INCORPORATING BEHAVIORAL PRINCIPLES IN PRIMARY DATA ANALYSIS WITH APPLICATION TO BEER DEMAND

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Abstract:

Recent advancements in microeconomics have resurrected a need for modern economists to grapple with principles of consumer behavior. This dissertation uses the American beer market as a starting place to present three ways applied researchers can incorporate behavioral principles into economic theory. The first essay uses choice experiments designed to estimate the price sensitivity of alcohol consumption to explore the efficacy of prompts targeted at reducing inattention bias. Upon receiving feedback, inattentive respondents are given the opportunity to re-answer a so-called “trap question” that checks for attentiveness. We find that individuals who miss trap questions and do not correctly revise their responses have significantly different choice patterns as compared to individuals who correctly answer the trap question. The second essay proposes an instrumental variable approach to address the endogeneity issues associated with distinguishing preferences from perceptions. Even after correction, we find beliefs/perceptions substantially affect consumer choices of beer brands, and that perceived taste and brand familiarity are key determinants of choice. In the final essay, we empirically tests the effectiveness of two institutional nudges on the ECE in a field experiment at a bar. Focusing on craft beer sales, we manipulate the number of options on the menu and use institutional nudges (a control menu, a menu with a special prominently displayed, and a menu with Beer Advocate scores). In the field experiment, the ECE was alive and well using the control menu, but the effect reversed itself when the menu included Beer Advocate Scores. Our results suggest the ECE might be turned on and off by manipulating search costs. Taken together, these three essays show that that behavioral principles can enrich understanding of human action as it relates to consumer decision-making.

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CHAPTER I

INTRODUCTION

Over the past half century, demand for value-added products has markedly shifted. Changes in technology, retailing, and incomes have led to a dramatic increase in the number of options from which consumers choose. In the past, more standardized commodities made purchasing decisions simpler, and subsequent policy analyses could be based on simple models (Malone and Lusk, 2016a). For many modern product markets, consumers must now consider an ever-increasing number of product-specific attributes, further complicating purchasing decisions. Estimates indicate that the number of products in an average supermarket has exploded from fewer than 9,000 in 1975 to nearly 47,000 in 2008 (Consumer Reports, 2014). Thus, it has become increasingly difficult for consumers to familiarize themselves with all options before they make a choice, as the cognitive cost of searching through many options is high. In some markets, this can potentially lead to choice overload, where overwhelmed customers might opt out of choosing. Without higher levels of familiarity, many consumer choices are more likely to be the outcome of unconscious perceptions rather than well-defined preferences.

Until now, consumer perceptions have remained a largely unexplored mechanism for empirically differentiating market participants. American beer is one market that exemplifies this drastic increase in search costs over the past half-century. At present, there are more breweries in the United States than ever before, and the growth trend shows little sign of slowing. The number of U.S. breweries has grown from fewer than one hundred in the 1980's to over 4,000 in 2014 (Elzinga et al., 2015). Even large traditional firms are now acquiring mid-sized craft breweries and experimenting with new styles in an effort to expand their product lines and reach new market segments (Malone and Lusk, 2017a). Although the number of breweries has not reached the per capita levels achieved by the United States before Prohibition, the number of beers available in American markets has nonetheless reached astounding levels. In total, the American beer market includes an estimated 30,000 different choices, with the number of options varying from area to area. Even consumers in Southern states like Georgia now have the opportunity to choose what beer they drink from more than 120 different domestic breweries (SeekaBrew, 2017). This proliferation of options makes it very likely that consumers rely heavily on their perceptions to make purchasing decisions. To date, the empirical literature on beer has generally focused on industrial organization and regulatory changes with little discussion of consumer behavior (Malone and Lusk, 2016). While regulatory analyses remain important,[1] models of heterogeneous consumer behavior in the beer market are likely to change policy recommendations.

In the first chapter, we consider the heterogeneity of survey response quality, and propose a method for reducing the consequences of inattention bias. Although survey-based methods are increasingly being used to estimate consumer preferences and judge the merits

[1] One estimate suggests that there are approximately 90,000 federal regulatory burdens in the beer value chain (Malone and Chambers, 2017).

of policy and health interventions, there is growing concern about the quality of survey data. Using choice experiments designed to estimate the price sensitivity of alcohol consumption, this chapter explores the efficacy of prompts targeted at reducing inattention bias. Upon receiving feedback, inattentive respondents are given the opportunity to re-answer a so-called "trap question" that checks for attentiveness. We find that individuals who miss trap questions and do not correctly revise their responses have significantly different choice patterns as compared to individuals who correctly answer the trap question. Adjusting for these inattentive responses has a substantive impact on policy impacts. Our estimates, based on attentive respondents, indicate that a minimum beer price would have to be substantial to significantly reduce beer demand; a minimum price policy that increased the price of macro-brewed beer by 57% would only reduce the share of consumers buying beer by 2.7 to 4.5 percentage points. We show that the ability of a minimum price policy to reduce beer consumption is mitigated by substitution toward craft beers, the prices of which are already higher and thus less impacted by the policy.

The second chapter proposes an instrumental variable approach to address the endogeneity issues associated with distinguishing preferences from perceptions. Recent developments in behavioral economics have prompted interest in identifying how product perceptions and consumer beliefs influence consumer choice. Unfortunately, perceptions and preferences are likely to be endogenous, as there are likely to unobserved factors that influence both. Finding credible instruments for economic work in this area has emerged as a key challenge. Using a branded discrete choice experiment for beer, this chapter uses the control function approach to identify a plausible instrument to correct for the endogeneity problem in a way that is applicable in a wide range of circumstances. Specifically, we build on the assumption that the perceptions of (or beliefs about) a specific product are likely to be correlated with the perceptions of other related products, but that preference for the product of interest are not likely correlated with the perceptions of (or beliefs about) other related products. Results indicate that this method can successfully correct for endogeneity, and that the endogeneity correction significantly affects estimates. Even after correction, we find beliefs/perceptions substantially affect consumer choices. In the context of beer brands, we find that the perceived taste and brand familiarity are key determinants of choice.

The third chapter, published in the *Journal of Behavioral and Experimental Economics*, focuses on a group of market participants who allegedly violate a common assumption in economics models. Research in psychology suggests that, somewhat paradoxically, providing consumers more choices can reduce the likelihood of making a purchase, producing the so-called excessive choice effect (ECE). To the extent an ECE exists, firms have an incentive to alleviate the effect through a variety of institutional nudges that promote consumers to make a choice. This study empirically tests the effectiveness of two institutional nudges on the ECE in a field experiment at a bar. Focusing on craft beer sales, we manipulate the number of options on the menu and use institutional nudges (a control menu, a menu with a special prominently displayed, and a menu with Beer Advocate scores). In the field experiment, the ECE was alive and well using the control menu, but the effect reversed itself when the menu included Beer Advocate Scores. Our results suggest the ECE might be turned on and off by manipulating search costs.

CHAPTER II

RELEASING THE TRAP: A METHOD TO REDUCE INATTENTION BIAS IN SURVEY DATA WITH APPLICATION TO U.S. BEER TAXES

Surveys are a mainstay in the social sciences, and choice experiments, in particular, have become a widely used tool to elicit consumer preferences in the heath economics literature (de Bekker-Grob et al., 2012). Yet eliciting quality responses has become an increasingly difficult task (Curtin, Presser, and Singer, 2005; Meyer, Mok, and Sullivan, 2015). Individuals have limited capacities for processing information, making it perfectly rational for a survey respondent to inattentively complete a survey. In an effort to identify the most problematic participants, some researchers now include “screeners” or “trap questions” to identify which participants are most likely to be inattentive (Oppenheimer, Meyvis, and Davidenko, 2009). Asking questions that have an unambiguously correct answer allow survey designers to identify who is and is not paying attention during the survey. Between a quarter and a half of the respondents in previous surveys miss the trap question, indicating that they are not paying attention (Maniaci and Rogge, 2014). The convention has been to delete these inattentive participants from the sample. However, this practice has the potential to restrict the study's representativeness and external validity (Aronow, Baron, and Pinson, 2016). Furthermore, omitting up to half of a sample is likely to be costly. As such, there is a clear need for a method that might reduce participant inattention without inviting selection bias.

If inattention simply increased noise, the error term might exhibit higher variance, but parameters would remain unbiased. Unfortunately, research has shown that inattention can substantively bias policy-relevant estimates, making inattention bias an important issue for survey research (Malone and Lusk, 2017). In the case of choice experiments, inattentive survey respondents tend to pay less attention to price changes, resulting in higher willingness-to-pay estimates. In this article, we propose an easy-to-implement method to reduce the measurement error associated with inattention bias. Before the battery of choice questions, we ask a question with an obvious answer designed to "trap" inattentive respondents into incorrectly answering. If the participant misses the trap question, we gently nudge participant to pay attention and provide them an opportunity to revise the incorrect response. Our results show that removing participants who do not revise their responses improves data quality. Additionally, we find that this simple reminder can improve subsequent policy recommendations without introducing additional bias.

By applying this method to an online discrete choice experiment for beer, we seek to contribute to an ongoing policy discussion. While alcohol consumption has been heavily studied in the health economics literature (e.g. Wagenaar, Salois, and Komro 2009 identified over a thousand demand elasticity estimates in the literature), most estimates generally focus on beer as a commodity. Moreover, previous approaches have relied on aggregate time-series data that are likely to suffer from problems associated with endogeneity. Only rarely have any segment-specific estimates been published (Toro-González, McCluskey, and Mittelhammer, 2014), making it difficult to determine the consequences of policies that exempt certain types of beer from taxation.

These shortcomings are particularly problematic for the beer market in the United States, where the craft segment beer market has experienced rapid expansion. As such, the United States is home to more breweries today than ever before (Hahn, 2016). While macro brewers like Anheuser-Busch InBev and MillerCoors still comprise nearly three quarters of the domestic market (Tremblay, Tremblay, and Swinnen, 2011), craft breweries comprise the largest growth area for the beer market. Policymakers across the country are considering changes to promote the burgeoning craft breweries, and it has been proposed, for example, to exempt craft beers from certain alcohol taxes. A complete analysis of such public policies requires data on segment- or brand-specific own- and cross-price demand elasticities because some tax policies may induce substitution effects, which may counteract the intended effects of policies (i.e., to reduce the externalities associated with alcohol consumption). To date, few demand studies account for the likely differences between craft and macro segments (Bray, Loomis, and Engelen, 2009; Toro-González, McCluskey, and Mittelhammer, 2014), and those that have focused on purchases for consumption at home rather than consumption away from home (i.e., at a restaurant or bar). Our choice experiment on beer builds on the existing literature by focusing on macro- and micro-brands in an away-from-home setting, thereby increasing policy-relevant information regarding alcohol taxes, and the projection of the effects of such taxes on public health outcomes.

The overall objective of this article is to estimate the effects of changes in U.S. beer policy on alcohol consumption via a discrete choice experiment that includes a new method to reduce measurement error caused by inattention. In the following section, we provide a background on the literature relevant to inattention bias in surveys and then introduce some of the literature related to beer taxes. In the third section, we describe the data and methods in further detail, providing additional motivation for controlling for inattention, and explicitly describe how we use trap questions with feedback to minimize the consequences of inattention bias. Our aim is to identify a method that might encourage participants to consider their responses more attentively. Fourth, we discuss the results of our discrete choice experiment and implications for beer taxes. The final section concludes with a brief review of our findings and recommendations for future research.

Background

While stated preference techniques can provide more control for making causal inferences, surveys are hampered by quality of responses. This article explores a new approach that minimizes measurement error due to inattention bias. Related to this approach is the use of "cheap talk" and consequential scripts, which inform participants of the tendency to exaggerate willingness-to-pay (Cummings and Taylor, 1999; Jacquemet et al., 2013). Cheap talk scripts can, in some instances, reduce hypothetical bias by reminding participants that their responses are consequential (List, 2001; Lusk, 2003). Unlike cheap-talk and hypothetical-bias, inattention is clearly and quickly identifiable at the individual level, and our method includes a "prompt" only to those respondents who need it.

We focus on choice experiments, as they have become a popular way to determine impacts of consumer-focused policies (Bryan and Dolan, 2004). Dozens of journal articles were published in health economics between 2001 and 2014 that utilized choice experiments, covering a wide range of topics (Clark et al., 2014). As choice experiments have become more common, so too have concerns regarding the validity of the method's results (Bryan and Dolan, 2004; de Bekker-Grob, Ryan, and Gerard, 2012). A common concern is that participants choose to ignore one or more attributes when deciding between alternatives. In some instances, participants might neglect an attribute because they are indifferent to one or all of the attributes or attribute levels (Carlsson et al., 2010). It might also be that the non-attendance is not created by inattention, but rather, the participant simply has a dominant preference for a specific attribute levels, making other attributes irrelevant (Scott, 2002). Regardless of the cause, "attribute non-attendance" has the potential to bias policy parameters (Hole, 2011). To adjust for these issues, some researchers recommend asking participants *ex post* whether they ignored any of the attributes (Hole, Kolstad, and Gyrd-Handsen, 2013) while others infer attribute non-attendance econometrically (Hensher and Greene, 2010). In this study, we focus on a related issue: that some proportion of participants inattentively answer entire survey questions (not just particular attributes), creating measurement error due to inattention bias. That is, we study inattention broadly – as whether someone pays attention to the survey itself – as opposed to the narrow way it has been studied in the attribute non-attendance literature, which confounds preferences for attributes (or lack thereof) with careless inattention.

Inattention Bias

A main purpose of this article is to test a method for identifying inattention while preserving the external validity of the discrete choice experiment. Specifically, we test the effectiveness of providing feedback to individuals who miss "trap questions."[2] These types of questions are

[2] Trap questions are synonymous with "validation" or "red herring" questions. Additionally, "screeners" or Instructional Manipulation Checks (IMCs) are a specific type of trap question where a participant is instructed to

crafted to identify inattention on surveys, often classifying a third to a half of all participants as "satisficing" or inattentive (Oppenheimer, Meyvis, and Davidenko, 2009; Berinsky, Margolis, and Sances, 2014). A typical trap question seeks to identify participants who briefly skim a task's instructions by providing the real sub-directions hidden within the larger overall instructions (e.g., a participant might be asked simply to check "strongly agree" to a particular item). Those who miss trap questions tend to be willing to pay more and are not as consistent in their responses, suggesting the possibility of higher error variance and even bias among people who miss trap questions (Gao, House, and Xie, 2015; Jones, House, and Gao, 2015; Malone and Lusk, 2017).

Often, the convention has been to delete these participants from the sample, as eliminating these observations has been shown to increase statistical power (Oppenheimer, Meyvis, and Davidenko, 2009). However, this convention can prove problematic as data collection is costly, and throwing out responses is akin to throwing away money. Furthermore, deleting these participants has the potential to threaten the survey's external validity by biasing the survey sample (Berinsky, Margolis, and Sances, 2014; Lancsar and Louviere, 2006). Instead, we argue that two types of people might mis-respond to a trap question – the inattentive participant who is unconcerned with providing honest answers to the survey, and the inattentive participant who simply needs to be reminded to pay more attention. Thus, we propose a simple approach that might "rescue" inattentive respondents: provide a simple prompt to people who miss a trap question requesting that they read more carefully.

ignore the response format and select a specific answer (Oppenheimer, Meyvis, and Davidenko 2009; Berinsky, Margolis, and Sances 2014). For clarity, we refer to them throughout as trap questions.

Beer Taxes

An extensive literature has identified negative externalities associated with heavy drinking. Beer constitutes more than half of the ethanol consumed in America, making governments keenly interested in the way policies might influence consumption habits (LaVallee and Yi, 2011). Additionally, beer has been identified as the alcoholic beverage most commonly consumed by binge drinkers (Naimi et al., 2007). Historically, restrictions on alcohol distribution have been used to minimize alcohol consumption (Fosdick and Scott, 1933), but those restrictions have the potential to reduce business development (Malone and Lusk, 2016) and subsequently raise concerns regarding corruption (Gohmann, 2016)

Another classic response to these negative externalities is a Pigouvian tax (Cesur and Kelly, 2014). By incorporating alcohol's negative externalities into the price of a pint, the consumption effects can be more directly targeted. Most research indicates that aggregate beer demand is generally price and income inelastic, making the beverage nearly "recession-proof" (Freeman, 2001) and implying that beer taxes might be less effective than other methods of reducing consumption (Nelson, 2014). Partially in response to these challenges, some policymakers have advocated modified tax structures that effectively impose minimum prices on alcoholic beverages (Ludbrook, 2009; Craven, Marlow, and Shiers, 2013). The notion of taxing alcohol via a minimum price mechanism recently merited a special section of the journal *Alcohol and Alcoholism* (Callinan, Room, and Dietze, 2015). European countries such as Scotland have already implemented a similar policy, and some have argued that the United States should follow suit (Brennan et al., 2015). American policymakers might be especially interested in this tax scheme given the aforementioned shift in the beer market with growing craft-beer consumption.

In fact, current proposals such as the Craft Beverage Modernization and Tax Reform Act already propose differing tax rates based on brewery size.

Of primary interest to this article is the influence of tax policies on consumer choice. We evaluate the consequences of a minimum price per pint, as the effectiveness of this policy is likely to be influenced by substitution effects across beer brands. For this study, we determine the minimum price that would have to be set to reduce beer consumed away from home in the United States by 1%. A choice experiment is appropriate to determine the effects of the policy as some beers would not be subject to this type of pricing policy. As such, substitution effects have the potential to mitigate the impacts of this policy, as consumers are likely to substitute toward similar products such as craft beer, which are not taxed as heavily. Assuming away substitution effects might indicate to policymakers that minimum pricing would more effectively reduce drinking habits than would be the case in reality. In the following section, we explain our data-collection and estimation methods.

Data and Methods

We employ a discrete choice experiment for beer choice with the population of interest being beer drinkers in the United States. Although most commonly used in the transportation, environmental, and agricultural economics literature, the discrete choice experiment has become increasingly popular in the health economics literature (de Bekker-Grob et al., 2012). While there are concerns about so-called “hypothetical bias”, previous research has shown that such bias is less of a problem when estimating marginal changes (Carlsson and Martinsson, 2001; Lusk and Schoeder, 2004). Moreover, prior research has shown that preferences measured via stated preference choice experiments are consistent with those inferred from revealed preference data (Hensher, Lourivere, and Swait, 1998; Louviere, Hensher, and Swait, 2000) and by combining both types of data, improved predictions can be obtained (Brooks and Lusk, 2010; Swait and Andrews, 2003). Moreover, choice experiment data have been shown to exhibit high levels of external validity (Chang, Lusk, and Norwood. 2009). Choice experiments can complement the extant literature on beer demand relying on secondary data in several ways. Choice experiments provide increased control by avoiding potential context-specific confounds. By designing experiments where attributes are uncorrelated with one another, it also avoids endogeneity concerns and concerns about unobserved quality attributes. Moreover, choice experiments allow the researcher to identify individual- specific information more clearly than secondary data.

We utilized a simple “branded” choice experiment, where individuals chose between six different beer brands at a given set of prices. A main effects orthogonal fractional factorial design was employed to assign prices to brands; the final design consisted of eight choice questions in which the price of each brand was uncorrelated with the price of other brands. Each person answered all eight choice questions (Figure 1). To control for effects related to beer type, all beers were lagers. Participants were given the following instructions: “We are interested in the types of beer you like to buy. Imagine you’re at a bar or restaurant. In what follows, we will ask you 8 different choice questions, and in each question we would like to know which type of beer you most prefer when you buy a pint of beer.” Participants chose between six randomly ordered beers: Miller Lite, Budweiser, Sam Adams Boston Lager, Marshall Old Pavilion Pilsner, and Oskar Blues Mama’s Little Yella Pils at price combinations of \$3 and \$6, or respondents could choose “none”. We included Miller Lite and Budweiser to represent domestic macrobreweries. As a proxy for import brands, we used Corona, as it is America’s largest import (Tremblay, Tremblay, and Swinnen, 2011). We included Sam Adams Boston Lager as Sam Adams is the largest American craft brewery. Two smaller beer brands (Mama's Little Yella Pils from Oskar Blues Brewing in Colorado and Old Pavilion Pilsner from Marshall Brewing in Oklahoma) were included to represent microbreweries.

We used a between-subject design to test for the effect of different trap question approaches. Participants were randomly assigned into a control group (N=547), a group given a long version of a trap question (N=559), and a group given a version of a trap question embedded in a Likert scale (N=591). The first trap question directed participants to select "High" on a Likert scale if they live in the United States (Figure 2A). If participants "straight-line" through the scale, they will be unlikely to see these directions, as the instructions originally ask the participant, "How would you rate your familiarity with each of the following brands?" For the second trap question, we use a multiple-choice question where the true instructions direct the participant to click "None of the above" (Figure 2B). Inattentive participants would be more likely to skim the long instructions, and then select their current emotional state. We hypothesize that this style of trap question will catch a higher number of inattentive participants than will the first trap question, as the cognitive effort required for a correct response is higher. Additionally, we hypothesize that a larger portion of participants will correctly revise their response once provided feedback, as a simple reading of the full instructions makes it clear what response is correct. This is likely to occur because Likert scales are often tedious for participants, and requiring an already-inattentive participant to revise responses on a Likert scale is likely to make the process even more tedious.

Instead of simply identifying survey participants who might be inattentive, we notified participants who responded incorrectly. Participants who incorrectly responded were given the following prompt, “You appear to have misunderstood the previous question. Please be sure to read all directions clearly before you respond.” The respondent then had the chance to revise their answers to the trap question they missed.

Data were collected in May 2015 with the population of interest being U.S. beer consumers. Participants were recruited online through the company SSI and the survey was conducted using Qualtrics (N=1,697). SSI recruits its panel of participants through a variety of means including phone calls and online ads, and offers a nominal award of approximately $1.50/survey in gift cards for completing surveys. We utilized a screener question that eliminated non-beer drinkers from the sample. Men make up 54.15% of our sample, and 42.07% of the participants are under 45 years of age. Nearly 15% of our participants live in a city of more than 1,000,000 residents and 34.84% of the participants identified themselves as drinking craft beer at least once a week. Our data indicate millennials aged 21 to 34 tend to be most likely to drink craft beer frequently, while adults 55 years or older are not as likely to drink craft beer.

We estimate a series of random parameter logit models to compare the choice behavior of participants who either correctly responded to the trap question or correctly revised their response and participants who did not correctly revise their response. We define participant *n'*s utility of selecting beer choice *j* in choice option s as:

$$U_{jsn} = \alpha PRICE_{js} + \beta_{jn} + \varepsilon_{jsn} \tag{1}$$

where $PRICE_{js}$ is the price of choice *j* in choice *s*, α is the marginal (dis)utility of the price, β_{jn} indicates the utility of beer *j* relative to the “none” option which is normalized to zero for identification purposes and ε_{jsn} is the unobserved portion of the utility function.

To account for heterogeneity, we estimate random parameter logit models.[3] Each of the "alternative-specific constants" β_{jn} are assumed to follow a normal distribution with mean $\overline{\beta_J}$ and standard deviation σ_j, making the alternative specific constant take on the distribution $\overline{\beta_J} + \lambda_{jn}\sigma_j$, where λ_{jn} is a draw from the standard normal probability distribution function (Train, 2009). Assuming the ε_{jsn} are distributed iid Type I extreme value, the probability of individual *n* choosing beer *j* in option *s* is:

$$(2) \qquad Prob_{njs} = exp(\overline{\beta_J} + \lambda_{jn}\sigma_j) / \sum_{k=1}^{9} exp(\overline{\beta_k} + \lambda_{kn}\sigma_k).$$

The above probability statement contains the random terms, λ_{jn}, which must be integrated out of the likelihood function. To achieve this task, we utilize simulated maximum likelihood estimation and utilize 1,000 Halton draws for each λ_{kn}.

In this study, we provided inattentive participants with the opportunity to revise their incorrect response, effectively "untrapping" themselves and thereby implying that they will be more attentive. As a prelude to the policy analysis, we explore the price sensitivity of attentive and inattentive respondents. In particular, we estimate the change in the probability of *not* purchasing a beer when all beer prices are increased 1% (percentage changes can be found in the Appendix). Base prices for each beer *j* are based on national averages generated by the consulting firm Restaurant Sciences, LLC (Jennings, 2013) where the price of Budweiser and Miller Lite are \$3.50, Corona and Sam Adams are \$5.00, and Marshall and Oskar Blues are \$5.25.

[3] It could be that differences between coefficients across treatments are due to differences in error variance (or scale) rather than differences in underlying preferences (Swait and Louviere, 1993). As such, we also estimated models that allow for differences in scale across treatment, but determined that estimating separate models fit the data better. Results from these tests, along with standard MNL models, are in included in the Appendix.

To estimate the consequences of minimum pricing legislation, we use the same base prices and consider several minimum price policies starting with a minimum price policy of \$3.50/pint and working up (in \$0.50 increments) to a minimum price policy of \$5.50/pint. The outcome of interest is the effect of the policy on the probability of *not* choosing a beer (since the goal of the policy is to reduce consumption). Because craft beers are already higher priced *ex ante*, minimum price policies will have differential effects on macro and craft beers. For example, a \$5.00/pint minimum price policy increases the price of Bud and Miller from \$3.50 to \$5 (a 42.8% increase), but the policy does not affect the prices of the microbreweries (which are already above the minimum at \$5.25). As a result, the policy will cause a shift away from macro to craft beer and not toward "no purchase" as the policy was perhaps intended. To derive standard errors on the probability of "no purchase", we follow the method outlined by Krinsky and Robb (1986) with 1,000 random draws.

Results

547, 559, and 591 participants were randomly assigned to the control, embedded trap question, and long trap question treatment groups, creating at least 4,300 individual choices per treatment. A chief concern about the inclusion of trap questions is the potential for widespread protest-like behavior, although trap questions themselves have not been found to bias responses (Berinsky, Margolis, and Sances, 2014). It might be that the obvious nature of the trap question offends respondents, thereby confounding the results from the choice experiment. It might also be that our feedback has the potential to increase social desirability bias, as warning messages embedded within a choice experiment have been shown to do (Clifford and Jerit 2015). To test this concern, we tested whether the choice experiment model parameters were the same for the treatment and control groups in aggregate. Table 1 shows the random parameter logit estimation results for the control treatment and the full dataset. A likelihood ratio test fails to reject the null that the preference parameters were equal across the two treatments and control, indicating that inclusion of the trap question, *per se*, does not significantly alter parameter estimates ($\chi^2_{df=26,0.05}$=18.6, p-value = 0.881). Another concern regarding the use of trap questions is that they might increase attrition rates. We do not find this to be the case. In fact, our findings indicate the opposite: across all treatments, 91.04% of all participants who opened the survey completed it, while the completion rate for the control group was only 80.6%.

Signs and significance of parameter estimates in table 1 are all as expected. In the full dataset, the average participant appears to derive the most utility from Corona or Sam Adams as the mean utilities are 3.306 and 3.442 (estimated relative to "none" which is normalized to zero for identification). There also appears to be substantial heterogeneity in preferences, as the estimated standard deviations are all over three. In the control group, the random parameter estimate of the mean preference for Oskar Blues was not statistically different from zero, although the estimated standard deviation for Oskar Blues is 4.437, indicating that approximately half of the control sample have a positive utility for the brand, and half negative.

Embedded Trap Question - Results

In the treatment with the trap question embedded in a Likert scale, 21.82% of the 559 participants incorrectly responded to the first iteration. When notified of their wrong response, 53 of the 122 originally incorrect responses changed their response to a correct answer. Relative to the control, a larger percentage of participants in this treatment actually completed the survey, as the completion rate for this treatment was 96.88%. Time to complete did not vary across inattentiveness; participants who responded correctly to the trap question completed the survey in 19 minutes, 47 seconds, while participants who incorrectly responded took 19 minutes, 30 seconds. A likelihood ratio test indicates participants who correctly answered the embedded trap question the first time are not statistically different ($\chi^2_{df=13,0.05} = 20$, p-value = 0.52) from the participants who correctly revised the embedded trap question after we prompt (estimates for these separate groups can be found in the Appendix), suggesting our prompt caused inattentive participants to become attentive.

Table 2 shows the parameter estimates for those in the embedded trap question treatment. Parameter estimates for persons who revised their responses were statistically different from those who did not revise their responses and who were persistently inattentive ($\chi^2_{df=13,0.05} = 48.4$, p-value < 0.001). We conclude that attentive and persistently inattentive participants exhibit different preferences. Estimated standard deviations are all smaller for the persistently inattentive participants than for the attentive participants, and parameter estimates for Corona, Sam Adams, and Marshall are all larger for attentive participants. Most importantly, the parameter estimate for price is significantly lower for attentive participants (-0.613) than for inattentive participants (-0.348). As such, this difference indicates that persistently inattentive participants on average have a lower marginal disutility for price.

Long Trap Question - Results

An even larger percentage of the sample finished in the long trap question treatment versus the embedded trap question treatment as the completion rate was 97.20%. Of the 591 participants randomly allocated to this treatment, 25.38% of participants answered incorrectly to the long version of the trap question. When notified of their wrong answer in the long trap question, 98 of the 150 originally incorrect responses were changed to a correct answer. Incorrect respondents took 13 minutes, 10 seconds on average to complete the survey while correct respondents took 15 minutes, 30 seconds to complete the survey, although this difference was not statistically significant (t-stat =1.16, p-value = 0.24). This group completed the survey in a notably shorter period as participants were not required to complete a Likert-type scale in the same fashion as the previous treatment. A likelihood ratio test indicates that parameter estimates for participants who correctly answered the long trap question the first time were still statistically different ($\chi^2_{df=13,0.05}$ = 183.2, p-value < 0.001) from the participants who correctly revised the long trap question after we prompt (estimates can be found in the Appendix). An additional likelihood ratio test indicates that participants who incorrectly revised and were persistently inattentive generated statistically different parameter estimates than did the participants who correctly revised their response to the long trap question after the prompt ($\chi^2_{df=13,0.05} = 49.4$, p-value < 0.001). In other words, while participants who revised were still different from the participants who responded correctly, they were also different from the persistently inattentive participants.

Table 3 shows the parameter estimates for those who missed the trap questions and did not revise to correct their wrong response (i.e., those who were persistently inattentive) versus those who did revise or correctly answered the first iteration of the trap question. Parameter estimates for persistently inattentive participants were statistically different from those who did correctly revise their responses or correctly responded to the first iteration of the trap question ($\chi^2_{df=12,0.05}$ = 276.8, p-value < 0.001). Standard deviations are all larger for attentive participants than for inattentive participants, but there is no clear pattern in the differences between random parameter estimates. Most importantly, the parameter estimate for price is significantly higher for attentive participants (-0.829) than for inattentive participants (-0.102). This difference again indicates that persistently inattentive participants appear to have a substantively higher marginal disutility for price. The price parameter difference indicates that demand for beer is more inelastic than actually might be the case, potentially leading to different policy recommendations and elasticities.

Policy Implications

Inattention bias has the potential to significantly influence policy estimates (Malone and Lusk, 2017). Table 4 displays the elasticity of beer for each group of participants. Results from the control group are consistent with the previous literature as the elasticity for beer in this treatment is -0.202%, which closely matches the demand elasticity estimated by Nelson (2014). Participants in the embedded trap question treatment were less price sensitive than those in the long trap question treatment. Both treatments, however, yield different elasticities than the control treatment; demand for beer was more elastic for correct respondents in both treatments than for the control group, even though the 95% confidence intervals of the control and embedded trap question treatments overlapped. Indeed, for both the embedded and long trap questions, the price increases affects persistently inattentive respondents less than correct respondents. Specifically, respondents who did not revise their answers in the embedded trap question treatment only decrease their probability of drinking by -0.094% in response to an across the board 1% increase in beer prices compared to -0.182% for respondents who correctly answered or revised. For the long trap question, respondents who did not revise only decreased their probability of drinking by -0.013% compared to -0.330% for attentive respondents. At this point, it is a bit unclear why the attentive respondents in the long trap question were more elastic than attentive respondents in the embedded trap question. The key point is that the respondents who remained persistently inattentive even after the prompt were less price sensitive than the control or the attentive respondents, and that the elasticities from the persistently inattentive respondents are much more inelastic, and therefore biased, in comparison with what has been estimated in previous literature.

We now turn toward estimating the effects of minimum price policies. Table 5 shows the probability of not selecting a beer for several minimum price policies. When only Budweiser and Miller Lite are priced at the minimum price of $3.50, we estimate between 5.45% and, 8.22% of the control group do not drink beer. From implementing the nudge and omitting persistently inattentive participants in the long trap question treatment, we find that up to 10.99% of our sample would not select a beer. By contrast, the embedded trap question treatment indicates that fewer people would change their drinking habits, although the difference is not statistically different from the control group.

While the intended effect of a minimum pricing policy is to reduce the amount of alcohol consumed, unintended consequences arise when there are substitution possibilities. Because we assumed all of the craft options cost at least $5.00, a minimum beer price might cause some consumers to opt for a craft beer substitute whose price remains unaffected. Table 6 shows the increase in the purchase of craft beers (Sam Adams, Oskar Blues, and Marshall) as the minimum price increases in $0.50 increments. When the minimum price is $4.00, we would expect a one percent increase in the demand for craft beer. Contrast this to the change in the quantity of none, which would take a $5.00 minimum price to achieve a similar change.

Discussion

Instead of generating quality responses, surveys incentivize inattention by tying payment to survey completion. We contribute to the literature on survey quality by testing an *ex ante* method to deal with inattention in choice experiments through feedback. Although not all participants correctly revise their answers, those who did provide statistically different answers. While notifying participants that they incorrectly answered a trap question might not entice all misresponders to pay more attention, it does have the potential to identify the most problematic in the sample set. We take our results to mean that the answers of "untrapped" participants are more consistent with a thoughtful response.

Trap questions can be useful for identifying the inattentive participants who might introduce measurement error, yet not all trap questions are as successful at identifying respondent inattention. Of the two most commonly used trap question, the long trap question appears to be most appropriate for our method. Rather than simply catching inattentive participants, we show that providing feedback to those who incorrectly respond to this trap question significantly alters parameter estimates. Nudging participants with a long trap question appears to most appropriately deal with inattention bias. Practitioners would benefit from taking this approach, as it shows potential to minimize measurement error in online surveys. Future research might consider similar methods for other data-collecting methods as they have the potential to generate different parameter estimates (Mjelde, Kim, and Lee, 2016). It is possible that reminding inattentive participants to pay attention will be incentive enough to reduce inattention bias, but we cannot conclusively claim that simply catching participants entirely eliminate participant inattention.

Even when we control for inattention, we would only expect slight changes in consumption habits from a tax policy. On the upper end, a one percent tax on all beers would not even lead to a 0.2% decrease in the number of beers demanded away from home. It would take a minimum beer price of at least $5.00 to reduce beer consumption by 2% from current levels. Even those changes are likely to be overestimated for heavy drinkers, as it is generally believed that binge drinkers are less sensitive to price than the general population. It is also important to remember that taxes are likely to have varying effects for different socio-economic characteristics. Future research might consider changes in consumer behavior across socio-economic characteristics.

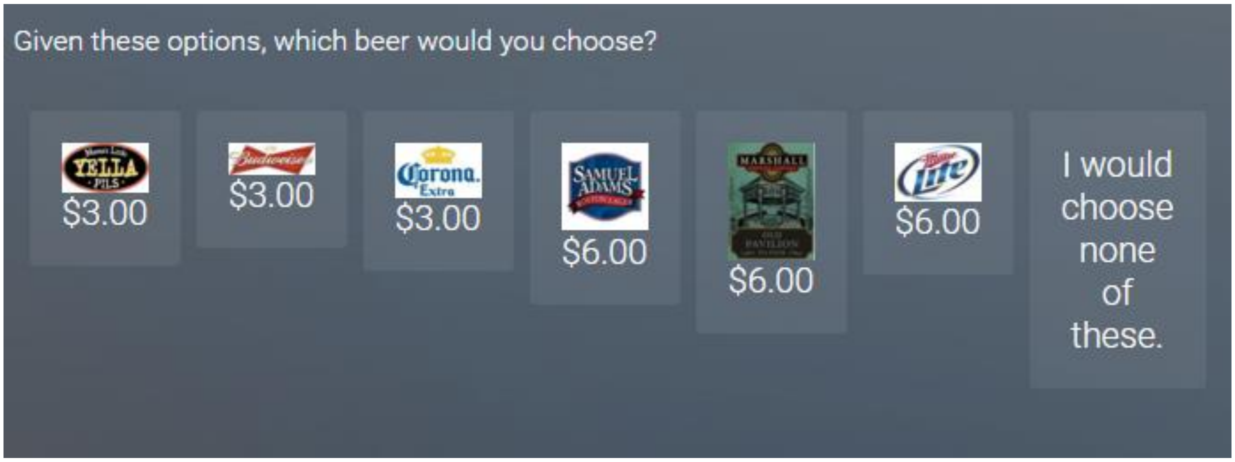
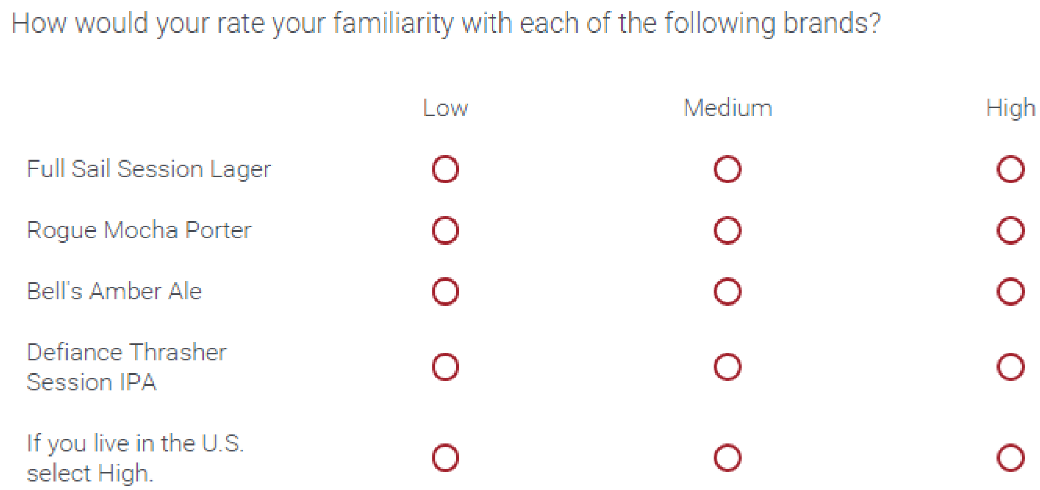


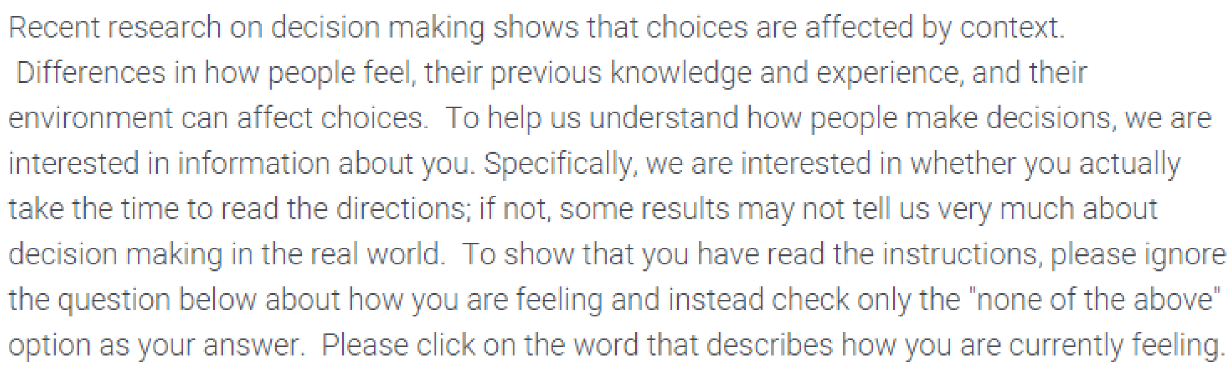
Figure 2.1. Example choice experiment question



How would your rate your familiarity with each of the following brands?

	Low	Medium	High
Full Sail Session Lager	○	○	○
Rogue Mocha Porter	○	○	○
Bell's Amber Ale	○	○	○
Defiance Thrasher Session IPA	○	○	○
If you live in the U.S. select High.	○	○	○

Figure 2.2A. Short Trap Question Embedded in a List



Recent research on decision making shows that choices are affected by context. Differences in how people feel, their previous knowledge and experience, and their environment can affect choices. To help us understand how people make decisions, we are interested in information about you. Specifically, we are interested in whether you actually take the time to read the directions; if not, some results may not tell us very much about decision making in the real world. To show that you have read the instructions, please ignore the question below about how you are feeling and instead check only the "none of the above" option as your answer. Please click on the word that describes how you are currently feeling.

Figure 2.2B. Long Trap Question[a]

[a]Twenty different emotions are listed after the question, with the final option being "none of the above."

Table 2.1. Random parameter logit estimates for treatments and all data

	Control		Long Trap Question		Embedded Trap Question		Full Dataset	
				Mean of random parameter				
Budweiser	1.812*[a]	(0.276)[b]	2.044*	(0.287)	1.892*	(0.297)	1.976*	(0.170)
Miller Lite	0.841*	(0.332)	1.451*	(0.331)	1.009*	(0.354)	1.206*	(0.184)
Corona	3.063*	(0.256)	3.282*	(0.218)	2.530*	(0.276)	3.306*	(0.135)
Sam Adams	4.111*	(0.234)	3.390*	(0.227)	3.504*	(0.243)	3.442*	(0.129)
Oskar Blues	-0.479	(0.375)	0.311	(0.336)	0.103	(0.365)	0.508*	(0.172)
Marshall	0.673*	(0.317)	1.341*	(0.284)	0.668	(0.357)	0.785*	(0.190)
				Nonrandom parameter				
Price	-0.659*	(0.024)	-0.563*	(0.021)	-0.692*	(0.024)	-0.634*	(0.013)
				Standard deviation of random parameter				
Budweiser	4.035*	(0.287)	4.059*	(0.320)	4.468*	(0.296)	4.007*	(0.162)
Miller Lite	4.752*	(0.402)	3.607*	(0.252)	4.157*	(0.307)	4.198*	(0.180)
Corona	3.885*	(0.249)	3.276*	(0.215)	4.351*	(0.238)	3.674*	(0.139)
Sam Adams	3.675*	(0.229)	3.620*	(0.245)	4.095*	(0.278)	4.032*	(0.140)
Oskar Blues	4.437*	(0.348)	3.122*	(0.266)	3.674*	(0.403)	3.159*	(0.136)
Marshall	4.044*	(0.304)	3.053*	(0.234)	3.851*	(0.300)	3.834*	(0.171)
No. of observations		4376		4472		4728		13576
No. of respondents		547		559		591		1697
Log likelihood		-4727.9		-5004.5		-5082.9		-14824.6

[a] * designates statistical significance at the 5% level.
[b] Numbers in parentheses are standard errors.

Table 2.2. Embedded trap question random parameter logit results

	Correct respondents	Correct or revised respondents	Respondents who did not revise	Correctly revised	Incorrectly responded
			Mean of random parameter		
Budweiser	1.253* (0.322)	1.062*[a] (0.392)[b]	1.908* (0.454)	2.835* (0.617)	2.335* (0.378)
Miller Lite	1.656* (0.331)	1.336* (0.336)	1.712* (0.562)	2.960* (0.787)	2.223* (0.436)
Corona	3.363* (0.277)	2.759* (0.345)	2.225* (0.433)	3.661* (0.626)	2.702* (0.376)
Sam Adams	3.696* (0.290)	3.990* (0.311)	1.757* (0.557)	3.014* (0.628)	2.271* (0.373)
Oskar Blues	0.161 (0.351)	-0.118 (0.448)	-0.596 (0.647)	-0.989 (1.639)	-1.014 (0.934)
Marshall	1.657* (0.267)	1.286* (0.317)	-0.996 (0.959)	0.525 (0.858)	-0.607 (0.751)
			Nonrandom parameter		
Price	-0.635* (0.026)	-0.613* (0.024)	-0.348* (0.049)	-0.435* (0.062)	-0.381* (0.039)
			Standard deviation of random parameter		
Budweiser	5.411* (0.450)	4.917* (0.429)	2.570* (0.493)	2.318* (0.499)	2.291* (0.337)
Miller Lite	3.693* (0.303)	4.302* (0.334)	3.164* (0.514)	3.789* (0.754)	3.117* (0.457)
Corona	3.442* (0.262)	3.861* (0.371)	2.195* (0.374)	2.969* (0.559)	2.492* (0.308)
Sam Adams	4.249* (0.357)	3.660* (0.277)	3.025* (0.544)	2.991* (0.603)	3.027* (0.507)
Oskar Blues	3.365* (0.301)	3.715* (0.347)	1.297* (0.360)	3.712* (1.118)	3.374* (0.811)
Marshall	3.234* (0.284)	3.168* (0.256)	3.126* (0.787)	2.120* (0.517)	2.960* (0.530)
No. of observations	3496	3920	552	424	976
No. of respondents	437	490	69	53	122
Log likelihood	-3784.7	-4274.3	-706.0	-479.6	-1188.5

[a] * designates statistical significance at the 5% level.
[b] Numbers in parentheses are standard errors.

Table 2.3. Long trap question random parameter logit results

	Correct respondents	Correct or revised respondents	Respondents who did not revise	Correctly revised	Incorrectly responded
			Mean of random parameter		
Budweiser	2.289* (0.395)	1.692*[a] (0.398)[b]	1.812* (0.464)	1.408* (0.718)	2.014* (0.353)
Miller Lite	1.155* (0.535)	1.425* (0.390)	1.652* (0.419)	1.038* (0.498)	0.907* (0.315)
Corona	4.085* (0.350)	3.232* (0.280)	2.135* (0.469)	1.644* (0.535)	1.788* (0.347)
Sam Adams	4.585* (0.360)	4.094* (0.253)	1.509* (0.552)	2.299* (0.525)	1.871* (0.365)
Oskar Blues	0.793 (0.470)	-0.366 (0.551)	0.898* (0.425)	-0.753 (0.707)	0.504 (0.345)
Marshall	0.785 (0.484)	0.935* (0.368)	-0.419 (0.700)	-0.148 (0.677)	-0.362 (0.471)
			Nonrandom parameter		
Price	-1.055* (0.038)	-0.829* (0.028)	-0.102* (0.047)	-0.324* (0.043)	-0.227* (0.032)
			Standard deviation of random parameter		
Budweiser	5.859* (0.442)	5.246* (0.544)	2.232* (0.461)	3.145* (0.586)	2.330* (0.313)
Miller Lite	5.565* (0.485)	4.691* (0.345)	1.201* (0.316)	2.948* (0.508)	2.523* (0.283)
Corona	5.323* (0.387)	4.962* (0.285)	2.231* (0.412)	4.282* (0.636)	3.414* (0.364)
Sam Adams	5.560* (0.423)	4.362* (0.266)	2.204* (0.441)	3.310* (0.581)	2.823* (0.372)
Oskar Blues	3.669* (0.270)	4.417* (0.479)	0.955* (0.334)	3.024* (0.645)	1.746* (0.326)
Marshall	5.631* (0.407)	4.508* (0.385)	2.692* (0.726)	2.708* (0.485)	2.792* (0.661)
No. of observations	3528	4312	416	784	1200
No. of respondents	441	539	52	98	150
Log likelihood	-3328.5	-4348.8	-595.7	-928.7	-1549.1

[a] * designates statistical significance at the 5% level.
[b] Numbers in parentheses are standard errors.

Table 2.4. Beer elasticities derived by estimating the percent change in the probability chosen given a 1% increase in the price of all the beers

Control	**Embedded TQ**	**Long TQ**
-0.202%	-0.141%	-0.268%
[-0.253, -0.162]	[-0.18, -0.109]	[-0.321, -0.216]
	Correct respondents	
	-0.183%	-0.503%
	[-0.236, -0.139]	[-0.641, -0.378]
	Correct or revised respondents	
	-0.182%	-0.330%
	[-0.232, -0.143]	[-0.408, -0.252]
	Respondents who did not revise	
	-0.094%	-0.013%
	[-0.152, -0.057]	[-0.031, -0.001]

Estimates based on random parameter logit model where attentive participants either correctly responded or revised the trap question. Numbers in brackets are 95% confidence intervals as calculated by the method derived by Krinsky and Robb (1986).

Table 2.5. The probability of not selecting a beer for alternative minium price policies for the attentive participants

Min. Price	**Beers at the Minimum Price**	**Control Group**	**Embedded TQ**		**Long TQ**	
			Correct or Revise	No Revision	Correct or Revise	No Revision
\$3.50	Budweiser, Miller Lite	6.70%	6.49%	6.31%	8.75%	2.96%
		[5.45, 8.22]	[5.19, 8.06]	[4.21, 9.30]	[6.95, 11.15]	[1.70, 4.97]
\$4.00	Budweiser, Miller Lite	7.20%	6.91%	6.71%	9.49%	3.01%
		[5.83, 8.82]	[5.55, 8.59]	[4.45, 9.84]	[7.54, 120.1]	[1.73, 5.05]
\$4.50	Budweiser, Miller Lite	7.71%	7.34%	7.12%	10.24%	3.07%
		[6.24, 9.42]	[5.93, 9.10]	[4.76, 10.36]	[8.18, 12.90]	[1.76, 5.15]
\$5.00	Budweiser, Miller Lite, Sam Adams, Corona	8.20%	7.75%	7.52%	10.99%	3.12%
		[6.65, 9.99]	[6.30, 9.60]	[5.03, 10.91]	[8.81, 13.75]	[1.79, 5.26]
\$5.50	Budweiser, Miller Lite, Sam Adams, Corona, Oskar Blues, Marshall	9.88%	9.24%	8.46%	13.29%	3.26%
		[8.07, 11.97]	[7.53, 11.32]	[5.67, 12.46]	[10.82, 16.49]	[1.87, 5.50]

Numbers in brackets are 95% confidence intervals calculated using 1,000 draws from the method derived by Krinsky and Robb (1986).

Table 2.6. Increase in the market share for craft beer[a] given alternative minium price policies for the attentive participants

Min. Price	**Beers at the Minimum Price**	**Control Group**	**Embedded TQ**		**Long TQ**	
			Correct or Revise	No Revision	Correct or Revise	No Revision
$3.50	Budweiser, Miller Lite	13.47% [8.66, 19.19]	11.87% [8.46, 15.79]	6.53% [3.52, 11.28]	12.14% [8.59, 16.77]	13.85% [8.89, 19.79]
$4.00	Budweiser, Miller Lite	14.00% [9.08, 19.83]	12.80% [9.20, 17.76]	6.81% [3.71, 11.70]	12.76% [9.03, 17.43]	14.00% [8.98, 20.11]
$4.50	Budweiser, Miller Lite	14.52% [9.44, 20.47]	13.35% [9.62, 18.44]	7.12% [3.87, 12.09]	13.32% [9.48, 18.10]	14.17% [9.18, 20.34]
$5.00	Budweiser, Miller Lite, Sam Adams, Corona	15.00% [9.84, 21.07]	13.92% [10.12, 19.05]	7.40% [4.04, 12.50]	13.83% [9.90, 18.73]	14.33% [9.31, 20.56]
$5.50	Budweiser, Miller Lite, Sam Adams, Corona, Oskar Blues, Marshall	15.48% [10.21, 21.65]	14.31% [10.38, 19.56]	7.63% [4.20, 12.86]	14.24% [10.15, 19.20]	14.53% [9.44, 20.78]

[a]Craft beer options in the sample are Sam Adams Boston Lager, Oskar Blues Mama's Little Yella Pils, and Marshall Old Pavillion Pilsner. Numbers in brackets are 95% confidence intervals derived by the method outlined in Krinsky and Robb (1986).

CHAPTER III

AN INSTRUMENTAL VARIABLE APPROACH TO DISTINGUISHING PERCEPTIONS FROM PREFERENCES FOR BEER BRANDS

The idea that perceptions and beliefs influence choice has been embedded in economic models since at least the foundations of expected utility theory if not before (Savage, 1954; Von Neumann and Morgenstern, 1944). As early as Hayek (1952), the effects of "softer" variables such as perceptions have been identified as shaping consumers' choices. He notes, "Perception is thus always an interpretation, the placing of something into one or several classes of objects. [E]ven the so-called elementary sensory qualities are in this sense 'abstractions,' since they are determined by bundles of relationships which we have learnt to attach a certain stimuli which in a physical sense may or may not possess identical properties" (Hayek, 1952; pp. 142-143). Thus, while distorted perceptions can have a meaningful impact on choice, they are not synonymous with preference (Kahneman and Tversky, 2000).

Identifying how perceptions and other psychological variables (i.e. beliefs, attitudes, and opinions) influence preferences has become especially popular in the economics literature of recent (e.g., Costanigro, Deselnicu, and Kroll, 2015; Johnston et. al, 2012; Lundhede et al., 2015; Lusk, Tonsor, and Schroeder, 2014), but psychology and economics have always been "bound together by perception" (Hoyt, 1965; pp. 106). Because economists have traditionally hypothesized that information and other variables affect choice through beliefs or perceptions rather than the preferences directly, there has been a longstanding interest in eliciting consumer perceptions. Alas, perceptions and preferences are likely to be endogenously determined. To date, few reliable options have been proposed to deal with the possible endogeneity problem. Accurately separating perceptions and preferences has significant practical implications, as omitting perceptions from models of consumer behavior can alter policy recommendations (Daly et al., 2012; Just, 2008; Marette, Roe, and Teisl, 2012; Malone and Lusk, 2017a). There is also value in capturing perceptions separate from preferences, as perceptions are posited to be more malleable, making them of additional interest to the effectiveness of advertising campaigns that might seek to exogenously shape perceptions.

The purpose of this study is to describe an instrumental variable method for differentiating perceptions from preferences via the control function approach. In particular, we draw insight from the widely cited method of Hausman (1996) that has been used in the industrial organization literature (e.g., Berry, Levinsohn, and Pakes, 1995; Nevo, 2001). The authors estimate discrete choice demand models, and to address endogeneity of prices in the demand equations, they utilized prices for a good in one location as an instrument for prices of a good in another location. The assumption is that there are common cost or supply-side factors affecting a brand in all locations, but that these are uncorrelated with preference for a brand in a given location. In our case, we utilize a choice experiment to exogenously vary the prices of brands, so price endogeneity is not an issue. However, we are unable to control individual, brand-specific perceptions about each brand. Yet, akin to the logic in Hausman (1996), there are likely genetic or environmental factors that jointly influence perceptions of multiple brands but that are uncorrelated with the preference for a given brand. If so, perceptions of brand A might be used as an instrument for perceptions of brand B and vice versa.

As such, the primary objective of this article is to investigate the aforementioned instrumental variable approach for dealing with the endogeneity bias. We compare this approach to one that uses personality variables, which are generally assumed relatively fixed individual-specific traits, as instruments. Our results come from a national survey of 1,697 participants who are likely to drink beer in the United States. Our estimation method relies on the control function approach for discrete choice models derived by Petrin and Train (2010), which relies on first estimating a series of linear regression models involving the instruments, obtaining the model residuals, and incorporating these residuals as explanatory variables in the discrete choice model.

The remainder of this article is organized as follows. In the following section, we provide a background of how this article relates to other studies that have incorporated perceptions into their models. We then outline our empirical method and further explain the justification for our instruments. In the data section, we describe the design and implementation of a choice experiment conducted to estimate the effect of brand equity perceptions on beer preferences. Beer makes an ideal candidate for this research, as the individual perceptions of each specific brand are likely to substantially influence which pint that consumer purchases. We then compare the results from a random parameter logit model that distinguishes perceptions from preferences but does not control for endogeneity with a random parameter logit model that distinguishes perceptions from preferences but includes our instrumental variables. The final section concludes with limitations and implications for future research.

Background

Choice modeling has become an especially popular tool for estimating consumer preferences, as the discrete choice experiment exhibits high levels of external validity (e.g., Chang, Lusk, and Norwood. 2009). Moreover, prior research suggests that stated preferences derived from hypothetical choice experiments are largely consistent with preferences inferred from revealed preference studies (Hensher, Lourivere, and Swait, 1998; Louviere, Hensher, and Swait, 2000). Even when the discrete choices are non-binding, previous research has shown that such "hypothetical bias" is often less problematic when estimating marginal changes (Lusk and Schoeder, 2004; Carlsson and Martinsson, 2001).

Historically, the discrete choice experiment literature has not differentiated between perceptions and preferences (for exceptions, see Adamowicz et al., 1997; Poor et al., 2001; Provencher, Lewis, and Anderson, 2012). Some researchers recommend incorporating factor analysis into a "hybrid" choice model where latent variables (or beliefs) are estimated, although this method has the potential to generate measurement error (Hensher, Rose, and Greene, 2015). Other research has focused on incorporating probabilistic beliefs into their analysis (Lusk, Tonsor, and Schroeder, 2014). For example, Teisl and Roe (2010) developed and applied a method to determine willingness-to-pay for new safety-promoting food technologies. To control for the endogeneity between subjective perceptions and consumer preferences, the authors provided each participant a randomly selected probability that the products in the choice experiment were contaminated. By subtracting the participant's stated subjective probability of contamination from the randomly assigned exogenous probability, interacting the difference with household demographics, and including these values in the model, the authors argue that they successfully control for endogeneity concerns. In short, the exogenous information provided to participants acted as their identifying instrument.

In contrast to Teisl and Roe (2010), the method we propose in this article can be identified via simple psychometric, Likert-type scales. We take two unique approaches for identifying potential instruments. Our first approach borrows from the approach established by Hausman (1996), as we posit that one can use perceptions about *other* brands as instruments in explaining perceptions about a given brand. The assumption is that whatever supply-side factors are jointly affecting perceptions of all brands are unrelated to preferences for a given brand. In essence, we are assuming that the residual differences across perceptions of taste and brand familiarity capture the unobserved, endogenous heterogeneity likely to confound the causal interpretation of our empirical model. This approach exploits the correlation between preferences for a product and their independence from perceptions for different products. As such, we identify our instrumental variable based on a rationale similar to Berry, Levinsohn, and Pakes (1995) who used prices of an automobile in one location as instruments for prices in other locations. Rather than focus on differences in regional prices across locations, our first instrumental variable approach is based on differences in product perceptions across participants. In other words, the intuition is that while perceptions of other products are likely to be correlated with perceptions of a particular product, they are unlikely to be correlated with preferences of the product in question. For example, taste perceptions for Miller Lite are likely to be correlated with taste perceptions for Budweiser, taste perceptions for Corona, etc.; *however* taste perceptions for Miller Lite are unlikely correlated with the "residual preference" for Budweiser. The argument is that there are likely exogenous factors, like genetics, that are likely to influence taste perceptions of multiple brands that are uncorrelated with factors, like advertising, which influence preferences for a particular brand. While the example is particular to our application, the idea has broad potential in a wide variety of contexts related to food, transportation, health, and environment. For example, perceptions of the beauty or cleanliness of one state park might be used as in instrument for perceived beauty/cleanliness of another state park in a choice model explaining recreational location choice.

To explore the robustness of our results, we also consider a second class of instrumental variables for perceptions based on individual-specific personality traits and demographic characteristics. There has been growing interest in the use of personality in economic work, and research suggests that personality traits are relatively fixed constructs that are heritable (e.g., Almlund et al., 2011). In addition to questions related to consumer demographics, we include scores related to four different personality scales, which were selected as they are likely to be causally related to a consumer's taste perceptions and brand familiarity. First, we include a scale used to capture consumer novelty-seeking (Manning, Bearden, and Madden, 1995), as previous research has shown that novelty-seekers tend to perceive beer brands differently than do consumers who do not seek our novel products (Malone and Lusk, 2017b). Although some consumers might be highly-selective in their taste perceptions, it is also possible that some consumers are not very discriminating in their tastes, and instead simply seek to "satisfice" some unspecified lower preference. In other words, some consumers might have lower a threshold for ranking the quality of a product. We then included a scale commonly used to capture the propensity for a consumer to be maximizing or satisficing, as well as a scale that measures how likely a participant will be regretful in their purchasing choices (Schwartz et al., 2002). Finally, it is possible that happy people are likely to be more optimistic about their taste perceptions of a product with which they are unfamiliar. As such, we include the Subjective Happiness Scale

(Lyubomirsky and Lepper, 1999), which identifies whether participants believe they are more or less happy than what they perceive to be average.

Theoretical Framework

The explanatory power of the discrete choice experiment originates from Lancaster's (1966) observation that a consumer's utility of a good is comprised of the sum of the utility of the characteristics of a good. Following McFadden (1974), we utilize the random utility model (RUM). In its simplest form, let U_{ij} be the utility consumer *i* experiences from selecting the choice alternative *j*:

$$U_{ij} = \beta_j + \alpha \cdot Price_j + \varepsilon_{ij} \tag{1}$$

where $Price_j$ is the price of choice j, α is the marginal (dis)utility of the price, β_j indicates the utility of beer *j* relative to the "none" option, which is normalized to zero for identification purposes, and ε_{ij} is the unobserved portion of the utility function. It is possible that probabilistic uncertainty can influence a consumer's utility (Glenk and Colombo, 2011), and other studies have identified methods to relax the random utility framework to address this potential issue (Lusk, Tonsor, and Schroeder, 2014; Teisl and Roe, 2010). Specifically, they estimate a model consistent with expected utility theory (EUT) where consumer *i* evaluates the risk associated with choice *j* (von Neumann and Morgenstern, 1944). Mathematically, expected utility (EU_{ij}) can be defined as:

$$EU_{ij} = \sum_{k=1}^{K} p_{ijk} \, U_{ij} \tag{2}$$

where p_{ijk} is the probability k of individual *i* receiving a specific outcome (or attribute) from option *j*. As explained by Lusk, Schroeder, and Tonsor (2014), these p_{ijk} can be interpreted as subjective, individual-specific beliefs. This probabilistic approach is somewhat limited for two reasons. First, participants might struggle to quantify their beliefs probabilistically (Manski, 2004). Second, while incorporating beliefs into a utility function in the form of a probability can help more precisely identify preference parameters, these probabilistic changes can be somewhat difficult to interpret.

Even if consumer *i*'s probabilistic beliefs are assumed to be known ($p_{ijk} = 1$), a product's intangible characteristics (i.e. perceptions) likely affect a consumer's utility. If left unidentified, some of the impact of this unobserved variable is likely to be captured within parameter estimates for the observed factors, thereby potentially biasing β_j in an ambiguous direction. That is, where previous studies have separated *probabilistic beliefs* from preferences, it is also important to distinguish *category-specific perceptions* from preferences. As such, we define U_{ij} as:

(3) $$U_{ij} = \beta_j + \tau_1 \cdot Price_j + \tau_2 \cdot Perception_{ij} + \varepsilon_{ij}$$

where the alternative-specific constant $\beta_j$could be considered the "brand loyalty effect" for choice alternative j, $Price_j$ is the price of choice alternative j, which is exogenously determined by the experimental design, $Perception_{ij}$ is participant *i*'s perception of choice alternative j, ε_{ij} is the unobserved random component, and all τ_i are parameters to be estimated. A key benefit to this approach is that it allows us to identify and causally interpret how a product's perception influences consumer decision-making, where τ_2 is the estimated preference for the issue in question.

A fundamental concern is that some unobserved factor might simultaneously influence a product's perceptions and preferences, which would cause the error term (ε_{ij}) to be correlated with $Perception_{ij}$. The endogeneity might be caused by many potential biases. For one, the observable price of a product in the discrete choice model might influence the consumer's taste perception, generating the potential for feedback effects. There might also be concern for heterogeneity associated with an omitted parameter, as might be the case of perceptions for bread products, where the perceptions of health and safety of a gluten-intolerant consumer are likely to be endogenously determined. There might be potential concern for some measurement error, as participants might struggle to identify their perceptions on a Likert scale. As such, the sign and significance of the potential endogeneity are somewhat ambiguous, yet likely to exist in many contexts.

Although many methods have been proposed to deal with endogeneity bias in other contexts, the instrumental variable (IV) approach is of the most common in the empirical literature. For an instrument to successfully eliminate the bias in the relevant parameter estimates, it should be highly correlated with the variable of interest (i.e. perceptions) while being uncorrelated with the residual of the dependent variable. Valid instruments fit two criteria. First, the covariance between the estimated instrument and the perception of interest is non-zero. Second, the covariance between the estimated instrument and the error term in the choice model is zero.

Because the discrete choice model is nonlinear, the traditional instrumental variable approach would not apply. Instead, we follow the "control function approach" proposed by Petrin and Train (2010) which retains much of the functional form specification, but works around the specific distributional assumptions for discrete choice modeling. As such, a discrete choice model that controls for endogeneity can be estimated in two stages. For the first stage, we estimate a series of regressions and obtain their residuals. In our first approach, we estimate OLS regressions for each choice option j, where each consumer i's perception of option j is the dependent variable and each consumer *i*'s perception of all of the other options (*k, g, l…)* are the explanatory variables. Mathematically, in the first stage, regressions for perception for all products *J* are estimated such that:

$$Perception_{ij} = f\left(Perception_{ik}, Perception_{ig}, Perception_{in} \dots\right) + \mu_{ij}, \tag{5}$$

where product perception *j* is a function of all other product perceptions in the discrete choice model, and μ_{ij} is the normally distributed error term for each regression. For our second approach, we estimate OLS regressions where each consumer *i*'s perception of option *j* is the dependent variable and each consumer *i*'s personality and demographic values are the explanatory variables. Based on these "first stage" regressions, we obtain the estimated residuals for each perception and option ($\hat{\mu}_{ij}$). Assuming the derived instrument is uncorrelated with the brand preferences, including the residual as an explanatory variable in the second stage of the estimation can effectively test and control for endogeneity between perceptions and preferences. Thus, by including the residual $\hat{\mu}_{ij}$ for consumer *i*'s perception of product *j* in a utility function updated from equation (3), we can successfully derive separate parameter estimates for the alternative-specific constants and the product perceptions. Mathematically, the updated utility function to be estimated can be defined as:

$$U_{ij} = \beta_j + \tau_1 \cdot Price_j + \tau_2 \cdot Perception_{ij} + \lambda \cdot \hat{\mu}_{ij} + w_{ij}, \tag{6}$$

where the linear control function is comprised of a vector of parameters (**λ**) associated with the vector of residuals ($\hat{\mu}_{ij}$) associated with each of the perceptions derived from equation (4), and w_{ij} is an iid Type I extreme value error term. The parameters β_j and τ_i in equation (6) can be interpreted in the same manner as in equation (3), although, depending on the significance of the endogeneity bias, the parameter estimates might vary substantially.

Data and Methods

We utilize a simple "branded" discrete choice experiment where individuals choose between six different pints of beers at varying prices. Our chosen study product category is lager beer, as brand-level perceptions are likely to influence beer-buying decisions. Blind taste tests indicate that consumers often cannot differentiate between two competing products, suggesting that perceptions of brand equity are an important influencer of consumer choice and perceived quality (Alison and Uhl, 1964; Almenberg, Dreber, and Goldstein, 2016). As such, perceptions and preferences are likely to be especially difficult to disentangle for this product. An additional benefit to using pints of beer is that beer allows us to keep prices below $10, which reduces the potential for hypothetical bias to invalidate the study findings (Murphy et al., 2005).

Data were collected in May 2015, and participants were recruited via an online panel maintained by the company SSI®, who maintains its panel of participants through a variety of means including phone calls and online ads, and offers their panel participants approximately $1.50 in gift cards for each completed survey. A total of 1,697 surveys were collected, generating a total of 1,697 x 8 = 13,576 choice observations. Table 1 shows the demographics of our sample, which are consistent with the characteristics of U.S. residents over 21 who might drink beer. Men made up 54.15% of our sample, 32.76% of the participants were under 35 years of age, and 45.67% of the median household income for the 1,697 participants earned less than $60,000.

A main effects orthogonal fractional factorial design was employed to assign prices to brands; the final design consisted of eight choice questions in which the price of each beer was uncorrelated with the price of other beers. Each participant answered eight choice questions (Figure 1), with the following instructions: "We are interested in the types of beer you like to buy. Imagine you're at a bar or restaurant. In what follows, we will ask you 8 different choice questions, and in each question we would like to know which type of beer you most prefer when you buy a pint of beer." Respondents chose between six randomly ordered beers at price combinations of \$3 and \$6, or respondents could choose "none".

After the choice experiment questions, we then used a Likert-type scale to identify participants' perceptions of two components of brand equity: perceived taste and brand familiarity. These two variables were chosen as new brewers are likely to be interested in how to most efficiently invest their advertising capital: by promoting the high-quality taste of their beers to a subset of the population, or by aggressively advertising to many people in an effort to promote brand awareness. As such, we asked participants to rate their familiarity with each of the aforementioned six brands on a three point scale (1=low, 2=medium, and 3=high). Then we asked respondents to rate their perceived taste of each brand on a five-point scale that spanned between 1="one of the worst tasting" and 5="one of the best tasting." Mathematically, we can define the indirect utility associated with participant *i*'s choice of beer brand *j* as:

(7) $V_{ij} = \beta_j + \tau_1 \cdot Price_j + \tau_2 \cdot Taste_{ij} + \lambda_1 \cdot \hat{\mu}_{1ij} + \ \tau_3 \cdot Familiarity_{ij} + \lambda_2 \cdot \hat{\mu}_{2ij},$

where the estimated control function instruments λ_1 and λ_2 are associated with residuals $\hat{\mu}_{1ij}$ and $\hat{\mu}_{2ij}$, which are associated with the perceptions $Taste_{ij}$ and $Familiarity_{ij}$, and all β_j and τ_i are parameters to be estimated.

Table 2 describes product-specific characteristics as well as the average taste perception ($Taste_{ij}$) and brand familiarity ($Familiarity_{ij}$) values of the beers in our sample. In an effort to focus on brand-specific perceptions, all beers in the sample are lagers. Alcohol percentages ranged from Miller Lite, which only contains 4.17% alcohol by volume (ABV), to 5.30% ABV (Oskar Blues Mama's Little Yella Pils). Beer Advocate rates the Samuel Adams Boston Lager the highest-quality option in our sample (86), while Corona and Miller Lite were the rated as the lowest quality options (55). Note that the data shown in table 2 on origin, ABV, and beer advocate ratings were *not* shown to participants and is shown here for informational purposes only. On average, Mama's Little Yella Pils from Oskar Blues Brewery was perceived to be the worst tasting of the six options, while the Sam Adams Boston Lager was perceived to be the best tasting. As one might expect, participants were most familiar with Budweiser, but were least familiar with Oskar Blues and Marshall. Consumer perceptions were heterogeneous, and some participants perceived the taste of the lesser-known beers as above average tasting even when they were unfamiliar with the beer.

Results

For our first approach to controlling for potential endogeneity, the first step consisted of 12 OLS regressions (two perceptions (taste and familiarity) and six brands) using other products' perceptions as instruments. Tables showing the 12 regression results can be found in the Appendix. When we regress brand familiarities on other brand familiarities, the majority of parameter estimates are significant at the $\alpha = 0.05$ level, and R-squared values range from 0.18 to 0.49, indicating that there is a significant correlation between familiarities for each brand. Most of these signs are positive, although there is a negative, statistically significant correlation between familiarity of the Budweiser and Oskar Blues brands. For the taste familiarity instrument, all significant parameters are positive. Similar to the results for the brand familiarity instrument, the R-squared values are sizable (given cross-sectional data), ranging from 0.16 to 0.48.

Our second approach to controlling for potential endogeneity involved 12 OLS regressions using individual-specific personality and demographics included as instruments. Tables that describe these regressions can also be found in the Appendix. These instruments did not exhibit as much explanatory power as did the models estimated for the first approach, although approximately half of all parameter estimates were statistically significant at the $\alpha = 0.05$ level. For the models of brand familiarity, R-squared values ranged from 0.02 for Budweiser to 0.26 for Oskar Blues and Marshall. When we regress individual-specific personality and demographic characteristics to predict taste perceptions, we again identified approximately half of all parameter estimates as statistically significant at the $\alpha = 0.05$ level. R-squared values ranged from 0.06 (Corona) to 0.12 (Oskar Blues and Marshall).

As described, we saved the residuals from these regressions and incorporated them into our random parameter logit (RPL) models. Table 3 displays the parameter estimates from the two different instrumental variable specifications, as well as for a model that does not control for the endogeneity (recall the brand effects are estimated relative to the "none" option, the utility of which was normalized to zero for identification). The AIC for both the random parameter models that include instruments is lowest, indicating that the original model specification was most likely endogenous.

The AIC for the model that includes instruments based on other products' perceptions data is lowest overall, suggesting that those instruments fit the data better than the other instruments. Moreover, in the model specification that uses the control function approach with perceptions data, the parameter estimates associated with the residuals for both taste and familiarity are statistically significant, indicating the presence of endogeneity. When we do not correct for endogeneity, the random parameter logit model indicates that, on average, the parameter for perceived taste is approximately twice as large as brand familiarity. When we control for endogeneity, however, the parameter estimate for perceived taste becomes approximately seven times larger for the average participant. Because of the size of the standard deviation of brand familiarity relative to the parameter mean, this model indicates that an increase in familiarity does not increase the likelihood a beer is selected for all consumers. In fact, results suggest that approximately 30.85% of participants are actually more likely to purchase unfamiliar beer, rather than the option with which they are most familiar.

Brand Familiarity and Perceived Taste Elasticities

As noted, results from hypothetical choice experiments are most useful when discussing marginal changes (Lusk and Schroeder, 2004). As such, we focus on the marginal effects and elasticities associated with changes in each perception. We use the model estimated where the instruments were derived from the perceptions themselves as this model best fit the data. Table 4 displays the elasticities for brand familiarity for both models reported in table 5 and reports the difference between elasticity estimates from the control function and the traditional approaches. The table suggests brand familiarity affects each beer differently. For example, the preferred model indicates that a one percent improvement in the brand familiarity of Miller Lite leads to a 0.603% increase in the probability the beer is chosen. By contrast, a one percent increase in the brand familiarity of the Marshall option would only lead to a 0.187% increase in the probability the beer is chosen. If we do not correct for endogeneity, the estimated elasticities are roughly twice as large – the own-familiarity elasticity for Miller Lite is now 1.208 and the own-familiarity elasticity for Marshall is now 0.597.

Perceived taste elasticities demonstrate the opposite pattern, which can be found in table 5. Changes in perceived taste matter much more for the craft options (Marshall and Oskar Blues) than do changes in the perceived taste of the premium and macro options. For example, a one percent increase in the perceived taste of the Oskar Blues option leads to a 6.375% increase in the quantity demanded of that beer, while the same increase in the perceived taste of Corona leads to a 4.261% increase in its quantity demanded. Similar to the own-taste elasticities, relative to the familiarity elasticities, cross-taste elasticities are much larger. According to the model estimated via the control function approach, the perceived taste of Sam Adams is the option most dependent on the perceived taste of the other beers. Specifically, the one percent increase in Corona's perceived taste would also lead to a 1.520% reduction in the probability a Sam Adams was selected, and the one percent increase in Oskar Blues' perceived taste would create a corresponding 1.991% reduction in Sam Adams' quantity demanded. Elasticity estimates from the traditional approach are all closer to zero, and own-taste elasticities are approximately 1.5% larger for all beer options, with the largest differences being for the Oskar Blues and Marshall beers. Even without correcting for endogeneity, perceived taste is more important for the small craft breweries than for the larger breweries.

Discussion

A key contribution from the behavioral sciences to economics has been that perception malleability can influence individual decision-making and preference formation. In this article, we used a discrete choice experiment for beer to show that perceptions of taste and familiarity substantially affect consumer choice. We also show that, although perceptions and preferences are endogenous, the endogeneity bias can be corrected by utilizing perceptions of other alternatives as instruments for perceptions of the alternative in question. Once we control for endogeneity, our estimates indicate that some participants actually prefer an unfamiliar beer (i.e., they are variety-seekers), and that taste is even more influential than what the endogenous model indicated. We also find that perceptions of taste are more important for the smaller craft breweries than they are for larger breweries.

Although this study makes an important contribution in distinguishing brand perceptions from preferences, some shortcomings remain. First, because we studied beer, there is potential that consumers in this market might be using price as a proxy for quality. This phenomenon has been documented in the literature surrounding wine (see Goldstein et al., 2008), and, although we have tried to avoid this problem by using an experimental design where prices are uncorrelated with brands, it is likely to exist in other markets for experience goods. Furthermore, this choice experiment focused on beer brands as these brands are largely created to influence consumer perceptions. As observed in Malone and Lusk (2017a) who conducted a discrete choice experiment for meat products, consumer choices regarding other goods with weaker brand equity might respond differently to perceptions, making this instrumental variable approach less necessary. Finally, these results have somewhat ambiguous implications for beer promotion, given that we do not know how much it would cost for each brewer to change consumer perceptions.

While the empirical application of this article focused on perceptions of brand equity for beer, the method can be applied to many other utility-maximizing choices. Just as price elasticities are important for private and public policy decisions, perception elasticities might help identify which perceptions most influence consumer decision-making. As such, any product or public policy that relies on marketing and advertising to alter perceptions in a manner that might shape preferences could benefit from this approach. Moving forward, future research might identify how the provision of information might influence consumer perceptions (Costanigro, Deselnicu, and Kroll, 2015). Similarly, perceptions might become more tightly correlated with preferences as consumers gain experience with the product in question. Future studies might test this hypothesis by utilizing a dynamic experiment with repeated observation choices and perceptions. Finally, this study focused on perceptions of brand equity. For some products, perceptions might be more relevant at the characteristic level rather than the brand/product level. Future studies might develop an empirical method for separating these perceptions from product attributes. These shortcomings notwithstanding, this article outlines an easy-to-implement method that successfully controls for endogeneity bias in discrete choice experiments that attempt to separate perceptions from preferences.

Table 3.1. Average perception ratings for each beer brand

	Perceived Taste [a]		**Brand Familiarity** [b]	
Miller Lite	3.000	(1.201) [c]	2.434	(0.697)
Corona	3.544	(1.113)	2.535	(0.652)
Sam Adams	3.687	(1.057)	2.399	(0.698)
Budweiser	3.198	(1.239)	2.570	(0.636)
Oskar Blues	2.921	(0.893)	1.342	(0.607)
Marshall	3.036	(0.917)	1.354	(0.613)

[a] Perceived tasted perceptions evaluated on a 5 point scale (1 = one of the worst tasting, 5 = one of the best tasting

[b] Brand familiarity evaluated on a 3 point scale (3 = high familiarity, 1 = low familiarity)

[c] Numbers in parentheses are standard deviations

Table 3.2. First stage model estimates for the brand familiarity instruments

Explanatory Variables	Brand Familiarity Dependent Variable: Miller Lite		Corona		Samuel Adams		Budweiser		Oskar Blues		Marshall	
Intercept	0.594*[a]	(0.080)[b]	0.874*	(0.076)	0.898*	(0.085)	1.045*	(0.068)	0.437*	(0.060)	0.306*	(0.061)
Miller Lite			0.119*	(0.023)	0.115*	(0.026)	0.405*	(0.019)	0.016	(0.018)	-0.020	(0.018)
Corona	0.126*	(0.025)			0.348*	(0.026)	0.214*	(0.022)	0.018	(0.019)	0.010	(0.019)
Samuel Adams	0.097*	(0.022)	0.277*	(0.021)			0.039	(0.020)	0.027	(0.017)	0.041*	(0.017)
Budweiser	0.504*	(0.024)	0.252*	(0.026)	0.058	(0.030)			-0.065*	(0.020)	0.015	(0.021)
Oskar Blues	0.029	(0.033)	0.031	(0.032)	0.058	(0.035)	-0.093*	(0.029)			0.697*	(0.018)
Marshall	-0.035	(0.032)	0.016	(0.031)	0.085*	(0.035)	0.021	(0.029)	0.680*	(0.017)		
R-squared	0.305		0.253		0.183		0.331		0.486		0.485	

Estimated from N=1,697 observations

[a]An asterisk indicates that the parameter is significant at the 95% level.

[b]Standard errors are in parentheses.

Table 3.3. First stage model estimates for the perceived taste instruments

Explanatory Variables	Perceived Taste Dependent Variable: Miller Lite		Corona		Samuel Adams		Budweiser		Oskar Blues		Marshall	
Intercept	0.768*[a]	(0.129)[b]	1.580*	(0.125)	1.892*	(0.115)	1.197*	(0.132)	0.470*	(0.081)	0.708*	(0.083)
Miller Lite			0.167*	(0.024)	-0.032	(0.023)	0.475*	(0.022)	0.046*	(0.015)	0.004	(0.016)
Corona	0.164*	(0.024)			0.159*	(0.023)	0.171*	(0.025)	0.038*	(0.015)	0.002	(0.016)
Samuel Adams	-0.035	(0.026)	0.177*	(0.025)			0.014	(0.027)	0.082*	(0.016)	0.114*	(0.016)
Budweiser	0.441*	(0.021)	0.161*	(0.023)	0.012	(0.022)			-0.001	(0.015)	-0.009	(0.015)
Oskar Blues	0.116*	(0.038)	0.096*	(0.039)	0.186*	(0.036)	-0.003	(0.040)			0.656*	(0.019)
Marshall	0.010	(0.037)	0.006	(0.038)	0.246*	(0.035)	-0.024	(0.039)	0.620*	(0.018)		
R-Squared	0.285		0.155		0.160		0.276		0.483		0.481	

Estimated from N=1,697 observations
[a]An asterisk indicates that the parameter is significant at the 95% level.
[b]Standard errors are in parentheses.

Table 3.4. Coefficients from the second stage of the control function approach derived by obtaining the residuals from tables 3.2 and 3.3

Residual	**Familiarity**	**Perceived Taste**
Miller Lite	0.591*[a] (0.080)[b]	-0.527* (0.050)
Corona	0.909* (0.071)	-0.640* (0.050)
Samuel Adams	0.694* (0.068)	-0.453* (0.049)
Budweiser	0.465* (0.079)	-0.475* (0.049)
Oskar Blues	0.700* (0.086)	-0.739* (0.075)
Marshall	0.225* (0.079)	-0.515* (0.059)

Estimated from N=1,697 observations
[a]An asterisk indicates that the parameter is significant at the 95% level.
[b]Standard errors are in parentheses.

Table 3.5. Random parameter logit model estimates for models that distinguish between perceptions and preferences

Variable	Model with perceptions that does not correct for endogeneity	Model with perceptions that corrects for endogeneity (1)	Model with perceptions that corrects for endogeneity (2)
		Non-Random Parameters	
Price	-0.613*[a] (0.013)[b]	-0.626* (0.013)	-0.615* (0.013)
Residual Taste		-0.612* (0.096)	-1.033* (0.098)
Residual Familiarity		1.105* (0.130)	0.710* (0.125)
Perceived Taste		2.707* (0.098)	
Brand Familiarity		0.230 (0.121)	
		Random Parameters	
Perceived Taste	2.250* (0.059)		3.286* (0.119)
Brand Familiarity	1.054* (0.072)		0.469* (0.127)
Miller Lite	-4.701* (0.261)	-7.420* (0.383)	-6.527* (0.406)
Corona	-4.359* (0.255)	-7.022* (0.408)	-6.610* (0.439)
Samuel Adams	-4.338* (0.253)	-7.229* (0.413)	-6.907* (0.450)
Budweiser	-4.840* (0.265)	-7.492* (0.402)	-6.826* (0.427)
Oskar Blues	-4.827* (0.244)	-8.075* (0.340)	-7.061* (0.368)
Marshall	-4.454* (0.250)	-7.768* (0.346)	-6.851* (0.379)
		Distributions of Standard Deviations	
Brand Familiarity	0.966* (0.053)		0.940* (0.053)
Perceived Taste	1.230* (0.095)		1.178* (0.096)
Miller Lite	1.878* (0.102)	2.583* (0.120)	1.896* (0.103)
Corona	1.654* (0.099)	2.405* (0.091)	1.619* (0.093)
Samuel Adams	1.622* (0.101)	2.349* (0.090)	1.734* (0.093)
Budweiser	1.837* (0.111)	2.638* (0.120)	1.867* (0.105)
Oskar Blues	1.827* (0.126)	2.388* (0.132)	1.689* (0.150)
Marshall	2.119* (0.129)	2.817* (0.119)	2.066* (0.126)
Number of parameters	17	15	19
Log likelihood	-12,688.3	-13,037.2	-12,596.4
AIC	25,410.6	26,104.4	25,230.8

[a]An asterisk indicates the parameter is significant at the 99% level.
[b]Standard errors are in parentheses.

Table 3.6. Brand familiarity elasticity differences estimated using the control function approach and the traditional approach

	Miller Lite	Corona	Sam Adams	Budweiser	Oskar Blues	Marshall
Control function approach						
Miller Lite	0.603	-0.114	-0.074	-0.165	-0.052	-0.051
Corona	-0.204	0.503	-0.187	-0.212	-0.089	-0.078
Sam Adams	-0.151	-0.213	0.371	-0.157	-0.097	-0.090
Budweiser	-0.200	-0.144	-0.094	0.583	-0.054	-0.056
Oskar Blues	-0.019	-0.017	-0.018	-0.016	0.238	-0.016
Marshall	-0.029	-0.024	-0.024	-0.026	-0.026	0.187
Traditional approach						
Miller Lite	1.208	-0.203	-0.152	-0.272	-0.144	-0.129
Corona	-0.370	1.013	-0.346	-0.381	-0.250	-0.216
Sam Adams	-0.309	-0.388	0.823	-0.316	-0.286	-0.260
Budweiser	-0.332	-0.258	-0.191	1.169	-0.154	-0.150
Oskar Blues	-0.044	-0.040	-0.040	-0.037	0.742	-0.068
Marshall	-0.063	-0.054	-0.058	-0.059	-0.114	0.597
Difference (Control function minus Traditional)						
Miller Lite	-0.605	0.089	0.078	0.107	0.092	0.078
Corona	0.166	-0.510	0.159	0.169	0.161	0.138
Sam Adams	0.158	0.175	-0.452	0.159	0.189	0.170
Budweiser	0.132	0.114	0.097	-0.586	0.100	0.094
Oskar Blues	0.025	0.023	0.022	0.021	-0.504	0.052
Marshall	0.034	0.030	0.034	0.033	0.088	-0.410

Elasticities derived in NLOGIT using probability weights, and reported with respect to change of X in row choice on the probability of the column choice.

Table 3.7. Perceived taste elasticity differences estimates using the control function approach and the traditional approach

	Miller Lite	Corona	Sam Adams	Budweiser	Oskar Blues	Marshall
			Control function approach			
Miller Lite	4.620	-0.738	-0.454	-1.071	-0.711	-0.601
Corona	-1.414	4.261	-1.252	-1.435	-1.376	-1.158
Sam Adams	-1.060	-1.520	3.725	-1.107	-1.991	-1.775
Budweiser	-1.331	-0.930	-0.601	4.502	-0.729	-0.670
Oskar Blues	-0.277	-0.279	-0.335	-0.225	6.375	-0.753
Marshall	-0.407	-0.408	-0.524	-0.361	-1.341	5.209
			Traditional approach			
Miller Lite	3.118	-0.489	-0.355	-0.642	-0.524	-0.440
Corona	-0.947	2.883	-0.894	-0.969	-0.984	-0.838
Sam Adams	-0.826	-1.074	2.627	-0.848	-1.337	-1.199
Budweiser	-0.804	-0.624	-0.463	3.059	-0.554	-0.517
Oskar Blues	-0.201	-0.195	-0.223	-0.168	4.295	-0.459
Marshall	-0.287	-0.283	-0.342	-0.268	-0.796	3.573
			Difference (Control function minus Traditional)			
Miller Lite	1.502	-0.249	-0.099	-0.429	-0.187	-0.161
Corona	-0.467	1.378	-0.358	-0.466	-0.392	-0.320
Sam Adams	-0.234	-0.446	1.098	-0.259	-0.654	-0.576
Budweiser	-0.527	-0.306	-0.138	1.443	-0.175	-0.153
Oskar Blues	-0.076	-0.084	-0.112	-0.057	2.080	-0.294
Marshall	-0.120	-0.125	-0.182	-0.093	-0.545	1.636

Elasticities derived in NLOGIT using probability weights, and reported with respect to change of X in row choice on the probability of the column choice.

CHAPTER IV

THE EXCESSIVE CHOICE EFFECT MEETS THE MARKET: A FIELD EXPERIMENT ON CRAFT BEER CHOICE

Whether people have too many choices has long been a topic of debate. Even in the first century, Seneca, the Roman philosopher, famously proclaimed in his *Letters from a Stoic* (1969), “Distringit librorum multitudo!” His charge that “the abundance of books is distraction” predates the modern psychological theory of an excessive choice effect (ECE). Popular books such as *The Paradox of Choice* (Schwartz, 2004a) posit that increasing the number of choice options in the marketplace increases levels of regret and decreases levels of satisfaction. “Information overload” can reduce the probability of making a choice, which challenges the standard economic conjecture that more choice options cannot decrease consumer utility. Confronted with a large number of choices, consumers might elect to walk away from the transaction altogether. Excessive choice can also overwhelm consumers, causing them to make decisions via sub-optimal heuristics. For example, if confronted with many unfamiliar options, a consumer may simply choose the first option listed or select the status quo.

Some authors suggest that exogenous constraints on the number of options might actually be desirable. For example, Schwartz advises readers to “learn to love constraints.” He argues: “We would be better off if we embraced certain voluntary constraints on our freedom of choice, instead of rebelling against them” (Schwartz, 2004a; pg. 5). Alternatively, some researchers have suggested interventions as a way to help consumers make better choices through behavioral “nudges” (Johnson et al. 2012). These nudges recommend reframing the choice architecture to increase the likelihood of selecting a particular option without imposing actual constraints on the available number of options. Altering the choice architecture has been shown to influence consumer choice (Thaler and Sunstein, 2003; Thaler and Benartzi, 2004). Yet, a deeper question permeates much of the discussion on the topic – who should be the one to redesign choice architecture? Should governments and experts reframe choices, or do private nudges emerge from market outcomes?

In a competitive market, the choice architecture is endogenous, and sellers compete to provide environments that consumers find appealing, thereby increasing profits. In such cases, the market, at least partially, provides incentives to ameliorate the ECE by, for example, reducing search costs for consumers (e.g., see Kamenica, 2008; Kuksov and Villas-Boas, 2010; Norwood, 2006). This raises the possibility that ECE may arise in laboratory contexts or one-shot field experiments while at the same time having limited relevance in day-to-day business decisions. Whereas prior research mainly focus on the identification of an ECE, we show that sellers have access to market-specific mechanisms (or informational nudges) that narrow its influence. We demonstrate that if the ECE exists, sellers can mitigate or exasperate its negative effects through targeted interventions.

That search costs affect consumers' decisions and market outcomes has been confirmed in many empirical studies (Besedeš et al., 2012; Besedeš et al., 2015; Gabaix et al., 2006; Reutskaja et al., 2011). For example, Caplin et al. (2011) show that laboratory participants search through choices in a sequential manner before they choose an option they perceive "good enough" rather than utility maximizing. Caplin et al. (2011) show that choice and search behavior depend on decision contexts such as, for example, time constraints. These experimental insights suggest sellers might be able to alleviate the ECE by lowering search costs. If consumers suffer from an ECE, they (or the businesses selling to them) might solve the issue through adopting various mechanisms that help consumers navigate through large choice sets. The evolution of these "informational nudges" might explain some of the diversity within the experimental results on choice overload. Many choices might be desirable in some settings or in the right context but not in others. Additionally, empirical research by Schwartz et al. (2002) and conceptual models like that of Irons and Hepburn (2007) indicate that the ECE is likely to vary across people according to preferences for variety and propensity to experience regret. This suggests the presence or absence of an ECE may depend on a firm's clientele and target market.

Conceptual models introduced by Kamenica (2008), Kuksov and Villas-Boas (2010), and Norwood (2006) suggest that the ECE may not, in fact, be at odds with economic theory if consumers face search costs and are uncertain about the value of the good. Selecting which option is most desirable takes time and cognitive resources. If consumers are presented with a large assortment of unfamiliar items, it can be perfectly rational for a consumer to forego the opportunity to buy rather than trying to figure out where each product fits in one's preference ordering. Such findings suggest differences in search costs and inability to understand contextual clues may be factors responsible for the heterogeneity in the ECE.

We test two different informational nudges that sellers might use to potentially influence search costs and thereby alter the size of the ECE in the context of consumers' choices in a market where the number of new options is burgeoning: craft beer. These nudges are: 1) the listing of a "special" on the menu and 2) the provision of Beer Advocate scores on the menu. We hypothesize that listing a weekly special will increase the chances a consumer will select a beer in small choice sets, but that it will have negligible effects on the ECE. "Specials" might imply to consumers that they are not only receiving the utility of the item, but are also earning a level of utility associated with the transaction (Thaler, 1985). At first glance, that transaction utility, along with making one of the options more cognitively available, should decrease the ECE. However, without any context given as to why the product is "special," this treatment may not actually convey information on relative quality rankings of options.

This chapter uses these insights as a springboard for studying the ECE in a market where there is a proliferation of new options, many of which are likely unfamiliar to some consumers: craft beer. The craft beer market was chosen because it is one in which an ECE is likely to exist. The number of these "small, independent and traditional" breweries has increased from two in 1977 to over 2,000 in 2012 (Elzinga et al., 2015). This increase in the number of new brands with limited advertising budgets suggests that most consumers are likely to be unfamiliar with the new options, resulting in no clearly dominant option (Dhar, 1997; Dhar and Nowlis, 1999). Moreover, beer is an experience good in that its quality is difficult to observe before consumption, increasing the cost of search. Craft beer has evolved as a novelty-driven market (Watson, 2016). It is not uncommon for a small brewery to offer a dozen different beer styles over the course of a year, and many have gained global notoriety in their aggressive desire for novelty. As an example, Delaware's Dogfish Head Brewing Company flaunts the mantra, "Off-centered ales for off-centered people." By extension, some restaurants and bars advertise the plethora of craft beers they offer on tap. It follows that a bar selling a high volume of craft beer will likely market to a specific demographic of people who are interested in novelty.

We hypothesize that quality scores from a third party scorer will have varying effects on the presence of an ECE. In many markets, professional and crowd-sourced ratings appear to assist individuals in the aggregation of information to make a more informed decision (Chen et al., 2012). Specific to this market, multiple ratings websites such as Beer Advocate make information freely available to the consumer. This company publishes scores on a 100-point scale for millions of beers based on ratings posted by both users and experts. A seller might list these scores to provide context clues and relevant information to a buyer so he can better identify which product might be the utility-maximizing choice (Kamenica, 2008). These scores might be helpful for a person who generally knows beer varieties and, for example, self-selects into drinking at a bar with many options. However, a person unfamiliar with beer varieties might find that scores will simply add more confusion to an already cognitively taxing menu.

The overall objective of this chapter is to empirically explore how the ECE varies with informational nudges, relying on economic models suggesting that search costs are a likely culprit explaining the phenomenon. The specific objectives of the current study are (1) to determine, in our particular context, whether the ECE exists and (2) whether the marketplace mitigates this problem through certain informational nudges. The next section provides more background on the ECE and outlines our particular contribution to that literature. We then introduce and discuss the results of a field experiment conducted at a wine bar whose patrons are likely to experience the ECE. Using this experimental data, we estimate the likelihood of selecting a beer while varying the number of options present from six choices to 12 in the presence of informational nudges likely to lower search costs. The final section concludes.

Background

The ECE has been documented as an increase in consumers' unwillingness to participate in a market transaction and as a decrease in well-being as the number of options grows (Schwartz, 2002; 2004b). The phenomenon tends to exist in markets with an unusually large number of options when consumers do not have clearly defined preferences and no clearly dominant choice exists (Scheibehenne et al., 2010). Iyengar and Lepper (2000), for example, offered varying types of jams for purchase. Over the course of the study, the experimenters changed the number of options available from six to 24 or 30 choices. They found consumers were less likely to purchase jam from larger choice sets. Schwartz (2002) and others have taken these findings as suggesting that policies which restrict choice might improve welfare. However, to the extent that more choice lowers the likelihood of purchase, it seems that businesses have a profit incentive to refrain from offering so many options or to alter the choice environment (Sela et al, 2009).

The results from studies that have tried to replicate the ECE in other contexts have proven inconclusive. Scheibehenne (2008) attempted to replicate the results found by Iyengar and Lepper (2000), but could find no ECE. Arunachalam et al. (2009) sought to determine whether participants would voluntarily reduce their choice set size, and found that only a small proportion of respondents would in fact prefer to choose from a smaller number of options. They found that while an ECE can exist for certain types of people, the size of the effect was small and difficult to detect, although the maximizer-satisficer scale introduced by Schwartz et al., (2002) was predictive of an ECE.

Because the effect varies from study-to-study, one might wonder whether there are systematic differences in the markets, goods, or consumers that give rise to the heterogeneity in the measured effects. Scheibehenne et al. (2010) conducted a meta-analysis of 50 studies, and determined that while there is a negligible ECE overall, the variability in studies is large. They found that effect sizes were not a function of whether the study was hypothetical or non-hypothetical. They were also unable to identify differences across international locations or culture. Their analysis also revealed a publication bias; published studies are more likely to find a statistically significant ECE than unpublished studies. They also found that studies which utilized goods for which consumers likely had less knowledge and weak prior preferences were associated with larger ECEs.

Social scientists have excessively studied the ECE, yet none has been able to definitively identify what causes the phenomenon, or whether it exists at all. Seminal articles by Stigler (1961), Coase (1960), and Williamson (1979) suggest that transactions costs, often associated with the cost of search, can lead to inefficiencies. Stigler (1961) noted that sellers can reduce search costs by, for example, identifying a few goods out of many to present to consumers. Sellers might also lower search costs by providing buyers with relevant information through advertising (Nelson, 1974; Chen et al., 2012). Several authors have proposed thoeretical models based on these concepts to reconcile the finding of an ECE with economic theory. Norwood (2006) treated the excessive choice problem as one of search costs in the presence of uncertainty about the quality of competing goods. When the number of options are exogenously chosen, a greater variety might increase the probability a consumer finds a more preferred option, but it could also create less efficient, less successful searches. His model highlighted the seller's role as a minimizer of search costs for the buyer. Kamenica's (2008) model focused on the role of the decision-making context as a provider of information in explaining the ECE. Different sized choice sets can provide different information (via the so-called compromise effect) about which varieties most likely suit consumers' tastes, resulting in an ECE. Kamenica (2008, p. 2140) noted that one prediction of his model is that "the firm could increase both its profits and social welfare by simply labeling the popularity of the varieties it introduces." As will be discussed later, one of the informational nudges we consider does precisely that: provide information about the relative popularity of different options.

The model introduced by Kuksov and Villas-Boas (2010) focused on search costs during sequential decision-making processes. They showed that the expected costs and subsequent expected utility associated with a choice are dependent on the distribution of the alternatives. As the number of options increase, search costs increase, and increases in expected utility from an additional option falls. This tradeoff implies that the firm selling a product can find optimal levels of the number of options, and that these sellers have an incentive to match their span of options in a strategic manner by accounting for customer demographics.

Other explanations of choice overload involve regret avoidance and consumer heterogeneity in the presence of search costs and bounded rationality. Irons and Hepburn (2007) modeled regret avoidance (rather than utility maximization) as the cause of the ECE. Building on Simon (1955), Schwartz et al. (2002) used a "maximizer-satisficer scale" to identify people who were more likely to suffer from the ECE. If a regret-minded maximizer were to make a choice, he might experience deep levels of regret after the decision when he considers the options foregone. Inbar et al. (2011) related choice regret to decision speed; yet it is possible to conceptualize a strict time constraint as being equivalent to an increase in the relative search costs associated with evaluating another option. They found that participants who feel they had enough time to evaluate an option experienced lower regret levels; or, that when the time allotted for a search was not constrained, the choice was more satisfying. Mogilner, Rudnick, and Iyengar (2008) found that simply categorizing a large number of options can increase levels of satisfaction, which is consistent with the hypothesis that reducing search costs can minimize the ECE. Ultimately, these studies support the notion that heightened search costs and individual personalities or idosyncraices might influence the ECE.

This chapter builds on the notion of search costs to contribute to the literature. Where most empirical tests of the ECE are limited to the anomaly's existence, we move the literature forward by incorporating context-specific, informational nudges in a marketplace likely to exhibit an ECE. We show that even if the ECE does exist, marketplaces have created institutions adept at dealing with them. By evaluating the effectiveness of these informational nudges, we provide an empirical test of one potential cause of the ECE. We find that the effectiveness of these nudges is conditional on the number of options presented to the customer.

Methods

The primary objective of this study is to determine whether the ECE in this marketplace can be manipulated by informational nudges. Our field location - a wine bar – is one where an ECE might exist because it targets wine drinkers who may be less knowledgeable of the options on an extensive craft beer menu. This field setting provides three additional benefits. First, contrary to making hypothetical, stated preference choices, this study provides the ability to collect revealed preference data. Second, collecting data in this field setting adds context to the consumers' choices. Finally, because of our quasi-experimental design, consumers were unaware they were taking part in the experiment. We anticipate that the wine bar's consumers might appreciate a wide variety, but simply need assistance in their decision-making. Thus, we expect that this environment is one in which the ECE will arise, but where it might be mitigated through informational nudges.

The study utilized a 2x3 between subject quasi-experimental design that varied the number of options (between six and 12) and the type of menu (a control, one highlighting a special, and one with Beer Advocate scores). All menus listed beers in order from mildest to strongest and were structured to be similar to what the restaurant was already using prior to the experiment. Data for the field experiment were collected from September 2, 2014, until October 31, 2014, as revenue for these days were anticipated to be similar across treatments.[4] Six beer options were listed on the menu for the first half of the experiment and 12 beer options were listed on the menu for the remaining treatments. Table 4.1 displays information regarding the beer options for the field experiment. Prices for each beer ranged from $5.00 to $7.50 and each beer price was held constant throughout the experiment. The beer with the highest Beer Advocate score was Great Divide Hercules IPA (92) and the beer with the lowest score was the Stella Artois European Pale Lager (71). The option sets sizes were chosen so as not to drastically change the decision set of the restaurant's regular patrons, as the menu included nine options before the experiment. Another key concern is that order effects might affect the results: namely, that a temporal trend in sales might influence the percentage of beers sold each day. In order to minimize the possibility that results were due to differences in demand across months, we ran the six-option control menu at the beginning and the middle of the study. This way, we could compare the six-option control menu directly with the 12-option control menu. For each of the choice set sizes, we utilized a control menu and the two treatment menus. For logistical reasons, the treatments were changed sequentially on Monday of each week. Prices for each option were held constant over the term of the experiment (the appendix shows all beer options that were available at each time period along with prices in each menu). The additional six options of the second treatment were added between the first six options on the menu

[4]The total revenue for the days with six options was not statistically different from the total revenue for the weeks with twelve, implying that the study weeks were similar.

to constrain any positioning effects; e.g., people might be more likely to choose the option on the top (Dayan and Bar-Hillel 2011).

As is inevitable in all field experiments, a certain amount of control was lost. For example, changes in wait staff cannot be controlled within the data. To minimize potential measurement error, two types of data are collected and merged for the primary analysis. We first collected output from the restaurants sales system, which counts the number of beers (and other beverages) sold each day. To supplement the data, servers saved drink receipts throughout the day. While the daily sales data included all purchases, they do not provide a breakdown of what each consumer purchased, and one of the beers counted in the sales system was a draft beer listed elsewhere on the menu (the draft beer remained the same across weeks). By contrast, individual ticket data are beneficial because each observation is customer-specific, and each receipt includes which beer was selected.[5] We merged the two datasets by subtracting the number of draft beers sold each day as recorded on the sales receipts from daily beer sales.

Results

Over the course of the experiment, only 16.88% of all drinks sold were off the beer menu, supporting our hypothesis that the consumers at this wine bar did not have strong preferences for craft beer relative to non-beer alternatives. A chi-square test rejects the null hypothesis that the probability of selling a beer is independent of the number of options and the menu treatments (i.e., the presence/absence of an ECE depends on the

[5] Because the wait staff collected the daily sales data, 30.5% of the 787 total beers ordered are not accounted for within the daily sales data, but are accounted within the data collected by the restaurant sales system.

menu treatment). Table 4.2 shows the number of beers chosen as a percentage of all drinks chosen in the field experiment. Increasing the number of options from six to 12 decreased the percent beverage purchases accounted for by beer sales from 16.86% to 13.64%. Compared to the six-option control menu, highlighting a special increased beer sales from 16.86% to 20.14% and Beer Advocate scores increased sales to 19.93%. The special appears to lose its effectiveness when the number of options increased – only 13.25% of drink sales were from beer if 12 options were present. Compared to the 12-option control menu, Beer Advocate scores increased the percentage of beer sales from 13.64% to 20.44%.

For the control, beer sales as a percentage of all drink sales decreased as the number of options increased, indicating that an ECE exists in this field location. The odds of a beer being purchased when 12 options are offered is 0.959 times that if 12 options were offered in the control. Adding a special to the menu amplified the issue; the odds of a beer being purchased when 12 options and a special are present is 0.920 that if 12 options were offered with the same special. There was no ECE when Beer Advocate scores were on the menu. In the six-option set, adding specials or Beer Advocate scores increased the odds a customer purchased a beer. In fact, Beer Advocate scores increased the odds a beer was purchased when the number of menu items increased to 12 beers. The odds a beer was purchased from the six-option control menu was 1.005 times the odds a beer was purchased from the 12-option menu with Beer Advocate scores.

The effectiveness of specials and scores appear to vary depending on the number of available options. Specials appear to be more effective in the smaller choice set than in the larger choice set. The odds of purchasing a beer when the special was listed with six options is 1.244 times the control and 0.968 times the 12-option control. Scores increased the odds a beer is purchased by 1.627 times when 12 options were provided, but only increased the odds of a purchase by 1.227 in the six-option menu. However, because the 95% confidence intervals overlap, we cannot conclude that there is a statistically significant difference in effects.

As shown in figure 4.1, if the bar were to offer twelve options with a standard menu, it would expect to sell fewer beers than if it only provided six options. Specials would be likely to increase the probability of selecting a beer when the number of options are only six, but this increase in probability dissipates when the number of options increase to twelve. Finally, the Beer Advocate scores would have the most lasting impact on beer sales. Where both specials and scores increase the probability of selection by roughly the same amount in the six-option set, the scores significantly reduce the excessive choice effect.

These results face several limitations. As previously mentioned, research in the field requires some loss of control. We cannot know for which customers the beer menu constituted a consideration set, making causal conclusions regarding search costs arduous. While this is a fair criticism, much of the ECE literature has followed a similar identification strategy. Ideally, we would have been able to alternate 6-beer and 12-beer treatments, or randomized treatments each week over a longer set of weeks to minimize temporal effects. To address these concerns, we ran the six-option control menu the week before we ran the twelve-option control menu. If a temporal effect is in fact driving these findings, we would expect there to be a significant difference in sales between the first week (six beer control menu) and the fifth week (the twelve beer control menu), but that the difference would be less substantial when comparing the fifth (also a six beer control menu) and six weeks. When we just compare the fifth and six weeks, we can reject the null hypothesis that the six- and twelve-option control weeks sold the same amount of beer ($\chi^2_1 = 5.67, p - value = 0.017$).

Discussion

While this research does not provide decisive evidence that the excessive choice effect is a function of search costs, it does show that the effect itself can be manipulated through search-cost-minimizing nudges. It is important to remember that markets include heterogeneous buyers and sellers. Buyers generally self-select into different markets based on their own preferences and personalities, and sellers have an incentive to make the transaction. As such, it is possible that the average craft beer consumer does not experience the choice paralysis endemic of the ECE. Rather, the ECE is likely to be reserved to specific consumer groups within narrowly defined contexts (Authors, 2017).

This research provides an example of how some sellers meet consumer demands by either increasing or decreasing the number of options. Adding informational nudges such as product ratings or specials to a menu might encourage consumers to select an option when there are few options in differing contexts. This might be why some bars specialize in providing a large number of options; to target novelty-seeking craft beer drinkers. Environments full of familiar buyers will more often find a beer to drink if a menu lists specials or scores; however, added information can overwhelm unfamiliar buyers even more as the choice becomes more difficult to process.

Moving forward, other search-cost-reducing informational nudges might be analyzed in this market and in others. Especially with experience goods, information agents such as bartenders or social networks might mitigate the ECE. Similar to Beer Advocate scores, ratings and reports have the potential of minimizing search costs in other marketplaces. Firms such as Amazon and Uber provide five-star rating mechanisms to assist consumers in finding the utility-maximizing choice. A next step in this research might be to explore how prices interact with the ECE and the effect of these informational nudges. For example, listing a special with a significantly different price than the other options might have a stronger effect on sales than simply reducing the price without indicating it as a special. Ratings might be more important when comparing like products with similar prices as compared to like products with dissimilar prices (or unlike products with similar prices).

This research argues that firms have developed marketing strategies to help customers more appropriately identify choices that suit their desires. We validate the results of Kamenica (2008) and Norwood (2006), namely, that search costs influence the consequences of the excessive choice effect. Regardless of the information's relevance, increasing the volume of information in a marketplace causes search costs to change. We show that search-cost-lowering mechanisms used in markets have different effects on the probability of purchase. If consumers are relatively unfamiliar with choice options, sellers increase sales by offering fewer options and highlighting preferred varieties. However, if consumers are familiar, sellers offer more options and allow consumers to choose for themselves.

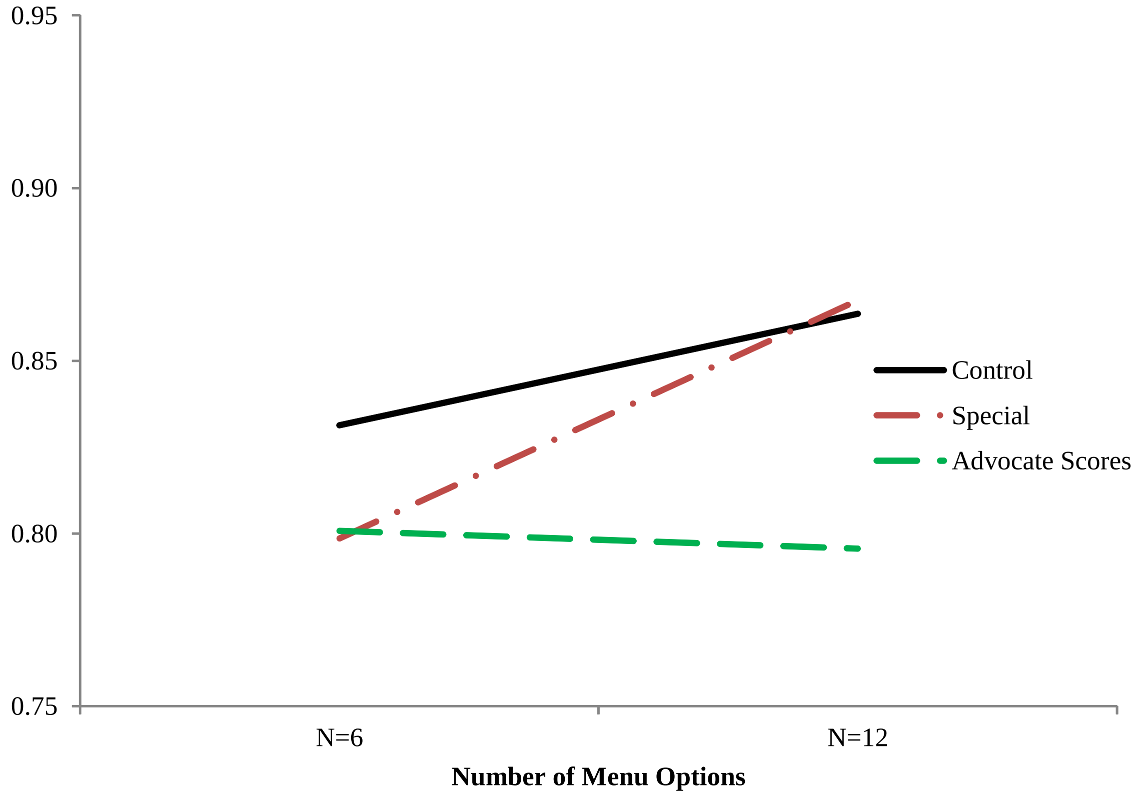


Figure 4.1. Probability of not selecting a beer in field experiment (note: upward sloping line indicates presence of an excessive choice effect)

Table 4.1. Beer options for the field experiment

Brewery	Name	Origin	Price ($)	Beer Advocate Score	Dates Available
Hoegaarden	Witbier	Belgium	5.50	85	10/5-10/25
Marshall	Old Pavilion Pilsner	Oklahoma	5.50	84	9/2-9/14, 10/5-10/25
Coop DNR	Belgian Strong Ale	Oklahoma	7.50	84	10/5-10/25
Stella Artois	European Pale Lager	Belgium	5.50	71	9/2-10/25
Big Sky	Trout Slayer Pale Wheat Ale	Montana	5.00	82	10/5-10/25
Great Divide	Hercules Double IPA	Colorado	7.50	92	9/2-10/25
Boulevard	Hibiscus Gose	Missouri	5.50	87	10/5-10/25
Rogue	Mocha Porter	Oregon	7.50	87	9/2-10/25
Kostritzer	Schwarzbier Black Beer	Germany	6.00	88	10/5-10/25
Steelhead	Extra Stout	California	6.00	90	9/2-10/25
Founders	Dirty Bastard Scotch Ale	Michigan	5.50	90	10/5-10/25
Coop	Horny Toad Blonde	Oklahoma	5.50	75	9/14-10/4
Angry Orchard	Crisp Apple Cider	Ohio	5.50	N/A	9/2-10/25

Table 4.2. Beer sales as a percentage of all drink sales in field experiment

Treatment	Number of Options		Odds Ratio - Effect of Number of Options[a]
	N=6	N=12	
Control	16.86%	13.64%	0.959 [0.907, 1.014][b]
Special	20.14%	13.25%	0.920 [0.867, 0.976]
Beer Advocate Scores	19.93%	20.44%	1.005 [0.958, 1.055]
Odds Ratio - Effects of Special and Scores[c]			
Control to Special	1.244 [0.922, 1.678]	0.968 [0.661, 1.417]	
Control to Scores	1.227 [0.929, 1.620]	1.627 [1.154, 2.295]	

[a]Odds ratio is the odds of beer being purchased in the N=6 treatment (i.e., the proportion of drink sales that are beer divided by the proportion of drink sales that are not beer) divided by the odds of beer being purchased in the N=12 treatment. An odds ratio less than one is indicative of an excessive choice effect.
[b]Numbers in brackets are 95% confidence intervals assuming each drink purchased represents an independent choice
[c]Odds ratio is the odds of beer being chosen in the control or specials menu (i.e., the proportion of times beer is chosen divided by the proportion of times beer was not chosen) divided by the odds of beer being chosen in the control menu. An odds ratio greater than one indicates the specials/scores increased beer sales relative to the control.

CHAPTER V

CONCLUSION

Recent findings from behavioral economists have called into question some of the standard assumptions maintained in econometric analysis. As such, the objective of this dissertation was to contribute to the reconciliation of these behavioral principles to economic theory. In the first chapter, we described a new method for reducing inattention bias in consumer surveys, and then showed how inattention bias can alter policy recommendations in the context of a beer tax. In the second chapter, we proposed and implemented a new way to distinguish brand perceptions from brand preferences. This approach showed how, relative to domestic macrobrew labels, a consumer's decision to purchase craft beer relates more to the perceived taste of the beer rather than the consumer's familiarity with the brand. In the third chapter, we conducted a field experiment focused on a small portion of craft beer consumers to determine the consequences of the excessive choice effect in market interactions. This study found that, although the ECE is likely to exist for some consumers, sellers have developed mechanisms targeted at reducing its negative consequences.

Given recent changes in the food value chain, future research might benefit by considering the behavioral principles outlined in this dissertation. At a minimum, discrete choice modelers might benefit by considering inattention bias in their study sample via a mechanism such as the Random Response Share (Malone and Lusk, 2017c). Demand modeling might benefit by separating perceptions and preferences within the utility function of their participants. Finally, future research might also benefit from identifying how markets interact with other apparent behavioral anomalies, such as time inconsistent preferences or sunk costs.

While each chapter contributes to a unique strand of literature, a unifying lesson can be gleaned from reading all three chapters together. Namely, that behavioral principles can enrich understanding of human action, and can remind economists to reassess the core principles of the discipline. By considering potential cognitive issues such as inattention bias, econometric models might more accurately determine policy impacts. Inconsistencies in perceptions need not generate "predictably irrational" preferences. Even Hayek (1952) alleged malleable perceptions as an additional reason for the necessity of the market and suggested that economic models include both preferences *and* perceptions. Additionally, when "cognitive failures" such as the excessive choice effect seem to challenges assumptions of economic theory, it is important to recall that markets are simply a dynamic collaboration between buyers *and* sellers. As such, the unifying lesson from this dissertation can be traced to Adam Smith's (1776, pp. 25) insight: "This division of labour, from which so many advantages are derived, is not originally the effect of any human wisdom, which foresees and intends that general opulence to which it gives occasion. It is the necessary, though very slow and gradual consequence of a certain propensity in human nature which has in view no such extensive utility; the propensity to truck barter, and exchange once thing for another."

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APPENDICES

By comparing models that allow for differences in scale variance, we can determine whether differences in models represent differences in preferences or differences in relative scale (Swait and Louviere, 1993). Table 2.A1 shows model estimates for the treatment with the embedded short trap question. The likelihood ratio test indicates that adding a scale parameter does not significantly improve the model's fit; however, dividing the dataset into two groups is a better fit. By conducting another likelihood ratio test, we determine that responses from those that incorrectly answered the trap question were statistically different from those who correctly answered the trap question the first time ($\chi^2_{df=1,0.05} = 126, \mathrm{p-value} < 0.001$). We conclude that participants who correctly and incorrectly responded to the trap question displayed different preferences from each other. The parameters that inflated were those for the most popular beer choices, while the parameters for the craft options actually converged to zero. For example, the price parameter for the Marshall Pilsner decreased to -0.082, making its value not significantly different from zero at the 5% level. Table 2.A2 shows model estimates for the treatment with the long trap question. In this instance, the likelihood ratio test indicates that adding a relative scale parameter improves the goodness-of-fit.

By conducting a likelihood ratio test, we determine that responses from those that incorrectly answered the trap question were statistically different from those who correctly answered the trap question the first time ($\chi^2_{df=12,0.05} = 145, \mathrm{p-value} < 0.001$). The results in this treatment follow a pattern similar to that exhibited by the above short trap question treatment, although more parameters lose their statistical significance. Five of the incorrect participant model's twelve parameter estimates are not statistically significant from zero. Again, parameter estimates for incorrect participants are not all higher than those who responded correctly. For example, while the Budweiser parameter estimate is 1.621 for incorrect participants, it is 1.968 for correct participants. For Miller Lite, we observe a parameter of 1.821 for incorrect participants and a parameter of 1.167 for incorrect participants.

Table 2.A3 shows the parameter estimates for those who missed the trap questions and did not revise to correct their wrong response versus those who did revise. When notified of their wrong response in the short trap question group, 53 of the 122 originally incorrect responses changed their response to a correct answer. Parameter estimates for persons who revised their responses were statistically different from those who did not revise their responses. ($\chi^2_{df=12,0.05} = 35, \mathrm{p-value} < 0.001$). Parameter estimates are different for those who correctly revised their responses. For example, the Corona parameter nearly doubled when comparing revising participants with non-revising participants. When notified of their wrong answer in the long trap question, 98 of the 150 originally incorrect responses changed their response to a correct answer. Parameter estimates for participants who correctly revised their responses to the long trap question were statistically different from those who did not correctly revise their

responses ($\chi^2_{df=12,0.05} = 32, \mathrm{p} - \mathrm{value} < 0.001$). Those who did not revise their responses responded more randomly than those who did revise their responses – fewer parameters were statistically significant at the 0.05 level. The participants who do revise their responses to the long trap question are statistically different from the participants who correctly responded the first time ($\chi^2_{df=12,0.05} = 46, \mathrm{p} - \mathrm{value} < 0.001$), but the majority of the variation comes from those who do not revise their answers, as those participants were statistically different from the rest of the sample ($\chi^2_{df=12,0.05} = 83, \mathrm{p} - \mathrm{value} < 0.001$).

Table 2.A1. Multinomial logit model estimates for the embedded trap question treatment

Variable	MNL	Relative scale parameter	Correct respondents	Incorrect respondents
Price of Miller Lite	-0.253*[a] (0.030)[b]	-0.231* (0.030)	-0.285*[d] (0.038)	-0.197* (0.053)
Price of Budweiser	-0.246* (0.025)	-0.224* (0.026)	-0.284* (0.029)	-0.131* (0.051)
Price of Corona	-0.226* (0.023)	-0.203* (0.024)	-0.231* (0.026)	-0.215* (0.055)
Price of Samuel Adams Lager	-0.219* (0.028)	-0.195* (0.028)	-0.201* (0.032)	-0.272* (0.057)
Price of Oskar Blues Pilsner	-0.334* (0.049)	-0.306* (0.047)	-0.370* (0.055)	-0.215* (0.104)
Price of Marshall Pilsner	-0.284* (0.037)	-0.258* (0.036)	-0.316* (0.040)	-0.082 (0.098)
Miller Lite	1.697* (0.142)	1.524* (0.161)	1.626* (0.171)	2.026* (0.267)
Budweiser	2.124* (0.124)	1.918* (0.159)	2.227* (0.141)	1.828* (0.262)
Corona	2.260* (0.117)	2.037* (0.161)	2.328* (0.130)	2.005* (0.273)
Samuel Adams Lager	1.743* (0.134)	1.555* (0.158)	1.605* (0.153)	2.192* (0.277)
Oskar Blues Pilsner	1.080* (0.208)	0.993* (0.194)	1.225* (0.233)	0.590 (0.466)
Marshall Pilsner	1.412* (0.166)	1.287* (0.163)	1.632* (0.179)	0.068 (0.468)
Scale parameter[c]		1.139* (0.083)		
Log likelihood	-7929.5	-7928	-6158	-1708.5
AIC	15883	15882	12340	3441
Number of observations	4472	4472	3496	976
Number of participants	559	559	437	122

[a] * designates statistical significance at the 5% level.

[b] Numbers in parentheses are standard errors.

[c] Scale parameter is the scale of correct participant relative to the incorrect participants, with the latter normalized to one.

[d] To compare these parameter estimates to the MNL with a relative scale parameter, the parameters for the MNL with correct respondents should be multiplied by the scale parameter value (1.139).

Table 2.A2. Multinomial logit model estimates for the long trap question treatment

Variable	MNL	Relative scale parameter	Correct respondents	Incorrect respondents
Price of Miller Lite	-0.291*[a] (0.032)[b]	-0.230* (0.029)	-0.311*[d] (0.038)	-0.254* (0.059)
Price of Budweiser	-0.250* (0.024)	-0.206* (0.021)	-0.331* (0.028)	-0.054 (0.044)
Price of Corona	-0.266* (0.023)	-0.215* (0.022)	-0.325* (0.027)	-0.087 (0.046)
Price of Samuel Adams Lager	-0.301* (0.029)	-0.245* (0.026)	-0.380* (0.036)	-0.136* (0.050)
Price of Oskar Blues Pilsner	-0.416* (0.057)	-0.347* (0.049)	-0.526* (0.075)	-0.213* (0.093)
Price of Marshall Pilsner	-0.289* (0.037)	-0.236* (0.032)	-0.356* (0.044)	-0.093 (0.074)
Miller Lite	1.270* (0.142)	0.970* (0.129)	1.167* (0.167)	1.821* (0.277)
Budweiser	1.801* (0.113)	1.417* (0.123)	1.968* (0.131)	1.621* (0.232)
Corona	1.941* (0.111)	1.523* (0.126)	2.093* (0.126)	1.629* (0.240)
Samuel Adams Lager	1.548* (0.130)	1.206* (0.126)	1.644* (0.156)	1.631* (0.253)
Oskar Blues Pilsner	0.661* (0.231)	0.560* (0.186)	0.858* (0.287)	0.625 (0.417)
Marshall Pilsner	0.910* (0.164)	0.736* (0.133)	1.086* (0.187)	0.529 (0.356)
Scale parameter[c]		1.366* (0.098)		
Log likelihood	-8307.5	-8296.5	-6104	-2131
AIC	16639	16619	12232	4286
Number of observations	4728	4728	3528	1200
Number of participants	591	591	441	150

[a] * designates statistical significance at the 5% level.
[b] Numbers in parentheses are standard errors.
[c] Scale parameter is the scale of correct participant relative to the incorrect participants, with the latter normalized to one.
[d] To compare these parameter estimates to the MNL with a relative scale parameter, the parameters for the MNL with correct respondents should be multiplied by the scale parameter value (1.366).

Table 2.A3. Multinomial logit model for participants who missed the trap question in the beer choice experiment

Variable	Embedded short trap question: Revised correctly		Embedded short trap question: Revised incorrectly		Long trap question: Revised correctly		Long trap question: Revised incorrectly	
Price of Miller Lite	-0.204*	(0.081)	-0.191*[a]	(0.070)[b]	-0.301*	(0.074)	-0.167	(0.098)
Price of Budweiser	-0.073	(0.075)	-0.180*	(0.070)	-0.058	(0.055)	-0.047	(0.073)
Price of Corona	-0.322*	(0.090)	-0.144*	(0.071)	-0.162*	(0.056)	0.088	(0.086)
Price of Samuel Adams Lager	-0.318*	(0.087)	-0.237*	(0.076)	-0.115	(0.065)	-0.170*	(0.080)
Price of Oskar Blues Pilsner	-0.141	(0.121)	-0.420	(0.221)	-0.293*	(0.129)	-0.110	(0.137)
Price of Marshall Pilsner	-0.012	(0.161)	-0.125	(0.125)	-0.168	(0.096)	0.019	(0.118)
Miller Lite	2.630*	(0.438)	1.711*	(0.344)	1.839*	(0.337)	1.922*	(0.497)
Budweiser	2.218*	(0.428)	1.692*	(0.342)	1.421*	(0.284)	2.128*	(0.416)
Corona	2.949*	(0.456)	1.470*	(0.350)	1.881*	(0.281)	1.067*	(0.480)
Samuel Adams Lager	2.997*	(0.449)	1.720*	(0.363)	1.249*	(0.320)	2.434*	(0.433)
Oskar Blues Pilsner	1.375*	(0.599)	0.370	(0.878)	0.606	(0.550)	0.908	(0.663)
Marshall Pilsner	0.172	(0.801)	0.070	(0.581)	0.595	(0.445)	0.636	(0.611)
Log likelihood	-730		-960.5		-1380		-735	
AIC	1484		1945		2784		1494	
Number of observations	424		552		784		416	
Number of participants	53		69		98		52	

[a] * designates statistical significance at the 5% level.

[b] Numbers in parentheses are standard errors.

VITA

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