Risky Choices over Goods

Patrick DeJarnette *

This Draft: April 1, 2017

Abstract

This paper examines how risk preferences differ over goods and in-kind monetary rewards. I study incentivized experiments in which subjects allocate bundles of either Amazon.com goods or Amazon.com gift credit (which must be spent immediately) across uncertain states. Under a standard model of perfect information of prices and goods available, I demonstrate risk preferences across these treatments would be identical. In practice, I uncover substantial differences in risk preferences across goods and in-kind monetary rewards. I examine whether these differences are driven by price or product uncertainty as in a search model, but find no evidence that this explains the differences. I further show that this is not being driven by fungibility, functional form, or good discreteness.

Keywords: risk preferences, goods, uncertainty.

*National Taiwan University. pdejar@ntu.edu.tw. I thank Jeremy Tobacman, Iwan Barankay, Daniel Gottlieb, Rob Jensen, and Judd Kessler for their guidance and feedback. This paper has also greatly benefited from the comments of Charles Sprenger and participants of the 2017 Asia Pacific Economic Science Association Conference. All errors are my own. The most recent version of this paper may be found at www.patdejar.net.

1 Introduction

Many important decisions involve risk, including insurance, portfolio choice, and labor relations. As a result, researchers have made great strides in understanding how decision makers perceive and value these risks. Yet the standard decision making models generally rely on a (concave) utility function over a single wealth variable. This often allows us to encapsulate risk preferences in a single parameter, which allows for comparisons across contexts and individuals.

While parsimonious, these models of risk aversion simplify a great deal of decision making. Recent evidence suggests that estimates of risk aversion may not be applicable across all domains. In Einav et al. [2012], demand for different types of insurance appears to be correlated, but does not correlate well with riskiness of the 401(k) investments.[1] In Barseghyan et al. [2011], demand for insurance (as measured by deductibles) were substantially different over two different goods, houses and cars.

One potential explanation for these differences in risk preferences is that individuals might treat pure monetary uncertainty (e.g. 401k or stocks) fundamentally different than good uncertainty (e.g. insurance for cars or houses). This paper sets out to test precisely this implication through an experiment with a real world market place, Amazon.com.[2] Subjects choose either Amazon.com credit amounts ($5, $10, etc.) or Amazon.com goods (books, clothing, etc.) that total up to either $20 or $100. They allocate these credits or goods across uncertain states that have equal probabilities of occurring. To remove temporal concerns, all credit awarded is spent immediately after the uncertainty is resolved.[3]

If individuals treat self-selected goods and time-allocated money identically without uncertainty, then with relatively weak assumptions, there should be no difference between the allocated distributions (with uncertainty). As a simple example, if given $20 of credit and an individual allocates $10 to each of the two states, they will receive $10 of credit for sure. This $10 could be used to purchase anything on Amazon.com under or up to $10. Therefore, it might be suprising that when asked to choose good(s) whose prices are at most $20, individuals often no longer choose two goods under $10, but may instead choose a $15 good and a $5 good. If this was indeed the optimal allocation of goods, it may seem strange that the subject did not choose a $15 credit and $5 credit allocation instead. The theory explains why

Contrary to this prediction, subjects exhibited considerably more risk aversion when selecting credit. Subjects were four times as likely to place "equal" quantities with credit than they were with goods. Furthermore, the mean standard deviation of credit allocations was about two-thirds that of the mean standard deviation of good prices. To analyze whether these differences could be driven by price uncertainty, subjects are randomly forced to spend more time on Amazon.com, but this does not seem to influence the allocations (with a rather precise zero effect).

Although this is the first research to explicitly test this uncertainty equivalence, an earlier theoretical literature

[1] It may be worth noting that Einav et al. [2012] focuses on how individuals rank relative to their peers. As a result, they are less focused on testing "absolute" differences between risk preferences over different goods and instead at the reliability of how individual will rank relative to others.

[2] A wide variety of goods is available on Amazon.com, making this an ideal environment to study evidence of real risk preferences over goods.

[3] Subjects were quizzed on this and several other topics to ensure understanding. See design section for more details.

uncovered several implicit assumptions about uncertainty over goods and money. Grant et al. [1992] tackles this by assuming preferences over monetary lotteries are induced by underlying preferences over goods lotteries.[4] The paper then goes on to establish implications for what risk aversion over monetary lotteries implies about risk aversion over good lotteries. However, rather than assuming preferences over monetary lotteries are induced by underlying preferences for good lotteries, I outline precisely what assumptions will generate indifference between a monetary lottery and equivalent value (self-selected) good lotteries.

This study is not unique in its interest in how consumers may treat money and goods differently. An extensive literature on the endowment effect indicates that subjects, after receiving a good, value that good more than subjects who do not (c.f. Knetsch [1989], Kahneman et al. [1991], Bordalo et al. [2012]). There is also a growing literature on salience and its impact on utiltiy over goods. While research has recently explored the potential for salience in monetary lotteries as in Bordalo et al. [2010] or for goods under certainty (Bordalo et al. [2012], Kőszegi and Szeidl [2013], Gabaix [2014]). This study therefore might contribute important empirical evidence on how individuals aggregate preferences over salient goods to create a limited-rationality utility framework over risk.

Consumers also face decisions daily about whether to purchase products running promotional contests (Dhar and Simonson [1992]). These contests pose somewhat of a mystery, given that they often feature prizes rather than equivalent cash values. In practice, these prizes may be sold at reduced costs to the promoter, but this study also indicates another possibility – individuals may wish to engage in risk over goods but prefer to avoid risk with equivalent cash prizes.This may have important implications for government run lotteries, which often serve to fund public programs. By adding physical items to these lotteries, it may be possible to encourage risk seeking behavior from participants and generate additional revenues for publicly funded programs.

The remainder of the paper is organized as follows. Section 2 demonstrates theoretical predictions. Section 3 outlines the experiment design. Section 4 presents the results and Section 5 concludes.

2 Theory

In this section, I demonstrate that under perfect information of goods available and prices, risk preferences across money and goods should be the same in a static model.

Each state $s = 1, 2, ..., S$ occurs with a probability γ_s. There are goods $n = 1, 2, ...N$ which can be consumed in each state, $g_{n,s}$ an element of the good-specific set $G_n \subset \mathbb{R}_+$, as well as a monetary good for each state, $m_s \in \mathbb{R}_+$. Thus, any particular lottery L is defined by the vector $(\gamma_1, m_1, g_{1,1}, g_{2,1}, ..., g_{N,1}; \gamma_2, m_2, g_{1,2}, g_{2,2}, ..., g_{N,2}; ...; \gamma_S, m_S, g_{1,S}, g_{2,S}, ..., g_{N,S})$, an element of $[0, 1] \times \mathbb{R}_+ \times G_1 \times G_2 \times ... \times G_N \times [0, 1] \times ... \times G_N$. For simplicity, I will call this vector space $\mathbb{L}_{\mathbb{S}}$ where S refers to the set of states.

This can also be written as the combination of degenerate lotteries L_s, where $L_s \equiv (1, m_s, g_{1,s}, g_{2,s}, ..., g_{N,s}) \in \mathbb{L}_1$. Thus, for any lottery L, for shorthand we may write it as $L = (\gamma_1 L_1,\ \gamma_2 L_2,\ ...,\ \gamma_S L_S)$ where $\gamma_s L_s$ refers to $(\gamma_s, m_s, g_{1,s}, g_{2,s}, ..., g_{N,s})$. To make assumptions of state independence more plausible,[5] I also assume that

[4]To the credit of Grant et al. [1992], they acknowledge alternate approaches in footnote 7, even though it was not the main focus of that study.

[5]If states were not mutually exclusive (with exactly one state occuring), it would be hard to believe that properties of other states

$\sum \gamma_s = 1$.

In addition, for any given state s define the market state as a vector of prices $P_s = (p_{1,s}, p_{2,s}, ..., p_{n,s})$. The market consists of a vector that consists of the individual market states $P = (P_1, P_2, ...P_S)$.[6] The agent has preferences relation $\gtrsim_P$ over lotteries $\mathbb{L}_{\mathbb{S}}$ for a given market P.[7] For notational simplicity, if there are lotteries $A, B \in \mathbb{L}_R$ with $R < S$, I write $A \gtrsim_P B$ as a shorthand for $(A, \vec{0}) \gtrsim_P (B, \vec{0})$ where $\vec{0} \in \mathbb{L}_{S-R}$. In words, even though preferences are over the entire S states, I pad out the remaining states with zeroes to use the same preference relation.

In addition to the basic relation assumptions, I assume the preferences have two additional properties: (a) Monetary Equivalence Under Certainty and (b) Independence.

Monetary Equivalence Under Certainty.

(i) For degenerate lottery $L_s = (1, m_s, g_{1,s}, g_{2,s}, ..., g_{N,s})$, the agent weakly prefers the bundle $L_s^{'} = (1,\ m_s + \sum_n p_{n,s} g_{n,s}, 0, 0, ..., 0)$, that is $L_s^{'} \gtrsim_P L_s$.

(ii) For any degenerate lottery $L_s = (1, m_s, g_{1,s}, g_{2,s}, ..., g_{N,s})$, there exists a degenerate lottery $L_s^{''} = (1, 0, g_{1,s}^{''}, g_{2,s}^{''}, ..., g_{N,s}^{''})$ such that $\sum_n p_{n,s} g_{n,s}^{''} \leq m_s + \sum_n p_{n,s} g_{n,s}$ and $L_s^{''} \succsim_P L_s$.

In words, Monetary Equivalence (i) states that in a case with no uncertainty, the agent is at least as happy off with converting any particular bundle into the money it would cost to purchase that bundle. Since this is true for all degenerate lotteries, including optimal bundles, it also implies that there are no transaction costs to converting money into goods.

Monetary Equivalence (ii) states that in a case with no uncertainty, the agent has no particular preference for holding onto money. In other words, money is only as useful as the things it can buy.[8] It is also worth noting that this does not mean that every dollar must get spent in an optimal bundle. For example, if goods are discrete rather than continuous, it may not be optimal to spend every last dollar. However, what this assumption indicates is that any money left over after purchasing the optimal goods bundle would have no value (as they would be indifferent between that and the same goods bundle with no money).[9]

Independence Property. For any lotteries L and $L^{'}$ in $\mathbb{L}_R$, preferences are independent if $L \succsim_P L^{'}$ implies $\forall \alpha \in (0, 1)$ and for all degenerate lotteries $L^{''}$, $(\alpha L,\ (1-\alpha)L^{''}) \succsim_P (\alpha L^{'},\ (1-\alpha)L^{''})$.

would not cause preference reversals. For example, one might prefer a 100% chance of a chocolate to a 100% chance of a piece of marshmallow. But if we had a 100% chance of marshmallow with an additional 50% chance of chocolate, this may now be preferred to a 100% chance of a chocolate with an additional 50% chance of additional chocolate.

[6] This assumption of linear pricing is for ease of notational simplicity and could be instead considered as a vector function.

[7] In this case, because the monetary good is allowed to enter directly into the bundle, market prices may influence preferences over bundles.

[8] Although this model is being presented as a static one, the same item at different periods could be thought of as different goods – as long as the uncertainty is resolved in one period with discrete and finite time periods, the same results hold true intertemporally.

[9] For example, let's say I am buying discrete apples and bananas. If apples are \$2 and bananas are \$3 and I have \$7 to spend, I may indeed prefer 2 bananas even though I have \$1 left over. But according to Monetary Equivalence (ii), I am indifferent between \$0 and 2 bananas and \$1 and 2 bananas.

With monetary equivalence under uncertainty and the independence property, we can establish the following: Let $G_L = (g_{1,1}, g_{2,1}, ..., g_{N,1}, g_{1,2}, ..., g_{N,2}, ..., g_{1,S}, g_{2,S}, ...g_{N,S}) \in G_1 \times G_2 \times ... \times G_N \times G_1 \times ... \times G_N$ denote the vector of goods for a given lottery L in which all monetary values are 0. Let $G(P, I) = \{G_L\ s.t.\ \sum_s \sum_n p_s g_{n,s} \leq I\}$, with $sup\, G(P, I)$ defined using the partial ordering $\succsim_P$ in which all monetary values are set to 0. By a similar notation, let $M_L = (m_1, m_2, ..., m_S) \in \mathbb{R}_S$ denote the vector of monetary values for a lottery L in which all non-monetary goods are 0. And $M(P, I) = \{M_L\ s.t.\ \sum_s m_s \leq I\}$, with $sup\, M(P, I)$ defined using the partial ordering $\succsim_P$ in which all non-monetary goods are set to 0.

Theorem 2.1 (Monetary Equivalence Over Uncertainty)**.** *Under the assumptions of Monetary Equivalence Under Certainty and Independence, if a lottery of goods is optimal, then the monetary lottery (with equivalent value in each state) will also be optimal. Mathematically, if* $G^* = (g_{1,1}, g_{2,1}, ..., g_{N,1}, g_{1,2}, ..., g_{N,2}, ..., g_{1,S}, g_{2,S}, ...g_{N,S}) \in sup\, G(P, I)$*, then* $M^* = (p_{11}g_{11} + p_{21}g_{21} + ... + p_{N1}g_{N1}, p_{12}g_{12} + p_{22}g_{22} + ... + p_{N2}g_{N2}, ..., p_{1S}g_{1S} + p_{2S}g_{2S} + ... + p_{NS}g_{NS}) \in sup\, M(P, I)$.

Proof. For proof by contradiction, assume that the condition is true, that $G^* \in sup\, G(P)$ but that, as defined above, $M^* \notin sup\, M(P, I)$. Note that G^* can be rewritten as the combination of degenerate lotteries $G^* = \gamma_1 L_1^* + \gamma_2 L_2^* + ... + (1 - \sum \gamma_s) L_S^*$ where $L_s^* = (1, 0, g_{1,s}^*, g_{2,s}^*, ..., g_{N,s}^*)$. Individually, each of these degenerate lotteries is weakly dominated by the degenerate lottery $L_s^{'} = (1, p_{1,s}g_{1,s}^* + p_{2,s}g_{2,s}^* + ... + p_{N,s}g_{N,s}^*, 0, ..., 0)$ via Monetary Equivalence under Certainty. By multiple applications of the independence assumption, this means that $(\gamma_1 L_1^*,\ \gamma_2 L_2^*, ..., \gamma_S L_S^*) \precsim_P (\gamma_1 L_1^{'},\ \gamma_2 L_2^{'}, ..., \gamma_S L_S^{'})$ but note that this compound lottery corresponds precisely to M^*.

However as $M^* \notin sup\, M(P, I)$ but $M^* \in M(P, I)$, that implies there is some $M^{**} \in M(P, I)$ with $M^{**} \succ_P M^*$. We can rewrite this lottery as a combination of degenerate lotteries

$(\gamma_1, m_1^{**}, 0, ..., 0; \gamma_2, m_2^{**}, 0, .., 0; ...; \gamma_S, m_S^{**}, 0, ..., 0) = (\gamma_1 L_1^{**},\ \gamma_2 L_2^{**},\ ...,\ \gamma_S L_S^{**})$. However, for each of these degenerate lotteries, L_s^{**} the Monetary Equivalence Under Certainty property (ii) states that there exists a degenerate lottery $L_s^{''} = (1, 0, g_{1,s}^{**}, g_{2,s}^{**}, ..., g_{N,s}^{**})$ such that $L_s^{''} \succsim_P L_s^{**}$. Repeated application of the Independence property gives us $G^{''} \equiv (\gamma_1 L_1^{''},\ \gamma_2 L_2^{''},\ ...,\ \gamma_S L_S^{''}) \succsim_P (\gamma_1 L_1^{''},\ \gamma_2 L_2^{''},\ ...,\ \gamma_S L_S^{''})$. Thus $G^{''} \succsim_P M^{**} \succ_P M^* \succsim_P G^*$. This is a contradiction, however, as G^* was the supremum of $G(P, I)$ and now there is a new lottery $G^{''}$ in $G(P, I)$ which strictly dominates it. □

2.1 Discussion

The assumptions that drive the theory in this case warrant additional discussion. First, if the preference relation is a weak order, that implies that the agent has both transitive and complete preferences. Transitivity of preferences over risk has been discussed as early as Tversky [1969] but more recent empirical evidence suggests that preferences can largely be summarized as transitive (c.f. Birnbaum and Gutierrez [2007], Birnbaum and Schmidt [2010], Regenwetter et al. [2011]).[10] Completeness of preferences is harder to test, as indecision between

[10]However, this is an ongoing field of research. It is also possible that the research may not apply to the lotteries employed, being arguably more intricate than previous lotteries studied. Yet intransitive preferences would also make choosing a bundle more

two lotteries might be interpreted as indifference. This is especially difficult given the great number of goods available on Amazon.com.

Regarding Monetary Equivalence under Certainty, part (i) states that the agent would be at least as well off with an equivalent amount of money as a goods bundle would cost. However, if agents are somewhat unaware of the goods available or the prices of the goods, this may not be the case. The agent might not remember that a good is available to purchase, or the agent might have incorrect beliefs about what the prices, influencing money is capable of purchasing. Indeed, this may be a big component for why individuals seem to treat money and goods differently. To minimize these concerns, Amazon.com was employed for the study, which as a real market should mimic the trade off between money and goods.[11] Lastly, an additional information treatment was employed to test this theory as is described in Section 3.

Monetary Equivalence under Certainty part (ii) states that money holds no inherent value above and beyond what can be purchased with it. In other words, with a given amount of money, the agent can always find a bundle that makes them at least as happy. Yet this assumption makes no mention of the psychic costs that may be associated with finding the bundle in question. In addition, this assumption may be true in our static model and the (static) experiment, but intertemporally agents may want to hold on to some of their money as future prices are not perfectly known.[12]

The Independence property is similar to the Independence property assumed for von Neumann-Morgenstern utility functions. In that set up, lotteries are probability distributions over fixed outcomes. If all goods are discrete, then as the possible lotteries are bounded by the endowment income, then the lottery structure in section 2 could be rewritten under that framework,[13] and the Independence property would be identical.

However the Independence property has been criticized as potentially too strong an assumption. In particular, the famous Allais 'paradox' in which the chance of another lottery may cause preference reversals. Yet the relative importance and frequency of these non-independent lotteries for decision making is an ongoing debate (c.f.Rubinstein [1988], Allais and Hagen [2013]).

3 Experiment Design Overview

In order to test this theory, I conducted an incentivized experiment with 124 undergraduate students at the Wharton Behavioral Lab in March 2016. During this experiment, the subjects selected goods on Amazon.com over uncertain states. Subjects sit at the computer and are informed about the upcoming uncertainty. Depending on their treatment, they select either Amazon.com credit (monetary allocations) or Amazon.com goods over several

difficult in this setting. In previous studies, the outcomes in particular states were largely fixed, whereas in this case the outcomes are subject-determined.

[11] In addition, subjects were allowed to sign in to Amazon if they preferred, to view existing wishlists.

[12] If future prices were perfectly known, then a good in different time periods could enter the model as different goods. However in addition to price uncertainty, there may be quantity uncertainty, e.g. car stolen, that might make Monetary Equivalence under Certainty (ii) unlikely to be true over time.

[13] If all goods are discrete and bounded by the endowment, then with a finite number of goods and states, there would only be a finite number of possible bundles. As a result, one could rewrite every possible goods bundle in every different state as a different (fixed) outcome. Doing so would make the intuition behind Monetary Equivalence under Certainty hard to understand, and the assumption of discrete goods is unnecessary to the proof I outline.

possible states. A wide variety of goods are available on Amazon.com,[14] making this an ideal environment for measuring risk preferences over goods and money.

The static decision is a 2x2x2 design, with agents allocating either {credit or goods} worth a total of {$20 or $100} and is {required or not required} to spend an extra 5 minutes browsing Amazon.com. Subjects perform this procedure twice (two rounds).[15] When given a total of $20, only 2 (equal probability) states can occur, but to keep the average payout the same, 10 (equal probability) states can occur when given a total of $100.

For example, they might be given $20 of Amazon.com credit to allocate over two states, A or B, each of which occurs with 50% probability. In this case, a typical "risk averse" decision would be allocate $10 of credit for State A and $10 of credit for State B, thus ensuring that regardless of which state occurs, $10 of Amazon.com credit will be selected. They are then required to spend any credit rewarded.

Alternatively, the subject might be given $20 of Amazon.com credit, but rather than asked to allocate the credit, the subject selects Amazon.com goods whose prices add up to at most $20. In other words, the subject determines what to "spend" the credit on goods before the uncertainty is resolved.

Important to this interpretation is the intertemporal fungibility of the Amazon.com credit. Amazon.com goods are purchased at one point in time. Thus, it is important to limit Amazon.com credit to a similar (static) time period. To test the theory outlined above, subjects were informed and quizzed that no matter what credit amount is selected, a single item would be selected at the end of the session whose price is less than or equal to the amount of credit. [16]

To remove concerns about "shrouded attributes" (c.f. Gabaix et al. [2006], Chetty et al. [2009], Brown et al. [2010]), only the list price of the good is considered. Subjects are informed and quizzed in both cases that only the list price will count toward the total, not shipping. In addition, for any URL entered, the browser instantaneously used the Amazon Affiliate API to calculate the price of the item. At the same time, a "total counter" at the bottom of the page informed subjects about the remaining credit available. Combined, these measures aim to limit any "price uncertainty" to prices of unsearched items, rather than the prices of items already selected or searched. For example, I may not know the precise prices of oven mitts, but once I find a particular oven mitt on Amazon.com, all price uncertainty of that specific oven mitt should be gone. Without taxing or shipping concerns, there are no further mental calculations required.[17]

A sample of subjects were randomly selected to spend an extra 5 minutes on Amazon.com to help understand the potential for price or product uncertainty driving potential results. This will be discussed in more detail in Section 4.

Prior to being allowed to start each period, the subjects had to correctly answer questions about the upcoming period, as seen in Appendix Figures 1 and 2. These procedures were implemented to ensure subjects fully understood the incentives they faced. To remove any subject overlap, the computer cookies and browsing history were also cleared in between sessions.

[14]As of 2015, it is estimated that there are between 300 and 400 million unique items sold on Amazon.com.

[15]Every agent receives both $20 or $100 treatments, but it is randomized which occurs first.

[16]2 subjects selected physical Amazon.com gift cards using either used their Amazon.com credit or Amazon.com items. This was not explicitly discouraged, as an individual willing to do this has made both the Amazon.com goods and Amazon.com credit fungible. However, dropping these individuals makes no difference to the qualitative results or significance.

[17]To further simplify things, agents are informed that only the "default" seller price matters. This is primarily Amazon.com itself.

4 Experiment Results

The first question is whether the primary treatment of selecting Amazon.com goods (rather than credit) impacted the distribution of good value. Recall that under the assumptions of section 2, there should be no difference between the good distributions and the monetary distributions. For example, if the agent preferred a 10% chance of a $100 item to a sure thing of a $10 item, then when selecting monetary distributions, they should have also preferred a 10% chance of $100 credit to a sure thing of $10 credit. As the credit needed to be spent immediately after awarded, there are no intertemporal savings, so any difference in distribution over the uncertain states would indicate one of the assumptions was not satisfied.

Result 4.1. *Contrary to the equivalence theory presented, subjects exhibited greater risk aversion when selecting credit amounts than they did when selecting goods. When selecting goods, subjects were also significantly less likely to select goods of the same value.*

There are several ways to analyze these differences in distribution. I provide results using multiple methods, including regressions of the standard deviation, tests of allocating risk equally across all states, and nonparametric methods to test differences in distribution. All of these methods support the conclusion that subjects selecting credit allocations were more likely to spread out the total amount over multiple states, while subjects selecting goods chose more risky allocations (measured with the price of the items).

Specifically, when investigating the standard deviation of values selected across the possible states, distributing credit meant that agents reduced the standard deviation of the distribution by a third. As seen in Table 3A, OLS estimates suggest the standard deviation of prices were significantly reduced by -2.66 to -2.43 down from a mean of 7.61. However, further analysis of the allocations suggests that not only the standard deviation, but also the mean differs between credit and goods treatments. This may be because goods on Amazon.com are discrete – it is likely difficult or suboptimal to spend precisely $20 (or $100).[18] As a result of this discreteness, one might also want to investigate the standard deviation after normalizing values by the total amount allocated. However, the results in Table 3B are nearly identical, with one-third of the standard deviation decreasing when selecting credit.[19]

In the experiment, 16% of subjects removed all risk by allocating a uniform distribution (equal values across all of the uncertain states). As seen in Table 4, this risk-less distribution were over 4 times as likely to occur when the subjects were selecting credit than when they were selecting goods ($p < 0.01$). Note that subjects were instructed and quizzed that they could place the same the same good in multiple slots, thus increasing the chance of having it selected (see Appendix Figure 2). Despite this quizzing, there were only 8 cases where a subject selected a uniform distribution of good prices, indicating a greater tolerance for risk.

In addition to these regression results, one can also non-parametrically analyze the distribution of values. However, these tests assume independence among observations, so it is not possible to simply use every individual allocation datapoint. Instead, each allocation is transformed into a single variable that can then be

[18]Indeed, the average allocation is $9.32 instead of the full $10. Selecting credit instead of the goods increases this average by about $0.50 (more details in Appendix Table 1).

[19]It is worth noting that the $100 treatment effect changes sign. This is likely because with more money to spend the potential standard deviation of allocations can increase; but once normalized to fractions, this effect goes away.

non-parametrically tested across the two primary treatments (goods and credit). The \$20 treatment is a good starting point for this, as most of the information of an allocation can be summarized in a single number, specifically "What is the price of the lower-priced good?" These distributions across individuals are plotted in Figure 2A, and the associated Kolmogorov-Smirnov test rejects equality of the distributions ($p < 0.01$). Figure 2B plots distributions of a similar nature, that is the normalized price of the lower-priced good (in other words, what fraction of the total spent is on the lower-priced good). Kolmogorov-Smirnov suggests borderline significant rejection for equality of the distributions of this transformation ($p < 0.07$).

However, though widely used, the Kolmogorov-Smirnov test uses the largest difference between the distributions. As a result, it tends to underweight differences in the tails of the cumulative distributions – Mason and Schuenemeyer [1983], Kim and Whitt [2015]. Given the large share of subjects who place \$10 and \$10 when using credit, the Kolmogorov-Smirnov test may not be the most efficient. Alternatively, we can also use more information from the distributions, such as a Kolmogorov-Smirnov test of the within-allocation standard deviation. In these cases, the Kolmogorov-Smirnov rejects equality of distribution both when using the distributions of standard deviations ($p < 0.01$) or the distributions of normalized standard deviations ($p < 0.01$).

Result 4.2. *When forced to spend more time searching Amazon.com, subjects did not significiantly alter the distribution allocations of goods and credit.*

To test the possibility that the difference in risk for the good domain is being driven by product or price uncertainty, some subjects were randomly submitted to an information treatment. In this treatment, subjects were forced to wait an extra 5 minutes before they could submit their allocations. During this time, subjects were only allowed to visit Amazon.com or sit quietly at the desk.[20] The intent was to lower the marginal cost of searching. It appears this treatment was indeed successful in inducing subjects to spend more time in a section – the average treatment effect was to spend an extra 8 minutes (3 minutes beyond the 5 minutes imposed). This extra time spent searching could be the result of product search being unexpectedly fun or that the the 5 minute timer was not visible while browsing Amazon, causing subjects to run over.

As we can see in the OLS regressions in Table 5A, the treatment information had no significant direct impact on allocation distributions (as measured by the standard distribution). If the information treatment's effect on allocation would be through the time spent searching, we can also use the information treatment as an instrumental variable for time spent in a section. This allows a causal impact of time spent searching on the distribution allocations. Table 5B presents results of this instrumental variable regression, but once again, spending more time searching has no significant impact on the standard deviation of the allocation.

5 Conclusion

Contrary to the equivalence theory money and goods under uncertainty, subjects exhibited reduced risk taking with selecting credit amounts than they did when selecting goods. When selecting goods, subjects were also significantly less likely to select goods of the same value across the uncertain states. These findings alone

[20] This website restriction, as well as a no cellphone rule, was enforced by lab assistants monitoring the study.

might indicate a general uncertainty of Amazon.com goods or prices, but forcing subjects to spend more time investigating Amazon.com does not change these differences.

As a result, one of the remaining assumptions of the equivalence theory must be false to result in this behavior. One possibility is the an endowment effect, which allows for ownership of the item to increase the owner's willingness-to-accept (Knetsch [1989], Kahneman et al. [1991]). In this experiment, if committing to a good allocation triggers a similar endowment effect, then a post-committed good allocation might be worth more than an equivalent price bundle. In order for this to cause greater risk taking with goods, there must be convexities in the endowment effect; otherwise good bundles would not necessarily be more risky.

Another possibility is a model of thinking aversion presented in Ortoleva [2013]. Although the information treatment resulted in subjects spending more time searching, it may be that they still dislike the large choice set. As a result, they may not decide on a particular allocation of goods until they have no other choice. This could result in less risk taking in credit relative to goods.

Lastly, it may be that credit, being easier to compare, may result in more "regret aversion" as described in Loomes and Sugden [1982]. If the prices of goods or inherent value of goods makes comparisons more difficult, it may be the case that the agent will experience less regret if the "best" outcome does not happen. As a result, they may be more willing to engage in riskier behavior over goods than the more easily comparable money outcomes.

It may also be the case that by reducing the choice set, the difference between money and goods risk taking could decrease. For example, if subjects were only able to choose between two goods for each uncertain state, one might expect a convergence of money and good risk taking. But in the real world, individuals face many possible uses for their money.

While these are interesting possibilities and warrant further study, this does not change the primary finding of this paper. In other words, whether convex endowment effects or thinking aversion is driving the difference in risk taking, it remains that individuals react differentially to risk over goods and risk over money.

This finding has important implications for public policy. In 2014 U.S. government sponsored lotteries raised $70 billion in revenues, helping fund state governments and programs. This paper suggests that individuals may be more willing to engage in lotteries that have goods, not just money. Indeed, anecdotally U.S. companies often run sweepstakes with prizes (cars, cruises, etc.) rather than a pure lottery.[21] For example, a prize of $1 million with a car worth $50,000 may cause more engagement in risk than a lottery with $1,050,000 million. Although the total ramifications of state-run lotteries are debatable, this greater willingness in risk could be used to reduce advertisement and overhead budgets without changing revenues.

[21] Some of this is likely driven by reduced costs of prizes, which may be seen as a marketing cost. However, this reduced cost may also be achieved for a potential state run sweepstakes.

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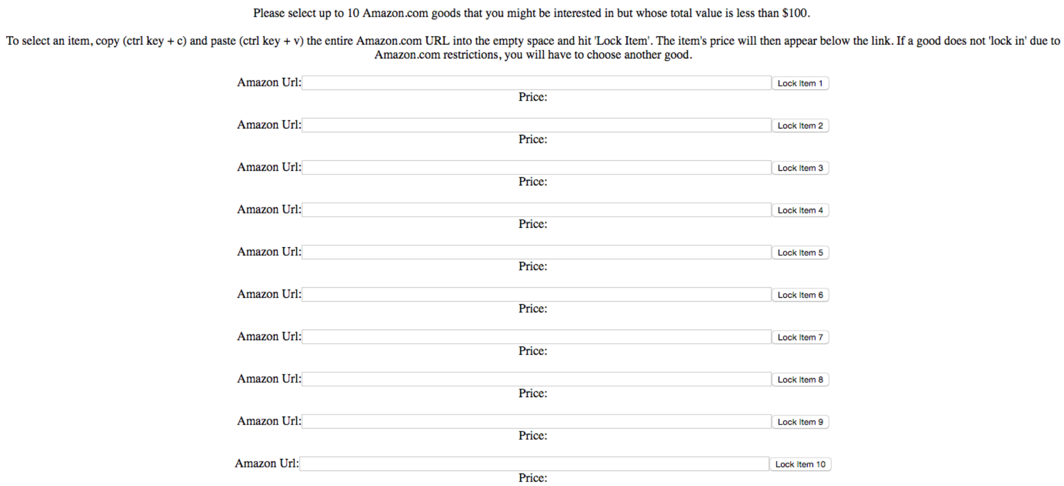
6 Figures

Figure 1: Example of Good Selection



Please select up to 10 Amazon.com goods that you might be interested in but whose total value is less than $100.

To select an item, copy (ctrl key + c) and paste (ctrl key + v) the entire Amazon.com URL into the empty space and hit 'Lock Item'. The item's price will then appear below the link. If a good does not 'lock in' due to Amazon.com restrictions, you will have to choose another good.

Amazon Url: Lock Item 1
Price:

Amazon Url: Lock Item 2
Price:

Amazon Url: Lock Item 3
Price:

Amazon Url: Lock Item 4
Price:

Amazon Url: Lock Item 5
Price:

Amazon Url: Lock Item 6
Price:

Amazon Url: Lock Item 7
Price:

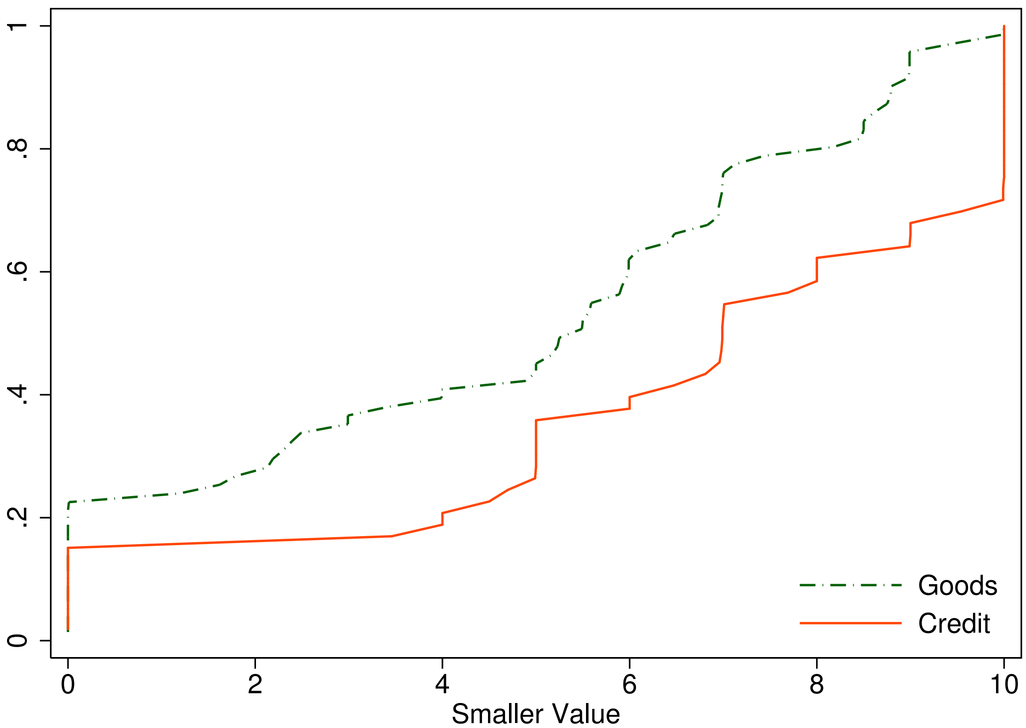
Amazon Url: Lock Item 8
Price:

Amazon Url: Lock Item 9
Price:

Amazon Url: Lock Item 10
Price:

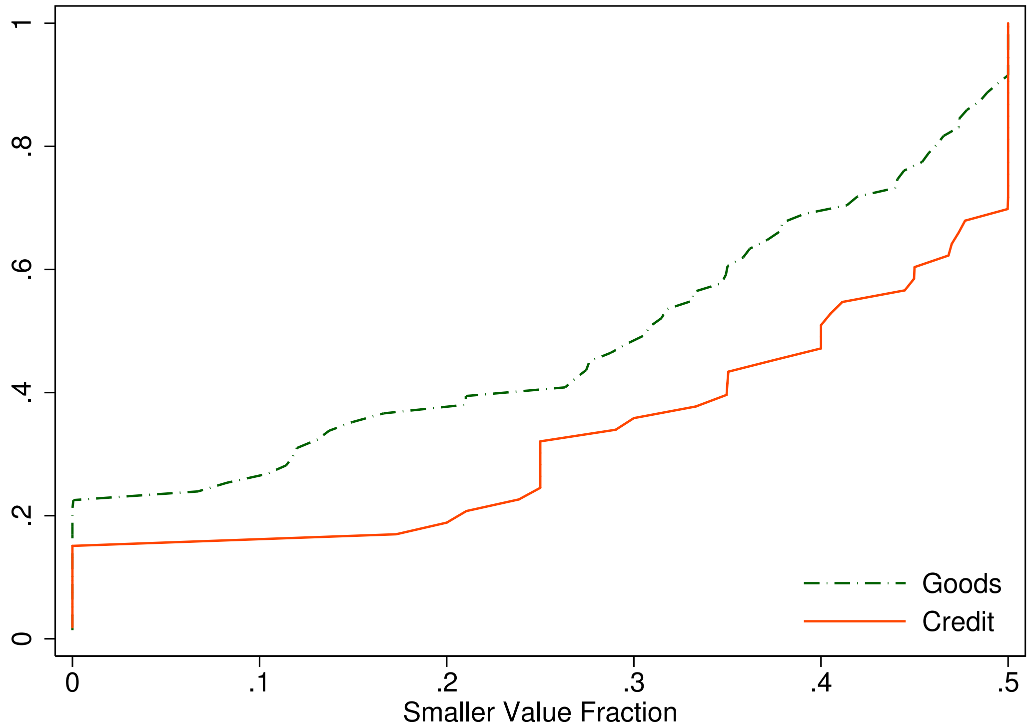
Notes: Figure demonstrates a typical good selection screen faced by subject. Whether subject was asked to select Amazon.com goods (via URLs) or Amazon.com credit amounts was randomized. Whether subject was asked to find up to 10 items that totaled at most $100 or up to 2 items that totaled at most $20 was also randomized. See Experiment Design for more details.

Figure 2A: Distribution of the Smaller Value When Total is $20



Notes: Plot shows two cumulative distributions of the value of the smaller good when total is $20. When given $20 to allocate, the agent chooses to allocate across 2 uncertain states, this represents the smaller of these two allocations. Vertical axis represents the frequency of that value occurring across the two different treatments (selecting goods or selecting credit allocations).

Figure 2B: Distribution of the Smaller Value (Normalized) When Total is $20



Notes: Plot shows two cumulative distributions of the (normalized) value of the smaller good when total is $20. Value is normalized by dividing by the total value allocated. When given $20 to allocate, the agent chooses to allocate across 2 uncertain states, this represents the smaller of these two allocations. Vertical axis represents the frequency of that (normalized) value occurring across the two different treatments (selecting goods or selecting credit allocations).

7 Tables

Table 1. Summary Statistics

	Mean	Standard dev	Min	Max	
Individual Level Variables					
Female	0.71	0.45	0	1	
Age	20.2	1.3	18	24	
SAT Math Score	733	62	540	800	(22 missing)
Computer Skill Test	2	0	2	2	(1 missing)
Number of Previous Lab Studies	26.6	24.8	1	133	
Period Level Variables					
Average Value of Entry	$9.33	1.07	4	10	
Standard Dev of Entry (within)	$7.06	6.92	0	31.6	
$100 Treatment Indicator	0.50	0.50	0	1	
Credit Treatment Indicator	0.46	0.50	0	1	
Time Spent Searching (seconds)	487	373	45	1699	
Number of Individuals	124				
Number of Treatment Periods	248				

Notes: Computer Skill Test was a demographic variable collected by the Wharton Behavioral Lab prior to the experiment, however among subjects above it had no variation. SAT Math score is missing for individuals who either took the ACT or otherwise did not wish to share that information with researchers.

Table 2. Randomization Check

Dependent Variable	Credit Treatment		Period # for $100 Treatment	
Female	−0.11	−0.01	−0.02	−0.01
	(0.07)	(0.11)	(0.10)	(0.11)
SAT Math Score ('00s of points)		0.01		0.01
		(0.08)		(0.09)
Previous WBL Studies		−0.001		−0.001
		(0.002)		(0.002)
F-test	2.59	0.24	0.05	0.24
p value	0.11	0.87	0.83	0.87
Dependent Variable Mean	0.46	0.48	1.54	1.54
Number of Observations	248	204	248	204
Number of Individuals	124	102	124	102

Notes: Standard Errors (clustered at individual level) presented in parentheses above. As every subject in experiment 1 receives both the $20 and $100 treatments, the dependent variable for $100 treatment is the period in which they received the treatment in question. If randomization was done properly, the pre-treatment variables should not predict the period they received this treatment. Indeed, the F-stats are all large enough that I fail to reject the hypothesis that all coefficients are zero under $\alpha = 0.05$. Thus, I conclude the randomization was adequately done. SAT Math score is missing for 22 individuals who either took the ACT or otherwise did not wish to share that information with researchers.

Table 3A. Credit and $100: Impact on Standard Deviation of Selection Value

$$Std.Dev_{i,t} \quad = \quad \alpha \cdot Credit_{i,t} + \beta \cdot 100Treatment_{i,t} + \gamma X_i + \epsilon_{i,t}$$

Dependent Variable: Value Standard Deviation	Specification (1)	(2)	(3)	(4)
Subject Selects Credit	-2.66***	-2.61***	-2.56***	-2.43***
(Binary Treatment Var.)	(0.77)	(0.77)	(0.81)	(0.82)
$100 Total Allocation	4.96***	5.00***	5.00***	4.99***
(Binary Treatment Var.)	(0.64)	(0.63)	(0.64)	(0.64)
First Period		0.59	0.59	0.61
		(0.64)	(0.66)	(0.67)
Session Fixed Effects			X	X
Individual Controls				X
Dependent Variable Mean	7.61	7.61	7.61	7.61
Number of Observations	248	248	248	248
Number of Individuals	124	124	124	124
Adj-R^2	0.16	0.16	0.19	0.22

Notes: The dependent variable is the standard deviation of value of the entries in a single period. All specifications report results from OLS regressions and also include a constant term. Individual Controls include sex, age, ethnicity bins, and number of sessions done. Standard errors are given in parentheses and clustered at the subject (individual) level. $* = p < 0.1$, $** = p < 0.05$, $*** = p < 0.01$.

Table 3B. Credit and \$100: Impact on Standard Deviation of Selection Value (Normalized)

$$Norm.Std.Dev_{i,t} = \alpha \cdot Credit_{i,t} + \beta \cdot 100Treatment_{i,t} + \gamma X_i + \epsilon_{i,t}$$

Dependent Variable:		Specification		
Normalized Value Std Dev	(1)	(2)	(3)	(4)
Subject Selects Credit	−0.08***	−0.07***	−0.07***	−0.07***
(Binary Treatment Var.)	(0.02)	(0.02)	(0.02)	(0.02)
\$100 Total Allocation	−0.17***	−0.17***	−0.17***	−0.17***
(Binary Treatment Var.)	(0.02)	(0.02)	(0.02)	(0.02)
First Period		0.01	0.01	0.01
		(0.02)	(0.02)	(0.02)
Session Fixed Effects			X	X
Individual Controls				X
Dependent Variable Mean	0.20	0.20	0.20	0.20
Number of Observations	248	248	248	248
Number of Individuals	124	124	124	124
Adj-R^2	0.20	0.20	0.25	0.27

Notes: The dependent variable is the standard deviation of normalized value of the entries in a single period. Values were normalized by dividing by the total value allocated. All specifications report results from OLS regressions and also include a constant term. Individual Controls include sex, age, ethnicity bins, and number of sessions done. Standard errors are given in parentheses and clustered at the subject (individual) level. $* = p < 0.1$, $** = p < 0.05$, $*** = p < 0.01$.

Table 4. Credit and \$100: Impact on Equality of Selection Values

$$SameValue_{i,t} \quad = \quad \alpha \cdot Credit_{i,t} + \beta \cdot 100Treatment_{i,t} + \gamma X_i + \epsilon_{i,t}$$

Dependent Variable:	Specification			
All Entries Same Value	(1)	(2)	(3)	(4)
Subject Selects Credit	0.22***	0.21***	0.23***	0.21***
(Binary Treatment Var.)	(0.05)	(0.05)	(0.04)	(0.04)
\$100 Total Allocation	−0.07	−0.07*	−0.08*	−0.07*
(Binary Treatment Var.)	(0.04)	(0.04)	(0.04)	(0.04)
First Period		−0.13***	−0.13***	−0.13***
		(0.04)	(0.04)	(0.04)
Session Fixed Effects			X	X
Individual Controls				X
Dependent Variable Mean	0.18	0.18	0.18	0.18
Number of Observations	248	248	248	248
Number of Individuals	124	124	124	124
Adj-R^2	0.10	0.13	0.21	0.22

Notes: The dependent variable is the whether all entries have equal value in a single period. Values were normalized by dividing by the total value allocated. All specifications report results from OLS regressions and also include a constant term. Individual Controls include sex, age, ethnicity bins, and number of sessions done. Standard errors are given in parentheses and clustered at the subject (individual) level.
$* = p < 0.1$, $** = p < 0.05$, $*** = p < 0.01$.

Table 5A. Timing: Impact on Standard Deviation of Selection Value

$$Norm.Std.Dev_{i,t} \quad = \quad \alpha \cdot Credit_{i,t} + \beta \cdot 100Treatment_{i,t} + \delta \cdot Info_{i,t} + \gamma X_i + \epsilon_{i,t}$$

Dependent Variable:	Specification			
Normalized Value Std Dev	(1)	(2)	(3)	(4)
Subject Selects Credit	−0.08***	−0.08***	−0.07***	−0.07***
(Binary Treatment Var.)	(0.02)	(0.02)	(0.02)	(0.02)
\$100 Total Allocation	−0.17***	−0.17***	−0.17***	−0.17***
(Binary Treatment Var.)	(0.02)	(0.02)	(0.02)	(0.02)
Information Treatment	0.01	0.01	0.03	0.04
	(0.02)	(0.02)	(0.03)	(0.03)
Round Fixed Effects		X	X	X
Session Fixed Effects			X	X
Individual Controls				X
Dependent Variable Mean	0.20	0.20	0.20	0.20
Number of Observations	248	248	248	248
Number of Individuals	124	124	124	124
Adj-R^2	0.21	0.21	0.25	0.27

Notes: The dependent variable is the standard deviation of normalized value of the entries in a single period. Values were normalized by dividing by the total value allocated. All specifications report results from OLS regressions and also include a constant term. Individual Controls include sex, age, ethnicity bins, and number of sessions done. Standard errors are given in parentheses and clustered at the subject (individual) level. $* = p < 0.1$, $** = p < 0.05$, $*** = p < 0.01$.

Table 5B. Timing: Impact on Standard Deviation of Selection Value

$$
\begin{aligned}
Clock_{i,t} &= \alpha_1 \cdot Info_{i,t} + \gamma_1 X_{i,t} + \nu_{i,t} \\
Norm.Std.Dev_{i,t} &= \alpha_2 \cdot Credit_{i,t} + \beta \cdot 100Treatment_{i,t} + \delta \cdot Clock_{i,t} + \gamma_2 X_i + \epsilon_{i,t}
\end{aligned}
$$

Dependent Variable:		Specification		
Normalized Value Std Dev	(1)	(2)	(3)	(4)
Subject Selects Credit	−0.07***	−0.07***	−0.06***	−0.07***
(Binary Treatment Var.)	(0.02)	(0.02)	(0.02)	(0.02)
$100 Total Allocation	−0.17***	−0.17***	−0.17***	−0.17***
(Binary Treatment Var.)	(0.02)	(0.02)	(0.02)	(0.02)
Time Searching (Minutes)	0.001	0.001	0.003	0.004
	(0.003)	(0.003)	(0.003)	(0.003)
First Stage F Stat (IV)	176.9	168.8	159.2	144.2
Round Fixed Effects		X	X	X
Session Fixed Effects			X	X
Individual Controls				X
Dependent Variable Mean	0.20	0.20	0.20	0.20
Number of Observations	248	248	248	248
Number of Individuals	124	124	124	124
Adj-R^2	0.21	0.21	0.25	0.27

Notes: The dependent variable is the standard deviation of normalized value of the entries in a single period. Values were normalized by dividing by the total value allocated. All specifications report results from GMM Instrumental variable regressions and also include a constant term. Time spent searching was instrumented by the information treatment, with F values from the first stage reported. Individual Controls include sex, age, ethnicity bins, and number of sessions done. Standard errors are given in parentheses and clustered at the subject (individual) level. $* = p < 0.1$, $** = p < 0.05$, $*** = p < 0.01$.

8 Appendix

8.1 Appendix Tables

Appendix Table 1. Credit and \$100: Impact on Mean of Selection Value

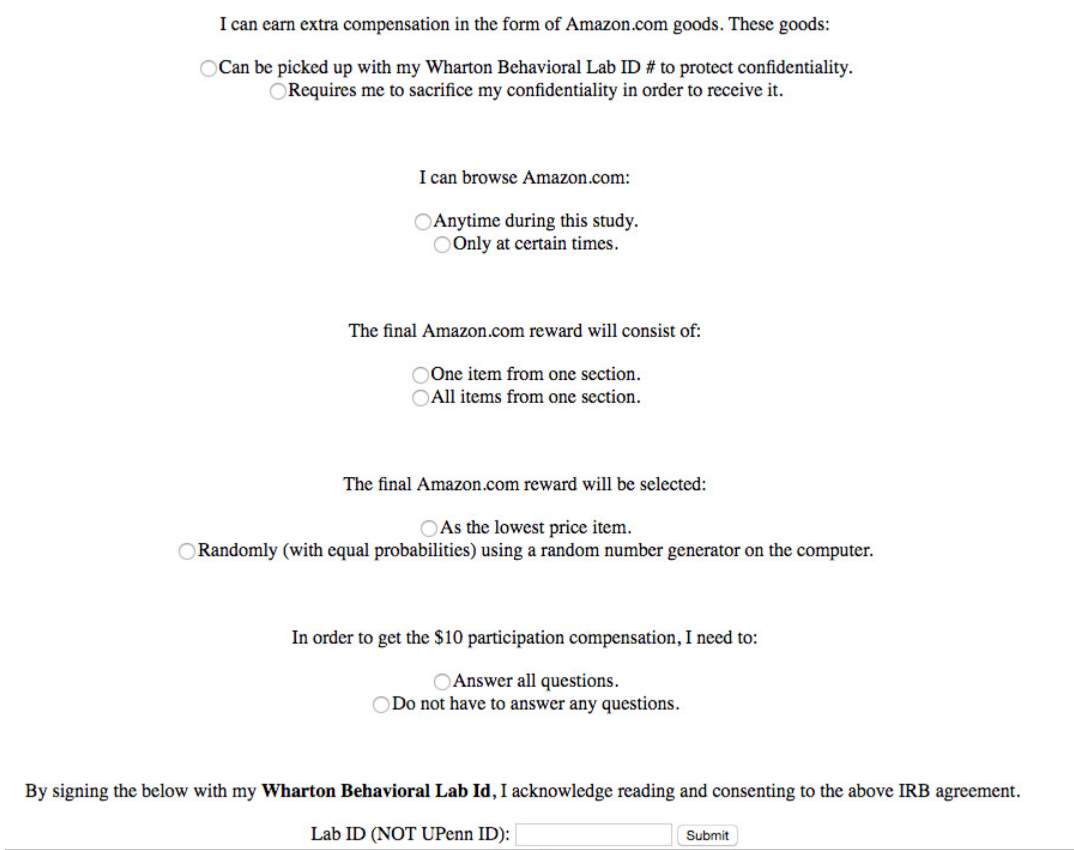
$$AverageValue_{i,t} = \alpha \cdot Credit_{i,t} + \beta \cdot 100Treatment_{i,t} + \gamma X_i + \epsilon_{i,t}$$

Dependent Variable		Specification		
Average Value	(1)	(2)	(3)	(4)
Subject Selects Credit	0.56***	0.57***	0.52***	0.52***
(Binary Treatment Var.)	(0.12)	(0.12)	(0.12)	(0.13)
\$100 Total Allocation	0.29***	0.30***	0.30***	0.30***
(Binary Treatment Var.)	(0.11)	(0.11)	(0.11)	(0.11)
First Period		0.12	0.12	0.12
		(0.11)	(0.11)	(0.11)
Session Fixed Effects			X	X
Individual Controls				X
Dependent Variable Mean	9.32	9.32	9.32	9.32
Number of Observations	248	248	248	248
Number of Individuals	124	124	124	124
Adj-R^2	0.09	0.10	0.12	0.17

Notes: The dependent variable is the average value of the entries in a single period. All specifications report results from OLS regressions and also include a constant term. Individual Controls include sex, age, ethnicity bins, and number of sessions done. Standard errors are given in parentheses and clustered at the subject (individual) level. $* = p < 0.1$, $** = p < 0.05$, $*** = p < 0.01$.

8.2 Appendix Figures

Appendix Figure 1. Quiz for Introduction Instructions



I can earn extra compensation in the form of Amazon.com goods. These goods:

- Can be picked up with my Wharton Behavioral Lab ID # to protect confidentiality.
- Requires me to sacrifice my confidentiality in order to receive it.

I can browse Amazon.com:

- Anytime during this study.
- Only at certain times.

The final Amazon.com reward will consist of:

- One item from one section.
- All items from one section.

The final Amazon.com reward will be selected:

- As the lowest price item.
- Randomly (with equal probabilities) using a random number generator on the computer.

In order to get the $10 participation compensation, I need to:

- Answer all questions.
- Do not have to answer any questions.

By signing the below with my **Wharton Behavioral Lab Id**, I acknowledge reading and consenting to the above IRB agreement.

Lab ID (NOT UPenn ID): Submit

Notes: Every participant had to answer questions after reading experiment instructions. Subjects had to answer all questions correctly to proceed. If the subject entered the wrong answers, the browser would alert them to this and ask for them to review the instructions again.

Appendix Figure 2. Quiz for Instructions Prior to Each Period

Please answer the questions below to continue

For this section:

The goods must be equal to or less than $

- ◯ If I leave more slots empty, I am more likely to receive a reward.
- ◯ Each slot has an equal chance of being chosen regardless of whether it is empty or not.

- ◯ I do not add shipping to the prices.
- ◯ I need to add shipping to the prices.

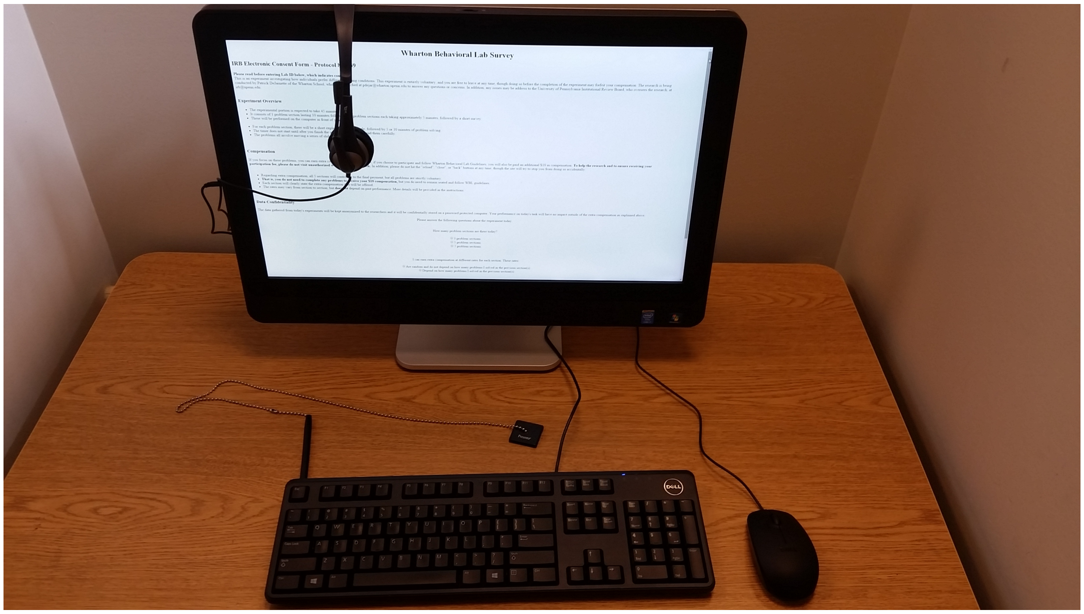
When selecting goods

- ◯ I can place the same item in multiple slots to increase the chance of it being selected.
- ◯ I must put different items in every slot.

13 seconds until you can move on

Notes: Every participant had to answer questions prior to every period. If the subject entered the wrong answers, the browser would alert them to this and ask for them to review the instructions again.

Appendix Figure 3. Cubicle Environment



Notes: Every participant had access to an identical computer with headphones as pictured above. Cookies and browser history were cleared after every session to limit any subject overlap. It was not possible to see other subjects from within the cubicle. Google Chrome was employed as the browser. All instructions were written, but lab assistants were on site to answer any additional questions.