INSIGHTS FROM BEHAVIORAL ECONOMICS ON DEVIATIONS

FROM RATIONAL CHOICE THEORY

A Dissertation

by

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ABSTRACT

This dissertation combines different research fields to enrich understanding of economic phenomena by integrating stated preference methods, experimental economics, and marketing. Specifically, two laboratory experiments are designed and conducted to study how the number of alternatives available impacts market valuation studies, and how small changes in the experimental design impact honesty and trusting in markets with asymmetric information.

Incentivizing has been proposed as a solution to the potential lack of candor in economic experiments, but how the number of alternatives available affects responses to incentives is uncertain. This question is explored using induced values in a discrete choice experiment, merging stated preference and experimental economics. Results indicate that engagement is positively correlated with profit-maximizing behavior, even after accounting for differences in payouts between alternatives. The number of alternatives available, however, does not affect profit-maximizing behavior when the difference between potential payouts is small, only when the difference between payouts is larger does profit-maximizing behavior improve. Results suggest that researchers can conduct incentivized choice experiments without all product alternatives available as long as participants are engaged with the task.

The second experiment studies markets with information asymmetry. In particular, how manipulating seemingly trivial aspects of a decision process influence honesty and trusting in an asymmetric market. This is accomplished using a seller-buyer game, merging experimental economics and marketing. Results show dishonesty of sellers with asymmetric information is partially mitigated by the interaction of adding a probability of being caught lying, making truth the default option, and having self-control available. Results indicate that self-control depletion also reduces trusting in buyers. Engagement plays an important role, with increased engagement resulting in less trust by buyers and more honesty in sellers.

A clearer picture of behavioral mechanisms in decision-making emerges by showing that engagement is more important than the number of alternatives available to promote profit-maximizing behavior. Further, the default option, self-control, and probability of being caught interact to affect honesty, and that engagement is crucial in economic decisions. This illustrates how behavioral economics contributes to solving economic problems in a multiple research area framework.

DEDICATION

Mamá, wherever you are, I hope I am making you proud.

Luna, if you ever get to read this, know that I pushed through it all with you in my heart and my mind at all times. Te amo hija.

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The data analyzed in Chapter 2 were captured at the Human Behavior Lab at Texas A&M University with the kind help of Professor Marco A. Palma. The data analyzed in Chapter 3 were captured at the Behavioral Lab of the Gatton College of Business and Economics at University of Kentucky. The analyses depicted in Chapter 2 and Chapter 3 were conducted by the authors.

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CHAPTER I

INTRODUCTION

Not so long ago, a widely shared view in various scientific circles was that some disciplines were inherently observational while others were experimental (Shapere 1982). The position of many economists was that the economics discipline was not an experimental field, as eloquently stated by Samuelson and Nordhaus (1985, p. 8) when they wrote "economists, like astronomers or meteorologists, … must be content largely to observe." The evolution of economics, however, challenges this view. Economic experiments and their thought-provoking results captured the interest of many economists (Kagel and Roth 1995). Early experiments conducted by Smith (1976), Plott (1982), Thaler (1980), and Camerer (1987), to name a few researchers that ventured in the field, provide examples of how experiments can play an important role in the science of economics.

Insights gained through carefully designed experiments have helped improve economic theory on reality, generality, and tractability – aspects that Stigler (1965) pointed out should be used to judge theories. In particular, behavioral economics – the subfield of experimental economics that relaxes rationality assumptions by using psychological theories – has contributed greatly to a better understanding of economic theory (Weber and Camerer 2006). Behavioral economics fills a knowledge gap at the individual level of analysis. At the aggregate level, economic theory explains most phenomena observed, while at the individual level, in contrast, economic agents may not behave as *homo economicus* (Fehr and Tyran 2005). Behavioral economics uses experiments to tease out explanations for deviations from strictly rational behavior (Camerer, Loewenstein, and Rabin 2004).

Two major contributions of behavioral economics have been to show (1) rational economic theory describes what consumers should choose, not what they choose (Thaler 1980), and (2) the context in which the decisions are made and how choices are presented matter (Kahneman and Tversky 1979). With this in mind, this dissertation shows how the actions that rational economic theory would predict from economic agents change when varying the context and presentation of decisions. The overarching objective of this dissertation is to use behavioral economics to elicit candid responses in experiments designed to increase the understanding of economic phenomena. This objective is achieved by designing and conducting two laboratory experiments that examine (1) how the use of monetary incentives to elicit truthful responses can be more complicated than just choosing between providing incentives or not, and (2) how trivial manipulations can be effective in moderating dishonesty in markets with information asymmetry. The two studies not only take advantage of the rigor that experimental economics stresses, but also benefit from technological advances that help understand the phenomena being analyzed. This dissertation is a display of the use of interdisciplinary research to enrich the understanding of economic phenomena by tapping into insights from stated preference methods, experimental economics, and marketing.

The Use of Incentives

Stated preference methods are widely used to estimate the value of market and non-market goods. A common stated preference method is choice experiments. Unfortunately, there are several potential biases and decision errors that occur commonly in choice experiments. Stated preference methods literature has proposed that the consequentiality of the decisions – the degree that participants perceive their choices to have consequences for them – reduces biases and decision errors (Carson, Groves, and List 2014). Meanwhile, in experimental economics and revealed preference methods it has been proposed that incentivizing – the use of monetary payments and/or making choices financially meaningful to participants – helps eliminate the lack of candid responses from participants in these kinds of studies, thus reducing biases and decision errors (Ding, Grewal, and Liechty 2005; Rakotonarivo, Schaafsma, and Hockley 2016; Penn and Hu 2018). There is an opportunity to merge these two streams of literature to evaluate how the number of alternatives (consequentiality) that are used to make choices financially meaningful to participants (incentivization) influences decision-making.

In a stated preference discrete choice experiment, a series of choices are presented to participants and they are asked to state their preferences between the alternatives in each choice set by choosing one of the alternatives (Louviere 2006). Based on the attributes, the attribute levels, and the choices that the participants make, the preferences of participants are collected, and the marginal utilities of the different attributes and attribute levels are estimated (Hensher 1994). To promote candid responses using this framework, researchers may decide to conduct an incentivized version of a choice experiment. Participants in an incentivized choice experiment (ICE) are told that one or more of the choices they make will be enacted and as such, it will have consequences, financial or otherwise. The common practice is to randomly determine which of the choices will be enacted, but all the alternatives that are presented to participants are readily available to participants if they choose them. With this setup, in an ICE all of the alternatives that are presented to participants could potentially have consequences. A setup where only some of the alternatives are available could change the perception of the consequentiality of participants and have some impact on the effectiveness of incentivization.

The objective of chapter II is to determine whether and how the number of alternatives with the potential for consequences has an effect on choice behavior in valuation studies for private goods. To achieve this objective, a discrete choice experiment is designed to determine whether the number of alternatives that could potentially have consequences changes participants' profit-maximizing behavior. Because there is a unique profit-maximizing alternative in each choice set, the performance of participants is captured objectively by evaluating when the option selected was or was not the profit-maximizing alternative. The effect of the number of alternatives with consequences is measured by randomly assigning participants into one of four treatments each with a different number of alternatives that could potentially have consequences. The choice experiment is presented to participants on a computer screen using an eye-tracking device that allows for the measurement of the search behavior of participants. The measurements captured with this device are used to better understand the decision-making mechanisms.

When accounting for individual heterogeneity, consequentiality (the number of options that would potentially have consequences to participants) seems to lose its ability to predict better performance, while engagement becomes an important explanatory variable of profit-maximizing behavior. Engagement remains relevant even when analyzing the data separately to account for differences between alternatives presented. These results suggest that engagement is a more accurate predictor of profit-maximizing behavior than the number of alternatives with the potential for consequences, highlighting the importance of increasing the relevance of the task to participants in experimental and valuation studies in eliciting candid responses.

The Use of Nudges

At the intersection of buyers and sellers with the intent to cooperate and transact, we find markets. It is the case that many, if not all, markets have asymmetry in the information available to the parties involved. Generally, one party knows more about the potential transaction than the other. Important research questions for markets with asymmetric information are what conditions increase sellers' propensity to be truthful and what conditions make buyers more trusting. The creativity of experimental economics and the behavioral considerations from marketing research are merged in Chapter III to explore seller and buyer behavior in markets with information asymmetry. The study presented in Chapter III has the objective of finding out how nudges, seemingly innocuous manipulations (like having a default option or depleting the self-control of a participant), may affect honesty and trust in a market with asymmetric information.

The study uses a computer-generated interactive experiment where participants are randomly assigned the roles of sellers and buyers to explore what conditions decrease (increase) lying behavior by sellers and trusting behavior by buyers. The focus of the study is on nudges and behavioral conditions that may affect honesty and trust. Nudges are ostensibly trivial manipulations that present options to decision-makers in such a way that the desired outcome is more likely to be selected. Some nudges, for example, change the presentation of how the alternatives are described, like having to check a box to opt-in to a retirement savings program versus having to check a box to opt-out of the program, for example. This type of manipulation may seem trivial, but it has been shown to dramatically change outcomes (Thaler and Sunstein 2008).

With this in mind, the study in Chapter III uses nudge manipulations to either promote lying or truth-telling. In the experiment designed for this study, sellers send a message to buyers, a one-line sales pitch, by clicking on one of two options. One nudge manipulation presents sellers with a pre-selected sales pitch. The pre-selected message (default option) that sellers can send to their buyers is either a lie or the truth. Sellers can then readily submit the pre-selected one-line sales pitch or change the message to the alternative option before submitting their sales pitch to buyers. The prediction is that the nudge would lead participants to truth-telling when the default option is the truth, and steer participants to lie when the default message is a lie. The effectiveness of the nudges is evaluated by examining if lying behavior differs when the default is the lie versus the truth.

Another nudge manipulation used in this study is varying self-control depletion of participants. Self-control is defined as the capacity to alter one's behavior (Baumeister and Vohs 2007) and can be interpreted as a depletable resource (Muraven and Baumeister 2000). By exposing some participants, buyers and sellers alike, to a self-control depletion task, the differences in lying (sellers) and trusting (buyers) between those participants exposed to the task and those who are not exposed give a measure of the influence of self-control depletion on behavior.

Finally, given that the probability of being caught mitigates (or fosters) dishonest behavior (Bryant and Eckard 1991), this study evaluates the effect of the likelihood of being caught in a lie. This behavioral condition is explored by randomly assigning some sellers to a manipulation where they may be caught if they lie. Morality, engagement, and numerical ability of participants are measured, along with demographic controls, to gauge the effect of such characteristics on participant behavior.

The results indicate that the interaction among the default option, the availability of self-control , and the chance of being caught matter when it comes to lying behavior of sellers in the market with asymmetric information. Further, depletion of self-control has an effect, not only on the behavior of the sellers but also on the buyers. Sellers are less likely to lie when self-control is available, there is chance of being caught lying, and the default message is the truth than in any other of the conditions. At the same time, buyers mistrust more the messages they get from sellers when self-control has been depleted. Results also highlight the importance of engagement in decision-making. Buyers' trusting behavior was higher when their engagement was lower. Sellers who were engaged with the task were less likely to lie. These findings also underline that how relevant the task is to participants can influence the results.

CHAPTER II

ON THE USE OF INCENTIVES: PARTIAL AND FULL INCENTIVIZATION IN DISCRETE CHOICE EXPERIMENTS

Stated preference methods such as choice experiments are widely used for market and non-market valuations. The potential exists for various biases in stated preference studies including response bias (Diamond and Hausman 1994), justification and policy bias (Bonsall 1985; Bates 1988), aggregation bias (Morrison 2000), strategic bias (Lu, Fowkes, and Wardman 2008), and hypothetical bias (List and Gallet 2001; Murphy et al. 2005; Penn and Hu 2018). Researchers are gravitating towards methods that help mitigate potential biases. One such method for market valuations is the incentivized choice experiment (ICE) (Ding, Grewal, and Liechty 2005; Rakotonarivo, Schaafsma, and Hockley 2016; Penn and Hu 2018). Contrasted to non-incentivized choice experiments, an ICE the participants make choices over a series of choice sets presented to them and some or all of these choices are acted upon. This framework implies that there are individual consequences to participants stating their preferences in an ICE.

Because of the nature of public goods – non-rival and non-exclusive (Loomis 1996), and difficult to provide in an experimental context (Gächter and Renner 2010) – ICEs are not as common in the public goods valuation literature, but instead are mostly used in private good valuations to determine how consumers respond to new products, new features of existing products, or new production technologies (Hensher 1994). New products or features not available in real markets, however, pose a challenge in designing ICEs for market valuation because some alternatives in the choice sets cannot be used for incentivization simply because they are not available. Given that deception is generally not allowed in economic experiments (Colson et al. 2016), researchers conducting ICE have to inform participants that some of the alternatives in the choice sets are not available. Consequently, the information about alternative availability may modify participants' choice behavior, especially when it is uncertain whether participants fully understand the procedures (Cason and Plott 2014). Furthermore, with the awareness that not all alternatives are available, participants may recognize not all decisions are consequential, which may change the perceptions of participants and how much effort they exert not only to understand the procedure (Bardsley 2005), but also to reveal their true preferences (Yang, Toubia, and de Jong 2018). Such a lack of engagement may be exacerbated by the fact that ICEs are a novel task for most participants, and hence potentially limiting the reliability of information obtained. The main question this study seeks to address is whether the participants' behavior in ICEs differs when the number of product alternatives with the potential of consequences differs.

The objective of this study is to determine whether and how the number of alternatives with the potential for consequences (a cash bonus payment in this study) has an effect on choice behavior in valuation studies for private goods. To achieve this objective, an ICE is designed to gauge whether the number of alternatives with the potential for consequences changes participants' profit-maximizing behavior. The study uses the induced values (IV) framework to assign value to the options presented in the ICE. Because there is a unique profit-maximizing alternative in each choice set, the performance of participants is measured objectively by evaluating the deviations from the profit-maximizing alternative. The effect of the number of alternatives with the potential for consequences is captured by randomly assigning participants into one of four treatments differing in the number of alternatives with potential consequences. The ICE is presented to participants on a computer screen using an eye-tracking device that allows for the measurement of the search behavior of participants, information that could help understand the decision-making process better.

Literature Review

Probability of Enactment

Enactment is crucial to consequentiality and economic research (Poe and Vossler 2011). Enactment in the context of economic experiments can be defined as the carrying out or putting into action the financial incentive structure to provide salient payoffs to participants (Hertwig and Ortmann 2001). Consequentiality as a result of availability is at the crux of the possibility of enactment for private goods valuation and ICEs. Rose and Hensher (2006), for example, highlight that non-availability in the market is an issue in constructing choice sets because the array of choice sets offered to participants may not represent the actual choices participants may have available in real-world settings. Other authors focus on the effect of the existence or absence of products in the market (Batsell and Polking 1985; Raghavarao and Wiley 1986) and how to incorporate market (un)availability into the marginal utility estimations of choice models (Anderson and Wiley 1992; Lazari and Anderson 1994).

Given the limitations to enactment in the public goods domain, research concerning public goods provision has been finding creative ways to measure the effect of availability of alternatives on decision-making and how it relates to consequentiality. Vossler and McKee (2006) in a public good provision experiment using majority voting find differences in the voting behavior of participants between hypothetical and consequential voting. The authors classify the differences in voting between conditions of hypothetical and consequential as decision errors. The authors observe there are fewer errors when the difference between the expected payout of the public good and the cost of provision is larger. Landry and List (2007) use a referendum to fund the provision of a good to all participants comparing the probabilities of enactment. They attempt to explore the difference between consequential and real by defining a "consequential" condition as a 25% chance of enactment and a "real" condition as 50% chance of enactment. These conditions are then contrasted to a hypothetical control condition with 0% chance of enactment. The comparison between the consequential and real treatments found no differences, but they both provided different results than the hypothetical control. Another study of consequentiality in a public good setting is Vossler and Evans (2009). They use a referendum where a majority vote will fund a classroom recycling container. The manipulation in this study conditions the enactment of the referendum on a third party. This third party acts as a moderator of the vote and has either 25% or 75% of the total votes. Results indicate if participants perceive their votes as consequential, even when they are unlikely to be (as in the 75% moderator votes condition), they behave in an incentive-compatible way. In a follow-up study with induced values, Collins and Vossler (2009) have the provisioning rule be either be majority vote, a second situation where the number of votes is the probability of implementation or a third scenario where a moderator has as many votes as all the participants. This study is close to the present one in that Collins and Vossler (2009) use induced values to test the different forms of referenda, whereas the current study uses induced values to test the different number of potentially consequential alternatives. Their results show that voting errors occur less frequently when the spread (the difference between the payments of the induced value options) is larger. Finally, another study related to the present study is the work of Carson, Groves, and List (2014), who in a referendum for a public good provision found that increasing the probability of enactment between 0, 20, 50, or 80% chance of enactment made participants more likely to vote in favor of the referendum in any of the non-zero chance of enactment versus the hypothetical (0% chance of enactment) condition.

Although these findings are informative, there exist differences between public and private goods that may make findings on consequentiality in one stream of literature not directly transferable to the other. One important distinction is that in public goods provision, enactment probabilities are not linked to the number of goods available, as there is one good to be distributed to every participant if the public good provision is approved and the vote is enacted. The present study explores an intersection between the two bodies of literature by extending the findings of public goods experiments into private goods valuation. This is done by exploring how enactment probabilities linked to the availability of the goods, affects the consequentiality of the choices, and changes the behavior of participants in a private good valuation setting.

Liebe et al. (2018) showed that consequentiality matters in a private goods valuation setting by observing that participants facing consequences have different preferences than participants who are not facing consequences. A challenge in the use of consequences with market goods, however, is that in many instances not all product alternatives presented to participants are physically available. Certain products or features of products do not yet exist, but it is important to evaluate their consumer potential before making large investments in product development (Hoyos 2010; de Bekker-Grob, Ryan, and Gerard 2012). An important methodological question, therefore, arises: what to do when some of the alternatives are not readily available, making it impossible to enforce market institutions and provide consequences to participants' choices? It remains unexplored whether participants' choice behavior in private good valuation changes with the probability of enactment as a form of consequentiality. Given that enactment probability in market good valuation is based on the number of product choices available for purchase in an ICE, this is a gap in the literature that the present research addresses.

Hypothetical Bias and Mitigation Techniques

ICEs have become one of the most widely used tools in market valuation (Hess, Hensher, and Daly 2012). Despite their wide use, ICEs still face strong criticism because of some implied assumptions regarding the behavior of participants that if violated may bias the results (Hensher, Rose, and Greene 2005; Janssen et al. 2017). One assumption under scrutiny is whether participants' revealed preferences are consistent with their true preferences (Sælensminde 2002; Ding, Grewal, and Liechty 2005; Quaife et al. 2018). Though there is evidence that revealed and stated preferences are similar under some circumstances (Haghani and Sarvi 2018), the literature suggests that this may not always be the case (Lusk and Schroeder 2004; Levitt and List 2007; Baltussen et al. 2012).

Inconsistency between stated and revealed preferences is exacerbated in the absence of economic incentives (i.e. hypothetical bias). List and Gallet (2001), for example, report an average overstatement factor of 3.16 in willingness-to-pay (WTP) elicitations in hypothetical versus incentivized conditions. To address hypothetical bias, practitioners implement procedures such as cheap talk, honesty priming, and certainty scales to mitigate this bias in WTP studies (Fifer, Rose, and Greaves 2014). The effectiveness of cheap talk – communicating in a non-binding way with participants before a hypothetical choice task scenario – in reducing hypothetical bias is mixed, with some studies showing cheap talk reduces bias while other studies find no effect (List 2001; Lusk 2003; Silva et al. 2011). Another commonly used approach is honesty priming – participants are primed with statements that value honesty under the idea that priming tasks encourage individuals to be more truthful – also has mixed results (de-Magistris, Gracia, and Nayga 2013; Bello and Abdulai 2016). Certainty scales that ask participants to report how certain they are of their stated preferences have also been documented to help mitigate hypothetical bias (Blomquist, Blumenschein, and Johannesson 2009).

While such techniques may be useful in mitigating hypothetical bias, the problem persists. Studies suggest that the most suitable solution for eliminating hypothetical bias is to use economic incentives (Brock and Durlauf 2001; Dong, Ding, and Huber 2010). Not surprisingly, researchers have followed suit by using ICEs (Lusk and Schroeder 2004; Michaud, Llerena, and Joly 2013). In choice experiments, incentivized or not, a series of choice sets are presented to participants. To incentivize the choices in an ICE, the decision made in one (or more) choice set(s) has(ve) consequences. The consequentiality in this context comes from the fact that participants will have to pay for and/or consume what they stated they would.

Determining which choice set is the one that will have consequences is generally based on a random draw (Vossler, Doyon, and Rondeau 2012). This is believed to be incentive-compatible under the idea that choosing randomly at least one choice set where participants will have to face consequences to their actions is sufficient enough to reveal their true preferences. Because neither the participants nor the experimenters know which choice set(s) will become consequential, participants have the motivation to respond truthfully to all choice sets presented (Collins and Vossler 2009; Beck, Fifer, and Rose 2016). Correspondingly, not all choices have consequences because not only would it be costly to give every decision a consequence, but also participants buying or consuming products for every choice set brings up issues with decreasing marginal returns, complementarities, wealth effects, and unobserved strategic behavior (Knetsch 1989; Yang, Toubia, and de Jong 2018). This study incentivizes the choices of participants in the non-hypothetical manipulations by choosing one choice randomly with the roll of a die.

Induced Value Theory

Exploring how any manipulation – such as the number of alternatives with the potential for consequences – affects behavior in choice experiments is difficult because a design that allows for the identification of optimal solutions *a priori* is necessary (Friedman and Sunder 1994). If the optimal choice in every choice situation is unknown, the choices made may not be as informative in terms of the treatment effects, as the choices observed could be the result of preference heterogeneity rather than treatments (Greene and Hensher 2013). One design tool that may address this issue is the induced values (IV) framework (Coursey, Hovis, and Schulze 1987; Taylor et al. 2001; Braga and Starmer 2005). With this framework, the "goods" being offered to participants are given an induced value by the experimenter, and the goods have no inherent value to participants, the goods only have the worth that experimenters inform participants (Smith 1976).

IV theory proposes that, in a controlled experiment, the decision between options is determined by how these options differ (Smith 1976; Smith and Walker 1993). If the researcher can unequivocally account for and measure how choices presented differ and uses random assignment of treatments, causal inferences can be made (Antonakis et al. 2010). If the experimenter, for example, knows which of the choices provides the largest monetary payoff, inferences can be made about how the treatments affect the profit-maximizing behavior of participants (Paul 1982). This study takes advantage of the properties of the IV framework to evaluate the effect of the number of alternatives with consequences on choice behavior in ICEs. By knowing which option presented to participant is the one with the largest payoff and making the reasonable assumption that the preferences of participants are increasingly monotonic on money – more money is preferred to less money – the IV design allows for identification of decision errors and a measure of performance in this study.

Aspects Affecting Effectiveness in the Use of Incentives

In economic experiments, treatment effects manifest if the treatments are relevant and noticeable to the participants (Bardsley 2005). When participants fail to correctly identify the game being played, their decisions are not connected to the consequences of their actions (Chou et al. 2009; Herriges et al. 2010; Vossler, Doyon, and Rondeau 2012; Vossler and Watson 2013; Cason and Plott 2014). Participants' cognitive ability can impede or enable identification of the optimal course of action (Robinson 1998; Banks, O' Dea, and Oldfield 2010; Alós-Ferrer et al. 2012; Chen et al. 2012; Kløjgaard, Bech, and Søgaard 2012). Numeracy, broadly defined as "…the ability to process basic probability and numerical concepts…" is one cognitive ability that has drawn attention in this context (Peters et al. 2006, p. 407). Participants who possess low numeracy skills may be less likely to make the connection between their choices and the consequences of those choices.

Subjective measures of numeracy have the benefit of measuring ability without imposing an additional cognitive load on participants. The main shortcoming of subjective measures is that they are self-reported measures and may not necessarily correlate with actual numerical abilities (Dunning, Heath, and Suls 2004). In contrast, objective measures do not have this shortcoming, although they may be taxing cognitively (Grebitus and Davis 2019). In this study, an objective measure of numerical ability is employed using questions originally developed by Schwartz et al. (1997) and adapted from Weller et al. (2013).

Participants in an ICE may also fail to recognize the saliency of incentives because they are simply not engaged with the task (Börger 2016). Levels of engagement with the choice task in ICEs result in participants systematically using or ignoring information, leading to choices that may not necessarily reflect their true preferences (Hensher, Rose, and Greene 2012; Hole, Kolstad, and Gyrd-Hansen 2013; Scarpa et al. 2013). Two methods are used to measure engagement in this study. One uses biometrics, which will be discussed in the next section. The other one is a test consisting of three questions, the cognitive reflection test (CRT) proposed by Frederick (2005).

A higher score in the CRT indicates a higher reflective state, albeit not necessarily a higher level of cognitive ability (Hoppe and Kusterer 2011). Studies on the use of CRT in decision-making show that higher levels of CRT have a positive effect on judgment and making better choices (Campitelli and Labollita 2010; Toplak, West, and Stanovich 2011), but it has not been used commonly in the context of ICEs. It is important to highlight that when Frederick (2005) proposed the CRT, he intended to measure the ability to resist answering with the first thing that comes to mind and instead reflects on a question deliberately. Research on the interpretation of CRT scores has suggested that a higher score indicates a higher degree of engagement to the task at hand. The ability to reflect on a decision has been shown to come from the ability of participants to engage in analytical processing (Pennycook, Fugelsang, and Koehler 2015). This is the context where CRT is used as a measure of engagement throughout this document. When participants in the studies score high in the CRT, they are engaging in analytical thinking with the task at hand.

Eye Tracking Measurements

It is the case that in experiments like the one conducted in this study, regardless of whether incentives are present or not, the responses are self-reports of participants. The use of incentives, as described earlier, is aimed to get participants to reveal their true preferences and provide candid responses. The effectiveness of different techniques to get candor from participants in their responses is still under scrutiny (Sælensminde 2002). It is the case, however, that the physiological responses of individuals are harder to fake (Houston and Holmes 1975) and can be trusted generally as truthful responses (Proverbio, Vanutelli, and Adorni 2013). Economic research can benefit from using biometrics to measure participants' responses (Camerer, Loewenstein, and Prelec 2005). The use of biometrics enables researchers to explore elements in the behavior of decision-makers in a non-intrusive way (Gilmore and Erdem 2008; Kaplinski and Tupenaite 2011), which is why this study uses an eye-tracking device.

Eye-tracking devices like the one used in this study are a set of high-resolution infrared cameras that follow participants' eye movements on a screen (TobiiAB 2015) gathering information on the position of the eyes on the computer screen, time spent perusing and moving between stimuli, distance to the screen, and, depending on the device, other measures related to visualization of the stimuli (Khushaba et al. 2013). Using eye-tracking in economics is not new, but is gaining popularity as the technology becomes more accessible (Reutskaja et al. 2011; Lahey and Oxley 2016). Eye-tracking usage spans many different decision-making aspects including fatigue (Louviere 2006), point of purchase behavior (Rasch, Louviere, and Teichert 2015), attribute non-attendance in choice experiments (Balcombe, Burton, and Rigby 2011; Chavez, Palma, and Collart 2017), and time spent on a choice (Maughan, Gutnikov, and Stevens 2007). Pupil dilation, a metric obtained in eye-tracking, is the second measurement of engagement used in this study since pupil dilation has been documented to be an indicator of engagement (Hoeks and Levelt 1993; Einhäuser et al. 2008; Wang, Spezio, and Camerer 2010). Previous studies have shown that, in the presence of economic incentives, engagement is enhanced (Small et al. 2005) even when participants have no prior experience with the task (Heslin and Johnson 1992). Using eye-tracking to measure pupil dilation in the context of this study is a helpful instrument to verify whether the number of alternatives with consequences has an effect on engagement in decision-making in ICEs.

Methodology

Experimental Design

The experiment is conducted using 152 general population participants (non-students) recruited through local newspaper ads in College Station, Texas. Participants are randomly assigned to one of four treatment conditions: hypothetical control, partial availability - low, partial availability - high, and full availability.

The experimental design uses a similar idea to the one developed in the IV experiment of Luchini and Watson (2014). Each participant is shown 12 choice sets with two alternatives and an opt-out option. The alternatives presented to participants to choose from are shapes of different colors. The value of each shape/color combination depends on three attributes: price to purchase, shape, and color. Four purchase prices are included: \$0.50, \$1.00, \$1.50, and \$2.00. The values of each of the three different shapes (square=\$0.50, triangle=\$1.00, and circle=\$1.50) and the two different colors (green=\$0.50 and blue=\$1.00) differ. This setup then has three attributes, with four, three, and two levels each, which produce a total of 24 different combinations of color, shape, and price. Profits are calculated as the sum of the shape and color values minus the purchasing price. For example, a blue circle at a purchase price of \$1.00 has a profit of \$1.50 (blue [\$1.00] + circle [\$1.50] – price [\$1.00]).

In each pair, one alternative maximizes profits. The difference in profits between low- and high-paying shape was \$0.50 for seven pairs, \$1.50 for four pairs, and \$2.50 for one pair of shapes. In this IV design, choosing the largest paying alternative for each choice set is the optimal strategy for maximizing profits. Further, every pair has at least one alternative with non-negative profits. In two of the choice sets, the profit-maximizing alternative had a payout of \$0, while the other alternative yielded a negative payoff. These choice sets are included as attention checks. See Appendices A and B for an example of a choice set presented and the instructions given to participants. The experimental design was developed in Ngene with a final D-error of 0.1492 (ChoiceMetrics 2014).

A total of 12 choice sets that include all possible 24 alternatives are presented to each participant. The same 12 choice sets are presented to all participants in all treatments.

The main difference across the treatments is the number of alternatives with consequences. In a traditional non-incentivized choice experiment, none of those 24 alternatives would have consequences. Therefore, in the hypothetical control, participant's choices are hypothetical and none of the choice sets is selected for payment. In the other three treatments, participants are informed that one of the alternatives they choose will be used for a bonus payment. In an ICE, all the 24 alternatives could potentially have consequences. Therefore, the full availability treatment is equivalent to the common practice in ICE – all 24 alternatives can potentially be consequential.

For the partial availability treatments, the procedure to inform participants about the number of alternatives with potential consequences is designed such that participants possess full information about how many alternatives can have consequences and showing participants that the experimenters possess the same level of information as they do. Each of the 24 alternatives was written down on a piece of paper. For each treatment, participants are informed that *n* randomly selected alternatives are to be placed inside a box to be eligible to become consequential. The number of alternatives placed inside the box corresponds to the treatment assignment (8 or 16). That is, the partial availability – low condition had eight alternatives with consequences, while the partial availability – high condition had 16 alternatives with consequences. The box containing the selected alternatives was placed next to the participant to ensure that the number of alternatives with consequences was salient. To preserve incentive compatibility, participants did not find out which alternatives were inside the box until the end of the experiment. After reading the instructions, subjects completed a practice round. Results of the practice round were extensively discussed to ensure that participants understood the procedure.

Table 1: Summary of Choices Enacted in Each Treatment Condition

Treatment	Number of available alternatives	Probability of an alternative is available
Hypothetical control	0	0%
Partial availability – low	8	33%
Partial availability – high	16	66%
Full availability	24	100%

Once participants had no more questions and verifying they remembered and understood the payout structure, they advanced to the choice task stage. After completing the choice task, subjects filled a survey consisting of demographic questions, a numeracy skill quiz adapted from Weller et al. (2013), and the CRT (Frederick 2005). The complete survey instrument including the scales can be found in Appendix C.

To incentivize the responses, participants are given the profit they made on a randomly selected choice set as discussed as a bonus to their $20 compensation for participating in all treatment conditions except the hypothetical control. The procedure to get the bonus was as follows. Upon completing the survey at the end of the study, subjects rolled the 12-sided die to determine the prize choice set.

To receive the bonus, the alternative a participant chose in the prize choice set had to be inside the box. In the full availability condition, since all 24 alternatives are inside the box, participants are guaranteed to receive a bonus payment based on the die roll and their chosen alternative. In the partial availability conditions, only 8 or 16 of the alternatives have randomly been placed in the box for the bonus payment. The number of potentially consequential alternatives and chances of getting a bonus in each treatment are summarized in table 1.

Participants are not guaranteed to receive a bonus payment in the partial availability conditions. The bonus payment depends on three aspects: the alternative chosen by the participant, the die roll, and whether the alternative is inside the box, i.e. if the alternative has consequences. The combination of the probability of enactment (determined by the die roll) and the probability of the selected option being available (determined by the random alternative selection) can be interpreted by participants as a compound lottery (Samuelson 1952; Segal 1990). Random payment systems that take the form of compound lotteries of this nature can generate distortions to how incentives are interpreted by participants, and alter the preference revealing properties of an incentive-compatible setup (Azrieli, Chambers, and Healy 2018). Research on the ability of random payment mechanisms of this sort has shown, however, that as long as researchers can safely assume that preferences of participants are monotonic, that one option pays more than the alternative (defined as a state-wise monotonicity condition), and the choices are not presented all at the same time, the incentive compatibility of the mechanism holds (Azrieli, Chambers, and Healy 2018; Brown and Healy 2018). In this study, each choice is presented separately in different screens, the state-wise monotonicity condition is satisfied, and the assumption that participants are profit-maximizers with monotonically increasing preferences over profits is made. These conditions imply that the bonus payment would be incentive compatible.

The experiment is presented as a slide show on a computer. The order of the choice sets is randomized for each participant to control for ordering effects. To balance the position presentation, the largest-paying alternative is the option on the left in half of the choice sets and on the right for the other half. Between choice sets a slide with a bull's eye at the center of the screen is inserted as a distractor and presented for three seconds to re-center the attention of participants and help avoid previous choice bias. Participants spend as much time as needed on the choice sets. The experiment is conducted in a laboratory with no windows, using fluorescent light of 750 lumens and 6500 Kelvin color temperature to light the room, thus keeping luminosity constant across participants, an important consideration for comparison of pupil dilation.

The iMotions platform (iMotions 2018) is used to display the choice sets on a 1920 x 1200 pixel screen using a Tobii TX-300 screen-based eye tracking device. The eye tracking device is embedded in the computer screen, tracking and recording eye movements using near-infrared technology at a sampling rate of 120 data points per second. At the beginning of the experiment, the eye tracking device is calibrated to ensure proper data collection for each individual using a nine-point calibration method. To analyze the eye movements of participants, areas of interest (AOI) on the slides with the choice sets are defined. These AOIs were polygons (rectangles) that include the shapes, the price for each shape, and the opt-out choice. Five AOIs per choice set were constructed. Because the slides are symmetric, the AOIs are also symmetric and equidistant. Fixation data from the AOIs are used for the analysis of the eye-tracking metrics of participants.

Research Hypotheses

Based on the experimental design and manipulations, four hypotheses are formulated. Although not stated, the respective alternative hypotheses cover all possible outcomes except the null.

Hypothesis 1: *There is a significant effect of the number of available choices with the potential for consequences on the performance of participants (selecting the profit-maximizing alternative) in ICE.*

The literature documents the benefits of using incentives in ICE (Collins and Vossler 2009; Cerroni et al. 2019). The design allows measuring the effect of the number of choices with consequences on a participant's behavior in a controlled environment. More choices with the potential for consequences may increase the saliency and relevance of the task to participants.

Hypothesis 2: *There is an increase in performance in the choice task as the number of alternatives with the potential for consequences increases.*

This hypothesis builds on hypothesis 1 by providing a direction and magnitude to the effect. If there is support for this hypothesis, users of ICE may want to have as many alternatives with consequences as possible to ensure participants engage with the experimental task and reveal their preferences.

Hypothesis 3: *A larger number of choices with the potential with consequences in ICE changes participants' visual behavior.*

Previous literature has shown that time spent perusing a choice and search behaviors within choice sets are predictors of decisions (Shi, Wedel, and Pieters 2013). Changes in the visual behavior measured with eye-tracking technology are compared across treatments. Support for this hypothesis implies that not only the outcomes, as in hypothesis 1 but also the cognitive processes vary with the number of choices with the potential for consequences.

Hypothesis 4a: *Better numerical ability increases performance in ICE regardless of the number of choices with the potential for consequences.*

Hypothesis 4b: *Better cognitive reflection increases performance in ICE regardless of the number of choices with the potential for consequences.*

Previous findings indicate higher numerical ability and engagement increase performance (Robinson 1998; Kløjgaard, Bech, and Søgaard 2012). The experimental design allows for exploring the extent to which choice behavior is robust regarding variations in numerical skills and engagement with the task when different numbers of alternatives with the potential for consequences are tested.

Econometric Modeling

The analysis quantifies the magnitude of the effect of the number of alternatives with the potential for consequences on the probability of participants making profit-maximizing choices, controlling for demographics, numeracy skills, CRT scores, and eye-tracking metrics. The binary nature of the outcome, either picking the profit-maximizing choice or not, allows for modeling the choices with the multinomial logit model (Greene 2012). Because participants make selections across choice sets, the data have a panel structure. In this setup, individual characteristics of participants – observed and unobserved – remain constant across choice sets and are unrelated to the choice sets. It follows from the independence of irrelevant alternatives and the lack of variation of participant characteristics that the choice sets can be used as panel units (Train 2009; Greene and Hensher 2010). With the panel structure then a random-effects specification of the logit model can be estimated.

The model specification selected for the analysis is a random-effects logit. A panel structure of the data implies that there is an unobserved effect of participants having to make choices over the 12 choice sets. In a fixed-effects model, the unobserved effect across panel units can be arbitrarily correlated with the explanatory variables, while assuming strict exogeneity of the explanatory variables. If this assumption does not hold, the fixed-effects estimator is biased. Furthermore, if the unobserved effect across panel units is uncorrelated with the explanatory variables, the fixed-effects model is an inefficient estimator. With the assumption of IIA, the unobserved effect across choice sets is left as part of the error term of the model. With this condition, the random-effects model produces a consistent estimator that accounts for the fact that there is a positive correlation between the error terms of the individual choice and the unobserved error across all choice sets. This estimator is also more efficient than the fixed-effects model.

The specification used in the analysis has the option selected as dependent variable (1 if it is the profit-maximizing option, 0 otherwise), conditional on the number of alternatives with consequences (qualitative 0/1 variables for each manipulation condition), participants' numeracy and CRT scores, average time spent perusing the choice set, average pupil dilation across choice sets, and demographic information. The numeracy and CRT scores are interpreted as discrete variables, thus incorporating the assumption that there is a non-linear response to the scores. Treating the scale values as discrete also allows for higher stability to the measure (Svensson 2000). To incorporate the scale values in the estimation, individual qualitative variables (0/1 for each scale value) are included in the model. The estimation imposes the constraint that the sum of the parameter estimates equals one (Bultez and Naert 1975).

Demographics included are a 0/1 qualitative variable for gender (female being 1), the age of participants in years, the reported yearly household income (converted to hourly income assuming a 40-hour workweek), and a 0/1 qualitative outcome variable for having a college education (college education being 1). To help account for heteroskedasticity and the variation between individuals, the model is estimated with robust standard errors.

Sample

Summary statistics of the demographic variables collected are given in table 2. Seven participants did not follow the instructions and are excluded from the analyses. A balanced Mann and Whitney (1947) – MW – test comparing the demographics across treatments found no statistically significant differences ($p > 0.05$) except for gender. The partially incentivized treatments had fewer females than the other two treatment conditions. Although the sample is balanced across the treatments, it is not representative of the general population. The proportions of individuals across demographics do not match those of the U.S. or the local area where the study was conducted. It could be argued that this compromises the findings of the study. Given that the study is testing human behavior differences between the sample and the population do not preclude findings from being informative.

Furthermore, the objectives of the study, do not aspire to provide policy recommendations, which would benefit from a representative sample, but rather the study aims to inform practitioners of ICEs and market valuation. More importantly, random assignment of participants to the treatments ensures causal inferences on the economic behavior of participants across the experimental conditions can be made.

Table 2: Summary of Individual Characteristics in the Sample

	Treatment				All	Local	US
Variable	0%	33%	66%	100%			
Median age	36.0	26.0	26.5	29.0	29.0	27.4	38.1
	(2.32)	(1.60)	(2.51)	(2.77)	(1.18)		
Yearly household income ('000s)	65.91	60.95	67.19	64.35	64.47	45.52	64.35
	(7.01)	(7.27)	(7.97)	(7.01)	(3.63)		
Females (%)	75.00	55.26	57.89	71.88	64.558	49.70	50.80
	(0.07)	(0.08)	(0.08)	(0.08)	(4.00)		
College degree (%)	100.0	97.44	92.11	96.88	96.55	36.10	56.20
	(0.00)	(0.03)	(0.04)	(0.08)	(0.02)		
White (%)	61.11	56.41	71.05	71.88	64.83	56.80	60.60
	(0.08)	(0.08)	(0.07)	(0.08)	(0.04)		
Numeracy score (max 5)	4.00	4.15	3.84	3.31	3.85		
	(0.18)	(0.16)	(0.19)	(0.25)	(0.10)		
CRT score (max 3)	0.94	1.36	1.05	0.83	1.01		
	(0.20)	(0.19)	(0.19)	(0.18)	(0.10)		
Number of participants	36	39	38	32	145		

Standard errors in parentheses. Source: experiment data and U.S. Census (2019)

Results and Discussion

The Effect of Product Availability on Performance

The present study sets out to evaluate whether the number of alternatives with the potential for consequences in an ICE affects the choice behavior of participants in a private goods setting, formally expressed in Hypothesis 1. By comparing the proportion of profit-maximizing choices in each treatment, the effect of the number of alternatives with the potential for consequences on choice is measured. The average proportions of optimal choices for all the 145 participants of the sample in each of the treatments are summarized in table 3. Comparing the proportion of optimal choices in each treatment condition using the MW test shows that the only treatment condition with a statistically smaller percentage of optimal choices is the hypothetical control ($p < 0.05$).

Table 3: Summary of Optimal Choices and Eye-tracking Metrics in Each Condition

	Measure			
Treatment	Optimal Choices (%)	TVD Mean (seconds)	TVD Variance	Pupil dilation (mm)
Hypothetical	60.18[a]*	1.79[a]	3.09[a]*	3.74[a]*
	(4.32)	(0.08)		(0.03)
Partial availability – low	67.09[b]	1.85[a]	2.22[b]	4.17[b]
	(3.03)	(0.07)		(0.03)
Partial availability – high	65.13[b]	1.74[a]	1.88[b]	4.08[bc]*
	(4.10)	(0.06)		(0.03)
Full availability	65.36[b]	1.86[a]	2.91[b]	3.95[c]*
	(4.19)	(0.09)		(0.04)

Statistical tests of every treatment against each treatment individually.
Standard errors in parentheses. The same letters by column indicate values are not statistically different. Significance at the 5% level is denoted by *.

The percentages of optimal choices are not statistically different in any of the other treatment conditions ($p > 0.05$). This result indicates that having any number of choices with consequences in an ICE versus a hypothetical choice experiment improves the performance of participants, supporting Hypothesis 1. This result aligns with the findings of Collins and Vossler (2009), who using an IV framework in a public goods provision experiment found that as long as there are incentives with real consequences, participants improve their performance.

The results do not provide support to Hypothesis 2: increasing the number of alternatives with the potential for consequences does not seem to affect the probability of selecting the profit-maximizing alternative. There are no statistical differences in the percentage of optimal choices between a different number of alternatives with the potential for consequences. A visual representation of the effect of the number of alternatives with the potential for consequences on performance can be seen in Figure 1.

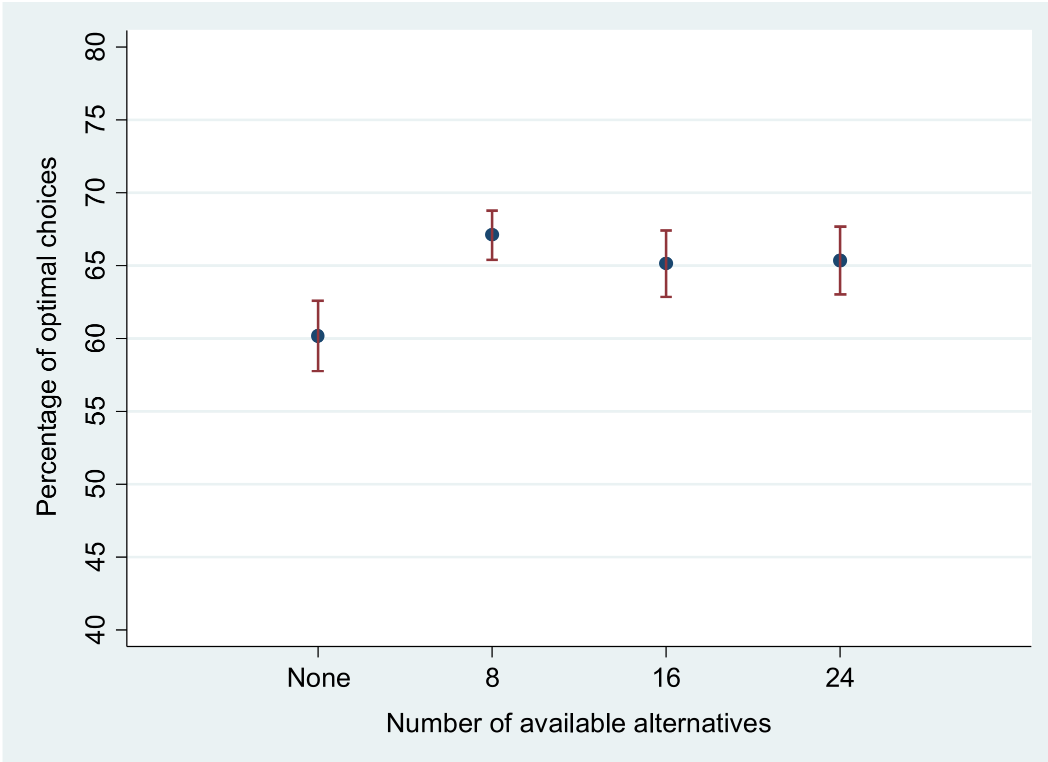


Figure 1. Average and 95% confidence interval of optimal choices made for the different number of available choices**.**

The average percentage of optimal choices with the different number of alternatives with the potential for consequences and the 95% confidence intervals are plotted in Figure 1. This result is in line with the findings of Yang, Toubia, and de Jong (2018), who showed that participants do not respond to the potential enactment of their choices in a monotonic manner.

Behavior and Engagement

To test Hypothesis 3, the visual attention measures collected using an eye-tracker are used. First, the total visit duration (TVD) across treatments is compared. TVD is the measure of the time participants spend looking at the AOIs in each choice set. This eye-tracking measure is strongly advised against when comparing two or more stimuli, as the differences in TVD between stimuli may exist because of inherent differences between stimuli rather than the treatment variables (Orquin and Holmqvist 2018). In this study, the choice sets – the stimuli – are identical across all treatments, thus minimizing the issues of using TVD as a metric. Furthermore, the purpose of using TVD is to measure whether participants are willing to invest more time evaluating the choice sets with a different number of alternatives with potential for consequences.

The mean TVD by treatment is summarized in table 3. The average of TVD by treatment is not statistically different (MW $p > 0.05$). Ratio tests (Brown and Forsythe 1974) on the variances of TVD (table 3) reveal that the variance of TVD in the hypothetical condition is larger than any of the incentivized treatments ($p < 0.05$), while variances of TVD in each of the incentivized treatments are not statistically different between each other ($p > 0.05$). These results suggest that, on average, participants behave similarly in terms of the average time they spend evaluating each choice, whether incentives are present or not. In the hypothetical case, however, search patterns are more erratic and have a larger distribution, which provides some support for Hypothesis 3.

Pupil size is used as an indicator of engagement with the task. A large difference in variation between choice sets would bar the use of an aggregate measure of pupil dilation (Hoeks and Levelt 1993). Tests for variation across choice sets show no significant differences ($p > 0.05$) and the random order of presentation helps account for any inherent variation between the choice sets. An aggregate measure of pupil dilation across choice sets, therefore, is used to compare the treatment effects. The mean pupil dilation in millimeters is shown in the last column in table 3. Differences in pupil dilation, just as the case of TVD, come from differences in the stimuli. Since the choice sets presented to the participants are identical across treatments, the only difference between is the number of choices with the potential for consequences. With equally complex stimuli across treatments, differences in pupil dilation are interpreted as the task deemed more relevant by the participants. Pupil dilation is statistically smaller in the hypothetical treatment condition relative to any of the incentivized treatments ($p < 0.05$). This result serves as a biometric indicator of higher engagement in the incentivized conditions.

Individual Characteristics' Effect on the Reaction to Incentives

Next, the effects of variations on numerical ability and cognitive reflection on decision-making are examined (Hypotheses 4a and 4b). The distribution of the scores in the numeracy scale is skewed to the right in every treatment. The median and proportions of participants above and below the median, however, are not statistically different between treatments. This allows for a comparison of the effect of numeracy across treatments, given the sample balance. The percentages of optimal choices for each value of the numeracy score are plotted in Figure 2. The increasing trend suggests a positive relationship between numerical ability and optimal choices, thus supporting Hypothesis 4a.

A closer look at the relationship between numeracy and profit-maximizing choices is provided by looking at the percentage of optimal choices that each treatment group had across the numeracy scale (table 4). Across all incentivized treatments, increased numeracy scores generally lead to better performance, although the relationship does not seem to be increasingly monotonic.

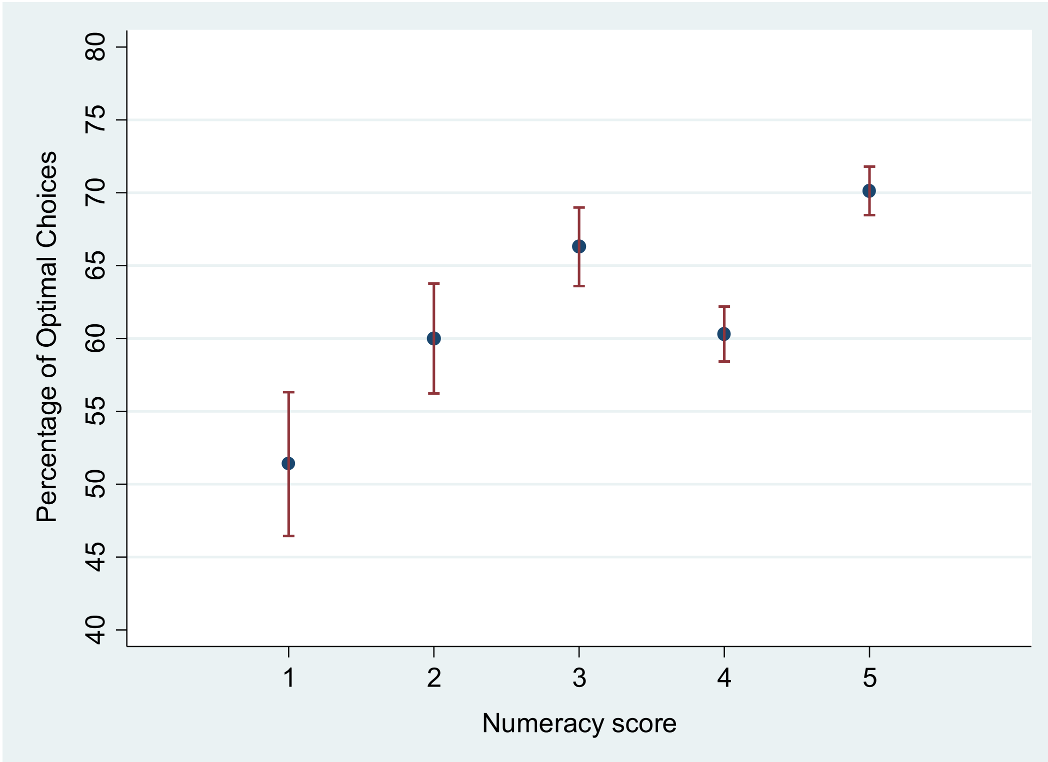


Figure 2. Percentage of optimal choices and a 95% confidence interval by numeracy score across all treatments.

The distribution of CRT scores across treatments is skewed towards the left, with over half of the participants scoring zero. Once again, tests of medians and proportions across treatments find no differences between treatments, allowing a comparison of the effect of CRT across treatments. The proportion of optimal choices and the 95% confidence intervals by CRT scores are plotted in Figure 3. A significant difference exists in the optimal choice percentage between participants who score zero and those that scored above zero. The percentages of optimal choice between participants who score between 1 and 3, however, are not statistically different.

Table 4. Summary of Optimal Choices in Each Treatment for Each Value of the Numeracy and Cognitive Reflection Scales

	Optimal Choice (%)									
	Numeracy score						Cognitive reflection score			
Treatment	0	1	2	3	4	5	0	1	2	3
Hypothetical	n/a	41.66	77.79	66.67	52.08	59.52	57.02	55.00	51.67	75.00
		(0.01)	(0.10)	(0.14)	(0.07)	(0.07)	(0.06)	(0.15)	(0.07)	(0.12)
Partial availability – low	n/a	25.00	25.00	71.67	69.87	70.83	60.42	70.83	70.14	69.05
		(0.01)	(0.08)	(0.05)	(0.04)	(0.04)	(0.07)	(0.06)	(0.05)	(0.06)
Partial availability – high	n/a	66.67	52.78	60.71	60.90	74.36	56.14	75.00	71.97	78.33
		(0.17)	(0.20)	(0.10)	(0.08)	(0.06)	(0.06)	(0.10)	(0.07)	(0.07)
Full availability	0.25	54.17	65.48	68.06	56.25	80.21	58.71	83.33	80.56	75.00
	(0.00)	(0.21)	(0.08)	(0.11)	(0.08)	(0.06)	(0.05)	(0.16)	(0.09)	(0.01)

Standard errors in parentheses. No participants are in the zero scores in the numeracy scale in the hypothetical or partially incentivized treatments.

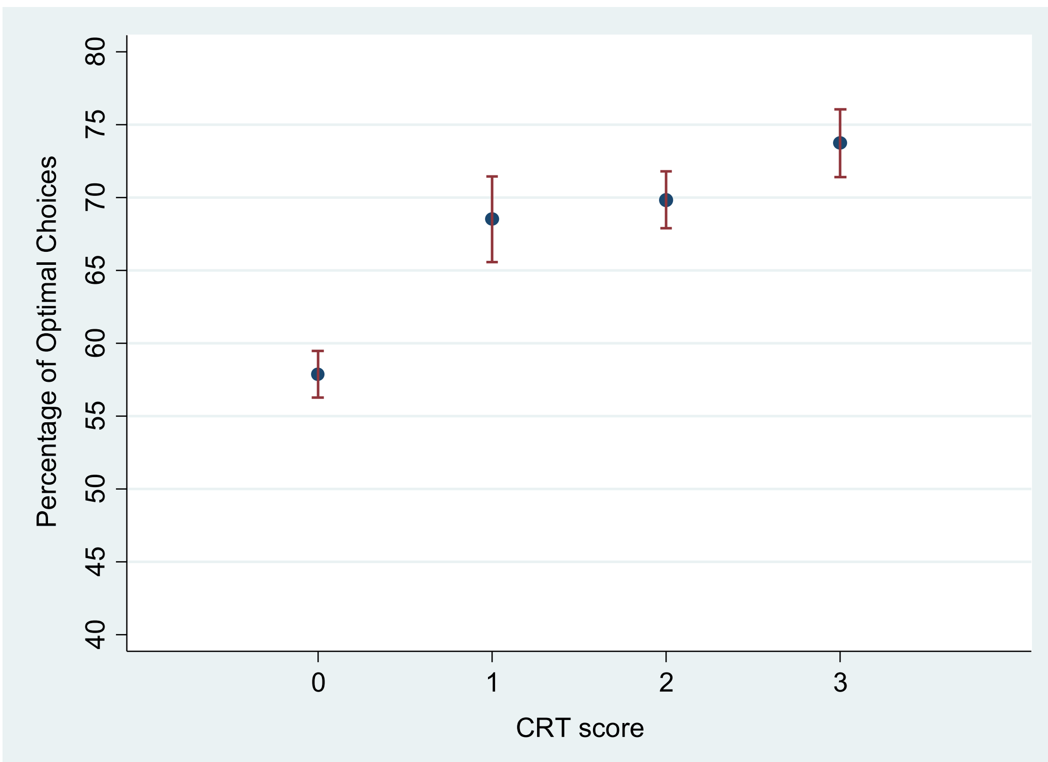


Figure 3. Percentage of optimal choices and a 95% confidence interval by CRT score across all treatments.

Evaluating the percentage of optimal choices for each treatment group by CRT scale scores (table 4) reveal patterns that support Hypothesis 4b. Participants in the incentivized treatments with CRT scores greater than zero (the last three columns in table 4) perform better than those with zero scores. However, participants in the hypothetical treatment only perform well when their CRT score is at its maximum. Participants in the hypothetical condition with the largest CRT score generally perform as well as participants in the incentivized conditions.

The implication is that participants who are engaged – given higher scores in CRT signal higher engagement in analytical processing (Toplak, West, and Stanovich 2011) – may not need incentives to perform well, while unengaged participants – lowest CRT score – may not react to incentives, regardless of the number of available alternatives.

Econometric Evaluation of the Interaction Between Treatments and Behavior

To test for the effects of treatment conditions and behavioral aspects simultaneously, three different specifications of a random-effects logit model are estimated (table 5) using the *xtlogit* command in Stata (StataCorp 2015). The first model estimates the changes in the likelihood of choosing the profit-maximizing option with different numbers of alternatives with the potential for consequences, relative to the hypothetical condition of using only qualitative indicator variables for the treatments. A second model includes the results of the numeracy and CRT scales. Finally, a third specification includes pupil dilation, TVD, age, gender, hourly income, and college education as controls. Correlations between the variables do not suggest multicollinearity problems in the data. Loss criteria suggest model 3 provides the best fit.

Table 5. Random-Effects Panel Logit Estimation of the Likelihood of Making the Optimal Choice Results

	Model					
Variable	1		2		3	
Partial availability –low	0.331**	(0.144)	0.317**	(0.139)	0.189	(0.151)
Partial availability – high	0.228*	(0.132)	0.255	(0.164)	0.247	(0.178)
Full availability	0.270**	(0.116)	0.345**	(0.152)	0.269	(0.151)
Numeracy score						
1			0.018	(0.186)	0.369***	(0.150)
2			0.238	(0.150)	0.194	(0.157)
3			0.354***	(0.109)	0.325***	(0.123)
4			0.054	(0.112)	-0.068	(0.085)
5			0.336***	(0.089)	0.181***	(0.099)
CRT score						
1			0.290***	(0.085)	0.317***	(0.106)
2			0.223**	(0.126)	0.312**	(0.124)
3			0.487***	(0.134)	0.381***	(0.089)
TVD					0.206***	(0.072)
Pupil dilation					0.110	(0.081)
Demographics						
Age					-0.015***	(0.005)
Female					0.124	(0.145)
Hourly income					-0.006**	(0.003)
College Education					0.241	(0.217)
Constant	0.051	(0.097)	-0.339	(0.089)	-0.628	(0.668)
N	1572		1572		1572	
Log-likelihood	-1057.31		-1043.48		-1016.41	
AIC	2122.64		2110.95		2054.83	
BIC	2144.08		2175.27		2113.79	

Cluster robust standard errors in parentheses. Significance at 10%, 5%, and 1% is denoted by *, **, and ***. AIC is the Akaike information criterion and BIC is the Bayesian information criterion.

Because the estimation of the parameters for the different variables required dropping incomplete observations, observations without complete demographic information, eye-tracking data, or scale values are not included in the estimation. This leaves 1572 observations[1]. As it generally is the case in econometric estimations, there are potential problems associated with the choice of model specification used. As a robustness check, other specifications were estimated, including fixed-effects models with standard errors clustered at the choice level, and linear probability models. Each specification varies in the assumptions they make and whether such assumptions may or may not be valid or relevant. A similar pattern in the direction and magnitude of the estimated coefficients is seen across all models. To illustrate the similarity of results, the linear probability model estimation results are presented in Appendix D. The main differences found between the results of the linear probability models and the random-effect specifications are in the parameter estimates for the number of alternatives with the potential for consequences.

The parameter estimates for the different numbers of alternatives with the potential for consequences are not statistically different between each other ($p > 0.05$) in either of the specifications, but in the linear probability models all incentivized conditions are statistically significant when accounting for numeracy and attention (model 2), versus in the random-effects model the partial availability – high is not statistically significant and the full availability condition is statistically different from zero when also accounting for demographics (model 3), while in the random-effects model none of the incentivized conditions is.

[1] The estimation of models 1 and 2 without dropping the observations with missing data yields the same significance and direction in the results. The comparative statistics were also calculated with the subsample and the same patterns and significance hold.

The results of model 1 indicate that partial availability – low and the full availability treatments increase the likelihood of choosing the profit-maximizing option relative to the hypothetical treatment. In model 2 the highest level of numerical ability and all non-zero degrees of engagement (CRT) have significant positive effects. In model 2, partial availability – low and full availability treatments still have a statistically significant effect improving the likelihood of profit-maximizing choices relative to the hypothetical control. The parameter estimates for the different treatments, however, are not statistically different between each other ($p > 0.05$). The results of model 2 also show that numerical ability may or may not increase the profit-maximizing choice chance, while any non-zero degree of engagement (measured with CRT) has a significant positive effect in increasing the probability of participants making profit-maximizing choices.

Inferences about treatment effects differ when controlling for demographics and visual attention in model 3. Numeracy is once again significant at some levels but not others. Meanwhile, all levels of engagement are statistically significant and positive when demographic controls included in the estimation. Visual attention captured by TVD has a positive and significant effect, implying that participants that spent more time perusing the choice sets were more likely to make optimal choices. The estimate for pupil dilation, which was statistically different between hypothetical and incentivized conditions in the statistical inference analysis, is not significant. This result indicates that while engagement may be driving optimal choice behavior by participants, the physiological measure does not show any differences. Hourly income is statistically significant and negative. The direction of the parameter is reasonable, as participants with larger opportunity costs would likely not exert as much effort in the task for the payouts involved. The main finding in model 3 is that when adding visual metrics and demographics to the model, none of the incentivized treatments is statistically different from the hypothetical control in increasing the likelihood of making optimal choices.

To explore if there are systematic biases in the participants' decision-making, willingness to pay (WTP) for each of the attributes is estimated. To estimate WTP, a random parameter logit model in WTP space (Train and Weeks 2005) is estimated, with the color, shape, and price of each choice as independent variables, the actual choice as the dependent variable, and the panel unit being the choice set. In models in WTP space, the utility function is reparametrized such that each attribute coefficient estimate is interpreted directly as the WTP for the attribute.

The model uses green and square as the baselines for color and shape. With this setup, the WTP for each attribute represents how much more the participants would be willing to pay relative to the baseline. For example, for the color blue, the WTP relative to the baseline should be \$0.50, because as noted earlier the value of the color green is \$0.50, and the value for the color blue is \$1.00. The WTP estimates are compared to the actual value of the attributes using a χ^2 test (table 6). Test results indicate the WTP estimates are not statistically different from the actual values of the attributes, except for the WTP estimate for triangle under partial availability – high. These results suggest no systemic biases driving the differences in choices made by the participants.

Table 6. WTP for the Different Attributes in Each Treatment Condition Using a Random Parameters Panel Logit Estimation in WTP Space

	Treatment			
Attribute	Hypothetical	Partial – Low	Partial – High	Full
Circle ($1.00)	$1.33	$1.08	$1.55	$0.94
Triangle ($0.50)	$1.25	$0.42	$1.33*	$0.65
Blue ($0.50)	$0.10	$0.28	$0.62	$0.40

The * denotes significance at the 5% level of the difference between the value and the WTP estimate

The econometric estimation results (table 5) are not entirely consistent with the statistical results discussed earlier. Comparative statistical inference results suggested that the average of profit-maximizing choices is significantly different between the hypothetical control and the incentivized treatments but are not statistically different between incentivized treatments. Differences between comparative statistics and econometric results beg the question as to why. Payout differences in each choice set may help explain the results. Prior research using the IV framework suggests that decision errors are related to payout differences (Taylor et al. 2001; Vossler and McKee 2006). As described in the methods section, the differences in payoffs between alternatives in each choice set are either $0.50 (7 out of 12) or more than $0.50 (5 out of 12). Given that all participants were presented with the same choice sets, the variation in the payoff is the same across participants[2]. The variance coming from differences in payouts might be obscuring the effect of the number of alternatives with the potential for consequences. To examine if

[2] Because differences in payoffs are collinear with the choice sets, a model including an estimator for the effect of the differences is limited to a logit model and not a panel logit. Estimation results of the logit model including the differences in payoffs have similar coefficients and significance as model 3 in Table 5.

payoff matters, the proportion of optimal choices and model 3 are estimated separately for large payoff differences (more than \$0.50) and small payoff differences (\$0.50).

The difference in profit-maximizing choices between payoff difference groups is statistically significant (MW < 0.05): 63.8% with large payoff differences versus 51.4% with small payoff differences. The results of the econometric estimations indicate that when the payoff differences are small, no treatment increases the likelihood to make optimal choices compared to hypothetical control, just like in the pooled model 3. The engagement measured by the CRT, visual perusal measured by TVD, and participants' age have any significant effects on the likelihood of making optimal choices (table 7).

In contrast, when payoff differences are large, any number of alternatives with the potential for consequences increases the likelihood of participants making optimal choices. The parameter estimates for each treatment are statistically equivalent ($p > 0.05$), implying that all the treatments increase the likelihood of optimal choices equally. Engagement is again significant, which implies that participants who are engaged regardless of the difference in payoffs make more profit-maximizing choices. The numerical ability does not affect optimal decision making with large payoff differences. The negative hourly income effect found in the pooled model 3 is present with large payoff differences.

Table 7. Random-Effects Panel Logit Estimation for the Payoff Difference Levels

	Model			
Variable	Small difference		Large Difference	
Partial availability – low	-0.044	(0.192)	0.537***	(0.132)
Partial availability – high	0.129	(0.270)	0.393***	(0.112)
Full availability	0.077	(0.219)	0.557***	(0.148)
Numeracy score				
1	0.278*	(0.165)	0.250	(0.242)
2	0.235	(0.284)	0.179	(0.175)
3	0.309**	(0.145)	0.430***	(0.161)
4	0.059	(0.164)	-0.186	(0.114)
5	0.119	(0.121)	0.326**	(0.147)
CRT score				
1	0.338**	(0.149)	0.304**	(0.153)
2	0.266	(0.188)	0.264	(0.225)
3	0.396***	(0.092)	0.432***	(0.182)
TVD	0.091	(0.069)	0.049	(0.088)
Pupil dilation	0.054	(0.112)	0.173	(0.123)
Demographics				
Age	-0.018**	(0.008)	-0.012*	(0.006)
Female	0.097	(0.099)	0.243	(0.433)
Hourly income	-0.005	(0.005)	-0.009**	(0.003)
College Education	0.518	(0.473)	-0.312	(0.213)
Constant	-0.479	(1.047)	-0.007	(0.713)
N	917		655	
Log-likelihood	-602.73		-399.95	
AIC	1241.46		835.90	
BIC	1328.24		916.49	

Clustered robust standard errors in parentheses. Significance at 10%, 5%, and 1% is denoted by *, **, and ***.

Econometric analyses (tables 5 and 7) show that the numerical ability of participants has some effect on optimal choice-making by participants. Meanwhile, significant parameter estimates for CRT throughout support the idea that engagement is crucial for game recognition and task performance, in the form of optimal choice-making.

The differences between the two approaches highlight the importance of participants' engagement, captured by the CRT.   Overall, the results suggest that researchers using choice experiments for market valuation do not need to set up their studies such that all choices potentially have consequences to get incentive-compatible answers, as long as the differences in potential profits are large. A study where only some alternatives may have consequences will work just as well as a study where all alternatives can have consequences as long as participants are engaged and/or motivated to exert the effort necessary by other means. In contrast, participants in a study using ICE where all alternatives can have consequences may perform similar to a hypothetical choice experiment if they lack the engagement and/or desire to exert the effort.

Conclusions

Given the potential for biases when asking participants hypothetical questions, researchers have reacted by using incentivized choice experiments (ICE) for the valuation of public and private goods. In ICE, one or more of the choices made by participants has consequences for the participant. Much of the literature in choice experiments implies that incentivizing is an all-or-nothing choice – either all the choices used in the ICE have the potential for consequences or no choice is incentivized. The results presented in this study suggest that this practice may not be necessary for participants to reveal their actual preferences under some circumstances.

Comparing the hypothetical baseline to any of the incentivized treatments, where all or some of the alternatives may have consequences, results show, not surprisingly, that better performance is realized when incentives are present. When estimating this effect including different controls, the presence of incentives seems to lose its ability to produce better performance, while engagement measured with the cognitive reflection test (CRT) becomes an important explanatory variable of profit-maximizing behavior. This result suggests that engagement provides a more accurate prediction of profit-maximizing behavior than the presence of incentives.

The evaluation of how individual heterogeneity in terms of numerical ability and engagement impact profit-maximizing behavior provides some useful practical implications. Using a numeracy scale to gauge numerical ability, it is discovered that numerical skills have a significant effect on performance, but this effect is far from monotonically increasing. In terms of engagement, the results indicate that higher engagement leads to better performance. This relationship between optimal choices and engagement is explored further by looking at the differences in payoffs. To evaluate the effect of payoffs on engagement, the model is estimated separately for choices where the differences in payoffs are large versus small. These estimations suggest that large payoff differences motivate participants to exert effort and engage in the task, resulting unequivocally in better performance, even when only a portion of the alternatives may have consequences. This result highlights the importance of increasing the relevance of the task for participants, a topic that demands more research in the field. When payoffs are large or participants are engaged by other means, any number of alternatives with the potential for consequences has a positive effect on the likelihood of making optimal choices.

Different opportunities for future research stemming from this study come from the study's limitations. First, it could be argued that the sample used for the study is not representative of the population, but such is not necessary to achieve the study's objective.

A more meaningful limitation is the methodology used. Future research could extend this by testing the methodology described here with actual private goods similar to Muller, Lacroix, and Ruffieux (2019). The related literature cited does not tend to find the same biases in induced values framework as it does with actual goods. There are several questions not addressed by this study that using actual private goods would help answer, such as the perceived availability of the products by participants. Using real products with different number of available alternatives would inform researchers of market valuation for new product and product features. Another limitation is the restricted number of factors affecting the performance of participants explored in this study. Future research could consider more potential aspects of individual heterogeneity beyond numeracy, engagement, and demographics. Some potential candidates include motivations, prosocial behavior, demand effects, and different dimensions of cognition. Future research could also go deeper into the interactions between participants' attributes. The numeracy of participants, for example, is shown to affect performance. More research into this particular result could shed some light on what the relationship with performance is and the process through which numeracy affects performance.

Results also indicate that higher engagement leads to better performance regardless of the number of alternatives with the potential for consequences, a result that highlights the importance of how relevant the task is for participants. How to increase engagement of participants and the relevance of the task would be a fruitful avenue for future research. Finally, the study's bonus assignment may not be salient enough to incentivize respondents to choose the profit-maximizing choice. This is a challenge to ICE in general that future research can address.

CHAPTER III

ON THE USE OF NUDGES: HOW DEFAULT OPTIONS AND SELF-CONTROL AFFECT HONESTY

A market is the intersection of sellers and buyers with the intent to cooperate and transact with each other. In almost every market, if not all, one party possesses more information about the transaction than the other party does, violating one of the assumptions of perfect competition. This situation where one party has more information than the other gives rise to a market with information asymmetry (Mishra, Heide, and Cort 1998). Markets where buyers withhold information that sellers do not possess (e.g. insurance, labor markets), or where sellers have information buyers do not possess (e.g. real estate, used cars) represent a large segment of modern economies. Information asymmetry may lead to a positive outcome in some instances, such as the division of labor (Becker 1985). Nevertheless, although asymmetric information is a natural outcome in markets (Leland and Pyle 1977), market asymmetry may result in inefficient markets. Akerlof (1970) shows information asymmetry can lead to a market ceasing to exist, reducing the welfare of society.

Two aspects of information asymmetry that have been a focus of many studies are moral hazard and adverse selection (Hölmstrom 1979; Igawa and Kanatas 1990; Chiappori and Salanie 2000). Moral hazard occurs when an agent possesses information that would benefit the other party, but the agent does not share or misrepresents the information (Pauly 1968). This generally occurs when the agent believes there is a small probability of being caught (Kübler, Müller, and Normann 2008). When moral hazard is present, parties transact on the limited information available, and a suboptimal outcome is often reached (Arrow 1978). Adverse selection occurs when the ability to withhold information encourages agents who benefit from the asymmetry to participate more actively in a market (Spence 1973). The classic textbook example of adverse selection in the health insurance market. In the insurance market, adverse selection occurs when people that have a larger probability than the average of using insurance (preexisting conditions, risky lifestyle, etc.) are the people who buy insurance (join the market) while concealing the truth about their activities to the insurance companies (second parties). Adverse selection may lead to suboptimal outcomes, such as incorrect pricing of insurance (Hallagan 1978).

Markets with products possessing credence attributes lend themselves to information asymmetry and potential subsequent problems. Credence attributes are properties of goods and services that cannot be easily verified by buyers (Darby and Karni 1973; Emons 1997). The uncertainty around the truth of credence claims is large. Customers generally do not know how to identify these value-adding features until after the purchase (if at all), with vendors being the only ones who know the truth about these claims. This is potentially problematic as there is a monetary incentive to deceive consumers for profit. The price differential for organic products compared to the non-organic counterpart, for example, ranges from 22% to 82% more for organic products (USDA-ERS 2019). Meanwhile, there are claims of as much as 20% of products being mislabeled as organic, despite the regulations in place (Giannakas 2002). The situation is even worse for other credence attribute labels such as “natural,” which unlike organic is not regulated (Berry, Burton, and Howlett 2017).

In asymmetric markets, the trustworthiness of the exchange is determined by signals (Rothschild and Stiglitz 1978). It is sensible to question whether the willingness to accept the word of the sellers about their signals as true – the trusting of buyers – and the willingness to reveal true information in the signals by sellers – honesty of the sellers – come to play in interpreting signals. The trusting of buyers and honesty of sellers may be a serious problem given the growing popularity of credence attributes in markets and an increasing number of transactions happening in such settings. Important research questions are: what conditions make sellers lie and what conditions make buyers more trusting? In particular, the objective of this study is to find out how seemingly innocuous manipulations can affect honesty and trust in a market with asymmetric information.

To answer these questions, the present study uses an experiment where participants are assigned roles of sellers and buyers in an artificial market with information asymmetry and explore what conditions increase (decrease) lying behavior by sellers and trusting behavior by buyers. The focus of the study is on behavioral nudges that affect honesty and trust. One manipulation varies the default action for sellers between a lie or the truth for sellers to evaluate if lying is different between treatments. A second manipulation is self-control depletion. By exposing participants – buyers and sellers alike – to a self-control depletion task, changes in lying by sellers and trusting by buyers are measured. Finally, the effect of the likelihood of being caught on honesty and trust is explored. The morality, engagement, and numerical ability of participants are also measured, along with demographic controls to gauge the effect of such characteristics on participant behavior. Comparative statistics and econometric analyses are used to explain the effects of the variables and treatments in the behavior observed.

Literature Review and Hypothesis Formulation

Information Asymmetry in Markets

Markets with information asymmetry are popular across the US. Some food consumers, for example, base their produce purchases on the availability of products with credence attributes such as GMO-free, locally-grown, and organic (Feagan and Morris 2009; Moser, Raffaelli, and Thilmany-McFadden 2011). This preference for credence attributes is potentially problematic. The accuracy of attribute claims is difficult and costly to verify by customers (Darby and Karni 1973). Verification difficulty is especially true before a purchase is made. Whether or not the products being sold possess the attributes declared is information only known by the sellers. The buyers' uncertainty about quality puts them at risk of being lied to (Bergen, Dutta, and Walker 1992). Verification difficulties and uncertainty are the makings of a classic asymmetric market (Akerlof 1970).

One type of lying that may arise in asymmetric markets is pricing low-quality items at high-quality prices. Rao and Bergen (1992) argue that quality-conscious customers use price premiums as a way to guard themselves against sellers who would deliver lower quality than promised. The way consumers use price to obtain accurate information on a product has a strong influence on a firm's pricing decisions (Devinney 1988). With information asymmetry, however, a moral hazard potentially exits. Sellers charging price premiums for products are perceived as having higher quality even if the quality is not present. Canonical pricing models predict the existence of such moral hazard (Varian 1980; Tellis and Wernerfelt 1987). These pricing models usually assume customers are not willing to exert the search costs to get accurate information on quality attributes and prices (Varian 1980; Tellis and Wernerfelt 1987). Customers in markets with information asymmetry are demanding more information, which has led to an increase in traceability, but this trend in traceability has not reached all markets (Hobbs 2004; Verbeke 2005).

A deterrent to moral hazard is the loss of repeat purchases. Liars are punished by loss of future sales. This potential loss of future revenue promotes honest behavior in markets even in the absence of contractual obligations (Kreps 1990). Some markets are operated by owners with little to no contractual obligation to buyers (Low et al. 2015). Furthermore, research has shown that in some markets buyers who discover they were lied to did not intend to stop buying from those venues (Gao, Swisher, and Zhao 2012). In the absence of potential future revenue loss, there is a small deterrent to dishonest behavior (Grover and Goldberg 2010; Karp 2010). This study explores if in artificial markets with information asymmetry and with no potential for loss of future revenue would sellers take advantage of their additional information to maximize profits.

Lying: A Means to an End

Research on honesty and lying by Gneezy (2005) highlights that participants in economic research studies will lie for self-gain even at the expense of other participants. Experiments conducted by Fischbacher and Föllmi-Heusi (2013) find participants are dishonest when they have incentives and leeway to lie. Participants in their study could lie to maximize their payoff. Most participants chose to lie to obtain increased profits, but not to the largest level of potential profits. A similar result is found in the field experiment in fish markets conducted by Dugar and Bhattacharya (2017). They find sellers lie but never more than 10% of the purchased quantity.

An economic study about lying cannot focus only on the potential gain without exploring if the probability of being caught mitigates (or fosters) dishonest behavior

(Bryant and Eckard 1991). Saha and Poole (2000) show that penalties as a function of monitoring costs play a role in lying. In asymmetric markets, the probability of being caught may be slim but non-zero. It is an interesting empirical and theoretical question of whether a small, non-zero probability of being caught can influence dishonesty in asymmetric markets, giving rise to the following hypothesis:

Hypothesis 1*: A non-zero probability of being caught decreases dishonest behavior in sellers with more information than buyers in asymmetric markets.*

Another interesting research stream that could be used to investigate dishonest behavior in asymmetric information markets is the use of behavioral nudges, in the spirit of Thaler and Sunstein (2008). Nudges that modify behavior can take the form of default options, such that the default option can be opting in versus opting out (Johnson and Goldstein 2003). Concerning honest behavior, it remains an unanswered question in the literature on markets with information asymmetry whether default options would make people more or less likely to lie. If the default option is truthful, does that make people less likely to lie? The literature on status quo biases (Samuelson and Zeckhauser 1988) suggests that this would be the case.

Because of the bias that the default or status quo option invokes, changing the default option to be the truth may mitigate dishonest behavior. An example of how participants in economic experiments can be dishonest when no status quo is promoted with the absence of a default option is found in Gneezy (2005). Senders in a sender-receiver game have to send a message about the distribution of a pool of money to the receivers and are allowed to lie to maximize their payoff. Gneezy (2005) finds that participants lie more when the potential payout differences were larger. This result supports the idea that people lie when no default option stops them from lying. The contrast is when the default option leads to a status quo bias in favor of honesty, such as the work of Azar, Yosef, and Bar-Eli (2013). Participants in their study receive more change than they should when paying cash at a restaurant. Participants are dishonest if they keep the extra cash (assuming they pay attention), which would be the default option. Azar, Yosef, and Bar-Eli (2013) find that participants are likely to return the extra cash if the change amount is large but otherwise participants keep it. This result shows that the status quo bias that the default option invokes could potentially be influential on honesty behavior in markets with information asymmetry.

The literature on moral licensing would suggest that at the very least there would be an increase in lying with the default option being a lie (Blanken, van de Ven, and Zeelenberg 2015). When the setup of a choice is such that a decision-maker can excuse their immoral or deceitful behavior by making it acceptable to do a bad deed, individuals are more likely to engage in such behavior (Merritt, Effron, and Monin 2010). The capacity of a default option working as a nudge and affecting dishonesty through a status quo bias is formalized in the following hypothesis:

Hypothesis 2: *Having the default option being the truth increases the honest behavior of sellers.*

A broad definition of self-control is the capacity to alter one's behavior (Baumeister and Vohs 2007). The ability to exert self-control is crucial in daily life. Society depends on individuals exerting self-control to function adequately. Self-control, interestingly, can be considered as a depletable resource (Muraven and Baumeister 2000) which when depleted is not only costly to individuals but also society. Examples of depleted self-control that are costly to society include large debt (Gathergood and Weber 2014), overspending (Shah, Mullainathan, and Shafir 2012), reduced productivity (Bucciol, Houser, and Piovesan 2011), and dishonesty (Baumeister and Vohs 2007). Being honest is an action that requires self-control that has been studied in economics because of the large social costs of dishonesty (Sutter 2009; Wang, Rao, and Houser 2017). From a utility standpoint, being dishonest may be a rational act when there are no consequences to lying (Hao and Houser 2017). Rational action – the expectation of economic theory – is that people will lie as long as they have incentives to do so and there is a small likelihood of being caught (Conrads et al. 2013; Chen and Houser 2017). Honesty requires effort and as a consequence when self-control is depleted lying is more prevalent than when self-control has not been reduced (Mead et al. 2009). The moderating effect of self-control on honesty is illustrated by Mazar, Amir, and Ariely (2008) who show that attention is impaired when self-control has been depleted. If there is a lack or reduction of self-control, attention to social norms may be reduced, reducing honesty. The issues that self-control depletion could bring to a market with information asymmetry are stated in the next hypotheses:

Hypothesis 3a: *Self-control depletion in sellers increases their dishonesty.*

Hypothesis 3b: *Self-control depletion in buyers decreases their trust.*

Individual Factors Affecting Honesty

Efficient economic exchanges require agents to be able to correctly identify their respective payoffs to maximize their utilities and assign values to the information in the market (Hirshleifer 1971). The cognitive ability of participants, therefore, is one trait that impacts agents' decisions in economic exchanges (Cokely and Kelley 2009; Alós-Ferrer et al. 2012; Agarwal and Mazumder 2013). One area of cognitive ability that would affect decision making in asymmetric markets is numeracy (Kløjgaard, Bech, and Søgaard 2012). Numeracy, defined as "…the ability to process basic probability and numerical concepts." (Peters et al. 2006, p. 407), enables sellers to make the connections between their actions – honest or dishonest – and their payoffs. Numeracy (or lack thereof) makes buyers mindful about the transaction and less trusting of the sellers' claims. The way numeracy could influence the interactions in a market with information asymmetry is formalized in the following hypotheses:

Hypothesis 4a: *Increased numerical ability decreases the honesty of sellers.*

Hypothesis 4b: *Increased numerical ability decreases the trust of buyers.*

Honest behavior can be the result of a social norm (Lindbeck 1997; Fehr and Falk 2002; Pruckner and Sausgruber 2013). To follow social norms, engagement with moral standards and how they relate to the task at hand is necessary. If the engagement of sellers is compromised, compliance with the social norm is less likely (Brekke, Kverndokk, and Nyborg 2003; Rege and Telle 2004). Besides, buyers who are engaged are less likely to be deceived, while less engaged buyers are more likely to be deceived (Pittarello et al. 2016). The influence on market exchanges with asymmetric information of engagement is formalized with the following hypotheses:

Hypothesis 5a: *Increased engagement of sellers increases their honesty.*

Hypothesis 5b: *Increased engagement in buyers reduces their trust.*

Morality

Lying is at its essence an ethical issue (Williams 2012). Individuals and firms who engage in lying are willing to transgress ethical boundaries. Sellers' traits may influence if they lie (Ogilby 1995; Hoffman, McCabe, and Smith 1996; Bohnet and Frey 1999), in a similar manner to buyers' traits influencing their level of trust (Doney and Cannon 1997; Doney, Cannon, and Mullen 1998; Sirdeshmukh, Singh, and Sabol 2002). Dishonest behavior and trusting in the honesty of others are dependent on the morality of individuals (Haidt 2008). A higher degree of morality serves as a deterrent to lying and encourages trust. The way these actions would influence behavior in asymmetric markets is formalized as:

Hypothesis 6a: *A higher degree of morality in sellers increases their honesty.*

Hypothesis 6b: *A higher degree of morality in buyers increases their trusting.*

Methodology

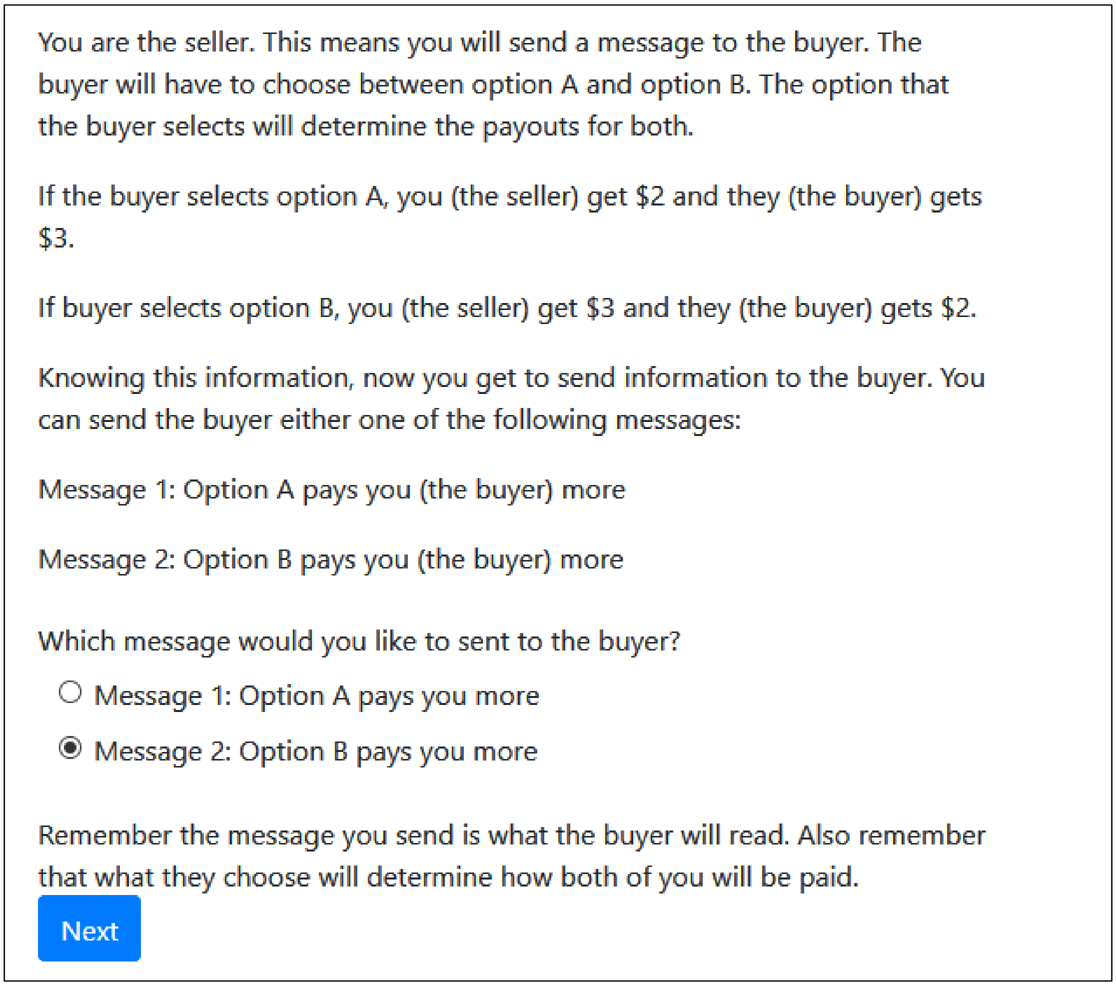
This study uses a seller / buyer game conducted between participants in a 2 x 2 x 2 design with a nudge for honesty (default lie vs. default truth), a self-control depletion (no depletion vs. depletion task), and the probability of being caught (0% vs 10%) as factors.

Seller-Buyer Game

The seller / buyer game is a modified version of Gneezy's (2005) sender / receiver game with two different payout options. The study is conducted on computers using a web-application developed with the Python programming language in the Otree platform (Chen, Schonger, and Wickens 2016). All public information is on the computer screens throughout the experiment as a reference for the participants. The game is set up as a single-shot game to exclude the possibility of repeated purchases and to avoid strategic behavior by participants. To guarantee anonymity, participants are assigned identification numbers uncorrelated to their personally identifiable information. Since the objective of the study is to investigate the interactions in markets with information asymmetry, the roles assigned to participants are named buyer and seller to prime participants with the notion that they are participating in a market. Using the terms sender and receiver would have not been as helpful in priming participants.

Before the game begins, participants are randomly assigned the role of seller or buyers and are then paired randomly to play their assigned roles of seller and buyer. Participants are informed that a monetary payment of $5 will be distributed between the members of each pair. Instructions to the participants explain there are two ways the $5 prize will be distributed, Options A and B.

At this point, it is made clear to all participants that the buyers will choose between the two options and that the buyer's decision is used for the distribution of the payment to the participants. To generate information asymmetry in this market sellers have information that buyers do not. Buyers do not know the distribution of the prize but obtain information from the sellers about the distribution. Only sellers are disclosed how Options A and B distribute the $5 prize. Sellers are provided the information that Option A pays $2 to the seller and $3 to the buyer, whereas option B payments are $3 for the seller and $2 for the buyer. Sellers are required to send a message to influence the buyer's choice – a one-line sales pitch so to speak. The payout distributions remain on the seller's computer screen while they decide on what message to send to the buyers. The message options that the seller can send are displayed in a box (Figure 4).



You are the seller. This means you will send a message to the buyer. The buyer will have to choose between option A and option B. The option that the buyer selects will determine the payouts for both.

If the buyer selects option A, you (the seller) get $2 and they (the buyer) gets $3.

If buyer selects option B, you (the seller) get $3 and they (the buyer) gets $2.

Knowing this information, now you get to send information to the buyer. You can send the buyer either one of the following messages:

Message 1: Option A pays you (the buyer) more

Message 2: Option B pays you (the buyer) more

Which message would you like to sent to the buyer?

- ○ Message 1: Option A pays you more
- ◉ Message 2: Option B pays you more

Remember the message you send is what the buyer will read. Also remember that what they choose will determine how both of you will be paid.

Next

Figure 4. Message sending box viewed by the sellers.

Default Option: Lie or Truth

The first factor explored in this study is the effect of the status quo brought by the default option. The intent is to evaluate if there is any effect on honest behavior when the default is honesty. This treatment varies the effort to express honesty in a very subtle way: changing the default message sellers can choose to send. For the honesty default manipulation, the radio button is defaulted to message 1 – Option A pays you (the buyer) more – the truthful message.

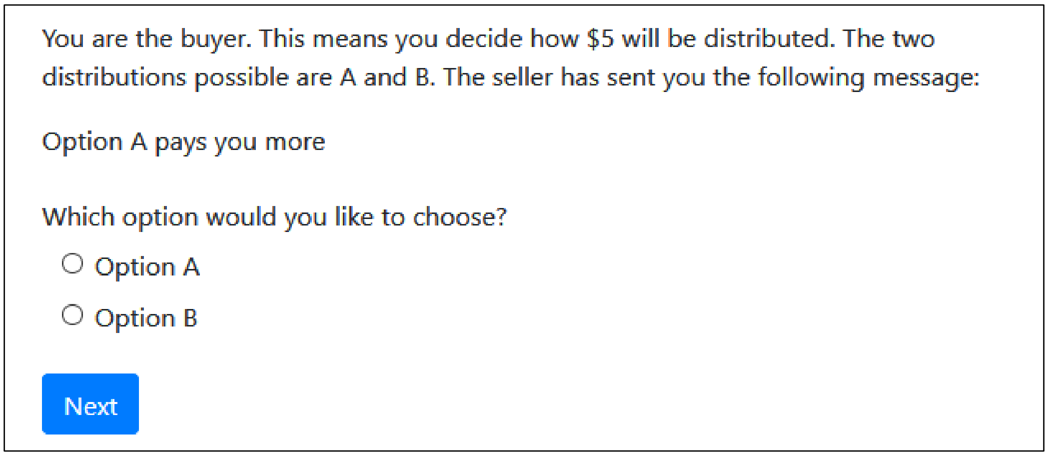


Figure 5. Options selection box viewed by the buyers.

Sellers have to change the radio button to message 2 – Option B pays you more – if they want to be dishonest. In the dishonesty status quo manipulation, the radio button defaults at message 2, the lie. Sellers have to change the radio button to message 1, to be honest. In either manipulation, the seller must click on the "next" button to send the message to the buyer. Participants have to exert effort, albeit very small, either to lie or to be truthful in either condition. To maximize their payment, sellers have the incentive to send message 2. This manipulation allows for the identification of differences in honest behavior by default option by comparing how often message 1 is sent.

While sellers are making their choice of message to send, buyers are watching a waiting screen with a timer. Once the seller has sent the message, buyers see a box (Figure 5) that tells them the recommendation of the seller and asks them which option they would like to choose for the distribution of the cash payout. Once buyers make the distribution option selection, a screen with open-ended questions that are discussed later is presented to participants, followed by a screen with the payout distribution and their respective payouts.

Self-Control Depletion Task

The second factor evaluated is the effect of self-control depletion. It has been documented that participants lie for self-gain when self-control is depleted (Mead et al. 2009). The two questions explored in this study on this regard are whether there is an effect of self-control depletion on lying by the sellers and if self-control depletion affects the likelihood of buyers to follow sellers' advice, none of which have been explored in the literature.

To deplete self-control resources, a task similar to the one developed by Houser et al. (2018) is presented to half of the participants. Before the seller / buyer game, participants (both buyers and sellers) in the self-control depletion manipulation are shown 30 series of nine digits (ones and zeroes) and are asked to count the number of times the number one appears in the series and enter the number in the space provided on the screen (Figure 6). Between the number series screens, participants were presented with a waiting screen, showing the time elapsed since the start of the experiment. Research has shown that a prior activity varies the response to a new activity and the engagement with a task over time (Yoon et al. 2006). To account for this effect the stimuli (number series) and the waiting screens were presented for different amounts of time, following the common practice in behavioral research (Lin et al. 2018). Each different series of numbers is displayed for either 10 or 15 seconds randomly. The waiting screens are presented for either 15 or 20 seconds randomly.

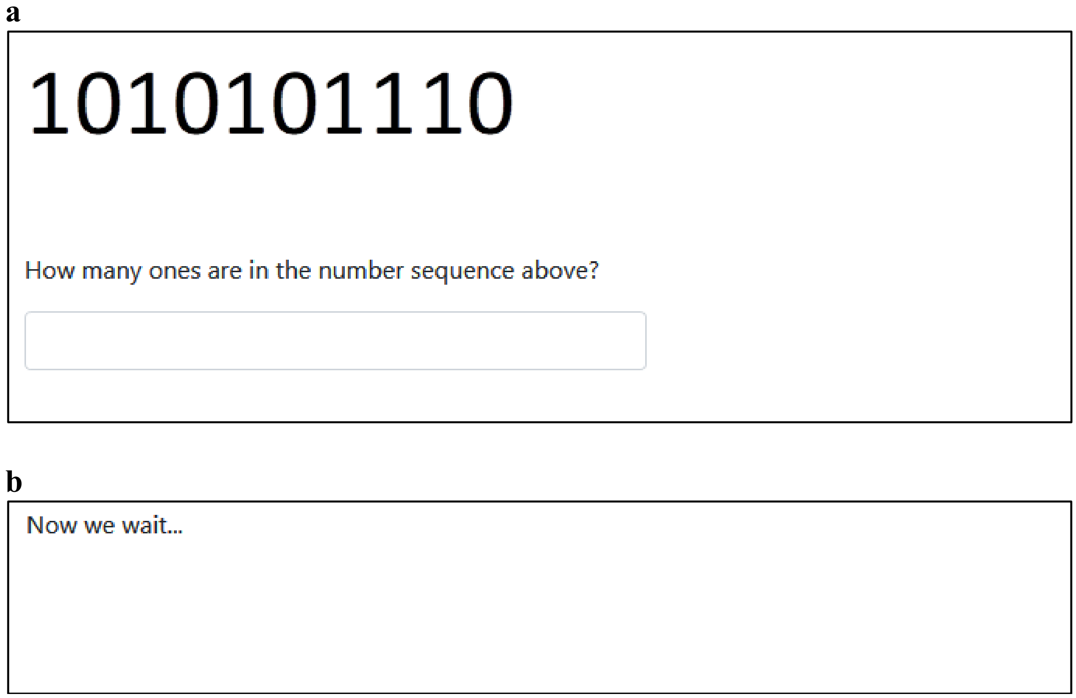


Figure 6. Number counting (a) and waiting (b) screens viewed by participants in the self-control depletion manipulation.

In total, participants in this manipulation are presented with 30 counting screens and 29 waiting screens. Participants exposed to the self-control depletion manipulation perform the self-control task for a total of approximately 15 minutes (30 counting screens for 10-15 seconds and 29 waiting screens for 15-20 seconds equals 880 seconds ~ 15 minutes). The other half of the participants are the control group and are not exposed to any counting task. To avoid a fatigue effect, the control group views a 15-minute video on “The History of Formula 1” before the seller / buyer game. Similar neutral manipulations have been used in the cognitive load literature (Marcora, Staiano, and Manning 2009). This activity is also included to help avoid the income effect of the payment distribution. If participants without the self-control depletion task take a shorter amount of time to complete the study, the payment per time spent in the study would be higher. There is a chance this could lead to an income effect on the results.

If self-control affects being truthful, sellers exposed to the self-control depletion task should be more likely to send the deceptive message to buyers relative to the control. Comparing differences in lying between self-control depletion manipulations will shed light on how self-control depletion impacts honesty. If self-control depletion makes participants more trusting, more buyers follow the suggestion of the seller under the self-control depletion manipulation. To gauge trusting of buyers, an analysis of the proportion of buyers that followed the advice of sellers, e.g. seller sent message 1 and buyer chose Option A and vice-versa, is done in each of the self-control depletion manipulations.

Probability of Being Caught

When the cost of being caught lying exceeds the potential benefits of lying, honesty is expected (Kajackaite and Gneezy 2017). Most markets operating under information asymmetry have a non-zero probability of being caught lying (Kübler, Müller, and Normann 2008). Authors across different fields document how decreasing (increasing) of the probability of being caught lying fosters (mitigates) dishonest behavior (Saha and Poole 2000; Treisman 2000; Wikström, Tseloni, and Karlis 2011).

To investigate the effect of the probability of being caught lying on truthful behavior, half of the participants, the control group, play the game under complete information asymmetry – sellers have 0% probability of being caught if they lie. For the other half of the participants, sellers have a chance of being caught if they lie. The purpose of having sellers face a likelihood of being caught has less to do with the actual probability of being caught lying and more to do with planting the idea in their minds that they may get caught if they do. It has been documented that a large probability of being caught would deter lying (Fischbacher and Föllmi-Heusi 2013). This study, however, explores if a small probability of being caught lying that may be perceived as inconsequential to some of the sellers interacts with the effect of the default option and self-control depletion on truth-telling. This follows from the idea that agents in markets with information asymmetries have relatively small probabilities of being caught.

To help determine what probability of being caught could potentially prompt a different course of action by sellers, the literature on the likelihood of being caught in different markets is examined. Bryant and Eckard (1991) find 13-17% is the likelihood that a firm in the United States may be caught price-fixing in any given year by regulatory agencies. Combe, Monnier, and Legal (2008) show that in Europe the probability of being caught as a part of a cartel is 12-13%. Scholz and Pinney (1995) found that though the probability of being audited by the IRS is 1%, the likelihood of being caught in tax evasion is much smaller (<1% of the audited sample). In food markets, it is even more challenging to gauge the real size of the problem, as most of the fraud goes undetected, and when fraud occurs the fear of retaliation leads to underreporting (Wiseman 2013). This is also related to the lack of attention that has been given to crimes on food by researchers and regulatory agencies (Lord, Flores Elizondo, and Spencer 2017). The work of Shears (2010) sheds some light by finding that fish markets in the UK sell salmon with either altered or no information on average 15% of the time, but the probability that vendors get caught is less than 2%. Experimental research on lying has used different probabilities of being caught ranging from 3% (Gamliel and Peer 2013) to 80% (Coricelli et al. 2010; Pascual-Ezama et al. 2015).

You are the seller. This means you will send a message to the buyer. The buyer will have to choose between option A and option B. The option that the buyer selects will determine the payouts for both.

If the buyer selects option A, you (the seller) get $2 and they (the buyer) gets $3.

If buyer selects option B, you (the seller) get $3 and they (the buyer) gets $2.

There is a 1 in 10 chance that the buyer will see this same information. There is a 9 in 10 chance that the buyer will only see the message you send them.

Knowing this information, now you get to send information to the buyer. You can send the buyer either one of the following messages:

Message 1: Option A pays you (the buyer) more

Message 2: Option B pays you (the buyer) more

Which message would you like to sent to the buyer?

○ Message 1: Option A pays you more

◉ Message 2: Option B pays you more

Remember the message you send is what the buyer will read. Also remember that what they choose will determine how both of you will be paid.

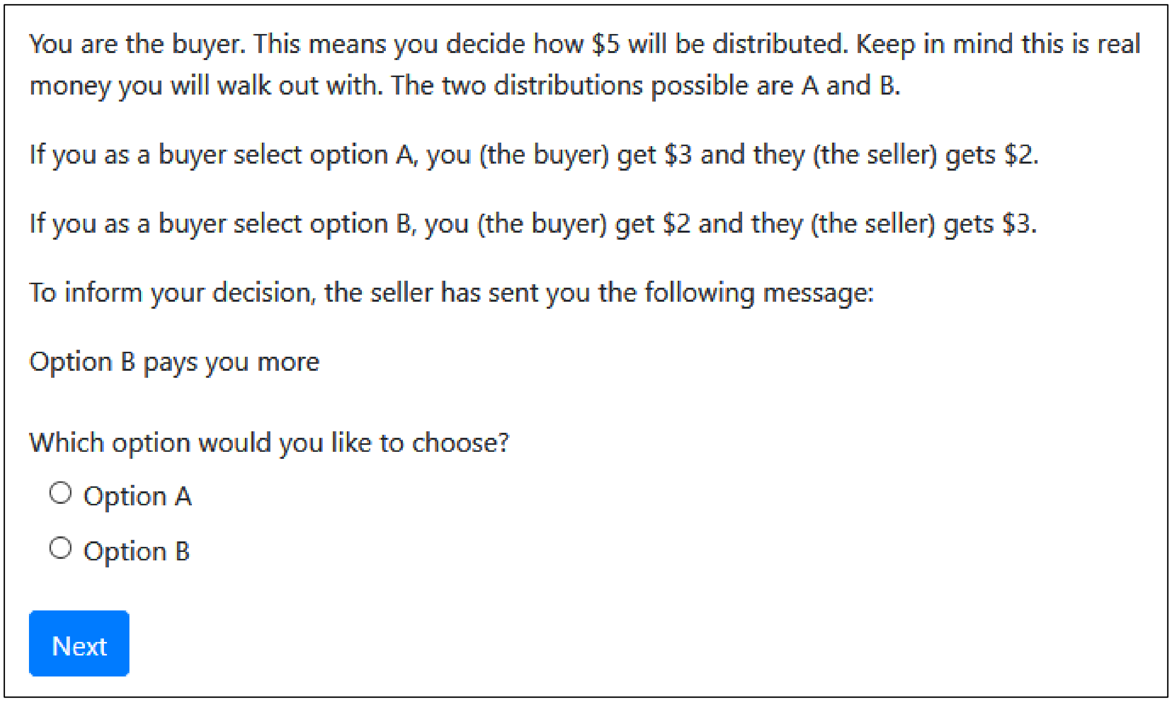
Next

Figure 7: Message sending box viewed by the sellers in the being caught manipulation.

Given the vast dispersion of probabilities that have been used in similar contexts and the large range of probabilities documented in markets, there is no clear guidance as to what constitutes an appropriate probability of being caught that could influence behavior in of participants this context. Therefore, as a probability that could be relevant for some participants and inconsequential to others, this study uses 10% as the probability of being caught lying in the game. A rationale for the use of 10% is that this percentage may be easier to understand than other probabilities due to its fluency (Coulter and Roggeveen 2014) – using 10% may help eliminate the potential confusion among participants by having trouble processing the probabilities (Peters et al. 2006).

To include the probability of being caught, for 1 of 10 pairs of participants in the disclosure manipulation both participants are given the payout distribution information. Sellers are made aware of the possibility of payout distributions being disclosed to buyers in the instructions before they choose which message to send to buyers (Figure 7). This allows sellers to decide if they would prefer to risk lying or not before sending the message.

No penalty comes to the sellers from this disclosure, but the setup allows sellers to be aware that their true intent may be revealed to the buyers. This is important in the design, as with no monetary consequence, the possibility of being caught lying affects only the intrinsic motivations of sellers, as the other manipulations do. Also, by not imposing a penalty on sellers the potential effects of different risk aversion over gains and losses are avoided. The probability of getting caught is enacted by showing the distributions of payments to buyers in 1 of 10 pairs in the disclosure condition. Those buyers who are shown the payout information see below the payment distribution information the message sent by the seller about which option the buyer should pick (Figure 8). Disclosing the payout distribution information will reveal to the buyer if the seller was being truthful or not in their message. A comparison of truthful messages sent informs on the effect of the possibility of being caught on truthful behavior.


You are the buyer. This means you decide how $5 will be distributed. Keep in mind this is real money you will walk out with. The two distributions possible are A and B.

If you as a buyer select option A, you (the buyer) get $3 and they (the seller) gets $2.

If you as a buyer select option B, you (the buyer) get $2 and they (the seller) gets $3.

To inform your decision, the seller has sent you the following message:

Option B pays you more

Which option would you like to choose?

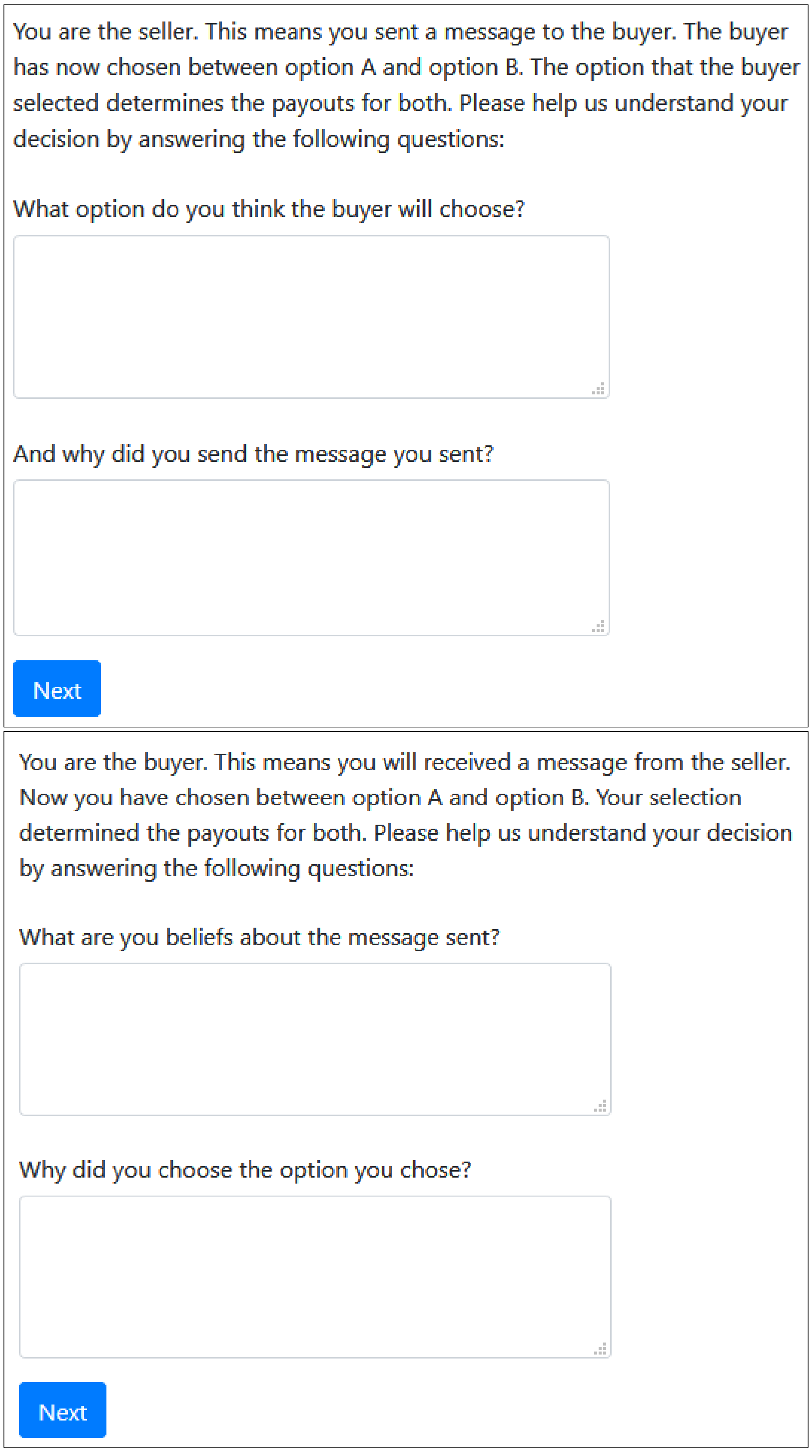
- Option A
- Option B

Next

Figure 8: Options selection box viewed by the buyers in the being caught manipulation.

Procedures

After participants complete the self-control depletion task (or distraction), they proceed to play the seller / buyer game. After participants play their roles of seller and buyer in the game but before their respective payouts are revealed, participants are asked open-ended questions about their motives (Figure 9). Sellers are asked why they sent the message they sent and what their expectations from the buyers are. Buyers are asked why they chose the distribution they did and what are their beliefs about the seller. Once the seller / buyer game is completed, all participants complete a survey collecting demographic information.


You are the seller. This means you sent a message to the buyer. The buyer has now chosen between option A and option B. The option that the buyer selected determines the payouts for both. Please help us understand your decision by answering the following questions:

What option do you think the buyer will choose?

And why did you send the message you sent?

Next

You are the buyer. This means you will received a message from the seller. Now you have chosen between option A and option B. Your selection determined the payouts for both. Please help us understand your decision by answering the following questions:

What are you beliefs about the message sent?

Why did you choose the option you chose?

Next

Figure 9: Open-ended question boxes viewed by the sellers (top) and buyers (bottom).

Table 8: Summary of each Treatment Manipulation

Treatment manipulation	Default option	Self-control depletion	Probability of being caught	Number of Participants
1	Truth	No	No	45
2	Truth	Yes	No	63
3	Truth	No	Yes	48
4	Truth	Yes	Yes	48
5	Lie	No	No	51
6	Lie	Yes	No	42
7	Lie	No	Yes	39
8	Lie	Yes	Yes	57

Participants also complete a scale that measures their numerical ability (Weller et al. 2013) and the cognitive reflection test (Frederick 2005) to capture their engagement level. The morality of participants is measured using the scale developed by Aquino and Reed (2002). A copy of the complete survey instrument is available in Appendix E. When finished, participants are paid according to the distribution chosen by the buyer and dismissed. The information about the prize being distributed is present on every screen the participants see. No other information beyond the information previously described is available to participants. A summary of the different treatments is presented in table 8.

Econometric Specification

The main outcome of this study is whether or not sellers lie. This is a binary choice. The decisions of sellers are identified as either 1 if the message sent is a lie and coded as 0 if the message sent is the truth denoted by the variable *lie*. The behavior of buyers is evaluated similarly. Whenever the buyer chooses the option recommended by the seller, the variable *trust* is coded as a 1. Meanwhile, a 0 in the *trust* indicates the buyer chose the option not recommended by the seller.

Given the outcomes are binary, the probabilities of the events can be modeled with different approaches. The two most common distributional assumptions made to model binary outcomes are the probit and logit models. These models assume that the data either come from an inverse normal distribution (probit) or a logistic distribution (logit) (Wooldridge 2010). The difference in the assumed probability distributions implies that a logit relative to a probit will be better at capturing extreme values (Greene 2012). This makes the estimation slightly more robust[3].

Given these properties, a logit model with robust standard errors is used to evaluate the choices of sellers and buyers in this study. The model selection implies that the estimation being fit is calculating the probability of lying or trusting (y) with the following specification: $P(y = 1|x_n) = \frac{\exp(\beta\, x_n)}{1+\exp(\beta x_n)}$, where x_n is the vector of individual characteristics and manipulations that are used to predict the choice. To measure the effects of the manipulations on lying and trusting, each treatment manipulation is modeled as a 0 /1 qualitative variable with 0 being the control group and 1 indicating participants were exposed to the manipulation. Numeracy, cognitive reflection, morality, and age in years are included as continuous variables. Gender is a qualitative variable with being male coded as 1 and 0 otherwise. Participants' race is accounted for in the model with 0 / 1 qualitative variables for white, Asian, and other races. The last variable includes participants who identified as Black, Latino, Native American, or mixed-race. These are not a separate category since participants that identify as such comprise 7% of the sample.

[3] The results are also evaluated with a probit model. The estimates have similar patterns, magnitudes, and significance.

Table 9: Summary of Individual Characteristics in the Sample

	Treatment								
Variable	1	2	3	4	5	6	7	8	All
Average age	20.07	20.38	20.63	19.94	20.29	19.79	21.38	20.26	20.33
	(0.19)	(0.15)	(0.21)	(0.26)	(0.17)	(0.13)	(0.46)	(0.18)	(0.08)
Males (%)	46.67	19.05	25.00	50.00	47.06	50.00	46.15	31.58	37.88
	(0.07)	(0.05)	(0.06)	(0.07)	(0.07)	(0.08)	(0.08)	(0.06)	(0.02)
White (%)	60.00	71.43	81.25	87.50	58.82	64.29	69.23	70.00	70.45
	(7.36)	(5.74)	(5.69)	(4.82)	(6.96)	(7.48)	(5.85)	(5.97)	(2.30)
Asian (%)	13.33	19.05	12.50	0.00	23.53	35.71	15.38	30.00	18.00
	(5.12)	(4.99)	(4.82)	(0.00)	(6.00)	(7.48)	(5.85)	(5.97)	(1.97)
Other[a] (%)	26.67	9.52	6.25	12.5	23.54	14.29	15.38	0.00	12.88
	(6.67)	(3.73)	(3.53)	(4.82)	(6.00)	(5.46)	(5.85)	(0.00)	(1.69)
Numeracy score (max 5)	3.93	3.48	3.63	4.06	3.29	4.14	3.54	3.68	3.68
	(0.15)	(0.18)	(0.17)	(0.11)	(0.18)	(0.14)	(0.23)	(0.14)	(0.06)
CRT score (max 3)	0.60	0.71	0.44	0.88	0.82	1.07	0.38	1.00	0.75
	(0.14)	(0.12)	(0.13)	(0.17)	(0.14)	(0.17)	(0.10)	(0.14)	(0.05)
Morality (max 7)	4.57	4.69	4.94	4.48	4.45	4.60	4.63	4.47	4.60
	(0.09)	(0.08)	(0.08)	(0.08)	(0.12)	(0.12)	(0.09)	(0.08)	(0.03)
Participants	45	63	48	48	51	42	39	57	393

[a] The other races declared by participants were Black, Latino, Native American, and mixed race. None of these comprised more than 7% of the sample so they are grouped for the analysis. Standard errors in parentheses.

Sample

Four hundred participants took part in the experiment. One seller did not follow the instructions and the data from this seller and buyer are removed. Two other buyers did not complete the survey portion of the study and are excluded from the analysis. These exclusions left 199 sellers, and 197 buyers.

Table 10: Correlations Between the Variables Used in the Estimations

Variable	Age	Male	White	Asian	Other	Numeracy Score	CRT Score	Morality
Age	1.00							
Male	0.17*	1.00						
White	-0.22*	-0.11	1.00					
Asian	-0.16	-0.02	-0.70*	1.00				
Other	-0.12	0.12	-0.54*	-0.07	1.00			
Numeracy score	-0.15	0.18	0.14	0.03	-0.22	1.00		
CRT score	0.06	0.32	-0.11	0.16	0.05	0.44*	1.00	
Morality	0.00	-0.10	0.05	-0.03	-0.11	-0.11	-0.19*	1.00

Significance at the 5% is denoted by *.

The study was conducted in 16 different sessions (two sessions per treatment) in the Behavioral Research Lab at the Gatton College of Business and Economics at the University of Kentucky. The assignment of treatments was random but was done by session and not to the experiment as a whole. Participants were undergraduate students enrolled in business courses at the University of Kentucky. Participants taking part in the experiment received extra credit in one of their courses in the Business School. Each participant only plays the game once in the role of a seller or a buyer.

A summary of the demographic variables and scale values by treatment is given in table 9. As expected from using a college student sample, the students' age, numerical ability, cognitive reflection, and morality are relatively homogenous. The gender and race proportions are not distributed equally between treatment manipulations (χ^2 $p < 0.05$). This could jeopardize the validity of the results. To evaluate how worrisome gender and race distributions may be, correlations between the variables are informative (table 10). The variables of interest as potential explanations for the behavior of participants (numeracy, CRT, and morality) are not correlated in a statistically significant way with the demographics. Therefore, though not ideal, it is unlikely that the unbalanced sample has a significant effect on the results, and race and gender are included in the econometric analysis[4].

Results

Effects of Information Asymmetry on Behavior

This study set out to test if the different manipulations affect lying behavior in an artificial market with information asymmetry. Treatment effects are tested by evaluating the percentage of sellers who lied in each of the experimental conditions. A summary of the average percentage of lying by sellers is shown in table 11. The proportion of sellers who lied is statistically different from zero in all conditions. The main inference drawn from these results is that, not surprisingly, sellers who possess more information than buyers use the information to make additional profits.

The percentage of sellers lying is 54.3% when there was a chance of being caught compared to 52.9% when there was not a chance of being caught. These percentages are not significantly different (χ^2=.01, $p > 0.10$); therefore, the results do not support hypothesis 1. Different default options produce statistically different levels of truth-telling. Sixty percent of sellers send the truthful message when the default is the truth condition compared to 40.0% when the default is a lie condition (χ^2 = 4.10, $p < 0.10$), providing support for hypothesis 2.

[4] The models are estimated excluding the unbalanced demographics and have the same pattern and significance, with similar magnitude.

Table 11: Summary of Choices by Sellers in Each Treatment

Chance of getting caught	Can get caught				Cannot get caught				Average
Self-control	Depleted		Not depleted		Depleted		Not depleted		
Default option	Truth	Lie	Truth	Lie	Truth	Lie	Truth	Lie	
Lying (%)	37.50	68.97	47.83	65.38	53.13	57.14	58.33	40.00	54.27
	(10.09)	(8.74)	(10.65)	(9.51)	(8.96)	(11.07)	(10.28)	(11.24)	(3.54)
N	24	29	23	26	32	21	24	20	199

Standard errors in parentheses.

The depletion of self-control increases lying behavior in sellers. This provides support for hypothesis 3a, with 62.0% of sellers lying in the self-control depletion group versus 38.0% in the control group (χ^2 = 6.58, p < 0.10).

Effects of Treatments and Individual Traits on Behavior

To examine the effects of the different manipulations simultaneously, the purpose of designing a full factorial study, the choices of lying and trusting are modeled as a logit model in Stata (StataCorp 2015) using the *logit* command (table 12). As discussed previously, the estimation includes qualitative (0/1) variables for the treatments (BC, where 1 is having a 10% chance of being caught lying, DT with 1 implying the default option, is the truth and SC where 1 means that self-control is available and not depleted), the interactions between the treatments, age, gender, race (other race as the base), and numeracy and engagement (measured with CRT) as individual traits. Correlations between the variables suggest multicolinearity is not a problem in the data[5] (table 10).

[5] A variance inflation factor (VIF) analysis was also done on the data and the mean VIF is 1.50, and the highest VIF was 2.35 supporting the idea that multicolinearity is not an issue.

Table 12: Logit Estimation of the Likelihood of Lying by Sellers and Trusting by Buyers

Variable	Lying		Trusting	
	Coefficient	Std error	Coefficient	Std error
Being caught (BC)	1.041	(0.758)		
Default truth (DT)	-0.545	(0.582)		
Self-control (SC)	0.006	(0.799)	-1.453***	(0.370)
BC x DT	-2.274**	(1.034)		
SC x DT	-1.016	(0.984)		
SC x BC	-2.974***	(1.128)		
BC x DT x SC	4.856***	(1.128)		
Numeracy score	0.212	(0.190)	0.049	(0.183)
CRT score	-1.111***	(0.256)	-0.365*	(0.206)
Morality	0.455**	(0.232)	0.211	(0.295)
Demographics				
Age	0.008*	(0.117)	-0.071	(0.110)
Male	0.612	(0.399)	-0.076	(0.397)
White	-0.684	(0.561)	0.466	(0.532)
Asian	-1.357*	(0.702)	-1.437**	(0.637)
Intercept	-0.839	(2.878)	2.516	(3.065)
N	199		189	
Log-likelihood	-114.96		-100.11	
AIC	259.93		218.21	
BIC	309.33		247.81	

Robust standard errors in parentheses. Significance at the 10%, 5%, and 1% is denoted by *, **, and ***.

The econometric estimation yields somewhat different results than the comparative statistics tests. With an experimental design like the one used in this study, the treatment conditions are evaluated simultaneously to understand how they interact.

First, the three-way interaction between self-control being available, having a likelihood of getting caught, and the default option being the truth is positive and statistically significant ($p < 0.10$; table 12). A significant three-way interaction implies that the interaction of two of the factors has different effects on the different levels of the third factor. Interactions at the different levels of a third factor are better understood by examining them graphically.

Predicted probabilities of lying based on the logit model are graphed in figure 10. The predicted probabilities are estimated using the *margins* command in Stata (StataCorp 2015). The probability of lying is estimated using the parameter estimates from the estimated logit model and calculating the probabilities at the mean value of the covariates for each of the different combinations of treatments. The predicted probabilities for lying when self-control has been depleted are given by the bars at the top of figure 10, whereas the bottom bars gives the predicted probabilities when self-control is available. To assess the three-way interaction, the interaction between the two factors, the default option and the probability of being caught lying are evaluated under the two levels of the third factor, self-control being depleted and self-control being available.

In the top bars, it is illustrated that when self-control is depleted having a 10% chance of being caught works together with having a default truth option to reduce lying behavior. When self-control is depleted and there is a 10% chance of being caught, sellers lie 86% in the default lie condition versus 29% when the default is the truth ($z = 3.73$, $p < 0.10$). With self-control depleted and no chance of being caught lying, sellers lie statistically the same amount under both default options, with 69% when the default is a lie versus 57% with a default truth ($z = 1.01$, $p > 0.10$).

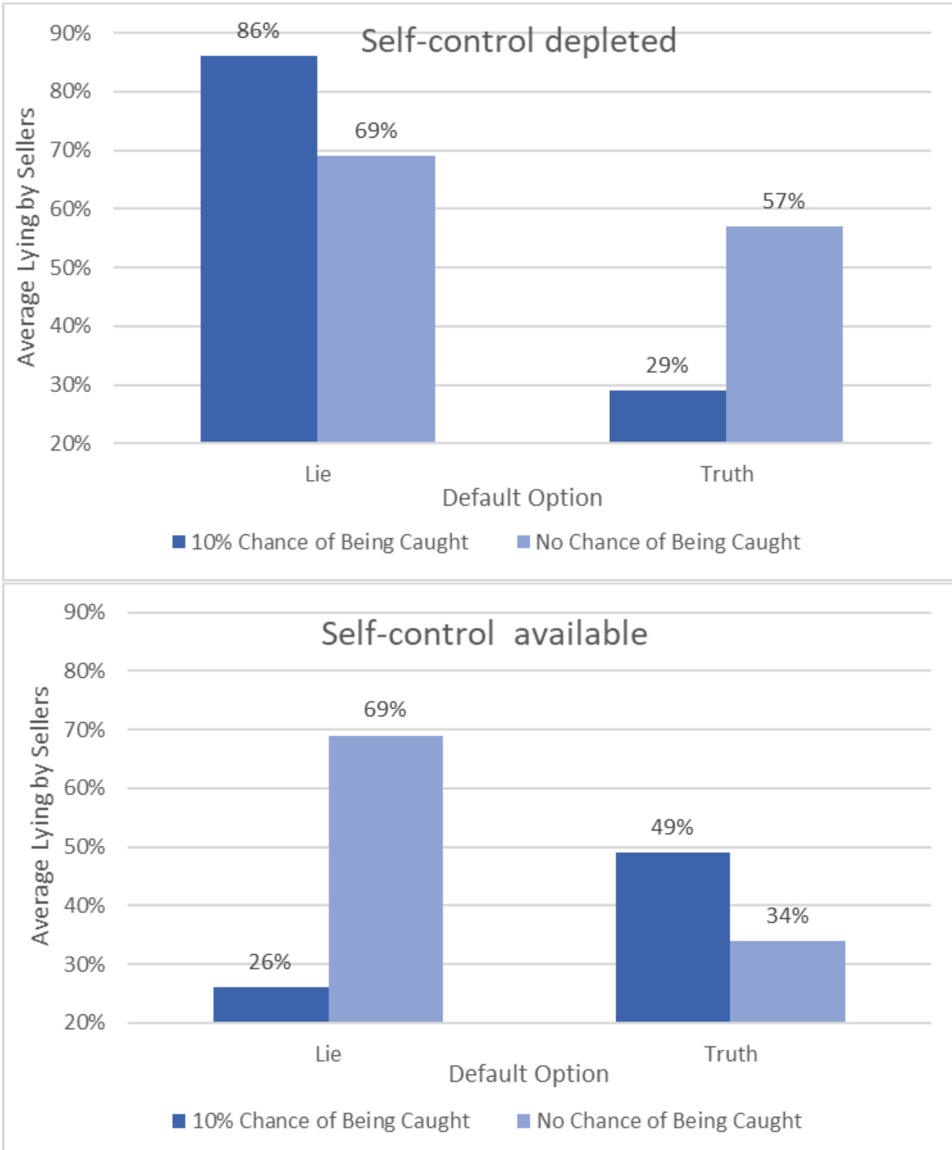


Figure 10: Lying probability by the sellers in different conditions when self-control is depleted (top) and when self-control is available (bottom).

Therefore, when self-control is depleted having both a chance of being caught lying and a default truth reduces lying behavior, but just one of these manipulations does not reduce lying in a statistically significant way. When self-control is available the previous results on the interaction between the chance of being caught and the default option does not hold ($p > 0.10$; bottom graph in figure 10). If self-control is available, with a 10%

chance of being caught lying, the probability of lying is statistically the same, 49% probability of lying when the default is the truth versus 26% when the default is a lie (z = 1.31, $p > 0.10$). Meanwhile, when there is no chance of being caught lying, sellers lie 34% of the time when the default is the truth and 69% when the default is a lie (z = 2.01, $p < 0.10$). These results indicate that when self-control is available the interaction between the 10% chance of being caught lying and the default option being the truth is lessened. There is less lying between a default truth and a default lie when the probability of being caught lying is zero. The three-way interaction, therefore, implies that the default option being the truth interacts positively with a 10% chance of being caught lying when self-control is depleted, but that when self-control is available, the default option being the truth does not interact with the chance of being caught ($p < 0.10$).

The next step is to address the two-way interactions. The interaction between the chance of being caught lying and having the default option be the truth is negative and statistically significant ($p < 0.10$; table 12). The interpretation is that if the default option is the truth and there is a chance of being caught lying, sellers are less likely to lie. In other words, these two conditions work together to reduce the chance of lying by sellers. The probability of lying is statistically the same when the default is a lie (68%) and a default being the truth (48%) when there is no chance of being caught lying ($p > 0.10$), but statistically lower for the default truth (37%) versus the default lie (63%) when there is a 10% chance of being caught lying ($p < 0.10$).

The second two-way interaction is between self-control availability and the probability of being caught lying, which is also negative and statistically significant ($p < 0.10$; table 12). The interpretation of this interaction is that when sellers' self-control is not depleted, they care more about being caught lying, which reduces their incentives to lie. When self-control is available the probability of being caught reduces dishonesty ($p < 0.10$). Comparing the marginal probabilities of lying when self-control is depleted versus when self-control is available illustrates this interaction. For example, when there is a 10% chance of being caught lying and self-control is available the probability of lying is 39%, which is statistically lower than the 54% probability of lying found when self-control has been depleted, ($p < 0.10$).

Finally, the interaction between having self-control available and the default option being the truth is not statistically different from zero ($p > 0.10$; table 12). This interaction implies that having the option being the truth does not make sellers who have self-control available lie less, nor does having the default option being a lie make sellers with self-control depleted lie more. The probability of lying with a default truth when self-control is available (41%) is statistically equivalent to the probability of lying when self-control is depleted (44%; $p > 0.10$).

The parameter estimates for the main effects of the treatments are not statistically significant ($p > 0.10$; table 12). Given that the interactions between the treatments are significant, it is not surprising that the main effects of the treatments are not statistically different from zero. Having significant interactions between the treatments implies that the effects of the treatments are conditional on the levels of the other factors. Therefore, the influence of each treatment on reducing lying is captured by the conditional effect that the interactions with the other treatments reflect. The marginal effects for each treatment, however, provide support for the hypotheses 1, 2, and 3a, corroborating the findings of the comparative statistics analysis. A 10% chance of being caught lying has a lower probability of sellers lying at 57% versus 47% with no chance of being caught ($z = 1.93$, $p < 0.10$). Having the default option being the truth will likely reduce lying (64% lying with a default lie versus 44% lying with a default truth; $z = 2.93$, $p < 0.10$). The last treatment, self-control depletion, makes sellers more likely to lie (59% with self-control depleted vs. 43% with self-control available; $z = 2.63$, $p < 0.10$). These main effects are then modified by interactions between the treatments but provide some guidance for future experiments that hope to reduce lying.

The estimation results (table 12) indicate there is no statistically significant relationship between numerical ability and lying behavior, therefore not supporting hypothesis 4a. The parameter estimate for CRT is negative and statistically significant. implying as the engagement of sellers increases lying is less likely, supporting hypothesis 5a. Estimation results show an opposite direction to that suggested in hypothesis 6a; larger scores in the morality scale increases the likelihood of lying by sellers. As for the demographic controls, lying is marginally reduced by age, and sellers who self-identified as Asian are less likely to lie.

To evaluate the trusting behavior of buyers it is important to understand how the manipulations play a role for buyers. Buyers can trust or mistrust the information received from the sellers if they do not possess the payout information. Recall that in sessions under the getting caught lying treatment, one out of ten buyers was randomly selected to have the payout information disclosed to them. Therefore, in sessions when treatments 3, 4, 7, and 8 were conducted there were buyers to whom the payout information was revealed. These buyers, unlike the rest of the buyers in the study, do not experience information asymmetry, as they possess the same information as the sellers.

Table 13: Summary of Choices by Buyers in Each Treatment

	Self-control depletion	No Self-control depletion	Average
Trusting (%)	63.87 (4.42)	84.21 (4.21)	76.46 (4.29)
N	72	117	189

Standard errors in parentheses.

These buyers, therefore, cannot choose between trusting or mistrusting the sellers, as they make their choices of payout distribution with full information regardless of the messages sent by their sellers. Therefore, although buyers and sellers were paired in the study, there are fewer buyers to analyze the effects of the treatments than there are sellers because some buyers having full information. Eight buyers are in the getting caught treatment manipulation and were randomly selected to have the payout information disclosed to them. The choices from these buyers with full information provide an idea of the default behavior of buyers who have full information. The remaining 189 buyer data points are used to estimate the treatment effects.

By design, the only treatment manipulation that the buyers are exposed to is the self-control depletion. The default option in the messages to be sent is not information that the buyers are given. The probability of being caught lying displayed to the sellers is also information that the buyers do not see. Therefore, there is no reason to expect that the default option and the possibility of being caught have any impact on the decisions of buyers. The eight buyers who faced full information can provide support to the assumption that the probability of sellers being caught and the default option for sellers did not change buyers' behavior. The eight buyers followed the sellers' advice in a similar proportion as the rest of the buyers in the being caught treatment (50.0% versus 48.3%, χ^2

= 0.17, p > 0.10). Furthermore, from the four buyers who were in the default truth and the four buyers who were in the default lie conditions half of each group chose the payout distribution that was recommended by the sellers. These results imply that the buyers behave similarly whether they are in the groups where sellers have a probability of being caught lying, or whether there are in a group where sellers have a default truth. With these results in mind, the trust demonstrated by the rest of the buyers is evaluated only by self-control depletion (table 13) finding that when self-control is depleted buyers trust more.

The Effects of Morality and the Qualitative Analysis of the Open-ended Responses

The estimation of the model showed that higher moral value scores did not affect the trusting behavior of buyers, but a higher score in morality increases the likelihood of lying by sellers. This last result is surprising and counterintuitive as the logic would suggest highly moral sellers would lie less. Potential explanations are found in the review of the comments expressed in the open-ended section of the study. Comments provided by the sellers about their motivations to send the message they sent and the buyers to accept or reject the offers from the sellers are evaluated using qualitative sentiment analysis (Pang and Lee 2008). With this analysis, the motivations of sellers and buyers are grouped into five categories (table 14).

The categories for the motivations of the sellers are profit-maximizing, honesty perceptions, strategic behavior, regards for others, and self-justification. The motivation categories for the buyers to accept or reject the offers are profit-maximization, honesty perceptions, strategic behavior, the expectation of negative reciprocity, and the nature of the information.

Table 14: Summary of the Stated Motivations of Sellers for the Message Sent

	Seller Behavior			Buyer Behavior	
Motivation	Dishonest	Honest	Motivation	Trust	Mistrust
Profit maximizing	82%		Profit maximizing	5%	
Self-justification	13%		Honesty perceptions	87%	58%
Honesty perceptions		56%	Strategic behavior		17%
Strategic behavior	5%	28%	Negative reciprocity		25%
Regards for others		16%	Nature of the Information	8%	

Buyers who accepted the offers from sellers mentioned they expected sellers to be honest as the main source of their desire to trust the information (87%), with the nature of the information (8%), and their desire to maximize profits (5%) as alternate explanations. Buyers who mistrusted the messages from sellers expressed their concern about the honesty of the sellers as the main motivation to not trust the seller (58%), with their expectations about reciprocity (25%) and strategic behavior by the sellers (17%) as alternate justifications. The motives expressed by truthful sellers indicated honesty perceptions as the main driver of their choice (56%), with strategic behavior (28%), and regards for others (16%) as second and third rationales. In contrast, the main reason cited by sellers who lied is profit-maximizing (82%). The other two motives used by sellers who lied are self-justification (13%) and strategic behavior (5%).

Focusing on the comments of sellers who scored high on the morality scale and lied, the type of comments that are classified as self-justification include phrases such as "it's a business and there isn't much else to consider besides who gets the most money," "because I think salespeople make more money," or "I'm the seller, I should make more profit." This qualitative analysis suggests a relationship between self-justification and lying. These responses came from sellers who scored high on the morality scale. Research on distributive justice and entitlement has found that participants in economic experiments who find themselves as morally equal or morally superior to their counterparts are likely to come up with justifications for unfair behavior (Hoffman and Spitzer 1985). The self-justification responses found in this study follow a similar pattern.

Conclusions

This study contributes to the economics and marketing literature by taking a behavioral economics approach towards investigation honesty and trusting in asymmetric markets. This is done by evaluating how seemingly trivial manipulations affect sellers' honesty and buyers' trusting. In particular, the study evaluates how self-control, the chance of being caught lying, and the default option interact to affect participants in an artificial market with information asymmetry. The results indicate that all of these conditions can interact to be effective in decreasing sellers' dishonest behavior, albeit with some caveats. When there is a 10% chance that sellers may be caught lying, a default truth increases honesty behavior in sellers, although this is only when self-control has been depleted. If sellers' self-control has been depleted, having a 10% chance of being caught is not enough deterrent to reduce lying, but self-control being depleted reduces lying when the default is truth. When sellers have self-control available, lying is less likely. In the scenario where self-control is available, being made aware that sellers may be caught lying decreases their likelihood to lie. Main effects indicate that having a 10% chance of being caught lying, making the default option the truth, and self-control not depleted reduces the probability of sellers lying. These main effects are moderated by the interactions between them but provide some guidance for future experiments that hope to reduce lying.

These results have important practical applications. In line with previous findings in other contexts having the potential for consequences (Saha and Poole 2000; Kübler, Müller, and Normann 2008), expressed in this study as a 10% chance of being caught lying, reduce dishonest behavior when self-control is available. This suggests that in markets where information asymmetry exists buyers are less likely to be lied to when verification of the information is feasible. An opportunity for future research is to explore whether the reduction in lying is monotonic on the probability of being caught in a lie. Another practical implication of the findings is that the default option being the truth helped reduce lying when self-control is available, indicating lying can be mitigated by a bias coming from the settings for opting out versus opting in. An opportunity for future research is then further testing the opting out versus opting in setup under other scenarios.

Finally, similar to the work of Kouchaki and Smith (2013), who find that less ethical behavior is observed in the morning versus the afternoon (an example of self-control depletion), the study finds less ethical behavior is found when sellers have engaged in self-control depletion. Sellers exposed to self-control depletion are less susceptible than their counterparts who were not exposed to self-control depletion to be nudged to be truthful. Conversely, buyers exposed to self-control depletion are less likely to trust messages sent by the sellers than the control group. This suggests that buyers in markets with information asymmetry are better off trading when they and the sellers are not self-control depleted. Future research can examine how self-control interacts with other deterrents of dishonesty to expand on the findings of this study.

One more insight found in the results is that morality, as measured in this study, increases dishonest behavior. This finding is counterintuitive. A qualitative approach to the open-ended responses of sellers indicates that there may be a relationship between morality and lying that is nuanced by justifiability of behavior by sellers. Previous research in social psychology has indicated that situations of this nature are feasible (Corcoran and Rotter 1987; Shu, Gino, and Bazerman 2011; Gino and Mogilner 2013), but this is not explored in this study.

Engagement, as measured by CRT, plays an important role in honesty and trust. Sellers' tendency to lie decreases as their engagement with the task increases. Buyers' lack of engagement induces them to trust the messages from sellers. More research is necessary to disentangle the differences between these two mechanisms. Numerical ability, on the other hand, does not affect honesty and trust. This is comforting for highly specialized markets with information asymmetry that require numerical abilities (stock markets, derivatives, insurance, etc.). Additional research is necessary using specialized markets to evaluate if there is an effect of numerical ability on honesty and trust.

There are several limitations to the study. One is the use of a student sample for the study. Future research using nonstudent samples would make the results more robust and help with external validity. An argument, however, could be made against this criticism. The students in the study were willing to lie for a potential $1 increase in the payoff. Real markets with information asymmetry offer incentives to lying greater than $1. In markets with information asymmetry where profit differences between honest and dishonest pricing can be much larger than $1, honesty patterns like the ones observed in the study would be expected. The use of students, therefore, is not an insurmountable limitation but instead can be viewed as a minimum expectation of what would happen in real markets.

A second limitation is the abstract nature of the experiment. In the study, sellers are asked to send a message to the buyers anonymously through computers. In real markets, often a product is sold not just by sending a message. Instead, sellers have opportunities to negotiate, read non-verbal cues, and receive (almost) immediate feedback. Future research could extend the methodology to more realistic manipulations. Another limitation is that the current study does not test the relationship between justification and morality empirically. This is a limitation of this study that more research in economic contexts exploring this relationship could cover.

The present study advances economic research by exploring boundaries to dishonest behavior in asymmetric markets. By showing that nudges can decrease dishonesty, a more complete picture of the behavioral mechanisms behind dishonesty in asymmetric markets emerges. Behavioral economics dis also advanced by showing how the behavior of buyers is impacted by self-control depletion. The findings of the study illustrate how behavioral economics can contribute to shedding light on important and interesting economic problems, such as information asymmetry and its potential consequences.

CHAPTER IV

CONCLUSIONS

This dissertation takes advantage of merging different streams of research. Using findings from the fields of stated preference, experimental economics, and marketing to study economic phenomena, the objective of this dissertation is to show how behavioral economics can be used to elicit candid responses in experiments designed to increase the understanding of economics. This objective is achieved by designing and conducting two laboratory experiments that examine (1) how a different number of alternatives with the potential for consequences affects truthful responses, and (2) how behavioral nudges can be effective in moderating truth-telling in markets characterized by asymmetry information. The results show candid responses may be obtained from participants in economic experiments with the proper incentive structure and adequate design. In particular, the study described in Chapter II explores whether and how the number of alternatives with the potential for consequences has an effect on choice behavior in valuation studies for private goods, while the study in Chapter III sets out to find out how nudges, seemingly innocuous manipulations, can affect honesty and trust in a market with asymmetric information.

Using Incentives in Discrete Choice Experiments

When researching preferences, issues receiving a large amount of scrutiny are: whether participants' revealed or stated preferences are consistent with their true preferences, whether their responses are candid, and whether these responses can be used for policy implementation (Sælensminde 2002; Ding, Grewal, and Liechty 2005; Quaife et al. 2018). Previous research shows that revealed and stated preferences are similar under some circumstances (Haghani and Sarvi 2018). There are competing findings, however, suggesting this may not always be the case (Lusk and Schroeder 2004; Levitt and List 2007; Baltussen et al. 2012). Several biases may come to play to generate inconsistencies between stated and revealed preferences, and research has come up with procedures such as cheap talk, honesty priming, and certainty scales that have been shown can help mitigate such biases to a certain degree (Fifer, Rose, and Greaves 2014).

Given the evidence that there are differences between true preferences and responses given to questions without consequences, researchers interested in preferences have moved toward incentivized choice experiments to elicit truthful responses from participants (Brock and Durlauf 2001; Dong, Ding, and Huber 2010). In incentivized choice experiments, one (or more) of the choices is (are) selected, generally at random, to be consequential (Vossler, Doyon, and Rondeau 2012). Using such a mechanism where neither the experimenters nor the participants know a priori what outcome will be randomly picked provides an incentive for participants to provide truthful responses because of its consequences (Collins and Vossler 2009; Beck, Fifer, and Rose 2016). The literature on stated preferences suggests the presence versus the absence of consequentiality leads to different outcomes (Carson, Groves, and List 2014), and some research goes further to propose that when participants know not all the decisions they make in a study are consequential, both the effort to understand and follow the procedures and the willingness to reveal their preferences are compromised (Yang, Toubia, and de Jong 2018). The literature on incentivized choices proposes that the level of engagement shown by participants is different when consequences are present (Liebe et al. 2018) and that unengaged participants may be unwilling to exert the effort necessary to understand and follow experimental procedures (Bardsley 2005).

With this in mind, the current practice in studies of valuation of private goods is to use incentivized choice experiments, where either all the choices are potentially consequential, or no choice is incentivized. This is a challenge when preferences about products or features that do not exist are being evaluated, implying that some choices will be unavailable (Hoyos 2010; de Bekker-Grob, Ryan, and Gerard 2012). With the existing literature on availability focusing on the effect of the existence or absence of products in the market (Batsell and Polking 1985; Raghavarao and Wiley 1994) and how to incorporate market availability into the marginal utility estimations of choice models (Anderson and Wiley 1992; Lazari and Anderson 1994), the influence of availability of alternatives and how the number of alternatives with the potential for consequences impacts the behavior of participants in experiments has not been addressed. This is a gap the study presented in Chapter II addressed.

Results indicate that, on average, the number of options with the potential for consequences does not improve the likelihood of optimal decision making. When analyzing the results for large and small payoff differences between alternatives presented, however, the results show a different picture. It is found that for large payoff differences any number of alternatives with the potential for consequences increases the likelihood of making the optimal decision. In contrast, when the payoff differences are small, having any number of alternatives with the potential for consequences is not different from the scenario where no incentives exist in eliciting optimal responses from participants. These findings suggest that large payoff differences motivate participants to exert effort to engage in the task, resulting in better performance, even when not all alternatives present the potential for consequences. Participants exhibiting better numerical ability do not make more profit-maximizing choices than participants with poorer skills, regardless of payoff differences. In contrast, participants performed better as their level of engagement measured by their cognitive reflection score increased, even in the absence of incentives and regardless of the number of alternatives with potential for consequences, or payoff differences. This finding highlights the importance of increasing the relevance of the task for participants, a topic that demands more research in experimental economics. When payoff differences are large or participants are engaged by other means, there is a positive effect on the likelihood of making profit-maximizing choices. Putting the findings together, a suggestion for practitioners is that if an incentivized design on all alternatives is not possible, then the measurement of engagement is recommended. Further, even when not using an incentivized framework, researchers should aim their best to increase the engagement of participants, as this would help elicit preference revealing behavior. The results suggest that one effective way to increase the engagement of participants with the task enlarging the difference in payoffs between alternatives.

Using Nudges in Markets with Asymmetric Information

Markets, the intersection of agents willing to transact and cooperate, are not exempt from the notion that in most cases, if not all, one party has more information about the transaction than the other. Such a situation is referred to as asymmetric information (Mishra, Heide, and Cort 1998). In markets with asymmetric information, the party with additional information can withhold the information for personal gain. Although asymmetric information is a natural occurrence in markets (Leland and Pyle 1977), it can lead to suboptimal outcomes, even leading markets to cease to exist (Akerlof 1970).

In markets where asymmetric information occurs, the signals sent by the agents determine the trustworthiness of the exchange (Rothschild and Stiglitz 1978). Therefore, sellers questioning the beliefs of their buyers and buyers questioning the honesty of the sellers are behavioral considerations of how the signals may be interpreted. These concerns are reasonable, given research has shown that participants in economic experiments will lie for self-gain, even at the expense of others (Gneezy 2005; Mead et al. 2009; Fischbacher and Föllmi-Heusi 2013). Certain mechanisms such as repeat purchases (Rayo 2007), potential loss of future revenue (Grover and Goldberg 2010; Karp 2010), and the possibility of being caught (Bryant and Eckard 1991; Saha and Poole 2000), are deterrents to lying behavior by sellers. On the other hand, fatigue and reduced self-control (Baumeister and Vohs 2007) are likely to increase dishonest behavior in sellers. Given the potential of lying in markets with asymmetric information, an important research question is how to reduce lying in such circumstances. The answer to this question has meaningful practical relevance, considering that markets for credence attributes – properties of goods or services that cannot be easily verified by the buyers (Darby and Karni 1973; Emons 1997) – such as organic, local, etc, are an example of markets with information asymmetry that have been growing in size and economic value (Feagan and Morris 2009; Moser, Raffaelli, and Thilmany-McFadden 2011).

The study in Chapter III takes a behavioral economics approach when investigating honesty and trusting in markets with asymmetric information. This is accomplished by using nudges, seemingly trivial manipulations, to affect sellers' honesty and buyers' trusting behavior in a market with asymmetric information. In this experimental market, participants are randomly paired and assigned arbitrarily the role of buyer or seller. Each seller must make a one-line sales pitch to the buyer. The results indicate that all of the conditions explored can be effective to decrease sellers' dishonest behavior, with some limitations. When sellers have their self-control depleted, a 10% chance they may be caught lying and making the default option the truth reduces lying in sellers. When sellers have self-control available, however, being made aware that they may be caught lying decreases their likelihood to lie. As a guideline for future research, including a chance of being caught lying, making the default being truthful, and not depleting the participants self-control will likely reduce lying in studies. These effects are to be taken with caution, as the observed interactions between them moderate their individual effectiveness in reducing lying.

Another behavioral insight found in the results of this study is that dishonest behavior increases with morality. There may be a relationship between morality and lying that is impacted by sellers being able to justify their behavior. While this is not fully explored in this study, social psychology research suggests this is feasible (Corcoran and Rotter 1987; Shu, Gino, and Bazerman 2011; Gino and Mogilner 2013). More research in economic contexts exploring this relationship further is necessary.

The numerical ability of participants has no distinct effect on the honesty of sellers and trusting of buyers. Given the demand for numerical abilities in some real markets with asymmetric information (e.g. stock markets, derivatives, insurance, etc.) this is a comforting finding. Meanwhile, just like in the availability of alternatives for incentivizing explored in Chapter II, engagement plays an important role in honesty and trust. Sellers lie less as their engagement increases. Buyers trust sellers less as their engagement increases. This once again highlights the importance of engaging participants and the relevance of the task for economic agents in experiments.

Limitations and Recommendations for Further Research

Different opportunities for future research stem from the studies' limitations. The first one common to both studies is that the samples used are not representative. An argument could be made, however, that given the studies' objectives representative samples are not necessary. Future research could attempt to replicate the findings with a sample representative of the population.

The studies in this dissertation use a limited number of factors to explain the behavior of participants. Future research could consider other potential aspects of individual heterogeneity beyond the ones examined here. Potential candidates include digging deeper into motivations, measures of prosocial behavior, and other dimensions of cognition. Future research could also go deeper into the influence of participants' attributes. For example, the numeracy of participants is shown to affect performance. More research into how numerical abilities affect performance may shed light on whether there is an interaction between incentives and numeracy that affects performance. A potentially fruitful avenue for further research is framing the results under expected utility, risk preferences, and decision making under uncertainty. In both of the studies described in this dissertation, participants were making choices under uncertainty, but no information about the risk preferences of participants was gathered. The findings of the studies are not compromised as a result, as the random assignment into the treatments takes care of this potential confound. Economic theory, however, could benefit from exploring the findings under this light.

A third limitation common to both studies is the abstract nature of the experiments. The study used in Chapter II is a discrete choice experiment using colored shapes with different prices and potential profits. In the vast majority of incentivized choice experiments and the real markets that they are used to investigate, actual goods with multidimensional attributes are used. For the study described in Chapter III, sellers are asked to send a one-line sales pitch anonymously in a message to the buyers through computers. In real markets, often a product is sold not just by sending a message. Instead, sellers have opportunities to negotiate, read non-verbal cues, and receive (almost) immediate feedback. Future research should use more realistic settings to explore the robustness of the findings.

Results across both studies indicate that higher engagement leads to better performance even in the absence of incentives, a result that highlights the importance of how relevant the task is for participants. How to increase engagement of participants and the relevance of the task would be a fruitful avenue for future research. Finally, the studies' bonus assignment and payment may not be salient enough to provide an incentive to respondents to become engaged enough to consider the best choice. This is a challenge to experiments in general, given the monetary costs of conducting research. Future research may want to examine the level of incentives necessary to engage participants.

Concluding Remarks

Research presented in this dissertation advances economic research by exploring behavioral aspects that affect markets and economic research methods. By showing engagement is as important as the number of alternatives with consequences in a valuation task, that nudges can increase honesty, that self-control depletion can increase dishonesty and mistrust, and that engagement plays a crucial role in economic decisions, a clearer picture of the behavioral mechanisms impacting decision-making emerges. Findings illustrate behavioral economics can contribute to shedding light on important and interesting economic problems, thus achieving the overarching objective of this dissertation.

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APPENDIX A

SAMPLE CHOICE SET

APPENDIX B

EXPERIMENTAL INSTRUCTIONS FOR STUDY USED IN CHAPTER II

The purpose of today's experiment is to help us understand purchasing decisions. To accomplish this purpose, you will be asked to select over a series of twelve items. I will explain how the experiment will work and we will have a practice round. After that, you will choose from the alternatives presented and then we will ask you to fill in a survey. After the survey has been completed, you will receive payment for your participation in today's session.

The experiment we will conduct today will probably be different from any experiment you have had experience with previously. In each slide, we will present you with two shapes of different colors and you will have to choose which shape to buy. For each circle you will get $1.50, for each triangle you will get $1.00 and for each square, you will get $0.50. As for the colors, green will pay you $0.50 and blue will pay you $1.00. The value of each shape is the combination of its shape and its color. You will buy this shape and profit from the value of the shape. The price for each shape/color combination is shown underneath the shape. The profit comes from subtracting the price from the value the shape and color give.

(For the hypothetical treatment)

In this experiment we want to know your preferences, so please just choose the one you prefer. If you decide you prefer one of the products, you do not have to pay the price, your choice will not affect your compensation.

(For the binding treatment with all products available)

This is a real experiment. All the alternatives shown are potentially binding. We will select one of the twelve rounds of colored shape pairs as binding and what you chose in that round will be your purchase. You will pay the stipulated price and in exchange, you will receive the respective payout.

(For the binding treatment with not all products available)

This is a real experiment, but not all options of colored shapes shown are available for purchase. Right now, you will randomly draw [eight/sixteen] cards with the shapes in the experiment from a box and we will place them in the tumbler on the desk. These are unknown to you and to us until the end of the experiment when we will select one of the twelve rounds as binding. What you chose in that round will be your purchase. If the alternative you chose is one of the cards in the tumbler, i.e. part of the [33/66] percent available, you will pay the stipulated price and in exchange, you will receive the payout. If it is not, you will not pay the price and it will not affect your compensation.

APPENDIX C

COMPLETE SURVEY INSTRUMENT USED FOR STUDY IN CHAPTER II

1. **Please indicate your age in years: __________ years**

2. **Please indicate what gender you most identify with:**
   - **a.** ___ Female
   - **b.** ___ Male
   - **c.** ___ Other
   - **d.** ___ Prefer not to answer

3. **Please indicate your ethnicity (select all that apply):**
   - **a.** ___ Hispanic, Latino or Spanish origin
   - **b.** ___ White
   - **c.** ___ Black/African American
   - **d.** ___ American Indian or Alaskan Native
   - **e.** ___ Asian Indian
   - **f.** ___ Chinese
   - **g.** ___ Filipino
   - **h.** ___ Japanese
   - **i.** ___ Korean
   - **j.** ___ Vietnamese
   - **k.** ___ Native Hawaiian
   - **l.** ___ Gaumanian or Chamorro
   - **m.** ___ Samoan
   - **n.** ___ Other lease List: ______________________________________)

4. **Please indicate your family yearly income.** (Include all forms of income, including salary, interest and dividend payments, tips, scholarship support, student loans, parental support, social security, child support, and allowance).
   - **a.** ___Less than $20,000
   - **b.** ___$20,000-$29,999
   - **c.** ___$30,000-$39,999
   - **d.** ___$40,000-$49,999
   - **e.** ___$50,000-$59,999
   - **f.** ___$60,000-$69,999
   - **g.** ___$70,000-$79,999
   - **h.** ___$80,000-$89,999
   - **i.** ___$90,000-$99,999
   - **j.** ___$100,000-$149,999

k. ___More than $150,000

5. Imagine you roll a fair, six -sided die 1,000 times. Out of 1,000 rolls, how many times do you think the die would come up as an even number? _________times

6. In the BIG BUCKS LOTTERY, the chances of winning a $10.00 prize are 1%. What is your best guess about how many people would win a $10.00 prize if 1,000 people each buy a single ticket from BIG BUCKS? _____people

7. If the chance of getting a disease is 20 out of 100, this would be the same as having a ____% chance of getting the disease.

8. In the ACME PUBLISHING SWEEPSTAKES, the chance of winning a car is 1 in 1,000. What percent of tickets of ACME PUBLISHING SWEEPSTAKES win a car? _____%

9. If the chance of getting a disease is 10%, how many people would be expected to get the disease out of 1000? ______people

10. A bat and a ball cost $11 in total. The bat costs $10 more than the ball. How much does the ball cost in dollars? $______

11. In a lake, there is a patch of lily pads. Every day, the patch doubles in size. If it takes 52 days for the patch to cover the entire lake, how long would it take for the patch to cover half of the lake in days? ______days

12. If it takes 10 machines 10 minutes to make 10 widgets, how long would it take 100 machines to make 100 widgets in minutes? ______minutes

APPENDIX D

LINEAR ROBABLITY MODELS

Table D.1. Linear Probability Model Estimation Results With Different Specifications

	Model					
Variable	1		2		3	
Partially incentivized – low	0.599***	(0.023)	0.178***	(0.027)	0.034	(0.037)
Partially incentivized – high	0.568***	(0.025)	0.196***	(0.028)	0.060	(0.036)
Fully incentivized	0.578***	(0.026)	0.270***	(0.029)	0.098**	(0.038)
Numeracy score						
1			0.077	(0.052)	0.259***	(0.056)
2			0.240***	(0.037)	0.260***	(0.036)
3			0.294***	(0.033)	0.249***	(0.032)
4			0.203***	(0.024)	0.128***	(0.025)
5			0.187***	(0.023)	0.106***	(0.025)
CRT score						
1			0.333***	(0.028)	0.328***	(0.029)
2			0.271***	(0.024)	0.298***	(0.024)
3			0.396***	(0.026)	0.374***	(0.027)
TVD					0.038***	(0.012)
Pupil dilation					0.026	(0.016)
Demographics						
Age					-0.002**	(0.001)
Female					0.110***	(0.027)
Hourly income					-0.001*	(0.001)
College Education					0.072	(0.068)
N	1572		1572		1572	
Log-likelihood	-1307.41		-1106.60		-1109.00	
AIC	2620.81		2235.20		2248.94	
BIC	2636.89		2294.16		2329.34	

Robust standard errors in parentheses. Significance at the 10%, 5%, and 1% is denoted by *, **, and ***.

Table D.2. Linear Probability Model Estimation Results for Different Payoffs

	Model			
Variable	Low		Large	
Partially incentivized – low	-0.028	(0.049)	0.105*	(0.047)
Partially incentivized – high	0.027	(0.049)	0.086	(0.048)
Fully incentivized	0.048	(0.051)	0.156***	(0.050)
Numeracy score				
1	0.266***	(0.072)	0.250***	(0.093)
2	0.263***	(0.046)	0.258***	(0.067)
3	0.238***	(0.042)	0.266***	(0.063)
4	0.147***	(0.033)	0.097**	(0.058)
5	0.087***	(0.033)	0.129***	(0.060)
CRT score				
1	0.333***	(0.039)	0.322***	(0.036)
2	0.295***	(0.032)	0.296***	(0.031)
3	0.372***	(0.036)	0.383***	(0.036)
TVD	0.044***	(0.016)	0.019	(0.010)
Pupil dilation	0.004	(0.029)	0.025	(0.027)
Demographics				
Age	-0.003**	(0.002)	-0.002	(0.002)
Female	0.091**	(0.036)	0.128***	(0.037)
Hourly income	-0.001	(0.001)	-0.002*	(0.001)
College Education	-0.100	(0.101)	-0.081	(0.119)
Constant	0.073	(0.166)	0.209	(0.179)
N	917		655	
Log-likelihood	-658.19		-432.98	
AIC	1348.38		897.96	
BIC	1425.52		969.72	

Robust standard errors in parentheses. Significance at the 10%, and 5% is denoted by *, and **.

APPENDIX E

COMPLETE SURVEY INSTRUMENT USED FOR STUDY IN CHAPTER III

1. **Please indicate your age in years: __________ years**

2. **Please indicate what gender you most identify with:**
    - **a.** ___ Female
    - **b.** ___ Male
    - **c.** ___ Other
    - **d.** ___ Prefer not to answer

3. **Please indicate your ethnicity (select all that apply):**
    - **a.** ___ Hispanic, Latino or Spanish origin
    - **b.** ___ White
    - **c.** ___ Black/African American
    - **d.** ___ American Indian or Alaskan Native
    - **e.** ___ Asian Indian
    - **f.** ___ Chinese
    - **g.** ___ Filipino
    - **h.** ___ Japanese
    - **i.** ___ Korean
    - **j.** ___ Vietnamese
    - **k.** ___ Native Hawaiian
    - **l.** ___ Gaumanian or Chamorro
    - **m.** ___ Samoan
    - **n.** ___ Other lease List: ______________________________________)

4. **Please indicate your family yearly income.** (Include all forms of income, including salary, interest and dividend payments, tips, scholarship support, student loans, parental support, social security, child support, and allowance).
    - **a.** ___Less than $20,000
    - **b.** ___$20,000-$29,999
    - **c.** ___$30,000-$39,999
    - **d.** ___$40,000-$49,999
    - **e.** ___$50,000-$59,999
    - **f.** ___$60,000-$69,999
    - **g.** ___$70,000-$79,999
    - **h.** ___$80,000-$89,999
    - **i.** ___$90,000-$99,999
    - **j.** ___$100,000-$149,999

k. ___More than $150,000

5. Imagine you roll a fair, six -sided die 1,000 times. Out of 1,000 rolls, how many times do you think the die would come up as an even number? _________times

6. In the BIG BUCKS LOTTERY, the chances of winning a $10.00 prize are 1%. What is your best guess about how many people would win a $10.00 prize if 1,000 people each buy a single ticket from BIG BUCKS? _____people

7. If the chance of getting a disease is 20 out of 100, this would be the same as having a ____% chance of getting the disease.

8. In the ACME PUBLISHING SWEEPSTAKES, the chance of winning a car is 1 in 1,000. What percent of tickets of ACME PUBLISHING SWEEPSTAKES win a car? _____%

9. If the chance of getting a disease is 10%, how many people would be expected to get the disease out of 1000? ______people

10. A bat and a ball cost $11 in total. The bat costs $10 more than the ball. How much does the ball cost in dollars? $______

11. In a lake, there is a patch of lily pads. Every day, the patch doubles in size. If it takes 52 days for the patch to cover the entire lake, how long would it take for the patch to cover half of the lake in days? ______days

12. If it takes 10 machines 10 minutes to make 10 widgets, how long would it take 100 machines to make 100 widgets in minutes? ______minutes

13. Listed below are some characteristics that may identify a person: Caring, compassionate, fair, friendly, generous, helpful, hardworking, honest, kind. The person with these characteristics could be you or someone else. For a moment,

visualize in your mind the kind of person that has these characteristics. Imagine, how that person would think, feel, and act. When you have a clear image of what this person would be like, move the sliders below to answer the questions.

- **a.** It would make me feel good to be a person who has these characteristics.
- **b.** Being someone who has these characteristics is an important part of who I am.
- **c.** I would be ashamed to be a person who has these characteristics.
- **d.** Having these characteristics is not really important to me.
- **e.** I strongly desire to have these characteristics.
- **f.** I often wear clothes that identify me as having these characteristics.
- **g.** The types of things I do in my spare time, e.g. hobbies, extra-curricular, clearly identify me as having these characteristics.
- **h.** The kinds of books and magazines I read identify me as having these characteristics.
- **i.** The fact that I have these characteristics is communicated to others by my membership in certain organizations.
- **j.** I am actively involved in activities that communicate to others that I have these characteristics.