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Essays in Consumer Behavior and Preference Elicitation

A dissertation submitted in partial satisfaction of the requirements for the degree Doctor
of Philosophy

in

Management

by

Coby Morvinski

Committee in charge:

Professor On Amir, Chair
Professor Terrence August
Professor Ayelet Gneezy
Professor Karsten Hansen
Professor Piotr Winkelman

2015

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The Dissertation of Coby Morvinski is approved, and it is acceptable in quality and form for publication on microfilm and electronically:

Chair

University of California, San Diego

2015

DEDICATION

I dedicate this dissertation to the loving memory of my father, Mendel Muravinski, whose role in my life was, and remains, immense. Although he did not stay long enough in this world to see his vision of his rebellious younger son pursuing a higher education, I know that on this day he could not have been any prouder.

TABLE OF CONTENTS

Signature Page.....	iii
Dedication.....	iv
Table of Contents.....	v
List of Figures.....	vi
List of Tables.....	vii
Acknowledgments.....	viii
Vita.....	xiii
Abstract of the Dissertation.....	xiv
Introduction.....	1
Chapter 1. Liking Goes with Liking: An Intuitive Congruence between Preference and Prominence.....	7
Abstract.....	8
Introduction.....	9
Materials and Methods.....	16
General Discussion.....	40
Appendix.....	48
Acknowledgments.....	56
References.....	57
Chapter 2. The effect of Stated Preference on subsequent Revealed Preference.....	62
Abstract.....	63
Introduction.....	64
Materials and Methods.....	68
General Discussion.....	91
Appendix.....	99
References.....	108
Chapter 3. “Ten Million Readers Can’t Be Wrong!”, or Can They? On the Role of Information about Adoption Stock in New Product Trial.....	111
Abstract.....	112
Introduction.....	113
Materials and Methods.....	120
General Discussion.....	146
Appendix.....	164
Acknowledgments.....	174
References.....	175

LIST OF FIGURES

Figure 1.1. Experiment 1 - Number of participants assigning a movie to a heads in ...	47
Figure 2.1. Experiment 3 - Revealed preference distribution across conditions.....	99
Figure 2.2. Experiment 4 - Revealed preference distribution across conditions.....	100
Figure 3.1. Experiment 1 - Purchase rate of book lotteries across conditions.....	159
Figure 3.2. Experiment 2 - Purchase rate of performance drink across conditions.....	160
Figure 3.3. Experiment 3 - Prediction models of purchase probabilities	161
Figure 3.4. Experiment 3B - Importance of CSOA information as a function of expertise and information quality.....	163

LIST OF TABLES

Table 1.1. Experiment 3 - Logit Model Results.....	46
Table 2.1. Experiment 1 - results.....	95
Table 2.2. Experiment 3 - Summary Statistics.....	96
Table 2.3. Experiment 4 - Summary Statistics.....	97
Table 2.4. Experiment 4 - regressions results, effects on stated and revealed preference.....	98
Table 3.1. The effect of a large CSOA signal on the purchase likelihood in different levels of affinity and uncertainty.....	154
Table 3.2. Experiment 1- Book lottery choice logit models results	155
Table 3.3. Experiment 2 - Performance drink sales logit models results.....	156
Table 3.4. Experiment 3 - Online participants' choice.....	157
Table 3.5. Experiment 3B - The impact of CSOA information on mattress purchase decision.....	158

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On's advising and friendship. I could not have imagined having a better mentor for my Ph.D study.

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No words can describe how grateful I am for being surrounded by the family that I have been blessed with in my life. I dedicate this dissertation to the memory of my dad, Mendel Muravinski, whose role in my life was, and remains, immense. I know that on this day he could not have been any prouder. I am also eternally thankful to my mom, Ida, who has shown nothing but support and encouragement throughout my education and life. I also thank my brothers Roni and Marik whom I have missed so much during my long stay in San Diego. Finally, without the exceptional support of my family none of this would have been possible. My dear wife Dinorah, whom I love so much, has been extremely supportive of me throughout this entire process and has made countless sacrifices to help me complete my classwork and research. If anything, you deserve as much credit as I do for this work. My children, Lian, Gail, and Millie who often had to endure my absence but seldom complained. Although, hardly aware of your stimulating role, you were the source of my passion, inspiration, strength, comfort, pride and joy. I am so fortunate to have you in my life.

Chapter 1, in part, is currently being prepared for submission for publication of the material. Coby Morvinski and On Amir. The dissertation author was the primary investigator and author of this paper.

Chapter 2, in part, is currently being prepared for submission for publication of the material. Coby Morvinski, Silvia Saccardo and On Amir. The dissertation author was the primary investigator and author of this paper.

Chapter 3, in full, is currently being prepared for publication of the material. Coby Morvinski, On Amir and Eitan Muller. The dissertation author was the primary investigator and author of this paper.

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ABSTRACT OF THE DISSERTATION

Essays in Consumer Behavior and Preference Elicitation

by

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Doctor of Philosophy in Management

University of California, San Diego, 2015

Professor On Amir, Chair

Our life is filled with choices which we describe as preferences. Preferences depend on the sensitivity of specific decisions to contextual and situational states that surround them as well as the support that individuals have in making them. As a result, science offers no simple summary of individuals' competence as decision makers, but a suite of theories and methods suited to capturing these sensitivities.

This dissertation contributes to the existing theory by exploring new grounds in consumer decision making that broaden our knowledge of decision science and making more sense of some of the otherwise unpredicted observed behaviors.

Chapter 1 explores conditions under which some individual's preference can be implicitly elicited. A series of five experiments demonstrate that people intuitively relate preferred choices to prominently labeled cues (such as Heads as opposed to Tails in a coin toss) and vice versa. Some findings suggest that preference-prominence congruence may be rooted in a deeper link between prominence and fluency.

Chapter 2 investigates well-known measures of individual preference: stated and revealed preferences. A series of four experiments involving consequential decisions demonstrate that the mere act of stating one's preference may influence subsequent behavior and the preferences it reveals. The results also suggest that consistency with previous judgments, but not greed, plays a central role in biasing observed preferences. Individuals who stated their desire compensation for a task they just performed, committed to a much higher compensation than those who haven't done so.

Chapter 3 investigates the conditions under which information about a large number of current adopters affects product attractiveness. The main results suggest a 'Goldilocks' requirement of product uncertainty in which large stock information that is

coupled with too much or too little uncertainty can have no or even detrimental effect on sales. Particularly, while current adoption information may be uninformative for consumers who are already well informed (e.g., experts), too much product uncertainty together with a statements about a large number of current adopters may undermine seller credibility and decrease adoption likelihood.

INTRODUCTION

Our life is filled with choices. We choose what to wear, buy, and eat, when to exercise and with whom we want to be with on a daily base. The unit of analysis used to describe such choices has been defined as preferences, and was initially argued to be axiomatically defined by observed behavior. More recently, behavioral researchers have looked at preferences as not only malleable, but also systematically constructed as a function of the situation and various types of mental processes. That is, a preference exhibited in one context may not necessarily manifest in a different one. This has led to some scholars questioning whether it even makes sense to use preferences as a fundamental construct and has made the task of identifying consumer preferences more challenging. Researchers and practitioners constantly strive to document potential factors contributing to our seemingly unstable preferences where the hallmark is the ability to forecast future behavior. Such factors have been documented in different research domains such as context-effects, heuristics and biases (or bounded rationality), emotions, norms and many others. Preferences depend on the sensitivity of specific decisions to contextual and situational states that surround them as well as the support that individuals have in making them. However, despite decades of intensive judgment and decision making research, the science has been advancing slowly. As a result, no single summary of individuals' competence as decision makers exists; rather, this field boasts a suite of theories and methods suited to capturing these sensitivities.

This dissertation contributes to the existing theory by exploring new grounds in consumer decision making that broaden our knowledge of decision science and advance our understanding of otherwise unexplained behaviors. In the following 3 chapters, I present new insights into the complex processes that shape some consumer behaviors and the preferences they reveal. My work advances our understanding of the factors influencing people's choices by informing conditions under which some outcomes can be better predicted. What is more, some of my findings can help investigators not only account for potential elicitation biases in their research program but also to develop more reliable methodologies to estimate individual preferences.

In Chapter 1 (coauthored with On Amir) I explore conditions under which individuals' preferences can be implicitly elicited. This investigation offers researchers and practitioners additional tools that can help to measure intuitive preferences which may be less susceptible to biases, particularly biases created by deliberate cognitive processes. Specifically, some labels and cues are more prominent than others. For example, in the pair of labels describing the outcome of a coin-toss, heads is a more prominent label than tails and, therefore, is more likely to be selected.

A series of five experiments demonstrate that people intuitively relate preferred choices to prominently labeled cues (such as heads as opposed to tails in a coin toss) and vice versa. Importantly, the observed congruence is asymmetric – it does not manifest for non-prominent cues and non-preferred choices. For example, in one study online participants tossed a virtual coin to determine which prize they would (hypothetically) win from a pair of prizes, where one prize was clearly more attractive than the other.

Before tossing the coin, participants decided which prize they would win in the case of each of the two possible coin-toss outcomes. A simple framing manipulation demonstrates the preference-prominence congruence effect mentioned above. The vast majority of those who were asked to assign a prize to a heads outcome, chose to assign their preferred prize with this prominent labeled option. However, those who were asked to assign a prize to a tails outcome (a less prominent alternative) were indifferent between assigning either prize. Pairs of labels other than heads and tails are also explored. I propose a mechanism in which preference-prominence congruence may be rooted in a deeper link between prominence and fluency. Higher fluency seems to elicit positive affective reaction, and the observed congruence is proposed to be the result of evaluative matching between two positive affective cues. The final study in this investigation provides evidence for the proposed mechanism.

When in doubt or facing a hard decision one may flip a coin, and while the coin is airborne, it may become apparent which face he or she would prefer the coin to land on. As evident in this chapter, flipping the coin may not even be necessary. Rather, simply following the option he or she assigns to a Heads outcome may in fact reveal their preferred choice.

Chapter 2 (coauthored with On Amir and Silvia Saccardo) investigates well-known measures of individual preference: stated and revealed preferences. Given the critical role that preference estimation plays in many domains of behavioral science, a better understanding of the influences of selected elicitation procedures on the estimation outcome is a valuable contribution. Many investigators continue to use both stated and

revealed measures of preference, many times simultaneously, without being aware of the effect one measure has on the other.

The results of four experiments involving consequential decisions show that the mere act of stating one's preference may influence subsequent behavior and the preferences it reveals. In particular, this chapter explores willingness to accept (WTA) elicitation for a supplied labor. Study participants who worked on a task could freely take any wage they deemed fit (revealed preference). However, before taking the money, some participants were asked to write how much they believe they should be paid (stated preference). Different behavioral science theories would yield contradicting predictions about the effect of our writing manipulation. While research about the effect of money and greed would predict that people should take more if engaging with money directly, other research on stated preference bias and consistency would predict that stating one's preference in writing a-priori should lead to taking a higher compensation. The results reported in this chapter suggest that consistency with previous judgments, but not greed, plays a central role in biasing observed preferences. Individuals who stated their desired compensation for a task they just performed, committed to a much higher compensation than those who haven't done so. It is further suggested that peoples' intuition is not aligned with the situation that would lead them to the highest payoff whereas committing to a higher wage because of a preceding stated preference procedure does not bear additional costs. These findings are in line with other evidence in economic and social psychology suggesting that people strive for consistency in their commitments even when they are non-binding. For example, people were more likely to vote if they were asked whether they plan to vote on the election-day eve. We also propose that the abstract and

detached nature of simply expressing one's WTA in writing contributes to overstate one's preference evaluation. Consequently, the desire to maintain consistency leads to an increase in accepted compensation. These findings bear direct implications to theory and practice around preference measurement in research, markets, and policy making.

Chapter 3 (coauthored with On Amir and Eitan Muller) challenges the over-arching assumption that the larger the number the current adopters of a new product, the greater the likelihood of additional adoption, presumably because of its positive signal to potential customers. For example, marketers use statements like "Ten million housewives can't be wrong [about purchasing the product]", and "Over 19 Billion served", to attract additional customers. This chapter investigates when a customer should make a positive inference from information about a large initial sales volume, if at all. In particular, it explores when information about a large number of current adopters decreases new product attractiveness. In this investigation, I employ both controlled and field experiments examining customer choices of new and unfamiliar products, and study the effect of information about a large-stock of adoption on people's purchasing decisions, as a function of characteristics of the stock itself (e.g., the affinity between the stock and the customer) and the degree of uncertainty around the new product (e.g., how much is known about the product or the category). Interestingly, the results reported in this chapter reject the lay notion that information about a large stock of adoption uniformly increases adoption likelihood. Instead, I find an interactive effect between two major factors influencing purchase decision in the presence of information about current adopters: the identity of the stock and the degree of product uncertainty. The main results suggest a 'Goldilocks' requirement of product uncertainty in which large stock

information that is coupled with too much or too little uncertainty can have no or even detrimental effects on sales. Particularly, while current adoption information may be uninformative for consumers who are already well informed (e.g., experts), too much product uncertainty together with a statement about a large stock of adoption may undermine seller credibility and decrease adoption likelihood. The effect of uncertainty is demonstrated repeatedly with a variety of product types, using both product information manipulations and measured expertise.

Chapter 1.

LIKING GOES WITH LIKING: AN INTUITIVE CONGRUENCE
BETWEEN PREFERENCE AND PROMINENCE

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ABSTRACT

In a series of five experiments, we find that people intuitively relate preferred choices to prominently labeled cues (such as Heads as opposed to Tails in a coin toss) and vice versa. Importantly, the observed congruence is asymmetric – it does not manifest for non-prominent cues and non-preferred choices. The first four experiments demonstrate the non-intuitive nature of the effect, while the last experiment suggests that preference-prominence congruence may be rooted in a deeper link between prominence and fluency. Higher fluency seems to elicit positive affective reaction, and the observed congruence is proposed to be the result of evaluative matching between two positive affective cues. We discuss the theoretical contributions to the study of preferences and decision making, as well as the practical implications to researchers and practitioners.

INTRODUCTION

A famous saying suggests that one in doubt, or facing a hard decision, should flip a coin, and while the coin is airborne it would become apparent which face he or she would prefer the coin to land on. Our current research suggests one can do even better. In the current work, we explore an intuitive process in which people implicitly associate their preferred choices to prominently labeled cues. For example, they associate their preferred alternative out of a choice set to the Heads face of a coin or to even outcome of a die roll (as opposed to an Odd one). Drawing on fluency research, we suggest this could be the result of the affective evaluative matching that occurs between the greater fluency of the prominent label and the preference for the alternative. By highlighting this intuitive process we not only improve our understanding of the decision making processes leading to observed choices and behavior, but also provide a lens towards uncovering intuitive preferences. In other words, we suggest that one should simply pick the alternative he or she matched with the Heads face of the coin in the abovementioned example – it is that alternative they intuitively prefer.

Some labels and cues are more prominent than others; for example, in the pair of labels describing the outcome of a coin-toss, Heads is a more prominent label than Tails and is more likely to be selected (Schelling 1960; Bar Hillel et al. 2014). We propose a possible mechanism in which an affect-based congruence between such prominence and preference resulting from evaluative matching associated with positive reactions. That is, we expect people to perceive an intuitive match between the positive evaluative judgment of a prominent cue and their preferred product. This account, however, also implies an asymmetry: cues or labels that are not prominent, being neutral but not negative, should

not be associated with less preferred choices. Therefore, this account implies that Heads would be matched with a preferred choice alternative, but Tails would have no specific association. We reason that some prominently labeled cues are more fluent than others and support our theory with the existing evidence that high fluency increases liking (e.g., Reber, Winkielman, & Schwarz 1998) as well as with a direct test to the hypothesized link between preferred choices and fluent cues.

We begin by establishing the theoretical grounds for the proposed congruence, continue with a description of the evidence supporting our predictions, and conclude with a discussion of the conceptual and practical implications of our findings.

Prominently labeled cues

For many decades now, researchers have been studying influences of stimuli prominence on decisions. As originally demonstrated by Schelling (1960), players in coordination games who cannot communicate with each other were much more successful in coordinating their choices when choosing the focal point of Heads over Tails, or a prominent traffic New York hub over any other possible meeting locations. These findings were replicated in many subsequent studies under controlled conditions and using other labels (see, e.g., Mehta et al. 1994 a, b; Bardsley et al. 2010). As noted by Lewis (1969), a prominently labeled cue is one that “stands out from the rest by its uniqueness in some conspicuous respect.” Lewis further argues that when players have no reasons to prefer one strategy over another, their default choices lean toward a prominent one.

That people have propensity to favor some labels as defaults is also evidenced by work documenting the affective response inherently triggered by some alternatives. According to the “Choosing by Default” heuristic (Frederick 2002) an alternative may become a default by virtue of its conspicuousness or psychological prominence. Because intuitive affective responses precede more cognitive evaluation, the alternative that elicits the most favorable affective response may enjoy the special status of being the default option, unless one can marshal a decisive case in favor of different alternative. This idea is similar to the effect of salience asymmetry on the figure-ground distinction in binary classification tasks (Rothermund & Wentura 2004). In the same vein, we expect the unequal prominence of labels of otherwise identical alternatives to impact decisions between these alternatives. We term such cases prominently labeled cues and argue that people are inclined to favor such labels over non-prominent ones. For example, when choosing between two 6-sided die outcomes that provide an equal chance of winning a prize (e.g., an even vs. an odd outcome), people are expected to prefer *even* over *odd*, because the former constitutes the more prominent alternative (e.g., Nuerk, Iversen & Willems 2004; lochy et al. 2000; Kinoshita & Peek-O’Leary 2006) and as a result elicits a more positive affective response.

The mechanisms that generate the prominence property vary. Associated prominence could stem from language priority, culture, pre-existing associations, valence, or familiarity. Moreover, a default can emerge from the relative location, or scale value, of the stimulus mental representation on the continuum it represents. For example, we ask “How *tall* are you?”, “How *heavy* are you?” and “Did you *like* the movie?” instead of “How *short* are you?”, “How *light* are you?” or “Did you *dislike* the

movie?” Linguistically, tall, heavy, and liking, are the unmarked ends (or labels) and short, light, and disliking are the marked ends of their corresponding scales (Mandler et al. 1987). That is, use of the unmarked end does not imply an existing pre-conception on the part of the person asking the question, but use of the marked end does. As noted by Rothermund & Wentura (2004), linguistic markedness can give rise to stimuli prominence. In most, if not all cases, the perceptually positive term is the unmarked end that names the dimension (Ktatzky et al. 1973), and therefore will be used as the default. Notwithstanding, in this work, we are less interested in the mechanisms that lead to the emergence of prominence, but rather in the fact that those labels become a default as a result of their psychological prominence. As such, a prominently labeled cue is the more accessible and easier to process of the two ends, and is likely to be perceived as more familiar and thus generate a more positive hedonic reaction¹. We discuss this latter relation in detail below.

From prominence to liking

We suggest that the psychological prominence inherent to some labels may be a result of high fluency-familiarity, which, consequentially, affects judgments. For example, Kinoshita & Peek-O’Leary (2006) argue that the more fluently processed category in an IAT (i.e., the familiar, the positive, and the linguistically unmarked) becomes the more prominent one. One of the first demonstrations of the influence of familiarity on evaluative judgment was the research into the mere-exposure effect

¹ For example, in a verification test run in our lab, participants were both faster to recognize “Head” over “Tail”, embedded in a series of neutral words (541 ms vs. 574 ms, $t[413] = 3.03, p = .002$), and were quicker to pay attention to it in a Dot-Probe task (392 ms vs. 409 ms, $t[519] = 3.98, p < .001$).

(Zajonc 1968) in which repeated exposure to an initially neutral stimulus increased its liking (for review, see Bornstein 1989). This increase in liking is suggested to be the result of changes in perceptual fluency of the stimuli-related information (Bornstein and D'Agostino 1994; Klinger and Greenwald 1994; Seamon et al. 1983), though the effect need not reach awareness to manifest (e.g., Moreland & Zajonc 1977; Wilson 1979; Kunst et al. 1980; Mandler et al. 1987). The link between fluency and evaluative judgment may even be more fundamental than mere-exposure. Individuals seem to monitor the fluency with which they can extract information from a stimulus such that it becomes hedonically marked: high fluency elicits a positive reaction (Winkielman et al. 2003). This paradigm presumes that fluency-based affective reactions are orthogonal to the first-order affective responses to the stimuli attributes, and that it may or may not be reflected in conscious experience. Therefore, people detect the fluency signals even before they fully decode the content (i.e., attributes) of the stimulus (Lewenstein and Nowak 1989; Norman et al. 2000; Smith 2000). Consequentially, they may construct a positive affective reaction to a stimulus prior to their feature-based evaluative judgment and independent of it (Pocheptsova, Labroo, & Dhar 2009). This is because fluency may serve as a cue that the stimulus has been previously encountered, eliciting a feeling of familiarity (Schwarz & Clore 1996; Smith 2000), which, in turn, is associated with processing ease (Jacoby et al. 1989), speed (Haber & Hershenson 1965; Jacoby & Dallas 1981), and greater validity (Begg & Armour 1991). The familiarity-positivity association may even be grounded in a biological predisposition for caution in encounters with unfamiliar and therefore potential harmful objects (Zajonc 1988). This relation is so fundamental that its reversal also holds: increasing positive affect leads to an increase in

perceived familiarity (Garcia-Marques et al. 2004). In sum, prominently labeled cues seem to be more fluent: They are more accessible and may be processed faster and therefore may elicit a positive affective reaction as other familiar cues. However, lack of prominence might simply be neutral. For example, while participants in Schelling's experiments may have experienced fluency-based positive affect to Heads, they need not have experienced a negative one to Tails. The latter may most likely have elicited a neutral reaction. We will return to this point of asymmetry in the next section.

Liking goes with liking

So far, we presume that prominently labeled cues representing otherwise identical stimuli (e.g., Heads or Tails outcomes), may be judged more positively because of an intuitive positive affective reaction. In what comes next, we hypothesize that people associate preferred products with prominently labeled cues through a process of evaluative matching. That is, given a choice, people would intuitively relate a prominently labeled stimulus to their preferred product, but not to a less preferred alternative. Put differently, we hypothesize that liking goes with liking.

As mentioned earlier, people rely on affective reactions in their decision making (e.g., Schwarz & Clore 1996), and at times render their feelings as a more diagnostic source of information in the absence of other relevant information to the judgment at hand. This is true even when the affective response is automatically generated, without awareness and regardless of its source. Moreover, this automatically generated affect influences subsequent reactions in favor of valence congruence: people respond faster when the affective valence of the response is congruent with the affective valence of the

stimulus, relative to when they are incongruent. For example, participants were faster to respond with a positive (negative) word such as Flower (Cancer) to stimuli with similar valence, such as Gift (Cruel) (De Houwer & Eelen 1998). Such evaluative matching is also rooted in the IAT paradigm which rests on the assumption that similarly valenced concepts are associated with one another (Greenwald, McGhee & Schwartz 1998), or can also emerge as a result of compatibility of prominence, whereby that the more fluently classified category is also the more prominent one (e.g., Chang & Mitchell 2011, Kinoshita & Peek-O'Leary 2006). Similarly, positive or negative affective priming facilitated the evaluation of subsequent target cues with similar valence (for review, see Fazio 2001). Evidence for automatic affective processing has also been obtained using variations of the Stroop paradigm (e.g., De Houwer & Hermans 1994).

Based on these results, we propose the existence of a correspondence or a natural match between evaluative stimuli generating positive affect. In the context of our work, we expect this affect-based evaluative matching mechanism to yield congruence between preferred choices and prominently labeled cues, two ends that carry positive affective reaction. In other words, we expect people to intuitively relate the preferred option in a choice set to a prominently labeled cue, rather than to a non-prominent one. By the same token, we would expect the reverse: people should intuitively relate a prominently labeled cue to a preferred choice alternative, rather than to a less-preferred one, because of the intuitive (and positive) affective reaction it promotes.

Importantly, we do not hypothesize a similar mechanism for the non-prominent less-affective stimuli, such as Tails in a Heads/Tails coin-toss. The mechanism described above reflects an important asymmetry in that non-prominently labeled cues may be less

fluent, but do not evoke a negative affective reaction. More likely they should be thought of as neutral in that respect. Therefore, we do not expect a congruence between non-prominent labels and the less preferred choices. In sum, we suggest an affective evaluative judgment process in which people intuitively associate preferred choices with prominently labeled cues. This fluency-based congruence should not emerge with less prominent, less fluent labels because more often than not, they are simply neutral.

MATERIALS AND METHODS

In what follows we describe five experiments demonstrating the preference-prominence congruence and its properties. Experiment 1 reveals that given two products, participants in a coin-toss game tend to assign the reward they prefer rather than its alternative to a Heads outcome. Experiment 2 confirms our hypothesis that the effect is asymmetric by showing that phrasing the same task using an assignment to a Tails outcome leads participants to an equal assignment of the two rewards. In Experiment 3, participants demonstrated similar behavior under time pressure, supporting the intuitive nature of the effect. Moreover, unlike previous studies in which participants assigned a reward to a coin-toss outcome (Heads vs. Tails), in Experiment 3 they matched in the opposite direction: they assigned Heads or Tails to a given reward, exhibiting the bidirectional nature of the congruence. In other words, it does not matter which is being assigned to which, the prominently labeled cue ends up being matched with the preferred reward. Experiment 4 generalizes our results to other prominent labels and provides another conceptual replication. Lastly, by manipulating fluency directly instead of

prominence, Experiment 5 demonstrates that the preference-prominence congruence may be rooted in a deeper link between preference and fluency. We conclude with a discussion of the theoretical and practical implications, as well as limitations and directions for future research.

Experiment 1

To explore the proposed congruence between preferred choices and prominently labeled cues, participants in Experiment 1 tossed a virtual coin to determine which of the two possible rewards (a choice between two DVD movies) they would hypothetically win. Before tossing the coin, participants decided which reward they would win if the coin landed on Heads and correspondingly, the reward they would win if the coin landed on Tails. We manipulated the rewards between participants: Half of the participants saw a pair of DVD movies comprised of a superior one and a medium preference one, while the other half saw the same medium preference DVD paired with an inferior movie. The share of the assignment of the medium preference DVD that was constant across both conditions to the Heads coin-toss outcome was the dependent measure. Based on the theory summarized above, we predicted that participants would intuitively favor the assignment of the medium preference DVD to a Heads, the prominent label (Schelling 1960, Ch. 3), only when that reward is paired with the inferior movie, that is, only when it is preferred over the alternative.

To establish base preferences for DVD movies as rewards, we conducted the following pretest:

Pretest: One hundred fifty two online participants were recruited through Amazon Mturk (69% male, $M_{\text{age}}=28.72$). Participants rated 16 movies from different genres using a 10-star rating system (in half-star increments). We selected the following 3 movies: *Forrest Gump* ($M_{\text{Forrest Gump}} = 7.19$), *The Alamo* ($M_{\text{The Alamo}} = 3.77$), and *Superbabies: Baby Geniuses 2* ($M_{\text{Superbabies}} = 1.63$) such that the Movie *The Alamo* was rated significantly higher than *Superbabies* ($t[151] = 10.49, p < .001$) but it was also rated significantly lower than *Forrest Gump* ($t[151] = 14.27, p < .001$). To construct the desired choice set, we used *The Alamo* as the focal (medium preference) choice joined by *Superbabies* and *Forrest Gump*, to construct the focal-preferred (*The Alamo* vs. *Superbabies*) and focal-not preferred (*The Alamo* vs. *Forrest Gump*).

Design

Four hundred and three participants were recruited through Amazon Mtrurk (62% males, $M_{\text{age}} = 30.5$ years), a population similar to the pretest participants. As part of a hypothetical game participants saw pictures of two DVD movie covers and read the following text: “*Consider the movies in the pictures above. Imagine you will be allowed to keep a DVD or Blu-ray of ONE of these movies for yourself. The movie you keep depends on the outcome of a coin flipping game you are about to play. Here is how the game is played: 1) before flipping the coin, you decide which movie you win if the coin lands heads and which movie you win if it lands tails. 2) you flip the coin and win the movie according to your previous decision.*” After confirming their understanding of the game rules, participants were allowed to play the game (Exact on screen instructions can be found in Web Appendix A). Importantly, we reminded participants that their

assignment of a DVD to a Heads outcome implies that if the coin lands on Tails, they would receive the unselected DVD.

All the participants were offered the movie *The Alamo* (the focal reward) as one of the two reward options, but for half of them it was paired with the movie *Superbabies* and for the other half it was paired with the movie *Forrest Gump*. Note, according to our pretest, the movie *The Alamo* was preferred to its alternative in the first condition, but less preferred to its alternative in the second condition. On the next page, participants were presented with a javascript program that allowed them to toss a virtual quarter-dollar coin. They were encouraged to “flip the coin a few times to convince [themselves] it [was] a fair coin”. Next, participants saw the movie they previously assigned to Heads and received an opportunity to change their choice before tossing the coin, to make sure they were cognizant of their choice. Once ready, they advanced to the actual game page where they tossed the virtual coin, and were presented with their winning – the movie corresponding to the coin-toss outcome.

Additionally, we indirectly measured the relative preference for the movies through participants’ self-reported affective states and selling prices. Specifically, participants rated their feeling of happiness, disappointment and regret on a 7-point scale ranging from “Not at all” to “Extremely” after they realized the outcome of the game (i.e., which movie they won). Participants also indicated the minimum price they would be willing to “sell” their DVD movie on a \$0-\$26 scale. Following an attention check question, the experiment concluded with a basic demographic questionnaire.

Results

Eleven participants failed to correctly answer the attention check question and were excluded from the analysis².

Manipulation check: We confirmed participants' movie preferences in two different ways, above and beyond the pre-test and the choice data reported below: their self-reported affective state as a result of the game outcome and their reported selling prices. To measure participants' affective state, we averaged their self-reported level of happiness, disappointment (reverse-coded) and regret (reverse-coded) to create a *positive-affect* index ($\alpha = .85$). Among participants offered the movies *The Alamo* and *Superbabies*, those who "won" the movie *The Alamo* were significantly happier than those who won the movie *Superbabies* ($M_{\text{The Alamo}} = 5.63$, $M_{\text{Superbabies}} = 3.55$, $t[197] = 11.51$, $p < .001$). However, among participants offered the movies *The Alamo* and *Forrest Gump*, those who won the movie *The Alamo* were significantly less happy than those who won the movie *Forrest Gump* ($M_{\text{The Alamo}} = 4.37$, $M_{\text{Forrest Gump}} = 6.01$, $t[191] = 8.94$, $p < .001$). This result replicates and confirms the findings of the pretest. We also used the reported selling price as a proxy for participants' liking of the movies. The average selling price of *The Alamo* was \$7.54. As expected, this price was significantly higher than the average selling price of *Superbabies* ($M_{\text{Superbabies}} = \5.69 , $t[205] = 3.71$, $p < .001$), but it was also significantly lower than \$8.57, the average selling price of the *Forrest Gump* movie ($t[184] = 1.95$, $p = .05$). Moreover, we observed no difference in the selling price of *The Alamo* regardless of whether it was preferred (\$7.40, relative to *Superbabies*) or not preferred (\$7.69, relative to *Forrest Gump*) ($t[197] = .48$, $p = .63$).

² Including participants who failed the attention check did not change the nature of the results.

The selling prices results, again, replicate and validate the relative preference assumptions.

Main results: Out of 199 participants offered the movies *The Alamo* and *Superbabies*, 161 participants (81%) assigned the movie *The Alamo* to a Heads. However, when the alternative movie was *Forrest Gump*, a far more popular movie than *The Alamo* among Mturk participants, out of 193 participants only 65 participants (31%) assigned this very same movie to a Heads (Figure 1.1). In both cases, the movie assignment to Heads, differed significantly from chance ($\chi^2[1] = 74.79, p < .001$ and $\chi^2[1] = 19.91, p < .001$, respectively). Therefore, participants in our study associated the focal movie of *The Alamo* to the prominent labeled outcome (Heads) when it was their preferred choice, but not when it was their less preferred choice. The observed results corresponded with our prediction and manifest congruence between a preferred choice and a prominent label. Combining these results, we find that when the focal movie is the preferred alternative significantly predicts its assignment to a Heads ($\beta = 2.12, Z[391] = 8.99, p < .001$).

Discussion

Experiment 1 demonstrates that participants, who toss a coin to determine which DVD movie they would hypothetically win, tend to assign the movie they prefer to a Heads, a prominent cue. This is in stark contrast to the normative prediction of an even assignment for a 50/50 chance device, such as a coin. Keeping the focal reward constant and merely changing its relative preference in the choice set clearly influences the relation between the focal reward and the prominent cue. Only preferred rewards were closely associated with a prominent label. These findings support the idea that the

reward-prominent label relation is a function of the relative preference for the reward. A preferred choice appears to be congruent with a prominent label. This apparent congruence may be the result of several potential mechanisms.

One possible explanation for the apparent congruence is that both preference and prominence are cognitively represented by means of codes and that such codes are hierarchically ordered (Wallace 1971). Recent work also suggests that many binary stimuli are automatically coded as positive and negative polarities (Proctor & Cho 2006). Such binary stimuli may include: same–different, true–false, old–new, up–down, and left–right. Therefore, one can assume that compared with a non-prominent label, a prominent one is mentally represented as of a higher order, or even the positive-coded end of the prominence dimension. As a result, compared with a non-prominent label, a prominent one should be mentally endowed with a higher rank. Moreover, preference, by definition, represents rank ordering. Together, the preference-prominence congruence may be seen as merely a result of a rank-matching process. If indeed a rank-matching process underlies our findings than one should also expect a similar relation between low rank stimuli. Specifically, a non-prominent label (e.g., Tails) should be congruent with the less preferred product. However, if our findings are driven by affective evaluative matching, as the above analysis proposes, in which the lack of prominence, as well as non-detrimental choices are affectively-neutral stimuli, we would not expect congruence between non-prominent labeled cues and less preferred choice alternatives.

Another alternative explanation might be that individuals assign subjective probabilities to prominently labeled outcomes that are different than their actual chances of winning. Therefore, participants might have assigned their preferred reward to a

prominent label simply because they felt it offered them a better chance of winning. Our proposed account, affective evaluative matching, however, should not manifest in biased subjective likelihood of winning, as it suggests that Heads feels like a better match for the preferred reward without feeling more likely to actually occur. Experiment 2 was designed to test these alternative accounts.

Experiment 2

Design

One hundred people from Amazon Mechanical Turk (68% males, $M_{\text{age}} = 30.8$ years) participated in same coin tossing game as in Experiment 1, having to assign their preferred reward of a choice set to the result of a coin-flip. In a between subject design, the difference from Experiment 1 was that some participants assigned one of two movies to a Heads outcome, while others assigned one of two movies to a Tails outcome. This yielded a two conditions framing factor. Also, unlike Experiment 1, in the current experiment participants in both conditions saw the same pair of movies (screen location counterbalanced): *Forrest Gump* (pre-tested to be preferred) and *Superbabies* (pre-tested to be less-preferred). Participants were also asked to directly report their preferred movie. The order of the movie assignment to the coin-flip and preference-reporting tasks was counterbalanced. Lastly, we introduced two questions that required participants to retrospect on their previous decision: Participants used a 7-point scale, ranging from “Strongly disagree” to “Strongly agree” to indicate the extent to which they felt control over the winning outcome; Participants also indicated their feeling associated with their relative chances of winning each movie using an 11-point scale (see Web Appendix B):

Each point on the scale represented the winning probability distribution over the rewards such that the upper number represented the chance of winning *Forrest Gump* and the lower number represented the chance of winning *Superbabies*. The scale ranged from 100% -- 0% (sure winning of *Forrest Gump*) to 0% -- 100% (sure winning of *Superbabies*) in 10% steps. For example, the mid-point represented 50%/50%, whereas the point to the right of it represented 40%/60%.

After tossing the coin and realizing their winning movie, participants completed the same positive-affect index as in Experiment 1, as well as a basic demographic questionnaire.

Results

Manipulation check: in line with our expectations, 95 out of the 100 participants who took the survey indicated they preferred winning the movie *Forrest Gump* ($\chi^2[1] = 79, p < .001$).

Main results: Among 49 participants in the Heads-frame condition (asking them to assign a movie to the Heads outcome), 86% assigned the movie *Forrest Gump* – the a-priori preferred movie. However, only 53% of the 51 participants in the Tails-frame condition assigned this movie. While the first assignment distribution is significantly different from chance, the second is not ($\chi^2[1] = 23.59, p < .001$ and $\chi^2[1] = .08, p = .78$, respectively). Moreover, and perhaps more importantly, the distributions of the movie assignments in the two framing conditions differed significantly from each other ($\chi^2[1] = 11.06, p < .001$). We further explore whether participants' behavior in the Tails-frame condition is simply the symmetrical complement of those in the Heads-frame condition as the rank matching alternative mechanism would predict. If the two conditions mirror each

other, then the complementary result to the Heads-frame condition should correspond to the behavior of those in the Tails-frame condition. That is, the Heads condition result should also suggest that 86% (1-14%) would assign the less preferred movie *Superbabies* to a Tails outcome if the task had been framed using a Tails label assignment. It turned out, however, that only 47% of the participants in the Tails-frame condition followed this assignment pattern, rejecting the rank matching account ($\chi^2[1] = 42.01, p < .001$).

Our design also allowed testing for changes in subjective probability as another alternative mechanism: Did assigning a preferred reward to a prominently labeled outcome lead participants to feel more control over the winning outcome or perceive a higher subjective likelihood of its attainment? Apparently not. The average reported control over the winning outcome did not differ between participants in the Heads-frame and those in the Tails-frame conditions (2.98 and 3.02, respectively; $t[98] = .12, p = .9$)³. Similarly, participants did not differ in their feeling of their likelihood of winning either movie ($M_{\text{Heads}} = 5.86, M_{\text{Tails}} = 5.8, t[98] = .03, p = .76$). Interestingly, participants in both conditions felt they had a better chance of winning the movie they preferred than the alternative, potentially reflecting general optimism bias ($t[99] = 2.45, p = .016$). Finally, a pure subjective probability account would most likely predict a symmetric effect of the Heads and Tails frames, such that if Heads feels more likely, Tails feels less so. As stated above, we can reject this symmetric pattern.

Discussion

³ Also, in both conditions, those who assigned the movie they preferred and those who assigned the movie they did not prefer did not differ in their feeling of control over the winning outcome. ($t[98] = 1.02, p = .31$)

The results of Experiment 2 replicated the previous findings in the Heads-frame condition. The preferred option (*Forrest Gump*) predominated participants' assignments when the task was framed with the prominently labeled outcome (Heads). More importantly, the results fell short of supporting the two alternative accounts discussed above. While participants in Experiment 2 associated their preferred choice to the prominent label, they did not reveal a similar relationship between a less preferred choice and a non-silent label⁴. First, participants' behavior in the two framing conditions seems unlikely to stem from a rank-matching mechanism, as such a mechanism predicts symmetry, and our results are strongly asymmetric: the relation between a less preferred choice and a non-prominent label was not influenced by the supposed low-ranking congruence. Indeed, participants' choices in the non-prominent framing condition converge to 50%, the true probability of winning either reward. What is more, a decision that incorporates a non-prominently labeled alternative is not simply the symmetrical complement of one that incorporates a prominently labeled alternative. Holding the choice set constant and merely manipulating the label prominence in the assignment task seems to elicit different behavior. Participants' choices were influenced only in the presence of congruence between two positively evaluated cues: a preferred choice and a prominent cue. Together, Experiment 2's findings lend support to the property of asymmetry in the preference-prominence congruence hypothesis.

⁴ To reconfirm the results of Experiment 2, we ran an additional, similarly designed, experiment that included only the Tails assignment condition. Among 50 participants in this experiment, exactly half chose to assign the movie *Forrest Gump* to a Tails outcome, although, as expected, most of them (48) indicated it was their favorite movie.

Finally, although our participants accounted for the equal probability of each of the possible game outcomes only in the Tails condition, those in the Heads condition did not feel more control over the winning outcome nor did they overestimate their subjective probability of winning their preferred reward. These findings appear to suggest that participants' predisposition to relate a preferred choice to a prominently labeled outcome was not simply the result of different subjective probabilities between the two lotteries⁵.

Thus far, we observed that individuals tend to assign preferred rewards to prominently labeled outcomes. We also demonstrated the asymmetry property of the effect by noticing that unlike those in the prominent label condition, participants' choices in the non-prominent label condition conformed to the real probability of the game outcomes. We propose that prominent labels can be hedonically marked, facilitating congruence between two positively evaluated ends at an intuitive processing level. We designed Experiment 3 to further explore this hypothesis. First, participants in the first two experiments assigned a reward to a given label, but congruence should be bi-directional. That is, we expect to find similar results whether the task requires an active assignment of a label or a reward. Second, we argue that the effect results from affective evaluative matching, an intuitive process that relies on one's feelings and does not require deliberate thinking. Reliance on one's feeling is particularly apparent under time pressure (Siemer & Reizenzein 1998; Finucane et al. 2000). If the observed effect is indeed

⁵ To further substantiate this point, we asked 153 additional participants what would be the most they would be willing to pay for a lottery ticket that offered them to toss a coin and get a \$100 for heads and nothing for tails. The average amount (\$14.91) did not significantly differ from that of participants who were asked the same question but with reversed rewards for heads and tails (\$16.29, $t[151] = .45, p = .63$). This reinforces our conclusion that subjective probability is not likely to account for our findings.

intuitive as we propose, then imposing time constraints should not change our findings.

We address these issues in Experiment 3.

Experiment 3

Design

Two hundreds and seventy three (273) undergraduate students participated in a study about “reaction and decision making” in exchange for course credits (53.8% Males, $M_{\text{age}} = 21.1$ years). We used Media Labs Direct RT v2012 software package for time-sensitive experimental design. Participants first read the instructions of a “betting game” they were about to play in which they could (hypothetically) win different products (See Web Appendix C for the full text). Next, all participants answered 3 questions that confirmed they understood the rules of the game. The game consisted of 15 consecutive trials of coin toss bets. In each trail, participants first saw pictures of a pair of products in the main area of the screen. For some pairs, one product was clearly preferred over the other (e.g. an iPad vs. a pocket calculator or a Flat-screen TV vs. TV stand) while for other pairs of products the preferences were more likely to be subjective (e.g. a Beach vs. a Ski vacation or a MAC vs. a PC laptop). Participants had 5000 ms to examine the products whereupon one of them had been highlighted. The highlighted product was the product they were betting on in which they did by selecting a Heads or a Tails to be associated with winning this product in an upcoming coin-toss. As before, not winning the highlighted products means winning the alternative one. Importantly, a product was highlighted only for 1000 ms before the page automatically advanced to the next trail. In other words, participants were afforded a one second response window to indicate their

Heads or Tails selection. The goal of the game was to collect as many points as possible according to the following scheme: 2 points for winning the highlighted product (i.e. participant's selection correctly match an upcoming coin-toss outcome), 1 point for winning the other product (i.e., no match – winning the alternative product) and no points for not registering a coin face selection within the allotted time. To facilitate a timely response, participants registered their bets using the 'A' and 'L' keys. The key assignments were counterbalanced so that half of the participants were instructed to use 'A' for Heads and 'L' for Tails and the other half were instructed to use 'A' for Tails and 'L' for Heads. Additionally, to help participants visualize which key should be used for Heads and Tails, images of both coin-faces were shown at the bottom of the screen ordered (right/left or left/right) in accordance with the key stroke instructions (see screenshot of an example bet in Web Appendix C). A new trial appeared immediately after a participant registered her selection or the betting time has expired. No choice could be made during the product examination period and before a product had been highlighted. Before participants could advance to the actual game, they engaged in 5 practice trials: one untimed and four timed trials using different product pairs than those in the actual game. In the second part of the study we measured participants' preferences for the different products. After completing all 15 trials, participant saw the same pairs of products one more time and were asked to "Select the product that is appealing to you more." Basic demographics were collected from an unrelated study.

Results

Eighty six participants failed to answer correctly all 3 comprehension questions and were excluded from the following analysis⁶. The rest of the 190 participants provided 2787 observations (i.e., bets) after excluding 50 trials (1.7%) in which no bet have been registered within the 1000 ms allotted time. We found the same pattern as in the previous experiments: When participants bet on a products they preferred over the alternatives, their choices of Heads differed significantly from a chance level (1124 Heads vs. 773 Tails, $\chi^2[1] = 64.57, p < .001$), but when they bet on products they liked less, they were indifferent between selecting either Heads or Tails (469 Heads vs. 421 Tails, $\chi^2[1] = 2.48, p = .12$). We use regression analysis to control for other factors that may explain the observed difference. For each bet, we created a dummy variable *Bet on Pref* that received the value of 1 if the highlighted product was also the product the participants reported as more appealing and 0 otherwise. That is, *Bet on Pref* is a binary variable that denotes whether the focal product is the preferred one of the pair of products. If preference-prominence congruence influences our participants' choices then we expect *Bet on Prep* to be a significant predictor of selecting a Heads. We ran a Logit of participant's choice (Heads or Tails) on *Bet On Pref*, a dummy indicating the heads assignment keystroke ('A' or 'L') (*Key Assign*). In the full model we also controlled for sequential order of the trial (*Seq Order*) as well as the response time measured as time (in milliseconds) taken to hit a selection key after a product had been highlighted (*Response Time*). In the full model we also added a participants fixed effects as a control for potential repeated measure biases. We summarize the results in Table 1.1. As one can see, the results are

⁶ We expected a high rate of comprehension failure because of the relatively complicated instructions. However, including all observations in the analysis did not change the results.

robust to the model used and therefore we will only discuss the full model results. It took significantly less time for participants to bet on Heads than TAILS ($\beta = .0009$, $Z = -2.23$, $p = .02$), confirming the prominence nature of this label. Importantly, whether participants bet on the preferred product or not significantly predicted Heads selection ($\beta = .35$, $Z = 3.66$, $p < .001$). When participants bet on products that were more appealing to them, they were also more likely to bet a Heads.

Discussion

In Experiment 3, participants who were asked to assign a coin-face outcome (Heads or Tails) to one of two possible rewards in each of 20 consecutive choices exhibited the same congruence observed in the previous experiments. When betting on their preferred product, participants favored the prominently labeled outcome (Heads). However, when betting on their less preferred product, their choice between Heads and Tails was normatively unbiased. Importantly, the direction of assignment seems to not matter for the observed congruence. That is, choosing a label (Experiment 3) rather than a product (Experiments 1-2), yields similar results. Moreover, we replicated our results even when participants had only one second to submit their choice, supporting the intuitive nature of the preference-prominence congruence.

Experiment 4

The reported experiments thus far used a variety of products to elicit preference, but a single label prominence stimulus (e.g., Heads vs. Tails). In spite of Heads being a well-studied prominent label (Schelling 1960; Colman 2003; Mehta et al. 1994), our

conceptual account predicts similar congruence effects with other prominent labels.

Using other labels can further help generalize our findings by showing that the observed congruence is more than “just” a “Heads Effect”. We test this in Experiment 4 using two other prominent labels: Even vs. Odd (Hines 1990, Nuerk, Iversen & Willems 2004; lochy et al. 2000; Kinoshita & Peek-O’Leary 2006) and card ranks.

Design

One hundred and forty online participants were recruited through Amazon Mechanical Turk (63% males, $M_{\text{age}} = 31$ years). Participants were presented with two game scenarios. Each game offered a pair of possible rewards with one clearly preferred over the other. Participants could hypothetically win one of the two rewards with equal probability. In the first game, the rewards were either \$1 or \$5, and before picking a card from a non-standard deck participants had to decide which card’s rank would win them each reward (e.g., label assignment). In the second game, the rewards were either an iPad or a pocket calculator, and before rolling a standard die participants had to decide which reward would they win on an Even roll and which on an Odd one (e.g., reward assignment). Note that we had participants assign a label on the first game and a reward on the second game to test both directions. For example, in the first game, participants read the following description:

A game allows you to win either \$1 or \$5. The amount you will win depends on the card that you will draw from a well shuffled but a non-standard pack: The pack contains only eight cards. Four of them are King-of-Spades and the other four are Three-of-Spades. Here is how the game is played: Before drawing a card, you decide which card will win you a \$1 and which card will win you a \$5. Then, you draw ONE card from the pack and win the amount represented by the drawn card. Please select the card that will win you a \$1 [\$5] :(this also means the other card will win you a \$5 [\$1]).

Participants saw pictures of \$1 and \$5 bills at the top of the screen and pictures of both King-of-Spades and Three-of-Spades below the text. They had to select one of the cards, and their order was counterbalanced across participants. Also, we manipulated the assignment to the \$1 prize vs. to the \$5 prize between participants. Similarly, on the page describing the second game, participant saw a picture of a standard 6-sided die at the top of the screen and pictures of both an iPad and a pocket calculator below the text, and had to select one of the products (display order counterbalanced). Again, assignment to an Odd outcome vs. to an Even outcome was manipulated between participants.

After reporting their selection in game one, participants were also asked to reflect on their previous choice by answering the following questions: i) “How good do you feel about your choice?” ii) “As a gut-level feeling, how likely are you to win?” iii) “Imagine you drew the card that wins a \$5. How good would you feel?” iv) “Imagine you drew the card that wins a \$1. How bad would you feel?” Participants reported their answers by dragging labeled analog horizontal bars from left to right. For example, the left side of the bar in the first question was labeled “Not so good” while the right side was labeled “Extremely good”. Participants were asked to reflect on their choice only in the first game scenario. We concluded the survey with a basic demographic questionnaire.

Results

Two participants failed to correctly answer an attention check question and were removed from the analysis.

Game one: Among those who were asked to choose the card that would win the \$1 prize, 30 participants selected the Three-of-Spades card and 38 participants selected the King-of-Spades card— a choice distribution not different from chance ($\chi^2[1] = .72, p = .4$). However, choosing a card for the \$5 prize, only 18 participants selected Three-of-Spades, while 52 selected King-of-Spades, which differed significantly from chance ($\chi^2[1] = 15.56, p < .001$). Obviously, our participants favored the prominent card (King-of-Spades) when asked to match a label to a preferred reward (\$5). Conversely, participants did not seem to care which label they assigned to the least preferred reward (\$1), again demonstrating the asymmetry predicted by the evaluative matching account. Finally, the choice distributions in the two framing conditions (assignment to \$1 or \$5 rewards) were significantly different ($\chi^2[1] = 11.8, p < .001$). Once again, framing the same task differently affected the way our participants chose a card.

Retrospecting on their choice, participants in the \$5 framing condition did not feel significantly better about their choice than those in the \$1 framing condition ($M_{\$5} = 66.74, M_{\$1} = 62.96, t[136] = 1.03, p = .3$). Noticeably however, participants who assigned King-of-Spades indicated feeling better about their choice than those who assigned Three-of-Spades ($M_{\text{King}} = 68.15, M_{\text{Three}} = 60.07, t[136] = 2.17, p = .031$), but whether participants assigned the card to a \$1 or to a \$5 prize did not qualify these results ($F[1,134] = .53, p = .46$). This result lends support to the positive affect elicited by the prominent cue. Neither the framing manipulation ($M_{\$5} = 52.33, M_{\$1} = 51.83, t[136] = .14, p = .88$), nor participants' card assignments ($M_{\$5} = 53.14, M_{\$1} = 50.53, t[136] = .79, p = .43$) significantly influenced their gut feeling about their winning likelihood. Finally, participants felt similarly good or bad winning the \$5 or \$1 reward respectively,

regardless of the framing manipulation ($M_{\$5} = 84.44$, $M_{\$1} = 82.23$, $t[136] = .66$, $p = .5$; $M_{\$5} = 30.28$, $M_{\$1} = 31.07$, $t[136] = .19$, $p = .84$) or their card's assignment ($M_{\text{king}} = 82.95$, $M_{\text{Three}} = 83.92$, $t[136] = .28$, $p = .78$; $M_{\text{King}} = 29.83$, $M_{\text{Three}} = 31.91$, $t[136] = .49$, $p = .62$)

Game Two: Among those selected a winning reward for the case of an Odd die-roll, 40 participants selected the iPad and 30 selected the pocket calculator. This choice distribution did not differ from chance ($\chi^2[1] = 1.15$, $p = .28$). However, when asked to select a winning reward for an Even die-roll, only 12 participants selected the calculator while 56 selected the iPad, a choice distribution significantly different from chance ($\chi^2[1] = 27.19$, $p < .001$). Once again, our participants favored the preferred reward (iPad) when they had to assign it to the more prominent labeled die-roll outcome (Even), but they chose a reward randomly when the task was framed with the non-prominent labeled outcome (Odd). Finally, comparing the choice distributions in the two framing conditions (i.e. assigning a reward to an Even or an Odd outcome) revealed a significant difference ($\chi^2[1] = 9.20$, $p = .002$).

Discussion

The current results further support the asymmetric congruence between preference and prominence. The findings of Experiment 4 verify that there is nothing particularly special about the Heads label, and that we find the same effect using other prominently labeled cues. Importantly, Experiment 4 provides more support for both the bi-directional nature of the congruence and for its asymmetric property. None of the

additional self-reported measures bear any explanatory power on these findings: Participants in the first game felt better about their choice when they assigned the prominent label. However, this pattern did not vary with the reward preference level, and thus suggests that it is less likely that the congruence we find is caused by awareness to a positive emotion. Moreover, neither gut feeling about the chances of winning (a subjective probability measure), nor the expectation of a positive or negative feeling of winning the high or low value prize, respectively, could explain our participants biased choices.

Experiment 5

Thus far we have demonstrated that people intuitively associate prominently labeled cues with their preferred choices and that this congruence effect is asymmetric. As discussed earlier, we propose an evaluative matching process between two positive affective cues: The more fluent prominent cues and the preferred outcome. But if there is indeed an intuitive link between preference and fluency, then we would expect people to associate their preferred choices with the more fluent cues regardless of their prominence. We designed experiment 5 to further explore the link between preference and fluency by manipulating fluency directly instead of manipulating prominence. In four different experimental settings, subjects assigned a pair of choices, one preferred over the other, to either fluent or dis-fluent cues. Our theorizing predicts that subjects would be inclined to assign their most preferred alternative to the fluent rather than to the dis-fluent cue.

Design

Online participants were presented with a pair of alternatives and a pairs of stimuli and were asked to assign each alternative to a stimulus (or vise-versa) as they deem fit. In Experiment 5A subjects assigned pictures of \$1 and \$5 bills to either the word PECUNIARY or the word FINANCIAL, presented either in uniform or alternating case form (i.e., PECUNIARY vs. pEcUnIaRy) (Whittlesea & Leboe 2002; Mueller, Tauber & Dunlosky 2012). In Experiment 5B subjects assigned DVD pictures of the movies *Superbabies: Baby Geniuses 2* and *Forrest Gump* to the words CINEMA and THEATER, presented in either uniform or alternating case form. In both Experiments the stimuli words were counterbalanced such that half of the participants saw the first stimulus words in its fluent form (uniform) and the second word in its disfluent form (alternating case), while the other half saw the first stimulus word in its disfluent form and the second word in its fluent form. In Experiment 5C subjects saw pictures of two differently designed laptops (a gaming laptop and a business laptop) as well as a pair of laptop descriptions. Importantly, the description texts were presented in either easy-to-read font (e.g., dark font color, 1.4em line height) or difficult-to-read font (e.g., light grayscale font color, 1em line height) (Alter & Oppenheimer 2008; Alter et al. 2007; Novemsky, Dhar, Schwarz, & Simonson 2007; Simmons & Nelson 2006b). We counterbalanced the fluency manipulations of the laptop description such that for half of the participants the first description was fluent and the second was disfluent and for the other half it was the opposite. After participants assigned the descriptions to laptop pictures, they reported which of the two laptops was more appealing to them. Finally, In Experiment 5D, participants were told to imagine that they were betting on horses at the racetrack and noticed that most of their friends are betting on two horses that had similar

odds: NAFPLIOTIS (a disfluent name⁷) and HARMONY (a fluent name) (Laham, Kovel & Alter 2012; Alter & Oppenheimer 2006). Participants were asked to split a total of \$6 bet, a \$1 and a \$5 bills, between the two horses as they deem fit. The order of the horses' names was counterbalanced. See Web Appendix D for full text and examples of Experiment 5 conditions.

Results

Four hundred and one people from Amazon Mechanical Turk (68% males, $M_{\text{age}} = 30.3$ years) participated in Experiments 5A and 5B. The two questions were presented in a random order so that some completed Experiment 5A first while other completed Experiment 5B first. In Experiment 5A, 233 individuals assigned the \$5 bill to a word that was presented in its fluent form (either PECUNIARY or FINANCIAL) and 168 assigned the \$5 bill to a word that was presented in its disfluent form (pEcUnIaRy or fInAnCiAl). A one-tailed test of proportion suggests that this distribution differs significantly from chance ($\chi^2[1] = 10.21, p < .001$). Similarly, in Experiment 5B, 219 individuals assigned the movie Forrest Gump (the preferred movie) to a word that was presented in its fluent form (either CINEMA or THEATER) and 182 assigned this movie to a word that was presented in its disfluent form (cInEmA or tHeAtEr), a behavior pattern that differs significantly from a random assignment ($\chi^2[1] = 3.23, p = .036$). One hundred and ninety four online subjects participated in Experiment 5C (67% males, $M_{\text{age}} = 29.9$ years). Of those participating, 164 assigned the easy-to-read laptop

⁷ We adapted the name Nafpliotis as a disfluent (difficult-to-pronounce) stimulus from Laham, Kovel & Alter 2012, Study 1.

description to the laptop they reported to be more appealing to them. The rest (78 subjects) assigned the difficult-to-read description to their preferred laptop and this distribution differs significantly from chance ($\chi^2[1] = 7.05, p = .004$). Finally, four hundred and two online subjects participated in Experiment 5D (67% males, $M_{\text{age}} = 32.6$ years). Conceptually replicating Alter and Oppenheimer (2006) stock name results, 220 individuals bet their \$5 on HARMONY, the horse with the more fluent name, while the rest 182 bet their \$5 on NAFPLIOTIS, and this distribution, again, significantly differs from chance ($\chi^2[1] = 3.40, p = .032$).

Discussion

The results of Experiment 5 suggest that the preference-prominence congruence may be rooted in a deeper intuitive link between preference and fluency. Participants were inclined to associate a higher amount of money or their preferred product (movie or laptop) to the higher processing fluency stimulus, across different manipulations of perceptual fluency (Uniform vs. alternate case fonts, clear vs. light grayscale fonts, and easy vs. hard to pronounce names). These results extend our knowledge of the cognitive processes underlying judgments of more fluent cues: Not only are fluent stimuli judged more likable, they are also automatically associated with other likable alternatives like preferred choices. Therefore, in the absence of other reasons to prefer one alternative over another, individuals' decisions are affected by the psychological prominence of the decision labels.

GENERAL DISCUSSION

Understanding preference dynamics has been at the forefront of behavior research, and there is overwhelming evidence that choices are context dependent and can be influenced by the interaction between different types of mental processes and situational cues. We describe five experiments that explore an intuitive congruence between preference and prominence. All else equal, people intuitively relate their preferred choice to the outcome that is more prominently labeled (and vice versa). We propose a hedonic congruence explanation based on evaluative matching between the processing fluency of the label and preferences.

We construct the preference – prominence congruence hypothesis by noting converging insights from previously unrelated fields of research in psychology and judgment and decision making. First, we note that a prominently labeled cue can generate an intuitive positive affective response. This may be a result of prominently labeled cues becoming a default, or because they are more accessible and/or processed faster than other labels. That is, they are more fluent which in turn, results in a positive affective reaction. Next, we propose intuitive congruence between this positive affective reaction and that of preference. Together, these lead to the prediction that a prominently labeled cue should be intuitively associated with a preferred alternative. Moreover, this relationship should be bi-directional, as a preferred alternative should likewise be associated with a prominently labeled cue. Finally, this account does not predict the congruence between non-preferred alternative and non-prominently labeled cues, as the latter tend to be neutral rather than negatively affected. This leads to an asymmetry which

helps disentangle this mechanism from other related accounts. We tested these predictions across five experiments.

Participants in Experiment 1 played a game in which they tossed a virtual coin to determine which of two alternative rewards they would hypothetically win. Holding a focal reward constant and manipulating its alternative, we observed that given a choice, participants demonstrated a strong tendency to assign the focal reward to the prominently labeled coin-toss outcome (Heads), but only when the reward was preferred over the alternative. In Experiment 2, we explored the asymmetry property of the effect in which framing the same task to a Tails outcome assignment (a non-prominent label), did not yield the reverse result. Rather, choice proportions in the non-prominent cue frame converged to the normative chance probabilities: When facing a non-prominent label, participants accounted for the equal probability of the game outcomes and assigned the rewards accordingly. These results were incompatible with a simple rank-matching alternative account. In other words, the effect is not simply mirrored when taking the inverse of its components, as would be predicted by rank-matching. Additionally, participants in the Heads frame condition, compared to those in the Tails frame condition, did not feel more control over the winning outcome, nor did they feel they had a better chance of winning their preferred reward.

Experiment 3 explored two essential properties of our congruence account. First, congruence should not have a specific direction and therefore we expect the effect to hold regardless of whether participants assign a preferred choice to a prominently labeled cue or vice-versa. Second, congruence between preferred choices and prominently labeled cues arises from intuitive reactions, thus imposing a time constraint on decision makers

should not change our findings. Participants in Experiment 3 performed 20 consecutive choice tasks of a coin-toss game, but in each task participants had only one second to assign either a Heads or a Tails to a randomly selected reward. Following the assignment tasks, participants also indicated their preferred reward in each pair previously presented to them. The results were consistent with our hypothesis: A task that involved assignment of a label (instead of a reward) yielded the same congruence as that of assignment of reward (to a label). Importantly, we also replicated our previous results even when participants had only one second to submit their choice, suggesting that the preference-prominence link is indeed rooted in intuitive processing, and does not require long deliberate thinking. Experiment 4 extended our findings to other prominent labels (i.e., Even vs. Odd numbers and card ranks), and helped generalize our account beyond coin-flips. Finally, Experiment 5 explored our hypothesis that fluency stands at the base of the observed effect and tests directly the relation between preferred choice and fluent stimuli. Specifically, by manipulating fluency directly instead of stimuli prominence, we found a deeper link between preference and fluency: people intuitively associated their preferred choices to the stimuli that offered a higher perceptual fluency.

Not all prominent cues generate a positively valenced response. For example, a red traffic light may be intuitively associated with other emotions, the conspicuous Swastika symbol (adopted by the Nazi Party) denotes auspiciousness in Hinduism and other related religions but has strong negative association in the western world, and the number 666, although being salient and fluent by its symmetrical structure, may elicit a strong negative reaction as it represents the "sign of the devil" for some individuals. The effect we document may be limited to prominent labels that generate a positive affective

response, and may not generalize to every other form of prominence. We also relied on research in linguistics suggesting that the unmarked ends of marked dimensions usually represent the positive end of the spectrum (Klatzky, Clark & Macken 1973). We suspect that our results would generalize to other such unmarked labels, though we only tested three instances in this paper. We caution, though, that there is likely to be cultural or language induced heterogeneity in what might be perceived as prominent. For example, some numerical values carry very differently valenced associations in different cultures or religions (the number 13, for example). Furthermore, as the root of the observed congruence lies in an intuitive affective response it may be drowned by stronger responses to a task, or overridden by strong System 2 controls. In such cases, we do not expect to observe this congruence. We do not expect the preference-prominence congruence to always have the upper hand. Our ability to speak to those is limited by the scope of the current investigation.

While the existing literature and the collected evidence point in the direction of an affect-based evaluative matching mechanism, we cannot fully reject all possible alternative accounts. Our evidence does, however, decrease the likelihood that the observed congruence is caused by a simple rank order matching (Exp. 2, 3, 4), distorted subjective probabilities (Exp. 2, 4), or even a biased sense of control (Exp. 2). Nevertheless, other mechanisms could also play a role in conjunction with the one proposed here and more research into the preference-prominence congruence effect is warranted. Finally, it is likely that there may be boundary conditions we did not investigate, such as high levels of expertise or when the cue itself carries negative associations or affect (e.g., a 6-sided die to one who morally opposes gambling).

Our contribution to the existing literature is twofold. At the conceptual level the current work adds to the growing body of knowledge regarding the nature of observed preferences. Demonstrating that people intuitively associate preferences and prominence helps explain why people tend to favor some choices, even in the absence of explicit relevant information or reasons. It is possible that in many cases people intuitively respond to a prominently labeled cue and choose accordingly. In that respect, prominently labeled cues add to the growing list of properties of the context that are worth noting when interpreting contextual influences on decision making.

At the practical level, the current account offers researchers and practitioners additional tool that would help them measure intuitive preferences which might even be less susceptible to biases, particularly, those biases created by deliberate cognitive processes. For example, when there is a known potential bias, such as social desirability, and a researcher is interested in identifying the intuitive preference, the preference-prominence congruent may come in handy. In addition, one may use our findings to better design contextual cues. For example, a shiny yellow sales sign may be more effective if positioned in a way that allows for an easy association with its target product. Finally, there may be counterintuitive effects whereby one may be better served writing an article and having prominent ads with it, as the article might benefit from the prominence-preference association. This latter part requires the article (or neutral cue) to have not yet been evaluated, something that was beyond the scope of the current investigation.

We conclude with an extension to the lay belief that a coin-flip may help resolve tough decisions because when the coin is in the air, one suddenly realizes what one is

hoping for. Our evidence suggests that flipping the coin may not even be necessary as one should simply follow the option he or she assigned to the Heads outcome to begin with. Most likely, what one assigned to Heads is one's preferred choice.

Table 1.1. Experiment 3 Logit Model Results

Predictor	β Basic	β Controlled	β Full model
<i>Intercept</i>	.06 (.07)	.62 (.25)	.09 (.59)
<i>Bet on Pref</i>	.27** (.08)	.26** (.08)	.35*** (.09)
<i>Key Assign</i>	.12 (.08)	.12 (.08)	.92 (.76)
<i>Seq Order</i>		-.008 (.009)	-.008 (.001)
<i>Response Time</i>		-.001*** (.0003)	-.0009* (.0004)
<i>Subject Fixed Effects</i>			✓
AIC	3799.2	3790.2	3536.2

NOTES. Standard errors are presented in parentheses below parameter estimates. Significant code: *** $p < .001$; ** $p < .01$
* $p < .05$.

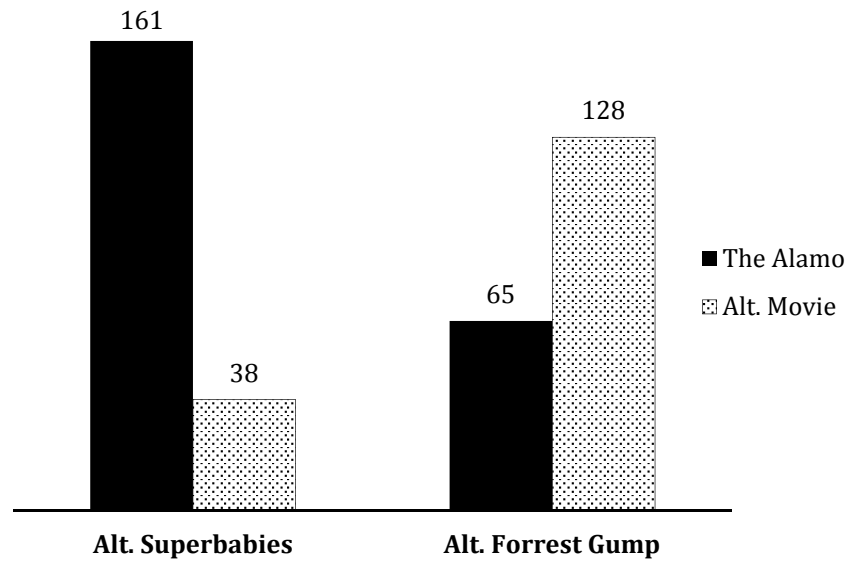


Figure 1.1. Experiment 1 - Number of participants assigning a movie to a heads

APPENDIX

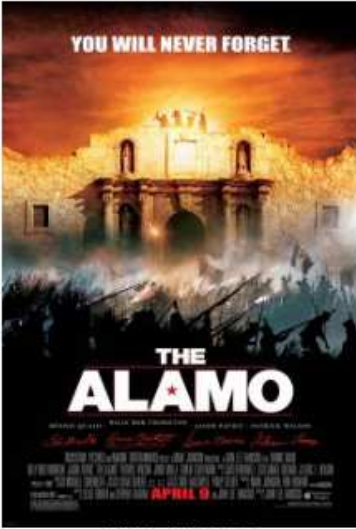
Appendix A

Experiment 1 screenshot:

In the case of **HEAD**, which movie will you win?

Here you decide which movie you will win as a result of the coin flipping outcome.

Select the button under the movie that you want to win in the case of HEAD.
In the case of TAIL, you will win the movie that appears above the unselected button.




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Appendix C

Experiment 3 Instructions:

In this study, you are about to play a game that involves flipping a coin.

In this game, you will be provided with a sequence of bets in which you can (hypothetically) win some products.

Please consider your answers as if you were facing these decisions in your real life.

Please - carefully read the instructions on the next pages.

Later, you will be asked questions to confirm your understanding.

Rules of the game:

In the game, you will make 15 consecutive coin-tosses and place bets on each one. For each toss, you are betting on one of two products and are guaranteed to 'win' one. However, which product you win depends on your bet. ONE of the products will be highlighted a few seconds after the page is loaded, this will be the product that you are betting on.

You will have to decide whether HEADS or TAILS will win you this product. If the coin lands on the opposite face of what you selected, you will win the other product (the object that was not highlighted). When betting, imagine these are real bets and that you will actually win the products.

You will only have one second (!) to submit your bet (HEADS or TAILS) after which the page will automatically advance to the next bet. In other words, the product you are betting on will only be highlighted for one second before the page advances. To submit your bet, press the 'A' ['L'] key to select HEADS and the 'L' ['A'] key to select TAILS. If you select heads for a product and then the computer simulated coin flip selects heads, you will win the product (and get two points)! If you select heads and the computer simulated coin flip selects tails, then you will win the non-highlighted product (and get one point). If you don't place a bet, you will not win a product (and get 0 points).

Collecting points:

Your goal is to collect as many points as possible. After collecting the bets, we will execute 15 actual coin tosses (using the computer simulator), one for each question, and then compare the outcomes to your bets.

You will get 2 points for each bet that won you the product you were betting on (i.e., your Heads/Tails selection for the highlighted product matches our coin-toss outcome). You will get 1 point for each bet that won you the other product (e.g., you bet Heads and we got Tails). You will get 0 points if you did not register a bet (i.e. select Heads of Tails) and the page advanced to the next bet with no selection.

Your score will be the total points you earned in all of your bets. You can earn a maximum of at most 30 points. To achieve a high score, it is in your best interest to register all of the bets within the allocated time (because you will receive 0 points for failing to select heads/ tails on a bet).

Note: It is not possible to bet before one of the products is highlighted.

Experiment 3 example bet:



Appendix D


Experiment 5 Screenshots

Experiment 5A

(The order of the items, as well as the fluency manipulation of the categories name were counterbalanced)

Sort the items on the left into the categories on the right as you deem fit.
(Drag-and-drop one item into each box).

Items

	pEcUnlaRy	FINANCIAL
		

Experiment 5B Screenshot

(The order of the items, as well as the fluency manipulation of the categories name were counterbalanced)

Relate the items on the left to the categories on the right as you deem fit.

(Drag-and-drop one item into each box).

Items



CINEMA

tHeAtEr

Experiment 5C Screenshot

(The orders of the laptop pictures and the fluency manipulation of the laptop descriptions were counterbalanced)

Please carefully read the computers' descriptions below and match them with the laptops in the pictures as you deem fit.

For each computer description, select whether it fits the laptop on the right or on the left hand side.



This laptop offers the best balance between performance and price. It provides a compelling combination of powerful performance, superior usability and affordability. It is configured with an Intel Core i7 processor, 8GB of memory, a 1TB hard disk drive and a Nvidia GeForce GT740M graphics card. It comes in 15-inch and 17-inch configurations with optional touch support and display resolutions of 1366 x 768. It offers about 10.5 hours of battery life and it comes with 18 months warranty.







This computer is the best all-around laptop available today. It is powerful, extremely functional and not ridiculously expensive. It is built with an Intel Core i7 processor, 12GB of memory, an Intel HD Graphics 4400 graphics unit and a 500GB hard disk drive. The laptop is offered in 14-inch, 15-inch and 17-inch screen configurations and 1920 x 1080 display resolutions. The battery will keep it working for 9.5 hours and it comes with 12 months manufacturer warranty.



Experiment 5D Screenshot

(The orders of the horses' names and the dollar bills were counterbalanced)

Imagine that you are with a group of friends betting on horses at the racetrack. You don't know much about the competing horses but you noticed that most of your friends are betting on two horses: Nafpliotis and Harmony. Both horses have similar odds. You decided to spend \$6 and bet on both horses, \$5 on one horse and \$1 on the other. Please divide your \$6 between the two horses below as you deem fit.

NAFPLIOTIS		HARMONY
		
<input type="radio"/>		<input type="radio"/>
<input type="radio"/>		<input type="radio"/>

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Chapter 1, in full, is currently being prepared for submission for publication of the material. Coby Morvinski and On Amir. The dissertation author was the primary investigator and author of this paper.

Chapter 2.

THE EFFECT OF STATED PREFERENCE ON SUBSEQUENT REVEALED
PREFERENCE

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ABSTRACT

In a series of four experiments involving consequential decisions, we demonstrate that the mere act of stating one's preference in writing may bias subsequent behavior and the preferences that behavior reveals. We survey the literature and challenge two contradicting predictions about the direction of the effect. Our findings suggest that consistency with previous judgments, and not greed, plays a central role in biasing observed preference. Individuals who stated their desired compensation for a task they had just performed, committed to a much higher compensation than those who had not done so. These findings have direct implications for theory and practice concerning preference measurement in research, markets, and policy making.

INTRODUCTION

Some evidence suggests that more than 40% of the privately held companies lack formal compensation philosophies (e.g., compensation resources survey), which means many owners' compensations determined arbitrarily. Consider the following scenario. A business owner needs to decide how much money to get paid at the end of the year. In one case, before taking the money, the owner states in writing how much money she feels should be paid. In another case, she makes no such statement before taking the money. In which of these scenarios will the business owner pay herself a higher wage? And more generally, how do individuals behave in situations that allow them to determine their own wages? Does a statement of one's willingness to accept valuation affect subsequent behavior?

In the current paper, we explore the potential instability of an individual's preference. We focus on situations that are characterized by full information, and investigate the relation between individual's stated preference regarding compensation after exerting effort on a task, and the subsequent behavior that reveals the individual's preference. Our main research question is whether initially stating one's preference affects the subsequent revealed preference, and if so, in which direction? We conduct a horse-race test between predominant theories that predict opposite outcomes. In a series of four experiments in which subjects need determine their payment for working on a task, we explore how a non-binding ex-post valuation statement influences an individual's subsequent behavior. We show that stated preference, rather than being merely a biased measure of one's "true" preference, may play an important role in the decisional process by affecting the subsequent accepted value. Moreover, if a revealed

preference is susceptible to one's stock of decisional precedents, we further ask whether such precedents can be identified and what benefits can be derived from such recognition. Our findings suggest stated-preference instability exists even under full information, and although true preferences may be rooted in people's behavior, as posited by the theory of revealed preferences, a less stable initial judgment can influence this behavior: their stated preferences. We speculate that the susceptibility of an economically committed accepted value casts doubt on the ability to infer stable preferences from one's behavior in the presence of a preceding-stated preference procedure. Importantly, we show that whether or not individuals state their valuations in writing before taking the money implicitly affects their final payoff.

Theoretical Background

Standard economic theory predicts that, in an analogous context, an individual would maximize her profit by choosing the highest compensation possible within the feasible range. Yet a large body of evidence suggests factors other than monetary profit affect individuals' choices. For example, other-regarding preferences such as fairness (Charness and Rabin, 2000), inequity aversion (Fehr and Schmidt, 2000), or reciprocity (Dufwenberg, Kirchsteiger, 2004; Falk and Fischbacher, 2006) may affect individuals' behavior. In addition, research shows that individuals do not always share the same fairness norms. Indeed, fairness considerations are often context dependent and may be subject to self-serving biases (see, e.g., Babcock & Loewenstein, 1997; Konow, 2000). These theories predict individuals' may not choose the highest possible compensation, even if presented with the option. Although standard economic as well as other-regarding

preference theories differ in their prediction of the final compensation that will be selected, they do not distinguish between the two scenarios presented above. In fact, behavioral science offers further evidence that the two scenarios may lead to different outcomes. However, the outcomes could be in line with two opposite predictions. On the one hand, the presence of money may invoke selfish or greedy behavior, which may be stronger when money is taken directly, in the absence of previously stated valuation. Some evidence indicates money causes cold behavior. For example, individuals who were reminded of money were less inclined to help other people, and physically distanced themselves from others (Vohs, Mead, & Goode, 2006, 2008). In addition, reminders of money cause individuals to cheat more frequently (Gino & Pierce, 2009) and engage more in behavior that could be harmful to other people (Reutner & Wanke, 2013). In sum, money causes coldhearted behavior (Reutner & Hansen, 2015), which predicts that individuals in the first scenario will take a higher amount.

On the other hand, individuals may overstate their desired compensation (e.g., cheap talk), which consequently inflates the actual amount they take. This prediction is based on evidence from two distinct streams of research: (1) a discrepancy exists between individuals' revealed-preferences, preferences that are directly inferred from behavior, and stated-preferences; and (2) people desire to maintain consistency. We discuss both literatures below.

Measuring consumers' preferences is central to decision science. Although researchers have developed a large number of techniques to estimate individuals' willingness-to-accept (WTA) and willingness-to-pay (WTP), broadly speaking, such techniques fall into two main categories. Revealed-preference (RP) infers individuals'

preferences from observation of consumer behavior or the actual choices made (Samuelson 1948). Stated-preference (SP) refers to a class of widely used survey techniques designed to elicit individual monetary valuation of a good (for a recent review, see Ben Akiva, McFadden & Train, 2015). Numerous behavioral studies employ either RP or SP techniques to estimate individuals' preferences, building on the idea that both elicitation protocols correctly represent an individual's true preference. Based on this idea, eliciting a desired compensation via stated preferences or observe action of compensation choice (revealed-preferences) should yield the same result. However, research has revealed the presence of a systematic bias between intentions and actions in preference elicitation. Particularly, much of this literature suggests people tend to overstate their actual value in hypothetical situations, which is often referred to as hypothetical bias (e.g., List & Gallet, 2001; Murphy et al., 2005). Hypothetical bias is shown to be robust across a wide array of valuation-elicitation producers, though results are inconclusive as to whether and to what extent different factors influence the disparities between words and deeds. Further, some findings show WTA usually yields larger hypothetical bias than WTP (List & Gallet, 2001). Ironically, most of the work in this field has been done on WTP and much less has been done in the area of demanded compensation (WTA).¹ This body of research would predict that individuals would *state* a higher desired compensation than they would take when asked to take if they did not have opportunity to state their preferences. Notably, behavioral scientists commonly

¹ For example, List & Gallet's (2001) meta-analysis of hypothetical bias included 29 experimental studies of which only eight are on WTA. Murphy et al. (2005) were even more conservative and excluded any WTA study from their meta-analysis by claiming they did not have enough observations to allow conclusive statistical conclusions.

accept that both SP and RP estimations of individuals' preferences suffer from a major potential shortfall: their outcome is susceptible to normatively irrelevant factors such as bounded rationality, emotions, social norms, and more. Further, research on preferences for constancy (Cialdini, 1987, Festinger, 1957; Heider, 1946; Newcomb, 1953) suggest that people have a desire to behave consistently with previous decisions or actions. For example, people were more likely to vote if they were asked whether they plan to vote on election-day eve (Greenwald et al., 1987). Accordantly, Falk and Zimmermann (2015, under review) demonstrated in a recent study that subjects who were asked to provide an initial estimation of the number of peas in a bowl were more likely to neglect valuable information in their final estimation than those who were not asked for an initial estimate. Consistency theory predicts that an overstated amount should inflate the actual amount of money an individual will take, because she will behave consistently with her previous valuation. Together, these postulations predict individuals in the second scenario, who first stated their desired wage, will take a higher amount.

MATERIALS AND METHODS

Experimental design

We explore these competing hypotheses in a series of experiments involving consequential behavior in which individuals are asked to determine their own wages after working on a task. In one case, individuals directly take their compensation after performing the task. In particular, they are allowed to take as much money as they want from a bowl containing \$10 in quarters. We consider this measure of RP (actual compensation taken) that is free from any preceding SP the baseline treatment. Our

experimental design allows us to examine the validity of this assumption. In the other case, we ask individuals to state their desired wage before they take their compensation from the bowl. In both cases, participants make the decision in private. We use an ex-post elicitation procedure (after participants exert an effort) to avoid uncertainty endogeneity, which is inherent in many preference-elicitation studies. That is, respondents may find it impossible to state their monetary valuations to get or rid of a good with which she has very little to no experience. In that respect, measuring WTA after participants have already worked on a task offers us a clean experimental working environment. We use open-ended SP questions that usually refer to a simpler version of other elicitation protocols, such as sealed-bid and Vickery auctions (as opposed to closed-end or binary-choice responses). Finally, we are aware of no study that explores potential influences of SP on RP beyond the expected correlational relationship between the two measures of preference. Our study takes an additional step by showing random shifts in an individual's SP can directly influence RP evaluation. As our investigation reveals, in some conditions, SP elicitation may influence subsequent RP evaluation, which is at odds not only with standard economic theory but also with the widely used assumption that actual accepted values are always true.

Next, we describe four experiments that demonstrate the effect. Experiment 1 demonstrates that individuals who state their WTA (desired wage) in an open-ended question *before* taking their money, take significantly more money than those who do not state their WTA but are rather simply asked to take the money. In Experiment 2, we show people are not cognizant of the effect, and even those who select to state their desired compensation state it for reasons other than payoff maximization. Experiment 3

demonstrates how a more concrete SP procedure (i.e., visualizing real money) reconciles the effect. Further, the results of Experiment 3 reinforce the argument that RP is more likely to be aligned with one's true preferences in the absence of a preceding SP procedure. Finally, Experiment 4 refutes potential alternative accounts for the observed bias and shows some of the boundary conditions. The paper concludes with a summary and a general discussion.

Experiment 1

Participants in a laboratory experiment completed a short listening task for a small wage, followed by a similar but longer task. The initial task served as a “calibrator” to reduce variability in the perceived difficulty of the task and anchor all participants in the same reference wage. We then gave subjects the choice to freely select their own wages for completing the longer task. Upon completion of the second task, and on the way out of the lab, the experimenter led participants to a payment room where they could privately collect payment from a bowl containing \$10 in quarters. After each participant left the lab, the experimenter counted how much money was left in the bowl, in order to measure how much money the participant took.² Our main manipulation allowed some participants to write down in an open-ended manner how much money they believed they should receive for completing the longer task (treatment Write). Subjects in this treatment wrote down their WTA immediately after completing the task and before learning about the chance to collect money from the payment room. Subjects in the Baseline treatment

² In fact, a new bowl with \$10 immediately replaced the old one, which was tagged with the participant ID and taken to an undisclosed “counting room.”

were not asked to state any amount, but were rather asked to take the money from the bowl. Importantly, in both treatments, we measured SP and RP upon completion of the tasks (ex post). All the participants collected the money in private regardless of the treatment. Therefore, all else equal, any differences between the treatment groups in the amounts taken should be attributed to the SP manipulation.

Design

Eighty-two undergraduate students ($M_{\text{age}} = 21.6$, 58.5% Females) participated in Experiment 1 for course credit. As part of a longer lab session, participants performed two listening tasks. As part of the tasks, participants had to count the number of ringing-bell sounds embedded in a white noise (see appendix A for the complete instructions). In the first task, participants performed the task for 67 seconds. The instructions informed them they would receive \$.25 for completing the first task. The task was followed by a similar but a longer task that lasted 285 seconds. When introduced to the second task, participants were informed they would be compensated for it at the end of the experiment. Subjects did not know about the possibility of choosing their own compensation at that stage.

RP was measured upon completion of the second task. At the end of the task, the experimenter led subjects, one by one, to a payment room located on the way out of the lab. The payment room was a small office containing only a table with a bowl. The bowl was filled with \$10 in quarters. A note placed on the table read: “Please take \$.25 for the first listening task, plus what you believe you should be paid for the second listening task” (emphasis in original). Therefore, participants could take as much as they wanted

(up to \$10) from the bowl, and their decision was private. In a between-subjects design, participants in the Baseline treatment performed the task and took their payment on their way out as described above. Participants in the Write treatment completed an additional SP question immediately after they completed the second task and before they were directed to the payment room. In particular, these participants were asked to indicate how much they “believed [they] should be paid for the second listening task.” An additional note stated the participants would receive the indicated amount on the way out of the lab (see appendix A for the exact script). No specific payment method was mentioned. After taking their payment and leaving the payment room, participants reported their total earnings from the lab session and were then dismissed. Additionally, all the participants completed a seven-item Dispositional Greed Scale (Seuntjens et al., 2015), which ostensibly was part of an unrelated study administrated about 45 minutes prior to the main study. We conjectured that dispositional greed might affect our results such that individuals who were high on the DGS scale would take more money for themselves. They completed a concluding demographic questionnaire as part of a separate study.

Results

As expected, SP was a significant predictor of RP in the Write condition regardless of gender, age, and dispositional greed, such that the higher the SP valuation was, the higher the actual accepted amount ($\beta = .41$, $t = 2.5$, $p = .017$, median regression). A pairwise Mann-Whitney test result suggests SP and RP measures come from different distributions ($Med-SP_{Write} = \$1.75$, $Med-RP_{Write} = \$1.25$, $p < .001$). That is, we observed

within-group hypothetical bias among participants in the Write condition.³ However, comparing the amount subjects took (RP) in the Baseline condition to the amount subjects indicated they deserved (SP) in the Write condition reveals no between-group hypothetical bias ($Med-SP_{Write} = \$1.5$, $Med-RP_{Baseline} = \$1.00$, Mann-Whitney test: $p = .104$).⁴

On average, participants in the Write condition took twice as much money as those in the Baseline condition ($M-RP_{Write} = \$2.59$, $M-RP_{Baseline} = \$1.28$, $p < .01$). The RP distribution was skewed, with some participants taking the maximum amount possible. A median comparison did not change our conclusions ($Med-RP_{Write} = \$1.50$, $Med-RP_{Baseline} = \$1.00$; Mann-Whitney test: $p < .001$). We report both linear and median regressions of RP on the treatment condition, DGC scale, age, and gender in Table 2.1. Age and gender did not influence the actual amount taken in this experiment. Importantly, the Write condition coefficient suggests employing an open-ended SP procedure predicts taking a higher amount, which was significant in the linear regression ($\beta = 1.14$, $t [77] = 2.55$, $p = .013$) and marginally insignificant in the median regression ($\beta = .45$, $t = 1.83$, $p = .071$).

Dispositional greed: The results of a seven-item Dispositional Greed Scale supposedly suggests individuals who are high on the DGC scale take more money ($\beta = .09$, $t = 2.29$, $p = .025$). However, a correlational relationship indicates we were unable to achieve random allocation of subjects between the treatment groups, but rather, the Write

³ The actual Median RP in the Write condition was \$1.75, but this amount included a payment of \$.25 for the first task. A large skewedness of our data suggests a non-parametric test would be more informative. However, t-test results are similar ($M-SP_{Write} = \$4.65$, $M-RP_{Write} = \$2.34$, pairwise t-test: $p < .001$).

⁴ A mean comparison suggests a significant between-group hypothetical bias ($M-SP_{Write} = \$4.4$, $M-RP_{Baseline} = \$1.28$, t-test: $p < .01$) but see previous foot note ⁴.

condition consists of greedier participants ($r_{\text{Pearson}} = .25, p = .023$). The latter finding casts doubt on the validity of the previous results. Evidently, controlling for the treatment condition in the regression results in a marginally insignificant effect of dispositional greed on the amount of money taken ($\beta = .06, t = 1.67, p = .099$).

Discussion

Experiment 1 results demonstrate a within- but not between-group hypothetical bias. Our analysis also suggests measured dispositional greed does not seem to predict actual compensation demanded, and participants who are high on the Dispositional Greed Scale do not reveal a preference for a higher wage. Importantly, Experiment 1 provides initial evidence not only for the prediction that the SP procedure may affect the subsequent RP measure, but also for the direction of the prediction: a combination of hypothetical bias and desire for consistency, rather than greed or a coldhearted state, underlie the observed bias. Simply, a written statement of the compensation demanded for a supply of labor significantly increased the consequential amount of money participants took in the Write condition. A non-binding SP led participants to take 50% more (and twice the amount on average) than what they would take otherwise. A natural question arising is whether people are conscious of the effect, because if individuals predict the effect, they may end up wealthier. In the next experiment, we ask whether, given a choice, people have correct beliefs about their behavior in the experiment and therefore choose the strategy that would maximize their wealth.

Experiment 2

Design

Ninety students who had not taken part in Experiment 1 participated in Experiment 2 ($M_{\text{age}} = 21.3$, 61% Females). Participants completed the same listening tasks as those in Experiment 1. However, we offered no real compensation in this experiment. Instead, upon completion of the second listening task, participants read a description of each of the two conditions (Write and Baseline; the order was counterbalanced) and indicated the situation they would rather be in (see Appendix B). Participants answered an open-ended question about their reasons for their choice. Finally, all the participants answered an open-ended SP question similar to the one in the previous experiment. The survey concluded with a demographic questionnaire.

Results

Experiment 2's question of interest was whether people are cognizant of the biased RP effect. Given the choice, we expect cognizant participants to prefer to write down their desired compensation, a situation that would maximize their wealth. The results of Experiment 2 suggest people are not aware of the effect: only 44 participants (48.9%) chose the Write situation, suggesting people have no particular preference for whether they should state their desired amount in advance. That is, people do not anticipate the effect of stating their desired compensation on behavior. Additionally, the majority of those who preferred the Write situation rationalized their decision using arguments that are allegedly not associated with wealth-maximization justification. In particular, the common arguments for selecting the Write situation were that it would help them stay moral, honest, less greedy, ethical, fair, more thoughtful, and less

impulsive, and would make them less tempted to take more money. Although not directly asked, several subjects who chose the Write situation predicted they would want to take more money if they rawent directly to taking the payment. Only one individual predicted that writing the deserved amount first would “gain [her] more money.” Finally, we found no significant difference in stated WTA between those who preferred to write their deserved wage and those who preferred not to ($Med-SP_{write} = \$2.00$, $Med-SP_{no-write} = \$2.00$; Mann-Whitney test: $p = .33^5$).

Discussion

Experiment 2 demonstrates that peoples’ intuition is not aligned with the situation that would lead them to maximize their earnings. Further, those who prefer to state their deserved wages first do so for reasons other than merely payoff maximization. Together, these results suggest people are not cognizant of the effect of stating their preferences in writing on subsequent behavior. Beyond the theoretical implications of our findings, Experiment 2’s results also suggest a direct practical recommendation: in many situations, people may further increase their wealth and claim a larger wage by merely stating their deserved amount even if the situation does not entail such action. In the next experiments, we explore conditions under which the observed bias exists.

Experiment 3

⁵ Five participants indicated a deserved amount that exceeded \$10 (up to \$50) and were excluded from this analysis. However, including those outliers did not change the qualification of the results ($Med-SP_{write} = \$2.75$, $Med-SP_{no-write} = \$2.00$; Mann-Whitney test: $p = .15$). Also, one participant failed to indicate a deserved amount and was excluded from the analysis.

Hypothetical bias predicts that stated rather than revealed valuation tend to be larger as evidently observed in Experiment 1. The previous results also suggest that SP valuation may affect subsequent RP valuation even when the information is fully disclosed (ex-post valuation). We argue that employing an SP procedure may construct a reference value and once such reference is set, people tend to conform to that value. This idea was previously proposed by Murphy and his coauthors who suggested that "participants might try to maintain some consistency between their hypothetical and actual values" in explaining their smaller hypothetical bias results in a within-group setting. Together, setting a higher reference valuation is expected to increase the actual amount taken. We further argue that the abstract and detached nature of simply expressing one's WTA in writing facilitates overstating one's preference. Consequently, conforming to the previous judgment results in a higher accepted compensation.

Although lacking a theoretical basis, one can still claim people undervalue the preferences they reveal only in the absence of a preceding SP procedure. In other words, undervalued RP in the Baseline but not in the Write condition, rather than the effect of an overstated SP on subsequent RP in the Write condition, underlie the observed RP discrepancy between the two conditions. We conjecture such behavior must be manifest in one's affective state. That is, undervalued individuals under pressure to take less money than they think they deserve in the Baseline condition should report more negative feelings. To test this account, we measure affective states in Experiment 3. Therefore, if the latter case holds, it should be reflected in our affect measurements. In addition, we added a new condition that employs SP protocol that is less abstract than the standard open-ended SP procedure. We expect a more concrete protocol to decrease participants'

preference overstatement and consequently weaken the observed RP bias. A reduced bias in the new condition would support our account that SP evaluation is central in affecting subsequent behavior, and would counter the alternative that the observed effect is a result of revealed-preference undervaluation in the Baseline condition.

The results of Experiment 2 suggest peoples' intuition may not be aligned with the situation that maximizes their payoff. One alternative explanation is that some people may avoid selecting the Write situation in order to maintain their well-being. In particular, they may expect to increase their committed amount, but refrained this situation because taking more money may also be associated with more negative emotions (shame, guilt, etc.). Therefore, one may assume people are aware of the effect, but some avoid the dominating strategy because of such a self-protection mechanism. The possibility that stating one's WTA conveys more negative feelings (because of taking more money) may explain the mixed results of Experiment 2 whereby some individuals avoided the SP alternative. If this account holds, we would also expect participants in the Write condition to experience more negative feelings. However, the possibility that the presence of SP does not influence one's affective state would work against the previous account.

Design

Experiment 3 (N=221) employed a similar design to that of Experiment 1 and was conducted with participants from the same subject pool ($M_{\text{age}} = 21.2$, 35.8% Females). As mentioned before, we made some adjustments to the base design. We replicated our main treatments (Baseline and Write) and explored two additional conditions: Visual

Realization (VR) and Norm. A JavaScript application in the VR condition allowed participants to state their WTA by selecting images of quarters on the screen, one by one, until reaching the desired amount. We expected those in the (more concrete) VR condition to report a lower SP evaluation than those in the original Write condition. More importantly, we also expected those individuals to end up taking less money because their committed amount was subject to a lower reference wage. In the Norm condition, participants received descriptive norm information of others' behavior. In particular, participants read, "Among other students who participated in this lab study in the previous weeks, the **median** amount claimed was **\$1.00**. That is, half of the participants believed they should have been paid \$1.00 or less and the other half believed they should have been paid \$1.00 or more" (emphasis in original). This information was true and based on some of the results of the first experiment. We added this condition in an attempt to provide additional support for the idea that a change in the reference WTA (SP) would affect subsequent behavior (RP). We expected participants in this condition to adjust their SP to the norm, which should cancel the observed effect. As stated earlier, Experiment 3 also employed affect measurements. Particularly, participants indicated on an 11-point scale (labeled "Not at all" to "Very much") the extent to which they felt each of the following: Happy, Embarrassed, Excited, Regretful, Guilty, Annoyed, Enthusiastic, Proud, and Ashamed. We measured affect twice: about 40 minutes before participants completed the listening tasks, as well as immediately after they collected the money. We used the first survey as an individual affect baseline and were interested in the differences between the later measurement and the baseline. We conducted unrelated studies between the first and the second affect measurements.

Results

We excluded from the analysis one extreme outlier observation with an SP of \$500⁶ but left three other outliers with an SP of \$100. Figure 2.1 depicts the RP distribution across conditions. We use median analysis to account for outliers in our data. We regress RP on SP, the treatment condition and their interaction, as well as basic demographics (see Appendix C for the complete regression results). As expected, stated desired compensation significantly predicted the amount of money participants took ($\beta = .08$, $t = 57.4$, $p < .001$). Importantly, the interaction between the SP and VR conditions was significant, confirming our VR manipulation by showing participants in the VR condition behaved differently than those in the Write condition ($\beta = .51$, $t = 5.75$, $p < .001$). The interaction between the SP and the Norm condition is not significant.

As Table 2.2 shows, we observe both within- and between-group hypothetical bias in all the treatment conditions (columns 7-8). Most importantly, the results of Experiment 3 replicate the effect previously observed. Participants in the Write condition took significantly more money than those who did not state their WTA ($Med-RP_{Write} = \$2.00$, $Med-RP_{Baseline} = \$1.25$, $p < .001$).⁷ Further, those in the VR condition stated a lower WTA value than those in the Write condition ($Med-SP_{VR} = \$1.25$, $Med-SP_{Write} = \$2.00$; $p = .01$), confirming that an abstract open-ended value-elicitation procedure differs

⁶ This participant was in the Norm condition. We discuss the anomaly results of the Norm condition later.

⁷ Running the rest of the analysis using the means as a central tendency yields similar results. For example, the mean difference between the Write and the Baseline conditions was even larger than the median difference ($RP_{Write} = \$2.70$, $ACPT_{Baseline} = \$1.45$, $p < .001$).

from a more concrete and visual one. Most intriguing is the significant decrease of the RP bias after we employed a more concrete SP protocol (VR condition): the accepted amounts in this condition did not differ significantly from those of the Baseline condition at a conventional level of significance ($Med-RP_{VR} = \$1.25$, $Med-RP_{Write} = \$1.25$, $p = .11$). These results support our argument that SP evaluation is central in affecting subsequent behavior, and the selected SP protocol is key. Contrary to our expectation, the norm information did not influence participants in Experiment 3. The SP of those in the Norm condition was significantly higher than the norm ($Med-SP_{Norm} = \$1.75$, $p < .001$), and consequently those individuals committed to a higher wage ($Med-RP_{Norm} = \$2.00$, $p < .001$). However, we do not have theoretical grounds for explaining these findings. We suspect participants in the Norm condition behaved disingenuously. For example, they could have suspected the experimenter was manipulating them to choose a lower amount, and reacted by selecting a higher wage than what they would have selected otherwise. Consequently, they accepted an amount that was consistent with their stated one. We will not discuss further analysis of this condition.

Emotions: For each participant, we calculated an affect-item score by subtracting the baseline affect measurement from the post-decision measurement.⁸ We also created two average scores from the negative and the positive items. Therefore, each participant received 11 affect scores (nine for each individual item, an average of negative items, and an average of positive items). We then ran an OLS regression of each affect score on RP,

⁸ Nineteen participants failed to report either the emotion or total earnings and were excluded from the affect analysis.

condition, the total amount of money participant received at the end of the lab session,⁹ as well as basic demographics. We report the complete results in Appendix C. None of the regressors significantly predicted affective states, individual affect-item or average positive or negative items. That is, experimental condition does not seem to influence participants' affective state. Somewhat unexpectedly, the results also indicate that taking more or less money from the bowl did not influence our participants' affective states (i.e., no main effect). We suspect that few individuals who took the entire 40 quarters from the bowl may differ significantly from an average subject in our pool. Excluding seven participants who took the maximum amount possible revealed RP was a significant predictor of average negative but not positive feelings ($F[1,187] = 6.54, p = .01, F[1,187] = .05, p = .83$ for average negative and positive feeling, respectively). In other words, the higher the amount of money participants took from the bowl, the more negative, rather than positive, feelings they experienced. We interpret the last result as a reality check of the validity of our affect measures. Importantly, the condition remains a non-significant factor in predicting both negative and positive feelings even in the trimmed data analysis ($F[3,187] = 1.74, p = .16, F[3,187] = 1.4, p = .24$, respectively).

Discussion

Experiment 3 reinforces the previous results in showing that within-group hypothetical bias is robust to the full-information condition, but unlike in Experiment 1, we also observed between-group hypothetical biases in all the SP conditions.

⁹ Our study was the last survey as part of a one-hour lab session. Some participants could earn money in other studies of the same session. Therefore, we included the total session earnings from the analysis to control for the effect of other earnings on participants' feelings.

Importantly, we replicated the RP-bias effect when employing a standard open-ended SP procedure, but not with the alternative procedure. Asking participants to state their preference in a more concrete way, where money could be visualized, significantly decreased their SP valuation, which in turn affected subsequent RP valuation. The observed results bolster our account that preference overstatement rather than revealed-preference undervaluation causes the observed bias. The results of Experiment 3 also suggests RP estimation that follows a SP procedure should be interpreted with caution because different SP-elicitation protocols may lead to different outcomes. Moreover, hypothetical-bias investigators who seek to employ within-group experiment designs should account for the selected SP protocol and be aware of its effect on the final measure of valuations. The results of Experiment 3's affect measures support the idea that once a reference wage is set (e.g., by writing), adhering to it requires less mental cost. Participants in the Write condition did not experience more negative emotions than those in the Baseline condition, although they took more money. Therefore, we have no evidence that participants in the Baseline condition committed to a lower compensation than what they thought they should have been paid. These findings also support the idea that RP estimation closely represents participants' true preference, at least as they perceive it at the time they made a commitment. However, as discussed above, the malleability of the preceding SP-elicitation procedure affects the final outcome. Finally, less plausible is the idea that individuals are aware of the effect, but in order to avoid negative emotions that could be associated with taking more money, did not favor the

dominating strategy in Experiment 2.¹⁰ Those in the Write condition who took a higher wage did not experience more negative feelings than those in the Baseline condition.

Experiment 4

Our last experiment was designed to address additional alternative accounts that may underlie the observed bias, as well as to explore potential boundary conditions. First, one may claim that participants may not perceive the listening tasks as an act that requires real effort but rather, they were just reacting accordantly to the experimenter instructions to count bell rings. In experiment 4 we replaced the listening task with a different real effort task and provided clear instructions on how participants' performance is measured. Second, to avoid a potential confusion about what the payoff from the bowl is for (e.g., entire lab session, both tasks etc.), all the participants received a verbal explanation before entering the payment room in addition to the instructions note left on the payment table. Finally, besides the Baseline and Write conditions, Experiment 4 employed four more conditions. Although the previous results show that employing a more concrete SP procedure does not inflates subsequent RP estimation (i.e., VR condition), it is still possible that participants who go directly to the payment room upon completion of the last task, are more focused on the scenario than those who spend time to state their preference. Therefore, 'hot' state participants (focused on the task) may behave differently than 'cold' state participants who face an additional task (SP

¹⁰ One may suggest people's predictions might diverge from reality and that people still predict that choosing the Write condition would be associated with more negative emotions even if this[what? Vague] is not what would happen in reality. Unfortunately, our experimental design did not allow for testing this assumption.

question). Therefore, one can claim that our results might be driven merely by changes in temporal proximity between the supply of labor and the payment, rather than by the proceeding SP procedure. To explore this alternative, Experiment 4 employs a ‘cool off’ condition which extends the time between the completion of the last task and the payment. Additionally, participants who state their WTA in an open-ended manner (Write condition) are not restricted to state any amount they deem fit, while RP valuation is restricted to the amount of money in the bowl. It could be suggested that the above discrepancy explains the observed between-group hypothetical bias. Further, it may also explain the absence of SP-bias in the VR condition, since the value stated in the VR condition is restricted to the same amount of money available in the bowl. An unbounded (open-ended) SP procedure could also lead those in the Write condition to assume there is more money available in the bowl which consequently caused them to take more. In the new ‘List’ condition, participants stated their preference by selecting a wage from a list containing values between \$0 and \$10 in \$.25 increment. This stating method clarifies the available wage range while maintaining some of the abstractness inherent in the open-ended SP task. If the presence of a value range in the VR procedure of Experiment 3 caused the RP-bias to disappear, then we should expect the List condition to yield similar findings (no RP-bias). However, observing an RP-bias in the List condition may suggest that the effect is not a result of restricting the maximum value allowed in the SP procedure. Two additional conditions were designed to explore potential boundaries of the effect. In the ‘Mental’ condition, participants are asked to think about their WTA but not to report it. Lastly, when asked to report a desired wage, people may construct their monetary valuation having whole dollar amounts central to their decision. A payment

offered in quarters may lead some people to take less money than the amount they initially had in mind (e.g. taking 12 coins may loom larger or greedier than taking 3 dollar bills), but so long as they do not initially overstate their valuation (since maintaining consistency with previous valuation leads to take a higher amount). In the ‘Bills’ condition we offer participants to take their compensation from a mix of quarters and dollar bills to explore the role of money medium on the effect.

Design

Two hundreds ninety-three (293) undergraduate students from the same pool as the previous experiments took part in Experiment 4 in exchange for course credits ($M_{age} = 21$, 47% Females). We did not allow individuals who participated in the previous versions of this experiment to participate in the current experiment. As mentioned before, we adjusted the previous design by utilizing a real-effort task: the slider task (Gill & Prowse, 2011). In the slider task, a large amount of slider bars with a scale ranged 0-100 and randomly selected initial positions are displayed on the computer screen (see Appendix D). Using the computer mouse, subjects needed to drag and position as many sliders as possible in the middle position within an allotted time. The subject’s score in the task, interpreted as effort exerted, is the number of sliders positioned at the middle at the end of the allotted time. Participants in Experiment 4 performed two slider tasks: a one-minute task in which they were told they would receive \$0.25 for completing the task and a similar but longer five-minute task. Before the long task, the instructions reiterated that the more sliders participants completed, the better their performance in this task (see Appendix E for the complete instructions). The instructions that followed the completion

of the longer task were similar to those in the previous experiments, with the additional emphasis that the total sum would be paid to the participants on their way out of the lab. In the Mental condition, participants were asked to reflect on the previous task and think about how much they should be paid for that task. However, they did not have to report that number. In the Cool-off condition, participants who completed the second task saw a program loading animation and a message that asked them to wait while the data were being loaded. Then, 120 seconds later (a number not revealed to the participants), the page automatically advanced and they were allowed to continue the study. Participants in the List condition read the same instructions as those in the Write condition but reported their WTA by selecting a value from a list of available wages as described before. Finally, while being led to the payment room, all the participants received the following instructions from the experimenter, who was blind to the condition: “You’re almost done for the day... Please go into this room to pick up your payment for today’s last couple tasks—this payment is only for the last part of today’s session in which you worked on the 2 slider tasks.” A note placed on the payment table repeated the previous message and added the following text in bold face: “Please take \$0.25 for the first slider task. On top of that, please take what you believe you should be paid for the second slider task.” We collected additional demographics data from participants in an unrelated study.

Results

Table 2.3 depicts Experiment 4 summary statistics. We observe a large between- or within-group hypothetical bias in both the Write and List conditions (p 's < .001). Extremely large SP values explain these results (median SP in the Write and List

conditions is \$9.25 and \$9.00, respectively). Although evidence of such a large hypothetical bias is not rare, we were not able to explain the large deviation from the previous experiment's results. Notably, our main effect is robust: once again participants in the Write condition took significantly more money than those in the Baseline condition ($Med-RP_{Write} = \$3.00$, $Med-RP_{Baseline} = \$1.00$, $p = .028$). By contrast, the amount that those in the Cool-off condition did not differ from that of that in the Baseline condition ($Med-RP_{Cool\ off} = \$1.25$, $Med-RP_{Baseline} = \$1.00$, $p = .97$), suggesting temporal proximity may not play a role in the observed bias. Participants in the List condition took more than those in the Baseline condition, but this difference does not reach significance ($Med-RP_{List} = \$2.25$, $Med-RP_{baseline} = \$1.00$, $p = .14$). However, this amount does not differ from that of the Write condition either ($Med-RP_{List} = \$2.25$, $Med-RP_{Write} = \$3.00$, $p = .35$). Together, the findings suggest that although stating a WTA by selecting a value from a bounded list may not completely remove the detached nature of an open-ended SP procedure, the RP bias is still robust to situations in which the range of available wages is unambiguous. Participants who were asked to think about (but not to report) their compensation before arriving to the payment room took a similar amount of money as those who were not required to think about their compensation ($Med-RP_{Mental} = \$1.50$, $Med-RP_{Baseline} = \$1.00$, $p = .56$), but a smaller amount than those in the Write condition ($Med-RP_{Mental} = \$1.50$, $Med-RP_{Write} = \$3.00$, $p = .054$). As the current results suggest, a cognitive construction of individuals' (unreported) preferences might not be strong enough to affect consequent behavior. However, we are not able to control whether participants in our study constructed their preference mentally, and thus we should interpret these results with caution. Finally, those who were offered a mix of bills and

quarters took more than those in the Baseline condition, but this difference is not significant ($Med-RP_{Bills} = \$2.50$, $Med-RP_{Baseline} = \$1.00$, $p = .24$). However, this amount is also not significantly different than the amount taken by those in the Write condition ($Med-RP_{Bills} = \$2.50$, $Med-RP_{Write} = \$3.00$, $p = .49$). That is, we observed no RP bias in the Bills condition.

Regression Analysis: As expected, SP and RP are positively correlated ($r_{Pearson} = .35$, $p < .001$). We regress both measures on the treatment condition, the performance score in the second real-effort task, as well as gender and age. Table 2.4 summarizes the median regression results. Starting with SP (column 2), we see no difference in the stated valuation between the Write and the List conditions ($p = .72$), suggesting that whether or not an SP-elicitation protocol is bounded should not affect the stated value.¹¹ The more effort participants exerted in the task (operationalized by the slider-task score), the higher the stated compensation, and this effect is marginally significant ($p = .059$). Looking at the actual committed value (column 3), we see that both the Write and List conditions differ significantly from the Baseline condition: employing an SP procedure predicts participants will take a higher wage regardless of whether the SP procedure is bounded ($p < .001$ and $p = .031$, Write and List conditions, respectively). Importantly, the coefficients of the Bills and Cool-off conditions are insignificant, suggesting none of these manipulations affected participants' RP valuation. In other words, only the presence of a preceding SP procedure (open-ended or list) affected participants' RP. The effort

¹¹ Obviously, a very low bound will affect stated valuations, but our experimental design set a reference wage that was significantly lower than the upper bound.

exerted in the task predicts the amount of money participants took, such that the more effort participants exerted, the higher the wage they took for themselves ($p = .027$). These results validate our real-effort manipulation and confirm that participants in Experiment 4 accounted for the efforts they exert in their decision. Finally, in the current Experiment, RP decreases in age ($p = .007$).

Discussion

The results of Experiment 4 reveal a large hypothetical bias in both within- and between-group experimental settings. However, the large deviation of the SP valuations from the previous experiments leads us to interpret these findings with considerable caution. The current results may also suggest a boundary condition by which merely cognitive construction of one's WTA may not affect RP valuation. These results are consistent with Falk & Zimmermann (2015) who show that in an estimation task, subjects were less likely to account for new information and revise their initial estimate when they had to write rather than mentally construct this estimate. However, as mentioned earlier, the inability to control whether participants in our experiment constructed their preference before arriving at the payment room limits our ability to reach firm conclusions. Yet Experiment 4 offers several important insights. First, temporal proximity does not seem to underlie the observed effect. A cool-off time of (apparently) "hot state" participants did not produce an RP bias in spite of having an ample time to reflect on the situation. Second, we find no evidence that the effect is a result of employing an unrestricted SP procedure (open-ended response). Having to select a desired wage from a bounded list of values predicts accepting a higher compensation

compared to the Baseline. The last results also strengthen our previous findings suggesting the effect is rooted in the abstract nature of the reporting protocol rather than the information about the wage limit. Lastly, we could not conclude the payment medium was central in participants' evaluation decision, because those who were offered both coins and bills did not demand a higher wage than those who were offered coins only. However, our standard test reveals no RP bias; therefore, we cannot conclude the medium plays no role in the preference-elicitation process. De facto, our Bill condition did not have a real control. A condition that would allow participants not only to take bills and quarters but also to state their preferences using corresponding stimuli (i.e., bills and quarters) would help shed more light on the role of medium in the RP bias. Importantly, the observed effect was not a result of reactance or experimenter demand. Rather, it occurred even when participants accounted for the effort they exerted in the tasks in their SP and RP valuations.

GENERAL DISCUSSION

We opened with a stylized example of a real-life situation that may involve both stated and actual committed values. We presented several theories and focused on two contradicting predictions. Our results show what any behavioral scientist would predict: people do not behave according to standard economic theories, but rather incorporate other normatively irrelevant factors in their decisions. Interestingly, we find conformity, and not greed, plays a central role in biasing observed preferences. Our investigation suggests the business owner will pay herself a higher wage if she chooses to state her wage in writing before taking the money.

We presented four experiments that address the main question under study. In Experiment 1, individuals who stated their compensation for a task they had just performed took significantly more money than those who had not stated their compensation (RP bias). Also, SP was significantly higher than RP, but only in a within-group test. Experiment 2 demonstrated the bias is intuitive: people are indifferent to stating their compensation prior to committing to an actual amount. Experiment 3 results suggest both within- and between-group hypothetical bias are robust to the full-information condition. Most importantly, the effect was resolved by reducing some of the bias that is inherent in an abstract open-ended response procedure. Therefore, an RP-estimation procedure that follows an SP-estimation procedure should only be interpreted in the context of the previous estimation. A post-decisional affect measure helped refute some alternative accounts for the previous results. Lastly, aside from replicating the effect, Experiment 4 offers two new insights. First, the bias still exists when individuals account for the effort they exert in the task in their judgment. Second, neither temporal distance nor information (or lack of) about the maximum wage allowed could explain the observed bias. Additionally, we could not conclude that different money mediums that are potentially central to evaluation and payment (i.e., valued in dollars and paid in quarters) underlie the results. However, observing no bias in the Bills condition justifies further investigation of the role of the money medium. Lastly, mental construction of one's preferences may not affect subsequent RP, although a more rigorous investigation is necessary as well.

Given the critical role that preference estimation plays in many domains of behavioral science, a better understanding of the effect of the selected elicitation procedures on the estimation outcome is invaluable. Moreover, WTA preference, the valuation one assigns to exerting efforts or getting rid of a good, is susceptible to a range of normatively irrelevant factors. Therefore, an individual's preference estimation should be interpreted in the context in which it was elicited. In this paper, we explore the relation between stated and revealed preference when both elicitation procedures are employed simultaneously. We identified an important violation of procedure invariance in which RP estimation that follows an SP procedure can be biased, and we suggest this bias is a result of individual conformity to an inflated hypothetical valuation. Put more simply, a mere act of stating one's preference may influence subsequent behavior. Furthermore, the effect is intuitive and costless. Therefore, we advise considerable caution when interpreting RP that follows SP estimation. Many investigators continue to use both stated and revealed measures of preference simultaneously, without being aware of the effect one measure has on the other. Although whether a biased measure of an individual's valuation is a problem depends on the question at hand, we encourage investigators to carefully consider their choice of preference estimation in light of these new findings. If violations of procedure invariance might affect the final conclusions, our findings should help researchers isolate such potential biases. Our results offer two additional insights. First, normatively irrelevant factors can still influence individuals' preferences (stated and revealed) even under full-information conditions. Second, hypothetical bias can still be a problem even when no ambiguity exists regarding the target stimuli (i.e., full-information condition).

Although we were mainly focused on the methodological implications on preference elicitation, our findings are compatible with previous work showing that people strive for consistency in their commitments even when those commitments are non-binding. We're more likely to do something after we've agreed to it verbally or in writing (Cialdini, 1984). On the practical level, one can think of a large number of implications of the observed results. A natural one would be negotiation. The current results reinforce the importance of good preparation and suggest one must write down the desired outcome before engaging in negotiation. Also, professionals whose fees are effort-based should use this technique to ensure they demand compensation that would maximize their profit.

Table 2.1. Experiment 1 Results

Predictor	B_{Med Reg.}	B_{OLS}
<i>Intercept</i>	.20 (2.14)	-2.77 (3.20)
<i>Write condition</i>	.45† (.24)	1.14* (.45)
<i>Greed</i>	.025 (.02)	.06 (.03)
<i>Age</i>	-.47* (.21)	-.62 (.44)
<i>Gender</i>	.05 (.09)	-.17 (.13)

F[4,77] = 3.87

Note: Standard errors are presented in parentheses below parameter estimates. Significance code: * $p < .05$ † $p < 0.1$. A median regression performed with R and quantreg package using nid standard error estimation.

Table 2.2. Experiment 3 Summary Statistics

Condition	N	Median RP	Mean RP	Median SP	Mean SP	Within HB Test	Between HB Test
Baseline	58	\$1.25	\$1.45				
Write	58	\$2.00	\$2.70	\$2.00	\$7.22	p < .001	p < .001
VR	51	\$1.25	\$2.22	\$1.25	\$2.69	p = .01	p = .01
Norm	53	\$2.00	\$2.46	\$1.75	\$4.67	p < .01	p < .001

Note: RP includes \$.25 paid for completing the first listening task. HB = Hypothetical Bias. A pairwise Mann-Whitney test was used to analyze within-group hypothetical bias.

Table 2.3. Experiment 4 Summary Statistics

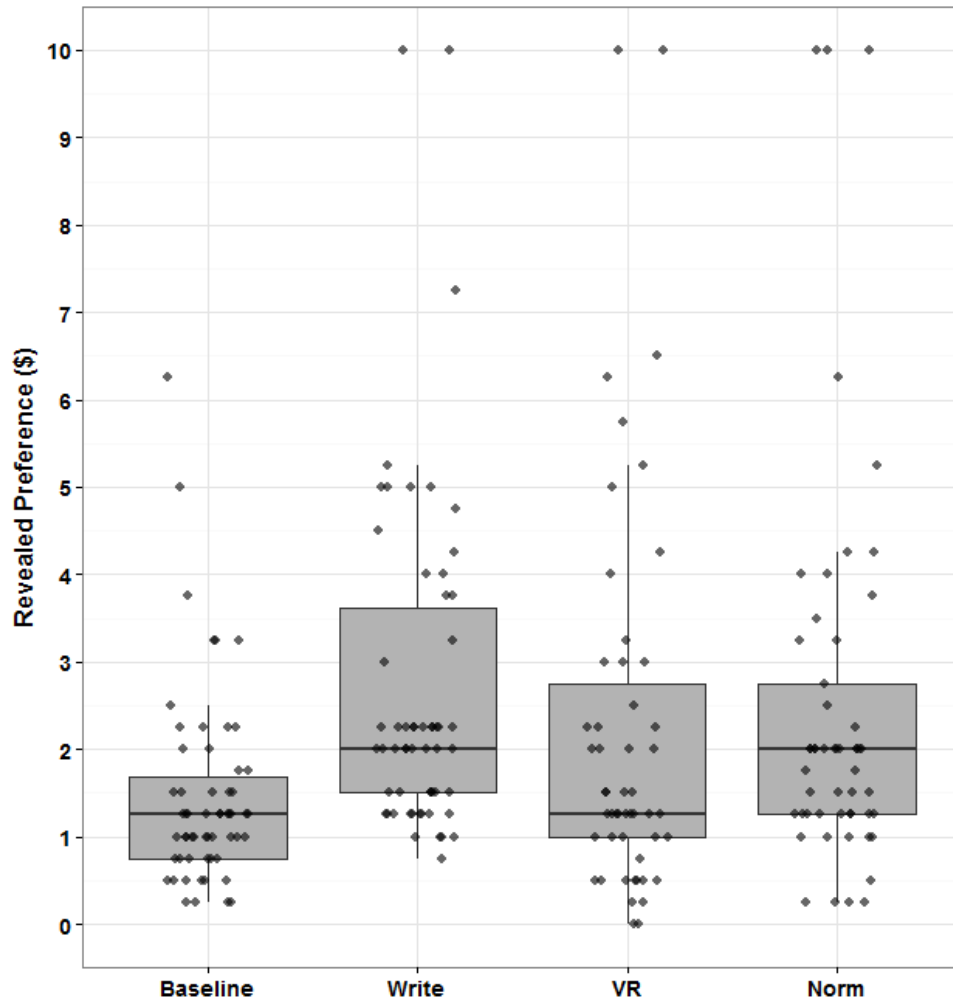
Condition	N	Median RP	Mean RP	Median SP	Mean SP
Baseline	51	\$1.00	\$3.11		
Write	50	\$3.00	\$3.92	\$9.25	\$12.02
List	51	\$2.25	\$3.62	\$9.00	\$6.16
Cool off	51	\$1.25	\$2.68		
Mental	49	\$1.50	\$2.89		
Bills	41	\$2.50	\$3.54		

Note: Amount taken includes \$.25 paid for completing the first slider task.

Table 2.4. Experiment 4 Effects on Stated and Revealed Preference

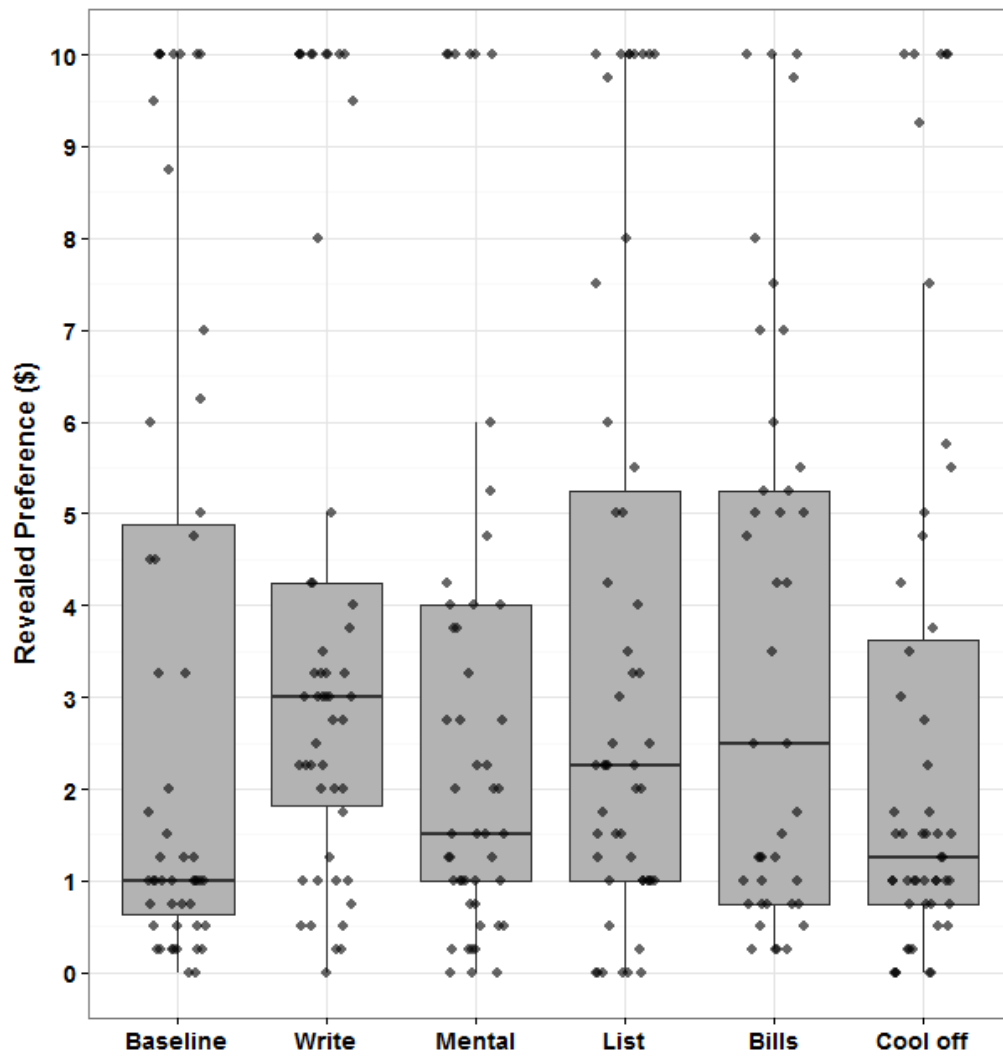
Predictor	SP	RP
<i>Intercept</i>	-1.86 (8.32)	2.06* (0.94)
<i>Write condition</i>		1.69** (.50)
<i>Mental condition</i>		.53 (.51)
<i>List condition</i>	.55 (1.56)	.91* (.42)
<i>Bills condition</i>		.87 (.81)
<i>Wait condition</i>		-.08 (.50)
<i>Slider score</i>	.06 † (.03)	.01* (.005)
<i>Gender</i>	-.09 (1.69)	-.50 (.32)
<i>Age</i>	.09 (.17)	-.06** (.02)

Note: Standard errors are presented in parentheses below parameter estimates. Significance code: *** $p < .001$ ** $p < .01$ * $p < .05$ † $p < 0.1$. Base conditions are Write and Take for columns 2 and 3, respectively. A quantile regression was run using `quantreg` r package with “nid” standard error estimations (bootstrapping yielded similar results).



Note: Standard box-and-whisker plot

Figure 2.1. Experiment 3 - Revealed preference Distribution across Conditions



Note: Standard box-and-whisker plot

Figure 2.2. Experiment 4 - Revealed preference Distribution across Conditions

APPENDIX

*Appendix A*First listening task instructions:

You are about to listen to a white noise for 67 seconds. White noise is a random signal with a constant power spectral density.

Besides the white noise, several bell sounds will also be heard at different times. Your task is to count and report the number of bell sounds embedded in the white noise.

You will be paid \$.25 for completing this task.

Please make sure that you have your headphones on and click Next when you are ready.

Second listening task instructions:

In the next task, you will listen to a white noise with similar bell rings as before. This task will be longer and will take about 4 minutes and 45 seconds.

You will be paid for completing this task.

Please make sure you have your headphones on and click Next when you are ready.

Write-Down-First manipulation treatment

Thank you for completing the two listening tasks of our study.

Please enter below how much you believe you should be paid for the **second** listening task.

The amount you will indicate below will be **added to the \$.25 you have already earned** in the first listening task and the **total sum will be paid to you** on your way out of the lab.

*Appendix B*Experiment 2 – Hypothetical Choice

Imagine that you were paid **\$0.25** for having completed the short listening task.

Now imagine that you are offered additional money for having completed the long listening task (4 minutes and 45 seconds). It is left up to you to decide how much money you will receive for having completed the long listening task. We will present two options from which you can choose how to receive this payment.

Option 1: In this option, you will be directed to the payment area where you will be presented with a bowl full of quarters in which you can take any amount of money you believe you deserve for completing the long listening task. No one but you will be present in the payment area.

Option 2: In this option, **you will be asked to write-down the amount of money you believe you deserve for completing the long task. You will then** be directed to the payment area and presented with a bowl full of quarters in which you can take any amount of money you believe you deserve for completing the long listening task. No one but you will be present in the payment area.

Which of the two options described above will you choose?

Appendix C

Experiment 3: Median regression results of RP on SP, treatment condition, and their interactions

Predictor	β
<i>Intercept</i>	1.28 (.87)
<i>SP</i>	.08*** (.001)
<i>VR condition</i>	-1.29*** (.24)
<i>Norm condition</i>	-.15 (.76)
<i>SO x VR condition</i>	.60*** (.09)
<i>SO x Norm condition</i>	.0002 (.26)
<i>Gender</i>	.00 (.00)
<i>Age</i>	.02 (.43)

Note: Standard errors are presented in parentheses below parameter estimates. Base treatment condition is Write. Significance code: *** $p < .001$. Median regression performed with R and quantreg package using nid standard error estimation.

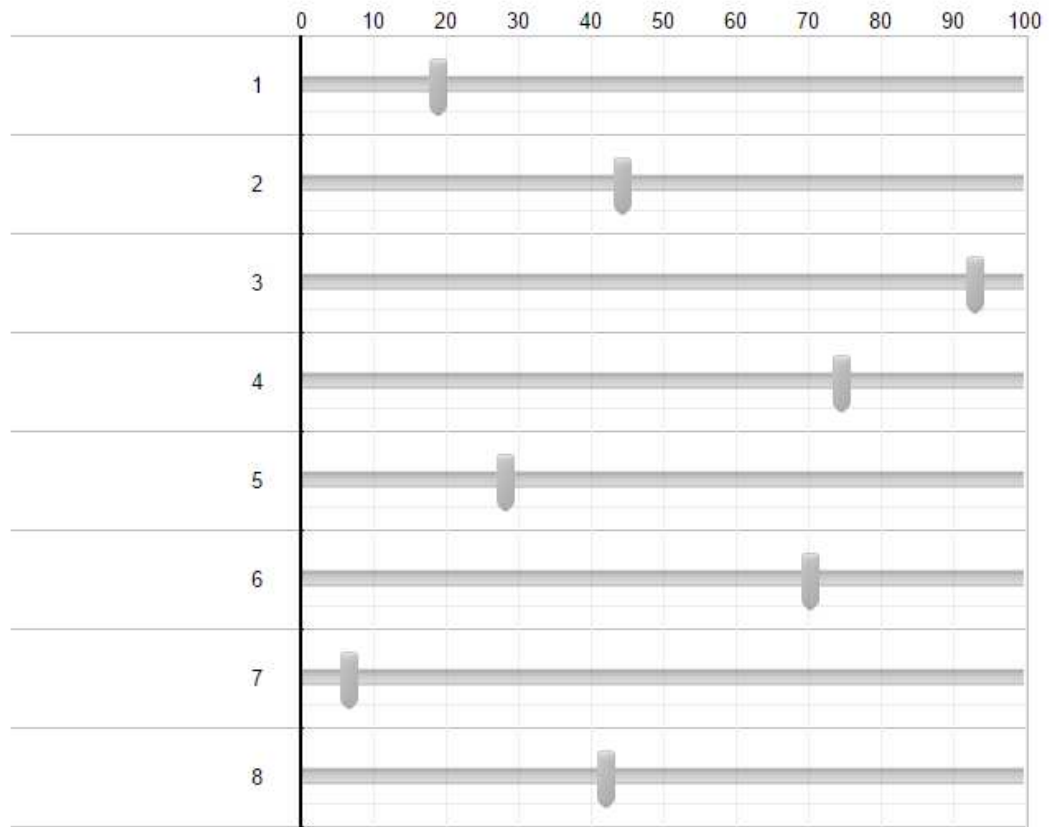
Experiment 3: Emotions OLS regressions results

	<i>Guilt</i>	<i>Regretful</i>	<i>Embarrassed</i>	<i>Annoyed</i>	<i>Ashamed</i>	<i>Happy</i>	<i>Excited</i>	<i>Enthusiastic</i>	<i>Proud</i>
<i>Revealed pref.</i>	0.078 (0.074)	-0.032 (0.083)	-0.004 (0.064)	0.014 (0.091)	0.017 (0.062)	0.122 (0.087)	0.006 (0.086)	0.173** (0.073)	0.027 (0.077)
<i>Write condition</i>	0.516 (0.358)	0.252 (0.403)	0.067 (0.308)	-0.030 (0.437)	0.564* (0.299)	-0.150 (0.422)	-0.462 (0.414)	-0.424 (0.353)	-0.198 (0.373)
<i>VR condition</i>	0.389 (0.371)	-0.451 (0.418)	-0.309 (0.320)	-0.345 (0.453)	0.161 (0.310)	-0.572 (0.437)	-0.449 (0.429)	-0.505 (0.365)	-0.273 (0.387)
<i>Norm condition</i>	0.602* (0.361)	0.541 (0.406)	0.271 (0.310)	0.220 (0.440)	0.244 (0.301)	-0.589 (0.424)	-0.413 (0.417)	-0.365 (0.355)	-0.540 (0.376)
<i>Total earnings</i>	0.032 (0.038)	-0.018 (0.043)	0.020 (0.033)	0.072 (0.046)	0.024 (0.032)	-0.059 (0.045)	0.009 (0.044)	-0.030 (0.037)	0.001 (0.040)
<i>Gender</i>	-0.022 (0.269)	0.075 (0.303)	-0.097 (0.232)	0.454 (0.329)	0.167 (0.225)	-0.523 (0.317)	-0.142 (0.311)	-0.129 (0.265)	0.353 (0.281)
<i>Age</i>	-0.027 (0.067)	-0.019 (0.075)	0.127** (0.057)	-0.061 (0.081)	0.025 (0.056)	0.107 (0.078)	0.054 (0.077)	0.118* (0.066)	-0.006 (0.069)
<i>Intercept</i>	-0.435 (1.565)	-0.266 (1.762)	-2.806** (1.348)	-0.823 (1.910)	-1.544 (1.306)	-1.077 (1.843)	-0.527 (1.810)	-2.521 (1.540)	-0.673 (1.631)
<i>Adjusted R²</i>	0.007	-0.002	0.013	0.001	-0.002	0.019	-0.022	0.022	-0.015
<i>F Statistic (df = 7; 193)</i>	1.201	0.956	1.362	1.014	0.931	1.558	0.378	1.634	0.577
<i>Note:</i>	* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$								

Appendix D

Slider-Task Screenshot

Please move each slider bar to the number 50.



Appendix E

Slider-Task Instructions

Task 1

You are about to perform a short task in which you will be presented with few slider bars. Your task is to slide the ball of each bar to the number 50 (exactly).

You will have 60 seconds to complete as many sliders as you can (only balls that land on the number 50 will be counted).

You will be paid \$.25 for completing this task (collected on your way out of the lab).

Please click Next when you are ready to start

Task 2

Next, you will perform another 'slider task' similar to the one before.

This time, you will have 300 seconds to complete as many sliders as you can (i.e., move the ball to the number 50).

The more sliders you complete the better your performance in this task.

As before, you will be paid for completing this task (collected on your way out of the lab).

Please click Next when you are ready to start

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Chapter 3.

“TEN MILLION READERS CAN’T BE WRONG!”, OR CAN THEY?
ON THE ROLE OF INFORMATION ABOUT ADOPTION STOCK IN NEW
PRODUCT TRIAL

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ABSTRACT

Most if not all new product frameworks in marketing and economics as well as lay beliefs and practices hold that the larger the stock of adoption of a new product, the greater the likelihood of additional adoption. Less is known about the underlying mechanisms as well as the conditions under which this central assumption holds. Using both controlled experiments and a field experiment involving new product purchase decisions, we demonstrate that the influence of information about a large stock of adoption on product diffusion is complex. We find that in order to increase the new product's purchase likelihood the large stock of adoption needs be of high affinity others and should not be coupled with too low or too high product uncertainty (e.g., an uninformative product description). Otherwise, information about a large stock of adoption may be insignificant to or even reduce the purchase likelihood. This is the first direct test and demonstration of the intricate role of information about a large stock of adoption in the new product diffusion process, and bares direct implications for marketers.

INTRODUCTION

Most if not all new product frameworks in economics and marketing as well as practitioners' beliefs hold that a large stock of initial sales of a new product increases the likelihood of subsequent adoption, presumably because of its positive signal to potential customers (e.g., Bass 1969; Mahajan, Muller & Bass 1990). Evidently, advertisers use statements like "Ten million housewives can't be wrong [about purchasing the product]", and "Over 19 Billion served", to attract additional customers. However, it is less clear *why* should such information provide a positive signal, or, better yet, *when* should a customer make a positive inference from information about a large initial sales volume if at all. Viewing the current stock of adoption information as a signal (Tirole 1988), we investigate the effect of such information on the purchase likelihood of new products, but rather than making an equilibrium statement, we seek to uncover processes governing the customers' belief structure. That is, we look at how informative and diagnostic the current stock of adoption signal is, as a function of the product information available (and its credibility), customer expertise, and the customer's affinity to said stock.

In this work we challenge the over-arching assumption that the larger the current stock of adoption of a new product, the greater the likelihood of additional adoption. We explore when and whether information about a large stock increases the adoption likelihood of new product. In particular, we study when information about a large stock of adoption decreases new product uncertainty. Employing both controlled and field experiments examining customer choices of new and unfamiliar products, we study the effect of information about a large-stock of adoption on people's purchasing decisions, as a function of characteristics of the stock itself (e.g., the affinity between the stock and the

customer) and the degree of uncertainty around the new product (e.g., how much is known about the product or the category).

Our main results reject the lay notion that information about a large stock of adoption uniformly increases adoption likelihood. In particular, we find an interactive effect with the identity of the stock and the degree of product uncertainty. Moreover, we find that if product uncertainty is too high, statements about a large stock of adoption may undermine seller credibility and decrease adoption likelihood. We demonstrate this effect of uncertainty using both product information manipulations and measured expertise. We find these effects repeatedly, and for a variety of product types. We discuss this complex relationship in the context of social influence and information credence, and conclude with implications for marketing practitioners.

The Current Stock of Adoption as a Signal

The current work falls into a growing stream of research about new product diffusion, which is characterized by the central role played by the information about a new product in determining consumers' tendencies to adopt it (Muller, Peres & Mahajan 2009). In general, individuals can adopt innovation as a result of two types of influences: exogenous influences like advertising and other communications by the firm, and endogenous influences resulting from peer interactions in the social system, based on word-of-mouth (WOM) and other interpersonal communications (Peres, Muller & Mahajan 2010; Mayzlin 2006). Unlike the scenario envisioned by the Bass model, diffusion processes have become more complex than ever, challenging the validity of the many basic assumptions of the original model. For example, while in the past most

endogenous influences were due to WOM and direct communication mechanisms, higher information and media availability these days enable individuals to both be influenced by others without direct communications and to learn about the current stock of adoption (CSOA thereafter) via exogenous means. These types of effects fall broadly under the umbrella of social influence.

Social influence has been a central area of research across the social sciences. Employing a variety of related theories such as *Social Proof*, *Social Comparison*, *Conformity* and *Social Norms*, *herding behavior*, and *information cascades*, researchers demonstrated that people are greatly influenced by others, and in particular, by their behavior (Asch 1951; Banerjee 1992). Notably, social influence is most effective when uncertainty is high (Wooten & Reed 1998), and when following the lead of similar others (Cialdini 2001, p.140; Festinger 1954). Moreover, the extent to which an individual perceives herself close to the influencing group (from here on, *affinity*¹) appears to determine the power of social influence (Abrams, Wetherell, Cochrane, Hogg & Turner 1990; Burn 1991). While the above evidence suggests that CSOA information should positively affect sales, in some conditions, CSOA information may actually hurt sales. In the case of conspicuous consumption of fashion, for example, as discussed in Pesendorfer (1995) and Amaldoss and Jain (2005a, 2005b), if one group of consumers (“snobs”) adopt the fashion trend as a signal that sets themselves apart from the rest of the consumers. The reason is that the value of the signal depends on the “right” sort of

¹ Affinity is defined as “a feeling of closeness and understanding that someone has for another person because of their similar qualities, ideas, or interests” (Meriam-Webster). While we use both affinity and fit terms in this paper, we refer to affinity to describe the relation between people and to fit to describe the relation between a customer and a product.

people delivering it. This suggests that the nature of the stock of adoption in the diffusion process, and in particular, its perceived affinity by the potential customer, should play a major role in its influence on potential adoption (White & Dahl 2007; Berger & Heath 2007).

Our main focus in this work is on the signaling value of the size and type of the CSOA, as opposed to other types of influences such as word-of-mouth communications or network externalities, and its interactions with the state of uncertainty. In particular, we focus on the role of signals in reducing uncertainty inherent in the new product adoption process (Kalish 1985; Caminal & Vives 1996; Simester 1995; Shin 2005). The rate of said process depends on the number of current adopters in each period (Bass 1969). Current adopters are assumed to interact with potential adopters and affect their adoption rate by some constant probability (usually denoted as q , the coefficient of imitation). CSOA information is also widely used by practitioners and deemed to be a major driver behind accelerating product diffusion [e.g., “Ten million readers can’t be wrong”] (usually captured by the coefficient p in the Bass model). Marketers tend to believe that the more customers adopt a new product (large CSOA), the stronger the signal about (higher) product quality. However, the underlying process, and when this assumption might not hold is poorly explored. Tucker and Zhang (2011) find, for example, that CSOA information mostly helps niche products rather than mainstream ones, as this signal is more diagnostic in this case. Moreover, it is even less clear how the effect of CSOA information provided by the firm (as opposed to observation or 3rd party sources) interacts with other types of information (this is essentially an exogenous signal

p about q). This distinction is important because, as discussed earlier, other factors might mitigate the effect of a CSOA signal on adoption – signal diagnosticity is key.

We argue that the mechanism behind the effect of CSOA information is more complicated than commonly assumed, as it interacts with other factors related to uncertainty reduction regarding the new product offering. While drawing a complete picture of the interactions of CSOA information with the full spectrum of signaling actions is beyond the scope of a single study, we analyze how the CSOA signal affects adoption as a function of degree of uncertainty influenced by the amount of detailed product information and its credibility, as well as the moderating factors of customer expertise and affinity.

When the characteristics of the product are not known or are not directly evident upon inspection, customer may face uncertainty about both the product quality² and fit³. Although products differ in their amenity to resolving different types of uncertainty (e.g., search vs. experience goods), the types of uncertainties we discuss broadly exist in all of them. CSOA information may reduce both types of uncertainty: Reduction in uncertainty about *product quality* has been shown to occur directly by displaying features of the product itself, but also via indirect signals, such as brand, seller reputation, price or even advertising (Monroe 1973; Gabor & Granger 1966; Gerstner 1985; Tellis & Wernerfelt 1987; Bagwell & Riordan 1991; Nelson, 1970; Milgram & Roberts 1986). Indeed, even uninformative advertising for an experience good could be a signal for product quality

² Defined as the objective measure of how the product performance such as durability, reliability, power, ease-of-use, is ranked within its category, i.e., its position in the vertically differentiated market.

³ Defined as a subjective evaluation of how compatible the product is with the customer's values and needs (e.g., size, color, shape etc.).

(Shin 2005). Reduction in *product fit* has been shown to be achieved by receiving various signals from the firm (e.g., advertising, placement, packaging, or sales people) (Wernerfelt 1994; but see Harris et al. 1997), or via other communication-channels like personal or non-personal recommendations, or product reviews. Customers consider both quality and fit to form their evaluation when deciding whether to adopt a new product. More generally, other characteristics of both the sender and the receiver may influence the degree of uncertainty surrounding the new product adoption decision, and are usually the result of information asymmetry between customers and sellers. Two relevant sources of uncertainty reduction are how informative the product description is together with its source credibility (i.e., *signal diagnosticity*), and the expertise of the receiver (i.e., *information accessibility*) (Feldman & Lynch 1988; Herr, Kardes, & Kim 1991).

Product description is the product information provided by the seller; it can be called *vague* or *non-informative* when it does not provide complete information or provides non-attribute related information (Mayzlin & Shin 2011), either because the seller potentially misrepresents the product, or does not fully disclose its characteristics, or because of the seller's inability to perfectly describe the product (Dimoka et al. 2012). In the current treatment we examine how a CSOA signal affects product uncertainty in light of an informative vs. a vague description of the product characteristics. Moreover, the lack of sufficient product description may also decrease seller *credibility* (Akerlof 1970; Pavlou et al 2007), if the customer expects the seller to disclose more information than is actually provided. If this is the case, the customer might discount any other signal from this seller, and be less likely to purchase (we term this situation “too-high uncertainty”). Even for well-established firms, new products often reach the market

without sufficient reputation, such that, many types of firms will likely attempt to reduce customer uncertainty using CSOA information.

When we allow uncertainty levels to vary, we obtain a more complex set of predictions, stemming from the interaction of the various message characteristics and the inference a customer makes about the credibility of the offer (as discussed above). In other words, we expect experts to respond differently from non-experts, as information accessibility differs: While experts are not likely to reach uncertainty levels that are too high, novices are unlikely to be at a state of no uncertainty. Consequently, for novices we expect a large CSOA message that lacks sufficient diagnosticity to backfire, because uncertainty would be sufficiently high that suspicion may arise about its credibility (Laurin, Kay & Fitzsimons 2012; Goldfarb & Tucker 2011). This is especially likely when the firm provides only vague or insufficient product information and thus does not alleviate any uncertainty. Alternatively, high uncertainty state may lead to a very low purchase probability (i.e. “Floor Effect”). Importantly, these predictions are still subject to the effects of affinity, such that even experts with little uncertainty about quality may forgo a purchase if low affinity signals low fit for them. Table 3.1 formalize these predictions. Note, in many cases of new products there are no real experts and this important factor may not always bear on the situation at hand.

Combining our predictions above we suggest that CSOA information may help customers resolve uncertainties about the product but only when presented in the right context. Importantly we also propose the novel idea that in certain contexts CSOA information can have a negative impact on product diffusion. To test these hypotheses we

designed a consequential choice experiment with newly released books, a field experiment with a brand new energy supplement product for surfers, as well as ran additional experiments involving three different hypothetical new products in the lab. The structure of the experiments conformed to the above analyses and is described below. We then follow with experiment specifics and a discussion of the results and their implications.

MATERIALS AND METHODS

General design of experiments

The experiments were designed to explore the customer's adoption decision of a new product. Our main dependent measure is choice (i.e., the percentage of participants who adopt the product of those who had the opportunity to do so). To study the effect of CSOA information and its interactions with stock-identity and the quality of product information, we manipulated the stock size information (large stock vs. no stock size information), the stock identity (high affinity vs. low affinity stock) and the product information quality (detailed vs. vague product description) in between participants design. In each of the eight treatment conditions, participants first receive the above information, and then decide whether to buy. In the first two experiments participants make real consequential decisions while in the last experiment the choice is hypothetical. We manipulate *stock size* information by including a statement about how many other individuals have already adopted the product. We manipulate product information quality by including either a detailed or a vague description of the new product. We ascertain *affinity* level in the first two experiments by manipulating the identity of the described

current adopters and then match it to the participants' self-reported information (ex-post). In the last experiment we hold stock identity constant and let the customers place themselves on a continuous scale with regards to their affinity with the described current adopters. Participants in Experiment 3 also report their subjective level of expertise with the product category, an additional factor influencing product uncertainty. . We then follow with a short survey administered to all participants, regardless of their purchase decision.

Experiment 1 (books):

In study 1 we offer one of two newly released books to online participants and measure their willingness to obtain the books as a function of the information provided. We use two different books that were published shortly before we conducted the study as two versions of the same study, between subjects, as a conceptual replication. The first book: "The why Axis", is a book about recent findings from behavioral economics and was written by researchers. The second book: "Talent Wants to Be Free" discusses drivers for successful innovation and was written by a law professor. The two versions of the study share the same design: Individuals in the online panel participate in studies and enter a lottery for a chance to win an Amazon gift certificate. As an alternative, we offered respondents to win the book instead of the gift certificate lottery. Although the value of the book was similar to the alternative gift certificate, the book lottery offered much better winning odds than the gift certificate lottery.

Method

Seven hundred participants from an online panel participated in a 2 (stock-size information: large-stock vs. no stock-size) x 2 (stock-identity: highbrow vs. mid-class individuals) x 2 (product information quality: detailed vs. vague description) between subject design. Three hundred and fifty four participants were offered one book, and the rest, the other book. The design of the two book experiments was identical: In each of the eight conditions participants saw a picture of the book cover and information about the book. Each condition carried a different information script. After reading the text, participants reported whether they would like to participate in the book lottery instead of the default gift certificate lottery. We interpret a decision to select the book lottery as a representation of participants' willingness to purchase the new book. Participants in the large-stock condition read that the book had already attracted "*thousands of individuals,*" while those in the no stock-size condition had no information about the number of current adopters. Half of the participants read that the book was "*attracting mid-class curious readers*" while the other half that the book was "*attracting graduate-degree holding, highbrow individuals.*" Finally, half the participants read a detailed description of the book and its author/s (clear information), while the other half a vague single-sentence description of the book's main idea. After choosing between a lottery for a book or a gift card, all participants reported their level of education on an 8-point scale ranging from 'Less than High School' to 'Professional Degree' and their annual income range. Participants also indicated the extent of which they agree with the sentence: "I had sufficient information about [book title] book to decide whether to participate in the lottery" (on a 7-point scale) as well as how often do you read books for pleasure (on a 5-point scale). Lastly, participants reported the extent of which they agree with the

statement “I am a risk taker” on a 7-point scale ranging from ‘Not at all like me’ to ‘Just like me’.

As an example, below is the scripted information from the “The why Axis” book study of the condition in which large stock, highbrow fit, and clear information is provided (The full text appears in Appendix A):

As an ALTERNATIVE to participating in the lottery for a cash reward, you may choose to participate in a different lottery that awards a copy of the “The Why Axis”. The book is currently offered on Amazon for \$17 (hard cover) and is rated 4.7 out of 5 stars. This alternative lottery affords a much higher chance of winning. A total of 50 books will be raffled among participants in this study who chose the alternative lottery.

*Here is some more information about the book: “The Why Axis” has been released only few weeks ago **but already attracted thousands of graduate-degree holding, highbrow individuals** who are interested in a better understanding of the motives underlying human behavior...*

Please indicate below if you are willing to participate in the lottery for “The Why Axis” Book IN EXCHANGE FOR the lottery for a cash reward.

Results

Participants’ distribution across the two versions of the study was nearly even, and the results were similar (see Figure 3.1). Therefore, we only discuss the analysis of the consolidated data henceforth.

Manipulation check

Participants who received detailed information about the book were more confident of having sufficient information to make their choice, than those who read a vague description of the book ($M_{clear\ desc.} = 4.59$, $M_{vague\ desc.} = 3.98$; Mann-Whitney one-tailed test: $p < .001$). In other words, detailed information decreased product uncertainty.

Affinity

To correctly assign participants into appropriate affinity status, we used their stated level of education as a proxy for affinity with the description of the books' current stock identity. We assume that those who report having a 4-year collage degree or more are more likely to be affiliated with a stock described as "*graduate-degree holding highbrow individuals*" and those who report having 2-year collage degree or less are more likely to be affiliated with a stock described as "*mid-class curious readers.*" We consider a *high affinity* condition whenever there was a match between the participant's (self-reported) level of education and the information embedded in the treatment condition. Otherwise, we consider the condition to be of a *low-affinity*. This resulted in categorizing 344 participants as *high-affinity* (49.1%) and 356 participants as *low-affinity*.

Choice

How often participants reported they read books for pleasure significantly predicts their education level ($\beta = .31$, $t[698] = 7.39$, $p < .001$) and therefore we did not include the former measure in the regression analysis of choice. We ran a Logit model of participants' lottery choice (book vs. gift certificate) on the three focal constructs: stock-size, stock-affinity, and product information quality, as well as all their interactions (mean-centered, for ease of interpretation; raw regressions in Appendix A). We also controlled for participants' self-reported education and risk attitude. Although these control variables significantly predicted participants' choice, excluding them from the regression did not change the results. We summarize the results of both the un-controlled as well as the full model in Table 3.2.

Does merely providing information about a large CSOA increase the book attractiveness in our study? Apparently not. We observed no simple effect of any of the three focal variables. In particular, providing participants with information about others who have already purchased the book did not increase (on average) the likelihood of choosing the book ($\beta = .13, Z = .59, p = .55$). Likewise, neither a detailed description of the book, nor affinity level significantly influenced participants' choice ($\beta = -.4, Z = -1.26, p = .21$ and $\beta = .1, Z = .33, p = .73$, respectively).

More importantly, we observed a 3-way interaction: While stock affinity moderates the effect of large CSOA information on participants' lottery choice, the effect varies as a function of the book description (detailed vs. vague). As Figure 3.1 shows, when CSOA information is associated with high-affinity individuals, large stock information positively affected the book choice likelihood when coupled with a detailed description of the book [columns 1-2], but negatively affected adoption when the book description was vague [columns 3-4]. Completing the picture, when CSOA is of low affinity [columns 5-8], we find no effect of CSOA information on purchase likelihood.

Finally, as one would expect, education significantly predicts book choice ($\beta = .19, Z = 3.6, p = .01$). The more educated participants, the greater their tendency to select the book. Additionally, participants who perceived themselves as greater risk takers were also more inclined to choose the book ($\beta = .58, Z = 7.37, p < .001$). This observation supports the idea that new product adoption involves the management of both risk and uncertainty (e.g., Shimp & Beardern 1982; Grewal, Gotlieb & Marmorstein 1994). However, since risk attitude is a post decisional measure, the reported effect can be biased by the decision taken.

Discussion

The results clearly demonstrate that the effect of CSOA on the likelihood of purchasing a new product is complex. Let us first examine the situation in which a detailed product description is provided. Detailed description of the book decreases uncertainty (quality and fit), and thus increases adoption. The provision of additional information about a large number of high-affinity others who have already adopted the book helps participants resolve more uncertainty which should further increase adoption [columns 1-2]. However, when the stock referred to low-affinity individuals, information about a large CSOA did not influence participants' lottery choice in this study [columns 5-8], as it likely were perceived as non-diagnostic (more on this below).

We propose two potential alternatives to explain these results. First, customers may completely discount the signal value of an irrelevant reference group. Second, the null effect could be the result of two opposing forces. Adoption by a large group of customers may signal a high quality product, yet large stock of low affiliation individuals may signal low product fit. It thus follows that when the current adopters are of low-affinity, the effect of information about large CSOA balances out. The analysis above holds as long as the product description is clear and uncertainty is not too high. Why does information about a large CSOA negatively affect adoption when the book description is vague? We suspect that this is where the uncertainty about the new product becomes too high, potentially impairing the credibility of the information about the large CSOA. Specifically, providing participants in our study with information that many others like them already adopted the book, coupled with a vague (e.g., less informative) description

of the book, could lead them to have less trust in the information provided as a whole and thus avoid the book all together. As discussed earlier, lack of credibility is expected in situations where uncertainty is very high and customers are less knowledgeable in the category which may fit well with the current setting.

Although participants in Experiment 1 made a consequential choice, they may have considered the lottery choice to be part of the study and behaved differently than they would normally. In Experiment 2 we attempted to validate the fickle role of CSOA information in a field setting, where participants were not aware they were taking part in a study. Additionally, participants in Experiment 2 purchased the product using their own money. In Experiment 2, we extended our examination to a different product category – energy drinks. In addition to further generalizing our results, this product category more naturally lends itself to the manipulation of product information using more objective product attributes than books: Describing the ingredients and benefits of a drink is more straightforward and in line with common consumer experiences in the market. Moreover, participants were able to hold the product in their hand and verify some of the information they received by reading the product label. The energy drink we used was originally designed for surfers, a fact that allowed us to define the construct of affinity in the target population with greater precision. Finally, people may make decisions regarding new products very differently when in a virtual environment as compared to when employing personal communications with sales people in the field.

Experiment 2 (energy drink, field):

Working with an innovative new brand, we designed an experiment in which we attempted to sell a new (and unfamiliar) product to the public at different locations. The product was a new ‘Performance Supplement’ drink that comes in a small 2oz. energy-shot like bottle. The drink was designed for surfers and has not been released to the market at the time the study took place. Conducting our study in a southern California’s beach city where surfing is extremely popular, we were likely to have a fair amount of surfing enthusiasts in our data. Research assistants who were dressed in brand related wear from the company and served as sales people, offered passersby the opportunity to buy the product at an introductory discount price. During the interaction with potential buyers, the “sellers” provided different scripted information according to treatment conditions similar to those in Experiment 1. After announcing their decision whether to buy the drink in the promotional offering, we asked ‘participants’ to complete a very short ‘marketing research’ survey. We collected post-decision information regardless of individuals’ purchase decision and all but 10 individuals agreed to take the survey. Individuals who did not wish to complete the survey distributed randomly across conditions.

Method

Four hundred nineteen passersby were approached at on- and off- campus locations and were offered the drink at an introductory promotional price. Similar to Experiment 1, we employed a 2 (stock-size information: large-stock vs. no stock-size) x 2 (stock-identity: surfer vs. people) x 2 (product information quality: detailed vs. vague) between subject design. In each engagement, the sales-person communicated to a

passerby one of the eight scripts selected at random. After hearing information about the new performance drink, individuals were offered to buy it for \$.5, described as a promotional discount price. Purchasing more than one bottle was not allowed.

Individuals in the large-stock condition were told that *thousands* of customers are already using the product, and those in the no-stock-size had no information about the number of current adopters. In the stock-identity conditions, some individuals were told that the performance drink was “*specifically formulated for Surfers*” while others were told that it was “*formulated for water men and women and for everyone who is enthusiastic about sport*”. Also, either the word *surfer* or *people* was used to identify the current customers in the large stock condition (e.g., “*Thousands of surfers [people] already use [Product Name] every day.*”) Finally, in the quality information conditions some received a detailed description of the product’s ingredients and their benefits, while others only received a vague product description that says: “[Product Name] *is an all-around performance supplement that scores a whole lot more than just plain energy.*”

As mentioned above, passersby were also asked to complete a post-decision questionnaire masked under a cover of a company market research. In this survey we asked responders to indicate how much they agree with the statement: “*I am a Surfer*” by choosing one of the following options: Agree, Neither agree nor disagree, or Disagree. Additionally responders indicated how often they surf on a 7-point scale ranging from ‘Never’ to ‘Daily’. Responders were also asked to indicate on a 5-point scale how good a fit for their needs they would expect our drink to be (scale ranged from ‘Definitely will not fit’ to ‘Definitely will fit’), as well as to answer the question “*how often do you consume performance/energy drinks*” by selecting one of the following answers: Not at

all, Occasionally or Frequently. Eventually, responders reported the extent to which they agree with the statement: "*I am a risk taker*" on a 7-point scale ranging from 'Not at all like me' to 'Just like me'. Demographic information concluded the survey.

As an example, below is the scripted information for the condition in which large stock of surfers is described and, high quality information is provided (the information script of each condition, as well as the post-decision survey, is presented as Appendix B):

*[Product Name] is a new innovative performance supplement.
It is an **All-Natural** performance drink **specifically formulated for Surfers.**
Thousands of Surfers already use *[Product Name]* every day.
As part of onetime market study, I'd like to offer you to join **all the local surfers** already using *[Product Name]* and see how *[Product Name]* can boost your performance, for only a fraction of its actual cost.
[Product Name] is free of artificial sweeteners, and **contains Super-Fruits** like Acai berry, Goji berry, Noni fruit and Pomegranate that provide a boatload of healthy antioxidants.
[Product Name] is designed to provide surfers with:*

1. **High-Performance and Natural Energy.**
2. **Better Hydration** by incorporating a blend of electrolytes.
3. **Better Metabolism** that lowers body fat by incorporating botanicals like Garcinia Cambogia and Green tea.
4. **Better Muscle Recovery** by incorporating nutrients like l-carnitine l-tartrate, l-tyrosine and magnesium.
5. **Enhanced Immunity** by incorporating vitamins C and D and other nutrition's like alpha lipoic acid.

Results

Manipulation Pretest: One hundred and four individuals who were recruited through Amazon Mechanical Turk (62.5% Males, $M_{age} = 32.2$ years), completed a survey about consumer choice. After reading a description of a new performance supplement product as described above (exactly the same stimuli), participants indicated on a 0-100 scale (also labeled Extremely Low - Extremely High), the extent to which they associate the following five qualities with the *source* of the product information: Trustworthy,

Believable, Reliable, Expert, and Credible (presented in random order). We averaged the five qualities to create a Credibility index ($\alpha = .97$). Using the same scale, participants also indicated the extent to which they consider the product as of *high quality*. In between-subject experimental design, some participants saw the detailed product information script while others saw the vague one. Comparing these groups we see that those in the detailed product information condition perceived the product as of a significantly higher quality than those in the vague product information condition ($M_{\text{detailed}} = 59.07$, $M_{\text{vague}} = 42.96$, $t[102] = 3.98$, $p < .001$). These results support our designed manipulations. Moreover, the more comprehensive information about the product we provided to our participants, the higher their perceived credibility of the information source was. Specifically, those in the detailed product information condition indicated the information source as a significantly more credible source than those in the vague product information condition ($M_{\text{detailed}} = 52.69$, $M_{\text{vague}} = 37.93$, $t[102] = 3.51$, $p < .001$).

Affinity

The two measures that pertained to surfing were highly but not perfectly correlated ($r_{\text{Pearson}} = .73$, $p < .001$). Some individuals indicated they have never surfed while still perceive themselves as surfers whereas others reported participating in surfing activity but were not sure they would describe themselves as surfers. We regard participants' self-perception of being surfers to be a more justifiable measure of affinity with the surfers' consumer segment. To construct the affinity measure, we generated a match measure such that those who perceived themselves as surfers and were in the

surfers condition (e.g., “...*designed for surfer*”), and those who do not and were in the non-surfers condition were all marked as high affinity. Conversely, those whose condition mis-matched their self-perception were marked as low affinity. Finally, those who marked themselves as only somewhat surfers were coded as middle affinity. Therefore, we constructed a 3-level scale of affinity adjusted to the stock-identity manipulation condition. This resulted in categorizing 155 individuals with high affinity, 70 individuals with medium affinity, and 179 individuals with low affinity to the message target group⁴. Individuals who fell into the *high affinity* category reported a higher pre-consumption perceived product fit than those who belonged to the *low affinity* category and this difference was marginally insignificant ($M_{high\ affinity} = 3.08$, $M_{low\ affinity} = 2.93$; Mann-Whitney one-tailed test: $p = .087$), infusing some validity in our categorization. As a robustness check, we also created an alternative dichotomous measure of affinity that was constructed from both surfing questions together. Particularly, in the alternative measure we defined a *Surfer* to be anyone who either reported participating in surfing activities or did not disagree with the statement about him/her being a surfer. As before, we consider *high affinity* to hold whenever a *Surfer* individual was assigned to a ‘Surfer’ stock identity condition (e.g., “...*designed for surfer*”) or when a *non-Surfer* individual was assigned to the ‘People’ stock-identity condition (e.g., *thousands of people already using the product*). This resulted in categorizing 190 individuals as *high affinity* and 215 individuals as *low affinity*⁵. Individuals who fell into the *high affinity* category in the

⁴ 15 individuals failed to report their agreement with the statement about being a surfer and therefore their affinity could not be categorized.

⁵ 14 individuals failed to report both measures of surfing affiliation and could not be categorized as high or low affinity.

alternative measure reported a higher pre-consumption perceived product fit than those who belong to the *low affinity* category ($M_{high\ affinity} = 3.15$, $M_{low\ affinity} = 2.95$; Mann-Whitney one-tailed test: $p = .018$).

Figure 3.2 illustrates the results of Experiment 2 using the dichotomous affinity scale data. Note the similarity between Figure 3.1 and Figure 3.2: The signal of large stock works well in the detailed information condition (detailed product description) **and** when the stock is of individuals toward which the customer has high affinity. It has a negative effect in the low affinity conditions and in the vague product description condition. If one is willing to assume that the average customer in Experiment 2 is less knowledgeable about the supplement products category, these results are in-line with our predictions.

Table 3.3 summarizes the Logit results of a the purchase decision on the stock size, affinity and product information quality (mean-centered, for ease of interpretation; raw regressions in Appendix B), as well as all interactions, for both the original affinity measure (3-level affinity scale) and the robustness check models (dichotomous affinity scale). In the full models we also controlled for individuals' self-reported energy drink consumption, perceived risk taking behavior, gender, and age. Thirty seven individuals failed to fully complete the post-decisional questionnaire and could not be included in the full model regressions⁶. Finally, we added a fixed effect to each 'sales representative' to account for seller heterogeneity (e.g., communication skills etc.).

⁶ After excluding incomplete observations in the full models, the categorization of individuals across affinity distributed as follow: 142 individuals were categorized as high-affinity, 67 as medium-

As Table 3.3 reveals, adding the control variables did not significantly change the results. Moreover, the robustness models all replicate the main model. Like Experiment 1, the model results suggest no simple effect of product information quality when holding affinity at its average level⁷. More importantly, once again our data suggests no simple effect of large CSOA information. Merely telling potential customers that many others are already using the product did not lead them to be more likely to buy ($\beta = 1.02$, $Z = 1.18$, $p = .23$). Stock affinity had a positive impact on purchase decision, although it did not reach significance ($\beta = .86$, $Z = 1.56$, $p = .12$). Most interestingly is the replication of the 3-way interaction ($\beta = 2.30$, $Z = 2.14$, $p = .03$): Experiment 2 supports our hypothesis that the effect of information about large CSOA on purchase likelihood depends on the amount of uncertainty customers bear which can be qualified by the product information quality (detailed vs. vague) and the customer affinity with the CSOA target group. Later, we will explore customer expertise as an additional factor influencing product uncertainty.

Various control variables were also significant predictors of choice, further supporting the robustness and validity of the results: As one would expect, energy drink consumption clearly predicts individuals' purchase decision ($\beta = 1.5$, $Z = .33$, $p < .001$), indicating that energy drinks consumers were much more likely to buy our new performance drink. Also, those who perceived themselves as risk takers showed a greater

affinity and 173 as low-affinity. In the binary affinity measure, 176 individuals categorized as high-affinity and 206 as low-affinity.

⁷ However, when affinity is at its minimum level, providing clear product information significantly increased the purchase decision ($\beta = 2.37$, $Z = 2.04$, $p = .04$). (see Appendix B)

tendency to buy ($\beta = .34, Z = 2.1, p = .03$). Additionally, males tended to buy the new product more than females ($\beta = 1.47, Z = 2.93, p < .01$), and older individuals purchased more than younger ($\beta = .16, Z = 3.72, p < .001$).

Discussion

The results of Experiment 2 support our previous findings. While information about large CSOA did not have a simple effect on sales, it simultaneously interacted with the stock target group and the product information quality. As Figure 3.2 shows (as well as Figure 3.1), when the CSOA information refers to high affinity individuals, large stock information had opposite effects on sales, depending on the quality of the product description provided. In line with our prediction, large stock information had a negative effect on sales when individuals received vague product information, and positive effect on sale when the information was detailed. As discussed earlier, lack of information credibility may negatively affect customers' decisions in the vague product description condition. What is more, the results of Experiment 2 support the notion that large stock information plays a significant role when it refers individuals toward which the customer feels high affinity, but less so when in the case of low affinity⁸.

Differences in the experimental setting may account for some of the different results between Experiment 1 and Experiment 2: The social nature of the sales interaction in the current experiment could play a role, by making potential customers feel more obligated to pay attention to the communicated information. Moreover, in the current

⁸ Running the no-controlled regression on the two extreme groups of the 3-level affinity scale separately reveals that the CSOA size interaction with information quality was significant among those with the highest affinity ($\beta = 3.20, Z = 2.25, p = .024$) and only marginally significant among those with the lowest affinity ($\beta = -2.35, Z = 1.85, p = .065$).

field settings customers were also allowed to physically scrutinize the product and compare the information with the products' label (e.g., ingredient list). Therefore, verbose product information in the current experiment might be more salient and seem more valid. In addition, a longer interaction with the sales representative in the detailed product description conditions might have lead individuals to feel more obligated to buy the product.

Recall that we found no significant difference between the low affinity conditions in Experiment 1, and two alternatives were considered: The null effect was either the case of customers completely discounting the signal value of an irrelevant reference group or the result of two opposing forces cancelling each other. The results of Experiment 2 support the latter, suggesting that high product quality and low product fit may have contradicting influences. If customers in Experiment 2 completely discounted the signal we would again expect an overall null. However, the current results reveal that telling customers that the performance drink was largely adopted by low affinity customers had an overall negative effect on sales, suggesting that customers in this study did not discount the signal even when it was associated with low affinity group. Moreover, if information about a large stock increased the perceived product quality but low affinity reduced the perceived product fit, the latter effect had a greater impact on the potential consumers for the performance drink in our experiment, decreasing sales.

Put together, we conclude that when product quality information plays a significant role in a new product adoption decision, customers do not ignore a CSOA signal even if it refers to low affinity adopters. However, the effect size and direction may vary as a function of the product category and the manner in which information is

provided. While some product domains are more quality centric, other domains may be more identity relevant, where product-social-group association is more likely to influence customers' choice (Berger & Heath 2007).

Experiment 3

In general, the design of Experiment 3 follows that of the previous experiments. Participants received information about a newly released product, followed by a purchase decision. Participants in Experiment 3 also completed a post-decision questionnaire as before. However, Experiment 3 differs from the previous two experiments in several important aspects. Unlike the previous experiments, participants in Experiment 3 faced a hypothetical choice. On the one hand, a non-binding choice allows participants the freedom to report they would buy a product even if they would be reluctant to do so, had they faced a real decision. On the other hand, replicating our previous findings with hypothetical choices would provide support to the proposed role of the different types of uncertainty and their interaction with information about adoption stock. Next, employing hypothetical choice allows us to test our theory with a wider range of product types. Furthermore, instead of manipulating participants' affinity with the product target group directly, we used a continuous self-reported measure of affinity. Observing individuals' behavior at different levels of affinity to the advertised target group allows a deeper understanding of its relationship with CSOA information. A relative large number of online participants in Experiment 3 also allowed us to investigate other dimensions which potentially affects uncertainty. In experiment 3A we let product expertise interact with the other variables that influence uncertainty (CSOA size, information quality and

affinity) and show that the predictions still hold under this complex 4-factor setting. Experiment 3A also demonstrates the effect of information credibility on product uncertainty. Experiment 3B provides evidence suggesting that the importance of stock information in a purchase decision varies as a function of the level of uncertainty and this signal is most informative when uncertainty is moderate.

Experiment 3A (laptop, mattress, and massage)

Experiment 3 included a laptop computer, a mattress, and a massage service extending our investigation to other durable-goods as well as to experience goods. We used a continuous self-reported measure of affinity and let participants report their level of expertise in the product category as an additional (and important) factor influencing customer uncertainty. Also, as we explained earlier, we expect information credibility to have similar effect on product uncertainty as quality information, such that higher message credibility reduces product quality uncertainty, as both inform the signal diagnosticity. We test this assumption by keeping the information provided about one of the products (i.e. mattress) constant, and instead manipulating the credibility of the source of the information provided. This allows for a more general view of the construct underlying uncertainty about quality.⁹

Method

⁹ We thank two reviewers for these last insights.

One thousand, one hundred and thirty online participants ($M_{\text{age}}=30.9$) recruited through Amazon Mechanical Turk took part in a study about “consumer products”¹⁰. In a 3 (product type) x 2 (stock size information) x 2 (product quality information) between subject design, participants read information about a new product and reported whether they would be willing to buy it for a given price, based on the product information provided. One group of participants read a description of *XBWL*, a new laptop designed for avid gamers. A second group of participants read a description of *Solero*, a new mattresses brand which is ideal for individuals with chronic long-term sleep disorder. A third group of participants read a description of DMR, a new massage technique that treats severe muscle pain. Within each product group, we manipulated CSOA size in the same vein as the previous experiments. For example, some participants in the massage group read that “*Hundreds of Thousands of people nationwide who suffer from severe muscle pain have opted to enjoy the benefits of DMR massage*” while others read that “*People who suffer from severe muscle pain have opted to enjoy the benefits of DMR massage.*” In the Laptop and massage condition, we followed the designs of the previous experiments to manipulate the product quality information such that some participants read a detailed description of the products and their benefits while others read a much less informative product description. To test our source credibility assumption, we let those in the mattress conditions read the exact same product description, but manipulated the source of the message, such that some participants were told that the description was “*provided by ConsumerReports.org, the nation’s premier independent product rating*

¹⁰ Nineteen participants failed to answer correctly an attention check question and were excluded from the analysis.

organization”, while others were told the information was taken from “*the www.oldbedguy.com mattress blog.*” The detailed description of the manipulations appears in Appendix C.

After reading the product description, participants reported whether they would buy the product for a given price, based on the information provided (\$1,399, \$1,199 and \$59.99/hr for the laptop, mattress, and massage respectively). Participants then answered 6 additional questions. First, using an analog scale, participants reported the extent to which they felt affinity to the target group highlighted in the product description (e.g., “avid gamers” in the laptop condition) ranging from “Extremely Weak Affinity” to “Extremely Strong Affinity”, resulting in a 0-100 continuous measure. In the second question participants used a similar scale to indicate the degree of product information credibility. An analog scale was also used in the third question where participants indicated “to the best of [their] understanding, how many people have already purchased [product name].” The scale ranged from “Very few people” to “Many people”. Next, participants used a 5-point scale (ranged from “Very little” to “A lot”) to indicate how much they felt they knew about the new product. Using an analog scale, participants also indicated the extent to which the product information provided influenced their level of uncertainty about the quality of the product. Finally, participants used a 7-point scale, ranging from “I Know Nothing about It” to “I’m an Expert” to describe their expertise with the product category (e.g. laptop technologies). This last measure was critical to capture the extent of the a priori product uncertainty. As our hypotheses pertain to various degrees of uncertainty, we would expect expertise, when coupled with clear or vague product descriptions, to form such a range of product uncertainties. The

Experiment concluded with an attention check question and a basic demographics questionnaire.

Results

Manipulation Checks: Participants in the large stock information conditions reported that more people had already bought the product than those in the no stock size information condition ($\beta = 9.33$, $t[1122] = 6.28$, $p < .001$)¹¹, validating our stock size manipulation. In the Laptop and Massage product conditions, those who received detailed product information reported feeling they knew more about the product than those who received vague product information ($\beta = .38$, $t[691] = 5.13$, $p < .001$), confirming our product quality information manipulation. In the Mattress condition, those who read information from ConsumerReport.com indicated the product information was more credible than those who read information from the mattress blog ($\beta = 7.19$, $t[434] = 3.79$, $p < .001$). Moreover, receiving detailed product information in the Laptop and Massage conditions also predicted higher perceived credibility ($\beta = 5.66$, $t[434] = 3.23$, $p < .01$), which confirms our earlier contention that detailed product information may help create trust in the information source. More importantly, source credibility was significantly correlated with the reported reduced uncertainty ($r_{\text{Pearson}} = .15$, $p < .001$).

Choice

We ran a Logit model of the purchase decision on the stock size, product information quality, and the self-reported affinity levels, as well as all their interactions,

¹¹ Six participants were excluded due to missing observations. Also, this significant difference holds for each product separately.

controlling for the product type (Table 3.4). None of the demographic information reached significance and was thus not included in the reported analysis. As a reality check for our affinity measure, the intensity of affinity felt by our participants towards the target group highlighted in the message influenced their choice in a positive manner, in line with the results of Experiment 2. Allowing self-reported expertise to enter the model reveals a significant 4-way interaction with stock-size, information quality, and affinity (see Appendix D for full model). This is to suggest that product expertise should be taken under consideration in the application of our findings, as it is an integral part of underlying product uncertainty. For clarity's sake, we explore the effect of self-reported expertise separately for high-expertise (individuals who reported having a fair knowledge, or better, about the product category; Table 3.4, 2nd column, a total of 645 individuals), and low-expertise customer groups (Table 3.4, 1st column, a total of 485 individuals).

As expected, detailed information and information credibility marginally increased the probability of buying the product for customers with low-expertise ($\beta = 1.12, Z = 1.71, p = .08$), but had no effect on those who perceive themselves as knowledgeable ($\beta = .12, Z = .19, p = .85$)¹². More importantly, once again we did not observe a main effect of information about a large CSOA in any customer group or in the entire sample. However, familiar results emerged for those who reported low product expertise (i.e., those who bear some product uncertainty) for whom the interaction

¹² The interpretation of these results refers to the case where no stock size information provided and affinity is minimal. Further analysis that held affinity at higher levels yielded the expected results: when affinity is average (or higher), detailed information significantly affects experts rather than non-experts ($\beta = .04, Z = .27, p = .85$ and $\beta = .50, Z = .25, p = .049$ respectively). Detailed information in certain categories needs a degree of expertise to process.

between the CSOA information, quality information, and affinity was significant ($\beta = .035$, $Z = 2.19$, $p = .028$), corroborating the context dependence property of CSOA information on new product adoption.

To illustrate the important role of uncertainty on the effect of CSOA information on new product trial, we used the above Logit model to forecast the purchase probability of each product at different levels of affinity, CSOA size, and quality information (Figure 3.3). The results suggest that large CSOA information mostly affects customers who bear some (but not too much) uncertainty. Those may be either knowledgeable customers who do not have sufficient (or credible) product information, or those customers with little product expertise who receive a detailed (or credible) product description. In such cases, the model predicts that large CSOA information should have a negative to no effect, when affinity is low and positive effect when affinity is high. However, two additional results emerge from the model. First, CSOA information may have no effect when uncertainty is minimal: Expert customers who received a detailed (or credible) product description were not affected by large CSOA information. In other words, large CSOA information is expected to play no role in alleviating customer uncertainty (quality and fit) when there is little of it. Second, CSOA information may also have null to negative effect when there is too much uncertainty: Large CSOA information had no effect (and even a detrimental one in some low affinity situations) on those customers with low product expertise who also received vague (or non-credible) product description. This suggests that large CSOA information may play no role in alleviating customer uncertainty (quality and fit) when such uncertainty is too large.

Arguably, some of our most intriguing findings suggest that when large CSOA information is coupled with a vague (rather than informative) product description, the more knowledgeable the target customers are, the greater the positive influence of CSOA signal is on the purchase decision. This suggests that experts would benefit more from stock information in the absence of other means to alleviate their (moderate) product uncertainty. On the contrary, as argued earlier, less knowledgeable customers who encounter vague product description are likely to discard the CSOA signal and avoid the offer all together. We designed Experiment 3B to shed light on this interesting insight.

Experiment 3B (mattress)

Experiment 3B utilized a design similar to Experiment 3A. We also used the mattress stimuli from Experiment 3A, because it had a broad distribution of expertise, but instead of manipulating the source credibility, we informed participants in all conditions that they are about to read product information provided by the firm. After reading the information and making a purchase decision, we asked participants to reflect on their decision and answer questions relating to the importance of the stock size and product description in making their decision.

Method

Six hundreds and ten Mturk participants (61.5% Males, $M_{age} = 31.2$ years), completed a similar “consumer decision” task to that of Experiments 3A. In a 2 (CSOA: small vs. large) x 2 (product quality information: vague vs. informative) between subject

design, participants read information about a ‘Solero Mattress’ and used a 0-100 point scale (to report the likelihood they would buy the mattress for the introductory price of \$699 (labeled Very unlikely to Very likely). Following the purchase decision, participants used analog scales (0-100) to report how informative they found the product description provided, and the extent to which they found the information about the people who were already using a Solero mattress important to their purchase decision. Participants also answered manipulation check questions, as well as questions about their expertise and the perceived credibility of the information provided.

Results

Participants’ responses to the two questions about the perceived current adopters and the level of product information confirmed our manipulations ($\beta = 4.65$, $t[606] = 2.7$, $p < .001$ and $\beta = 0.43$, $t[608] = 6.26$, $p < .001$, respectively). Most importantly, the extent to which CSOA information played an important role in participants’ purchase decisions varied as a function of their (self-reported) expertise, and the quality of the product information they received. Table 3.5 summarizes the regression results of stock information importance on product information quality and expertise. As shown, both information quality and expertise positively affected how important CSOA information was to the purchase decision ($\beta = 18.39$, $t[601] = 2.33$, $p = .02$ and $\beta = 5.6$, $t[601] = 4.42$, $p < .001$, respectively). However, the significant interaction suggests that the effect of expertise is moderated by information quality. That is, a more informative product description decreases the importance of CSOA information for experts ($\beta = -3.85$, $t[597] = 2.03$, $p = .042$). In other words, as predicted CSOA information and an informative

description are substitutes in decreasing uncertainty for the more knowledgeable customers. As evident in Figure 3.4, the influence of expertise on the importance of CSOA information to the purchase decision was mainly driven by the negative effect of the vague product description. Expert consumers deem CSOA information more valuable to their purchase decision when the product description is vague, rather than clear. Conversely, as before, novice consumers who lack sufficient product information are less likely to be affected by the CSOA signal, because of the high uncertainty state they are in.

GENERAL DISCUSSION

By and large, most if not all new product frameworks in economics and marketing as well as lay beliefs hold that the larger the current stock of adoption of a new product, the greater the likelihood of additional adoption. Employing both controlled experiments and a field experiment we show that the influence of information about large current stock of adoption on product diffusion is more complicated than the commonplace assumption. That is, not only does information about a large stock of adoption need to refer to high affinity others in order to increase purchase likelihood, consistent with social influence theories, it only does so when some product uncertainty exists. Otherwise, information about a large current stock of adoption may be insignificant or even harm marketing efforts.

Participants in Experiment 1 were more inclined to choose a lottery that offered a newly released book when they received information that thousands of others, similarly-educated, already adopted the book. Such positive influence on the book selection

occurred as long as participants also received diagnostic information about the book. When the book description was vague, the influence of large stock information reversed, decreasing the attractiveness of the book. When the information about current readers referred to low affinity others (e.g., thousands of other dissimilarly-educated book readers) participants did not find this information informative, regardless of the quality of the book description.

In our field experiment (Experiment 2) potential customers approached on the street who were told that a new performance drink was already consumed by thousands of other individuals like them (regardless of group affiliation), were more inclined to buy a trial product than those who did not receive such information. Similar to Experiment 1, information about large current stock of adoption had a positive influence on sales as long as individuals also received a detailed description of the product. When the description of the new drink was vague, including a statement about thousands of other high affinity adopters decreased the likelihood that a potential customer would buy a trial product. Importantly, in this experiment the small CSOA conditions described a small stock in a positive frame (“be among the first”). We discuss below how this frame might cause additional inferences, such as exclusivity or enhanced risk aversion. That our results in these conditions converge with our other experiments suggests that few of these other potential effects were present, and more importantly, that our findings are robust to particular wording or framing of the CSOA signal.

Finally, in Experiment 3A, we replicated and extended these results using 3 hypothetical new products (gaming laptop, mattress, and massage service). In line with the theoretical role of uncertainty, we found expertise with the product category to be an

important qualifier for the aforementioned effects. When experts receive detailed information, the added signal about the current stock of adoption does not further reduce uncertainty, and has no effect. However, when experts do not have detailed information, we find results similar to the other studies, whereby the effect of information about current adoption stock depends on the level of affinity with the adopting stock. For non-experts we replicate the same effects as in the other studies. In addition, we confirmed the role of product uncertainty using a different but conceptually equivalent manipulation: information source credibility. When the same information was presented as coming from a credible source, it reduced uncertainty in a similar manner to detailed information in other studies, but when its source was less credible, behavior conformed to the vague information conditions of other experiments. Moreover, in Experiment 3B, we confirm the assertion that for expert customers information about a large CSOA and an informative product description are substitutes in reducing product uncertainty. Novices, alas, may not be able to use the informative description to reduce as much uncertainty, and still enjoy some uncertainty reduction from the large CSOA signal.

Marketers have long documented the idea that large stock information can increase sales, and recently scholars and practitioners are increasingly investigating the influence of social networks and contagion among customers, on new products adoption (e.g., Godes & Mayzlin 2009; Hartmann et al. 2008). Recent studies also acknowledge the fact that uncertainty about the product characteristics plays a major role in determining customers' product evaluation (Hong & Pavlou 2010). What is less known, however, is how these signals interact, and in particular, how information about a large stock of adoption influences people to try new products in conjunction with (or lack of)

other signals. Testing this broadly held assumption, the main contribution in the current work is in refining the conditions (e.g. marketing information) in which customers are, in fact, influenced by information about current adopters, as well as the directions of these effects. This is the first direct test and demonstration of the fickle role of information about a large current stock of adoption. The findings reported here converge to demonstrate the positive, negative, or sometimes lack of influence of information about a current large stock of adoption on the sales of a new product. The results are of interest both to researchers seeking to better understand the relationships between marketing signals of large stock and different product uncertainty types, and to practitioners seeking to identify the conditions under which large stock information aids product adoption.

Our results stress the relationship between the identity of the adopting stock and its signal value (and sign) to potential customers. From the firm perspective, the identity of the adopting customer stock may be critical to the assessment of success of the new product launched. On the one hand, lead users can serve as opinion leaders, fueling the diffusion process of new innovations (Urban & Von Hippel 1988; Morrison et al. 2004). On the other hand, large initial adoption by certain customer types may at times actually be a negative signal of success (Anderson et al. 2014). Additionally, popularity information has been recently found to be of a greater benefit to products that serve a niche of the market (“narrow-appeal”) than it is to products that suit mainstream tastes (Tucker & Zhang 2011). Together, these complementary works significantly challenge the ubiquity of reliance on the large stock assumption.

Finally, one can interpret our results as providing an additional layer above and beyond that of a large stock of adoption being a proxy for internal market influences, e.g.,

word of mouth (e.g., Bass 1969; Mahajan, Muller, & Bass 1990). Our results stress the informational role or the signal value inherent in a large stock of adoption. As such, existing models of new product diffusion may be enhanced by including this informational effect.

Implications for Marketers

Our findings should allow marketers to more effectively communicate information about stock of adoption and to better understand the scope in which such information would be beneficial. For example, marketers who cannot clearly communicate their product's characteristics (e.g. limited ad space, media choice) or when product quality uncertainty might be high because of the nature of the product category, might prefer to avoid using information about large stock of adoption (but see Mayzlin & Shin 2011). On the other hand, when information about stock of adoption can be coupled with a detailed product description, information about adoption by a large stock of high affinity others might be an effective tactic. Therefore information about large stock of adoption is a marketing tool that should be used with caution. Our findings also help scholars better understand the mechanism underlying product diffusion in the context of information about stock of adoption. Finally, we believe that both practitioners and scholars of marketing should now be able to refine models of new product adoption, and potentially improve diffusion forecasts.

Limitation and Future Research

The first potential limitation of any experimental study lies in the specificity of its design. We attempted to tackle this by using consequential choices of real new-to-the-market products, as well as by employing a field and hypothetical choice experiments. Despite the converging results in five distinctly different product domains supporting generality, signal effects may still be contingent on the nature of the product, the customer, and the information source; more evidence from the field would thus help paint the overall picture.

The current work investigates the effect of large stock information while taking into account the interactions with stock identity and product information quality. Although stock identity and complementary product descriptions are some of the most common signals coinciding with information about current stock of adoption, other types of signals may also interact and could be further investigated. For example, the effect of a large stock can be mediated by the price (e.g., Grewal, Gotlieb & Marmorstein 1994), seller reputation, communication channel, warranty coverage (Shimp & Beardern 1982), customer or culture heterogeneity and even temporal moods or feelings. Although we offered several alternative accounts for our results, we could not include all potential effects within the scope of our experiments. For example, while low seller credibility could potentially account for the negative effect of large stock information in some of the conditions, we did not directly measure the credibility of the seller itself, but only of a 3rd party information source. We leave this deeper investigation to subsequent research.

In the context of our current investigation sending a message about a small current stock of adoption would be an unambiguous negative signal. However, one can see how such a message can signal exclusivity and be framed as positive. In such cases,

we can think of the CSOA signal as having multiple levels and dimensions. Moreover, a signal about a small CSOA might lead to increased motivation of innovators or early adopters, while at the same time deter the more risk averse customers. Alas, fully investigating the breadth of potential effects and the generality of our results to such product categories is beyond the scope of the current work, as well.

The current research uses the affinity to the adopting stock as a proxy for reduction of uncertainty regarding product fit. While we mostly used affinity as a high-low scale, it may either range from positive to negative, where low affinity adoption reflects negatively on the product, or start at zero, where low affinity is simply non-diagnostic. We suspect this distinction would determine the existence of a negative effect for the former, but a null effect for the latter. Moreover, there may potentially be many other ways for customers to reduce this uncertainty, such as role-models, expert opinions, geographic and/or group membership (Godes & Ofek 2004; Bell & Song 2007; Grinblatt et al. 2008; Manchanda et al. 2008; Duflo & Saez 2003; Gleaser & Sacerdote 2010). Any of these may influence customer perceptions of product fit, and may potentially lead to somewhat different interactions. Since we base our predictions on broad rather than specific theories of social influence, our best guess is that the same results would hold, but this remains an empirical question.

A related topic of interest may be the particular type of uncertainty affected by the CSOA signal. While some of our results can speak to when uncertainty about quality is reduced, and when uncertainty about fit is diminished, we cannot conclude with a simple statement because of the interactive nature of our results. To see this, one can look to a high affinity CSOA signal as reducing uncertainty about fit, but it also may reduce

quality uncertainty at the same time. For example, Tucker and Zhang (2011) propose that popularity information may actually be of greater benefit to narrow-appeal products because narrow-appeal products are less likely to attract customers, and when they are actually chosen this choice conveys a greater quality signal to other customers. Conversely, a CSOA signal of low affinity may or may not signal low quality, and as mentioned above, be a negative or a non-informative signal of fit. It is therefore the subject of specific inquiry, holding some of these variables constant that may be able to shed light into this particular question.

Finally, it is worth noting that stock identity can in itself also serve as a signal of high product quality irrespective of stock size or level of similarity. This is because early adopters, less loyal, and heavy users have been shown to have greater impact on subsequent adoption (Iyengar, Van den Butle & Valente 2011; Godes & Mayzlin 2009; Li & Hitt 2008), and thus information about a small set of the right type of customers may be perceived as very valuable. While we attempted to design the stimuli to reduce such dual effects, and check the robustness of our results to particular affinity formulations, we cannot be certain to fully control for this potential confound. If it is indeed still present in our results, our findings should be read as relative as opposed to absolute (e.g., when greater quality uncertainty is reduced relative to fit uncertainty, and so forth). The qualitative nature of our findings, however, remains the same.

Table 3.1. The effect of a Large Current Stock of Adoption (CSOA) signal on the purchase likelihood in different levels of Affinity and Uncertainty.

		UNCERTAINTY		
		Very Low	Moderate	Too High
AFFINITY	Low	No Effect	None to Negative Effect (Low Fit vs. Potential Quality)	No Effect (Floor)
	High		Positive Effect (Diagnosticity)	None to Negative Effect (Credibility)

Table 3.2. Experiment 1- Book lottery choice logit models results

Predictor	β no controls	β full model
Intercept	-1.67 *** (0.15)	-4.95 *** (0.50)
CSOA Size	0.16 (0.21)	0.13 (0.22)
Affinity	0.07 (0.29)	0.10 (0.31)
Information Quality	-0.11 (0.30)	-0.40 (0.32)
CSOA Size x Affinity	-0.18 (0.41)	-0.18 (0.44)
CSOA Size x Information Quality	0.50 (0.41)	0.83 (0.44)
Affinity x Information Quality	-0.81 (0.60)	-0.71 (0.63)
CSOA Size x Affinity x Information Quality	2.25 ** (0.83)	2.34 ** (0.89)
Education		0.19 * (0.07)
Risk Taking		0.58 ** (0.08)
χ^2 [d.f.]	700[692]	693.3[690]
AIC	655.05	580.85
BIC	691.46	626.36

Notes. Standard errors are presented in parentheses below parameter estimates. Significant codes: *** $p < .001$ ** $p < .01$ * $p < .05$

Table 3.3. Experiment 2 – Performance drink sales logit models results

Predictor	Original Models		Robustness Models	
	β no controls	β full model	β no controls	β full model
Intercept	-2.48 ** (0.71)	-7.88 (1.76)	-2.37 *** (0.68)	-8.06 (1.79)
CSOA Size	-.27 (.67)	-1.02 (.86)	-.19 (0.61)	-.79 (.80)
Information Quality	.08 (.55)	.68 (1.62)	.79 (.53)	.72 (.62)
Affinity	1.05 (1.49)	.86 (.55)	1.41 † (0.85)	1.73 † (0.98)
CSOA Size x Affinity	-1.57 * (.73)	-1.78 † (.93)	-1.75 (1.22)	-2.79 † (1.60)
CSOA Size x Information Quality	.38 (.77)	1.62 (1.01)	.32 (.73)	1.39 (.96)
Affinity x Information Quality	-1.90** (.59)	-1.57 * (.69)	-2.85 ** (1.05)	2.61 * (1.23)
CSOA Size x Affinity x Information Quality	2.56 ** (.86)	2.3 * (1.07)	3.69 * (1.46)	3.57 † (1.84)
Energy Drink Consumption		1.50 *** (0.33)		1.53 *** (0.33)
Risk Taking		.34 * (0.16)		0.37 * (0.16)
Age		0.16 *** (0.04)		0.15 *** (0.04)
Gender		-1.47 ** (0.50)		-1.43 ** (0.49)
Salesperson Fixed-effect	√	√	√	√
n	404	382	405	382
χ^2 [d.f.]	394.6[385]	396.9[359]	378.9[386]	400.2[359]
AIC	323.2	253.28	327.52	255.3
BIC	399.22	344.02	403.59	346.05

Notes: Standard errors are presented in parentheses below parameter estimates. Fixed-effects coefficient estimations are not shown. The full model also includes medium affinity coefficient estimations (main and interactions) but due to space consideration, only the effects of high affinity are presented. We report the entire model estimations in the web appendix.

Significant codes: *** $p < .001$ ** $p < .01$ * $p < .05$ † $p < 0.1$

Table 3.4. Experiment 3 – Online participants choice

Predictor	β low-expertise	β high-expertise	β entire sample
Intercept	-3.88 *** (0.78)	-1.88 *** (0.46)	-2.42 *** (0.36)
CSOA Size (CS)	0.40 (0.68)	-0.30 (0.63)	-0.45 (0.46)
Information Quality (IQ)	1.12 † (0.65)	0.12 (0.64)	0.46† (0.44)
Affinity (ANF)	0.03 *** (0.008)	0.012 * (0.006)	0.021 *** (0.004)
CS x ANF	-0.006 (0.011)	0.01 (0.009)	0.004 (0.007)
CS x IQ	-1.66 † (0.94)	0.28 (0.89)	-0.39 (0.63)
ANF x IQ	-0.02 † (0.01)	0.007 (0.009)	-0.02 (0.007)
CS x ANF x IQ	0.035 * (0.015)	-0.01 (0.013)	0.005 (0.001)
Product - Massage	2.00 *** (0.60)	1.49 *** (0.22)	1.20 *** (0.18)
Product - Mattress	2.03 *** (0.60)	1.12 *** (0.21)	1.12 *** (0.18)
χ^2 [d.f.]	590.31[475]	788.11[635]	1405[1120]
AIC	610.31	808.11	1425
BIC	652.14	852.80	1475.27

Notes: Standard errors are presented in parentheses below parameter estimates. Significant codes: *** $p < .001$ ** $p < .01$ * $p < .05$ † $p < 0.1$. Product effects are relative to the base product (Laptop).

Table 3.5. Experiment 3B – The impact of CSOA information of the mattress purchase decision

Predictor	β
Intercept	36.64 *** (5.26)
Information Quality (IQ)	18.39 * (7.89)
Expertise	5.60 *** (1.27)
IQ x Expertise	-3.85 * (1.89)

Notes: Standard errors are presented in parentheses below parameter estimates. Significant codes: *** $p < .001$ * $p < .05$.

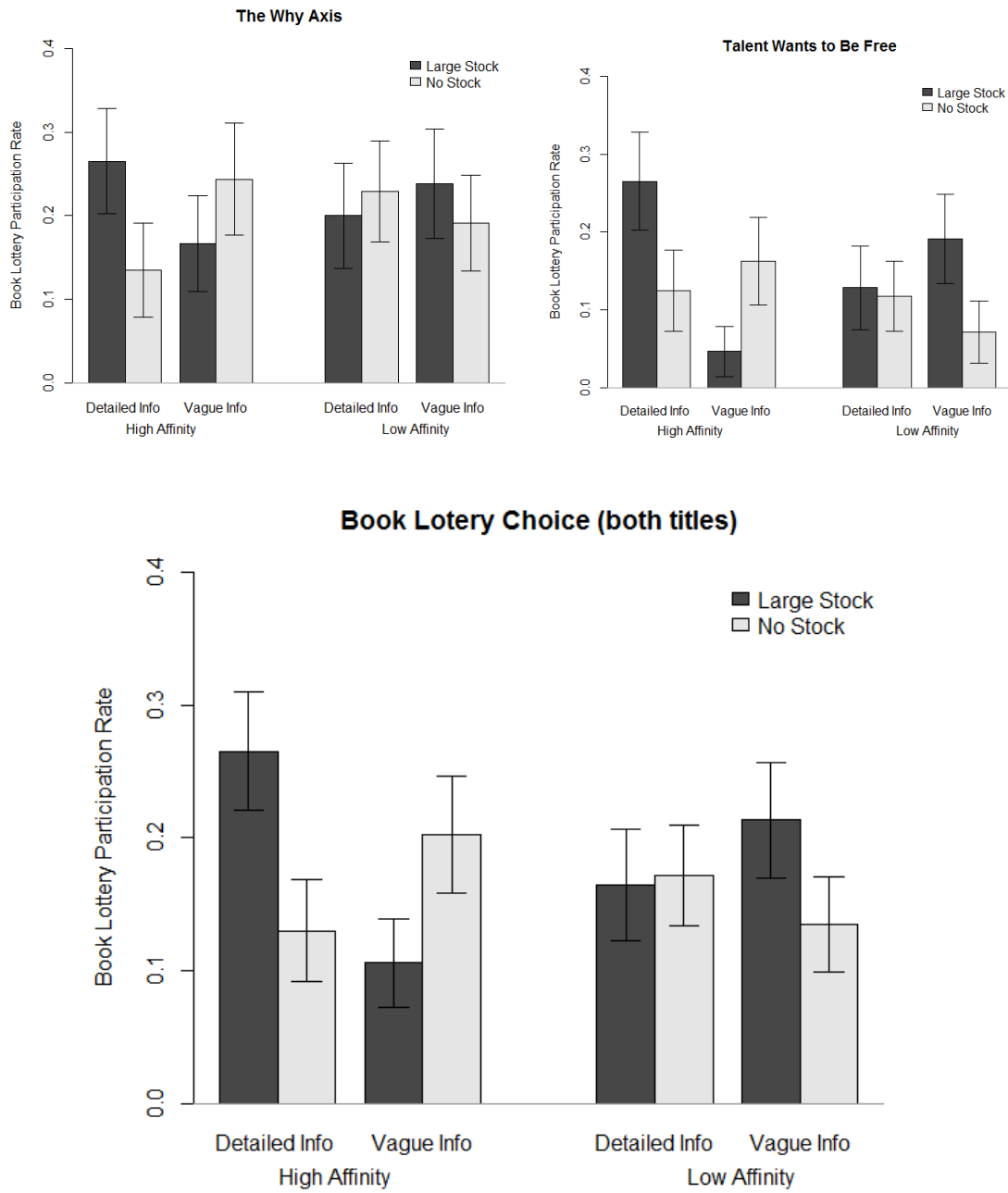


Figure 3.1. Experiment 1 - Purchase rate of book lotteries across conditions

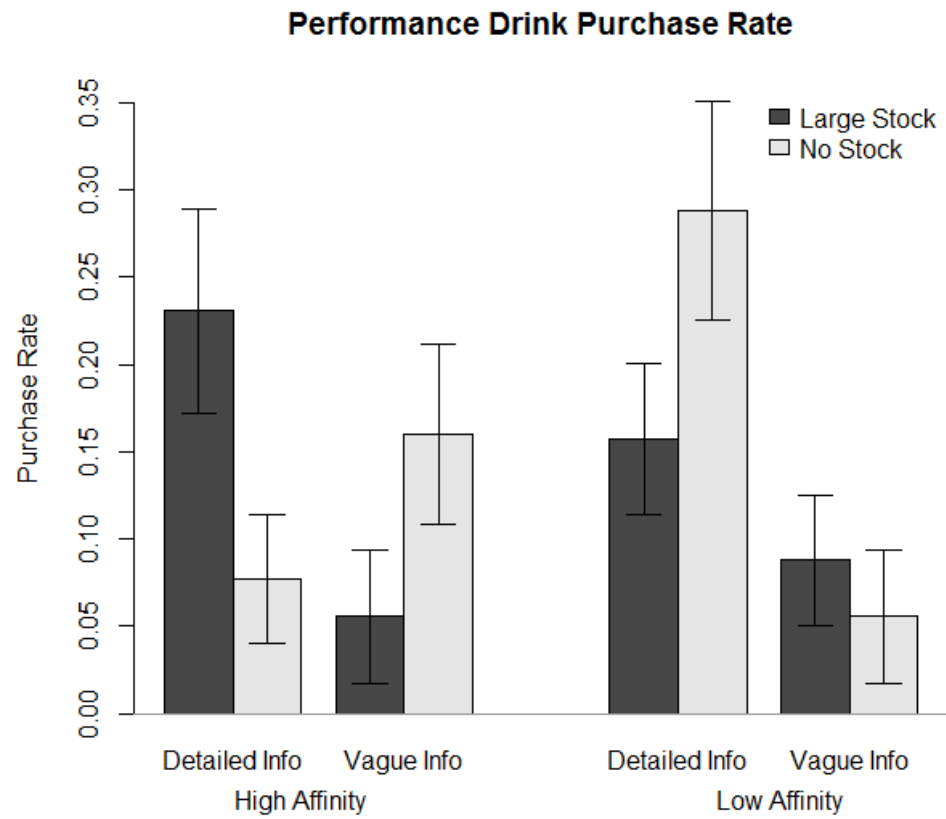


Figure 3.2. Experiment 2 - Purchase rate of performance drink across conditions

Figure 3a: Low-Expertise

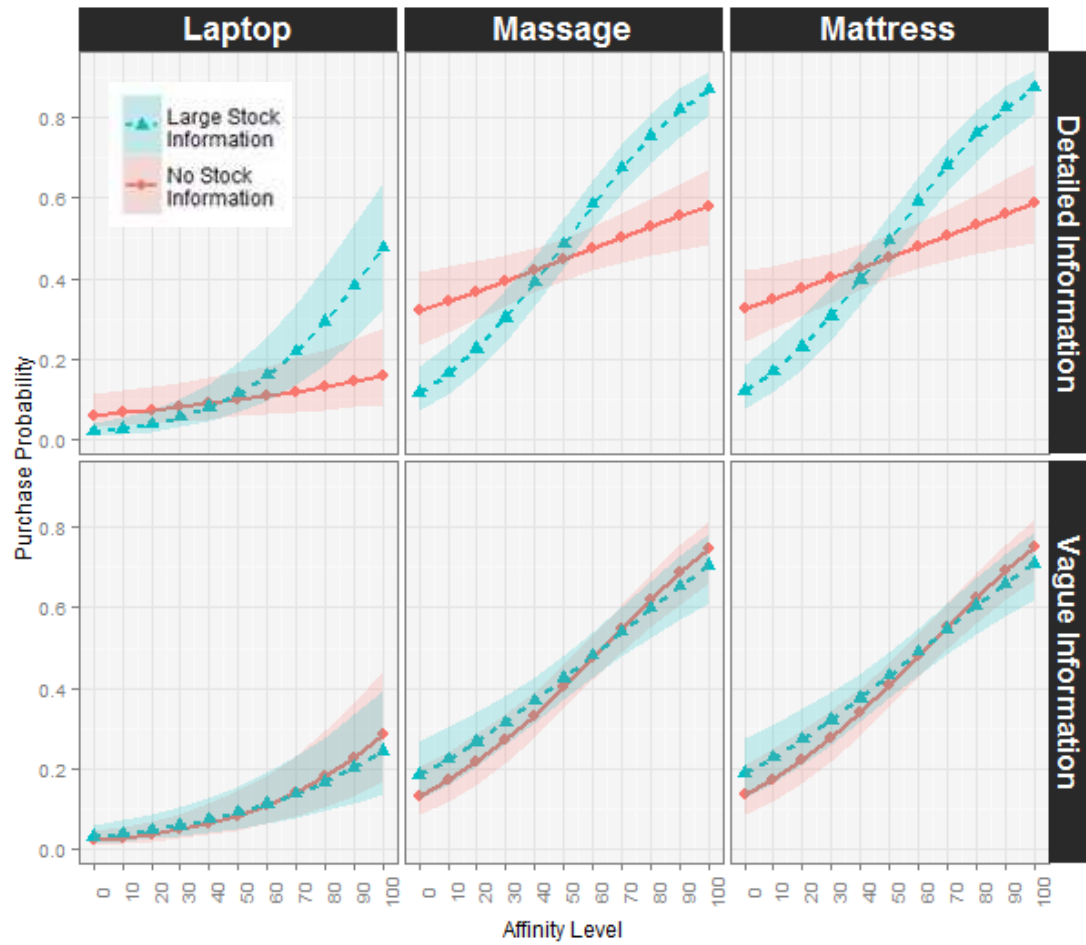
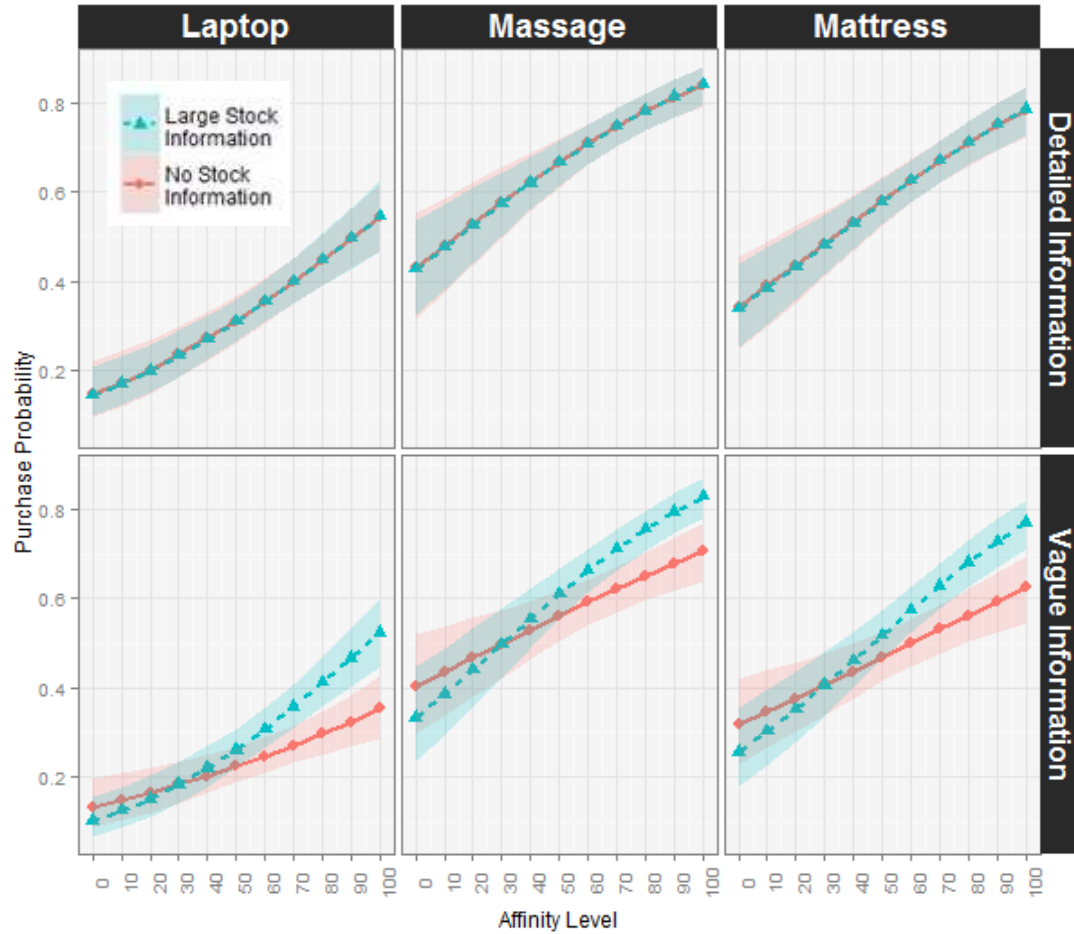


Figure 3.3. Experiment 3 - Prediction models of purchase probabilities

Figure 3b: High-Expertise



Notes: The shaded area around the prediction line represents one standard error above and below the point estimate.

Figure 3.3. Experiment 3 - Prediction models of purchase probabilities, cont.

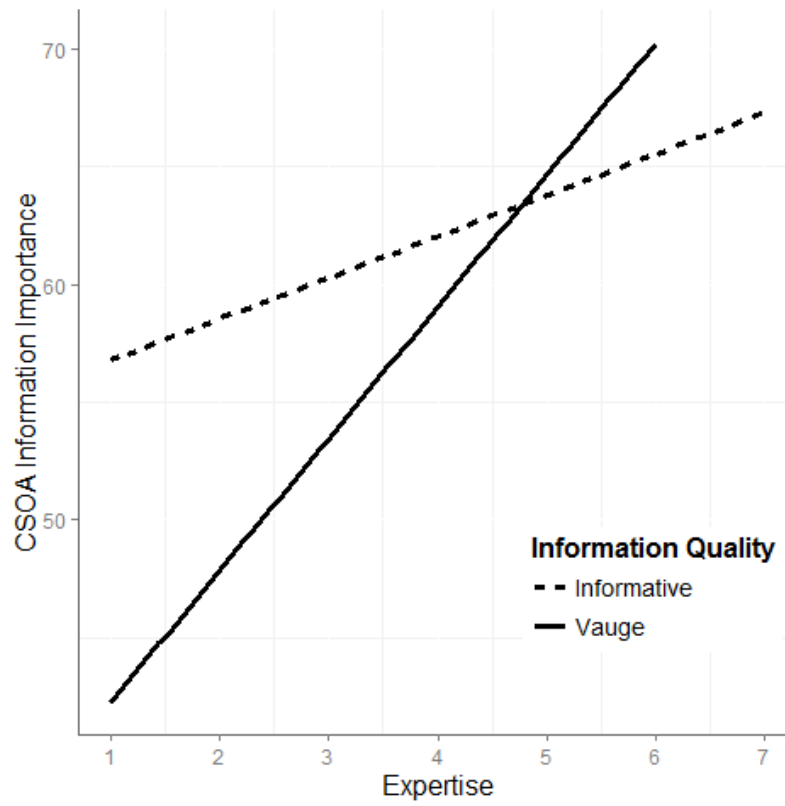


Figure 3.4. Experiment 3B - Importance of CSOA information as a function of expertise and information quality

APPENDIX

Appendix A

Scripted information from the “The why Axis” book study of the condition of large stock, high fit, and high quality information

As an ALTERNATIVE to participating in the lottery for a cash reward, you may choose to participate in a different lottery that awards a copy of the “The Why Axis”. The book is currently offered on Amazon for \$17 (hard cover) and is rated 4.7 out of 5 stars. This alternative lottery affords a much higher chance of winning. A total of 50 books will be raffled among participants in this study who chose the alternative lottery.

*Here is some more information about the book: “The Why Axis” has been released only few weeks ago **but already attracted thousands of graduate-degree holding, highbrow individuals** who are interested in a better understanding of the motives underlying human behavior. The authors’ ideas and methods for revealing what really works in addressing big social, business, and economic problems give the readers new understanding of what drives people’s behavior. Gneezy and List’s pioneering approach is to embed themselves in the factories, schools, communities, and offices where people work, live, and play. Then, through large-scale field experiments conducted “in the wild,” Gneezy and List observe people in their natural environments without them being aware that they are observed. To get the answers to their questions, Gneezy and List boarded planes, helicopters, trains, and automobiles to embark on journeys from the foothills of Kilimanjaro to California wineries; from sultry northern India to the chilly streets of Chicago; from the playgrounds of schools in Israel to the boardrooms of some of the world’s largest corporations. In “The Why Axis” the authors take us along for the ride, and through engaging and colorful stories, present lessons with big payoffs.*

Please indicate below if you are willing to participate in the lottery for “The Why Axis” Book IN EXCHANGE FOR the lottery for a cash reward.

Experiment 1 – Row regression

Experiment 1- Book lottery choice

Predictor	β no controls	β full model
Intercept	-1.86 *** (0.31)	-4.98 *** (0.55)
CSOA Size	0.55 (0.40)	0.38 (0.43)
Affinity	0.49 (0.41)	0.46 (0.43)
Information Quality	0.28 (0.41)	-0.05 (0.43)
CSOA Size x Affinity	-1.31 * (0.60)	-1.37 * (0.64)
CSOA Size x Information Quality	-0.60 (0.57)	-0.31 (0.61)
Affinity x Information Quality	-0.81 (0.60)	-0.71 (0.63)
CSOA Size x Affinity x Information Quality	2.25 ** (0.83)	2.34 ** (0.89)
Education		0.19 * (0.07)
Risk Taking		0.58 *** (0.08)
χ^2 [d.f.]	700[692]	693.3[690]
AIC	655.05	580.85
BIC	691.46	626.36

Notes. Standard errors are presented in parentheses below parameter estimates. Significant codes: *** $p < .001$ ** $p < .01$ * $p < .05$

Appendix B

Field Experiment information scripts for a new supplement drink

Clear Product Description - High Fit -Large Stock

[Product Name] is a new innovative performance supplement.

It is an **All-Natural** performance drink **specifically formulated for Surfers**.

Thousands of Surfers already use [Product Name] every day.

As part of onetime market study, I'd like to offer you to join **all the local surfers** already using [Product Name] and see how [Product Name] can boost your performance, for only a fraction of its actual cost.

[Product Name] is free of artificial sweeteners, and **contains Super-Fruits** like Acai berry, Goji berry, Noni fruit and Pomegranate that provide a boatload of healthy antioxidants.

[Product Name] is designed to provide surfers with:

1. **High-Performance and Natural Energy**.
2. **Better Hydration** by incorporating a blend of electrolytes.
3. **Better Metabolism** that lowers body fat by incorporating botanicals like Garcinia Cambogia and Green tea.
4. **Better Muscle Recovery** by incorporating nutrients like l-carnitine l-tartrate, l-tyrosine and magnesium.
5. **Enhanced Immunity** by incorporating vitamins C and D and other nutrition's like alpha lipoic acid.

Clear Product Description - Low Fit - Large Stock

[Product Name] is a new innovative performance supplement.

It is an **All-Natural** performance drink **formulated for water men and women and for everyone who is enthusiastic about sport**.

Thousands of people already use [Product Name] every day.

As part of onetime market study, I'd like to offer you to join **all the people** already using [Product Name] and see how [Product Name] can boost your performance, for only a fraction of its actual cost.

[Product Name] is free of artificial sweeteners, and **contains Super-Fruits** like Acai berry, Goji berry, Noni fruit and Pomegranate that provide a boatload of healthy antioxidants.

[Product Name] is designed to provide surfers with:

1. **High-Performance and Natural Energy**.
2. **Better Hydration** by incorporating a blend of electrolytes.
3. **Better Metabolism** that lowers body fat by incorporating botanicals like Garcinia Cambogia and Green tea.
4. **Better Muscle Recovery** by incorporating nutrients like l-carnitine l-tartrate, l-tyrosine and magnesium.
5. **Enhanced Immunity** by incorporating vitamins C and D and other nutrition's like alpha lipoic acid.

Clear Product Description - High Fit -No Stock Size

[Product Name] is a new innovative performance supplement.

It is an **All-Natural** performance drink **specifically formulated for Surfers**.

As part of onetime market study, I'd like to offer you **to be one of the first to try** [Product Name] and see how [Product Name] can boost your performance, for only a fraction of its actual cost.

[Product Name] is free of artificial sweeteners, and **contains Super-Fruits** like Acai berry, Goji berry, Noni fruit and Pomegranate that provide a boatload of healthy antioxidants.

[Product Name] is designed to provide surfers with:

1. **High-Performance and Natural Energy**.
2. **Better Hydration** by incorporating a blend of electrolytes.
3. **Better Metabolism** that lowers body fat by incorporating botanicals like Garcinia Cambogia and Green tea.
4. **Better Muscle Recovery** by incorporating nutrients like l-carnitine l-tartrate, l-tyrosine and magnesium.
5. **Enhanced Immunity** by incorporating vitamins C and D and other nutrition's like alpha lipoic acid.

Clear Product Description - Low Fit - No Stock Size

[Product Name] is a new innovative performance supplement.

It is an **All-Natural** performance drink **formulated for water men and women and for everyone who is enthusiastic about sport**.

As part of onetime market study, I'd like to offer you **to be one of the first to try** [Product Name] and see how [Product Name] can boost your performance, for only a fraction of its actual cost.

[Product Name] is free of artificial sweeteners, and **contains Super-Fruits** like Acai berry, Goji berry, Noni fruit and Pomegranate that provide a boatload of healthy antioxidants.

[Product Name] is designed to provide surfers with:

1. **High-Performance and Natural Energy**.
2. **Better Hydration** by incorporating a blend of electrolytes.
3. **Better Metabolism** that lowers body fat by incorporating botanicals like Garcinia Cambogia and Green tea.
4. **Better Muscle Recovery** by incorporating nutrients like l-carnitine l-tartrate, l-tyrosine and magnesium.
5. **Enhanced Immunity** by incorporating vitamins C and D and other nutrition's like alpha lipoic acid.

Vague Product Description - High Fit -Large Stock

[Product Name] is a new innovative performance supplement.

It is an **All-Natural** performance drink **specifically formulated for Surfers**.

Thousands of Surfers already use [Product Name] every day.

As part of onetime market study, I'd like to offer you to join **all the local surfers** already using [Product Name] and see how [Product Name] can boost your performance, for only a fraction of its actual cost.

[Product Name] is all-around performance supplement that scores a whole lot more than just plain energy.

Vague Product Description - Low Fit - Large Stock

[Product Name] is a new innovative performance supplement.

It is an **All-Natural** performance drink **formulated for water men and women and for everyone who is enthusiastic about sport**.

Thousands of people already use [Product Name] every day.

As part of onetime market study, I'd like to offer you to join **all the people** already using [Product Name] and see how [Product Name] can boost your performance, for only a fraction of its actual cost.

[Product Name] is all-around performance supplement that scores a whole lot more than just plain energy.

Vague Product Description - High Fit -No Stock Size

[Product Name] is a new innovative performance supplement.

It is an **All-Natural** performance drink **specifically formulated for Surfers**.

As part of onetime market study, I'd like to offer you **to be one of the first to try** [Product Name] and see how [Product Name] can boost your performance, for only a fraction of its actual cost.

[Product Name] is all-around performance supplement that scores a whole lot more than just plain energy.

Vague Product Description - Low Fit - No Stock Size

[Product Name] is a new innovative performance supplement.

It is an **All-Natural** performance drink **formulated for water men and women and for everyone who is enthusiastic about sport**.

As part of onetime market study, I'd like to offer you **to be one of the first to try** [Product Name] and see how [Product Name] can boost your performance, for only a fraction of its actual cost.

[Product Name] is all-around performance supplement that scores a whole lot more than just plain energy.

Experiment 2– Row regressions

Experiment 2 – Performance drink Sales

Predictor	Original Models		Robustness Models	
	β no controls	β full model	β no controls	β full model
Intercept	-3.72 ** (1.16)	-9.41 (2.08)	-3.03 *** (0.91)	-8.88 (1.95)
CSOA Size (CS)	1.45 (1.14)	0.94 (1.25)	0.63 (0.89)	0.52 (1.02)
Information Quality (IQ)	2.75 * (1.09)	2.37 * (1.16)	2.06 * (0.81)	1.95 * (0.91)
Affinity (AFN)	2.30 * (1.12)	1.91 (1.24)	1.41 † (0.81)	1.73 † (0.98)
CS x AFN	-3.29 * (1.61)	-3.74 † (2.02)	-7.51 † (1.22)	-2.79 † (1.60)
CS x IQ	-2.21 † (1.25)	-0.63 (1.41)	-1.41 (1.01)	-0.28 (1.16)
AFN x IQ	-4.11 ** (1.33)	-3.47 * (1.53)	2.85 ** (1.05)	2.61 * (1.23)
CS x AFN x IQ	5.37 ** (1.86)	4.96 * (2.30)	3.69 * (1.46)	3.57 † (1.84)
Energy Drink Consumption		1.51 *** (0.33)		1.53 *** (0.33)
Risk Taking		0.40 * (0.17)		0.37 * (0.16)
Age		0.16 *** (0.04)		0.15 *** (0.04)
Gender		-1.40 ** (0.50)		-1.43 ** (0.49)
Salesperson Fixed-effect	√	√	√	√
n	404	382	405	382
χ^2 [d.f.]	394.2[396]	376.9[377]	378.9[386]	400.2[359]
AIC	329.92	259.33	327.52	255.3
BIC	421.95	365.86	403.59	346.05

Notes: Standard errors are presented in parentheses below parameter estimates. Fixed-effects coefficient estimations are not shown. The full model also includes medium affinity coefficient estimations (main and interactions) but due to space consideration, only the effects of high affinity are presented. We report the entire model estimations in the web appendix.
Significant codes: *** $p < .001$ ** $p < .01$ * $p < .05$ † $p < 0.1$


Web Appendix C

Experiment 3 information scripts


Laptop:

Imagine that while looking for a new powerful laptop you came across a NEW brand – XBWL. On the next page you will find product information from the seller's website. Please read it carefully and answer the questions that follow.

Clear Product Description – Large Stock Size

Thousands of avid gamers have already discovered the power of XBWL laptops	
<p>XBWL is engineered for high-performance gaming anywhere. It combines an incredibly mobile design, outstanding graphics and it is among the most powerful laptops available in the market. The XBWL is powered by the fastest 4th gen Intel core i7 processor, the latest NVIDIA GeForce GTX870M graphics processing unit (GPU), and uses a 256GB solid-state storage technology for boot speeds up to four times faster than a traditional notebook hard-drive. Its Quad HD 2560x1440 display provides stunningly beautiful image quality for the most intensely realistic gaming possible. This ultra-portable laptop features 8GB of fast 1600 MHz DDR3L memory and its integrated Dolby Digital audio system provides an immersive audio quality output that is custom tuned to deliver a cinematic sounds experience. XBWL also comes with a build-in HD webcam and the latest Intel wireless and Bluetooth adapter. Thousands of avid gamers have already discovered the power of XBWL laptops.</p>	

Vague Product Description – No Stock Size

XBWL laptops	
<p>XBWL is engineered for high-performance gaming anywhere. It combines an incredibly mobile design, outstanding graphics and audio quality, and it is among the most powerful laptops available in the market.</p>	

The other two scripts in the Laptop conditions are combinations of the titles and the message bodies of the scripts presented above.

Mattress:

High-Credibility (Experiment 3A)

Imagine that while looking for a new mattress, you came across the product description below, provided by **ConsumerReports.org**, the nation's premier independent product rating organization:

Low-Credibility (Experiment 3A)

Imagine that while looking for a new mattress, you came across the product description below, in the www.oldbedguy.com mattress blog:

Large Stock Size

Thousands of Solero mattresses have already sold to people with chronic long-term sleep disorders

It is well known that uncomfortable mattresses cause sleep troubles. Solero mattresses are ideal for individuals with **chronic long-term sleep disorders**. They also offer a perfect solution for those who want to pay less without compromising quality. They are built with a distinctive high quality feature known as 'coil on coil' construction. The top coil unit features a luxurious individually wrapped coil system that contours and responds perfectly to the body, eliminating most motion transfer. Each individually wrapped coil is separate to ensure that the rest of the bed will not be disturbed by the movement of surrounding coils. This construction coupled with an advanced euro pillow top, can only be found in ultra premium mattresses that cost over \$2,500. Solero are made to last 10 to 15 years and expected to perform consistently well over time. Solero offers 4 different comfort levels to choose from.



Consumer reports rates this mattress as a good value for money.

No Stock Size

Same information as in large stock size but the title was changed to: "Solero mattresses have already sold to people with chronic long-term sleep disorders."

Massage:

Imagine that a national SPA network has recently opened a new massage and spa center in your area. They are specialized in DMR massage technique and their advertisement includes the information provided on the next page. Please carefully read the information and answer the questions that follow.

Clear Product Description – Large Stock Size

Hundreds of Thousands of people nationwide who suffer from severe muscle pain have opted to enjoy the benefits of DMR massage

DMR is a NEW massage technique designed to treat **severe muscle pain**. Headaches, back pain, carpal tunnel syndrome, shin splints, shoulder pain, sciatica, plantar fasciitis, knee problems, and tennis elbow are just a few of the many conditions that can be resolved quickly and permanently with DMR. As a result, DMR treatment may decrease anxiety, enhance sleep quality and improve concentration.

Art consists of over 500 unique treatment protocols that allow providers to identify and correct the specific problems that are affecting each individual patient. Every DMR session is a combination of examination and treatment and is performed by a certified DMR therapist.

Vague Product Description – No Stock Size

People who suffer from severe muscle pain have opted to enjoy the benefits of DMR massage

We provide quality and professional DMR massage therapy. **DMR** is a NEW massage technique designed to treat **severe muscle pain**. Every DMR session is performed by a certified DMR therapist. Like other massage techniques, DMR is good not only for the body but also for the mind and soul.



The other two scripts in the massage conditions are combinations of the titles and the message bodies of the scripts presented above.

Appendix D

Experiment 3. Full model of online participants choice

Predictor	β
Intercept	-4.99 *** (1.07)
CSOA Size (CS)	1.66 (1.35)
Information Quality (IQ)	2.01 (1.40)
Affinity (ANF)	0.052 *** (0.015)
Expertise (EX)	0.68 * (0.28)
CS x ANF	-0.02 (0.02)
CS x IQ	-3.49 † (1.88)
ANF x IQ	-0.04 * (0.02)
CS x EX	0.47 (0.36)
IQ x EX	-0.38 (0.38)
ANF x EX	0.008 * (0.003)
CS x ANF x IQ	0.07 * (0.03)
CS x IQ x EX	0.78 (0.51)
CS x ANF x EX	0.007 (0.005)
ANF x IQ x EX	0.01 † (0.005)
CS x ANF x IQ x EX	-0.02 * (0.007)
Product - Massage	1.49 *** (0.20)
Product - Mattress	1.33 *** (0.20)
χ^2 [d.f.]	1377.5[1112]
AIC	1413.5
BIC	1504.01

Notes: Standard errors are presented in parentheses below parameter estimates. Significant codes: *** $p < .001$ ** $p < .01$ * $p < .05$ † $p < 0.1$

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