigned and ownership is determined (period 5-20). Standard errors are in parenthesis and clustered at the individual level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Low switching

There is overwhelming evidence that consumers often fail to switch to more favourable contracts or suppliers. Examples include markets for energy (Hortaçsu et al., 2017; Ito et al., 2017; Competition & Markets Authority, 2016), health insurance (Handel, 2013; Handel and Kolstad, 2015; Polyakova, 2016), credit cards (Shui and Ausubel, 2005; Stango and Zinman, 2015; Galenianos and Gavazza, 2021), paid TV (Shcherbakov, 2016), mobilephone services (Shy, 2002), auto insurance (Kiss, 2019), and mortgages (Keys et al., 2016; Andersen et al., 2020). Our finding offers the explanation that after choosing a supplier, consumers are less willing to update their beliefs about both the chosen contract and competing offers.

2.5 Conclusion

In this paper we study how making a choice influences belief updating. We employ an online experiment where participants learn about the fundamental quality of financial investments by observing price changes in multiple rounds. Using a between-subject design, we compare the beliefs of participants who choose the products for themselves (*Choice* condition) to participants who receive the products exogenously (*Allocation* condition). We find that choosing makes learning stickier compared to exogenous product allocation: participants respond less to both good news and bad news for both own and non-owned investments. We estimate a structural model and demonstrate that belief updating on average resembles closely the Bayesian benchmark after exogenous product allocation. On the other hand, we find excess stickiness in learning after making a choice.

Our findings may have interesting policy implications. Recent research documents that default policies are significantly less effective in the long run than in the short run. For example, Beshears et al. (2018) study the effect of automatic enrollment on retirement savings over a horizon of eight years. They find that withdrawals and borrowing against savings offset approximately 40% of the positive effect of automatic enrollment.

Our results suggest that active choice policies may be more effective policy tools than opt-out defaults.

In this study we provided participants with the same set of information in both treatment conditions to investigate how they process information. However, acquiring information is a decision to be made in most real life settings. An emerging literature documents that gathering or avoiding information is influenced by non-instrumental factors such as preferences, self-image concerns, etc. (Golman et al., 2017; Chen and Heese, 2021). As future work, it would be interesting to explore how making a choice affects information acquisition.

Chapter 3

Optimistic beliefs and market outcomes

3.1 Introduction

People tend to be optimistic about their own future. A realization of this general phenomena is that even if people are pessimistic about the average firm, they still tend to believe that they have managed to find a good one. For example, most survey respondents think that the financial industry as a whole can not be trusted, real estate agents have a low level of business ethics or most car mechanics would try to deceive them. At the same time, the vast majority of them think that their own financial adviser is trustworthy, their own real estate agent has a high level of business ethics or their own car mechanic would never try to deceive them.

In this paper I analyze the effect of optimistic belief distortions on market equilibrium. I extend the classical Hotelling model by allowing consumers to distort their beliefs about the quality of the products in the choice set. I show that if distorting beliefs is costly then consumers engage in wishful thinking: they become more optimistic about the product that they are more likely to buy. This increases perceived product heterogeneity and leads to higher equilibrium prices than the standard theory would predict. It implies that the safety in markets hypothesis does not hold in this model: competition itself does not eliminate the consequences of consumers biases. This finding is consistent with the observation that markups remain high in some industries with a large number of firms and fairly homogeneous products. My model identifies a novel channel through which product heterogeneity affects competition. In the standard models, competition is stronger if the products are less heterogeneous. However, if the products are similar then belief distortion is less costly: the consumers can pick any of the products and become very optimistic about the quality of the chosen one. As a result, perceived product heterogeneity increases and competition becomes weaker.

In the standard Hotelling model, consumers can choose between two products of the same quality, while they have a differential taste for each product (capturing product heterogeneity). Thus, if prices are equal, product choice is driven by the consumer's taste. This logic carries on to the case when the quality is stochastic, but it is expected to be identical across products. I extend the standard setup by allowing consumers to form subjective beliefs: consumers can choose a belief about each product's quality by considering the gains and losses from deviating from the rational beliefs[1]. I show that subjective beliefs are determined by product choice: optimistic beliefs about the chosen product increases the consumer's utility, while there is no incentive to be optimistic about non-chosen products. As a result, consumers become more optimistic about a product if they are more likely to purchase it.

The key driver of equilibrium outcomes in this model is the asymmetry of consumers' belief distortions about product qualities: they are more optimistic about the product that they are more likely to buy. As a result, perceived product heterogeneity increases because in addition to having different taste for the products, consumers also believe that they differ in quality. Higher perceived product heterogeneity decreases competition and therefore leads to higher equilibrium prices. I start solving the model in a special case when consumers observe their taste about the products. In this case, consumers can perfectly predict which product they will buy. Distorting beliefs about the other product is costly and does not increase subjective utility, therefore they only distort beliefs about the chosen product. This leads to strong asymmetry in the valuations: consumers who prefer product 0 based on their taste will be overoptimistic about the quality of product 0 while holding rational beliefs about the quality or product 1. Importantly, the magnitude

[1] I discuss the similarities and differences between my model and other models of motivated beliefs (Brunnermeier and Parker, 2005; Bénabou and Tirole, 2002) in subsection 3.2.5.

of overoptimism is constant, thus valuations as a function of taste become discontinuous. Consider two consumers whose taste differ only marginally, but the first one prefers product 0 while the second one prefers product 1. As the first consumer is overoptimistic about the quality of product 0 while the second one has rational beliefs, there is a non-marginal difference between their valuations about product 0. The discontinuity has in the valuation has severe consequences on competition: firms can charge the monopoly price. The key idea is that a marginal price decrease can not be a profitable deviation if both firms set the monopoly price: it would not attract any of the competitor's consumers because of the discontinuity in the valuations.

While the special case of observed taste already highlights the main mechanism of the model, it yields an implausibly stark prediction: firms have monopoly power even for a tiny bit of product differentiation. In the main specification of the model, consumers only observe a noisy signal about their taste. As a results, consumers who do not have a strong taste for either products, will purchase both products with positive probabilities. These consumers distort their beliefs about both products proportionally to the purchasing probabilities. As consumers generally assign different probabilities to buying different products, belief distortion will be asymmetric, perceived product heterogeneity increases and firms can still charge higher prices. Nevertheless, there will be no discontinuity in the valuations, thus competition does not disappear entirely.

In the standard models, the level of competition decreases in product heterogeneity: if the consumers have a stronger preference for one of the products, then firms will have more market power and can set higher prices. However, in this model, product heterogeneity also affects belief distortions. If the products are similar then belief distortion is less costly: the consumers can pick any of the products and become very optimistic about the quality of the chosen one. Even if the consumer ends up buying a product for which she has somewhat lower taste, optimistic beliefs about its quality will compensate the resulting utility loss. But if products differ from each other, then belief distortion becomes costly: sub-optimal product choice may lead to severe utility loss that is not compensated by optimistic beliefs about product quality. Thus, consumers distort beliefs more evenly across products. As a result, the asymmetry in belief distortions is decreasing in product heterogeneity.

It implies that, contrary to the standard effect, product heterogeneity increases competition trough this channel. For some parameter values, the two effects combine in a U-shape relationship between product heterogeneity and competition. Prices are high when the products are similar (because belief distortion is asymmetric) or when products are highly differentiated (standard effect) while prices are lowest for intermediate levels of product heterogeneity.

Belief distortion in the model is consistent with empirical evidence on optimistic belief formation about own future outcomes. In an experimental study, Mayraz (2013) asks participants to predict prices at which they will later trade. Some participants are randomly assigned to be "farmers" who benefit from high prices, while other participants are assigned to be "bakers" who benefit from low prices. Consistently with optimistic belief formation, Mayraz (2013) finds that after observing the exact same information, "farmers" forecast prices to be higher than "bakers". Coutts (2019b) ask participants to estimate the probability of various events and demonstrates that estimates are higher when participants have a financial stake in the occurrence of the event[2]. In Chapter 1 we do not observe a pure ownership effect after exogenous product allocation: participant do not report more optimistic beliefs about payoff probabilities of financial portfolios when they own them compared to when they do not own them. However, we also compare participants who choose the products to participants who receive the same products exogenously. We find that making a choice results in pessimism about non-chosen products compared to non-received products. Even in the absence of a pure ownership effect, this mechanism leads individuals to hold more optimistic beliefs about chosen products than about non-chosen products. From the perspective of competition between firms, the difference itself is important, while it does not matter whether it originates from optimism about chosen products or pessimism about non-chosen products.

This paper contributes to the theoretical literature on behavioral industrial organization that analyzes the effect of various consumer biases on competition. Many papers build models where naive consumers ignore some component(s) of the total price that they have to pay, including

[2]Optimistic belief distortions have also been documented for non-financial stakes. For example, Oster et al. (2013) find that people are optimistically biased about the risk of having Huntington Disease.

Gabaix and Laibson (2006), Spiegler (2006), Carlin (2009), Ellison and Ellison (2009), Armstrong and Vickers (2012), Chioveanu and Zhou (2013), Heidhues et al. (2016), Heidhues et al. (2017) and Heidhues et al. (2020). My model is closer to the approach of Gamp and Kraehmer (2018) who develop a search model in which firms choose the quality of their product and naive consumers mistakenly believe that all firms offer high quality products. The main difference is that in my model firms sell identical products, but consumers mistakenly believe that there is a difference in qualities.

The proposed mechanism helps explaining the observation that high prices and markups can persist in markets with many sellers and almost homogeneous products. Ausubel (1991) and Stango (2000) find that interest rates on credit cards have exceeded the cost of funding substantially. Hortaçsu and Syverson (2004) documents high markups in the mutual fund industry. Hastings et al. (2017) shows that prices are very high in the privatized market for social security in Mexico. While most consumers cannot perfectly evaluate the value of these products, they do not have strong reasons to believe that products are significantly different from each other, thus competition is expected to be strong. However, the observed high prices and markups are consistent with wishful thinking. If consumers convince themselves that their chosen product is better than the others, then competition will be weaker and prices and markups will be higher. This explanation is more likely to be important when the incentives to form optimistic beliefs are stronger and it is easier to maintain such beliefs. Many of important life decisions (choosing a university, a spouse or a job) fall into this category. As people make very few of these decisions, beliefs about these decisions are important and there are fewer opportunities to learn about the value of alternatives.

The paper proceeds as follows. I present the model in section 3.2. First, I solve a model in a special case when there is no uncertainty in the consumers' taste section 3.3. Then, I solve the general model and discuss the implications in section 3.4. Finally, I conclude in section 3.5.

3.2 Model

To model the effect of belief distortions on market outcomes, I extend the classical model of competition with horizontal differentiation (Hotelling, 1929). In the standard setup, there are two firms selling a single product to a unit mass of consumers with heterogeneous taste for each product. An important assumption of the classical setup is that consumers know the utility that each product delivers to them. In particular, the utility has the following form:

$$U = \begin{cases} v - t\theta - p_0 & \text{if } x = 0 \\ v - t(1-\theta) - p_1 & \text{if } x = 1 \end{cases} \tag{3.1}$$

where v denotes quality (identical across products), $\theta \in (0,1)$ captures the heterogeneous taste, p_i is the price and x is the product choice.

I depart from the classical setup in two important ways. First, I assume that quality v is unobserved and i.i.d. across firms. Importantly, consumers can deviate from rational expectations when forming beliefs about v but such deviation is costly in utility terms. Second, I assume that initially consumers don't observe θ. Instead, they only get a partially informative signal. Note that observed θ is a special case when the signal is perfect. While this assumption is not necessary for the main mechanism to operate, it is helpful in deriving results with plausible magnitudes.

3.2.1 Firms

There are two firms ($i = 0, 1$), producing a single product with quality v_i at marginal cost c. Quality v_i is random, distributed identically and independently across firms:

$$v_i = \begin{cases} v^L + \Delta v & \text{with prob. } \pi \\ v^L & \text{with prob. } 1-\pi \end{cases}, \tag{3.2}$$

where $v_L > 0$ and $\Delta v > 0$. Firms can neither affect the realization of v_i nor they can communicate it to the consumers. Firms maximize profit $\Pi_i = D_i(p_0, p_1)(p_i - c)$ by setting price p_i.

3.2.2 Consumers

Consumers buy one unit of the product from one of the firms ($x \in \{0, 1\}$). They have heterogeneous taste for the firms, captured by their type θ.

Consumers do not observe the quality of the products. Their prior beliefs are correct: for each product, they believe that the probability of high quality is π. As they do not receive any additional information, posterior beliefs should be equal to the priors. However, they can choose subjective beliefs π_i about the probability of high quality. Choosing such a belief is costly if deviating from the rational posterior π:

$$C(\pi_0, \pi_1) = \frac{b}{2}(\pi_0 - \pi)^2 + \frac{b}{2}(\pi_1 - \pi)^2 \tag{3.3}$$

In principle, beliefs about probabilities should lie in the (0,1) interval. However, enforcing this constraint would complicate the analysis because it would be effective for some consumers while not for others while it would not change the results qualitatively. Moreover, the fact that beliefs are about probabilities is not an essential part of the model. For example, in a model in which consumers distort their beliefs about the level of product quality would deliver similar results. Thus, for simplicity, I do not restrict beliefs π_i to be between 0 and 1.

Taken all these together, consumers' subjective utility is determined by four components: the subjective quality, the taste for the given product, the price and the cognitive cost of belief distortions. Let U_i denote the subjective utility if the consumer chooses product i:

$$U_i = v^L + \pi_i \Delta v - t|\theta - i| - p_i - C(\pi_0, \pi_1) \tag{3.4}$$

Type θ is initially unobserved, consumers only receive a noisy signal $\tilde{\theta} = \theta + e$. I assume that the joint distribution of type θ and noise e is an independent bivariate uniform distribution on $(0, 1) \times (-\varepsilon, \varepsilon)$. For simplification, I assume that consumers don't know that θ is between 0 and 1, that is I do not restrict posterior beliefs about θ to be between 0 and 1.

Similarly to Bénabou and Tirole (2016) I assume that consumers can choose an outside option $\underline{U} = 0$ which is available at the endpoints.

That is, consumers have to pay the same cost $t|\theta - i|$ as if they choose firm i. This assumption ensures that a change in t only affects the level of product differentiation while leaving the relative value of the outside option unchanged.

3.2.3 Timing

In period 1 consumers receive a realization of the signal $\tilde{\theta}$ about their type. Then they form expectation about the prices (p_i^e) and simultaneously choose product choice strategy $x(\theta)$ and subjective beliefs π_i. In period 2 firms set prices p_i. In period 3 consumers observe their type θ and prices p_i. They decide which firm to buy from (x).

The key timing assumption is that subjective beliefs are chosen in period 1 and they are no longer adjustable in period 3 when the uncertainty about type is resolved and prices are observed.

3.2.4 Equilibrium concept

I look for symmetric Perfect Bayesian Equilibria in pure strategies in which:

1. Consumers choose optimal subjective beliefs π_i given predicted prices (p_i^e) and product choice strategy $x(\theta)$.
2. Consumers choose optimal product choice strategy $x(\theta)$ given predicted prices p_i^e and subjective beliefs π_i.
3. Firms maximize profit given demand.
4. Consumers predict prices correctly: $p_i^e = p_i$

3.2.5 Remarks about the model

Belief distortion. The focus of this paper is to study the effect of optimistic belief distortion on competition. Thus, my goal is to employ a simple model of belief formation that generates optimism. Similarly to the optimal expectation framework of Brunnermeier and Parker (2005), individuals can choose beliefs directly. In Brunnermeier and Parker (2005), individuals trade off anticipatory benefits from holding optimistic beliefs and the material costs of these beliefs arising from sub-optimal decision making. As a result, the extent of optimism is constrained by the cost of distorted choices. However, in my model, overoptimistic beliefs do not necessarily lead to worse decisions because the set of possible actions is very coarse. If an individual chooses a certain product with rational beliefs, then becoming more optimistic about the product will not change the decision. Hence, optimism is not always constrained by the the cost of distorted choices.

In order to avoid infinite optimism, I introduce a direct utility cost of belief distortion. Similar approach is used for example by Bénabou and Tirole (2002). In their model, individuals can influence the probability of recalling a piece of information at some utility cost. They assume that the cost is zero at a "natural" rate of recall and becomes larger if individuals increase or decrease the recall rate. As in my model individuals choose beliefs directly, the cost depends on beliefs and not on the recall of previous information. Similarly to Bénabou and Tirole (2002), I assume that the cost is zero at some "natural" beliefs (equal to the rational beliefs) and becomes larger if individuals increase or decrease their beliefs.

Uncertainty. Consumers face three types of uncertainty that they handle in different ways. First, as a primary interest of this paper, the quality of the product remains unobserved in all periods. Consumers are allowed to deviate from rational beliefs and they do not learn any information that could potentially contradict their distorted beliefs. Second, consumers only observe a noisy signal about their type in period 1. They are assumed to infer their type according to Bayes rule (except that they do not know that type is between 0 and 1). Thus, in period 3, their actual type will be consistent with their expectations. Finally, consumers do not know prices in period 1, but they are assumed to understand firms' behavior and therefore they can predict prices correctly. In other words, price expectations are pinned down by the equilibrium.

3.3 Analysis with observed taste

The core of the model is how consumers choose their subjective beliefs. As a first step it is useful to consider the special case when θ is observed ($\varepsilon = 0$). Importantly, consumers can perfectly predict which product they will buy. Distorting beliefs about the other product is costly and does not increase subjective utility, therefore they only distort beliefs about the chosen product.

If the chosen product is i then the marginal utility of belief distortion is Δv and the marginal cost is $b(\pi_i - \pi)$. Thus, optimal subjective beliefs are

$$\pi_i^* = \pi + \frac{\Delta v}{b} \tag{3.5}$$

$$\pi_{-i}^* = \pi \tag{3.6}$$

In a symmetric equilibrium where firms set the same price, consumers with $\theta < \frac{1}{2}$ purchase product 0, consumers with $\theta > \frac{1}{2}$ purchase product 1 and consumers with $\theta = \frac{1}{2}$ are indifferent and randomize with equal probabilities. All consumers distorts beliefs only about the chosen product. Figure 3.1 plots the subjective utilities with optimal belief distortion. Consumers closer to 0 distort their beliefs only about product 0, while consumers closer to 1 distort their beliefs only about product 1.

The discontinuous change in the valuation of products has severe consequences on competition: firms can charge the monopoly price.

Proposition 3.1. *Suppose that θ is observed ($\varepsilon = 0$) and one of the following conditions is satisfied:*

1. $v^L + \pi\Delta v - c < t$
2. $v^L + \pi\Delta v - c \geq t$ *and* $\frac{\Delta v^2}{b} \geq \gamma$ *where* γ *depends on product characteristics (the rational expected value of quality, marginal cost of production and product heterogeneity) and is described in section C.1.*

Then, there exists an equilibrium in which both firms set the monopoly price:

$$p_0^* = p_1^* = p^M = v^L + \left(\pi + \frac{\Delta v}{b}\right)\Delta v \tag{3.7}$$

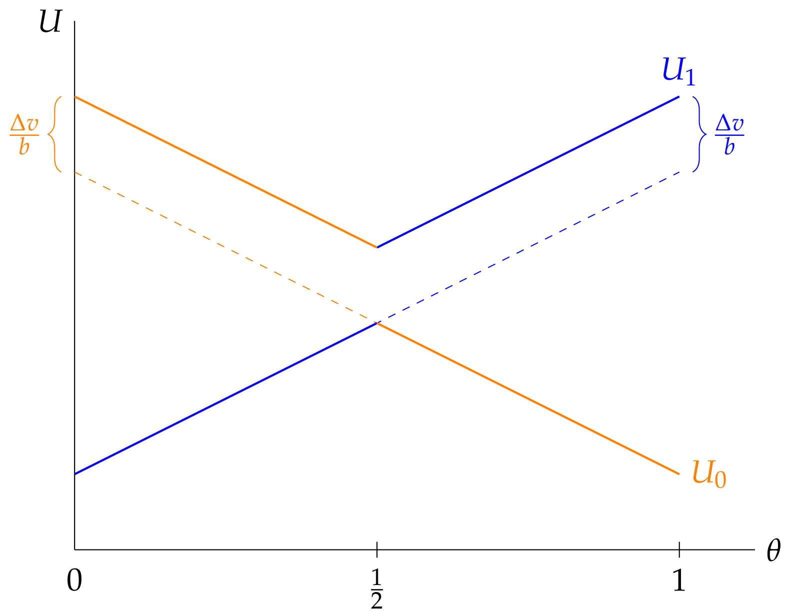


Figure 3.1: Subjective utility if θ is observed

The monopoly price is equal to the subjective belief about the chosen product's quality because consumers incur the cost of belief distortion and the cost of $t|\theta - i|$ even if they choose the outside option.

Consider a candidate equilibrium in which both firms set p^M. Increasing the price is not a profitable deviation because all of the firm's consumers would choose the outside option. A marginal decrease in the price is not a profitable deviation either: it would not attract any of the competitors consumers because of the discontinuity in the subjective utilities. It would, however, decrease the margin on the firm's own consumers. Finally, consider a price decrease that is large enough that some of the competitor's consumers decide to switch. The net effect of such a strategy is a sum of two effects: gain from attracting new consumers but loss from decreasing the margin on existing ones. It depends on the parameters whether such a price decrease is a profitable deviation. For example, if the utility functions are steep (t is large) or the belief distortion is large (Δv is high and/or b is small), then the price cut required to attract new consumers would lead too much loss on existing consumers [3]

In the standard Hotelling model, the equilibrium price is $c + t$. If the products are almost homogeneous (t is small) then firms compete intensively and prices are close to the marginal cost. However, with op-

[3]See section C.1 for the exact conditions.

timistic belief distortions, firms have monopoly power even for a tiny bit of product differentiation. While this result is implausibly stark, it highlights the main effect: consumers convince themselves about the high quality of the chosen product, perceive product differentiation to be higher and become less responsive to prices. The result is increasing market power for firms that leads to higher equilibrium prices.

3.4 Analysis of the general model with unobserved taste

Now I move on to solve the general model, when θ is not observed ($\varepsilon > 0$). I start by deriving the consumers' behavior.

3.4.1 Consumers - product choice

In period 3, consumers observe prices p_0 and p_1, their type θ and take subjective beliefs π_0 and π_1 as given. This means that they can calculate subjective utilities according to (3.4) and simply choose the product that gives higher subjective utility. Suppose that prices are the same ($p_0 = p_1 = p$)[4]. Then consumer buys product 0 if θ is less than a cutoff λ:

$$x^*(\theta) = \begin{cases} 0 \text{ if } \theta < \lambda(\pi_o, \pi_1) \\ 1 \text{ if } \theta > \lambda(\pi_o, \pi_1) \end{cases}$$

$$\lambda(\pi_o, \pi_1) = \frac{1}{2} + \frac{(\pi_0 - \pi_1)\Delta v}{2t}, \tag{3.8}$$

3.4.2 Consumers - belief distortion

In period 1, consumers choose subjective beliefs π_0 and π_1. They observe the signal $\tilde{\theta}$ and infer that $\theta \sim U[\tilde{\theta} - \varepsilon, \tilde{\theta} + \varepsilon]$. Using optimal product choice (Equation 3.8), consumers can calculate the probabilities of buying each product ($P_i(\pi_0, \pi_1)$)[5]. In addition, consumers can also predict the subjective utility conditional on buying product i:

$$U_i(\pi_0, \pi_1) = v^L + \pi_i \Delta v - t|\theta_i(\pi_0, \pi_1) - i| - p_i^e, \tag{3.9}$$

[4]I will verify later that this is indeed the case

[5]To simplify the notation I will omit that all expressions are conditional on $\tilde{\theta}$

where $\theta_i(\pi_0, \pi_1)$ is the expected value of θ conditional on buying product i and p_i^e is the expected price. Consumers maximize expected subjective utility by choosing beliefs:

$$U_0(\pi_0, \pi_1)P_0(\pi_0, \pi_1) + U_1(\pi_0, \pi_1)P_1(\pi_0, \pi_1) - C(\pi_0, \pi_1) \to \max_{\pi_0, \pi_1} \quad (3.10)$$

Based on the purchasing probabilities, there are three cases: the consumer buys product 0 for sure, buys product 1 for sure or buys both products with positive probabilities.

First, consider the case when the purchase cutoff is larger than the highest possible θ: $\lambda(\pi_0, \pi_1) \geq \tilde{\theta} + \varepsilon$. The consumer is certain that she will buy product 0, thus expected subjective utility reduces to $U_0(\pi_0, \pi_1) - C(\pi_0, \pi_1)$ with $\theta_0(\pi_0, \pi_1) = \tilde{\theta}$. As a result, optimal beliefs are:

$$\pi_0^* = \pi + \frac{\Delta v}{b} \quad (3.11)$$

$$\pi_1^* = \pi \quad (3.12)$$

Note that optimal beliefs are the same as in the model with observed type. As consumers can perfectly predict which product they are going to buy, they distort their beliefs fully about the chosen product and stick to rational beliefs about the non-chosen one.

The second case is when the purchase cutoff is less than the lowest possible θ and the consumer is certain that she will buy product 1. Similarly to the previous case, she distorts her belief fully about product 1 and not at all about product 0.

Finally, consider the case when the cutoff falls within the interval of possible realizations of θ. Figure 3.2 illustrates the purchasing probabilities and the conditional expected values of θ. It also highlights two additional channels through which beliefs influence subjective utility. First, increasing π_0 makes it more likely that the consumer will buy product 0. Second, increasing π_0 increases the expected value of θ conditional on buying product 0: as consumers become more optimistic about product 0, they will be willing to incur higher θ costs to purchase it.

Maximizing expected subjective utility (Equation 3.10) yields the follow-

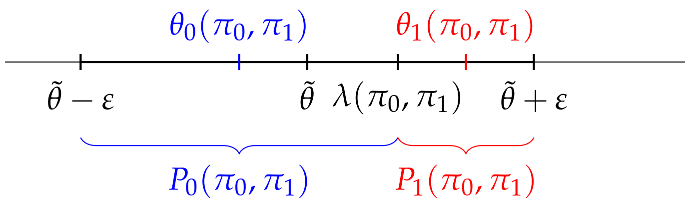


Figure 3.2: Purchasing probabilities and expected values of θ

ing beliefs:

$$\pi_0^* = \pi + \frac{\Delta v}{b}\left[\frac{1}{2} + \frac{bt}{2b\varepsilon t - \Delta v^2}\left(\frac{1}{2} - \tilde{\theta}\right)\right] \tag{3.13}$$

$$\pi_1^* = \pi + \frac{\Delta v}{b}\left[\frac{1}{2} + \frac{bt}{2b\varepsilon t - \Delta v^2}\left(\tilde{\theta} - \frac{1}{2}\right)\right] \tag{3.14}$$

I would like to highlight three characteristics of optimal beliefs. First, distortion about product 0 is decreasing in $\tilde{\theta}$. Consumers with a lower $\tilde{\theta}$ buy product 0 with higher probability, thus they have stronger incentives to distort their beliefs about it. Similarly, distortion about product 1 is increasing in $\tilde{\theta}$. Second, the total distortion is constant, consumers allocate the same amount of total distortion ($\frac{\Delta v}{b}$) between the two products. Finally, consumers in the middle ($\tilde{\theta} = \frac{1}{2}$) distort their beliefs symmetrically by $\frac{\Delta v}{2b}$.

Using optimal beliefs, we can calculate purchasing probabilities. It turns out that belief distortion and purchasing probabilities are proportional: belief distortion is equal to the full distortion ($\frac{\Delta v}{b}$) multiplied by the probability of buying the given product.

We can separate consumers based on which of the above cases they fall into. Low-signal consumers ($\tilde{\theta} \leq \tilde{\theta}_L$) will buy product 0 for sure. High-signal consumers ($\tilde{\theta} \geq \tilde{\theta}_U$) will buy product 1 for sure. Intermediate-signal consumers ($\tilde{\theta}_L < \tilde{\theta} < \tilde{\theta}_U$) will buy both products with positive probabilities. We can find the cutoffs by looking for $\tilde{\theta}$ such that optimal beliefs in (3.13) and (3.14) imply that the consumer buys either product 0 or product 1 for sure:

$$\tilde{\theta}_L = \frac{1}{2} - \left(\varepsilon - \frac{\Delta v^2}{2bt}\right) \tag{3.15}$$

$$\tilde{\theta}_U = \frac{1}{2} + \left(\varepsilon - \frac{\Delta v^2}{2bt}\right) \tag{3.16}$$

There are three important observations about the cutoffs. First, realizations of $\tilde{\theta}$ range from $-\varepsilon$ to $1+\varepsilon$. Thus, there will be always some consumers below $\tilde{\theta}_L$ and some consumers above $\tilde{\theta}_U$. Second, the distance between the cutoffs and $\frac{1}{2}$ is less than ε. Consider a consumer whose signal is just above $\frac{1}{2}-\varepsilon$. With rational beliefs she would buy product 1 with positive probability because there is some chance that her θ will slightly exceed $\frac{1}{2}$. However, choosing product 0 is more likely that leads to more optimistic belief about product 0 than about product 1. As a result, even when θ turns out to be higher than $\frac{1}{2}$, she will prefer product 0. Anticipating this, the consumer distort her belief fully about product 0 and sticks to the rational belief about product 1. Third, there are consumers who buy both products with positive probabilities if $\tilde{\theta}_L < \frac{1}{2} < \tilde{\theta}_U \iff \Delta v^2 < 2b\varepsilon t$. This condition is satisfied if there is little incentive for optimistic distortion about the chosen product (low Δv), belief distortion is costly (high b), type is uncertain (high ε) or taste is important (high t).

If $\Delta v^2 \geq 2b\varepsilon t$, the model becomes analogous to the model with observed taste (section 3.3). Consumers can perfectly predict product choice: they buy product 0 if $\tilde{\theta} < \frac{1}{2}$, buy product 1 if $\tilde{\theta} > \frac{1}{2}$ and randomize between the two products if $\tilde{\theta} = \frac{1}{2}$. As a result, they distort their beliefs fully about the chosen product and stick to rational beliefs about the non-chosen ones. This leads to a discontinuous jump in the expected subjective utility functions, similarly to Figure 3.1.

In the following analysis, I focus on the case when $\Delta v^2 < 2b\varepsilon t$. Figure 3.3 plots how belief distortions influence expected subjective utilities. As some consumers are uncertain about product choice, there is no discontinuous jump. Instead, belief distortions make the utility functions steeper in the range of intermediate $\tilde{\theta}$-s.

3.4.3 Firms - price setting

In period 2, firms maximize profit by taking into account how consumers formed beliefs in period 1 and how they will choose products in period 3. Recall that consumers observe a noisy signal about their type in period 1, but they learn their actual type before choosing in period 3. Thus, it is useful to group consumers by type and noise (Figure 3.4). Con-

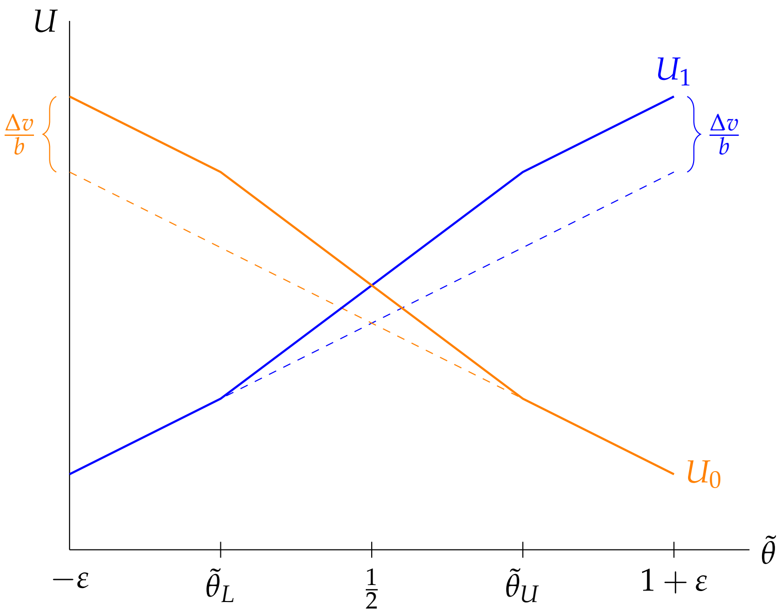


Figure 3.3: Expected subjective utility if $\Delta v^2 < 2b\varepsilon t$

sumers with low type and/or low noise realization observe low signals and distort beliefs only about product 0. They are located on the left of $\hat{e}_0(\theta)$. Consumers with high type and/or high noise realization observe high signals and distort beliefs only about product 1. They are located on the right of $\hat{e}_1(\theta)$. Finally, intermediate-signal consumers distort beliefs about both products and are between $\hat{e}_0(\theta)$ and $\hat{e}_1(\theta)$.

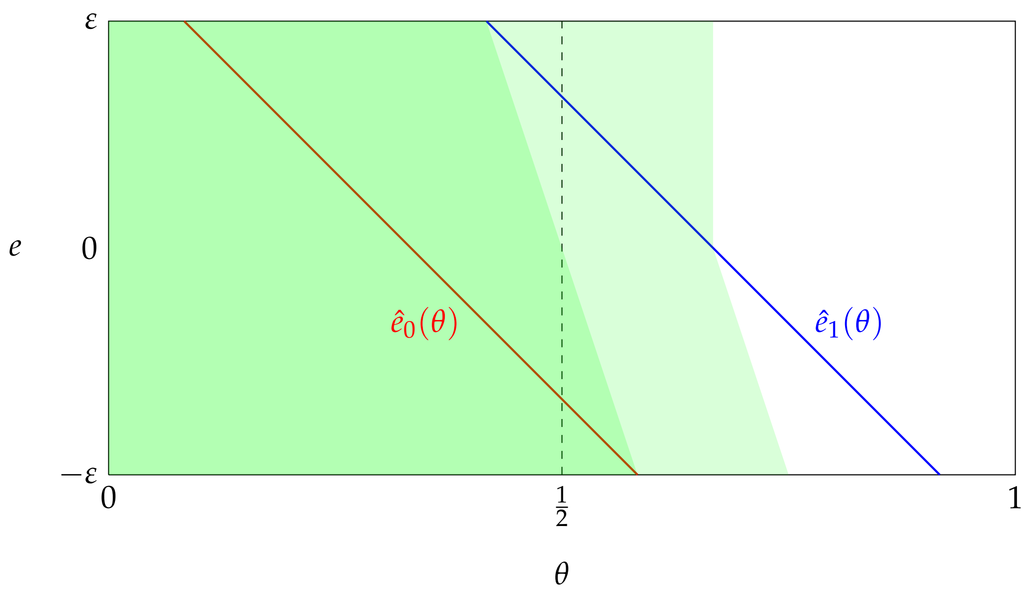


Figure 3.4: Demand for product 0 as a function of type θ and noise e

When firms set their prices, they take into account how consumers in each group will respond to the prices given their distorted beliefs. If the firms offer the same prices then the market is split equally. The darker green are indicates that all low-signal consumers and half of the intermediate-signal consumers choose product 0. Contrary to the model with observed taste, product choice is not necessarily in line with θ. There are consumers with $\theta > \frac{1}{2}$ who observe a low $\tilde{\theta}$, become optimistic about product 0 and decide to buy it. On the flip side, there are consumers with $\theta < \frac{1}{2}$ who observe a high $\tilde{\theta}$, become pessimistic about product 0 and decide not to buy it.

What happens if firm 0 decreases the price to make its product more attractive? Low-signal consumers will still prefer product 0, while some of the intermediate- and high-signal consumers will switch from the other product. The additional demand is represented by the light dark area in Figure 3.4. Note that the realization of e does not affect the product choice of high-signal consumers because they distort their beliefs independently from $\tilde{\theta}$. As a result, the boundary of the new demand is vertical above $\hat{e}_1(\theta)$.

We can derive the equilibrium prices by solving the firms' profit maximization problems (see section C.2 for the details).

Proposition 3.2. *If $\Delta v^2 < 2b\varepsilon t$ and $\Delta v^2 < bt$, then there exist an equilibrium in which some consumers distort beliefs about both products and firms set the same prices:*

$$p_0^* = p_1^* = p^* = c + t\left(1 + \frac{\Delta v^2}{2b\varepsilon t - \Delta v^2}\right) \tag{3.17}$$

The first condition ensures that there are some consumers who distort beliefs about both products. The second condition simplifies the analysis[6], the mechanism of the model remains the same if it is not satisfied.

3.4.4 Discussion

An important implication of Proposition 3.2 is that the equilibrium price is higher than in the standard Hotelling model. As consumers allocate their optimism towards the product that they are more likely to buy, they

[6]Specifically, it ensures that $\hat{e}_0(\theta)$ crosses the horizontal axis between $\frac{1}{2}$ and 1 in Figure 3.4.

perceive product differentiation to be higher that leads to weaker competition. The safety in market hypothesis does not hold in this model: competition in itself can not eliminate the effect of consumer biases.

Proposition 3.2 yields several comparative static predictions. First, the equilibrium price is decreasing in the distortion cost b and increasing in the uncertainty about product quality Δv. Both parameters affect consumers' incentives to distort beliefs directly.

Second, the equilibrium price is decreasing in the uncertainty about type (ε). When consumers can predict product choice less accurately (ε is high), then the purchasing probabilities are more balanced. As a result, belief distortion is also less asymmetric. However, if consumers are more confident in which product they are going to choose (ε is low), then they assign a high probability to purchasing one of the products. In this case, belief distortion is more asymmetric.

Third, product heterogeneity t affects the equilibrium price in two different channels. There is the standard effect: competition is stronger if products are less heterogeneous. However, if products are similar, then the indirect cost of belief distortion (choosing the "wrong" product) is low. In this case, it is worth becoming very optimistic about one of the products that leads to more asymmetric belief distortion and weaker competition. In this model, the two channels lead to a U-shaped effect: the asymmetric belief distortion dominates for small t and the standard effect of product heterogeneity dominates for large t. As a consequence, the model predicts competition to be the strongest for intermediate levels of product heterogeneity.

Proposition 3.3. *Suppose the conditions in Proposition 3.2 are satisfied. Then, equilibrium price p^* is decreasing in t if $t \leq \tau$ and increasing in t if $t \geq \tau$ where $\tau = \frac{\Delta v^2}{b\varepsilon}$.*

3.5 Conclusion

In this paper, I developed a model with endogenous belief distortions to study its effect of competition. As future research, the current model can be extended in various directions to provide additional predictions.

First, in many markets, consumers do not only choose a product, they also decide how much to buy from it. Including consumers' decision on quantity could potentially enrich the mechanism of belief distortion: optimism may increase the purchase quantity that in turn would lead to even more optimistic beliefs. Second, optimistic belief distortions can be introduced in the Salop-model to study its effect on the number of firms. As optimistic belief distortions lead to higher markups compared to the in the standard model, we can expect that more firms would enter to such a market.

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Appendix A

Appendix for Chapter 1

A.1 Portfolio characteristics

Table A.1: Portfolios

N	K	Type	N_A	Payoff probability
6	3	Benchmark		0.456
		Low	1	0.317
		Medium	2	0.385
		High	4	0.526
5	3	Benchmark		0.317
		Low	2	0.279
		Medium	3	0.350
		High	4	0.425
4	2	Benchmark		0.525
		Low	1	0.437
		Medium	3	0.613
		High	4	0.688
3	1	Benchmark		0.784
		Low	0	0.657
		Medium	1	0.755
		High	2	0.825

Notes: In the Benchmark portfolios firms are randomly selected regardless of their industry.

A.2 Balance table

Table A.2: Balance table

	(1) Allocation		(2) Choice		(3) Relative choice		(4) Ego choice		T-test Difference					
Variable	N	Mean/SE	N	Mean/SE	N	Mean/SE	N	Mean/SE	(1)-(2)	(1)-(3)	(1)-(4)	(2)-(3)	(2)-(4)	(3)-(4)
Age	361	32.626	340	31.929	143	33.147	148	32.446	0.697	-0.521	0.180	-1.217	-0.517	0.701
		(0.621)		(0.658)		(0.982)		(0.994)						
Female	361	0.496	339	0.510	143	0.441	148	0.534	-0.014	0.055	-0.038	0.070	-0.023	-0.093
		(0.026)		(0.027)		(0.042)		(0.041)						
Any degree	362	0.588	340	0.526	143	0.545	148	0.574	0.062*	0.043	0.014	-0.019	-0.048	-0.029
		(0.026)		(0.027)		(0.042)		(0.041)						
High income	339	0.448	313	0.396	133	0.383	138	0.341	0.052	0.065	0.108**	0.013	0.056	0.043
		(0.027)		(0.028)		(0.042)		(0.040)						
Mistakes	362	0.448	340	0.391	143	0.378	148	0.331	0.056	0.070	0.116	0.014	0.060	0.047
		(0.072)		(0.065)		(0.083)		(0.069)						
Puzzle correct	362	2.630	340	2.638	143	2.650	148	2.669	-0.008	-0.021	-0.039	-0.012	-0.031	-0.019
		(0.034)		(0.034)		(0.054)		(0.051)						
Puzzle confidence	362	79.166	340	78.550	143	79.056	148	82.115	0.616	0.110	-2.949	-0.506	-3.565	-3.059
		(1.323)		(1.382)		(2.103)		(1.872)						

Notes: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

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A.3 Additional results

Table A.3: Number of correct answers for the economics-related questions.

Correct solutions	No.	Col %	Cum %
0	10	1.0	1.0
1	55	5.5	6.5
2	216	21.8	28.3
3	712	71.7	100.0
Total	993	100.0	

Table A.4: Manipulation check questions

	(1) Focusing on comparison	(2) Feeling proud for good choice
Allocation	-0.646***	
	(0.118)	
Delayed Choice	-0.217	0.159
	(0.155)	(0.134)
Ego Choice	0.147	-0.140
	(0.153)	(0.133)
Constant	4.853***	4.715***
	(0.0845)	(0.0732)
Observations	993	631
R^2	0.041	0.006
Choice vs Delayed Choice p-value	0.16	0.24
Choice vs Ego Choice p-value	0.34	0.29

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix B

Appendix for Chapter 2

B.1 Price paths

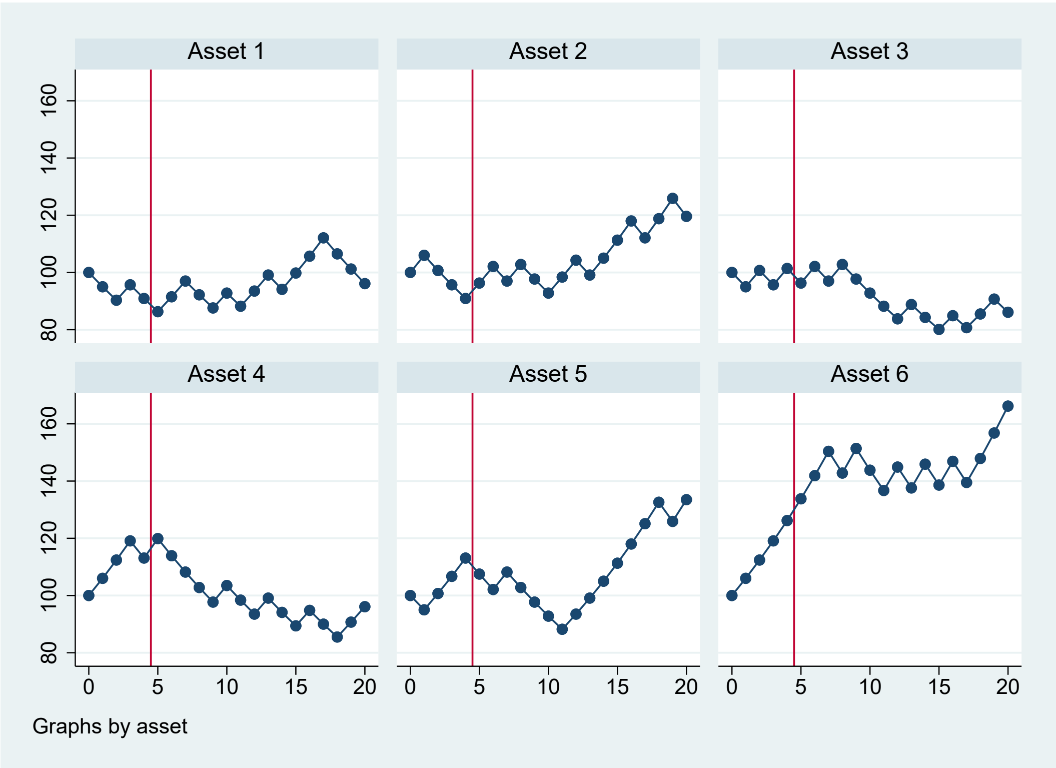

Figure B.1: Price paths

B.2 Additional results

Table B.1: Difference in belief updating between treatment conditions by investment type and current signal

	(1)	(2)	(3)
Dependent variable: Normalized belief change			
Price decrease	2.957***	-1.017***	
	(0.443)	(0.127)	
Price decrease × High	2.534***	2.668***	
	(0.302)	(0.322)	
Price decrease × Low × Choice	-0.924	-0.937	-0.926
	(0.601)	(0.600)	(0.600)
Price decrease × High × Choice	-0.894*	-0.890*	-0.885*
	(0.494)	(0.491)	(0.493)
Price increase	4.031***		
	(0.435)		
Price increase × High	-0.0540	0.124	
	(0.275)	(0.294)	
Price increase × Low × Choice	-1.107*	-1.102*	-1.091*
	(0.581)	(0.577)	(0.578)
Price increase × High × Choice	-0.794	-0.813*	-0.812*
	(0.494)	(0.491)	(0.492)
Observations	50400	50400	50400
R^2	0.033	0.003	0.020
Lag belief FE		Yes	Yes
Round × Asset FE			Yes

Notes: The unit of observation is participant × period × investment. The dependent variable is belief change in percentage points normalized by the sign of the price change. We restrict the sample to period 5-20, thus we only use observations after participants received or chose the investments. In Column 1 we only include dummy variables for treatment condition, investment type and price change and their interactions. In Column 2 we add fixed effects for a categorical variable splitting beliefs in the previous round into intervals of 0-10, 11-20, ..., 91-100. In Column 3 we also include round × investment fixed effects. Standard errors are in parenthesis and clustered at the individual level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table B.2: Difference in belief updating between treatment conditions by ownership and current signal

Dep. var.: Normalized belief change	(1) OLS	(2) OLS	(3) OLS	(4) IV	(5) IV	(6) IV
Price decrease	2.957***	-1.023***		2.957***	2.214**	
	(0.443)	(0.127)		(0.442)	(0.960)	
Price decrease × Own	2.534***	2.626***	-0.00346	2.534***	2.708***	2.717***
	(0.302)	(0.322)	(0.579)	(0.302)	(0.323)	(0.327)
Price decrease × Not own × Choice	-0.385	-0.392	-0.937	-1.732***	-1.805***	-1.802***
	(0.594)	(0.593)	(0.607)	(0.672)	(0.676)	(0.678)
Price decrease × Own × Choice	-1.470***	-1.477***	-0.874*	-0.0899	-0.0248	-0.0230
	(0.510)	(0.508)	(0.497)	(0.524)	(0.524)	(0.524)
Price increase	4.031***			4.031***		
	(0.435)			(0.434)		
Price increase × Own	-0.0540	0.0770	-0.472	-0.0540	0.169	0.212
	(0.275)	(0.294)	(0.556)	(0.275)	(0.295)	(0.300)
Price increase × Not own × Choice	-0.825	-0.819	-1.041*	-1.174*	-1.221**	-1.226**
	(0.582)	(0.579)	(0.586)	(0.625)	(0.623)	(0.622)
Price increase × Own × Choice	-1.068**	-1.088**	-0.864*	-0.726	-0.691	-0.682
	(0.497)	(0.494)	(0.492)	(0.516)	(0.513)	(0.515)
Observations	50400	50400	50400	50400	50400	50400
R^2	0.033	0.003	0.020	0.031	0.032	0.035
Lag belief FE		Yes	Yes		Yes	Yes
Round × Asset FE			Yes			Yes

Notes: The unit of observation is participant × period × investment. The dependent variable is belief change in percentage points normalized by the sign of the price change. We restrict the sample to period 5-20, thus we only use observations after participants received or chose the investments. In Column 1 and Column 4 we only include dummy variables for treatment condition, ownership and price change and their interactions. In Column 2 and Column 5 we add fixed effects for a categorical variable splitting beliefs in the previous round into intervals of 0-10, 11-20, ..., 91-100. In Column 3 and Column 6 we also include round × investment fixed effects. Standard errors are in parenthesis and clustered at the individual level. We estimate Column 1, Column 2 and Column 3 by OLS and Column 4, Column 5 and Column 6 by instrumenting Own with High and Not own with Low. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table B.3: Difference in belief updating and attention between treatment conditions by gender

	Belief change	Squared recall errors		Seconds spent on screen	
	(1)	(2)	(3)	(4)	(5)
	Normalized	Price	Belief	Ownership	Predictions
Female	4.283***	0.468***	446.2***	45.96***	62.71***
	(0.611)	(0.0288)	(70.30)	(13.84)	(3.693)
Female × Choice	-1.134	-0.0469	-66.58	-8.593	-3.607
	(0.760)	(0.0410)	(88.99)	(14.43)	(5.200)
Male	3.805***	0.438***	271.3***	40.76***	57.78***
	(0.424)	(0.0368)	(60.58)	(8.108)	(4.980)
Male × Choice	-0.596	0.0428	85.41	1.444	-2.284
	(0.583)	(0.0492)	(86.46)	(8.567)	(6.153)
Observations	49728	1036	1036	518	8288
R^2	0.031	0.454	0.144	0.153	0.279
P-value	0.574	0.162	0.221	0.55	0.87

Notes: Standard errors are in parenthesis and clustered at the individual level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table B.4: Difference in belief updating and attention between treatment conditions by age

	Belief change	Squared recall errors		Seconds spent on screen	
	(1)	(2)	(3)	(4)	(5)
	Normalized	Price	Belief	Ownership	Predictions
Young	4.283***	0.449***	370.7***	41.12***	61.78***
	(0.494)	(0.0303)	(64.21)	(6.433)	(4.055)
Young × Choice	-1.221*	-0.00530	14.74	4.575	-0.362
	(0.672)	(0.0436)	(89.75)	(7.909)	(5.857)
Old	3.813***	0.459***	372.8***	46.93***	58.73***
	(0.633)	(0.0342)	(73.00)	(17.75)	(4.171)
Old × Choice	-0.455	-0.00689	-19.35	-13.21	-5.331
	(0.751)	(0.0460)	(90.81)	(17.86)	(5.143)
Observations	49536	1032	1032	516	8256
R^2	0.032	0.451	0.140	0.154	0.280
P-value	0.448	0.980	0.790	0.36	0.52

Notes: Standard errors are in parenthesis and clustered at the individual level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table B.5: Difference in belief updating and attention between treatment conditions by education

	Belief change	Squared recall errors		Seconds spent on screen	
	(1)	(2)	(3)	(4)	(5)
	Normalized	Price	Belief	Ownership	Predictions
Nodegree	4.832***	0.457***	376.4***	30.84***	64.87***
	(0.684)	(0.0386)	(85.14)	(2.707)	(4.895)
Nodegree × Choice	-1.786**	-0.00244	44.80	7.127*	-9.996
	(0.855)	(0.0550)	(115.2)	(3.685)	(6.327)
Degree	3.626***	0.455***	369.1***	51.55***	58.05***
	(0.476)	(0.0281)	(57.82)	(13.78)	(3.786)
Degree × Choice	-0.405	-0.00427	-24.13	-11.46	0.594
	(0.609)	(0.0387)	(75.37)	(14.22)	(5.039)
Observations	49536	1032	1032	516	8256
R^2	0.031	0.454	0.141	0.155	0.278
P-value	0.189	0.978	0.617	0.21	0.19

Notes: Standard errors are in parenthesis and clustered at the individual level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table B.6: Difference in consistency of beliefs between treatment conditions

	Belief changes		Corner beliefs		Standard deviatons	
	(1)	(2)	(3)	(4)	(5)	(6)
	Any	Consistent	Belief 0	Belief 100	Belief	Belief change
Choice	0.0113	0.0000792	0.00806	0.00135	-0.258	-0.438
	(0.0168)	(0.0144)	(0.00653)	(0.00115)	(0.628)	(0.940)
Constant	0.813***	0.545***	0.0156***	0.00157**	19.15***	17.91***
	(0.0122)	(0.0103)	(0.00370)	(0.000716)	(0.458)	(0.693)
Observations	50400	50400	50400	48825	525	525
R^2	0.000	0.000	0.001	0.000	0.000	0.000

Notes: In Column 1, we look at whether the belief changes from the previous period in any direction. As prices always change, a constant beliefs is by all means inconsistent. In Column 2, we check whether beliefs change in the same direction as prices. In Column 3 (and 4), we use beliefs indicating that prices never go up (down) despite price increases (decreases) have already occurred. Finally, in Column 5 and 6, we compare the standard deviations of beliefs and belief changes, respectively. Standard errors are in parenthesis and clustered at the individual level. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

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B.3 Instructions

Figure B.2: Example prediction screen

This is Period 5. You can see price movements for each investment in the tables. The first table shows all prices, while the second table shows the current prices.

	Period 0	Period 1	Period 2	Period 3	Period 4	Period 5
Price investment 1	100.0	95.0	90.3	95.7	90.9	86.3
Price investment 2	100.0	106.0	112.4	119.1	113.1	119.9
Price investment 3	100.0	106.0	100.7	95.7	90.9	96.3
Price investment 4	100.0	106.0	112.4	119.1	126.2	133.8
Price investment 5	100.0	95.0	100.7	106.7	113.1	107.5
Price investment 6	100.0	95.0	100.7	95.7	101.4	96.3

	Investment 1	Investment 2	Investment 3	Investment 4	Investment 5	Investment 6
Price	86.3	119.9	96.3	133.8	107.5	96.3
Your investments		Yes		Yes	Yes	

How likely it is that the different investments will go up in the next period? Use the sliders to predict the probabilities.

Investment 1	0
Investment 2	0
Investment 3	0
Investment 4	0
Investment 5	0
Investment 6	0

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Notes: The 'Your investments' row in the second table is displayed only in Stage 3 (period 5-20).

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Figure B.3: Example choice screen

From this period you will own 3 of the 6 investments. You will keep the selected investments until the end of the experiment and earn additional bonus payment based on their final price. The total points of your investments will be converted at an exchange rate of:

200 experimental points = £0.50

Indicate which investments you choose in the table below. You can select exactly 3 investments.

	Investment 1	Investment 2	Investment 3	Investment 4	Investment 5	Investment 6
Price	113.1	113.1	90.9	126.2	90.9	101.4
Your investments	☐	☐	☐	☐	☐	☐

Click on the Next button to continue with the predictions.

Next

Figure B.4: Example allocation screen

From this period you will own 3 of the 6 investments. You will keep the selected investments until the end of the experiment and earn additional bonus payment based on their final price. The total points of your investments will be converted at an exchange rate of:

200 experimental points = £0.50

The computer selected the 3 highest price investments for you. You can see the selected investments in the table below:

	Investment 1	Investment 2	Investment 3	Investment 4	Investment 5	Investment 6
Price	113.1	90.9	126.2	90.9	113.1	101.4
Your investments	Yes		Yes		Yes	

Click on the Next button to continue with the predictions.

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Appendix C

Appendix for Chapter 3

C.1 Supplement to the proof of Proposition 3.1

Consider the candidate equilibrium where both firms set the monopoly price. Here I derive the conditions under which a large price decrease is not a profitable deviation. The difference between U_0 and U_1 at $\theta = \frac{1}{2}$ is $\frac{\Delta v^2}{b}$, firms have to decrease the price by a larger amount to attract consumers from the competitor. Equivalently, as firms trying to attract consumers who has rational beliefs about their product, they have to offer a price below it. Suppose firm i set the following price:

$$p' = p^M - \left(\frac{\Delta v^2}{b} + \Delta p\right) = v^L + \pi\Delta v - \Delta p. \tag{C.1}$$

Changing the price firm p^M to p' shifts U_i upwards and attracts a mass of $\frac{\Delta p}{2t}$ consumers from the competitor. As the total mass of the competitor's consumers is $\frac{1}{2}$: $\Delta p \in [0, t]$. The net effect on the profit is a sum of two effects: attracting new consumers and imposing a smaller margin on existing ones:

$$\begin{aligned}\Delta\Pi_i(\Delta p) &= \frac{\Delta p}{2t}(v^L + \pi\Delta v - \Delta p - c) - \frac{1}{2}\left(\frac{\Delta v^2}{b} + \Delta p\right) \\ &= -\frac{\Delta p^2}{2t} + \left(\frac{v^L + \pi\Delta v - c - t}{2t}\right)\Delta p - \frac{\Delta v^2}{2b}\end{aligned} \tag{C.2}$$

Thus, p' is not a profitable deviation if the maximum of $\Delta\Pi_i(\Delta p)$ for $\Delta p \in [0, t]$ is negative. The expression for $\Delta\Pi_i(\Delta p)$ is a quadratic function, it has an inverted U-shape. First, I look for the unconstrained maximum, which is attained at:

$$\Delta p^* = \frac{v^L + \pi\Delta v - c - t}{4} \tag{C.3}$$

Then, there are three cases:

1. $\Delta\Pi_i(\Delta p)$ is decreasing in the $[0, t]$ interval. This is the case if

$$\Delta p^* \leq 0 \iff v^L + \pi\Delta v - c \leq t \tag{C.4}$$

   Profit change is maximized at 0 and $\Delta\Pi_i(0)$ is always negative:

$$\Delta\Pi_i(0) = -\frac{\Delta v^2}{2b} < 0 \tag{C.5}$$

2. $\Delta\Pi_i(\Delta p)$ is increasing in the $[0, t]$ interval. This is the case if

$$\Delta p^* \geq t \iff v^L + \pi\Delta v - c \geq 5t \tag{C.6}$$

   Profit change is maximized at t and $\Delta\Pi_i(t)$ is negative if

$$\frac{\Delta v^2}{b} > v^L + \pi\Delta v - c \tag{C.7}$$

3. $\Delta\Pi_i(\Delta p)$ has an inverted U-shape in the $[0, t]$ interval. This is the case if

$$0 < \Delta p^* < t \iff t < v^L + \pi\Delta v - c < 5t \tag{C.8}$$

   Profit change is maximized at Δp^* and $\Delta\Pi_i(\Delta p^*)$ is negative if

$$\frac{\Delta v^2}{b} > \frac{3(v^L + \pi\Delta v - c)^2}{16t} \tag{C.9}$$

To summarize, profit change is negative if $v^L + \pi\Delta v - c \leq t$ for all Δv and b (Case 1). Otherwise, the profit change is negative if $\frac{\Delta v^2}{b}$ is large enough (Case 2 and 3). The exact conditions are given by (C.7) and (C.9), respectively.

C.2 Proof of Proposition 3.2

I derived optimal beliefs (Equation 3.13 and 3.14) and signal cutoffs (Equation 3.15 and 3.16) in the text. Then we can derive the demand for both products by calculating product choice for low-signal, intermediate-signal and high-signal consumers (Figure 3.4):

$$D_0(p_0, p_1) = \begin{cases} \frac{1}{2} + \frac{2b\varepsilon t - \Delta v^2}{2b\varepsilon t}\frac{p_1 - p_0}{2t} + \frac{1}{4\varepsilon}\frac{\Delta v^2}{2b\varepsilon t}\left(\frac{p_1 - p_0}{2t}\right)^2 & \text{if } p_0 \leq p_1 \\ \frac{1}{2} + \frac{2b\varepsilon t - \Delta v^2}{2b\varepsilon t}\frac{p_1 - p_0}{2t} - \frac{1}{4\varepsilon}\frac{\Delta v^2}{2b\varepsilon t}\left(\frac{p_1 - p_0}{2t}\right)^2 & \text{if } p_0 \geq p_1 \end{cases} \tag{C.10}$$

$$D_1(p_0, p_1) = 1 - D_0(p_0, p_1) \tag{C.11}$$

Firms maximize profit by setting their price:

$$\Pi_i(p_0, p_1) = D_1(p_i, p_1)(p_i - c) \rightarrow \max_{p_i} \tag{C.12}$$

Finding optimal prices is not straightforward because the demand function is different when the firm offers a lower or higher price than its competitor. I look for a symmetric equilibrium where both firms set the same price p^*. My goal is to find p^* such that neither $p^* + \Delta p$, nor $p^* - \Delta p$ is a profitable deviation. As the model is symmetric, I consider deviations from the perspective of firm 0 without loss of generality.

Suppose both firms offer p^* and therefore split the market equally. If firms 0 changes the price by Δp then the demand changes in the opposite direction by

$$\Delta D_0(\Delta p) = \frac{2b\varepsilon t - \Delta v^2}{2b\varepsilon t}\frac{\Delta p}{2t} + \frac{1}{4\varepsilon}\frac{\Delta v^2}{2b\varepsilon t}\left(\frac{\Delta p}{2t}\right)^2 \tag{C.13}$$

First, suppose that firm 0 increases the price by Δp. It can charge a higher prices on the remaining consumers but looses consumers who paid the original price. The total effect on profit is negative if p^* is high enough such that loosing consumers is very costly:

$$\underbrace{\left(\frac{1}{2} - \Delta D_0(\Delta p)\right)\Delta p}_{Gain} - \underbrace{\Delta D_0(\Delta p)(p^* - c)}_{Loss} \leq 0 \tag{C.14}$$

$$c + t\left(1 + \frac{\Delta v^2}{2b\varepsilon t - \Delta v^2}\right) \leq p^* \tag{C.15}$$

Now suppose that firm 0 decreases the price by Δp. It can charge the new price on new consumers but looses the price difference on existing consumers. The total effect on profit is negative if p^* is low enough such that attracting new consumers is not very beneficial:

$$\underbrace{\Delta D_0(\Delta p)(p^* - \Delta p - c)}_{Gain} - \underbrace{\frac{1}{2}\Delta p}_{Loss} \leq 0 \tag{C.16}$$

$$c + t\left(1 + \frac{\Delta v^2}{2b\varepsilon t - \Delta v^2}\right) \geq p^* \tag{C.17}$$

Combining (C.15) and (C.17) gives the equilibrium price.

C.3 Proof of Proposition 3.3

Proposition 3.2 gives the equilibrium price. Taking the derivative with respect to t yields:

$$\frac{\partial p^*}{\partial t} = \frac{4b\varepsilon t(b\varepsilon t - \Delta v^2)}{(2b\varepsilon t - \Delta v^2)^2} \begin{cases} \leq 0 \text{ if } t \leq \dfrac{\Delta v^2}{b\varepsilon} \\ \geq 0 \text{ if } t \geq \dfrac{\Delta v^2}{b\varepsilon} \end{cases} \tag{C.18}$$