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ABSTRACT

From its beginnings, behavioral economics has been concerned with how people make systematic and costly deviations from financially optimal decisions. In this dissertation I present three articles contributing to that tradition. In chapter one, co-authored with Samuel M. Hartzmark and Alex Imas, I examine how owning an asset biases peoples' learning in response to new information. In chapter two, I examine how biased beliefs interact with preferences to produce one of the most well-studied behavioral anomalies in finance, the disposition effect. In chapter three, coauthored with Abigail B. Sussman, I examine the effects of a ubiquitous choice architecture in credit card debt repayment, minimum required payments.

CHAPTER 1

INTRODUCTION

From its beginnings, behavioral economics has been concerned with how people make systematic and costly deviations from financially optimal decisions. In this dissertation I present three articles contributing to that tradition. In chapter one, co-authored with Samuel M. Hartzmark and Alex Imas, I examine how owning an asset biases peoples' learning in response to new information. In particular, I document over-extrapolation by owners relative to non-owners leading to overly pessimistic beliefs after negative information and overly optimistic beliefs after positive information. Increased attention to owned goods drives this effect. Finally, I examine the implications of this learning bias for the endowment effect (Kahneman, Knetsch, and Thaler, 1990) and in real world asset markets.

In chapter two, I examine how biased beliefs interact with preferences to produce one of the most well-studied behavioral anomalies in finance, the disposition effect (Shefrin and Statman, 1985a; Odean, 1998). I find that participants' beliefs are conservative relative to Bayesian, but also replicate the finding from chapter one that owners extrapolate more than non-owners. After demonstrating the conservative bias in beliefs I demonstrate a robust disposition effect in aggregate behavior. I then examine the relationship between beliefs and selling decisions. Participants are less likely to sell goods they believe are more likely to go up in price, which is to be expected; however, because their beliefs are biased, they are somewhat more likely to sell goods a Bayesian updater would believe are more likely to go up. Then, using both reduced form and structural estimates, I compare the preference parameters necessary to rationalize the disposition effect using Bayesian beliefs, as is standard, to those estimated using subjective beliefs.

Finally, in chapter three, coauthored with Abigail B. Sussman, I examine the effects of a ubiquitous choice architecture in credit card debt repayment, minimum required payments. Prior work (e.g., Stewart, 2009) has shown in single card settings that people tend to anchor on minimum payments. However, it is not clear how these minimum requirements will affect people's repayment decisions across multiple cards, a common situation in the world. I find that minimum payments lead people to make more dispersed repayments relative to a no minimum payment control, which I term the dispersion effect of minimum payments. As a result of this dispersion effect, people tend to pay more in interest charges. I show that this effect occurs in conjunction with the anchoring effect previously discussed. Finally, I examine how this effect interacts with the information environment and discuss ways that the choice architecture of the environment can help or harm consumers.

Any errors are my own.

CHAPTER 2

OWNERSHIP, LEARNING, AND BELIEFS

Introduction

Ownership is an intrinsic component of most economic settings. Goods are priced based on the beliefs and preferences of those who own them versus those who do not, and trade occurs when non-owners judge a good to be more valuable than owners. An implicit assumption of standard theory is that ownership *per se* does not affect people's preferences for a good or how they interpret information about it.[1] Prior behavioral work on ownership has largely focused on the initial stage of endowment. In demonstrating the endowment effect, Kahneman, Knetsch, and Thaler (1990) show that randomly assigning someone to own a good increases their valuation of it, generating a gap between the minimum owners are willing to accept to part with the good and the maximum non-owners are willing to pay to attain it. The large literature that followed has largely focused on preference-based explanations for the valuation gap (Ericson and Fuster, 2014). However, many important economic contexts involve periods of learning about both goods that are owned and those that are not, with people making decisions after receiving information and updating their beliefs accordingly. Because behavior is a function of both preferences and beliefs, documenting differential learning as a function of ownership is important for both theory and empirical analysis.

This paper examines whether owning a good has a causal effect on how people respond to information about it. In a series of experimental studies, we show that ownership has a substantial impact on people's learning: when seeing the *same* information, they display more extreme reactions to both positive and negative signals about an owned versus a non-owned good. Compared to goods that they do not own, people become more optimistic after

1. For example, the Coase theorem (Coase, 1960), that market exchange leads to efficient allocations of goods regardless of the initial allocation, holds only if ownership does not influence valuation or learning about the goods.

seeing positive signals and more pessimistic after seeing negative signals. We then show that this difference in learning is driven by owners being more likely to over-extrapolate from recent signals: compared to a Bayesian benchmark, they place more weight on recent information when forming beliefs about owned versus non-owned goods. This leads to greater overreaction as a function of ownership, where updated beliefs are more likely to deviate from the Bayesian benchmark in both the positive and negative directions. On the other hand, belief updating is close to Bayesian when learning about non-owned goods. Using techniques from cognitive psychology, we provide direct evidence that these results are due to ownership-driven attention. Specifically, we show that ownership channels greater attention towards associated signals and that this increased attention leads to greater over-extrapolation. Finally, we use a memory recall paradigm to explore a potential mechanism for the relationship between attention and over-extrapolation.

These results have implications for how owners and non-owners assess the value of a good. If owners are more pessimistic than non-owners after observing negative information about a good and more optimistic after observing positive information, then the valuation gap for a good between owners and non-owners (i.e. the endowment effect) will expand after good news and shrink after bad news. We demonstrate that this is indeed the case. Finally, we document a similar relationship between ownership and over-extrapolation in a large field survey on stock market expectations.

To study whether ownership has a causal effect on learning, we constructed a setting where ownership can be as-if exogenously assigned, beliefs can be cleanly elicited and a normative benchmark for learning can be reasonably established. To do this, we employed a controlled laboratory experiment where people choose to buy any three of six ex-ante identical goods and report beliefs about their quality.[2] Participants know that each good has a good-specific probability of a price increase in each period, which we refer to as its

2. We also ran a version of the study with two goods, one that is owned and one that is not. The method and analyses are reported in the Internet Appendix. The same pattern of results is obtained.

fundamental quality. Specifically, in each period t a good i has a constant probability s^i of increasing in price and a constant probability $1 - s^i$ of decreasing in price.[3] Because s^i does not change across periods, a price increase (decrease) is a positive (negative) signal about good i's quality. Participants observe 15 periods of price movements and are incentivized based on the final price of the goods they own. In each period t, we elicit beliefs $\hat{s}_t^i$ about the probability of a price increase s^i for each good i — both those that they own and those that they do not — with truthful reporting incentivized. Since participants are not given information about the goods' quality before making their allocation decisions, the choice of which goods to own is as-if random.[4] Thus, ownership can be thought of as exogenous to any omitted variable related to differences in preferences, skill or knowledge. Additionally, this is a fairly simple learning environment since in each period the total number of price increases and decreases — which is easily inferred in every round— is a sufficient statistic for forming a Bayesian posterior.

A Bayesian agent would report beliefs $\hat{s}_t^i$ that do not vary depending on ownership because signal histories are equally informative for goods that are owned and not owned.[5] In contrast, we find that positive signals (price increases) lead to greater optimism (higher $\hat{s}^i$) about goods that are owned relative to those that are not. The opposite pattern emerges in response to negative signals (price decreases), which leads to greater pessimism about goods that are owned relative to those that are not. The ordering holds under any prior that does not condition on ownership and cannot be explained by fixed subject characteristics.

3. Prior work has used an asset market with a similar structure to study the disposition effect in a controlled environment (Fischbacher, Hoffmann, and Schudy, 2017).

4. In a separate treatment, presented in the Internet Appendix, we show that our results do not depend on whether participants actively choose the goods or are randomly endowed with them.

5. Models of rational inattention (Martin, 2017; Caplin and Dean, 2015; Mackowiak, Matejka, and Wiederholt, 2018) similarly predict no differences in learning based on ownership, because beliefs are equally incentivized for owned and non-owned goods and therefore there are no instrumental motives to pay more attention to one type over the other. Also, findings about heterogeneity in learning based on fixed characteristics, such as IQ (DAcunto et al., 2019), life experience (Malmendier and Nagel, 2015), or socioeconomic status (Das, Kuhnen, and Nagel, 2017) predict no difference as these characteristics are balanced across ownership conditions and fixed differences can be controlled for.

Our setting also allows us to examine how ownership influences learning relative to a normative Bayesian benchmark. Using multiple methods to construct these benchmarks, we find near-Bayesian learning from information about goods that are not owned. Specifically, belief errors relative to a Bayesian benchmark for non-owned goods are not significantly correlated with associated signals. In contrast, belief errors have a strong, positive correlation with signals about owned goods. This indicates that, relative to a Bayesian benchmark, individuals overreact to information associated with owned goods. These results are not consistent with rational models, which predict no ownership effects. Nor are they consistent with behavioral models of motivated beliefs (Brunnermeier and Parker, 2005; Kunda, 1990) or misattribution (Bushong and Gagnon-Bartsch, 2019), which predict asymmetric belief updating for owned goods in addition to level effects.[6]

This overreaction reflects over-extrapolation from recent signals about owned goods, both relative to non-owned goods and the Bayesian benchmark (Bordalo, Gennaioli, and Shleifer, 2017, 2018). While Bayesian updating predicts that beliefs should be independent of signal ordering, we find that recent signals play a substantially larger role in explaining beliefs for owned goods than non-owned goods. The increased over-extrapolation for owned goods is robust to a host of normative benchmarks — including priors that vary with ownership and cumulative signals — as well as benchmarks that do not require distributional assumptions.

A series of studies provide evidence that the observed differences in learning are due to an ownership-driven 'more-is-less' effect of attention. While it is often assumed that more attention improves decision quality (see Gabaix, 2017, for review), theoretically this need not be the case. Notably, Dawes (1979) and Dawes, Faust, and Meehl (1989) argue that greater attention can impair judgment if it is combined with an incorrect mental model of

6. Models of motivated beliefs (e.g. Brunnermeier and Parker, 2005) predict that people should update more in response to positive signals than negative signals about goods that they own compared to goods that they do not. This is due to people deriving utility from holding more optimistic beliefs about the fundamental qualities of owned goods. The misattribution model of Bushong and Gagnon-Bartsch (2019) predicts an overreaction to signals about owned goods, but with a stronger effect for negative signals due to loss aversion. This generates greater pessimism about owned versus non-owned goods.

the decision problem.[7] However, this conjecture has yet to be tested in a learning context.

To identify attention as a mechanism in our setting, we sought to demonstrate that ownership channels attention to associated signals and that greater attention leads to over-extrapolation, and, as a result, overreaction. In exploring the first link, we used tools from cognitive psychology to incorporate a change detection task into the baseline experiment, allowing us to both look at whether ownership affects the allocation of attention and whether increased attention generates the predicted effect on beliefs. In this study, participants were told that in each round one of the prices would change color and their task was to identify which good this price was associated with as quickly as possible.[8] Consistent with ownership channeling attention, participants were more accurate when identifying owned goods. Moreover, greater attention, measured by reaction time, is associated with more extreme belief-updating and overreaction to signals. To provide causal evidence for the attention channel, we designed a manipulation to exogenously shift attention to goods that are not owned. In this treatment, beliefs are elicited only for non-owned goods. This allows us to perform a comparative static exercise on the decision environment to identify the role of attention in belief-updating. We find that exogenously manipulating attention in this manner leads to a similar belief pattern for non-owned goods as for owned goods in the baseline condition.

Finally, we developed a signal recall paradigm to provide additional evidence for ownership-driven attention and to explore a potential mechanism for the relationship between attention and over-extrapolation. Attention has been implicated as a key driver in what information is encoded into memory so that it can later be recalled (Mrkva, Ramos, and Van Boven, 2020; Oberauer et al., 2016; Schwartzstein, 2014). Further, associative recall—the increased

7. The authors conjecture that more attention leads forecasters to overweight features of the decision problem relative to the normative benchmark. In a similar vein, Massey and Wu (2005) argue that an overreaction to signals in a belief-updating task may be driven by attention.

8. Similar change detection tasks have been used to study the allocation of attention by measuring the accuracy of responses (Mrkva and Van Boven, 2017; Mrkva, Westfall, and Van Boven, 2019; Verghese, 2001).

tendency to recall information that is similar to the current cue (Kahana, 2012)—can generate over-extrapolation (Enke, Schwerter, and Zimmermann, 2019). Drawing on these findings, we designed an experiment where participants observe price signals about owned and non-owned goods, and are then asked to recall previous signals about each. We show that recall is more accurate for owned goods compared to non-owned goods, providing further evidence for ownership-driven attention.[9] We also show that this increased accuracy is driven by people being more likely to correctly recall signals that match the most recent one. As shown formally in the Internet Appendix, these results provide suggestive evidence for a specific mechanism connecting ownership-driven attention and over-extrapolation. Our findings also shed light on when ownership may lead to less versus more well-calibrated beliefs, which we discuss in Section 2.

We explore the implications of our findings in two settings: the influence of ownership on the endowment effect and on extrapolation in asset markets. To examine the endowment effect, participants were assigned to own one of two goods and provided with real Amazon ratings over five rounds, which served as signals about quality. Consistent with our predictions, seeing the same positive ratings led the initial valuation gap between owners and non-owners to double in size. In contrast, negative ratings eliminated the endowment effect. For the second setting, we use the Michigan Survey of Consumers to examine whether asset ownership effects extrapolation from prior performance (Greenwood and Shleifer, 2014). We find that those who own assets extrapolate about *twice* as much from prior market returns compared to those who do not.

Our results have theoretical implications for settings involving durable goods with learning before the opportunity for resale, particularly in the case of understanding behavior in financial markets. A well-known puzzle in finance is that standard models predict only a small fraction of the trade volume observed in financial markets. Models of disagreement,

9. We first verify that attention improves recall accuracy in our paradigm.

where agents disagree about the value of an asset given the same information, are the dominant explanation for this puzzle (see Hong and Stein (2007) for a survey of this literature). However, the mechanism for *why* agents have different beliefs is not well understood. Our findings provide a potential microfoundation for such heterogeneity in beliefs: if owning an asset systematically changes the way that an agent updates to information compared to an agent who does own the asset, then the two will disagree about its value despite seeing the same signals.

Related Literature

Our findings contribute to the literature on behavioral biases in belief formation. Prior research has shown that people tend to neglect base-rates (Kahneman and Tversky, 1973), sample size (Kahneman and Tversky, 1972), display overconfidence (Moore and Healy, 2008), and exhibit difficulty with contingent reasoning (Esponda and Vespa, 2014, 2019; Martínez-Marquina, Niederle, and Vespa, 2019) when forming their beliefs (see also Benjamin (2019) for a review of this literature). Moreover, research on over-extrapolation demonstrates that biases in belief formation can have significant implications for the broader economy by affecting market expectations (Armona, Fuster, and Zafar, 2017; Kuchler and Zafar, 2019; Da, Huang, and Jin, 2019). Recent research has also studied the role of attention in belief formation. For example, people have been shown to not sufficiently account for correlations in the data generating process (Enke and Zimmermann, 2019), to not account for the absence of information (Enke, 2020), and to be inattentive when considering alternative causes, which leads to overly precise beliefs (Graeber, 2019).

While the empirical literature on this topic has largely focused on inattention as a source of biases in belief formation, theoretical work suggests that in some settings, more attention may generate less calibrated beliefs. Bordalo, Gennaioli, and Shleifer (2012; 2013) and Kőszegi and Szeidl (2013) present models where attention leads to an overweighing of certain attributes, which leads to biased decisions in consumer choice and decisions under risk. More generally, Dawes (1979) argues that greater attention may generate less calibrated beliefs if a person has an incorrect mental model of the decision-problem. In this spirit, Gagnon-Bartsch, Rabin, and Schwartzstein (2018) formally demonstrate that incorrect mental models — in their language, mistaken theories — will generate stable errors in learning even when agents have feedback about choice outcomes. Unless the mental model is corrected, greater attention is unlikely to mitigate biases and may exacerbate them if, for example, this leads a person to overweigh certain attributes of the choice problem. We contribute to this literature in two ways: first, by demonstrating the relationship between ownership and a specific bias in belief-updating (i.e. over-extrapolation), and second, by providing some of the first empirical evidence that belief-biases can be exacerbated through increased attention.

Another related line of work examines how trade and prior investment experiences affect beliefs and behavior. Kuhnen and Knutson (2011) and Rudorf, Weber, and Kuhnen (2016) show that beliefs are significantly impacted by prior investment choices. Both studies document an asymmetric belief-updating pattern where participants respond more to news that is consistent with their prior choices: those who had previously selected an asset update more (less) in response to good (bad) news about it, and vice versa for the assets they did not select. Using a similar experimental design, Kuhnen (2015) finds that learning is more biased when the *same* information is framed negatively versus positively.[10] Those with the opportunity to trade display a more pronounced asymmetry in beliefs in response to negatively versus positively-framed signals, compared to those who do not have the opportunity to trade. However, the paradigm used in these studies was not designed to study the effects of ownership on learning and beliefs. In their experimental design, participants chose between a risky stock or a bond and observed whether the former yielded a high or

10. Note that this is distinct from the environment studied in the current paper where the valence of signals is informative about the underlying state (fundamental quality). In our setting, this framing effect would likely exacerbate the errors that owners are already making in response to negative signals.

low dividend. The portfolio was reset after each choice and before beliefs are elicited. As noted in Kuhnen and Knutson (2011), because beliefs are elicited when participants do not hold assets—they are in the process of deciding what to purchase next—it is not clear which assets are considered "owned" or not.[11]

Another line of research looks at the effect of repeated trade on behavior. In experimental asset markets, traders generate bubbles—with prices that are substantially higher than fundamentals would suggest—which then go on to disappear with experience in a common group (Smith, Suchanek, and Williams, 1988). This line of work has argued that markets appear to have a disciplining effect on decision errors and judgment biases, suggesting that repeated trade leads to a convergence to rational predictions (Loomes, Starmer, and Sugden, 2003; Camerer, 1987). On the other hand, work by Hussam, Porter, and Smith (2008) suggests that errors re-appear as soon as the decision environment is perturbed.[12]

Evidence from the field presents a similarly complex picture of learning from trade. Strahilevitz, Odean, and Barber (2011) shows how the perception of prior investments influences future decisions, and Hoffmann and Post (2017) and Giglio et al. (2019) explore how past investments influence expectations. Greenwood and Shleifer (2014) show how prior market movements influence expectations and Malmendier and Nagel (2011) explore how life experience influences financial decisions. Further, a line of work on individual investors has shown that behavioral biases persist despite repeated decisions and frequent feedback.[13]

11. When describing their paradigm, Kuhnen and Knutson (2011) write that they do not examine situations where "individuals have ownership of certain assets." Rather, in their task, "subjects are not endowed with the stock or bond. They had to decide at every trial which of the 2 assets they wanted to hold, for that trial only. When beliefs are elicited in our experiment, subjects do not have any asset in their portfolio and are getting ready for the next round of portfolio selection." (p. 617).

12. This leads the authors to conclude that "subjects do not think about their task, and generalize from it, the way we do using economic reasoning."

13. For example, trade is influenced by individual positions (Odean 1998) and relative portfolio performance (Hartzmark 2014). See Barber and Odean (2013) for a review of this research. Similarly, both Haigh and List (2005) and Larson, List, and Metcalfe (2016) find that professional traders display marked myopic loss aversion. Looking at the endowment effect, List (2003) shows that experienced participants display a muted effect for sports cards, while Anagol, Balasubramaniam, and Ramadorai (2018) demonstrates a significant endowment effect for IPOs even among an experienced set of investors.

Professional investors managing substantial sums of money have been shown to exhibit significant behavioral biases as well.[14] Akepanidtaworn et al. (2019) show that these biases amongst institutional investors lead to substantial financial losses. There is also evidence of biases influencing information aggregation in the form of market prices.[15]

This research has substantially increased our understanding of how trade affects judgment and decision-making. However, as noted in prior work (e.g. Kahneman, Knetsch, and Thaler 1990), the decision to trade implies that ownership will be a function of ex-ante preferences and beliefs. This creates an identification problem for studying its causal effects. As-if random assignment of ownership is particularly critical for studying its influence on learning and beliefs. The ability to buy and sell goods as a function of beliefs means that ownership selects on people's reactions to signals, generating a confound precisely on the variable being studied. Owners who have larger reactions to negative signals or smaller reactions to positive signals are most likely to sell and be classified as non-owners. In turn, when looking at the relationship between ownership and belief-updating, reactions to positive (negative) signals will be overestimated (underestimated) compared to the underlying causal effect. In settings with trade, this generates an association between ownership and asymmetric updating to positive versus negative signals. To demonstrate this directly, Section 2 presents an experiment where we introduce the ability to trade into our basic paradigm and generate this asymmetric updating pattern. We then show that the ability to select in and out of ownership as a function of beliefs accounts for the pattern. After controlling for this selection effect, we recover the symmetric ownership-driven extrapolation pattern documented in our main studies.

14. For example, Hartzmark (2014), Frazzini (2006), and An and Argyle (2015)on mutual fund managers, Hartzmark and Solomon (2019) on professional analysts, and Locke and Mann (2005) and Coval and Shumway (2005) on professional futures traders and market makers, respectively.

15. For example extrapolation (in the cross section of stocks (Cassella and Gulen 2018; Chang et al. 2014) and the term structure of bonds (Shiller 1979; Giglio and Kelly 2018), contrast effects (in the cross section of stocks Hartzmark and Shue (2016)) and misperceptions of performance (in the aggregate stock market Hartzmark and Solomon (2017)).

Moreover, the choice to hold a good in a trade context is inherently driven by prior beliefs. Kuhnen and Knutson (2011) show that participants are more likely to choose an asset if their priors on its quality are higher. As a result, differences in learning as a function of holdings cannot be isolated separately from differences in priors. People may be more likely to account for information that confirms or disconfirms their priors (Klayman, 1995), which leads to differential learning outcomes for reasons independent of ownership. Thus, while trade settings have generated important insights for the domains being studied, they are not suited for identifying the causal influence of ownership on learning and beliefs.

The rest of the paper proceeds as follows. Section 2 describes the experimental paradigm used to explore the influence of ownership on learning and documents the basic effect. Section 2 presents results on learning relative to a normative benchmark and on extrapolation. Section 2 explores the mechanism. Section 2 demonstrates the interaction of differential learning with the endowment effect and provides complimentary evidence from survey field data. Section 2 discusses the implications of our findings and concludes.

The Effect of Ownership on Learning and Beliefs

Method

In order to examine the causal effect of ownership on learning and beliefs we sought to design a setting with the following features: 1) ownership was as-if exogenously assigned, 2) the relationship between signals and the underlying quality was simple to infer and transparent to facilitate learning, and 3) beliefs could be compared to a normative benchmark. To do this, we employed an experimental market on a large crowdsourcing platform called Amazon Mechanical Turk. Participants acquired goods, viewed a sequence of signals about fundamental quality, and reported their beliefs about the fundamental quality of goods that they owned and did not own. The market consists of six goods with equal starting prices of 100 experimental points per share. Participants were endowed with 2000 experimental points (500 points = 50 cents) and asked to spend the entire sum on shares of three of the six goods. The goods were ex-ante identical; as a result, ownership can be viewed to be as-if random in this setting.[16]

In each round t, a good $i \in \{1, .., 6\}$, has a fixed probability of a price increase, s^i, which represents its fundamental quality. This good-specific quality remains constant throughout the experiment. In each round, the price level of the good (e.g. 106 per share) either increases or decreases by a constant amount; a price increase is always 6% and a price decrease is always 5%. The current and prior price levels for each good were provided to the participants in every round. Since a price increase is more likely to be observed if a good has a higher fundamental quality — a good with $s^i = 0.7$ has a higher probability of experiencing a price increase in any period t than a good with $s^i = 0.4$ — prices represent signals about a good's fundamentals. Throughout the analyses, we use percent returns as our measure of prior cumulative signals since they are isomorphic to price changes and the net number of positive and negative signals in our context; a good that had an initial price of 100 and a current price of 112 is classified as having a return of 12. Participants were incentivized based on the performance of their portfolio (see discussion of the payment mechanism below).

While participants were told that each good had a fixed s^i, they were not informed of the actual quality for any of the goods: their task was to infer this quality from the signals. The key component of our study is the elicitation of beliefs about each good's fundamentals in each round.[17] We refer to these elicited beliefs as $\hat{s}^i_t$. Participants observed price signals and reported their beliefs about each good's quality value over the course of 15 rounds.

16. A separate experiment, presented in the Internet Appendix, replicates our findings when goods are randomly assigned rather than chosen. Participants are endowed with three goods while three other goods are impossible to own. This rules out choice as the driver of our results.

17. Participants report beliefs both for goods that are owned and not owned. This is an important feature of our experimental design as it allows us to test and identify specific mechanisms related to attention, which naturally requires data on both sets of goods.

We used two treatments for generating the fundamental qualities of the goods. In the first treatment, termed Discrete, participants were told that good-specific probabilities would be randomly selected, with replacement, from the set $s^i \in \{0.25, 0.3, 0.35, 0.45, 0.55, 0.65, 0.7, 0.75\}$. One concern with this method is that participants do not internalize the exogenously provided information as their prior belief.[18] As such, we run a second treatment, termed Continuous, where no information was provided about the distribution of fundamental quality. Values of s^i ranged from 0.1 to 0.9 with a median of 0.43. The main findings are similar across both treatments and we collapse across them in the main text; separate results for each treatment are presented in the Appendix.

We follow convention (e.g. Fischbacher, Hoffmann, and Schudy, 2017) in randomly generating the price paths before the experiment. This facilitates between-subject analyses since it allows for comparisons of beliefs by ownership status conditional on seeing the same price paths. We drew six sets of price paths, two for the Continuous treatment and four for the Discrete treatment.[19]

Participants were also incentivized based on the accuracy of their forecasts, potentially receiving a bonus of \$1 if a randomly selected estimate was within plus or minus 5% of the true probability s^i. We chose to use this elicitation procedure as opposed to more complex mechanisms such as versions of the Binarized Scoring Rule (e.g. the quadratic scoring rule) due to recent evidence showing that the BSR can systematically bias truthful reporting. Danz, Vesterlund, and Wilson (2019) demonstrate that the BSR mechanism leads to conservatism in elicited beliefs, resulting in greater error rates relative to a simpler mechanism that offers little to no information about the specific incentives. The authors argue that simpler mechanisms that incentivize reporting of belief quantiles — such as the one used here — will result in more truthful reporting while imposing fewer cognitive burdens

18. This is documented in recent work by Crosetto et al. (2020), discussed further below.

19. Price paths for both treatments are presented in the Appendix.

on participants.

In the Continuous treatment, participants were compensated for both the accuracy of their beliefs and the performance of their owned goods. In the Discrete treatment, it was randomly determined whether participants would be compensated based on either their belief accuracy or portfolio performance, and this was communicated to them ex-ante. This rules out hedging as a potential motive.[20] All participants were paid a base fee of $1.20.

This setting represents a simple learning environment for a Bayesian agent. Across both treatments, the number of prior increases u_{ti} and decreases d_{ti} — which is captured by the good's round-specific return — is a sufficient statistic for calculating the posterior. In the Discrete treatment, a rational prior belief is a probability of one eighth for each possible s^i. In each round, a rational agent would update their beliefs based on the signal using Bayes rule; posterior beliefs become the priors for the next round and the agent repeats the process for each new signal. In the Continuous treatment, beliefs can be represented using β distributions, which are distributions over probabilities. We conducted additional studies to elicit and estimate participants' priors. Results suggest that subjects entered the experiment with an average prior that can be well approximated using a $\beta\,(2.62, 2.62)$ distribution (both the method and estimation strategy are described in the Appendix).[21] Beliefs are updated based on signals to generate a round t posterior mean of $\left(\frac{2.62+u_{ti}}{2*2.62+u_{ti}+d_{ti}}\right)$.

The process of benchmarking an appropriate prior highlights the advantages and disadvantages of each treatment. A clear advantage of the Discrete treatment is that the simple, known

20. As discussed in the next subsection, we found no evidence for hedging since results in the Continuous treatment point in the opposite direction of what would be predicted by hedging motives (i.e. greater pessimism after positive signals and greater optimism after negative signals about owned versus non-owned goods), and findings in the Continuous and the Discrete treatment are similar despite no motive for hedging in the latter.

21. This distribution is centered at 50% with more mass in the middle, though it is relatively diffuse. In the Internet Appendix we demonstrate the robustness of our results to alternative benchmarks, with priors $\beta\,(2, 2)$, $\beta\,(2.5, 2.5)$, $\beta\,(3, 3)$, $\beta\,(3.5, 3.5)$, a simulation and ex-post forecast errors. The main patterns are robust to any of these specifications. Further supporting this parameterization, we find similar results in a treatment where participants were explicitly told that s^i was drawn from a $\beta\,(2.62, 2.62)$ distribution.

method for determining good quality s_i provides a clear candidate for a benchmark prior. The disadvantage is that a participant's prior need not be equal to that of a rational Bayesian after reading the instructions, so this benchmark may not represent the prior that participants actually use. The Continuous treatment addresses this issue by using empirical estimates of the participants' priors. In this treatment, we let the data from additional conditions and initial rounds of the experiment calibrate the prior, as opposed to assuming that participants read, understand and internalize the instructions. Recent work by Crosetto et al. (2020) supports the benefits of this approach. There, participants told that draws would be from a uniform distribution reported single-peaked beliefs with more mass in the center and less in the tails, similar to the symmetric β distribution estimated in the Continuous treatment. In turn, we believe that demonstrating that our results are robust to using either method should increase confidence in the benchmarking results that follow.

Because of the structure of the market, a decrease in price is a negative signal about quality while an increase in price is a positive signal. In order to restrict our sample to those who understood this structure, we include participants whose beliefs were positively correlated with prices and significant at the 10% level. The Discrete treatment also includes a set of questions that are separate from the main task but which feature an analogous updating problem—eliciting beliefs about fundamental quality in response to signals. In the Internet Appendix, we use these separate questions as an alternative comprehension check, demonstrating that our main results are robust to this exclusion restriction. Participants also answered control questions to assure they understood that the probabilities of each good going up or down in price were independent in each round, that their reported beliefs did not influence these prices, and that they would purchase and hold three of the six goods. This results in a final sample of 571 out of 840 subjects who completed the survey.[22]

22. Similar inclusion restrictions—which filter on whether participants are mostly updating in the same direction as the signal—are commonly used in belief-updating experiments (e.g. Mobius et al. (2011); Coutts (2019)). Because the restriction is applied equally to both conditions (owned and not owned), it does not bias inference about the variables of interest. The number of participants excluded through this comprehension

Results

Our experimental design allows us to examine the causal impact of owning a good on learning about its fundamental quality. Since goods are ex-ante identical to the participants, differences in the elicited beliefs $\hat{s}^i$ between owned and non-owned goods will be driven purely by the effect of ownership.

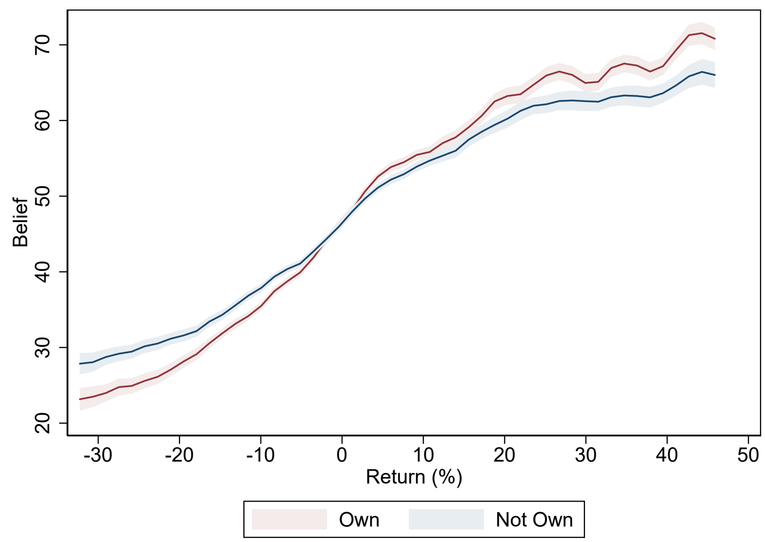


Figure 2.1. Beliefs by Return. This graph shows a local linear plot of beliefs, $\hat{s}^i_t$, on returns separately for goods that are owned and not owned. Data include observations with returns from the 5th to the 95th percentile. Shaded area represents the 95% confidence interval.

We begin by comparing the beliefs about various goods at different return levels in Figure 2.1. The red line shows the average beliefs $\hat{s}^i_t$ associated with goods that are owned for each return level. The blue line shows the average beliefs for goods that were not owned. The shaded areas indicate 95% confidence intervals. The red line has a steeper slope than the blue, consistent with a greater response to a cumulative signals for goods that are owned. For lower returns on the left of the graph, the red line is consistently below the blue line. This indicates that, for a given return level, participants are more pessimistic about goods

filter is within the range of prior belief-updating studies (e.g. 25% in Mobius et al. (2011), 49% in Enke and Graeber (2019)).

they own—believing them to be worse than goods they do not own. For higher returns on the right side of the graph the general pattern is reversed. The red line is consistently above the blue line indicating that participants are more optimistic about goods that they own compared to goods that they do not.

Table 2.1 examines this pattern in greater detail by examining beliefs based on the return level and how they differ depending on ownership. Beliefs are regressed on the return, an *Own* dummy variable equal to one if the good is owned and an interaction of the two variables. The coefficient on *Return* in Column 1 shows that there is a strong positive relationship between good i's performance and the respective belief $\hat{s}_t^i$ for non-owned goods, which is expected given the structure of the experiment. The coefficient of interest is on the interaction of *Own*Return,* which is positive and significant. This indicates that beliefs about goods that are owned respond to cumulative signals more than beliefs about goods that are not owned, consistent with the red line being steeper in the prior figure.

In our setting, a rational Bayesian would need to know only the return level and the round to form their expectations. Thus in Column 2 we include return by round fixed effects. This column shows how beliefs about owned positions differ from those that are not owned given any Bayesian benchmark that does not condition on ownership.[23] This also controls for any non-Bayesian benchmark that takes price paths as its input and does not condition on ownership when forming beliefs. If anything, the results are slightly stronger.

People may update their beliefs differently depending on their individual characteristics, for example due to differences in IQ (DAcunto et al., 2019), differences in life experience (Malmendier and Nagel, 2015), or differences in socioeconomic status (Das, Kuhnen, and Nagel, 2017). Column 3 adds subject fixed effects to control for such differences. Results are similar, suggesting that the effect is not driven by individual differences. After removing individual averages, the *same* person is more optimistic for owned goods after receiving

23. This is the same as fixed effects for the number of price increases and decreases, the main inputs in forming posterior beliefs through Bayesian updating in our setting. See Section 2 for further discussion.

positive signals and more pessimistic for owned goods after receiving negative signals, compared to receiving similar signals about goods she does not own.

We sought to test whether this pattern was robust to decreasing the number of goods that participants had to keep track of. We ran a version of the experiment where participants chose to own one of two ex-ante identical goods. All other features of the experiment were kept the same. Table IA.II in the Internet Appendix presents the results, which follow the same pattern as in the case with six goods.

These findings suggest a robust difference in learning in response to cumulative signals about owned goods compared to those that are not owned. Participants react more to the same information about goods that they own compared to those that they do not. Under any Bayesian prior that does not differ by ownership, they are more pessimistic about owned goods that experienced negative signals and more optimistic about owned goods that experienced positive signals, relative to goods that are not owned.

Ownership, Belief Errors, and Extrapolation

The previous section demonstrated that the same information leads to different beliefs as a function of ownership. Our setting allows us to construct normative benchmarks for learning to explore whether owners or non-owners are closer to Bayesian when learning about the quality of a good. When looking at belief errors, we find that they are more extreme for owned goods compared to non-owned goods in both the positive and negative domains. We show that this overreaction to information appears to be driven by people over-extrapolating from recent signals about owned goods.

Belief Errors

Figure 2.2 graphs the belief errors relative to the associated Bayesian benchmarks by return level. The blue line, representing goods that are not owned, is relatively flat. This implies that in our experiment, learning about non-owned goods is similar to the predictions of the Bayesian model. On the other hand, the red line — which represents belief errors associated with goods that participants own — has a positive slope. This indicates that participants update to a greater extent than a Bayesian agent for goods that they own, consistent with an overreaction to signals about owned goods.

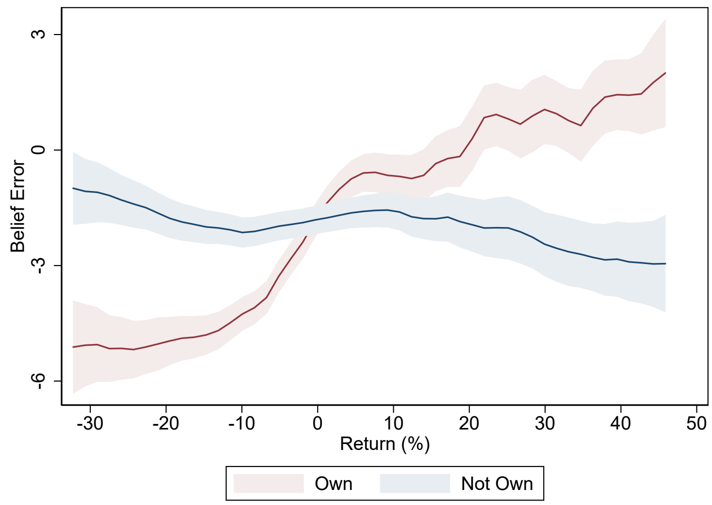


Figure 2.2. Belief Error by Price. This graph shows the belief error relative to the Bayesian benchmark based on whether a good is owned as a function of its return. Data include observations with returns from the 5th to the 95th percentile. Shaded area represents the 95% confidence interval.

Table 2.2 repeats the regression analysis from the previous subsection using the belief error. Column 1 shows the regression without controls. The coefficient on *Return,* which corresponds to belief-updating for non-owned goods, is roughly 0. In turn, we cannot reject that participants learn about non-owned goods similar to a Bayesian agent. In contrast, the coefficient on *Own*Return* is roughly 0.1 and significant at the 1% level. This implies that in response to a positive signal about an owned good, participants increase their beliefs by 20% more than both a Bayesian and the response to the same signal about a non-owned good.[24] Column 2 includes an individual fixed effect and shows similar results.[25]

Columns 3 to 6 repeat the analysis separately for the Discrete and Continuous treatments. Columns 3 and 4 examine only data from the Discrete treatment while columns 5 and 6 examine data from the Continuous treatment. All four columns display a similar pattern. There is no statistically significant coefficient on the *Return* variable, indicating that belief updating about non-owned goods was indistinguishable from the Bayesian benchmark. On the other hand, the coefficient on *Own∗Return* is positive and significant in each specification.

These results indicate that irrespective of the method used (Continuous or Discrete), ownership leads to a more extreme reaction to information compared to the Bayesian benchmark, as well as relative to seeing the same information about a non-owned good. Thus, the observed pessimism after negative signals and optimism after positive signals about owned goods can be interpreted as an *overreaction* to the signals.

Extrapolation

Previous theoretical work has shown that over-extrapolation of recent signals can produce the type of symmetric overreaction documented in the previous section (Bordalo, Gennaioli, and Shleifer, 2018; Bordalo et al., 2018). In this section, we demonstrate greater extrapolation of recent signals relative to Bayesian benchmarks for positions that are owned compared to those that are not owned. Moreover, we show over-extrapolation from signals about owned goods, but we find little evidence for over-extrapolation with respect to non-owned goods.

To look at differences in extrapolation, we regress beliefs on *Price Increase,* a dummy variable equal to one if there was a positive signal in round t and zero if there was a negative

24. In response to a positive signal (6% return) about a non-owned good, participants increase their stated quality by 3% (based on the coefficient on *Return* of 0.5), which is consistent with Bayesian updating. In contrast, the 0.1 coefficient on *Own*Return* implies that in response to the same positive signal, participants increase their beliefs about quality by 3.6% — 20% higher than a Bayesian observing the same information.

25. We do not add a round by return fixed effect as this controls for any Bayesian prior and thus does not add information when explicitly including a benchmark.

signal, the *Own* dummy variable and an interaction between the two.[26] The coefficient on *Own*Price Increase* corresponds to how much more or less agents respond to a recent price increase for positions they own compared to positions they do not. The coefficient on *Own* represents how much more or less agents respond to a price decrease for positions they own compared to those they do not.

Table 2.3 Panel A presents the results which show that agents appear to extrapolate more from both recent price increases and decreases for positions that they own. Column 1 examines raw beliefs without controls. The interaction of *Price Increase* with *Own* has a significant coefficient of 5.03, which indicates that individuals update their beliefs by 5% more after seeing a positive signal about an owned good compared to a non-owned good. The coefficient on *Own* is negative, which indicates there is also a larger negative reaction to price decreases for positions that are owned.

Without further controls it is unclear whether the difference in updating based on ownership is due to differences in extrapolation, or whether it simply reflects differential updating based on a given information set. To identify extrapolation, and over-extrapolation in particular, we proceed to examine how such beliefs deviate from a Bayesian benchmark that incorporates the dynamic nature of belief updating. In our simple learning setting, the ordering of signals does not matter for a Bayesian as the number of positive and negative signals is sufficient to calculate the posterior in a given round.

The order of price signals does matter for an agent who over-extrapolates from recent signals. We use the following expression of the mean posterior belief $\hat{s}_{ti}$ to estimate the degree of extrapolation for good i in round t:

$$\hat{s}_{ti} = \hat{s}_{ti}^{Bayes} + \nu * Z_t^i \tag{2.1}$$

26. We exclude the first round for the extrapolation tests because after receiving only one signal there is no difference between the most recent information and the total information.

where $Z_t^i = 1$ if good i experienced a price increase and $Z_t^i =$ -1 if it experienced a price decrease in round t, and $\hat{s}_{ti}^{Bayes}$ corresponds to the Bayesian posterior in that round. The parameter ν in the second term captures the extent of over or under-extrapolation from the recent signal. A $\nu > 0$ corresponds to over-extrapolation while a $\nu < 0$ corresponds to under-extrapolation. The expression reduces to Bayesian updating when $\nu = 0$.

To estimate ν we measure belief errors relative to a Bayesian benchmark and use them as the dependent variable in the extrapolation regression. If an agent updates as a Bayesian, the difference between $\hat{s}_{ti}$ and the Bayesian benchmark $\hat{s}_{ti}^{Bayes}$ should not be influenced by recent price changes as the benchmark accounts for updating with regard to that information. If an agent over- or under-extrapolates from recent signals, we expect recent signals to have significant explanatory power for $\hat{s}_{ti}$ even after controlling for the benchmark.

Column 2 presents belief errors relative to such benchmarks, capturing the degree of over- or under-extrapolation relative to a Bayesian agent.[27] The coefficient on *Price Increase* is -0.982 which indicates mild underreaction from price increases for non-owned positions. The point estimate on *Own*Price Increase* is 3.87 and the point estimate on *Own* is -2.28, both significant at the 1% level. This suggests that the majority of the effect in Column 1 represents over-extrapolation from recent signals for owned positions rather than Bayesian updating.[28]

These results illustrate over-extrapolation relative to a prior that does not condition on ownership. However, the regressions may be capturing the general difference in beliefs for owned versus not owned positions rather than differential extrapolation from the most recent signal. To test for such a possibility, we allow for benchmarks where the prior varies

27. Results split by Discrete or Continuous treatment are presented in the Internet Appendix.

28. Equation 1 above imposes a uniform ν to price increases and decreases, which means the degree of extrapolation from positive signals and negative signals is uniform. The regression specification used in Panel A allows for differential extrapolation from positive and negative signals. The coefficient on *Own*Price Increase* can be interpreted as the ν in response to price increases and the coefficient on *Own* can be interpreted as the ν in response to price decreases. The analysis imposing a symmetric ν is conducted in the Internet Appendix. The results are materially similar.

by ownership and by round. We do so in two ways.[29] First, we repeat the technique used to calibrate priors from participants' initial judgments (discussed in the Internet Appendix), but do so separately for owned and not owned positions. We refer to the belief relative to this benchmark as $\beta\,(Own)$ *Error*. Second, we use the average belief reported for a given price signal and ownership status to identify the implied value of the prior in a given round $t-1$.[30] Using this estimate, we can calculate the posterior for a Bayesian who observes the realized price signal in the next round. We term this benchmark $\beta\,(Own\ Round)$ *Error*.

Columns 3 and 4 present belief errors relative to these benchmarks and provide further evidence of over-extrapolation for owned goods. In Panel A, the coefficients on *Own*Price Increase* are positive and significant and the coefficient on *Own* is negative and significant. In some specifications the coefficients on non-owned goods are weakly positive, weakly negative or insignificant suggesting there is not a strong pattern for non-owned goods. For owned goods, across all of our specifications the results indicate that even after allowing for different prior beliefs based on ownership and ownership interacted with price, participants exhibit greater over-extrapolation.

As with any benchmark, there is a concern that it is misspecified. We address this by presenting a series of results which do not rely on distributional assumptions where we control for return levels. This non-parametric test is related to the concept of "divisible updating," which characterizes belief updating processes that are independent of how the individual chooses to partition information (Cripps, 2018). Bayesian updating satisfies this property as the order of signals should not matter for a Bayesian. In turn, showing that the order of signals matters, in that a recent signal is treated differently than the same signal received further in the past, suggests that a non-Bayesian process such as over-extrapolation

29. We calculate the benchmarks for the discrete and continuous treatment using the same method because priors that vary based on ownership need to be estimated in both treatments.

30. We drop observations where an equal number of positive and negative signals have been observed. For such observations, a response of $\hat{s}_{t^i} = 50$ is consistent with any symmetric β prior, and hence any other response is inconsistent with any symmetric β prior.

is taking place and rules out other belief biases that could potentially generate the observed overreaction (Bohren and Hauser, 2018). Thus, if dummy variables for the direction of recent price movements are significant after controlling for the effect of returns, this is evidence that these participants are over-extrapolating from recent signals.

Column 1 of Panel B in Table 2.3 includes a linear control for returns. The coefficient on $Own * (Price\ Increase)$ indicates that participants extrapolate 3.53 more from a positive signal about owned goods than they do from the same signal about non-owned goods. The coefficient on Own indicates that participants extrapolate 2.32 more from negative signals about owned goods than they do from the same signals about non-owned goods. Linear controls may obfuscate interesting dynamics of the return response pattern, so in Column 2 we include dummy variables for levels of return in 10% increments. Including these controls yields similar results. It may also be the case that the extrapolation coefficients are capturing differential updating to return levels based on ownership rather than extrapolation. Column 3 includes a linear control for returns and also an interaction of return with Own to capture such a differential reaction. Again, results are similar, suggesting 2.20 greater extrapolation from positive signals and -1.83 greater extrapolation from negative signals about owned positions. Column 4 includes dummy variables for returns along with an interaction of those dummy variables. This flexibly controls for level of returns separately for owned and non-owned positions. The pattern of results is unchanged.

Together, these findings imply that people over-extrapolate from recent signals to a substantially greater extent when learning about owned goods, both relative to non-owned goods and a variety of normative benchmarks.

Exploring the Mechanism

The previous two sections demonstrate differential learning as a function of ownership. People who own a good are more optimistic (pessimistic) about its quality after seeing positive (negative) signals about it compared to people who do not own it. Moreover, individuals overreact to information about owned goods compared to non-owned goods, and this difference in learning appears to be driven by over-extrapolation from recent signals. In this next section, we aim to provide evidence for a specific mechanism behind the effect.

The relationship between ownership and beliefs is not consistent with Bayesian learning, which predicts no differences by ownership status. The symmetric over-extrapolation and overreaction we observe is also not consistent with behavioral models of motivated beliefs (Brunnermeier and Parker, 2005; Kunda, 1990), which predict asymmetric updating and overall optimism, nor models of misattribution (Bushong and Gagnon-Bartsch, 2019), which also predict asymmetric updating but overall pessimism. Moreover, models of rational inattention cannot rationalize our findings because reported beliefs are incentivized in the same way for owned and non-owned goods. Finally, since our results are robust to the inclusion of subject fixed effects, the learning pattern cannot be explained by heterogeneity based on fixed participant characteristics.

We now consider a mechanism where ownership channels attention towards signals associated with owned goods. Under this mechanism, rather than affecting how information is interpreted (as models of motivated beliefs and misattribution predict), greater attention exacerbates over-extrapolation from recent signals. Work in cognitive psychology has shown that attention has an intimate relationship with value-relevant information (Smith and Krajbich, 2019, 2018; Enax, Krajbich, and Weber, 2016); in turn, more attention is likely to be allocated towards signals associated with payoff-relevant assets, such as owned goods.

Why would greater attention lead to the observed over-extrapolation? Recent research has argued that over-extrapolation is at least in part driven by the associative nature of what 'comes to mind' through recall when making judgments (Enke, Schwerter, and Zimmermann, 2019; Gennaioli and Shleifer, 2010; Bordalo, Gennaioli, and Shleifer, 2020). Enke, Schwerter, and Zimmermann (2019) show that people over-extrapolate from information because they are more likely to recall similar prior information. For example, a person seeing an asset with positive returns is more likely to recall prior instances of price increases than decreases. In turn, judgments about future performance will over-extrapolate from the recent signals because the information set being used is more likely to include prior congruent signals than non-congruent signals. This process of associative memory implies that people behave as if they are over-weighting the most recent signal.

In order to recall a signal, it must first be encoded into memory. Work in economics and cognitive psychology posits that attention determines what information is encoded into memory, such that signals which are not attended to cannot later be recalled (Chun and Turk-Browne, 2007; Schwartzstein, 2014). If ownership-driven attention determines which signals are available for recall, then the associative process outlined above can lead to over-extrapolation. This generates broad testable hypotheses on comparative statics between ownership and beliefs: ownership is predicted to channel greater attention towards information about owned goods, which leads to a more extreme reaction to both negative and positive signals compared to the same signals about non-owned goods. Additionally, owners will be more likely to over-extrapolate than non-owners relative to a normative benchmark.

Importantly, in our setting there is no need for a Bayesian to recall prior information because the current round-specific price level contains a sufficient statistic for Bayesian updating.[31] In turn, a rational agent should ignore recalled signals when forming beliefs. However, research has shown that recall is both spontaneous and involuntary (Mace, 2007) and that redundant information is not ignored (Eyster and Rabin, 2014). In both individual and social learning settings people have been show to 'double count' redundant information (Eyster, Rabin, and Weizsacker, 2015; Enke and Zimmermann, 2019). If prior signals are less likely to be encoded and recalled for non-owned goods, then this can lead to an overreaction and less well-calibrated beliefs about owned versus non-owned goods. This

31. Such sufficient statistics are likely present in a variety of economically important settings, e.g. Grossman (1976) argues that prices serve this function in markets.

'more is less' hypothesis is in the spirit of Dawes (1979), who conjectures that greater attention may lead forecasters to overweight features of the decision-problem relative to the normative benchmark. In our setting the mechanism corresponds to attention leading to the overweighting of recent signals due to associative recall.[32] These hypotheses are derived formally in the Internet Appendix.

Testing these predictions requires evidence for the following conjectures: ownership channels attention towards signals about owned goods, increased attention generates greater over-extrapolation, and ownership increases recall accuracy of congruent signals. We test these conjectures in three experiments.

The first employs a change detection paradigm from cognitive psychology to measure visual attention. We demonstrate that participants are more accurate when identifying changes associated with signals about owned goods than non-owned goods. This suggests that more attention is channeled towards information about owned goods. Moreover, data on reaction times offers suggestive evidence that greater attention increases the ownership-driven effect on learning and overreaction.

The second study uses a comparative static approach to exogenously manipulate attention towards non-owned goods. Here, we find that increasing attention towards non-owned goods produces a similar pattern to owned goods in the baseline paradigm.

The third study incorporates a recall task into our basic paradigm: after observing a set of signals, participants are asked to recall prior signals about owned and non-owned goods. We first verify that attention increases recall accuracy in our setting. Consistent with our proposed mechanism we find a positive effect of ownership on recall accuracy and show that this increased accuracy is driven by people being more likely to correctly recall similar signals to the one they just saw. We stress, however, that associative recall is one potential mechanism for the relationship between attention and over-extrapolation in our

32. Note that this hypothesis is less general than the hypotheses on comparative statics of ownership because it depends on the nature of updating about non-owned goods; we discuss this further in Section 2.

setting. Our results present direct evidence for ownership-driven attention exacerbating over-extrapolation, but do not rule out other mechanisms, such as attention increasing the salience of recent signals.

Ownership-Driven Attention

We incorporated a change detection task into our basic paradigm to examine whether ownership channels attention towards related signals. Participants (N=176) took part in the Discrete treatment, but were also told that one of the six prices would randomly be highlighted in green. In addition to reporting their beliefs, participants were tasked with correctly identifying the green good as fast as possible.[33] This change detection task is similar to those used in cognitive psychology, which examine the allocation of attention by measuring the speed and accuracy of responses (Mrkva and Van Boven, 2017; Mrkva, Westfall, and Van Boven, 2019; Verghese, 2001). If ownership channels greater attention towards related signals, then participants should be faster and more accurate when identifying owned goods.

Consistent with this prediction, we find that participants are more accurate when identifying owned versus non-owned goods. When participants guess about an owned good, they are 11% more accurate ($t(2501) = 3.09, p = .002$). These results provide evidence that more attention is paid to signals about owned goods.

Attention and Over-Extrapolation

To investigate the relationship between attention, learning and over-extrapolation, we use reaction time data on the change detection task as a proxy for attention. Faster response times are a hallmark of greater attention paid to the task (Ninio and Kahneman, 1974).

33. Similar to the procedures outlined in Section 2, we randomly chose either one decision on the change detection task, belief elicitation task or performance of the goods to be paid. As in the case of belief elicitation, we sought to incentivize accuracy and speed on the change detection task as transparently as possible. Participants were told that if the change detection task was chosen for payment, then conditional on being accurate, they had a better chance of earning $2 if their reaction time was faster.

Indeed, we find that accurate answers on the change detection task were 9% more likely to have a below-mean response time ($t(2501) = 2.62, p = 0.010$). We classify a round as High Attention if response time is below the mean time. In Table 2.4, we see that high attention is associated with a stronger effect of ownership on learning and overreaction to information. We view this as suggestive evidence for the proposed relationship between attention and belief-updating.

In the next study, we sought to induce exogenous variation in attention by only eliciting beliefs for non-owned goods. If the effects in the main study are driven by ownership channeling attention to related signals, then belief updating about non-owned goods in this study should resemble those of owned goods in the baseline condition. Table 2.5 compares belief-updating between treatments by adding the data from the exogenous attention paradigm to the data from the baseline analysis. The *Own* dummy is equal to one for goods owned by participants in the main study. *No Own Treat* is a dummy variable that is equal to one for observations in the attentional paradigm. Thus, the *Own* variables can be interpreted similar to the prior regressions: the difference in updating from signals about an an owned good relative to a non-owned good in the main study. The *No Own Treat* coefficient represents the difference in beliefs about non-owned goods in the attentional paradigm compared to non-owned goods in the baseline study.

Table 2.5 shows that beliefs about non-owned goods in the attentional paradigm resemble beliefs about owned goods in the main study. For example, looking at Column 2 of Panel A which includes price fixed effects, the coefficient on *(No Own Treat)*Return* is 0.185 and is significant at the 1% level, which is similar to the point estimate in the main study. Examining extrapolation in Panel B we again see a positive and significant coefficient on the *(No Own Treat)*(Price Increase)* variable, consistent with over-extrapolation of recent signals in the attentional paradigm. Beliefs about goods in the attentional paradigm are generally closer to owned goods than to non-owned goods in the main study.

Ownership and Recall

As outlined above, ownership-driven attention is predicted to improve recall of signals linked to owned goods. To test this prediction participants (N=298) completed a version of the main study with two assets rather than six, resulting in one asset that was owned and one asset that was not. Each was randomly assigned a round t and asked to recall whether the signal in the previous round $t-1$ was positive or negative. For example, after seeing a signal in round 5, the participant would be taken to a separate page and asked to recall the signal in round 4 for both the owned and non-owned goods.

Table 2.6 presents data on aggregate recall accuracy, as well as split by whether the previous signal matched the most recent realization or not.[34] The first column verifies our assumption that attention determines what is encoded in memory by regressing recall accuracy on the absolute value of returns. Bordalo, Gennaioli, and Shleifer (2013) argue that an attribute's departure from the average level draws attention to the good, so more extreme returns should channel greater attention to a good. The regression shows improved recall of signals about goods that have larger absolute returns. Column 2 shows that even after controlling for the absolute value of returns, participants are significantly more accurate when recalling signals about owned goods. This provides further evidence for ownership-driven attention. Importantly, owners are significantly more accurate in recalling signals that match the most recent one: associative recall is nearly double the aggregate recall effect and significant at the 1% level. On the other hand, there is no difference between owners and non-owners when the prior signal does not match the current realization.

Together, these results provide evidence for a potential mechanism driving the relationship between ownership-driven attention and over-extrapolation.

34. Note that we examine recall accuracy rather than an unconditional measure of signal matching because we are interested in capturing recall rather than simple matching.

Applications

In this section, we explore applications of the documented relationship between ownership and learning. First, we return to the classic endowment effect paradigm to demonstrate how differential learning affects valuations. We then replicate the ownership-driven extrapolation effect in field data on beliefs about aggregate stock market performance.

The Effect of Ownership on Valuation

In many contexts, owners and non-owners have opportunities to learn about the quality of a good before trading. Our results imply that after observing negative signals, owners will be more pessimistic and decrease their valuation of the good more than non-owners. In contrast, after observing positive signals, owners will be more optimistic and increase their valuation of the good more than non-owners. Prior work has documented an initial valuation gap between owners and non-owners, termed the endowment effect. Kahneman, Knetsch, and Thaler (1990) showed that ownership increases people's minimum willingness to accept (WTA) to part with the good relative to non-owners' maximum willingness to pay (WTP) for the same good (see Ericson and Fuster (2014) for review). In this context, we predict that the valence of information will have an asymmetric effect on this initial WTA-WTP gap: the gap will shrink in response to negative signals, as owners become more pessimistic than non-owners about the good, and expand in reaction to positive signals, as owners become more optimistic than non-owners.[35]

35. In exploring the mechanism for the endowment effect, Johnson, Haubl, and Keinan (2007) argue that the initial valuation gap can be explained by owners (non-owners) spontaneously generating reasons to own (not own) the product, before generating reasons to not own (own) it. This process leads owners to have more reasons to own the product than non-owners, translating to a valuation gap. This research is distinct from our own in that the authors do not look at learning from new information, nor at how the endowment effect evolves as a function of this information.

Experiment

To test this, we endowed participants with power banks, which are auxiliary batteries for charging cell phones.[36] After being endowed with one of two power banks, each participant observed signals about the quality of the power bank they owned and the one that they did not own over the course of five rounds. Signals came in the form of ratings (1 to 5 star ratings) of the power banks taken from individual customer reviews on Amazon.[37]

After observing a rating, we elicited a WTA for the owned power bank and WTP for the non-owned power bank on a \$0 to \$100 scale in each round.[38] To categorize positive and negative signals requires characterizing a neutral level of information. In this context, a reasonable "neutral" benchmark for quality is likely around 4 stars given a participant's experience on Amazon (Chen, Dhanasobhon, and Smith, 2008) and the average rating in our experiment (3.7 stars). In turn, we follow the literature on Amazon's ratings system to classify cumulative ratings below 3.5 as a negative signal, between 3.5 and 4.5 as a neutral signal, and above 4.5 as a positive signal (Bhatt et al., 2015). We drew multiple sets of ratings such that the cumulative signals were better for one power bank than the other in some sets, and vice versa in the other sets. Both endowment and the set of ratings drawn was counterbalanced. Details on the methods can be found in the Internet Appendix.

To ensure that our paradigm replicated the standard endowment effect without information, we ran a separate treatment without ratings. In turn, the WTA and WTP measures were elicited once. We found a sizable and significant endowment effect. Non-owners had an average WTP of \$28.93 while owners had an average WTA of \$34.47 ($p < .01$). Endowing participants with a good in our setting increased their valuation of it by 19%, which is well

36. We chose power banks as they are generic products with substantial heterogeneity in quality. Thus, there is scope for significant learning about product quality from signals. They are also reasonably priced goods, making it practical to purchase a large number of them to give to participants.

37. Participants saw a generic picture of the powerbank that could not easily be found online.

38. We are not the first to study the endowment effect in a repeated setting (e.g. Shogren et al. (2001) and Loomes, Starmer, and Sugden (2003)). We discuss how our study relates to these papers in Section 2.

within the range of prior demonstrations of the effect (Ericson and Fuster, 2014).

Results

Figure 2.3 graphs the average valuation based on the cumulative signals in that round. Each bar represents the difference between the WTA for the good that is owned and the WTP for the good that is not owned. The red bar to the left is the endowment effect in the absence of any information ($5.54).

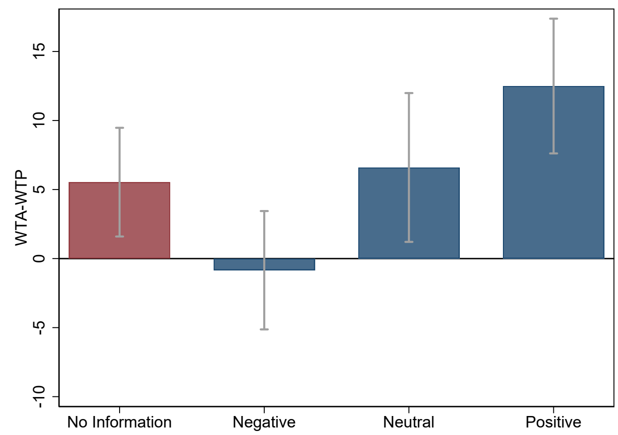


Figure 2.3. Valuation by Rating in Endowment Effect. The figure shows the willingness to accept for owned goods and willingness to pay for not owned goods based on its average cumulative signals. Gray bars represent the 95% confidence intervals.

The figures show that ownership influences valuations in line with the documented influence on learning and beliefs. In response to positive signals, the valuation gap increases. The Positive bar (above 4.5 stars) to the right of the figure indicates that when participants observed positive signals , the valuation gap increases substantially, roughly *doubling* in magnitude. On the other hand, the opposite pattern is observed for the Negative bar (below 3.5 stars): the gap between the valuations disappears, and even directionally reverses.[39]

39. The elimination of the valuation gap in response to negative signals is related to the findings of Lerner, Small, and Loewenstein (2004), who show that inducing negative emotions prior to trade similarly eliminates the endowment effect. If the induced emotions spillover to the valuation process, as the authors argue, then they can be interpreted as generating a negative signal about the good. In turn, these results can be seen as a complimentary demonstration of the effect presented here.

Table 2.7 examines the pattern more formally. It reports coefficients from the regression:

$$Value_{it} = \alpha + \beta_1 Own * Rating_{it} + \beta_2 Rating_{it} + \beta_3 Own_{it} \tag{2.2}$$

Rating is measured as the average rating observed for good i by round t in Panel A and the most recent rating in Panel B.[40]

The coefficient of interest is the interaction term, which we find to be robustly positive – consistent with ownership influencing valuations in line with our predictions. Columns 3 reports a coefficient on the interaction term of \$3.78 with no controls. This implies that a one-star decrease in the good's rating decreases the valuation gap by \$3.78. Column 4 adds subject fixed effects to control for heterogeneity in valuation of power banks. The coefficient on the interaction term decreases, though remains a significant \$1.41. The next column adds round fixed effects and finds a significant coefficient on the interaction term of \$3.79. While the analysis examines responses to the average rating, subjects may weight ratings differently than a simple arithmetic average. Column 6 includes a round by rating dummy, which removes the average value for any sequence of ratings. After doing so, the interaction coefficient is \$3.77 and significant. The final column includes individual fixed effects and round by rating fixed effects; the interaction term is similar to the case of subject fixed effects alone. Panel B repeats the analysis using the most recent rating rather than the cumulative average rating. The interaction term is positive and significant across all specifications.

In addition to exploring valuation effects, this setting also demonstrates the robustness of the results obtained using the baseline paradigm. While we attempted to make that experiment as transparent as possible, one may be concerned that participants were confused

40. To make the coefficients easier to interpret, $Rating_{it}$ is normalized to 3 stars meaning a five-star rating has a value of $Rating_{it} = 2$ and a one-star rating to has a value of $Rating_{it} = -2$. Centering at 3-stars does not change the coefficients on $Own * Rating_{it}$ or $Rating_{it}$. Rather, it leads Own_{it} to represent the difference in value between owned and non-owned positions at a 3-star rating.

about trading financial assets, reporting of probabilities, or the abstract nature of the setting. The endowment setting involves physical goods rather than abstract assets and participants reported valuations rather than probabilities. The setting has been utilized in so many experiments in part because it is viewed as intuitive and straightforward. Thus, the fact that we find analogous results in a classic endowment effect setting should assuage concerns that the learning results documented in our main investigation were driven by experimental artifacts.

Stock Market Expectations and Ownership

To examine the generalizability of our laboratory findings, we explore the impact of ownership on learning and beliefs in field data. Studying this question requires information on signals and beliefs, and a setting where it is plausible that agents who hold and do not hold a given good are reasonably aware of the signals when forming beliefs. For this reason, we examine beliefs about aggregate stock market performance.

We study whether the belief response to recent market performance — the signal analogue to our experiment — is different depending on whether the individual owns stocks or not. The data comes from the University of Michigan Survey of Consumers. The survey asks whether a respondent owns stocks as well as how they think stocks will perform in the future. Specifically, respondents are asked: "What do you think is the percent chance that a one thousand dollar investment in a diversified stock mutual fund will increase in value in the year ahead, so that it is worth more than one thousand dollars one year from now?" We interpret stated beliefs about expectations for the stock market similarly to beliefs about the fundamentals $\hat{s}^i$ in our experiment. The data set contains the relevant data for 187 months, covering the years 2002 until 2019.

In this setting, investors select into owning stocks and may be differentially aware of information relating to those investments (unlike in our experiment where goods are identical ex-ante). These concerns are somewhat mitigated by examining aggregate market performance since recent market performance (i.e. the signals) is widely reported and discussed in the media. In turn, it is likely that people are aware of it regardless of whether they own stocks. Additionally, to the extent that owners and non-owners differ based on observable characteristics, we employ a rich data set to control for these factors. However, concerns about systematic differences based on non-observable factors remain, so these results should be viewed as complimentary to the experimental findings where such issues are mitigated.

To begin, we examine how belief expectations vary with horizon of past market performance. Greenwood and Shleifer (2014) show that in six different surveys, investors extrapolate past market performance to form expectations about the future. To document a similar pattern in our sample, we examine two different left-hand side variables. The first is the percent ranking from 0 to 100 on whether the market will be higher. The second is whether a participant thinks the chance of a market increase is greater than 50%, a proxy of the bearish versus bullish measure used in Greenwood and Shleifer (2014). For past stock market returns we use the CRSP value weighted index over the prior quarter, six months, year and two years.[41] Market returns are from the period ending the month prior to when the survey was conducted. For example, if a participant took the survey in June of 2014, the lagged quarterly return would be the cumulative return from March 2014 through May of 2014.

Results

We explore how over-extrapolation varies with stock ownership. The appendix shows that, consistent with Greenwood and Shleifer (2014), investors in our data generally over-extrapolate past market performance.[42] To examine whether such over-extrapolation varies with ownership

41. Hartzmark and Solomon (2017) argue that investors actually pay attention to market indices such as the S&P 500 or the Dow Jones. The Internet Appendix shows similar results using these measures.

42. We also replicate the Greenwood and Shleifer (2014) finding that this is a mistake. The data suggest that a investor should predict an inverse relationship between recent past performance and future market performance, but respondents mistakenly over-extrapolate from past signals which leads to incorrect beliefs

we regress beliefs about future market performance on past performance and interactions of ownership and past market performance. Specifically we examine:

$$Probability\ Increase_{it} = Market_{[-m,-1]} + Market_{[-m,-1]} * Own_{it} + Own_{it} \tag{2.3}$$

where $Market_{[-m,-1]}$ is the previous market return during the relevant horizon and Own_{it} is equal to one if the participant states that they own the assets. Thus, the coefficient on $Market_{[-m,-1]}$ is the degree of extrapolation of past performance by participants who indicate that they do not own the assets. The coefficient on Own_{it} controls for the average difference in expectation between those who own and do not own the assets. The coefficient of interest is $Market_{t-1} * Own_{it}$. This corresponds to the difference in extrapolation between those who own and do not own the assets.

Table 2.8 shows that owners of stocks extrapolate significantly more than those that do not own stocks. Panel A examines the percent measure, while Panel B examines the expectations above 50% dummy variable. The first two columns in Panel A present the probability of a market increase regressed on lagged quarterly market return. In Column 1, the *Own* dummy has a coefficient of 13.26 and is significant at the 1% level, indicating that asset owners are about 13% more optimistic than non-asset owners. This is consistent with more optimistic people selecting into owning stocks. The coefficient on lagged market returns is 14.22 and significant at the 1% level. This indicates that those who do not own assets extrapolate based on past market performance. Most important for our investigation, the coefficient on the interaction term with ownership is 17.85 and is significant at the 1% level. This indicates that those who own assets extrapolate from recent signals at roughly *twice* the level of those who do not.

The decision to own stocks is correlated with other demographic variables, so it could

about the future.

be that the ownership effects reported in Column 1 capture differences in demographic attributes. Column 2 presents the analysis including a large number of controls; specifically, dummy variables for sex, race, age, geographic region, education, and income. Interestingly, the coefficient on *Own* nearly halves, which indicates that a significant amount of the base level of optimism between owners and non-owners can be accounted for with demographic variables. That being said, the estimates of extrapolation are robust to demographic controls; if anything, the difference in extrapolation between owners and non-owners becomes larger upon their inclusion.[43] The coefficient for those who do not own the assets is 13.89 while the interaction term has a coefficient of 18.99 — both significant at the 1% level. Even after adjusting for differences in observables, asset owners extrapolate about twice as much as non-asset owners, consistent with the results we observed in the experiment.

Lastly, we explore a variety of different lags of market performance and find similar results across the board. In the 16 specifications using various lags of past market performance, two measures of future expectations, and with various demographic controls, we find that owners of assets extrapolate more than non-owners, with each specification significant at the 1% level.

Discussion and Conclusion

In this paper, we examine how owning a good affects learning and beliefs about its underlying quality. We find that upon receiving a negative signal about a good they own, people become systematically more pessimistic. They underestimate the good's quality both compared to receiving the same signal about a good they do not own and a normative benchmark. We observe the reverse pattern after observing positive signals: people overestimate the good's quality relative to seeing the same signals for goods they do not own and the normative

43. To further demonstrate robustness, the Internet Appendix repeats the analysis including interactions of the demographic controls with year by month fixed effects, thereby allowing for time varying effects of demographics on beliefs. Results are materially similar.

benchmark. In exploring the mechanism, we demonstrate that ownership channels attention, leading to overreaction and over-extrapolation from recent signals for goods that are owned. This provides support for a "more is less" effect of attention, whereby more attention leads to less accurate judgments. Finally, we present evidence that the relationship between ownership and over-extrapolation may be due to the associative nature of signal recall. In what follows, we discuss the implications of ownership on learning and beliefs in environments with trade, endogenous information, and outline directions for future research.

Ownership and Learning in Markets with Trade

We designed our experimental paradigm to identify the causal effect of ownership on learning and beliefs and thus do not allow for trade. As-if exogenous ownership is important in our setting for similar reasons as to why goods are randomly assigned to study the endowment effect: it ensures that observed differences are driven by ownership rather than ex-ante disparities in preferences or beliefs. In settings with trade, people select to hold assets that they believe are superior to the alternatives. This is nicely demonstrated in the experiments of Kuhnen and Knutson (2011) and Kuhnen (2015): participants are more likely to select a stock over a bond as their priors about the former increase. Since ownership is a function of beliefs, and not vice versa, differences in learning can be attributed to disparities in priors rather than ownership *per se*. A large literature in psychology has shown that, depending on the context, people overweight or underweight information that conforms to their prior (Klayman and Ha (1989); see also Klayman (1995) and Nickerson (1998) for reviews).[44]

We also precluded trade because the ability to buy and sell leads to self-selection precisely on the variable of interest— reactions to signals—which biases the data in favor of finding asymmetric updating. In a setting with trade, owners who have more extreme reactions to

44. Controlling for prior beliefs would not solve this issue because it requires that changes in beliefs in response to signals are constant with respect to belief levels—an assumption that is unlikely to be satisfied, especially when beliefs are bounded (e.g. probabilities).

positive news are more likely to hold their position and remain owners, while owners who have more extreme reactions to negative news are more likely to sell and become non-owners.

When estimating the influence of ownership on beliefs, this selection process leads to an overestimation of reactions to positive signals and an underestimation of reactions to negative signals. For example, consider the case where ownership leads to greater over-extrapolation (as we document in this paper). After a negative signal, greater over-extrapolation by owners *increases* the probability of selling, with those who over-extrapolate the most being the most likely to sell. As a result, the beliefs among those who maintain ownership will be more positive than the estimate without trade. After a positive signal, greater over-extrapolation by owners *decreases* the probability of selling. In turn, the selection effect will lead to a positive bias in measured beliefs of owners because those who over-extrapolate the most are the least likely to sell. This pattern will lead to ownership being associated with asymmetric belief updating in a setting with trade, despite the underlying causal effect being symmetric.

To demonstrate the difficulty of examining the impact of ownership in settings with trade, we ran a version of our experiment where participants could buy and sell assets in every round. The design is otherwise identical to our Discrete treatment.[45]

Allowing for trade, ownership appears to generate a different pattern in belief updating than the one documented in our main study. Figure 2.4 Panel A shows how participants who retained ownership reacted to negative signals (left figure) versus positive signals (right figure). Blue (red) bars represent the average change in belief upon seeing a signal about an owned (non-owned) good. We document what appears to be asymmetric updating, which could be interpreted as evidence of motivated reasoning or confirmation bias. Owners appear to update more on positive signals than negative ones. They also respond more to positive signals than non-owners, while the response to negative signals is similar between owners and non-owners.

45. Details on the methodology and analysis can be found in the Internet Appendix.

However, the figure is not an accurate depiction of belief updating as a function of ownership. The blue bars exclude people who responded to signals by selling, thus selecting out of ownership. As shown in Figure 2.4 Panel B, the pattern of belief-updating changes substantially when the sample of owners includes those who sold in response to the prior signal. After correcting for this selection, we find a symmetric effect similar to what we document in the paper: people update more in response to *both* positive and negative signals about owned goods compared to non-owned goods. The Internet Appendix presents these results in a regression framework.

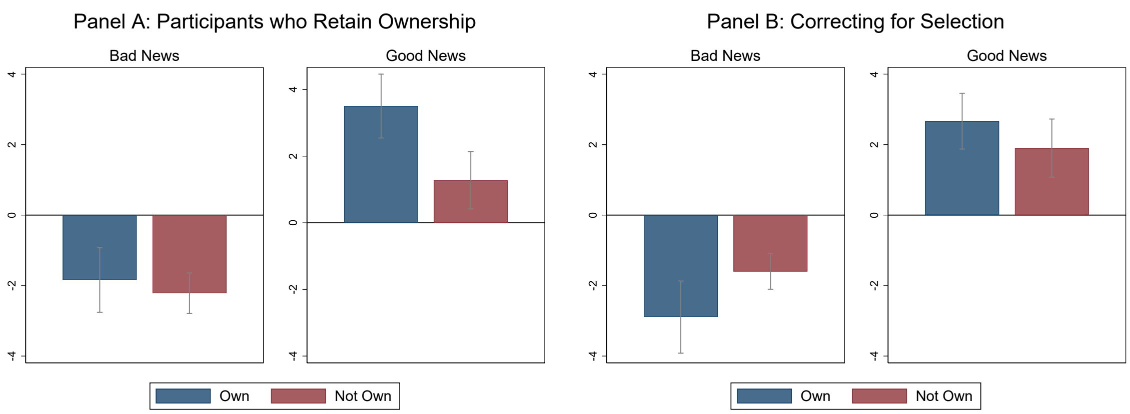


Figure 2.4. Belief Updating by Ownership in Trade Treatment. The figure shows the average change in belief in response to a price decrease (the left "Bad News" figure) and a price increase (the right "Good News" figure). In Panel A ownership is defined as holding the asset after the trade decision, when beliefs are elicited. Panel B includes the owners in Panel A as well as those who sold in response to the prior signal. Gray bars represent the 95% confidence intervals.

Our results illustrate the importance of understanding the influence of ownership as distinct from trade. The asymmetric updating that we document in the trade paradigm is quite different from the patterns documented elsewhere in the paper. On the other hand, the pattern is similar to what has been shown in other trade paradigms, such as Kuhnen and Knutson (2011) and Kuhnen (2015). After correcting for selection effects in only the prior round, a pattern more similar to what we document in the paper is found. With that said, it is still not possible to cleanly address further confounds, such as selection in other rounds and endogeneity based on differing priors.

This demonstrates the necessity of studying the causal effect of ownership in a setting with exogenous ownership and no trade. We believe that this is important to document because, for example, many economically important settings involve learning before there is an opportunity to trade, e.g. in the case of illiquid goods or situations where there are barriers to trade in place. Additionally, even in settings with trade, our results imply that previous owners will have systematically different beliefs than non-owners.

Ownership and Learning from Endogenous Information and Experience

Our experimental paradigm explored learning as a function of exogenously generated signals of quality. This design choice allowed us to study belief updating as a function of ownership while shutting down other channels that could interfere with inference. This section discuses learning from prices that are generated endogenously through the actions of market participants, as well as the influence of market experience on ownership effects.

While it would be interesting to explore the interaction between ownership and learning from endogenously formed prices, it is difficult to do so while cleanly identifying the influence of ownership. Prices that are endogenously generated, such as from trading decisions or auction mechanisms, will confound the valence of information and ownership effects because good news for owners may be bad news for non-owners (and vice versa) in these settings. This is because prices will endogenously reflect not only the quality of the underlying assets, but also differences in the preferences and beliefs of owners and non-owners. For example, if ownership generates an endowment effect where there is an initial WTA-WTP gap in valuations, then the transaction prices will likely fall between this WTA and WTP. This signal will be interpreted as "bad" news for the owners and "good" news for the non-owners. Our paper shows that both owners and non-owners update positively to good news and negatively to bad news—the documented effect is on the extent of updating rather than the direction. Thus, in response to viewing endogenous prices in the absence of new exogenous signals, the initial WTA-WTP gap should converge because the same signal is perceived with a different valence based on ownership. This can account for why repeated exposure to endogenously generated prices mitigates the endowment effect. Shogren et al. (2001) and Loomes, Starmer, and Sugden (2003) show that an initial endowment effect disappears after multiple rounds of auctions where participants view the endogenously determined price each round. This pattern can be explained by owners and non-owners updating their beliefs in accordance to the mechanism proposed in this paper, but perceiving the same price signal with a different valence as a function of ownership.

However, this process does not speak to how owners and non-owners learn from signals of the *same* valence. The convergence of the WTA-WTP gap neither provides direct evidence for nor rules out the ownership-driven over-extrapolation documented in the current paper. Examining how ownership affects learning requires a setting where owners and non-owners observe the same signal that is interpreted as having the same valence for both (e.g. a price decrease that is interpreted as bad news about quality by both owners and non-owners). While the endowment effect may be mitigated when information is endogenously generated by market participants, we show that exogenous positive news exacerbates it.

Though some market signals are endogenous to participants' preferences and beliefs, many common sources of value-relevant information — such as earnings announcements, patent applications, product launches, etc. — are exogenous. Additionally, large idiosyncratic price movements are typically in response to such information, and in these cases, the price signal will have the same valence for both owners and non-owners. The survey results in Section 2 provide suggestive evidence that owners and non-owners *do* respond differently to market prices in a manner consistent with ownership-driven over-extrapolation, and this has significant implications for investor sentiment and expectations.

A related topic is whether the disciplining hand of market experience will weaken the influence of ownership on learning. For example, List (2003) argues that professional experience makes the endowment effect disappear.[46] While this question is outside the scope of the current research, Section 2 illustrates that the impact of market experience on heuristics and biases is far from settled. While some find that market experience disciplines biases and eliminates decision errors, there is also evidence suggesting this is not always the case. An interesting area for future research would examine how market dynamics and professional experience interact with the effect of ownership on learning and beliefs.[47]

Attention: More is Less versus More is More

The 'more is less' effect of attention we document provides some of the first empirical evidence for the argument in Dawes (1979) that attention can degrade decision-making by leading people to place greater weight on normatively irrelevant information. The Internet Appendix formally derives a potential mechanism that can account for the empirical patterns in our paper. In the context of this model, our results suggest that attention exacerbates over-extrapolation by leading people to incorporate redundant information—prior signals— into the judgment process. This effect may be due to a mistaken theory for belief updating (Gagnon-Bartsch, Rabin, and Schwartzstein, 2018; Handel and Schwartzstein, 2018), where redundant signals are assumed to have informational content (Eyster, Rabin, and Weizsacker, 2015).

While we contribute to the literature by empirically demonstrating a 'more is less effect'

46. Notably, the endowment effect experiment in our paper focuses on the reaction to news rather than the level of the WTA-WTP gap. It would be interesting to know if professionals who do not have an endowment effect will nonetheless exhibit differential learning as a function of ownership. We leave this for future research.

47. Whether or not market forces influence how such a bias is manifested, the documented effect of ownership on beliefs and learning may still have significant implications in economically important environments. As noted in Section 2, perturbations to the decision environment leads to the reemergence of behavioral biases even in simple learning settings, suggesting that documenting and exploring them remains important. List (2020) makes a similar argument when he states "Does the conclusion that market experience attenuates the endowment effect lead one to conclude that KKT has no value outside of theory? Absolutely not."

of attention, we emphasize that this does not imply that ownership always leads to less accurate beliefs. The more general conclusion is on a relative (i.e. comparative static) effect of ownership and attention: we predict that ownership-driven attention will lead to more extreme updating to information and increase over-extrapolation of recent signals, but this does not imply that inattentive inference will lead to more calibrated beliefs in all settings. In some contexts, such as when individuals have access to sufficient statistics for Bayesian updating, inattentive inference may lead people to employ efficient updating heuristics (Anderson and Sunder, 1995; Green and Daniels, 2018) or a naïve use of Bayes rule (Barash et al., 2019). This is likely to be the case in many economically important environments; for example, Grossman (1976) argues that market prices act as sufficient statistics for this purpose. In these environments owners are predicted to have less well-calibrated beliefs than non-owners and overreact to signals. However, while our findings suggest that greater attention will exacerbate over-extrapolation (relative to the normative benchmark) across a broad variety of settings, it may lead to better calibrated beliefs (relative to the inattentive case) in environments where information aggregation requires recall. For example, take a decision that involves accurately estimating electric-car mileage. Our recall experiment implies that owners of a Tesla will be paying more information to developments on this issue, commit relevant information to memory, and be more likely to recall it correctly than non-owners.[48] At the same time, owners will over-extrapolate from value-relevant information and thus be more optimistic or pessimistic than non-owners, depending on the valence of prior signals, when trading goods or assets related to the company. Additionally, in settings where attention leads people to shift away from heuristics towards more deliberative processing, such as when individuals have the correct mental models but cognitive costs lead them to form noisy expectations (Stanovich and West, 2008; Gabaix and Laibson, 2017; Imas, Kuhn, and Mironova, 2019), ownership is likely to lead to learning closer to normative

48. We thank an anonymous referee for providing this example.

benchmarks.

Future Research

We demonstrate an overreaction to information about owned goods. However, prior work has shown that people overreact to information in some settings (Bordalo et al., 2018; Frydman and Nave, 2016) and underreact in others (Edwards, 1982; Barry and Pitz, 1979). We discuss some potential factors that generate over- or under-reaction above, but predicting which effect will dominate in a given setting is beyond the scope of the current paper.[49]

Our findings leave open a number of interesting questions regarding how the documented phenomenon interacts with contextual factors and orthogonal psychological mechanisms which could be related to ownership. Our evidence on the attentional mechanism suggests that the documented relationship between ownership and learning is perceptual. In settings where more cognitive factors like wishful thinking play a larger role, such as when ownership is linked to identity, a level effect of greater optimism may indeed arise (e.g., Mobius et al. 2011). Conditional on this level effect, however, we would still anticipate an interaction between ownership status and the valence of incoming signals.

In our studies, we largely followed the work in economics by using value-relevance as a sufficient condition for ownership (e.g. Kahneman, Knetsch, and Thaler (1990); Ericson and Fuster (2014)). However, the rich literature across the social sciences suggests that value-relevance is not a necessary condition for *psychological* ownership (Jussila et al., 2015; Pierce and Jussila, 2010; Shu and Peck, 2011). Future research should explore the boundaries and moderators of the effects documented here as a function of psychological ownership: would

49. Massey and Wu (2005) attempt to reconcile the evidence on over- versus under-reaction by distinguishing between two types of uncertainty: uncertainty over which system is generating a signal and the signal diagnosticity. They argue that people pay too much attention to the signal and not enough to its diagnosticity and the stability of the system. This generates overreaction when the system is stable and the signals are noisy, but under-reaction when the system is unstable and the signals are precise. Note that the former characterization applies to our setting, as well as other settings that have found overreaction (e.g. Frydman and Nave (2016)), in that fundamental quality remains stable.

our results extend to settings with multiple owners of the same good, or to identity-based goods with no extrinsic payoffs? Additionally, the relationship between ownership and direct versus indirect experience is an interesting direction for future work. For example, Simonsohn et al. (2008) show that the behavior of players in a repeated prisoner's dilemma is influenced more by direct interactions with others compared to observing similar interactions as a third party. Lastly, future research should examine the ownership effect in settings where attention has an unambiguously positive effect on accuracy.

Our results have implications for the measurement of psychological frictions from data in settings that involve ownership. Economic analysis typically assumes that owners and non-owners form beliefs using the same process, explaining differences in behavior through preferences. Our findings suggest that ignoring the influence of ownership on the learning process can lead to erroneous conclusions. For example, one of the most well-documented behavioral anomalies is the disposition effect, where people are more prone to sell a good after a gain than a loss.[50] Our results imply that that this behavioral pattern cannot be driven by beliefs, suggesting that studies likely understate the psychological frictions stemming from preferences by ignoring the influence of ownership on beliefs.[51] More generally, ascribing a result to preferences rather than beliefs may lead to different conclusions about the underlying mechanism, which in turn can lead to different policy prescriptions.[52]

The results also have significant implications for the dynamics of trade volume in response to public signals. As demonstrated in our endowment effect experiment, the valuation gap

50. Beginning with the initial discussion of the phenomenon (Shefrin and Statman, 1985b) and demonstration in a large brokerage data set (Odean, 1998), the disposition effect has been replicated in a variety of settings (e.g. equity and housing markets) and with different types of market participants (e.g. retail and day traders) (Kaustia, 2010).

51. A belief in mean reversion has been offered as a potential explanation for this effect: people hold on to losers and sell winners because they believe that the former will go back up and the latter will go back down. While many studies have proposed preference-based mechanisms such as realization utility for the disposition effect (Barberis and Xiong, 2012), belief in mean reversion has not been ruled out — largely due to a lack of data on beliefs in trading contexts (Barber and Odean, 2013).

52. For example, Bohren et al. (2019) argue that wrongly ascribing discrimination to preferences rather than beliefs can lead to vastly different implications for policy.

between owners and non-owners shrinks in response to bad news and expands in response to good news. This should increase the potential for trade in the former case and decrease it in the latter case. Future research should explore these dynamics in observational and experimental data.

Tables

Table 2.1

Beliefs by Returns and Ownership

This table shows how beliefs vary with ownership based on returns. *Own* is a dummy variable equal to one if the good was purchased by the subject. *Return* is the level of returns. Fixed effects are indicated below the regression results. Standard errors are clustered by subject, and t-statistics are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)
Own*Return	0.0763***	0.114***	0.0865***
	(3.68)	(6.00)	(5.39)
Return	0.510***		
	(26.27)		
Own	-0.619	-0.343	-0.296
	(-1.37)	(-0.82)	(-0.70)
Ret x Round FE	No	Yes	Yes
Subject FE	No	No	Yes
R^2	0.322	0.375	0.558
Observations	51390	51390	51390

Table 2.2

Belief Errors by Returns and Ownership

This table shows how belief errors relative to a benchmark vary with ownership based on returns. Columns labeled *All* include all the baseline data relative to their benchmark. Those labeled *Discrete Treatment* include only data from the Discrete treatment, which uses the discrete initial prior provided in the instructions. Those labeled *Continuous Treatment* include only data from the Continuous treatment which uses an initial prior of $\beta\,(2.62)$. *Own* is a dummy variable equal to one if the good was purchased by the subject. *Return* is the level of returns. Fixed effects are indicated below the regression results. Standard errors are clustered by subject, and t-statistics are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	All		Discrete Treatment		Continuous Treatment	
	(1)	(2)	(3)	(4)	(5)	(6)
Own*Return	0.105***	0.0780***	0.131***	0.0816**	0.0988***	0.0769***
	(5.67)	(5.00)	(3.40)	(2.42)	(4.68)	(4.34)
Return	-0.0174	-0.00799	-0.00579	0.0232	-0.0233	-0.0170
	(-0.95)	(-0.48)	(-0.16)	(0.71)	(-1.10)	(-0.87)
Own	-0.616	-0.546	-0.313	-0.389	-0.720	-0.631
	(-1.46)	(-1.29)	(-0.38)	(-0.47)	(-1.45)	(-1.27)
Subject FE	No	Yes	No	Yes	No	Yes
R^2	0.00724	0.296	0.0105	0.365	0.00631	0.266
Observations	51390	51390	14490	14490	36900	36900

Table 2.3

Extrapolation of Signals and Ownership

This table shows how beliefs vary with recent price changes based on ownership. *Price Increase* is a dummy variable which is equal to one if the good experienced a price increase in the prior round. In Panel A, column 1 the dependent variable is the raw belief. In Column 2 it is the belief error relative to a Bayesian benchmark. In Column 3 it is the belief error relative to priors calibrated separately for owned and non-owned positions, indicated by $\beta\,(Own)\ Error$. Column 4 uses priors based on the average parameter from subjects from the prior round by price by ownership condition, indicated by $\beta\,(Round\,Own)\ Error$. Panel B examines raw belief as the dependent variable. *Ret* indicates a linear control for return. *Ret Dummy* indicates a dummy variable for intervals of 10% returns. Below are interactions for those variables with the *Own* dummy variable. Fixed effects are indicated in the bottom row. Standard errors are clustered by subject, and t-statistics are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Extrapolation Relative to Benchmarks

	Belief	Benchmark Error	β(Own) Error	β(Round Own) Error
	(1)	(2)	(3)	(4)
Own*(Price Increase)	5.029***	3.874***	2.460***	2.693***
	(8.02)	(7.54)	(4.85)	(4.86)
Price Increase	12.32***	-0.982**	-0.560	2.713***
	(22.70)	(-2.07)	(-1.18)	(5.37)
Own	-2.050***	-2.277***	-1.412***	-1.330**
	(-2.93)	(-4.58)	(-2.86)	(-2.48)
R^2	0.0937	0.00322	0.00135	0.0116
Observations	48930	48930	48930	42401

Panel B: Extrapolation Relative to Return Controls

	(1)	(2)	(3)	(4)
Own*(Price Increase)	3.525***	4.184***	2.203***	1.914***
	(6.48)	(7.79)	(5.59)	(4.82)
Price Increase	1.315***	-0.0608	1.990***	1.060***
	(3.23)	(-0.15)	(5.60)	(2.91)
Own	-2.318***	-2.333***	-1.829***	-0.840
	(-4.39)	(-4.49)	(-3.56)	(-1.21)
Ret	Yes	No	Yes	No
Ret Dummy	No	Yes	No	Yes
Own x Ret	No	No	Yes	No
Own x Ret Dummy	No	No	No	Yes
R^2	0.332	0.351	0.333	0.353
Observations	48930	48930	48930	48930

Table 2.4

Level of Attention and Ownership

This table examines differences in beliefs and belief errors based on levels of attention. Attention is measured as High Attention, reaction time below the mean response time, in Panel A and Low Attention, reaction time above the mean response time, in Panel B. *Own* is a dummy variable equal to one if the good was purchased by the subject. *Return* is the level of returns. Standard errors are clustered by subject, and t-statistics are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: High Attention

	Belief			Belief Error	
	(1)	(2)	(3)	(4)	(5)
Own*Return	0.121**	0.100**	0.167***	0.111**	0.187***
	(2.34)	(2.01)	(3.13)	(2.14)	(3.29)
Return	0.541***			0.0370	0.0440
	(11.19)			(0.75)	(0.93)
Own	-0.381	-0.0243	0.0339	-0.504	-0.384
	(-0.32)	(-0.02)	(0.03)	(-0.44)	(-0.33)
Ret x Round FE	No	Yes	Yes	No	No
Subject FE	No	No	Yes	No	Yes
R^2	0.298	0.361	0.607	0.0133	0.401
Observations	4122	4122	4122	4122	4122

Panel B: Low Attention

	Belief			Belief Error	
	(1)	(2)	(3)	(4)	(5)
Own*Return	-0.00400	0.0143	0.0736	0.00372	0.106
	(-0.05)	(0.15)	(1.01)	(0.04)	(1.39)
Return	0.491***			-0.0513	-0.0468
	(7.26)			(-0.72)	(-0.73)
Own	0.855	0.911	1.022	0.941	1.112
	(0.77)	(0.73)	(0.82)	(0.83)	(0.91)
Ret x Round FE	No	Yes	Yes	No	No
Subject FE	No	No	Yes	No	Yes
R^2	0.144	0.227	0.607	0.00215	0.496
Observations	3102	3102	3102	3102	3102

Table 2.5

Difference Across Attentional Study and the Main Study

This table shows how beliefs and extrapolation vary in the attentional study. Panel A explores beliefs and belief errors based on returns while Panel B explores the degree of extrapolation based on a positive price signal the prior period. Regressions include the main study and the data from the attentional study. *No Own Treat* is equal to one if the data is from the treatment condition. *Own* is equal to one if the good is owned and the observation is from the main study. Regressions also include *No Own Treat* and *Own* dummy variables. Columns labeled Belief examine raw beliefs while columns labeled Belief Error examine belief errors relative to a Bayesian. The attentional treatment uses the Continuous treatment, so only data from the Continuous treatment in the main study is used in this analysis. Fixed Effects are indicated in the bottom row. Standard errors are clustered by subject, and t-statistics are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Returns

	Belief		Belief Error
	(1)	(2)	(3)
(No Own Treat)*Return	0.170***	0.185***	0.178***
	(3.63)	(4.01)	(3.98)
Own*Return	0.0715***	0.116***	0.0986***
	(2.95)	(5.28)	(4.68)
Return	0.505***		-0.0230
	(22.23)		(-1.09)
Ret x Round FE	No	Yes	No
R^2	0.343	0.403	0.00905
Observations	40275	40275	40275

Panel B: Extrapolation

	Belief		Belief Error
	(1)	(2)	(3)
(No Own Treat)*(Price Increase)	3.570*	4.811***	4.707***
	(1.85)	(3.46)	(3.42)
Own*(Price Increase)	4.980***	4.208***	3.781***
	(6.41)	(6.37)	(6.02)
Price Increase	12.64***	-3.219***	-1.157**
	(18.82)	(-5.50)	(-1.99)
Ret x Round FE	No	Yes	No
R^2	0.0967	0.410	0.00356
Observations	37590	37590	37590

Table 2.6

Recall and Ownership

This table explores the relationship between recall, absolute value of returns and ownership. A dummy variable equals one if the direction of the signal is recalled correctly, is regressed on the absolute value of returns and the *Own* dummy variable in Columns 2 through 4. Column 3 includes observations where the most recent signal realization and the previous signal match. Column 4 includes observations where they do not match. t-statistics are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	All		Match	No Match
	(1)	(2)	(3)	(4)
Own		0.102**	0.177***	0.0228
		(2.16)	(3.31)	(0.31)
Absolute Value of Return	0.00613***	0.00649***	0.00555***	0.00467
	(4.56)	(4.81)	(4.26)	(1.62)
Observations	402	402	219	183

Table 2.7

Endowment Effect Updating based on Ownership

This table shows how the value of a good varies with ownership based on its ratings. *Own* is a dummy variable equal to one if the subject was endowed with the good. *Rating* is the average star rating for a product in that round. *Last Rating* is the most recent rating. Fixed effects are indicated below the regression results. Regressions in the all studies column contain a fixed effect for the treatment. Standard errors are clustered by subject, and t-statistics are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Cumulative Rating

	(1)	(2)	(3)	(4)	(5)
Own*Rating	3.783***	1.405**	3.786***	3.771***	1.334*
	(3.39)	(2.03)	(3.39)	(3.37)	(1.95)
Rating	2.391***	3.591***	2.323***		
	(3.49)	(6.68)	(3.34)		
Own	3.620***	4.688***	3.607***	3.548***	4.640***
	(2.98)	(3.93)	(2.97)	(2.92)	(3.90)
Subject FE	No	Yes	No	No	Yes
Round FE	No	No	Yes	No	No
Review x Round FE	No	No	No	Yes	Yes
R^2	0.121	0.635	0.124	0.130	0.644
Observations	2650	2650	2650	2650	2650

Panel B: Last Rating

	(1)	(2)	(3)	(4)	(5)
Own*Last Rating	1.757***	0.890***	1.714***	1.723***	0.849***
	(5.41)	(3.96)	(5.28)	(5.31)	(3.88)
Last Rating	2.286***	2.723***	2.535***		
	(8.17)	(10.59)	(8.08)		
Own	3.525***	4.132***	3.569***	4.032***	4.644***
	(2.89)	(3.36)	(2.96)	(3.54)	(4.06)
Subject FE	No	Yes	No	No	Yes
Round FE	No	No	Yes	No	No
Review x Round FE	No	No	No	Yes	Yes
R^2	0.0837	0.611	0.0862	0.117	0.644
Observations	2650	2650	2650	2650	2650

Table 2.8

Field Data Extrapolation by Ownership

This table shows how extrapolation of prior market performance varies with ownership. Panel A examines the probability of a stock market increase over the next 12 months and Panel B examines a dummy variable equal to one if this is greater than 50. Prior market return is from month -m to -1, with m indicated in each column. *Own* is a dummy variable equal to one if the subject owns stocks. Demographics indicate fixed effects for income, age, race, marital status and education. Standard errors are clustered by month, and t-statistics are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Probability of Increase

	3 Month=m		6 Month=m		1 Year=m		2 Year=m	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Own*Mkt[-m,-1]	17.85***	18.99***	18.22***	18.86***	14.16***	14.61***	8.427***	8.516***
	(4.28)	(4.41)	(8.49)	(8.90)	(8.47)	(8.89)	(7.63)	(7.65)
Mkt[-m,-1]	14.22***	13.89***	9.857***	9.775***	9.734***	9.217***	7.366***	7.140***
	(2.73)	(3.42)	(2.90)	(3.56)	(4.78)	(5.74)	(5.04)	(6.25)
Own	13.26***	8.074***	12.75***	7.562***	12.20***	7.010***	11.97***	6.833***
	(47.87)	(30.48)	(48.50)	(29.45)	(41.02)	(24.60)	(33.82)	(20.32)
Demographics	No	Yes	No	Yes	No	Yes	No	Yes
R^2	0.0514	0.112	0.0546	0.116	0.0591	0.120	0.0601	0.121
Observations	98828	92264	98828	92264	98828	92264	98828	92264

Panel B: Increase Probability >50

	3 Month=m		6 Month=m		1 Year=m		2 Year=m	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Own*Mkt[-m,-1]	0.240***	0.253***	0.243***	0.247***	0.182***	0.183***	0.105***	0.104***
	(3.98)	(4.01)	(7.29)	(7.16)	(6.69)	(6.80)	(5.68)	(5.64)
Mkt[-m,-1]	0.209**	0.204***	0.149***	0.151***	0.154***	0.151***	0.116***	0.114***
	(2.44)	(2.88)	(2.77)	(3.37)	(4.66)	(5.58)	(5.05)	(6.20)
Own	0.190***	0.108***	0.183***	0.101***	0.176***	0.0950***	0.174***	0.0931***
	(43.23)	(23.89)	(42.31)	(22.12)	(34.62)	(18.29)	(29.00)	(15.92)
Demographics	No	Yes	No	Yes	No	Yes	No	Yes
R^2	0.0395	0.0910	0.0419	0.0935	0.0454	0.0968	0.0462	0.0973
Observations	98828	92264	98828	92264	98828	92264	98828	92264

CHAPTER 3

THE INTERPLAY OF BELIEFS AND PREFERENCES IN THE DISPOSITION EFFECT

Introduction

Behavior is determined by both beliefs and preferences. Many systematic biases can be explained by non-standard preferences, biased beliefs, or some combination of the two, but preferences usually receive more focus. This paper examines the disposition effect, the tendency for people to sell gains and hold losses in the stock market.This tendency has been shown in both real world and experimental asset markets (see Kaustia (2010) for a review. Real World: Shefrin and Statman (1985b); Odean (1998); Heimer and Imas (2020); Experimental: Weber and Camerer (1998); Frydman and Rangel (2014); Frydman, Hartzmark, and Solomon (2015); Heimer and Imas (2020)). The predominant explanations are preference based with a particular focus on Prospect Theory preferences (Shefrin and Statman, 1985a; Odean, 1998) and models of realization utility (e.g., Barberis and Xiong, 2009; Ingersoll and Jin, 2013; Magnani, 2014).

By contrast, there is relatively little direct examination of belief biases and their role in the disposition effect (Jiao, 2017; Grosshans, Langnickel, and Zeisberger, 2018). A growing literature on the role of belief biases in experimental asset markets has documented a number of important belief distortions including differences in learning from gains and losses (Kuhnen, 2015), about goods that are owned (Hartzmark, Hirshman, and Imas, 2019), and about goods that have been previously purchased (Kuhnen, Rudorf, and Weber, 2017).[1] While a belief in mean reversion (Jiao (2017), cf Kadous et al. (2014)) has been proposed to explain the disposition effect, the way in which belief biases affect the interpretation of the

1. The literature on belief biases outside of experimental asset markets is large as well. See Benjamin (2019) for a broader review of this literature

disposition effect is underexplored.

Using data from an experimental asset market modeled on Frydman and Rangel (2014), I show in both reduced form and structural estimates that using subjective beliefs alters the conclusions drawn about the impact of gains and losses on trading behavior.[2] I first document that participants' beliefs are conservative relative to Bayesian updating in my sample. In particular, they underrespond to new information relative to Bayesian updating. This leads participants to be overly optimistic for low levels of Bayesian beliefs and overly pessimistic for higher values. This finding is consistent with literature on the strength and weight of evidence (Griffin and Tversky, 1992; Massey and Wu, 2005). I also examine belief updating separately for owned and non-owned goods, following Hartzmark, Hirshman, and Imas (2019), and replicate their finding that owners extrapolate more than non-owners. Additionally, updating is symmetric (Barron, 2020).

I then turn to examination of peoples' decisions to sell. I show that participants display an aggregate disposition effect of .14, significantly above 0, using the Odean (1998) measure. Notably, peoples' selling decisions are well-aligned with their own beliefs, but not with Bayesian beliefs. Participants' selling decisions are negatively related to their self-reported beliefs, that is as people believe the asset is more likely to rise in price they are less likely to sell it. A similar analysis using Bayesian beliefs shows a significant positive relationship, that is people become more likely to sell as the Bayesian belief increases. The relationship between subjective beliefs and selling strengthen when I account for gain-loss utility, which shows participants are more (less) likely to sell larger gains (losses). The estimates for the impact of gains and losses on selling increases when using subjective beliefs instead of Bayesian beliefs

Finally, I estimate preference parameters in a realization utility model based on Magnani (2014) using both subjective and Bayesian beliefs. I estimate significantly more utility

2. A similar experimental design is used in Frydman et al. (2014); Frydman, Hartzmark, and Solomon (2015); Heimer and Imas (2020) and Grosshans, Langnickel, and Zeisberger (2018)

curvature on average using subjective beliefs. I also find lower loss aversion when estimated using subjective beliefs. The combination of these two effects suggests a stronger emphasis on realization utility because the increase in curvature has a larger impact on estimated utility than the decrease in loss aversion.

Related Literature

My findings contribute primarily to two literatures, the literature on belief biases and the literature on the disposition effect. There is substantial disagreement in the literature on whether people update symmetrically or asymmetrically from signals. Bayesian updating implies that there should not be systematic deviations in the response to positive and negative signals. However, the literature in psychology on motivated reasoning suggests people will distort their opinion in a direction that is favorable to them (Kunda, 1990) and confirmation bias suggests people will distort their beliefs in the direction of what they already believe (Klayman, 1995; Kuhnen, Rudorf, and Weber, 2017). Recent research has found essentially all possible combinations of symmetry and asymmetry in a variety of different domains. In a self-relevant domain (Ertac, 2011), a financial domain (Kuhnen, 2015), and across self-relevant, financial, and non-value relevant domains (Coutts, 2019), there is evidence that people tend to overweight negative signals leading to excess pessimism. By contrast, other work has found that people tend to ignore negative signals, particularly in self-relevant domains, leading to excess optimism (Eil and Rao, 2011; Mobius et al., 2011). Finally, recent work in financial domains has found symmetric updating that is close to Bayesian (Barron, 2020) but also symmetric and extrapolative, leading to both excess optimism and excess pessimism (Hartzmark, Hirshman, and Imas, 2019). Even within the financial domain, most relevant to this paper, it is not clear whether signals are interpreted symmetrically or asymmetrically.

In addition to whether signals lead to symmetric updating, there is also disagreement over whether and why people underweight (e.g., Edwards, 1968) or overweight (e.g., Kahneman and Tversky, 1973) signals relative to Bayesian updating. To reconcile these two conflicting predictions, Griffin and Tversky (1992) propose that people overweight the representativeness, what they call the strength of evidence, and then adjust insufficiently for things like base rates and sample sizes, what they call the weight of evidence. They use this framework to explain signal overweighting, which occurs when evidence has high strength but low weight, and underewighting, when evidence has low strength and high weight. Building on Griffin and Tversky (1992), Massey and Wu (2005) propose that in detecting regime changes people neglect the system, analogous to the weight of evidence, and emphasize the signals observed, the strength of evidence. They propose a comparative static that as the transition probability increases, the extent of underweighting of signals should increase. This suggests that I should see underweighting relative to Hartzmark, Hirshman, and Imas (2019), who find overextrapoplation, because the underlying states in their task are fixed.

It is not clear how biases in learning will affect the disposition effect. Most of the experimental literature on the disposition effect has relied on the assumption of Bayesian learning in a well specified market structure (e.g., Weber and Camerer, 1998; Frydman and Rangel, 2014; Frydman et al., 2014) or abstracted away from learning entirely (Magnani, 2014) to account for beliefs. The main question of interest tends to be focused on how a particular manipulation will affect the disposition effect (e.g., the ability to "roll" mental accounts (Frydman, Hartzmark, and Solomon, 2015), the ability to use leverage (Heimer and Imas, 2020)). For example, Frydman and Rangel (2014) examine whether the tendency for investment accounts to provide the last purchase price for a stock influences the disposition effect. They show that removing this information does indeed reduce the disposition effect through its impact on the utility of capital gains and losses. An alternative approach in Magnani (2014) has been to change the decision environment to focus on liquidation timing instead of portfolio choice motives. However, there are a few examples that attempt to test the speculation in Odean (1998) and Weber and Camerer (1998) that an irrational belief in mean reversion can produce the disposition effect.

In Jiao (2017), some participants report beliefs about whether an asset with a static probability of increasing will go up or down in the last period, other participants decide how many of 10 shares of the asset to sell, and a third set of participants do both tasks. He finds a belief in mean reversion, even though the probability is static, in the prediction data and that belief explains about 17% of the between-subject disposition effect in his sample. By contrast, Kadous et al. (2014) find people only behave ”as if” a stock at a loss will mean-revert when they currently own the stock, not when they are following the stock to decide whether to buy it or not. They take this as evidence that a general belief in mean reversion does not explain the disposition effect.[3] Finally, my paper is closest in nature toGrosshans, Langnickel, and Zeisberger (2018), who elicit subjective beliefs about a single asset in a market with a similar price evolution structure. They find that beliefs do not significantly differ from Bayesian, and, as a result focus their analysis on relative impact of beliefs on buying and selling decisions. My paper differs on three dimensions. First, practically, I examine a market with a portfolio of assets and not just one. This allows me to examine standard measures of the disposition effect. Additionally, Hartzmark, Hirshman, and Imas (2019) show in a related setting that attention plays a substantial role in learning from price changes. Specifically, building on literature in cognitive psychology, they find that increased attention via ownership leads to increased responsiveness to returns. Having participants learn about multiple assets may decrease attention to any given asset, and, as a result, alter the learning process. Second, conceptually, the main focus of this paper is on the decision to sell assets and what we can learn about peoples' preference parameters if their beliefs differ from Bayesian updating. Grosshans, Langnickel, and Zeisberger (2018) focuses on the relative impact of beliefs on buying and selling behavior. These are related,

3. The differential learning patterns documented in Hartzmark, Hirshman, and Imas (2019) may complicate the interpretation of this paper.

but quite different questions. Third, our findings on learning differ substantially. While they find excess pessimism at low Bayesian values and excess optimism at higher values, I find the opposite. Numerous differences in design, as mentioned above, and in the pool of participants could contribute to these differences in findings.

The rest of the paper is organized as follows. Section 2 outlines the structure of the experimental asset market. Section 3 outlines the empirical specifications for the reduced form analyses. Section 4 presents the reduced form results. Section 5 describes the structural realization utility model and reports estimates. Section 6 concludes.

The Experimental Asset Market

Amazon Mechanical Turk participants (N=146) completed a 45 round investment task with three stocks, A, B, and C, modeled on Frydman and Rangel (2014) and Heimer and Imas (2020).[4] They were endowed with one share of each stock and 50 experimental dollars to begin the task. Data collection and analysis procedures were preregistered using AsPredicted.com.[5]

The task had two components, a belief elicitation component paired with changes in stock prices and a trading component. The first nine rounds were a learning period that did not include trading. Participants saw the price of one stock change per round in a set order and reported their beliefs about the likelihood each stock would increase the next time its price updated. They reported their subjective beliefs that the good would increase the next time its price moved $\hat{s}_{i,t}$ using a slider that ranged from 30% to 70%, the minimum and maximum possible given the structure of the market. Starting in the 10th round, on the first page for they round, participants observed a price change for one stock selected at

4. It is worth noting that many of the prior papers using similar paradigms have used quite sophisticated subject pools. Frydman and Rangel (2014) use Cal Tech students and Grosshans, Langnickel, and Zeisberger (2018) required "a study background in economics, finance or business administration". Heimer and Imas (2020) have, however, replicated the paradigm on MTurk.

5. https://aspredicted.org/blind.php?x=dp487n

random and reported their beliefs about all three stocks. On the second page, they were able to trade one randomly selected stock. If they did not own a share of the selected stock, they were offered the opportunity to buy it. If they did own a share, they were randomly offered the opportunity to buy another share or sell the stock. If they owned two shares, they were only given the option to sell. If a participant decided to buy a stock but did not have enough experimental dollars to do so, one of their other holdings was sold at random. There was no new price information presented on the second page.[6]

Participants in this market are price takers, but can learn about the quality of the assets from the direction of the price changes. The price paths for each stock evolved independently according to a Markov process. Each stock had an equal chance (50%) of starting in a good state, where it had a 70% chance of going up and a 30% chance of going down, or a bad state where those probabilities were reversed. In each round where its price changed, the stock had an 80% probability of continuing its current state and a 20% probability of switching states. Price changes were selected from a uniform distribution over 5, 10 or 15 experimental points. That is, if a good went up (down), it had an equal probability of going up (down) by 5, 10, or 15 experimental points. This structure implies momentum in the prices of the stocks. A stock that has recently gone down (up) should be considered more likely to continue to go down (up) in the future. As a result, a risk neutral Bayesian should on average, sell goods that have gone down recently and hold those that have gone up (Frydman and Rangel, 2014).

I calculate the Bayesian posterior, $q_{i,t}$, that stock i is in the good state in round t as follows. If the price does not update in round t, then $q_{i,t} = q_{i,t-1}$. If a stock receives a price update, z_t it either goes up, $z_t = 1$, or down $z_t = -1$. Using the notation from Frydman and Rangel (2014), we can then write the condition distribution of z_t as $Pr(z_t|s_{i,t} = Good) =$

6. One limitation of this design is that by asking beliefs before trading it may induce a stronger relationship between beliefs and selling behavior than we would observe in the absence of the belief elicitaiton. One design element that is helpful in mitigating that concern to a degree is that participants do not know which stock they will be able to trade when they are reporting their beliefs. Additionally, their exact reported beliefs are not available to them when they do trade. Second, we can benchmark the size of the aggregate disposition effect against prior work in this paradigm to see if the elicitation of beliefs seems to be altering behavior.

$(0.5 + 0.2z_t)$. We can then solve for $q_{i,t}$ in each round as follows

$$q_{i,t}(q_{i,t-1}, z_{i,t}) = \frac{Pr(z_{i,t}|s_{i,t} = \text{Good})Pr(s_{i,t} = \text{Good}|z_{i,t})}{Pr(z_{i,t}}$$

$$= \frac{Pr(z_{i,t}|s_{i,t} = \text{Good})Pr(s_{i,t} = \text{Good}|q_{i,t-1})}{Pr(z_{i,t}|s_{i,t} = \text{Good})Pr(s_{i,t} = \text{Good}|q_{i,t-1}) + Pr(z_{i,t}|s_{i,t} = \text{Bad})Pr(s_{i,t} = \text{Bad}|q_{i,t-1})}$$

$$= \frac{(.5 + .2z_{i,t})(.8q_{i,t-1} + .2(1 - q_{i,t-1}))}{(.5 + .2z_{i,t})(.8q_{i,t-1} + .2(1 - q_{i,t-1})) + (.5 - .2z_{i,t})(.2q_{i,t-1} + .8(1 - q_{i,t-1}))}$$

To compute the Bayesian posterior that the good will increase in price the next time its price changes $m_{i,t}$ we multiply the likelihood it is in the good state by the likelihood of increasing in that state:

$$m_{i,t} = .7q_{i,t} + .3(1 - q_{i,t})$$

At the end of the task, participants received a bonus based on the performance of the investments and the accuracy of one of their beliefs. All owned shares were sold at the final round price. Experimental dollars were converted to dollars at a rate of 100=\$1. In addition, a stock-round pair was selected at random and participants earned an extra \$1 if their beliefs were within 5% of the Bayesian belief $m_{i,t}$ (i.e. $|\hat{s}_{i,k,t} - m_{i,k,t}| <= 5$).[7] I chose to use this elicitation procedure as opposed to more complex mechanisms such as versions of the Binarized Scoring Rule (e.g. the quadratic scoring rule) due to recent evidence showing that the BSR can lead to less truthful reporting Danz, Vesterlund, and Wilson (2019). Participants received \$1.5 for completing the task and an average bonus of \$3.96.

7. One limitation of this incentive structure is the possibility that people hedge their market outcomes with their beliefs because they are incentivized with both. However, hedging predicts less sensitivity to signals for owned than non-owned goods. I find more sensitivity to signals for owned goods, which suggests people are not hedging.

Reduced Form Empirical Specifications

Because I am interested primarily in the way beliefs influence trade behavior, I restrict the data to rounds 10 and later, and use the data from rounds 1-9 to exclude participants whose responses are in the opposite direction of the Bayesian predictions in those rounds. My first analyses focus on the relationship between subjective beliefs and Bayesian beliefs. I use the following OLS regression:

$$\hat{s}_{i,t,k} = \beta m_{i,t,k} + \gamma X_k + \epsilon \tag{3.1}$$

where $\hat{s}_{i,t,k}$ and $m_{i,t,k}$ are the beliefs defined above, and the X_k are subject fixed effects. Under this specification, β significantly less than one suggests conservatism (Edwards, 1982), while β significantly greater than one suggests representativeness (Kahneman and Tversky, 1973). I also consider extrapolation from recent signals in two ways. One compares the change in subjective beliefs to the change in Bayesian beliefs:

$$\delta\hat{s}_{i,t,t-1,k} - \delta_{,i,t,t-1,k} = \beta z_{i,t,k} + \gamma X_k + \epsilon \tag{3.2}$$

The other looks at the change in subjective beliefs and allows me to assess whether participants are updating symmetrically:

$$\hat{s}_{i,t,k} - \hat{s}_{i,t-1,k} = \beta z_{i,t,k} + \gamma X_k + \epsilon \tag{3.3}$$

Under this specification, β represents the amount of updating following a negative or positive signal relative to no information. Following Hartzmark, Hirshman, and Imas (2020), I also examine how owning a stock changes participants response to information by interacting $m_{i,t,k}$ and $z_{i,t,k}$ with ownership. Importantly, only owners' beliefs factor into selling decisions.

I then turn to the decision to sell. The experimental data allow me to examine the disposition effect in an aggregate sense. I use the classic formulation from Odean (1998). I define the purchase price of the good as the reference price, paper gains as all situations when participants have the option to sell a stock at a gain relative to the reference price, and paper losses as all situations when participants have the option to sell a stock that is at a loss relative to the reference price. I compute the proportion of paper gains and losses that are realized at the subject level as follows

$$PGR = \frac{\text{\# of gains realized}}{\text{\# of gains realized} + \text{\# of paper gains}}$$

and

$$PLR = \frac{\text{\# of losses realized}}{\text{\# of losses realized} + \text{\# of paper losses}}$$

. The measure of the disposition effect is PGR-PLR, with PGR-PLR> 0 defined as exhibiting the disposition effect. A risk-neutral Bayesian in this setting would exhibit a -.56 on this measure.

While the Odean (1998) measure is useful for establishing the presence of a disposition effect, it does not provide scope for including beliefs. The key advantage of my design is that to examine both implied Bayesian beliefs as welll as subjects reported beliefs about the likelihood the stock will increase in price. To investigate the role of beliefs I restrict the rounds to ones in which subjects had the opportunity to sell. I then run two sets of OLS regressions with sale as the outcome variable. In the first set, I examine the simple impact of subjective and Bayesian beliefs on selling:

$$sold_{i,t,k} = \beta \hat{s}_{i,t,k} + \gamma X_k + \epsilon \tag{3.4}$$

and

$$sold_{i,t,k} = \beta \hat{m}_{i,t,k} + \gamma X_k + \epsilon \tag{3.5}$$

The coefficients on beliefs in these regressions indicates the relationship between the beliefs and the decision to sell. We would expect this relationship to be negative. That is as the likelihood of of a stock increasing in price goes up, the likelihood of selling it should go down, and vice versa.

In the second set, following Frydman and Rangel (2014), I include the capital gains/losses $CG_{i,t,k}$ associated with the stock. These are defined as the deviation of the current price from the last purchase price. The capital gains represent a measure of the role of preferences in decision to sell. I run the following OLS regression:

$$sold_{i,t,k} = \beta_1 \hat{m}_{i,t,k} + \beta_2 CG_{i,t,k} + \gamma X_k + \epsilon \tag{3.6}$$

and

$$sold_{i,t,k} = \beta_{1'} \hat{s}_{i,t,k} + \beta_{2'} CG_{i,t,k} + \gamma X_k + \epsilon \tag{3.7}$$

This specification allows me to disentangle the relative contribution of beliefs and preferences to the decision to sell. I also compare β_2 to $\beta_{2'}$ to quantify the impact of misspecifying beliefs on the implied preference parameters.

Reduced Form Results

I exclude thirty-four participants whose beliefs are negatively correlated with the Bayesian belief in the first 9 rounds of the task. The data from these rounds are are not used in the subsequent analyses because these rounds are purely learning rounds. I am focused on selling decisions primarily, which only begin in round 10.

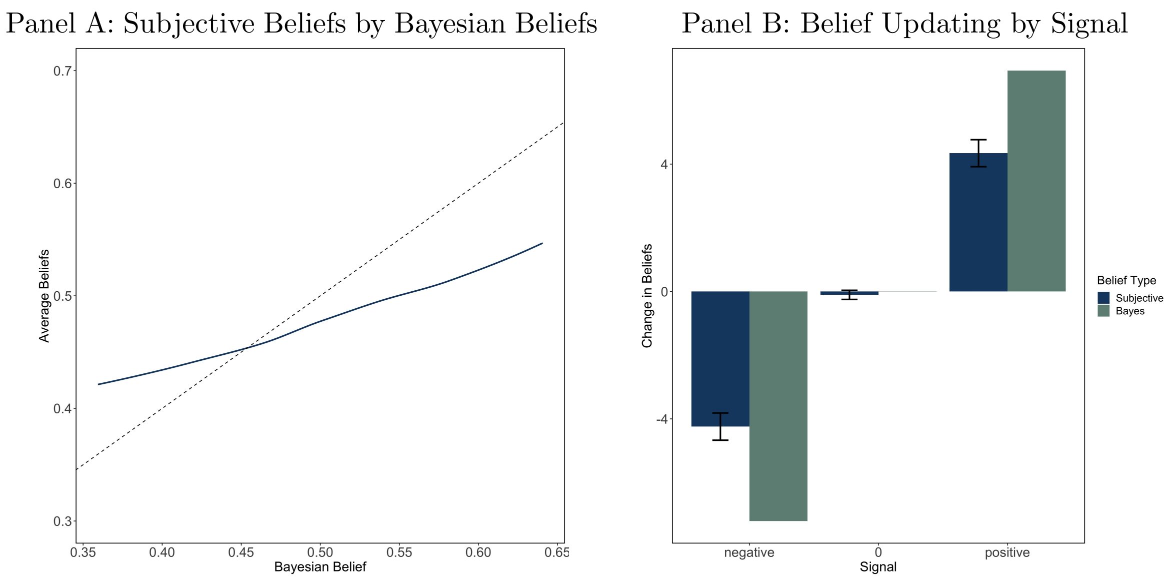


Figure 3.1. Beliefs and Belief Updating in Response to Information. Panel A plots how subjective beliefs respond at different levels of Bayesian beliefs. The blue line shows the average subjective beliefs for all participants and all stocks in rounds 10 to 45 plotted by the Bayesian belief. The dotted black line shows the identity line representing accurate Bayesian beliefs. Panel B shows how subjective beliefs update in response to new information, relative to how a Bayesian would. The blue bars show the average change in subjective beliefs in response to negative, no signal, and positive signals respectively. The light green bars show average change in Bayesian beliefs in response to those signals. The error bars represent 95% Confidence intervals with standard errors clustered at the subject level.

I first examine the relationship between participants' subjective beliefs and Bayesian beliefs. In Figure 3.1, Panel A shows the relationship between Bayesian beliefs and subjective beliefs. The blue line the subjective beliefs for all participants between rounds 10 and 45 for each level of Bayesian beliefs. The dashed black line, the identity line, shows accurate Bayesian beliefs. The red line has a shallower slope than the black line, showing conservatism. This implies that participants' beliefs are excessively optimistic for low levels of Bayesian beliefs and excessively pessimistic for high levels of Bayesian beliefs. Table 3.1 column one examines the pattern in greater detail by including subject fixed effect. As described in equation 2.1, subjective beliefs are regressed on Bayesian beliefs. The coefficient, .47, significantly below 1, indicating that a 10 percentage point increase in the Bayesian beliefs only translates to a 4.7 percentage point increase in subjective beliefs. Panel B shows belief updating in response to the three possible signals, negative, no signal, or positive. The blue bars show the average change in beliefs, while the light green show how much a Bayesian would update on average. Updating is close to zero on average when people receive no signal. Peoples' updating is in the correct direction for both positive and negative signals, but people update less from signals than a Bayesian would. Column two shows in a regression framework that people update about 3 percentage points too optimistically after a negative signal and 2.6 percentage points too pessimistically after a positive signal. Column three shows that updating is roughly symmetric, that is people update close to the same amount in response to positive and negative signals. This finding is consistent with Barron (2020) and Hartzmark, Hirshman, and Imas (2019).

Because the participants can only sell goods they own, I examine whether there is a difference in these relationships by ownership. In Figure 3.2, I reproduce the figures from the previous figure separated by ownership. The patterns are quite similar for owned goods to the overall sample, though people are less conservative for owned than non-owned goods. Similarly, participants update more in response to signals about owned than non-owned goods, but remain conservative relative to Bayesian. Table 3.1 shows the regression results for these measures. These results conceptually replicate the findings on updating in Hartzmark, Hirshman, and Imas (2019), but differ in two important ways. First, while they find no main effect of ownership on beliefs, I find a positive and significant main effect. This may be due to the fact that in my paradigm all stocks are owned to start and so most non-owned stocks are stocks that someone has chosen to sell. Second, while they find more extreme updating for owned goods in response to signals, they find owners are actually further from Bayesian than non-owners. This deviation may be due to the structure of the price evaluations in my task as opposed to theirs. While the likelihood of a stock increasing in their paradigm is fixed, stocks in this paradigm can change state. This finding is in line with Massey and Wu (2005) who suggest that when transition probabilities are higher, people will be more likely to under-react.

Panel A: Subjective Beliefs by Bayesian Beliefs   Panel B: Belief Updating by Signal

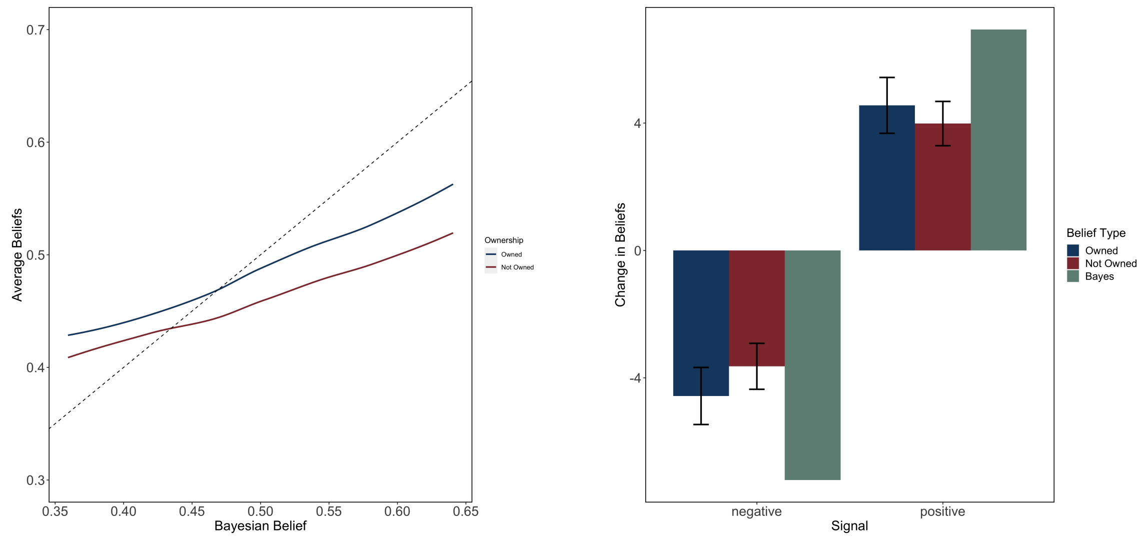


Figure 3.2. Beliefs and Belief Updating in Response to Information by Ownership. Panel A plots how subjective beliefs respond at different levels of Bayesian beliefs for owned and non-owned stocks. The blue line shows the average subjective beliefs for owned goods and the red line shows the average for non-owned goods in rounds 10 to 45 plotted by the Bayesian belief. The dotted black line shows the identity line representing accurate Bayesian beliefs. Panel B shows how subjective beliefs update in response to new information for owned and non-owned stocks, relative to how a Bayesian would. The blue bars show the average change in subjective beliefs in response to negative and positive signals respectively for owned goods, while the red bars do so for non-owned goods. The light green bars show average change in Bayesian beliefs in response to those signals. The error bars represent 95% Confidence intervals with standard errors clustered at the subject level. No signal is not shown for presentation purposes, but close to zero.

Turning to the aggregate measure of the disposition effect, consistent with Frydman and Rangel (2014), I find a positive disposition effect using the standard measure of the disposition effect following Odean (1998). Three participants did not have an opportunity to sell at a loss, and are excluded from the analysis. Participants display a disposition effect of .14 more of their paper gains than paper losses[8]. This significantly above zero ($SE = .042$,$t(108) = 3.27$, $p = .001$) and above the risk-neutral Bayesian benchmark of -.56.

8. This is not particularly surprising given the robustness of the disposition effect in this paradigm. Frydman and Camerer (2016) show a positive disposition effect even when telling participants of the Bayesian estimates. Additionally, my estimate is close to the estimates of the disposition effect without leverage in Heimer and Imas (2020).

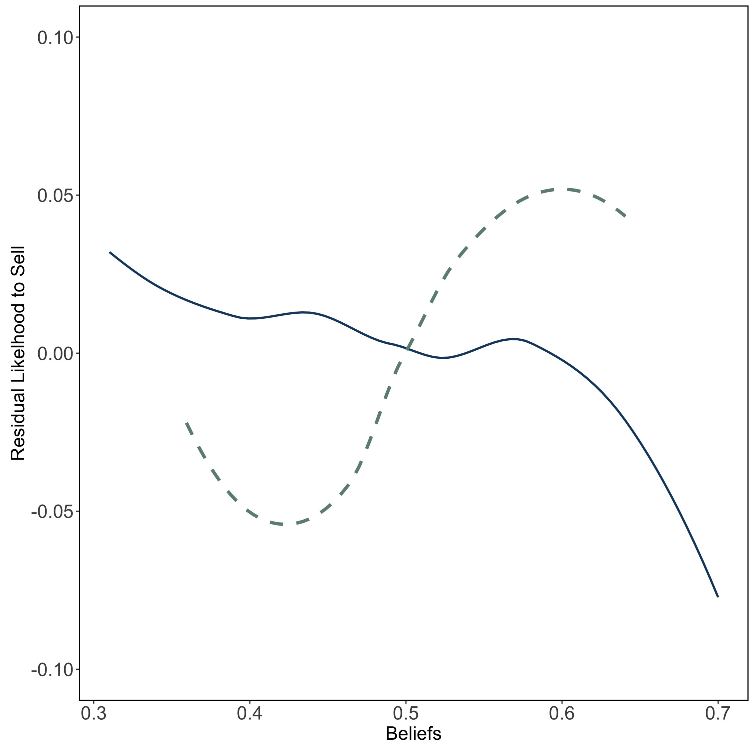


Figure 3.3. The Relationship Between Beliefs and Selling. The plot shows the relationship between selling decisions and beliefs. I plot the residuals of a linear probability model of whether or not a good was sold against subject fixed effects. The blue line shows the relationship for between the residuals and subjective beliefs while the light green line shows the relationship for Bayesian beliefs.

While the Odean (1998) measure is a standard measure for estimating disposition effects, it is not conducive to examining the role of beliefs. Figure 3.3 shows the relationship between both beliefs and the decision to sell, accounting for subject fixed effects. The solid blue line shows the relationship for subjective beliefs, while the dashed light green line shows the relationship for Bayesian beliefs. The relationship for subjective beliefs is downward sloping, meaning that as the subjective likelihood of the stock going up increases the likelihood of selling decreases. By contrast, the relationship for Bayesian beliefs is upward sloping. Table 3.3 column one presents the estimates for equation 2.3 and column three presents the estimates for equation 2.4. A 10 percentage point increase in subjective beliefs leads participants to be 2.6 percentage point less likely to sell the stock. By contrast, a 10 percentage point increase in the Bayesian beliefs lead participants to be 5.1 percentage points more likely to sell the stock. Peoples' selling decisions appear to be related to their reported beliefs, but not the Bayesian beliefs. Columns two and four present the estimates for equations 2.5 and 2.6. In these specifications, I add in a term to capture preferences over gains and losses. Accounting for these preferences, the relationship between Bayesian beliefs and selling lessens implying a 10 percentage point increase in Bayesian beliefs leads to only a 1.5 percentage point increase in the likelihood of selling, though this relationship is no longer significant. The relationship between subjective beliefs and selling gets stronger, with a 10 percentage point increase in subjective beliefs implying a 5.7 percentage point decrease in the likelihood of selling. In addition, the coefficient on capital gains is directionally larger when using subjective beliefs ($\beta_{2'} - \beta_2 = .0013$, $z = 1.64$ two-tailed $p = .102$) suggesting that estimates of the impact of preferences on the disposition effect may be underestimated using Bayesian beliefs.[9] To probe this possibility further, I turn to a simple realization utility model of the disposition effect.

Estimates of a Simplified Realization Utility Model

Under some conditions the disposition effect can be explained by standard Prospect Theory preferences (Kahneman and Tversky, 1979), but realization utility models provide a more general foundation (Barberis and Xiong, 2009). I estimate a simplified realization utility model drawing on the work of Ingersoll and Jin (2013); Barberis and Xiong (2009); Imas (2016), and the estimation described in Magnani (2014). In this model participants get utility over two dimensions, the price of the stocks in levels, and gain-loss utility when they sell stocks, that is realization utility. The parameter $\alpha \in (-\infty, \infty)$ captures the curvature of the utility function over prices. The parameter $\lambda \in [0, \infty]$ captures the relative weight

9. I derive a z statistic as $z = \frac{\beta_{2'} - \beta_2}{\sqrt{se^2_{\beta'_2} + se^2_{\beta_2}}}$

placed on realizing losses to realizing gains, analogous to loss aversion in Prospect Theory. I define the probability to sell the randomly selected stock for subject k in round t as a logistic function

$$P_{k,t} = \frac{e^{u_{hold}}}{e^{u_{hold}} + e^{u_{sell}}}$$

where

$$u_{sell} = \begin{cases} price^{\alpha} + (\frac{price-r}{r}) & \text{if } price \geq r \\ price^{\alpha} - \lambda(\frac{|price-r|}{r}) & \text{if } price < r \end{cases}.$$

and

$$u_{hold} = m_{k,t} * (price + 10)^{\alpha} + (1 - m_{k,t}) * (price - 10)^{\alpha}$$

or

$$u_{hold} = \hat{s}_{k,t} * (price + 10)^{\alpha} + (1 - \hat{s}_{k,t}) * (price - 10)^{\alpha}$$

It is important to note that the beliefs enter into the estimation through their influence on the subjective expected utility of holding the stock. My definition of u_{hold} builds in an assumption of narrow bracketing, as the participant does not solve for the net present value of the stream of outcomes as in Magnani (2014). This assumption is standard within this experimental paradigm (Frydman and Rangel, 2014, see). It also makes an editing assumption by collapsing the next price move gamble to its expected value of 10 experimental dollars. Additionally, agents are not sophisticated about their realization utility, that is they do not anticipate that in a future period if they sell a good they will recieve the additional utility shock. This framework is held constant regardless of whether beliefs are the subjective or Bayesian, so to the extent that these are misspecificaitons of the true decision process they should not bias the comparison of parameters.

I estimate α and λ at the participant level using maximum likelihood by minimizing

$$LL = -\sum_t \ln\left(\mathbb{1}_{hold,t} P_t + \mathbb{1}_{sell,t}(1 - P_t)\right)$$

Figure 3.4 shows the CDFs of the estimated parameters for all subjects. In panel A, I show the CDF of the curvature parameters. The distribution appears to be somewhat bimodal with a large substantial portion of the estimated parameters ranging in the range $[-.5, 0]$ and portion in range of $[.5, 1]$. Using subjective beliefs there is a large portion in in the $[-.5, 0]$, leading to an average of .312 and a median of .501 relative to .482 and .789 for the estimates using Bayesian beliefs. In panel B, I show the CDF of the loss aversion parameter. The CDF of the loss aversion parameters estimated from the subjective beliefs is to the left of the Bayesian CDFs for essentially the entire range. The average parameter estimates are 5.55 and 6.64 for the subjective and Bayesian estimates respectively, while the medians are 4.68 and 5.61. Table 3.4 presents regression results comparing the difference in means for these two distributions clustering standard errors at the subject level. The differences are significant at the 1% level. These analyses suggest that using subjective beliefs increases the curvature (lower α implies more curvature) while also decreasing estimated loss aversion. The increase in curvature makes some intuitive sense when examining the structure of the model. Much of the variation identifying α is coming from the expected utility of holding the stock. As we saw in the reduced form results, peoples' selling decisions are not strongly related to the Bayesian beliefs, and, if anything, go in the wrong direction. If holding decisions are not well-capture by Bayesian beliefs, then greater weight needs to be placed on the utility of the outcomes.

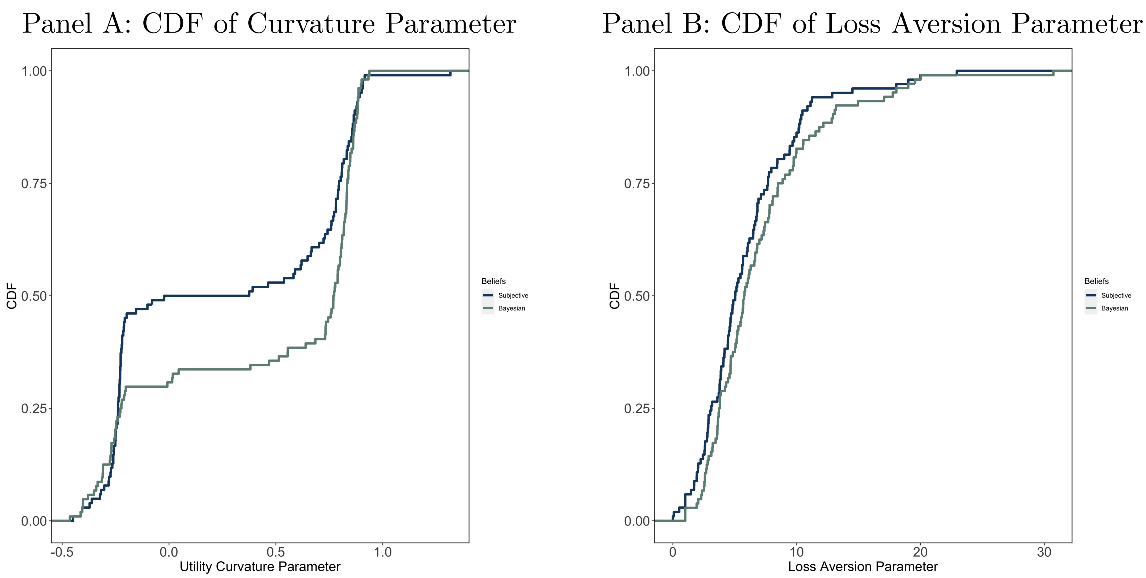


Figure 3.4. Curvature and Loss Aversion Parameters Estimated using both Subjective and Bayesian Beliefs. Panel A plots the CDF of the curvature parameter. The blue line shows the CDF for estimates using subjective beliefs, while the light green line shows the CDF for those using Bayesian beliefs. Panel B plots the CDF of the loss aversion parameter with the blue line showing the estimates using subjective beliefs and the light green line showing those estimated with Bayesian beliefs.

Figure 3.5 shows the utility function over prices and the realization utility function over meaningful values for prices and realization utility in the task. The gap between the functions is larger over much of the range for the utility from prices than for the realization utility. This suggests that even though the loss aversion parameter decreases when estimated with subjective beliefs, the relative impact of realization utility increases.

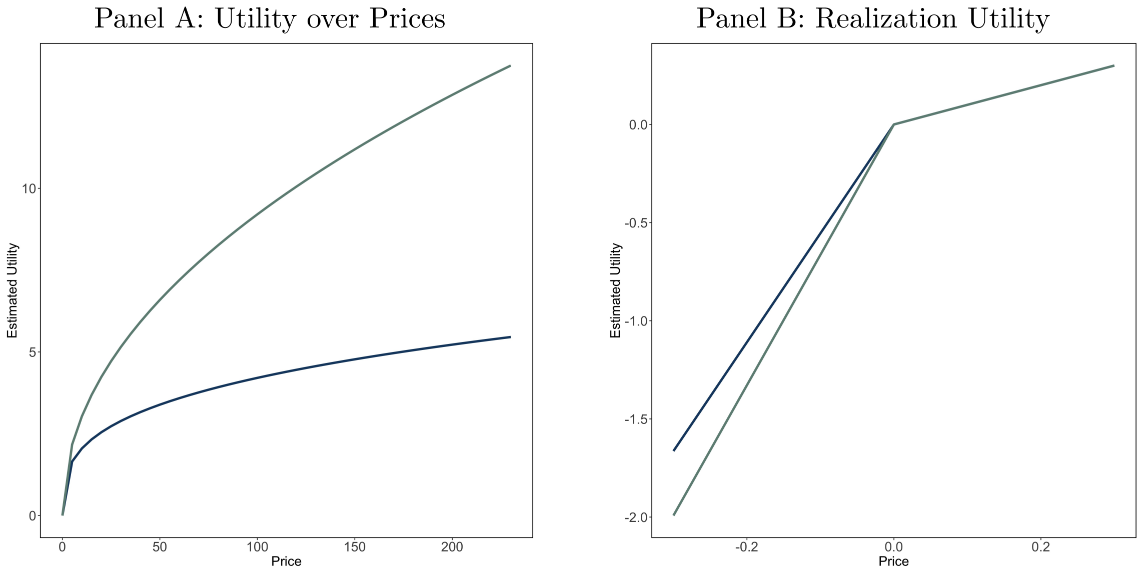


Figure 3.5. Utility Function Over Prices and Realization Utility at Average Parameter Values. Panel A plots the utility over prices at the average parameter values estimated using subjective and Bayesian beliefs. The blue line shows the function for estimates using subjective beliefs, while the light green line shows the function for estimates using Bayesian beliefs. Panel B plots the realization utility function at average parameter values with the blue line showing the estimates using subjective beliefs and the light green line showing those estimated with Bayesian beliefs.

Discussion and Conclusion

I examine how belief biases interact with non-standard preferences to produce the disposition effect. First, I document belief biases relative to Bayesian updating. I find that peoples' beliefs about price increases are conservative relative to Bayesian in my paradigm and that updating is symmetric. I replicate the finding in Hartzmark, Hirshman, and Imas (2019) that people extrapolate information more for owned than non-owned goods, but I find that beliefs are still conservative relative to Bayesin updating. Conservatism in this environment is consistent with the system-neglect hypothesis advanced in Massey and Wu (2005). I then examine trading decisions, which show a positive disposition effect. I find that peoples' selling decisions are negatively correlated with their subjective beliefs, as expected, but positively correlated with Bayesian beliefs. These relationships hold when accounting for capital gains and losses, and the estimate of the impact of capital gains and losses on selling is larger when estimated with subjective beliefs instead of Bayesian beliefs. Finally, I estimate a simplified structural model of realization utility and find that using peoples' subjective beliefs alters utility parameter estimates. I find suggestive evidence that the relative importance of realization utility increases when using subjective beliefs.

These results contribute to both the literature on the disposition effect and belief biases, particularly in the domain of financial decision making. While the disposition effect is extremely robust in both lab and field settings, the original theoretical underpinnings from Prospect Theory have been reexamined in favor of realization utility models (Barberis and Xiong, 2009; Ingersoll and Jin, 2013). Consistent with a long line of literature, I find a substantial positive disposition effect in my experimental paradigm (e.g. Weber and Camerer, 1998; Frydman and Rangel, 2014; Fischbacher, Hoffmann, and Schudy, 2017). However, these papers create paradigms with solvable Bayesian beliefs and assume that participants' beliefs match those Bayesian beliefs. While it is clear from prior work that simply making peoples' beliefs more Bayesian does not eliminate the disposition effect (Frydman and Camerer, 2016), I show both that peoples' beliefs are biased in this setting and that biased learning alters my estimates of preference parameters. While Grosshans, Langnickel, and Zeisberger (2018) also elicit subjective beliefs in a trading paradigm, they find over-extrapolation, not the conservatism I document here. Additionally, they cannot directly examine the disposition effect because there is only one asset in their market. Further work is needed to reconcile these two results.

I also contribute to the literature in belief biases on two dimensions. I contribute to a growing literature on asymmetric vs. symmetric belief updating by finding conservatism in addition to symmetric updating. Bayesian updating suggests that for an equal positive and negative signal, people should update equally. However, particularly in response to self-relevant information, people update more from positive than negative news (e.g., Eil and

Rao, 2011; Mobius et al., 2011; Coutts, 2019). There is also evidence that people update asymmetrically from financial information Kuhnen (2015). Most recently however, in a purely financial domain, Barron (2020) finds no asymmetry in updating and that people do not deviate substantially from Bayesian updating. I also find symmetric updating for both owned and non-owned goods, as do Hartzmark, Hirshman, and Imas (2019).

I also provide suggestive additional evidence for the system neglect hypothesis advanced by Massey and Wu (2005).The system-neglect hypothesis, building on Griffin and Tversky (1992), proposes that more unstable transition probabilities will lead to relatively more underreaction. Comparing my results to a similar paradigm of Hartzmark, Hirshman, and Imas (2019), I find conservatism while they find overextrapolation by owners. Massey and Wu (2005) can reconcile these two results because the transition probabilities in my paradigm are more unstable.[10]

Unless the bias is specifically concerned with learning or perceptions of probability (e.g., probability weighting (Kahneman and Tversky, 1979)), behavioral economics tends to explain systematic deviations from the standard model with preference based explanations. However, people are often in environments where they are both deriving utility from outcomes and learning from those outcomes. As a result, it is not clear how belief biases and non-standard preferences will interact in behavior. Beyond the results presented here, Hartzmark, Hirshman, and Imas (2019) have examined how the endowment effect responds to information. More work should explore the interaction between biased learning and non-standard preferences.

10. The transition probability in Hartzmark, Hirshman, and Imas (2019) are 0, while the transition probabilities in my paradigm are .2.

Tables

Table 3.1

Subjective Beliefs by Bayesian Beliefs .

This table shows the relationship between subjective and Bayesian beliefs in my sample. The variable *Bayesian Beliefs* is the level of Bayesian belief. The variable *negative signal* is a dummy variable equal to one if the price decreased. The variable *positive signal* is a dummy variable equal to one if the price increased. Column one shows the results of a regressing Bayesian beliefs on subjective beliefs. The coefficient on Bayesian beliefs is less than 1 implying conservatism. Column two shows the difference between the change in subjective beliefs and the change in Bayesian beliefs after negative and positive signals. Column three shows the raw change in subjective beliefs after negative and positive signals. Standard errors are clustered by subject and shown in parentheses.*, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Dependent variable:		
	Subjective Beliefs	Change in Beliefs-Bayesian Change	Change in Beliefs
	(1)	(2)	(3)
Bayesian Belief	0.467***		
	(0.042)		
Negative Signal		0.030***	−0.042***
		(0.004)	(0.004)
Positive Signal		−0.026***	0.045***
		(0.004)	(0.004)
Observations	12,096	12,096	12,096
Subject Fixed Effects	Y	Y	Y
R^2	0.366	0.042	0.094
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 3.2

Subjective Beliefs by Bayesian Beliefs and Ownership.

This table shows the relationship between subjective and Bayesian beliefs in my sample accounting for ownership. The variable *Bayesian Beliefs* is the level of Bayesian belief. The variable *Negative Signal* is a dummy variable equal to one if the price decreased. The variable *Positive Signal* is a dummy variable equal to one if the price increased. The variable *Own* is a dummy variable equal to one if the good was owned by the participant. Column one shows the results of a regressing Bayesian beliefs on subjective beliefs. Column two shows the difference between the change in subjective beliefs and the change in Bayesian beliefs after negative and positive signals. Column three shows the raw change in subjective beliefs after negative and positive signals. Standard errors are clustered by subject and shown in parentheses.*, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Dependent variable:		
	Subjective Beliefs	Change in Beliefs-Bayesian Change	Change in Beliefs
	(1)	(2)	(3)
Bayesian Beliefs	0.415***		
	(0.047)		
Own	0.026***	0.002	0.002
	(0.006)	(0.001)	(0.001)
Own *Bayesian Beliefs	0.086**		
	(0.040)		
Negative Signal		0.038***	−0.035***
		(0.005)	(0.005)
Positive Signal		−0.027***	0.042***
		(0.006)	(0.005)
Own* Negative Signal		−0.012**	−0.011**
		(0.005)	(0.005)
Own*Positive Signal		0.002	0.004
		(0.005)	(0.005)
Observations	12,096	12,096	12,096
Subject Fixed Effect	Y	Y	Y
R^2	0.378	0.043	0.095
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 3.3

The Decision to Sell Assets .

This table shows the relationship between selling decisions and beliefs.The variable *Subjective Beliefs* is the level of Subjective belief reported by subjects. The variable *Bayesian Beliefs* is the level of Bayesian belief. The variable *Capital Gains* is the difference between the last purchase price and the current price of the stock. Column one shows the impact subjective beliefs on selling decisions. Column two shows the impact of subjective beliefs on selling when accounting for the capital gains earned. Column three shows the impact Bayesian beliefs on selling decisions. Column four shows the impact of Bayesian beliefs on selling when accounting for the capital gains earned. Standard errors are clustered by subject and shown in parentheses.*, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Dependent variable:			
	Sold Asset			
	(1)	(2)	(3)	(4)
Subjective Beliefs	-0.256^{*}	-0.566^{***}		
	(0.131)	(0.130)		
Bayesian Beliefs			0.507^{***}	0.150
			(0.187)	(0.196)
Capital Gains		0.004^{***}		0.002^{***}
		(0.001)		(0.001)
Observations	1,609	1,609	1,609	1,609
Subject Fixed Effect	Y	Y	Y	Y
R^2	0.246	0.270	0.251	0.259
Note:		*p<0.1; **p<0.05; ***p<0.01		

Table 3.4

Comparing Estimated Parameter Values

This table shows the difference between the structural parameters estimated using subjective and Bayesian beliefs. The variable *Subjective Beliefs* is the intercept term of the regression and represents the average parameter value in the estimation using subjective beliefs. The variable *Used Bayesian Beliefs* is a dummy variable equal to 1 if the the estimate used Bayesian beliefs. Column one shows the estimates for the curvature parameter for the utility over price levels. Column two shows the estimates for the realization utility loss aversion parameter. Row two presents the difference between the two estimates. Standard errors are clustered by subject and shown in parentheses.*, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Dependent variable:	
	α	λ
	(1)	(2)
Subjective Beliefs	0.312***	5.546***
	(0.051)	(0.396)
Used Bayesian Beliefs	0.170***	1.095***
	(0.063)	(0.299)
Observations	112	112
R^2	0.026	0.014
Note:	*p<0.1; **p<0.05; ***p<0.01	

CHAPTER 4

MINIMUM PAYMENTS ALTER DEBT REPAYMENT STRATEGIES ACROSS MULTIPLE CARDS

Imagine that you have accumulated debt across several different credit cards, and you are now allocating your monthly budget towards repaying this debt. As you look at your bills, each one has a different total balance, interest rate, and minimum payment. How do you decide how much to allocate to each debt? You might focus on balance amounts, for example, paying the smallest balance first to feel like you are making progress. Alternatively, you might focus on interest rates, paying the highest interest card first to minimize total interest paid. Regardless of how you choose to prioritize your debts, you will likely consider the minimum payment amounts across all cards. You plan to pay at least the minimum to avoid fees, interest rate increases, and credit score implications associated with failing to make the minimum payments. You may even feel like you should pay more than the minimum.

The situation above is a pressing reality for many households. US Households currently hold $808 billion of credit card debt (of New York, 2017). Rather than being consolidated into a single monthly payment on one card, the average American household with at least one credit card must manage decisions across an average of four accounts (Consumer Financial Protection Bureau, 2015). To make cost minimizing debt repayments, sole reliance on interest rates and minimum payments as numeric cues can lead consumers to a cost minimizing strategy. However, it is not clear that consumers use the cost minimizing strategy, instead opting for heuristics based on amounts (e.g., Amar et al., 2011; Gathergood et al., 2019).

We propose that minimum payments fundamentally alter allocation strategies when dividing ones' budget across several different credit card debt accounts. In field data, consumers' repayments are more dispersed than they should be to minimize interest costs (Gathergood et al. (2019); Kettle et al. (2016); Web Appendix A). We use controlled experiments to isolate the effect of minimum payments on overdispersion. Specifically, when participants allocate debt payments in the presence of minimum payments, they tend to allocate in a more dispersed way, which we term the dispersion effect of minimum payments. This leads participants to pay more in interest than a no minimum payment control because participants tend to focus less on interest cues when minimum payments are required. We provide evidence that the dispersion effect occurs in addition to anchoring effects previously associated with minimum payments on single cards (e.g., Stewart, 2009), magnifying the negative consequences of minimum payments for credit card allocation decisions. Finally, we examine the role that alternative information displays Soll, Keeney, and Larrick (2013); Salisbury and Zhao (2020) can play in reducing or increasing the cost consequences of consumers' allocation strategies in the presence of minimum payments.

Minimum payments are a common choice architecture in debt repayment. Our findings suggest that minimum payments tend to lead consumers to pay more in interest when managing multiple accounts. Understanding the psychology underlying these decisions allows firms and policy makers to effectively design choice environments that aid consumers.

Impacts of Minimum Payments and Debt Statements on Debt Repayment

Minimum payment amounts are a central element of the credit card statement and, more broadly, of the financial system surrounding debt repayment. Making at least these payments lowers consumers' risk of defaulting and incurring corresponding late fees. Previous work studying the effects of minimum payments has focused on single debt settings. Participants in these studies are typically randomly assigned to a debt that either has a minimum payment or not, and they are asked how much they would allocate to repaying on that card (e.g., Stewart, 2009). Consumers faced with minimums in this context tend to repay less than those without them (e.g., Stewart, 2009; Navarro-Martinez et al., 2011; Hershfield and Roese, 2015). One explanation for this effect is that minimum payments lead consumers to anchor on the minimums and, as a result, pay just above the minimum (Stewart, 2009; Navarro-

Martinez et al., 2011). An alternative explanation of these findings is that the minimum payments operate as a recommendation or appropriate payment amount (Hershfield and Roese, 2015). Subsequent work has shown similar effects in the field, with consumers paying at or just above the minimum when the minimum required payments change (e.g. Keys and Wang, 2019).

Beyond the minimum payment, the CARD act of 2009 required credit card companies to provide additional information to consumers including the total interest costs associated with paying only the minimum payment, the time required to pay off the full credit card balance if paying only the minimum, and the repayment amount required to pay off the full credit card balance in three years. Research examining repayment decisions has aimed to understand the impact of providing this new information for consumer behavior in the context of a single card. For example, Soll, Keeney, and Larrick (2013) find that consumers misunderstand the growth of debts over time due to misunderstanding compound interest. The new information mandated by the card act corrects some but not all of consumers' misunderstanding of the time it takes to repay debt. Focusing on the provision of three year repayment costs and three year repayment time, Salisbury (2014) finds that the provision of three year cost information has the desired effect of increasing repayments above minimum, but the information about how long it would take to repay a debt paying only the minimum has an unintended consequence of moving repayments below the three year repayment amount.

The goal of the CARD act disclosures was to nudge consumers to make larger repayments, and these small changes to the information environment did affect repayment decisions (Agarwal et al., 2015). However, the prevalence of online repayments has diminished the likelihood that consumers even see CARD act disclosures. As a result, Salisbury and Zhao (2020) investigate the way online payment modules with default options are constructed and the consequences of these displays for repayment decisions. Using the active choice displays, which consist of default options of paying the minimum and the total debt amount, increases the likelihood that consumers repay a single card in full. Overall, the literature on information disclosure in single card settings points to the susceptibility of consumers' debt repayment decisions to the choice architecture of their environments both from minimum payments and alternative repayment suggestions.

Each of these demonstrations examines the impact of minimum payments on the decision of how much money to repay towards a single card (e.g., Stewart, 2009; Keys and Wang, 2019; Hershfield and Roese, 2015; Navarro-Martinez et al., 2011). However, when determining the most efficient allocations across *multiple* accounts, the cost minimizing strategy changes. Specifically in the debt context, paying only the minimums on lower interest-rate cards can be consistent with the optimal strategy. As a result, these studies cannot determine whether consumers are in fact committing an error, or if they are instead responding to minimum payments as they should when considering repayment strategies across a consumers' portfolio of cards.

Debt Repayment Allocation Decisions Across Multiple Accounts

Most consumers face multiple credit card debts and, unlike the settings above, must decide both which debts to prioritize and how much money to allocate to each debt. A debt's interest rate is the key piece of information for determining how to repay debt as cheaply as possible. The intuition is that the interest rate represents the price of the debt, with higher interest rates implying higher prices. As a result, once minimum payments have been paid, a dollar goes the farthest on the margin by paying down the debt that has the highest price. Assuming a given amount of money available to repay debt across cards, the strategy that leads to the lowest amount of accrued interest over time is to pay the minimum required payment on each card first and then to use the discretionary funds (i.e., repayments in excess of the minimum payments on all cards) to pay off the debt associated with the highest interest rate card. If, after doing so, funds remain, the consumer should repay the debt associated with the second highest interest rate, and so on.

A variety of studies both in the lab (e.g., Amar et al., 2011; Besharat, Carrillat, and Ladik, 2014) and the field (Gathergood et al., 2019) have shown that the heuristics consumers use to make debt repayment decisions differ from the cost minimizing strategy and have attempted to explain the underlying psychological processes. Using field data from the United Kingdom, Gathergood et al. (2019) argue that consumers are balance matching, that is they behave "as-if" they pay in proportion to their account balances. For example, a consumer who held a $1,000 balance on one card and a $500 balance on another would allocate $200 to the first debt and $100 to the second if they had $300 available to repay. The authors argue that this heuristic is consistent with other matching heuristics shown in humans and animals (e.g., probability matching; Herrnstein and Prelec (1991)). Notably, this heuristic predicts that people should make their largest repayment to their largest debt amount. They find little evidence for alternative heuristics in a model comparison, such as those emphasizing paying off the smallest debt.

However, both financial advice gurus like Dave Ramsey and academic researchers have documented positive motivational effects of paying off debts, particularly the smallest debt. Under the strategy, termed "debt account aversion", consumers repay more money towards their debts with the smallest balances to close accounts and feel a sense of progress (Amar et al., 2011; Besharat, Carrillat, and Ladik, 2014) . In the initial demonstration of this effect, participants were more likely to repay their smaller debts earlier than an optimal strategy would dictate (Amar et al., 2011). Drawing on theories of motivation, the authors argue that people feel better after completing a subgoal by repaying an account in full and as a result want to close accounts as quickly as possible.

Building on this idea, additional work has shown that closing accounts can increase motivation over time. Consumers in a debt repayment program are more likely to persist if they have an account closed, even though the debt repayments are decided by the program

(Gal and McShane, 2012) and sorting tasks from smallest to largest can reduce the time it takes to complete the task (Brown and Lahey, 2015). Additionally, the motivational benefits of focusing on smaller debts can manifest without paying debts off in full if consumers make a repayment with the largest proportional balance reduction (Kettle et al., 2016).

Current research

When making payments across multiple cards, we predict that repayment decisions in contexts with minimum payment requirements will be dispersed relative to a no minimum payment control condition (i.e., the dispersion effect of minimum payments). We define dispersion following Kettle et al. (2016) measure of concentration (see study 1 for formal mathematical definition This prediction is motivated by the literature on naïve diversification and the 1/n heuristic (Benartzi and Thaler, 2001). The authors show a tendency to split their repayments evenly across available investment accounts. Additional work has shown a more general tendency to split allocations evenly across accounts with any investment (Morrin et al., 2012). We expect that the required minimum payments will increase the number of accounts participants choose to allocate to, either to avoid the fee or due to the perceived recommendation (Hershfield and Roese, 2015). This in turn will increase the dispersion of their allocations relative to a no minimum payment control as a result of naïve diversification. Additionally, we expect that making it easy to repay in a more concentrated fashion via choice architecture will attenuate the differences in concentration between the minimum payment and no-minimum payment control condition Salisbury and Zhao (2020).

Concentration does not inherently lead consumers to pay lower interest costs, though the cost minimizing strategy described above is typically concentrated. As a result, what consumers choose to concentrate on will affect the amount of interest they pay. Unlike the investment context where naïve diversification is often studied (Benartzi and Thaler, 2001; Morrin et al., 2012), the debt repayment context has more salient allocation cues, namely interest rates and amounts. Although consumers do not have a strong understanding of how interest compounds (e.g., Soll, Keeney, and Larrick, 2013), there is evidence that consumers overweight teaser interest rates in contract choice and thus must be attending to interest to some degree (Agarwal, Chomsisengphet, and Lim, 2017). As a result, we propose that participants will attend to the interest rates of their debts, measured by the likelihood of paying off their highest interest-rate debt in full or making the largest allocation to their highest interest-rate debt. However, this will be attenuated by the diversification induced by the minimum payments. If this is the case, decreasing concentration will increase the amount of interest paid in the minimum payment condition. This need not be the case if participants use alternative cues, such as the amount, to repay their debts (e.g., Amar et al., 2011; Gathergood et al., 2019). Additionally, the information environment consumers face can make interest information less salient, leading to increased costs.

In study one, we find that participants' allocations in the minimum payment condition are more dispersed than a no minimum payment control using nationally representative samples and incentive compatible design. We also find that the presence of minimum payments leads participants to focus their repayments less on interest rates, and, as a result, the increased dispersion leads to larger interest costs. In study two, we replicate these findings in a context where consumers can choose their budgets for debt repayment and, consistent with prior literature (e.g., Stewart, 2009), show that minimum payments decrease the budget allocated to debt repayment. We also provide suggestive evidence that concentration influences motivation to engage in debt repayment. Finally, in study three, we examine how different information environments and allocation interfaces, modeled on real world debt repayment environments, affect both concentration and interest costs. Four studies reported in the appendix corroborate these key findings and provide additional evidence of robustness to variations in the experimental design. The exact materials and data for all studies are available on OSF, at https://bit.ly/2Tt17Zw.

We make several novel theoretical and practical contributions. First, we provide evidence of the dispersion effect of minimum payments. Second, we find evidence consistent with naïve diversification in debt repayment, adding to the literature on the use of 1/n heuristics (Benartzi and Thaler, 2001; Morrin et al., 2012). Third, we demonstrate that minimum payments tend to lead consumers to pay more interest costs because of both changes in strategy and because of decreased focus on debt repayment. Finally, we provide recommendations for how the government or firms can alter the information and repayment environment to reduce the costs of consumer repayment strategies. The results have implications for firms, marketers and policy-makers interested in consumer welfare, in identifying how consumers allocate scarce resources, and in how choice architecture can alter these allocation strategies and be used to attenuate negative effects for consumers.

Study 1

A range of structural factors could potentially impede consumers' ability to implement the cost minimizing strategy in allocation decisions and lead to overdispersion. Prior research on targeting minimum payment amounts makes these a logical starting point for examination. While research has shown that minimum payments can lower the amount that consumers pay towards any given card, little attention has been given to understanding the influence of minimum payments across multiple accounts. Yet, when making debt repayment decisions, most consumers are in fact repaying multiple cards at once.

We hypothesize that minimum payments will increase the use of naïve diversification because consumers must make decisions for all accounts, leading to increased dispersion in the minimum payment condition. While it is difficult to find variation in the presence or absence of minimum payments in the field because they are almost universally required, examining repayments in the lab allows us to assess the causal impacts of minimum payments. In a nationally representative population, we use a debt management game to test participants' strategic responses to minimum payments. Data collection was preregistered at aspredicted.org.

Method

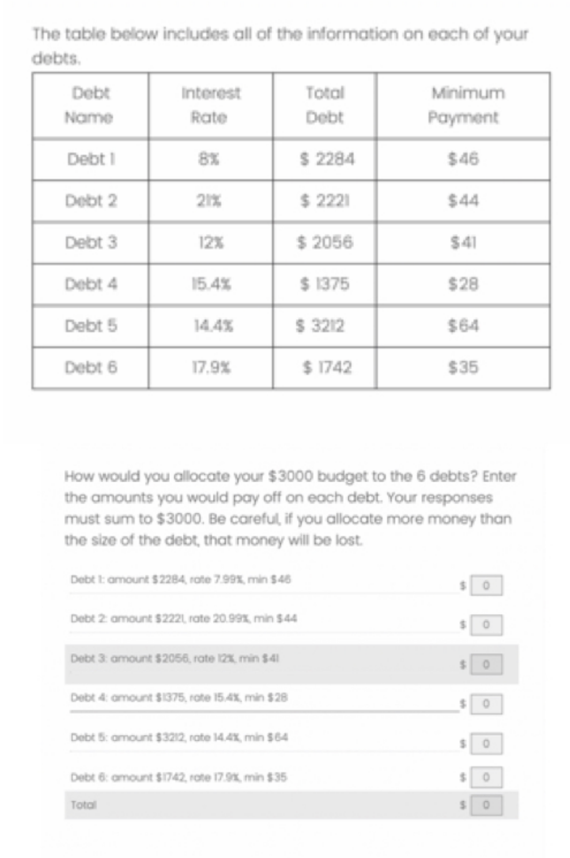
Participants. Four hundred and thirteen participants from a market research panel aggregator operated by CloudResearch completed the study. 50.7% were female with a median age of 48 with a modal education level of a "Some college (no degree)" and median income of $30,000-$39,999. The sample was selected to be approximately nationally representative of US adults on age, gender, and income. Seventy-nine percent reported having at least one credit card with a median of 2 cards per participant. The sample was limited to US participants.

Design and procedure. Participants played a three round debt management game modeled on Amar et al.'s (2011) task, but modified to mimic the average US consumer's debt more closely. Participants were provided with information on six debt accounts including interest rates and amounts. We drew interest rates at random from a CFPB database of national credit card terms and drew debt amounts from a normal distribution designed to add up to $16,000, approximately the amount of credit card debt for the average indebted American household (Nerdwallet, 2017). In each round, participants received a budget of $3,000 dedicated to debt repayment, and they selected the amount they wanted to allocate to each debt. Their allocations in each round were forced to equal their budget (see figure 1 for participants' view of the task).

Participants were randomly assigned to either a minimum payment or a no minimum payment control condition. All participants were instructed that their goal in the task was to reduce their debt as much as possible. After reading the instructions all participants answered one comprehension check question about their goal in the task. In addition, participants in the minimum payment condition answered a question about the size of the minimum payment fee. If a participant answered a comprehension check question incorrectly the first time, the question was shown again with the relevant section of the instructions. All participants saw a table indicating the balance and interest rates for each of six credit card accounts. Participants in the minimum payment condition saw an additional column in the table with the minimum payment amounts. The minimum payments were set at 2% of the total debt amount, consistent with the typical range of 1-4% of balance (Keys and Wang, 2018). Participants faced a $25 fee for each minimum that they failed to pay, similar to the initial "safe harbor" late fee set by the (Bureau, 2015). After each round, the task was updated to reflect the decisions the participants had made in the previous round. Participants were told that they would be paid a bonus that ranged from $0-1$ based on their performance in the task .

Results and Discussion

We excluded participants ($N = 13$) who failed to answer the comprehension check questions twice. We also excluded participants ($N = 26$) who allocated more than they owed to any debt in more than one round. Analyses including all participants are included in the appendix and results do not substantially change (see appendix W2 for robustness to exclusion criteria).



The table below includes all of the information on each of your debts.

Debt Name	Interest Rate	Total Debt	Minimum Payment
Debt 1	8%	$ 2284	$46
Debt 2	21%	$ 2221	$44
Debt 3	12%	$ 2056	$41
Debt 4	15.4%	$ 1375	$28
Debt 5	14.4%	$ 3212	$64
Debt 6	17.9%	$ 1742	$35

How would you allocate your $3000 budget to the 6 debts? Enter the amounts you would pay off on each debt. Your responses must sum to $3000. Be careful, if you allocate more money than the size of the debt, that money will be lost.

Debt 1: amount $2284, rate 7.99%, min $46	$ 0
Debt 2: amount $2221, rate 20.99%, min $44	$ 0
Debt 3: amount $2056, rate 12%, min $41	$ 0
Debt 4: amount $1375, rate 15.4%, min $28	$ 0
Debt 5: amount $3212, rate 14.4%, min $64	$ 0
Debt 6: amount $1742, rate 17.9%, min $35	$ 0
Total	$ 0

Figure 4.1. The Information Table and Entry Screen Shown to Participants in the Minimum Payment Condition. This figure shows the table of debt account information table and the participant entry screen for the minimum payment condition.

As a measure of the diversification heuristic, we use the concentration described in Kettle et al. (2016). The measure is defined as follows

$$concentration_{it} = \frac{\left(\sum_{k=1}^{A_{it}} \frac{(x_{ikt} - \bar{x}_{it})^2}{A_{it} - 1} \right)}{\bar{x}_{it} \times \sum_{k=1}^{A_{it}} x_{ikt}}$$

Where x_{ikt} is the allocation made by participant i to debt k in round t, $\bar{x}_{it}$ is the mean allocation made by consumer i in round t, and A_{it} is the number of accounts with nonzero balances for consumer i in round t. Intuitively, it captures the ratio of the variance in repayments to the mean of the repayments, with a normalization to create a measure bounded between zero and one. This measure of concentration has a number of advantages. It builds on information theoretic concepts, is continuous over the range of zero to one, and allows meaningful comparisons between participants with different numbers of accounts. Because the measure is bounded between zero and one, we analyze concentration using a fractional regression in a panel over the three three rounds, controlling for the round, with heteroskedasticity robust standard errors clustered at the subject level (Clark, 2019). To get a sense for the strategies participants were using, we examine the likelihood that the participant either paid off their highest interest rate debt in full or made their largest allocation to their highest interest rate debt using a logistic regression with round fixed effects and heteroskedasticity robust standard errors clustered at the subject level. To capture the costs associated with the repayment decisions in each condition, we examine the log of the interest paid per round by condition using a linear regression, controlling for round, with heteroskedasticity robust standard errors clustered at the subject level. We also conduct a mediation analysis to assess the amount of variance in interest costs between conditions explained by differences in concentration using the mediation package in R (Tingley et al., 2014) .

Participants in the minimum payment condition allocated their repayments in a less concentrated way than those in the control condition (including round controls $M_{nominimum} = .26$ vs $M_{min} = .2$, $\beta = -.33$, 95% CI= $[-.59, -.06]$, $z = 2.43$, $p = .015$), consistent with use of a diversification heuristic (see figure 2b). We also observe significantly lower likelihood of focusing on the highest interest rate debt in the minimum payment condition (including round controls $M_{nominimum} = 65\%$ vs $M_{min} = 49\%$, $\beta = -.65$, 95% CI= $[-.98, .32]$, $z = 3.86$, $p < .001$). These repayment patterns translated to 4% more interest paid per round in the minimum payment condition ($M_{min} = 7.24$) than in the control condition

($M_{nominimum}$ = 7.20, β = .044, 95% CI= [0.024, 0.064], $t(372)$ = 4.36, p < .001). A mediation analysis suggests that 31% (95% CI= [−.005, .56], p = .051) of the difference in interest paid between conditions was driven by the decreased concentration in the minimum payment condition.

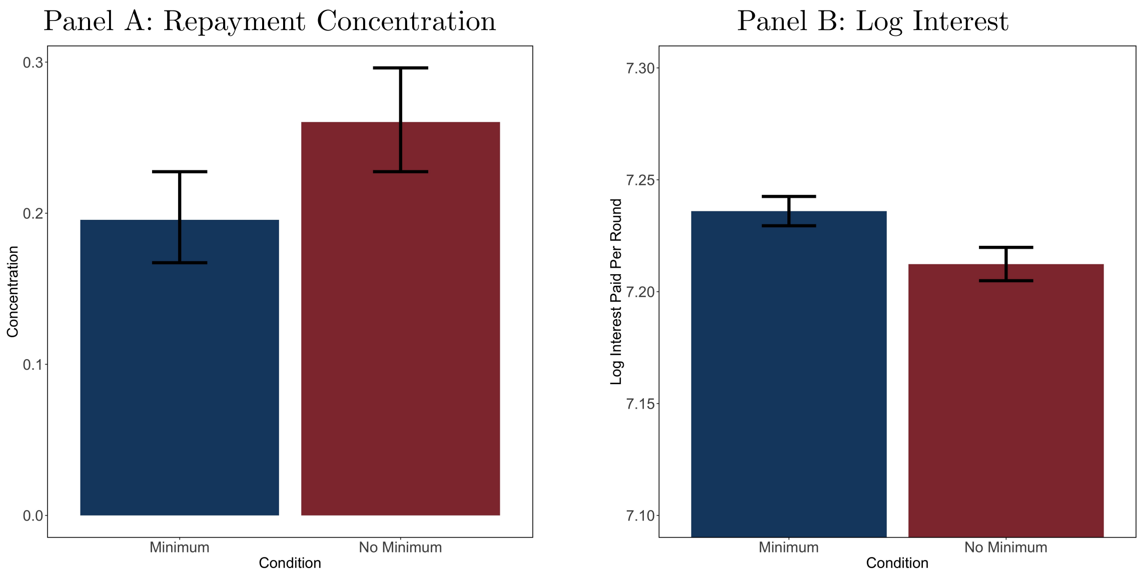


Figure 4.2. Changes in Repayment Concentration and Interest Paid by Condition in Study One. Panel A plots the average concentration by condition, controlling for round fixed effects. Panel B plots the average log interest per round, controlling for round fixed effects. The error bars represent 95% confidence intervals of the mean with standard errors clustered at the subject level.

Minimum payments shifted participants' debt repayment strategies when allocating a fixed sum across multiple accounts. In the minimum payment condition, participants' allocations were significantly less concentrated than in the no-minimum payment control condition. This effect is consistent with an increase in naïve diversification as a result of a more complex decision environment. The decrease in concentration is associated a significant increase in the amount of interest paid in each round. These results are robust to accounting for financial literacy and other demographic controls, and when examining only participants with multiple credit cards.

Study 2

In the previous study, participants were given a fixed budget to allocate across six accounts. The prior literature on minimum payments focuses on the tendency of consumers to pay at or just above the minimum payment in a single card setting (e.g., Stewart, 2009). Our design does not allow for people to pay only the minimums for all debts as they must allocate their full budget to debt repayment in each round. In study two, we examine how the dispersion effect of minimum payments relates to prior literature on minimum payments examining a single card by allowing participants to save some of their budget. This is more consistent with the decision people face in the real world to allocate their unspent money across debt repayment and saving (e.g., Gathergood and Olafsson, 2020).

Method

Participants. Four hundred and two participants completed the study on MTurk. Fifty-four percent of our participants were female with a median age of 33 and median income in the range of $50,000-59,000, and modal education of a "Bachelor's degree". Eighty-four percent of our participants report having at least one credit card with an average of 2 cards per participant. *Design and procedure.* Participants in this version of the debt game had the option to allocate money to a savings account if they desired. They were not given information about interest earned by this account. We instructed participants to answer as they would in their everyday life to gain a realistic picture of consumer behavior and avoid having participants treat this as an optimization problem, which could bias participants against using the savings account. Participants were randomly assigned to either a minimum payment or no minimum payment control condition. In addition to the measures from prior studies, we also examined the amount and likelihood of savings across conditions.

Results and Discussion

Prior to examining the data, we excluded participants (N=9) who allocated more than they owed to any debt in more than one round. Consistent with study one, participants in the minimum payment condition (Mmin=.27) allocated their chosen budgets to debt repayment in a significantly less concentrated fashion than the no minimum payment control (Mcontrol=.34, including round controls ($\beta = -.34$, 95% CI=$[-.57, -.11]$, z=2.87, $p = .004$). Participants were less likely to focusing repayments on the highest interest rate debt in the minimum payment condition (M_{min} = 64%, $M_{nominimum}$ = 75%, $\beta = -0.56$, 95% CI=$[-.89, -.22]$, $z = 3.26$, $p = .001$). Participants in the minimum payment condition also paid 7.6% more interest than those in the control ($M_{min} = 7.27$, $M_{nominimum} = 7.19$, $\beta = .076$, 95% CI= $[.042, .11]$, $t(392) = 4.38$, p¡.001). A mediation analysis suggests that differences in concentration explained 28 % of the variance in interest paid (95% CI=$[9.4\%, 46\%]$, $p = .008$).

Prior work (Stewart, 2009) suggests that people should allocate less money toward debt repayment when the minimum payments are present. We replicate that finding in a multiple card setting by examining the log of savings in each round. Participants in the minimum payment condition saved 152% more than those in the no minimum payment control ($M_minimum = 3.73$, $M_{nominimum} = 2.81$, $\beta = .92$, 95% CI= $[.39, 1.46]$, $t(392) = 3.42$, $p < .001$). Because their overall budget was fixed, this implies participants in the minimum payment condition allocated less money to debt repayment. Participants in the minimum payment condition were also significantly more likely to save any money (M_{min} = 63%, $M_{nominimum}$ = 49%, $\beta = .60$, 95% CI= $[.24, .96]$, $z = 3.26$, $p = .001$).

To link these two sets of results, we regressed the log of savings in round t on condition, concentration in round t-1, and log of savings in round t-1 with a round control. This analysis allows us to examine a prediction, inspired by Kettle et al. (2016), that less concentrated repayment strategies will lead to less focus on debt repayment (more focus on savings).

Unsurprisingly, prior round savings, added to account for propensity to save, predicts current round savings ($\beta = .73$, 95% CI= $[.675, .785]$, $t(391) = 26$, $p < .001$). In addition, prior round concentration is negatively correlated with future savings ($\beta = -1.54$, 95%CI= $[-2.21, -.865]$, $t(391) = 4.91$, $p < .001$), suggesting that participants with more concentrated strategies are more focused on repaying debt in subsequent rounds. Controlling for those two effects, there is no effect of condition on savings ($\beta = .02$, $t(391) = .20$, $p = .839$). The negative relationship between concentration and savings is consistent with the negative motivational effects of dispersed debt repayments Kettle et al. (2016).

Prior literature on minimum payments in single card settings finds that the presence of minimum payments reduces the amount repaid toward debt. We replicate that finding, but also show that consumers allocate their remaining budget in a less concentrated way. This suggests that minimum payments induce two separate costs across multiple accounts, one decreases focus on debt repayment, and the other leads people to spread their repayments accounts, similar to naïve diversification. There may also be negative motivational consequences to more dispersed repayments.

Study 3

We have documented a new cost to minimum payments, namely that they lead participants to make more dispersed repayments. However, minimum payments serve important functions for the financial system, particularly allowing credit card companies to detect defaults, reducing prices in the market overall. Additionally, though prior research and study two show that minimum payments can reduce the budget allocated to debt repayment, there may costs to consumers associated with eliminating the minimum payments or switching to autopay (Sakaguchi, Stewart, and Gathergood, 2018). As a result, the place that policy markers, firms, and consumer welfare advocates have the most ability to improve consumer decision making or mitigate the costs of errors is through the design of repayment interfaces.

In this study, we return to the setting for study one, but introduce three new conditions with minimum payments: a high interest salience condition, an active choice condition building on the findings of Salisbury and Zhao (2020), and a standard statement condition, which attempted to approximate the consumer experience of searching for relevant information in credit card debt statements. To make interest salient, we sorted the table of debt information by interest rate, expecting that this would lead to an increase in focus on interest as a cue without affecting concentration leading to a decrease in interest costs. The active choice condition makes it easier to choose more concentrated allocations by making the minimum and the full payment default choice options, so we expected it would increase concentration and decrease interest paid. Finally, in our standard statement condition, participants needed to search for information about the interest rate, so we expected this would reduce interest focus leading to higher interest charges.

Method

Participants. One thousand and three participants completed the study on MTurk. Forty-six percent of our participants were female with a median age of 37, median income in the range of $40,000-49,000, and modal education of a "Bachelor's degree". Eighty-seven percent of our participants report having at least one credit card with a median of 2 cards per participant. *Design and procedure.* Participants were randomly assigned to one of five conditions. The first was the no minimum payment control condition. The other four conditions had minimum payments. The second condition was the same as the minimum payment condition from study 1. The third condition had the debts were sorted from highest to lowest interest rate. This was designed to increases the salience of the interest cue, and facilitate more effective repayments even in the presence of the minimum payment.

The fourth condition, an active choice condition, had the same information table as the minimum payment condition but the input interface was based on the online interface that many consumers now use to repay debts. Modeled on Salisbury and Zhao (2020), instead of using a six item numeric input table, participants were shown radio buttons for each debt that represented the minimum payment, the full balance, or other with a text box (see Figure 4.3). This interface made it easier for participants to pay exactly the minimum or the full balance and reducing the number of active allocation decisions. As a result, we expected this condition would be more concentrated than the minimum payment condition. In addition, they were shown the total amount they had allocated. They could not advance if they allocated more than $3000. If they allocated less, they were prompted to try again up to three times per round.

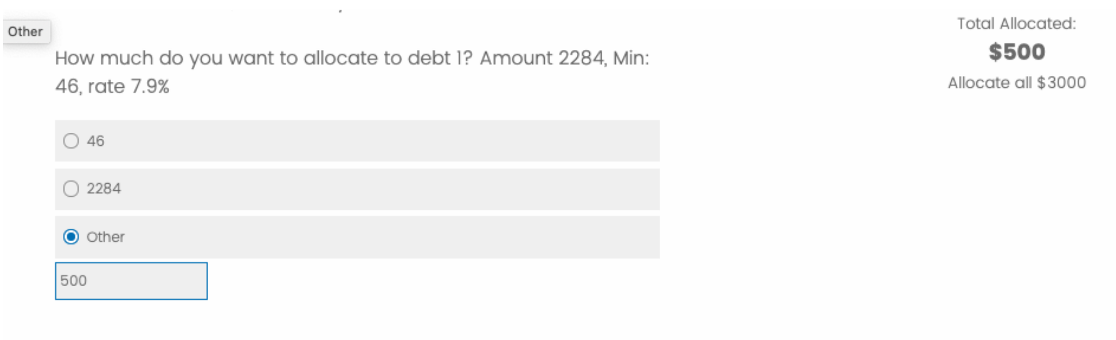


Figure 4.3. The Entry Format for Debt One in the Active Choice Condition. This figure shows the entry format for the Active Choice condition.

The final condition, the typical bill, was modeled on debt statements for US credit cards. Information on debt amounts and minimum payments are presented in a salient way, but more detailed information like interest rates and interest paid are presented after all of a cardholders' transactions for the month. To approximate the need for information search, we created a table similar to the standard information table except that only amounts and interest rates were featured. In addition to that information, participants could click a link to see additional information. The information on the new included how much you would have to pay to pay off the debt in full in three periods, similar to the three year number reported on credit card statements after the CARD act of 2009, the previous balance, the last payment, the amount of interest paid, and the interest rate (see figures 4.4 and 4.5 for examples of the participants' screens). The goal of this condition was to increase the ecological validity of the task by adding an element of information search, but its impact on concentration was not clear. If participants sought the additional information, it could increase the complexity of the task and thus might decrease concentration relative to our standard minimum payment condition. By contrast, participants could decide not to seek the information in which case, the main shift would be that participants would be making allocations without the relevant interest information increasing costs while leaving concentration the same.

The table below includes all of the information on each of your debts. Clicking each link will open a **new page** with more detailed information about your debt.

Debt Name	Total Debt	Minimum Payment	Additional Debt Information
Debt 1	$1925	$39	Full Statement for Debt 1
Debt 2	$2081	$42	Full Statement for Debt 2
Debt 3	$1743	$35	Full Statement for Debt 3
Debt 4	$1010	$20	Full Statement for Debt 4
Debt 5	$3103	$62	Full Statement for Debt 5
Debt 6	$1464	$29	Full Statement for Debt 6

Figure 4.4. The Information Table for the Standard Statement Condition. This figure shows the information table for the Standard Statement condition.

Full Statement for Debt 2:

New Balance:
$2081

Minimum Payment Due:
$42

If you make no additional charges using this card and each month you pay...	You will pay off the balance shown on this statement in...	You will end up paying an estimated total of
$829	3 periods	$2487

Account Summary:

Account Number	2
Previous Balance:	$2221
Last Payment:	$500
Interest Charged:	$360
Interest Rate	20.9%

Figure 4.5. An Example of the Debt Statement Information Show to Participants in the Standard Statement Condition. This figure shows a representative example of the information shown to participants if they clicked the link on a particular debt. In this case, it is round 2 for Debt 2 after an allocation of $500 had been made to that debt in the previous round.

Results and Discussion

Consistent with our preregistration, we excluded participants who failed our attention checks twice ($N = 10$) and participants who allocated more than their debt amount in more than one round ($N = 26$). Participants' allocations in each of the conditions with minimum payments were less concentrated in the no minimum payment control, though this was only marginally significant for the Active Choice condition. This was also true for log interest paid per round, even though the high interest salience condition was directionally more likely to allocate their largest amount to the highest interest rate debt (see figures 4.6 and 4.7 and table 4.1 for results). Overall, this suggests that the minimum payment effect we document in previous studies holds across the different information formats.

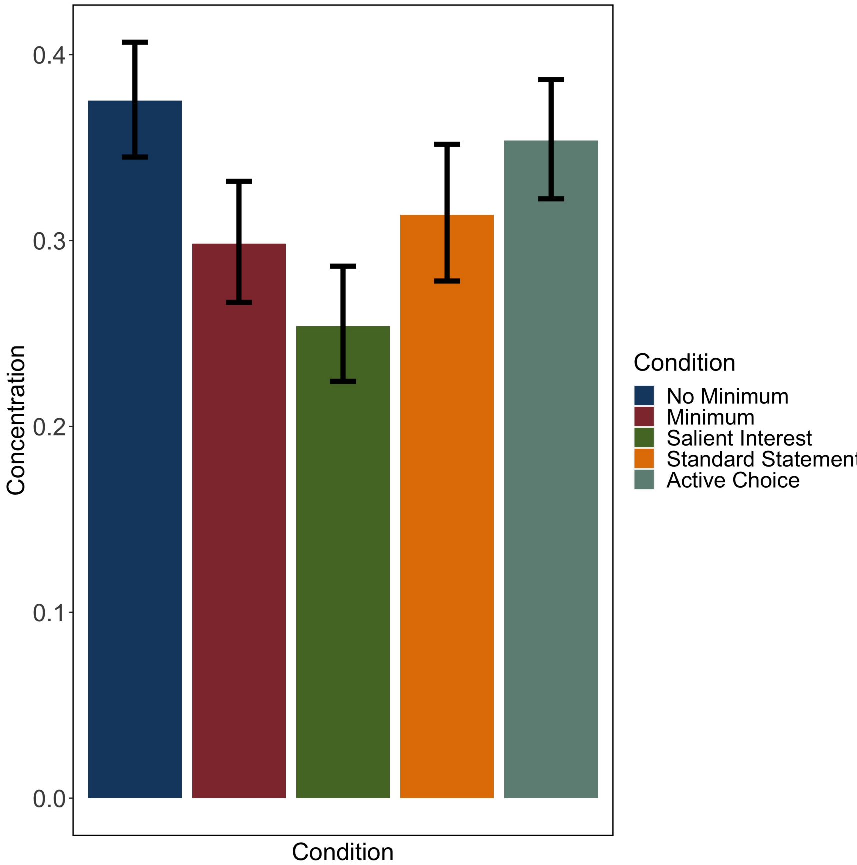


Figure 4.6. Changes in Repayment Concentration by Condition in Study Three. This figure plots the average concentration for the five conditions in study three, controlling for round fixed effects. The error bars represent 95% confidence intervals of the mean with standard errors clustered at the subject level.

Though all the minimum payment conditions were more dispersed and paid more interest than the no minimum payment control, the different information presentations altered the effects of the minimum payments. Ordering the information in the debt table by interest rates decreased the concentration relative to the original minimum payment condition ($\beta = -.238$, 95% CI$= [-.463, -.013]$, $z = 2.08$, $p = .038$), but the difference in concentration did not have an impact on interest paid ($\beta = -.004$, $t(382) = .49$, $p = .62$). The reason costs do not increase with concentration is that participants in the interest salient condition were significantly more likely to focus on their highest interest rate debt ($\beta = .59$, 95% CI= $[.23, .95]$, $z = 3.21$, $p = .001$). Providing participants in the active choice condition with preset amounts for the minimum and full payment increased concentration ($\beta = .205$, 95% CI= $[-.005, .415]$, $z = 1.91, p = .056$), but did not substantially affect the amount of interest paid ($\beta = -.003$, 95% CI= $[-.02, .02]$, $t(375) = 0.286$, $p = .78$) or the likelihood of focusing on the highest interest rate debt ($\beta = .077$, 95% CI= $[-.27, .43]$, $z = .431$, $p = .67$). By contrast, the condition modeled on the standard format of credit card statements did not substantially affect concentration ($\beta = .062$, 95% CI= $[-.16, .29]$, $z = .548$, $p = .584$), but did significantly increase the interest paid per round ($\beta = .05$, 95% CI= $[.03, .07]$, $z = 4.79$, $p < .001$) because participants were significantly less likely to focus on their highest interest rate debt ($\beta = -.81$, 95% CI= $[-1.16, -.46]$, $z = 4.50$, $p < .001$).

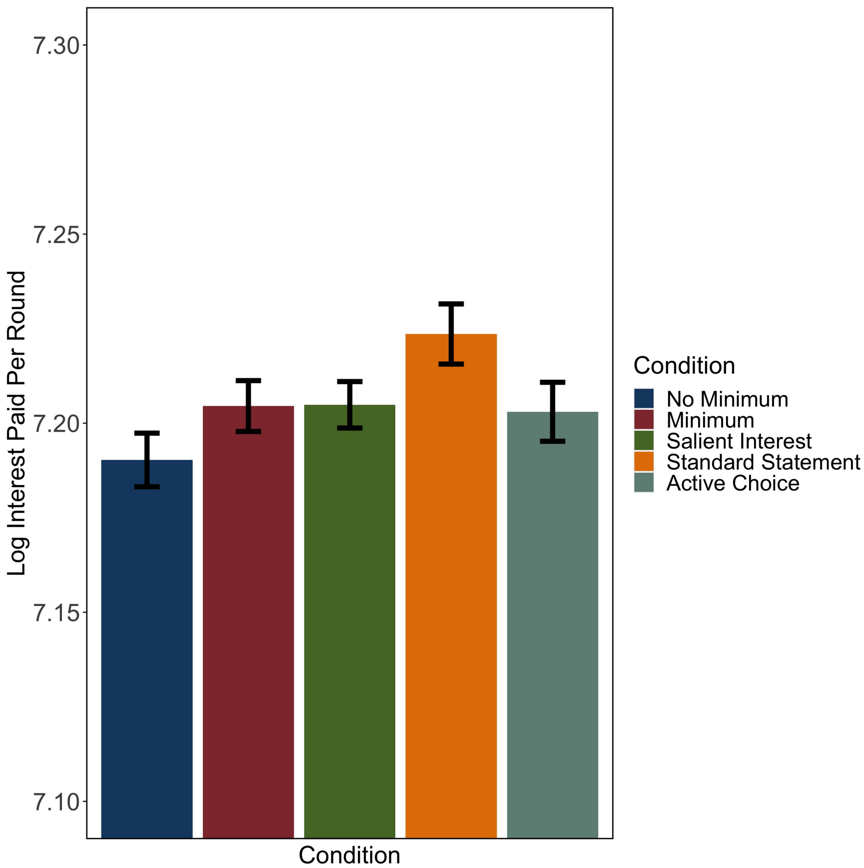


Figure 4.7. Changes in Log Interest Payments by Condition in Study Three. This figure plots the average log of the interest paid per round for the five conditions in study three, controlling for round fixed effects. The error bars represent 95% confidence intervals of the mean with standard errors clustered at the subject level.

Overall, none of the choice architectures were able to fully bridge the gap between the minimum payment and no minimum payment control condition; however, the differences within the minimum payment conditions do suggest a role for policy makers and firms in designing the choice environments consumers face. In particular, it appears that aggregating information on consumers debts, particularly interest rates, can help consumers repay in a less costly way. Also, making it easier for consumers to repay their entire debt balance or just the minimum can increase concentration, which may have additional motivational benefits (Kettle et al., 2016).

Additional Robustness Checks

In addition to the studies reported above, we conducted four studies to further test the robustness of these effects, using a variety of alternative designs. We briefly summarize results here, but the studies are fully reported in the online appendix. The first variant replicates our findings in an MTurk sample using a similar design to study one (appendix W1 study a). The second variant enhances the ecological validity of the experimental design. Since credit card bills tend to arrive at different points throughout the month, we extend our findings to a version of our task in which participants make allocation decisions one at a time (appendix W1 study b). The third variant aims to address the possible concern that the mathematical sophistication required in the minimum payment condition is higher than in the control condition, which could induce rounding. Our findings are directionally consistent when minimum payments are round numbers ($25), though some do not reach significance (appendix W1 study d). The fourth variant introduces a condition where participants have the minimum payment amount filled in by default. This is similar to the active choice condition in study three, and produces similar effects.

General Discussion

The current research makes three central contributions to the literature on debt repayment strategies. First, we document a dispersion effect of minimum payments that leads consumers to spread their allocations across more options. Second, we show that because participants overall tend to focus on their higher interest rate debts, the minimum payments lead to larger interest costs. Third, we find that the information and allocation environments can be used to help consumers reduce interests costs in the presence of minimum payments, but can also exacerbate the impact of allocation decisions. Our experimental results suggest that minimum payment requirements may contribute to the overdispersion of repayments observed in field data.

Our findings also document a novel path, beyond anchoring , through which minimum payments can harm consumers by inducing naïve diversification and leading to increased allocation dispersion. Consumers facing minimum payments fundamentally alter their strategic approach to repayments across accounts. In particular, we find evidence in favor of a novel dispersion effect in which consumers allocate in a more dispersed fashion. Although minimum payments serve an important role in ensuring that debts are not neglected, they may negatively impact consumer well-being by drawing consumers' attention from more important cues, and leading to higher interest costs as a result. Our work suggests that consumers might repay more efficiently with automated minimum payments, complemented by prompts to consider an additional payment each month. Further work should investigate the possibility of leveraging the reminders to offset the costs associated with setting repayments at the minimum level and forgetting to ever reexamine (Sakaguchi, Stewart, and Gathergood, 2018).

While it is unlikely that the government would mandate the removal of minimum requirements, our research has several implications for policy. For example, one change that could aid consumers is featuring interest rates on the front page of credit card statements. Currently interest rate information is not required on the first page of credit card statements, where minimum payments and other debt amount information is shown. In study three we show, that making interest rates more salient helps consumers prioritize across cards, and reduces interest paid relative to a standard credit card statement even in the presence of minimum payments. However, changing the display of credit cards requires an act of congress. As a result, firms that provide financial advice or fintech apps that aggregate finances could aid consumers by aggregating their credit card debt information including interest rates.

A more extreme solution already being offered by the marketplace is automated credit debt management. For example, users of the app Tally pay a single sum to the app which then allocates the lump sum to minimum payments and the highest interest rate debt a consumer has (Tally, 2020).However, it is not clear that consumers are fully aware of their misallocations, which may reduce demand for these products.

Our findings add to the literature on heuristics in debt repayment by investigating the role of minimum payments in allocation across multiple cards. A number of papers suggest that consumers allocate money to their smallest accounts first (Amar et al., 2011; Besharat, Carrillat, and Ladik, 2014; Besharat, Varki, and Craig, 2015) and recent work has provided evidence for a balance matching heuristic (Gathergood et al., 2019). In our data, these heuristics occur alongside the dispersion effect we document, though they occur at fairly low levels. Future work should examine the interplay between the use of these heuristics, dispersion, and motivation.

Conclusions and Implications

Given high levels of consumer debt and consequential interest costs, understanding impediments to implementation of optimal allocation of money towards debt repayment is critical. However, our work provides broad insight into the allocation decisions people make when facing scarce resources across multiple options simultaneously. Unlike in debt repayment, where more dispersion tends to lead to more costs, there may be situations, where minimum requirements can encourage consumers to maximize their utility. The literature on variety seeking (e.g., Simonson, 1990) suggests that people have a preference for variety in simultaneous allocation decisions. Minimum requirements may fulfill the desire for variety, leading additional allocations to be more aligned with consumer preferences than those without minimums. Future work should investigate the interplay between variety seeking and minimum requirements.

Our work has specific relevance to policy makers in the debt repayment context. Policy makers have been relying on marketing research to determine what kinds of information to display on credit card statements to help consumers repay their debts more effectively. For example, the CARD Act of 2009 added information to credit card statements to help consumers understand their debts and minimum payments better. The act mandated that statements include how long it would take to pay off credit card debts while only paying the minimum as well as the amount consumers would need to pay to repay their debt in three years. While the impacts of the changes under the CARD act have had limited effects (Keys and Wang, 2019), the assumption underlying the addition of a three year repayment amount was that it would increase the amount allocated to debt repayment consistent with findings around minimum payment targeting (Navarro-Martinez et al., 2011). In addition to the more specific recommendation discussed above, our work highlights the importance of considering the consumer situation when designing policy. Most consumers have multiple debt accounts, so nudges targeted at increasing repayments on individual accounts may lead consumers to incur additional costs. In this instance, policy makers need to consider the impacts of nudges on the full portfolio of debt accounts when they make policy changes. More broadly, applying marketing research to policy requires careful consideration of the environments consumers face. We recommend marketers use make use of descriptive research not just to motivate their research questions, but in their empirical designs to maximize the likelihood of real-world impact.

Tables

Table 4.1

Results from Study 3.

This table shows the results of study three comparing each minimum payment condition to the no minimum payment control condition. Column one shows the results for the concentration dependent variable, column two shows the likelihood of making the largest repayment to the highest interest rate debt or paying it off in full, column three shows the log of interest paid per round. All regressions include round fixed effects. Standard errors are clustered by subject and shown in parentheses.*, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Dependent variable:		
	Concentration	Interest Focus	Log Interest Paid
	(1)	(2)	(3)
No Minimum Payment Control	-0.450^{***}	1.417^{***}	7.174^{***}
	(0.071)	(0.140)	(0.007)
Minimum Payment	-0.404^{***}	-0.340^{*}	0.028^{***}
	(0.108)	(0.188)	(0.010)
Salient Interest	-0.643^{***}	0.252	0.024^{**}
	(0.109)	(0.185)	(0.009)
Active Choice	-0.197^{*}	-0.263	0.025^{**}
	(0.102)	(0.181)	(0.010)
Standard Statement	-0.341^{***}	-1.153^{***}	0.079^{***}
	(0.109)	(0.181)	(0.011)
Round Fixed Effects	Y	Y	Y
Note:			*p<0.1; **p<0.05; ***p<0.01

CHAPTER 5
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