Can Market Experience Eliminate Loss Aversion?

An Experimental Test Of The Risk Preference Disparity

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Section 1: Introduction

According to Tennyson's oft-quoted poetic assertion, "it is better to have loved and lost, than never to have loved at all." (In Memoriam: 27 (1850)) Within the framework of classical expected utility theory (EUT), this statement holds true if increased happiness is a positive side effect of being in love. If loving someone yields extra utility during the duration of the relationship, when the relationship is over, the supplemental utility remains, leaving you better off in your current state having loved and lost than never having loved at all. Without the added utility assumption, and still within the framework of EUT, one would be indifferent between the two states of the world.

In the context of Tennyson's quote, prospect theory presents a less idealistic view. Specifically, while in EUT one's utility is determined relative to an absolute level of wealth, prospect theory (a reference dependent utility theory) allows for utility to vary depending on whether a given level of wealth represents a perceived loss or gain. While by EUT, an individual (all else held constant), is at worst indifferent between loving and losing, by the assumptions of prospect theory, a loss averse individual is always worse off having loved and lost. This reference dependent utility function not only allows for a feeling of dissatisfaction at the prospect of gaining and then losing something in life, but also allows for this dissatisfaction to lead to a change in behavior. To see this in practice, consider the following two scenarios.

Scenario 1:

On your lunch break one afternoon, you run into an old friend on the street. During the course of the conversation, he mentions an encounter with a rather odd salesman from a kitchen store he knows you like. Intrigued, you ask him to elaborate. Your friend tells you that the salesman was offering a gamble to customers on the street. The odds of winning are 10%. If you win, you become the proud owner of brand new Nespresso machine. If you lose, you have to let the salesman pick someone at random to give you a punch in the face. "Would you take the gamble?" he asks.

Scenario 2:

It's 8am. You're running late for work because you spent half an hour trying to fix your coffee machine which seems to have mysteriously broken just days after the expiration of your one-year money back guarantee. On your way to work, you pass an odd looking salesman outside of your favorite kitchen store. You remember the story your friend told you, and decide to see if he is still offering the same deal. "Absolutely!" he says. "Would you like to take the gamble?"

For simplicity, assume you're not a masochist, so there is no utility gained from pain received. Furthermore, assume that while in Scenario 1, you might not need a Nespresso machine, you would be able to sell the machine for cash without incurring any significant transactions costs. Would you be more likely to take the gamble in scenario 2? While many would agree that it is rational to want to avoid a loss before it occurs, only a reference dependent utility function that is convex over losses can account for a change in behaviour caused by the unwillingness to realize a loss ex-post. Such a change in behavior might occur in the example above if an individual were unwilling to take the gamble in scenario 1, but underwent a reversal of preferences once exposed to the loss of their coffee machine, rendering the previously unattractive gamble much more appealing. While it has been well established[1] that the aversion to (and expected disutility from) a loss can encourage people to take risks otherwise unattractive to them, little attention has been paid to people's ability to change their behavior by learning from their own experiences.

[1] For example, see Khaneman and Tversky (1979)

The question addressed in this paper is whether subjects who would take the gamble in scenario 2 (but not in scenario 1) will, through actually experiencing a proverbial punch in the face, be less likely to take the gamble if presented with the same scenario again.

In this paper, I present evidence from a laboratory experiment designed to test the stability of loss aversion over two types of experience. First, I test whether people become less loss averse after experiencing the bad outcome through the hypothetical simulation of a risky gamble. Second, I test whether incurring a loss (i.e. a proverbial punch in the face) in your initial session decreases the tendency towards loss aversion when returning to the same scenario at a later date.

My results suggest that both types of experience can lead to less loss aversion. I find strong support that the hypothetical simulation of outcomes associated with a risky gamble lead to fewer instances of uncharacteristic risk seeking over losses. While the results regarding the second form of experience are less conclusive, they also support the hypothesis that the risk preference structure of prospect theory may not, in all cases, be stable over time.

The remainder of the paper is organized as follows. In Section 2 I review the relevant literature. In Section 3 I present my hypotheses. Section 4 is dedicated to an explanation of my experimental design. In Section 5 I outline the methods used, and provides a contextual restatement of my hypotheses. In Section 6 I present my results. In Section 7 I discuss these results and provide a brief framework for future experiments. Section 8 concludes.

Section 2: Literature Review

According to classical expected utility theory (EUT), preferences over lotteries are determined by the probability and absolute level of income associated with each outcome. If diminishing marginal utility of income is assumed, then utility curves in EUT are concave and steeper over losses than over gains. The relative steepness of the curve over losses reflects the fact that losses have a larger absolute effect on utility than similar size gains, and is consistent with decreasing absolute risk aversion. Decreasing absolute risk aversion implies that holding the probabilities and outcomes of a particular gamble constant, a greater level of wealth implies a greater likelihood of taking the gamble. To be clear, the assumption of decreasing absolute risk aversion does not necessarily hold in all cases. Indeed it is theoretically possible to have a utility function where utility is determined by probabilities and incomes associated with each outcome which displays constant absolute risk aversion. However, constant absolute risk aversion implies that one's willingness to bet $1000 on a coin toss in no way depends on the value of their initial endowment. While it is theoretically possible to have a utility function consistent with EUT and constant absolute risk aversion, there is little empirical evidence that such a model is indicative of true behavior.[2]

As mentioned, while EUT can, with the assumption of decreasing absolute risk aversion, account for changes in risk preferences which vary with absolute levels of wealth, this theory cannot account for changes in risk preferences dependent on whether a perceived endowment is viewed as a loss or a gain. Not only can EUT not account for the

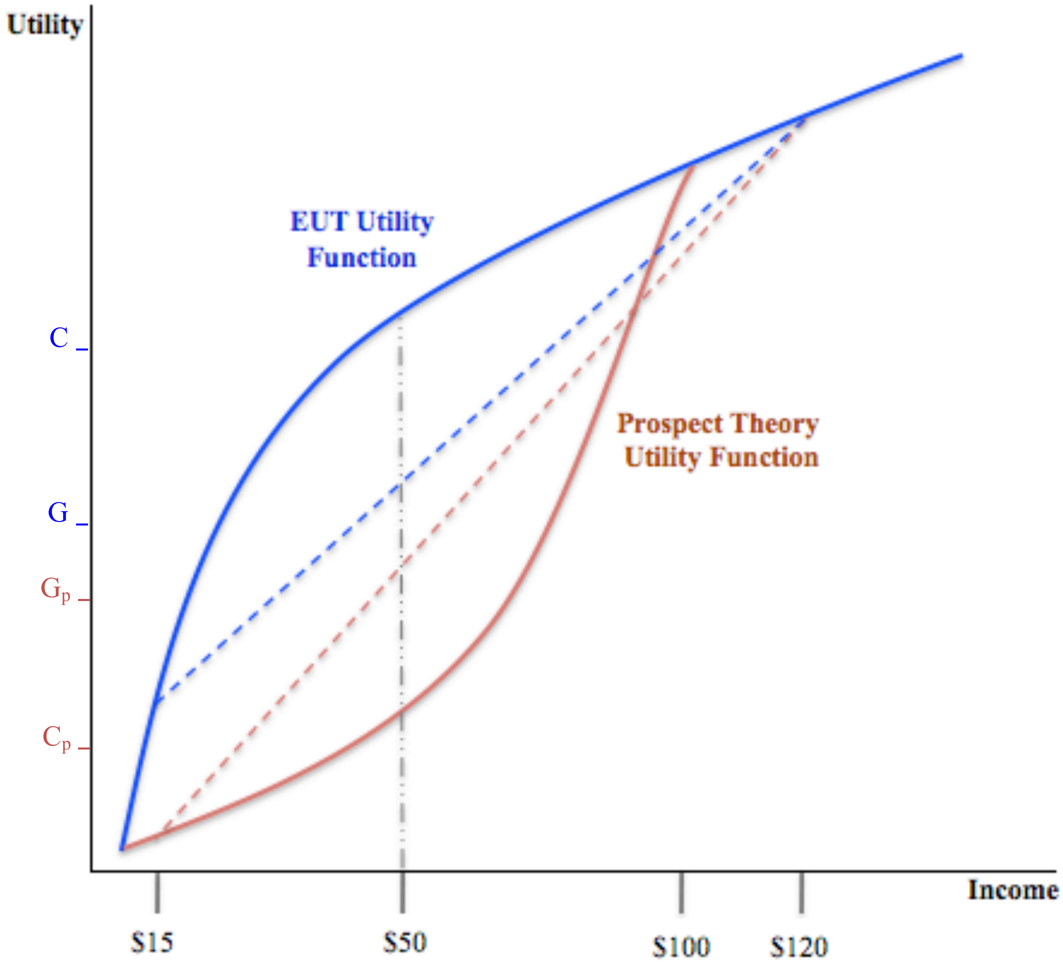
[2] Hirschleifer and Riley (1979)

effect of prior gains and losses on future choice, EUT assumes that these prior gains and losses do not matter.

However, many documented violations to this principle suggest risky behavior is in fact, reference dependent. [Kahneman and Tversky (1979, 1984); Thaler (1980); Tversky and Khaneman, (1991)] For example, the amount of risk a gambler is willing to take has been shown to depend on prior gains. This effect, called the *House Money Effect,* is used to describe the increase in risk-taking behavior present after a large windfall. [Thaler and Johnson (1990)] More generally, this is the principle that if two people have the same endowment and similar status quo risk-preferences, but one person is playing with their hard earned cash while the other is given money with which to gamble, the person gambling with money to which he has little attachment is likely to exhibit more risk-seeking behavior.

The first study to comprehensively document violations of EUT, and formulate them in the context of a new model of economic theory (commonly known as prospect theory), was published by Khaneman and Tversky (KT) in 1979. In 1991 the authors published a more formal description of the proposed reference-dependent model of consumer choice. The basic tenets of this theory are loss aversion (that losses are weighted heavier than gains), diminishing marginal sensitivity to losses and gains, and reference dependence.

Figure 1: Expected Utility Theory vs. Prospect Theory



The differences between EUT and prospect theory are best elucidated through an example (graphically depicted in Figure 1). Suppose an individual (who only has $50 to their name) is presented with the following choice. They may either keep their $50 endowment for certain, or take a bet which yields $120 with probability 1/3 and $15 with probability 2/3. According to EUT (the blue utility curve), if the individual is risk-averse, they will always prefer to keep their $50 with certainty, rather than risk incurring a loss by taking a gamble. In Figure 1, the utility that corresponds to keeping the $50 with certainty is labeled with C on the vertical axis. The expected utility from taking the ($120, $15) gamble (which has an expected value of $50) corresponds to G on the vertical axis. Because the utility of income is greater than the expected utility (C > G), a risk averse person would always prefer to keep the income with certainty, rather than to take the gamble and risk the chance of the bad outcome.

Now suppose the same individual instead began their day with an endowment of $100, but somehow lost $50 before they were given the chance to take the gamble. According to EUT, because both the gamble and the initial endowment remain the same, the individual will again choose not to take the gamble. However, within the framework of prospect theory, if the individual's $50 loss has not yet been realized (or 'sunk'), their otherwise neutral initial endowment is mentally framed as a loss, causing them to take more risk. Prospect theory assumes that gains and losses are coded relative to the status quo. Since the individual in this example hasn't accepted the initial $50 loss, their status quo is still $100. Thus, the utility curve of prospect theory is drawn with an inflection point at the utility of income $100.[3] The convexity of the utility curve for values of income less than the status quo (which is still $100, even though the individual's actual endowment is $50) is consistent with a greater risk seeking over losses. Prospect theory suggests that if a loss is incurred and a new status quo has not been psychologically accepted, then a risk-averse person is likely to accept risky gambles that would be unacceptable to him otherwise in the hope of reclaiming their initial loss.

There are several effects, other than increased risk taking in the presence of a loss, which result from a combination of the reference dependence and loss aversion which have been the topic of numerous economic studies. Two of the most immediate and relevant

[3] For the purposes of this example, I have assumed a convergence of EUT and PT utility curves at incomes above the status quo

effects are the endowment effect and loss realization aversion.

The Endowment Effect and Loss Realization Aversion

Standard economic theory implies that when transactions costs and income effects are sufficiently small, there should be only a negligible difference between a seller's minimum acceptance price (or, willingness to accept), and a buyer's maximum purchase price (willingness to pay). As many have noted, this is not the case in practice. The endowment effect, a term coined by Thaler (1980) has been used to explain the disparity between a seller's willingness to accept (WTA), and a buyer's willingness to pay (WTP) present in many trading scenarios. Thaler uses the term to account for the perceived increase in a goods value upon incorporations into a subject's endowment which stems from a genuine effect of reference-dependent preferences.[4] Furthermore, as many laboratory and field experiments have shown, this process is almost instantaneous and is directly responsible for inflated WTA values among sellers in induced trading scenarios [Knetsch and Sinden (1987), Ketsch (1989), Kahneman, Knetsch and Thaler (1990, 1991)]. In many respects, a seller's inflated WTA value is an immediate consequence of loss aversion. If a seller is loss-averse, the absolute change in utility when the good exits the seller's endowment will be greater than the absolute change when the good was received. Thus, the disparity between a seller's WTA and a buyers WTP arises both as sellers raise asking prices in attempt to compensate for the asymmetric utility tradeoff inherent in a fair trade, and as a consequence of the reference dependent nature of utility.

[4] Kahneman, Knetsch and Thaler (1991), p.1326

As with almost any economic theory, there are those who question the explanatory validity of the endowment effect. Most notably, Plott and Zeiler argue that the evidence in support of the endowment effect found in laboratory experiments is not indicative of the anomaly itself, but rather, is a function of the experiment design.[5] Although Plott and Zeiler's claim may have some resonance within the realm of laboratory experiments, their argument cannot expand to counteract similar evidence of anomalies caused by reference dependence and loss aversion found within real world markets.

One such example is the documented reluctance of professional traders to realize losses. As Locke and Mann (2000) report, even in the presence of various "rules of trading" such as 'buy low, sell high' and 'cut your losses, ride your gains' many professionals fail to abide by these self-created rules and ride losses for a longer period of time than gains. This anomaly, dubbed 'loss realization aversion', is, like the endowment effect, a consequence of an aversion to losses and the reference point from which losses and gains are evaluated. [See also, Frino, Johnstone, and Zheng (2004); Heisler (1994); Odean (1998)].

While I have, until now, discussed loss realization aversion and the endowment effect as two related yet distinct anomalies, it is important to note their coexistence in some markets. As Genosove and Mayer (2001) show, using data from the Boston housing market during the 1990's, not only do houses sit on the market for long periods of time during a market decline (a signal of loss realization aversion), but the amount of time spent on the market is largely a function of inflated asking prices (a heightened WTA).

Both pairs of authors find significant evidence of the monetary costs associated with loss aversion. According to Locke and Mann, the more 'disciplined' traders – i.e. the ones

[5] Plott and Zeiler 2007 p 1450

who are better able to abide by their self-created 'rules' of investor behavior – earn higher revenues than their less disciplined peers. Genosove and Mayer find that find that a 10% increase in projected loss on a home leads owners to increase their asking prices by approximately 3.5%.[6] Moreover, in light of the costs associated with a longer time-to-sale, there are clear costs associated with this behavior.

Another highly relevant result of the Genosove and Mayer study is that professional real estate investors, while still guilty of inflating their asking prices as expected losses increase, do so at about half the rate of owner-occupants. This, the authors argue, is puzzling given the greater direct costs (of inconvenience, constant cleanliness, real estate fees etc) to homeowners in comparison to investors. The authors hint at the irrationality of the documented behavior of owner-occupants by positing that perhaps they are "overly optimistic in their listing behavior."[7] Both the discrepancy between investor and owner-occupant increases in WTA and the costs associated with realization aversion lead to the question of whether loss aversion is a stable preference. That is, if a homeowner incurs large costs by an inflated WTA in one market, if that same seller is ever in the position to sell their house again, will they exhibit the same behavior, or will they be more inclined to accept a loss and avoid undesirable additional costs?

Evidence on the attenuating effect of experience

List (2003) found evidence that as sellers increase their experience in the sports card market, the WTA/WTP disparity diminished over time. The design of List's experiment matches real world settings where traders endogenously select into the market

[6] Genosove and Mayer (2001), p. 1243
[7] Genosove and Mayer (2001), p. 1248

for trading sports cards.[8] The main concern with List's results stems from the fact that field experiments must be able to control for selection. In particular, there is no way of knowing for sure whether the experienced traders in List "exhibit no endowment effect due to experience, or because a prior disposition toward having no such gap leads them to trade more often."[9] In trying to determine whether learning was an effect of treatment or selection, List recruited subjects for a return session approximately one year later. While the participants in this return session exhibited a higher rate of exchange than in the initial session, List cannot adequately control for selection into the return treatment. Not only does he find that those who participants who chose to return had gained significant experience over the time between his two sessions, he also cannot control for potential sample selection bias. If those who returned for a second session had retained significant interest in the sports card market, then their increased trading rates (decreased value disparity) might be a function of selection rather than treatment.

Furthermore, although List shows that the endowment effect can be attenuated through market experience, he offers little theoretical explanation as to why this is so. The main hypothesis of this paper is that through experiencing the downside to taking this otherwise unacceptable risky behavior, the initial loss to their endowments will have less of an overshadowing effect, allowing them to recognize the risks associated with the otherwise unacceptable gamble, and reducing the amount of loss aversion displayed. The hypothesis is that the cycle will continue with each round, and, in the limit, subjects will learn to make peace with their initial losses and avoid risk-seeking behavior. This is based off of psychological evidence that "risk-averse people look more at the downside and risk

[8] List (2003), p.44
[9] List (2003), p.54

seekers more at the upside. But risk seekers may play it safe from time to time, and even the most risk-averse person will take chances – big chances—when necessary."[10] However, if an individual is risk averse in the status quo, then the loss has led them to look more at the upside of a further risky bet.

In order to overcome the selection issues present in List (2003), I design an experiment to test the effect of experience on loss aversion, where levels of experience can be exogenously defined and tested.

The results of this study can be summarized as follows. Firstly, I find overwhelming support for the presence of loss aversion. These results are particularly salient among participants who are risk-averse. Secondly, I find weak support for the hypothesis that the simulation of chance outcomes over losses reduces the frequency of loss aversion. Finally, I find support for the hypothesis that experience reduces a loss-averse participant's risky choices over loss rounds. Overall, these results are interpreted as support for the hypothesis that the degree to which an individual is loss-averse can be reduced through market-like experience.

Section 3: Hypotheses

By definition, a participant who is loss averse will take an uncharacteristic amount of risk after experiencing a loss, with the hope of recovering (or avoiding) that initial loss. As with all risky bets, the additional risk taken can either result in a 'good' or a 'bad' case scenario. Assuming the risky bet was placed after an initial loss, the 'good' case yields the recovery of the loss, and the 'bad' case ends in the forced realization of an even larger loss.

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[10] Lopes (1987), p. 277

All of my hypotheses are based on the notion that people who are willing to take on an uncharacteristic amount of risk after an initial loss, do so without fully digesting the likelihood of the 'bad' scenario. That is, loss-averse people are willing to take more risk following a loss because they focus almost exclusively on the possibility of recovering their loss, and pay little attention to the very likely possibility that they could end up worse off. Therefore, when people are forced to recognize the likelihood and expected disutility from a 'bad' case scenario, my hypothesis is that loss aversion will decrease.

Hypothesis 1: 'Experience'

> H_0: As participants become more experienced, we will see a reduction in risk taking over losses.
>
> H_A: Preferences over losses are rational and consistent with expected utility. Experience has no effect on the stability of increased risk taking over losses.

Hypothesis 1A: '*Learn-by-Seeing'*

> H_0: Participants who are 'treated' with exposure to hypothetical simulations of risky bets will be more likely to recognize the downside of these bets, and will decrease their loss aversion as the number of these simulations increases over time.
>
> H_A: Hypothetical simulations do not have an attenuating effect on loss aversion. We cannot learn from others mistakes.

Hypothesis 1B: '*Learn-by-doing'*

> H_0: As participants learn through actual (as opposed to simulated) experience, we will see a reduction in risk taking over losses.
>
> H_A: Participants loss aversion tendencies are not changed with experience.

The *simulation* treatment is designed to test whether subjects will change their risk preferences (over losses) by simulating hypothetical outcomes of the risky bets. The hypothesis is that by experiencing the outcome of each chance lottery in the loss round, participants who are loss averse will put more weight on the potentially negative outcome and reduce their uncharacteristic risk-taking with each additional period.

Both the 'learn-by-doing' and the 'learn-by-seeing' hypotheses are based on the conjecture that expected regret from a bet not taken motivates risk seeking over losses. Furthermore, these hypotheses center around the idea that at the time that the risky choice is made, people underweight the expected regret (or disutility) of taking the risky choice and ending up with the 'bad' outcome. Hypothesis 1A posits that simulating the outcomes of risky lotteries will have an attenuating effect on loss aversion because it will force participants to focus on the potential downside to the risky bet. Hypothesis 1B posits that experiencing the bad outcome will further reduce the tendency towards loss aversion.

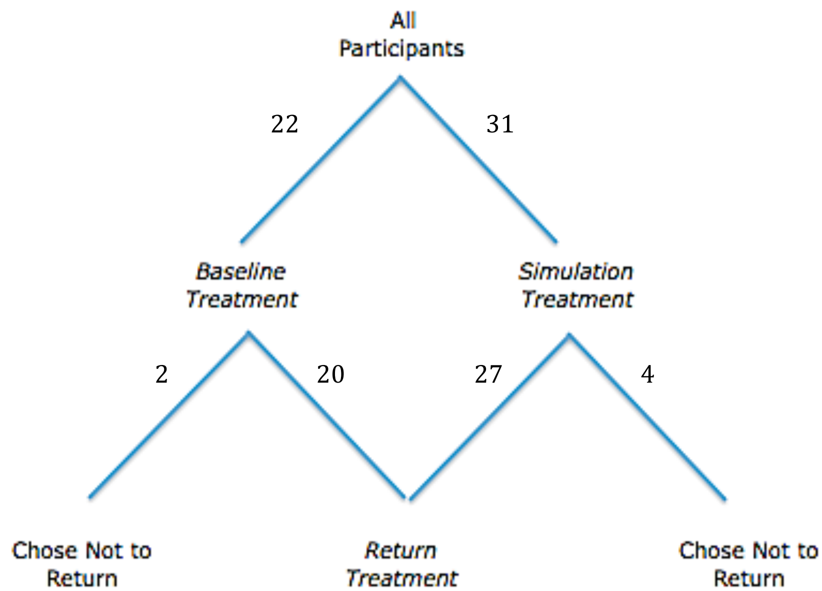
Section 4: Experiment Design

Section 4.1: Design Overview

In order to test my hypotheses most efficiently, and to minimize the confounding effects of natural selection into the financial professions that provide a suitable basis for studies on loss aversion, my experiment was implemented in a laboratory setting. The experiment described herein was run over several sessions in February and March of 2010 using 53 student participants from Williams College.

The experiment was comprised of three treatments. The first treatment (referred to as *baseline*) was used as a benchmark against which to measure the effect of the other treatments. The two other treatments were designed to test whether experience through hypothetical simulations (referred to as the *simulation* treatment) or by task repetition (referred to as the *return* treatment), has a dampening effect on loss aversion.

Figure 2: Treatment Map



As shown in the treatment map (Figure 2), all 53 subjects participated in either the *baseline* or the *simulation* treatment. (22 participated in the *baseline*, and 31 in the *simulation*). After the completion of their initial session, all participants were then invited to return for a second session. All participants who chose to return participated in a treatment I intuitively refer to as *return*. As shown in Figure 2, 47 of the initial 53 participants chose to return. The length of time between initial and return sessions varied from 1 day to approximately 2 weeks.

All treatments were run in computer-based sessions using zTree[11] (an experimental economics computer software). In all treatments, participants were asked to make a choice between options in a total of 12 decision periods. Participants were given a $20 endowment with which to participate, but were paid based solely on the outcome of one

[11] Fischbacher (2007)

randomly selected decision period. Each decision period contained a choice between three options, with the payout from two of these options depending on chance. If participants chose an option whose outcome depended on chance, they did not learn the outcome of this choice until the end of the experiment. (And even then, participants only learned the outcome of their choice in the decision period that was randomly selected for payment). If the outcome of their selected decision left them with more than $20, participants were given additional money at the end of the experiment. Similarly, if their decision left them with less than $20, money was taken away.

Figure 3: Experiment Timeline

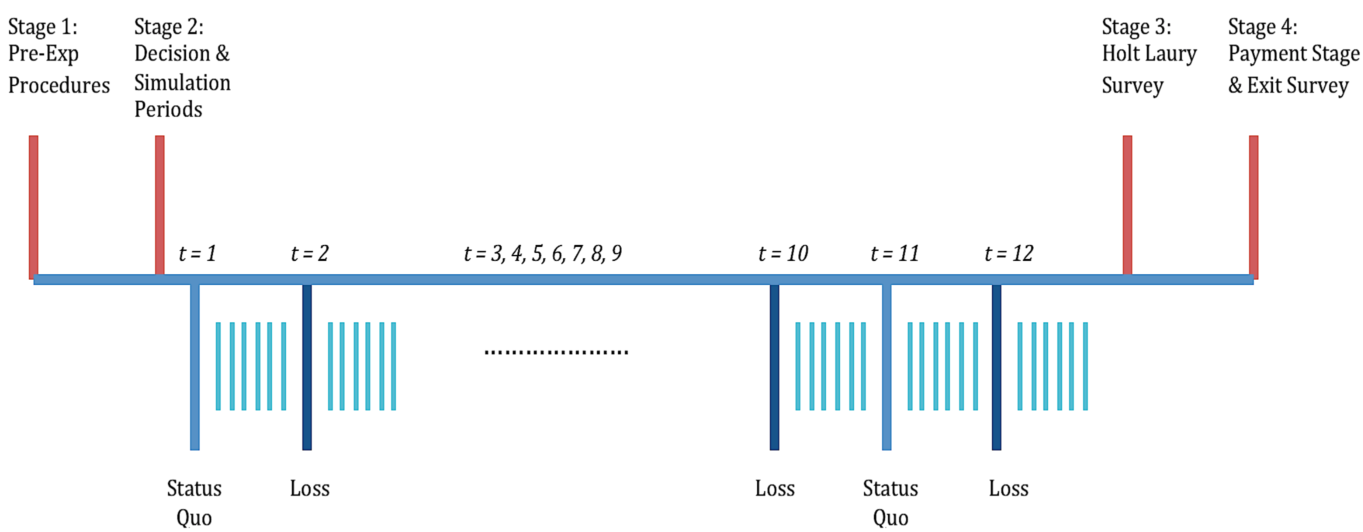


Figure 3 presents a timeline of the components within the *baseline, simulation* and *return* treatments. The *benchmark* and *simulation* treatments are designed to isolate the effect (if any) of simulating outcomes on loss aversion. In the *benchmark* treatment, the 12 decision periods are presented in a randomized sequence, with no interruption between decisions. In the *simulation* treatment, an additional component, call the simulation stage, is added to each of the 12 periods. So, while in the benchmark treatment subjects move from decision stage 1 to 2 to 3 and so on, without interruption, in the *simulation* treatment participants make their selection in decision stage 1, perform 6 simulations, and then continue the pattern for each of the 12 periods. The simulation stage also included an on-screen history box to allow participants to keep track of their results, and, after a simulation stage is completed, the history box remained on screen for 20 seconds. This was designed to encourage participants to focus on the results of their simulations.

The *return* treatment was designed to determine whether loss aversion is consistent or diminished over time. There is no structural difference between the *simulation* and *return* treatments. The only difference lies in the subject pool. While in the *simulation* all participants are 'first-timers,' all participants in the *return* treatment are required to have previously participated in either a *simulation* or *baseline* treatment. (See Figure 2)

As shown in Figure 3, all treatments include pre-experiment procedures & instructions, a Holt Laury survey and a payment stage. After all 12 decision and simulation rounds were completed, participants were asked to complete a brief risk-aversion exercise.[12] This exercise was an exact computer replica of the risk aversion survey given by Holt and Laury (2002). The choices made in this survey were later used to create a HLScore[13] variable which provides a raw index of risk-aversion.[14] Earnings from this stage were later added to participant's final payment.

In the payment stage, participants are shown the number of the random decision

[12] For participants in the first *baseline* session this survey was not computerized. However, results from the manually administered version of this survey do not differ significantly from the computerized results.
[13] HLScore is the first decision on the survey in which a participant selects option B instead of option A. Interpretation of variable: the higher the HLScore, the more risk-averse the participant is.
[14] Admittedly, the HLScore variable is (at best) a noisy estimate of risk aversion. However, I include the risk aversion survey in the experiment in order to provide a benchmark with which to compare an independently generated risk aversion index.

period selected for payment. If in that decision period a participant selected an outcome depending on chance, that outcome is then calculated. This is done in a manner quite similar to the simulation stage. The pre-experiment procedures require additional explanation and will be described in the next section.

Section 4.2: Pre-Experiment Procedures & Motivation

There are several ethical and moral challenges to the design of a laboratory experiment testing behavior over losses. While in a field study, one would be able to take only behavioral data gathered after an investor has experienced a financial loss, collecting similar data in the laboratory is less straight forward. One way of modeling this in the lab requires a two-part experiment whereby participants complete an initial task followed by a secondary lottery or investment decision. However, if the initial task had outcomes that were evenly distributed over losses and gains it would be highly inefficient (and *expensive*) to take the chance that participants experience a loss – and disregard all data for participants who make a gain. And, if the initial task had outcomes which were skewed to induce losses, participants would be likely to notice that the game was 'rigged' and would be more likely to expect making loss. This would likely result in a decreased endowment effect, and preference-independent behavior over losses.

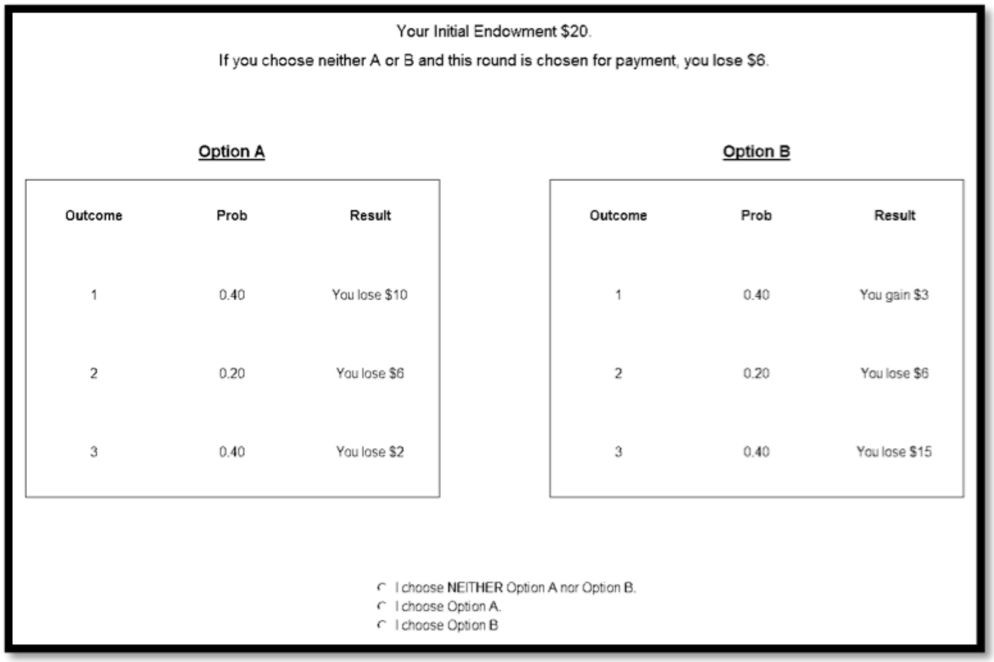
Given the difficulties inherent in two-part experiments, my experiment included only a one-part decision stage. To frame this decision stage as a choice over losses, participants were told that they had lost money from their initial endowment (which had been provided to them at the beginning of the experiment), and asked to make their choice keeping in mind their loss. However, because this study was specifically designed to measure behavior over losses, it was necessary to take additional steps in order to *ensure* that the mere act of telling a subject they had lost money actually forced them to experience this loss. All of the following steps were intended to increase individual feelings of ownership over the initial endowment.

Naturally, the first step was to assign an endowment to each participant. While some experiments[15] suggest having participants earn their money in order to generate an attachment to it I was able to generate an attachment without this process. To do this, I gave subjects the value of their endowment in cash at beginning the experiment. Because I wanted subjects to interact as much as possible with their $20, endowments were distributed in envelopes filled with twenty single-dollar bills. Not only did participants have the endowment in their possession throughout the experiment, but subjects were also asked to complete two pre-trial exercises involving their endowment. Specifically, subjects were asked to confirm that there were twenty dollars inside each envelope, and to indicate (in as much detail as possible) how their endowment would likely be spent. My hypothesis was that by physically counting the money, participants would be more likely to think of it as their own.

While the aforementioned steps were implemented to generate an endowment effect *before* the start of the experiment, two key measures were taken to ensure that participants continued to feel ownership of their endowment *throughout* the duration of the experiment. The simplest of these two steps was the wording of the on-screen text, and the framing of each lottery choice. (See Figure 4)

[15] Gazzale and Walker (2009)

Figure 4: Loss Decision Stage



The second element of experiment design used to promote the presence of an endowment effect throughout the experiment was the use of 'filler lotteries'. These 'filler' lotteries are used in every odd decision period, and have an expected value of $20. So, if participants choose to take no chance when making their choice in a 'filler' period, they can expect no change to their endowment (i.e. the status quo). Because of the efforts made to promote integration of the $20 into subject's endowment, and because these 'filler' periods involve choices amoung options with expected value of $20, we refer to these periods as choices in these periods as 'choices over the status quo.'[16] Additionally, the status quo decision periods have the added benefit of providing a measure of risk aversion for gambles not involving a loss.

[16] The term 'filler period' will not appear in the rest of this paper.

Section 4.3: Specifics of the Decision Stage

As mentioned, each decision period was comprised of three options, two of which depended on chance. Furthermore, while the same set of loss and status quo lotteries were presented to each subject regardless of treatment, the order in which the lotteries were presented was randomized for each participant.

The first option I refer to as the certainty option - to do nothing, and leave the experiment with an amount corresponding to the expected value of the chance lotteries. The remaining two options I refer to as the low and high-risk lottery options. Participants who selected these options elected to take some an amount of risk which varied with the particular option.

The lottery design closely mirrors those found in Brooks and Zank (2005). In their study, Brooks and Zank found evidence of loss aversion using three part lotteries (loss, gain, neither) with equal probability weights for the loss and gain. The use of a three-part lottery is based on evidence [Thaler and Johnson, 1990] that 50-50 symmetric lotteries are not appropriate to model behavior because they induce greater risk-seeking.

The variation across loss lotteries was also motivated by Thaler and Johnson (1990). The maximum outcome of the high-risk choices alternates between a gain of $1 and $3 maximum outcome of the low-risk choice is fixed at $18 (a loss of $2). The experiment used two different risky lotteries (instead of one) to distinguish between risk seeking over losses where the participants does (and does not) have the opportunity to completely recover their initial loss.

Table 1: Lottery Design

Lottery Type	High Risk	Low Risk	Expected Value
Status Quo	(29, 20, 11)	(26, 20, 14)	20
Status Quo	(28, 20, 12)	(25, 20, 15)	20
Status Quo	(27, 20, 13)	(24, 20, 16)	20
Status Quo	(26, 20, 14)	(23, 20, 17)	20
Status Quo	(25, 20, 15)	(22, 20, 18)	20
Status Quo	(24, 20, 16)	(21, 20, 19)	20
Loss	(23, 14, 5)	(18, 14, 10)	14
Loss	(21, 14, 7)	(18, 14, 10)	14
Loss	(23, 15, 7)	(18, 15, 12)	15
Loss	(21, 15, 9)	(18, 15, 12)	15
Loss	(23, 16, 9)	(18, 16, 14)	16
Loss	(21, 16, 7)	(18, 16, 14)	16

**All lottery outcomes (a, b, c) have corresponding probability distribution (.4, .2, .4)

As indicated in Table 1, the main difference between the different sets of loss round lotteries is the expected value. The loss lotteries have expected values of $14, $15 or $16, and each expected value has a frequency of two. Cross-lottery differentiation between loss lotteries of the same expected value will occur in the high-risk choice: in one case the maximum outcome in the high risk choice will leave the participant with $23, in the other case, $21. Given that these lotteries will have equivalent expected values, this will also affect the value of the minimum outcome in the high-risk choice.

All status quo lotteries are 'mixed' gambles which involve equal probability of a loss and a gain. As visible in Table 1 the expected value of both the low and high-risk lotteries remains constant across all status quo rounds.

Section 5: Methods

In order to measure risk preferences across treatments and across individuals, I create an index over which to measure choice behavior. The ordinal variable *choice(i_j)*, measures the relative riskiness of a choice *j* in session *i* (with *i* equal to 1 or 2 depending on whether it is the participant's initial or return session). The value of *choice(i_j)* is 0, 1 or 2 depending on whether a participant selected the certainty, low-risk or high-risk option in the relevant decision stage. Importantly, the risky choice index variable only accounts for the relative riskiness of a choice made *within* a given decision period, it does not account for the relative riskiness of the options *over* decision periods. By this measure, selecting the high-risk option would yield a risky choice index (*choice(i_j)*) value of 2 in any decision period regardless of the risk associated with that option.

Cross-lottery variation is then controlled for with the presence of certain choice independent lottery characteristics (v*ariance, islossround* and *is23*) in later regressions. *Variance* is calculated as $\sigma^2 = .8*(x_{lh}-\mu)^2$, where x_{lh} is the highest outcome in the low-risk lottery, and μ is the corresponding expected value of a given decision period. In most decision periods is it sufficient to calculate variance for only the low-risk option because the difference between the high (low) outcome of the high-risk choice and the high (low) outcome of the low-risk choice is consistently $3. To adequately account for the three loss round lotteries where this spread is increased to $5, I create a binary *is23* variable to be included in regressions uniquely involving loss rounds. To further control for the differences between the loss and status quo round lotteries, I introduce a binary *islossround* variable.

With a measure of risky choice, and controls for variation across lotteries, we can establish an empirical definition of loss aversion. As discussed, loss aversion manifests itself in a disproportionate appetite for risk over losses. Accordingly, we can say that a person is loss averse if the sum of their risky choice indices over losses (∑*losschoice(i_j)*) is greater than or equal to the corresponding choice sum over the status quo. To account for the varying degree to which a subject can be loss averse I also create a loss aversion index variable (A_i), as in Definition 1 below.

LOSS AVERSION
Let G_i be the sum of risky choices over gains for an individual *i*. Let L_i be the sum of risky choices over losses for an individual *i*.

A subject is considered to be loss averse iff:

i. $G_i \leq L_i$ (with $0 \leq G_i. L_i \leq 12$)

DEFINITION 1: LOSS AVERSION INDEX
Let L_i & G_i be defined as in Definition 1. Let A_i denote a subjects' loss aversion index.
Then, $A_i = L_i - G_i$

A participant is strictly loss-averse if this inequality holds in the strict case, and weakly loss-averse if the sum of risky choices over losses is equal to those in the status quo. As stated in Section 2, both constant *and* decreasing absolute risk aversion can work within the framework of EUT. While there is little empirical evidence to support constant absolute risk aversion, it is possible that a subject might take the same amount of risk with an income of 20 that they would if their income were 14. Therefore it is possible that a subject I characterize as weakly loss averse might not be loss averse. For the purposes of this paper I assume a model of decreasing absolute risk aversion and therefore include weakly loss averse participants in my catergorization of loss aversion. While this method is not without flaw, the theoretical flaws do not affect my results. Any discrepancies in my definition of loss aversion are overcome in my regressions because I control for loss aversion index (the degree of loss aversion) rather than a binary measure.

To test for any interaction effect between risk and loss aversion, I create a risk-aversion index using Kahneman and Tversky's (1979) definition of risk aversion. According to their definition, a subject is said to be risk-averse in status quo decision period they prefer to keep their $20 with certainty, rather than to take a chance in one of the low or high-risk options. In order to measure the relative magnitude of participants' aversion to risk, I assign each participant a risk-aversion index. (see Definition 2, below)

DEFINITION 2: RISK AVERSION INDEX

Let R_i be an individual's risk aversion index. Let C_s=1 if an individual chooses Option C (the certainty option) in a given status quo round. If Option C is not chosen we set $C_s = 0$. Then:

$$R_i = \sum_{s=1}^{6} C_s$$

In other words, subject's risk aversion index is the sum of the number of option C's chosen over all status quo rounds. Given that there are six status quo rounds, all participants will have a risk aversion index R_i such that $0 \leq R_i \leq 6$. We say that a person is risk-averse if $R_i > 0$. From this it follows that a greater R_i corresponds to a greater absolute level of risk aversion.

Section 4.4 Restatement of Hypotheses

In this section I restate my hypotheses with direct reference to the experiment design.

Hypothesis 2: 'Experience'

H_0: As participants become more experienced, we will see a reduction in risk taking over losses in either the *simulation* or *return* treatment.

H_A: Preferences over losses are rational and consistent with expected utility. Experience has no effect on the stability of increased risk taking over losses.

Hypothesis 2A: '*Learn-by-seeing'* or *Simulating*

H_0: There will be a significant negative effect of period on risky choice (*choice(i, j)).* As the treatment continues, greater information on the possible outcomes of selecting the chance lotteries in loss rounds will discourage participants from selecting these choices. As a consequence, loss aversion indices in the *simulation* will be significantly lower than those in the *baseline.*

H_A: There is no significant difference in the mean loss aversion index between the *baseline* and *simulation* treatments, and there is no evidence that the number of simulations correlates to less risky choices over loss rounds. The simulation of hypothetical prospects has little effect on the risky choices participants make.

Hypothesis 2B: '*Learn-by-doing' i.e. Returning*

H_0*:* When comparing participant's choices over time, we will see a reduction in risk taking over losses, and significantly lower loss aversion indices. Changes in preferences will only occur for those who were initially loss averse.

H_A: Participants loss aversion indices are consistent over time. (Or, alternatively, the experiment design was unable to provoke an endowment effect in a second session provoking all participants to change their behavior in an unsystematic way. In this case, the choices participants make in the *return* will not be consistent or indicative of true preferences.)

Section 6: Results

Section 6.1: Consistency

In order for my results to have significant interpretive value, it is important to have an indication of choice consistency within subjects. To that end, I create a measure of consistency over both loss and status quo rounds for all subjects within each individual treatment. I say that a subject is consistent if, for example, when a participant chose the high-risk option in the most risky status quo lottery, he chose the riskiest choice for every other status quo lottery. If a subject's appetite for risk is an increasing function of lottery risk over the status quo, he is considered to be inconsistent.

My definition of consistency over loss rounds allows for an individual to be either loss averse or consistent with EUT, which is essential given that not all subjects are loss averse. Accordingly, a subject is considered to be consistent if their risk appetite either increases or decreases with lottery risk. As long as a subject does not unpredictably waiver between high, low and zero-risk outcomes over loss rounds, they are deemed consistent.

Using my measure of consistency I find that choices over the status quo are subject to less variation than choices over loss rounds. These results are consistent with the literature. [(Lopes (1987), Baucells and Villasís (2009)]. According to my measure, approximately 75% of participants are consistent over both loss rounds and the status quo. Of the remaining 25%, 80% are only inconsistent in one decision.[17] Furthermore, only 16% of *simulation* participants and 22% of *baseline* participants were inconsistent. Because of the large number of participants who are mostly consistent, I do not throw out the data from inconsistent participants.

As mentioned roughly 20% of participants are labeled inconsistent because of their choice in one decision period. Because these minor deviations are consistent with human error, and the remaining decisions are largely reflective of an overall predictive pattern, I do not exclude the choices of inconsistent participants from my data set.

[17] For example, there are a number of participants who consistently chose the certainty (zero-risk) option in the status quo, but in one random decision they chose a risky option. Because these participants both increase and decrease their risk preferences as lottery risk increases, they are labeled inconsistent.

Table 2: Treatment Summary

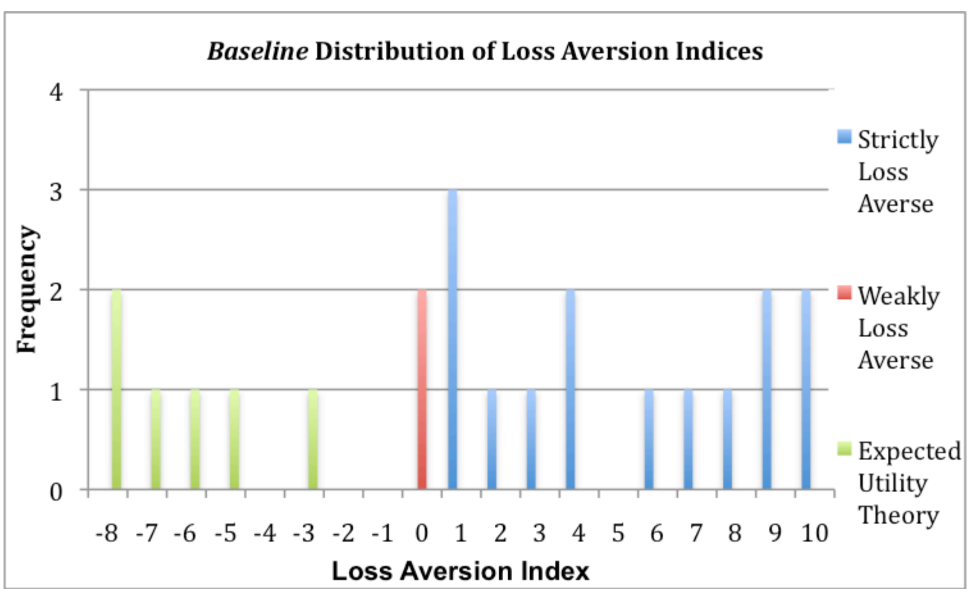
	Baseline	Simulation	Return
Num Participants	22	31	47
Num Sessions	2	3	7
Average Earnings	16.31	17.35	18.02

I ran two sessions for the *baseline,* three sessions for the *simulation*, and seven sessions for the *return* treatment. The *baseline* treatment included 22 participants with average earnings of $16.31 while the *simulation* treatment included 31 participants and had average earnings of $17.35.[18] Of the 53 initial participants, 47 came back for the *return* treatment. Earnings in the *return* treatment were $18.02.

6.2 Initial Loss Aversion

The main hypothesis of this paper (that loss aversion can be attenuated with experience) hinges on the presence of loss aversion in the *baseline* treatment.

Figure 5: *Baseline* Distribution of Loss Aversion Indices



[18] Average earnings are calculated *without* the earnings from the Holt Laury risk aversion survey.

Figure 5 shows the distribution of loss aversion indices for the 22 *baseline* participants. As defined, a subjects loss aversion index is the sum of their risky choices over losses *minus* the sum of their risky choices in the status quo. A subject who took more risk in loss rounds (relative to the status quo) is considered strictly loss averse. As the figure illustrates, roughly three quarters (72.7%) of all participants had a loss aversion index (A_i) of zero or greater in the *baseline* session. The mean loss aversion index was 1.73 indicating that, on average, baseline participants were loss averse.

My results suggest that the tendency towards taking more risk in loss rounds is particularly salient for participants who are also risk averse. That is, while not all risk averse participants are also loss averse, a higher level of risk aversion seems to be correlated with a larger loss aversion index.

Figure 6: Loss and Risk Aversion by Participant

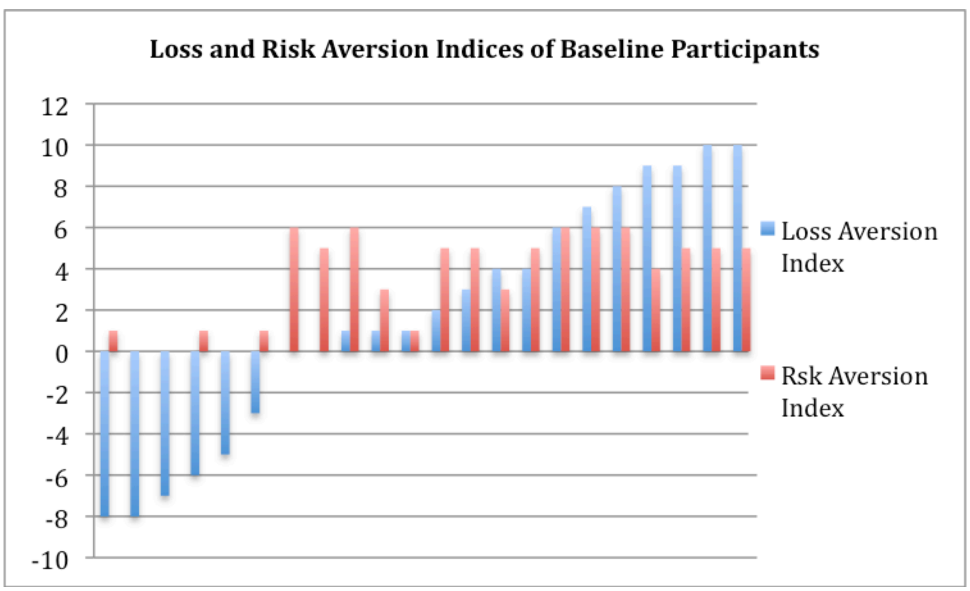


Figure 6 shows the initial risk and loss aversion indices (R_i and A_i) of all participants in the *baseline*. When subjects are arranged in increasing order of their loss aversion indices, we see no participants with a loss aversion index of zero or more have a risk aversion index of zero. Moreover, the figure shows (albeit qualitatively) that participants with larger risk aversion indices tend to be more loss averse, and those who tend to be more risk seeking in the status quo (i.e. those with risk aversion indices near zero) tend to be less loss averse.

In order to test the saliency of these results, I estimate three ordered probit regressions with clustering at the individual level. All regressions in this subsection are completed using index of risky choice (*choice(i_j))* as the dependent variable. All regressions in this subsection also include controls for the independent variables *period,* lottery variance (*variance*), loss round (*isLossRound*) and gender (*isFemale*). In subsequent models I add in controls for risk aversion index (R_i) and an interaction term between risk aversion index and loss round (R_i *_islossround*) in order to determine if a greater risk aversion index is a predictor of loss aversion. Recall, a larger value of *choice(i_j)* corresponds to a riskier choice in a given decision stage. If the regressions support the data in Figure 6, we should expect to see a negative significant coefficient on R_i (consistent with the definition of risk aversion which implies that more risk-averse individuals take less risk) and a positive significant coefficient on R_i *_islossround* (consistent with the notion that greater risk aversion is consistent with greater risk-seeking in loss rounds).

Table 3: Correlation Risk Aversion Index and Loss Aversion Index in *Baseline*

VARIABLES	(1)	(2)	(3)
Period	-0.02	-0.02	-0.03
	(0.017)	(0.019)	(0.023)
isLossRound	0.42	0.46	-1.66***
	(0.326)	(0.350)	(0.339)
Variance	-0.01	-0.01	-0.01
	(0.008)	(0.009)	(0.014)
isFemale	-0.38	-0.05	-0.02
	(0.300)	(0.268)	(0.333)
Risk Aversion Index (R_i)		-0.19***	-0.59***
		(0.043)	(0.058)
R_i_isLossRound			0.64***
			(0.079)
Constant	-0.26	-0.87***	-2.27***
	(0.258)	(0.217)	(0.255)
Observations	264	264	264
Pseudo R-squared	0.03	0.08	0.23

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3 provides the estimated coefficients of these regressions on each of the risky choices of the 22 *baseline* participants.[19] Model (1) shows no predictive value of *period*, lottery variance (*variance*), loss round or gender on risky choice (*choice(i_j)*). A control for initial risk aversion index (R_i) is introduced in model (2), and this coefficient is positive significant. When I allow risk aversion index to vary with loss round (the interaction term *R_i_IsLossRound*) as in model (3,) the coefficient is positive significant, while the coefficient on R_i remains negative significant. The results of model (2) suggest that a more risk-averse individual is less likely to make a risky choice in any decision period. However, model (3) implies that while in status-quo rounds, a higher level of risk aversion is predictive of less risky choices, in loss rounds, greater risk aversion is predictive of *riskier* choices (as suggested by the positive coefficient on *R_i_IsLossRound*). The results of model (3) provide

[19] Recall, each participant made 12 decision period choices, corresponding to 22 * 12 = 264 total observations.

empirical backing for the qualitative results we see in Figure 6. More importantly, these results support Khaneman and Tversky's proposition that a risk-averse person who has not made peace with his losses is more likely than usual to make a risky choice.

6.3 Simulation vs. Baseline Treatment

Having established the presence of loss aversion within the *baseline*, I now move on to a comparison of the *baseline* and *simulation* treatments. Specifically, I test whether the mean loss aversion index differs across treatments, and I test the hypothesis that within the simulation treatment, participants are able to learn from their own experience and make significantly less risky choices in loss rounds. My results suggest that subjects in the *simulation* treatment display significantly less loss aversion. However, I do not find evidence that a greater number of simulations leads to less risky choices over losses.

Figure 7: *Simulation* Distribution of Loss Aversion Indices

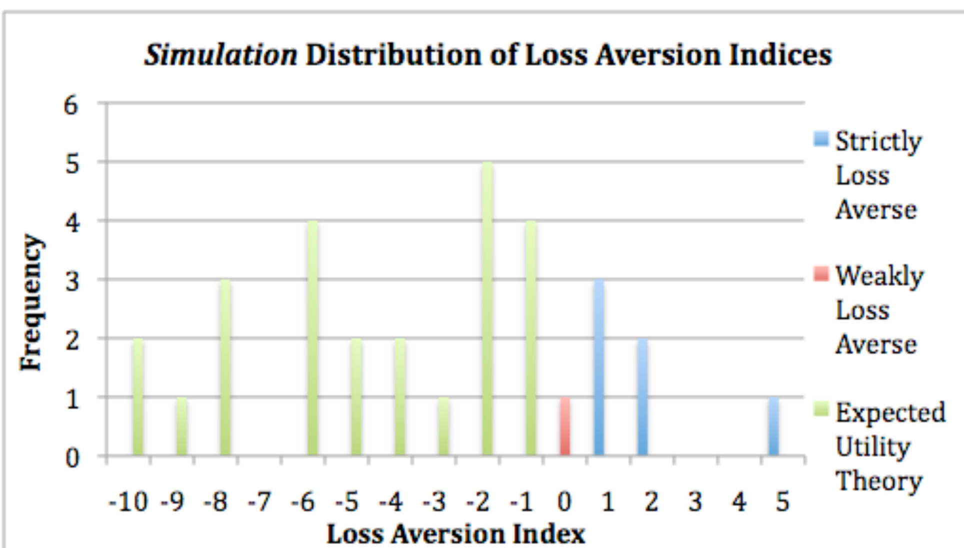


Figure 7 shows the distribution of loss aversion indices for all 31 *simulation* participants. As the figure illustrates, roughly a quarter (22.7%) of participants in the *simulation* treatment had a loss aversion index (A_i) of zero or greater. The mean loss aversion index in this treatment was -3.23, suggesting that on average, *simulation* participants were not loss averse. The difference in loss aversion indices across baseline and simulation treatments is significant at the 1% level. (Prob > |z| = 0.0019)[20]

Notably, there is also a statistically significant difference risk aversion indices across treatments. While the mean risk aversion index in the *baseline* was 3.59, in the *simulation* the mean was only 1.67. This difference is significant at the 1% level. (Prob > |z| = 0.0036) The cross treatment variation in risk aversion is validated by the fact that mean scores on the Holt Laury risk aversion survey do *not* differ across treatments. (Prob > |z| = 0.6723) These results suggest that while overall risk preferences (i.e. Holt Laury scores) do not differ across treatments, there is some aspect of the *simulation* causing participants to seek more risk in status quo decision periods.

To test the hypothesis that participants in the simulation treatment are more loss averse, and are able to learn to make less risky choices with each subsequent decision period, I estimate several ordered probit models with clustering at the individual level. All regressions in this subsection are completed using index of risky choice (*choice(i_j))* as the dependent variable.

The first set of regressions I present in this subsection are estimated in order to determine whether simulation participants make less risky choices in loss rounds, and whether the number of simulations corresponds to a lower likelihood of risky choice. The controls in these regressions follow the same general algorithm as the regressions in the previous subsection. However, because these regressions are estimated using pooled data

[20] Using a two-sample Wilcoxon Mann-Whitney test, we can reject the null hypothesis that the distribution of loss aversion indices is identical across treatment.

from the *baseline* and *simulation* treatments, all models also include a dummy variable to control for treatment. This dummy variable (*treatment*) is equal to 1 if the participant participated in the *simulation* treatment, and 0 otherwise. I later allow *treatment* to vary by loss round (the interaction term *IsLossRound_Treatment*) and *period* (the interaction term *Period_Treatment*). If the estimated regressions are to support the 'simulation' hypothesis we should expect to see a negative significant coefficient on both interaction terms.

Table 4: Cross-Treatment Results for Inexperienced Subjects (Ordered Probit)

VARIABLES	(1) All Rounds	(2) All Rounds	(3) All Rounds	(4) Status Quo Rounds	(5) Loss Rounds
Period	0.01	0.01	-0.02	0.00	-0.05**
	(0.014)	(0.014)	(0.016)	(0.029)	(0.018)
IsLossRound	-0.26	0.41	0.44		
	(0.180)	(0.302)	(0.308)		
Variance	0.00	0.00	0.00	0.00	-0.00
	(0.006)	(0.006)	(0.006)	(0.006)	(0.013)
isFemale	-0.19	-0.20	-0.20	-0.37	-0.04
	(0.179)	(0.187)	(0.188)	(0.224)	(0.265)
Treatment	0.19	0.79***	0.51**	0.67***	-0.99***
	(0.166)	(0.274)	(0.237)	(0.220)	(0.282)
IsLossRound_Treatment		-1.16***	-1.21***		
		(0.337)	(0.351)		
Period_Treatment			0.05*	0.02	0.08**
			(0.027)	(0.040)	(0.034)
is23					0.11
					(0.107)
Constant	-0.25	0.09	-0.07	0.14	-0.76***
	(0.186)	(0.246)	(0.227)	(0.190)	(0.250)
Observations	636	636	636	318	318
Pseudo R-squared	0.01	0.04	0.04	0.05	0.02

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 4 provides the estimated coefficients of the regressions. Model (1) indicates no overarching treatment effect. When only controlling for the bare minimum, no variable is a significant predictor of risky choice. In model (2) I allow lossround to vary by treatment (the interaction term *IsLossRound_Treatment*). With a control for this interaction term, we see a positive significant coefficient on *treatment* and a negative significant coefficient on the interaction term between treatment and loss round. This suggests that participants in the *simulation* treatment make predictably *more* risky choices than their *baseline* counterparts over the status quo, and *less* risky choices over loss rounds. The larger implication of this is that *simulation* treatment participants are less loss averse.

In model (3) I allow period to vary by treatment (the interaction term *Period_Treatment*) to test the hypothesis that as participants have the chance to simulate further outcomes, and *experience* the downside to risky choices, they make less risky decisions. The positive significant coefficient on the interaction term suggests that in the *simulation* treatment participants actually take on more risk in subsequent decision periods. However the results of model (3) do not rule out the chance that participants learn to take less risk with each subsequent loss round, while taking more risk over the status quo.

To test this hypothesis I estimate model (3) using only loss round choices (model (5)). Because model (5) is conducted only over loss rounds, I include a variable to control for the variation in the high-risk loss round lotteries (this variable is denoted by *is23*). The results of model (5) suggest that *period* has a different effect on the loss round choices of participants in the *simulation* and *baseline*. Specifically, the positive significant coefficient on the period treatment interaction term indicates that in the *simulation* treatment, participants take on *more* risk with each subsequent loss round. This directly refutes the hypothesis that simulating outcomes will reduce risky choices over loss rounds. The interpretation of the negative significant coefficient on the *period* variable in model (5) suggests that *baseline* participants are likely to make less risky choices in each subsequent loss round.

The positive significance of the period treatment interaction term in model (5) is only suggestive of increased loss aversion in participants do not increase their risk taking over the status quo. To test this, I run the same model over only status quo rounds (model (4)).[21] The lack of significance on the *period* and the *Period_Treatment* variables in model (4) suggest that period has little predictive effect on behavior over the status quo. Furthermore, with no change in status quo risk taking in each subsequent period, and a positive change in loss round risk taking over each period, this is evidence that participants in the *simulation* treatment, while less loss averse overall, become more loss averse as the session continues.

Table 5: *Simulation* Treatment Results (Fixed Effects)

VARIABLES	(1) Loss Round	(2) Gain Round	(3) Loss Round	(4) Gain Round
period	0.02*	-0.03*	0.02	-0.03*
	(0.014)	(0.016)	(0.016)	(0.016)
variance	0.01	0.00	0.00	0.00
	(0.012)	(0.005)	(0.011)	(0.005)
is23	0.18*		0.18*	
	(0.094)		(0.093)	
CumWinRate	2.12***	0.55		
	(0.801)	(0.917)		
CumLossRate	1.46*	1.17		
	(0.773)	(1.064)		
CumWinRate2			-0.88	0.00
			(0.707)	(0.775)
CumLossRate2			-1.17*	0.87
			(0.701)	(0.769)
Constant	-1.09*	0.81	1.30**	1.16*
	(0.588)	(0.727)	(0.508)	(0.594)
Observations	186	155	155	155
R-squared	0.10	0.04	0.07	0.05
Number of uniqueID	31	31	31	31
R-sq	-0.11	-0.23	-0.20	-0.22

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

[21] This model does not include the *is23* variable because by construction, there is no such variation in the high-risk choice options over the status quo)

To better understand the effects of the simulations on risky choices within the *simulation* treatment, and how they affect risky choice, I run a fixed effects model using only participants from this treatment. In order test the effects of the simulation win and loss rate on choices in the subsequent decision stage I create two variables to control for the cumulative win and loss rates at the time a decision is made. (denoted *CumWinRate* and *CumLossRate*)

The fixed-effects model has significant explanatory power regarding the effects of individual simulation outcomes on subsequent decision stage choices. As shown in Table 5, the cumulative win and loss rate variables are positive significant over loss rounds. In even loss round decision stages we see a positive effect of the cumulative win rate as of the end of preceding loss-round period. This suggests that the number of "winning" simulations in the preceding status quo round has a direct correlation to the amount of risk a subject is willing to take in loss rounds. We see similar results for the effect of the cumulative loss rate on loss round choices. Although classical EU theory suggests that a high loss-rate might induce a participant to take less risk in the subsequent period of investment, our results are quite the opposite. In accordance with prospect theory, the positive significance of the cumulative loss rate (over loss rounds) suggests the presence of loss-aversion *even when the participant has 'experienced' the worst-case scenario option in previous periods.*

Another variable is necessary to determine the effects of the loss and status quo round-specific win and loss rate variables. In model (3) and (4), the variables *CumWinRate2* and *CumLossRate2* control for the win and loss rate over loss or status quo rounds only. In model (3), the negative significant coefficient of *CumLossRate2* suggests that participants *do* reduce their preference for risk after experiencing losses in the loss-round simulation stages. That being said, the positive coefficient on *is23* suggests that participants in the simulation session are still taking more risk in the most risky loss rounds. At best, the combined results of model (3) are *weakly* in support of the hypothesis that experience (in the form of simulations) reduces the presence of loss aversion.

6.4 Return Treatment

In this subsection I present evidence which suggests that a high loss aversion index in initial sessions predicts lower risky choices in second session loss rounds. This result, in tandem with results suggesting that a high initial loss aversion index predicts higher risky choices in second session status quo rounds, is taken as evidence that participants in the return treatment are able to reduce their aversion to losses.

The *return* treatment was designed to test whether loss aversion can be further reduced through a more tangible form of experience. While the results of the *simulation* treatment show that loss aversion is slightly decreased through the generation of hypothetical chance lottery results, the significance of the simulation win and loss rates suggests that the randomly selected hypothetical outcomes had an influence on loss aversion.

With a relatively small initial sample size, it was very important that a large proportion of participants returned for a second session. In order to accommodate the natural variation in participant's schedules, it was necessary to run eight *return* treatment sessions. In the end, the effort exerted during the re-recruitment process paid-off, and 47 of the initial 53 subjects came back for a *return* session.

The *return* treatment had a significant diminutive effect on initial loss aversion. The mean loss aversion number in the *return* treatment was smaller than the mean in both the *simulation* and *baseline* treatments. This suggests that the physical realization of a loss is a better learning tool than the hypothetical simulation of chance outcomes. These results are particularly salient because there is no differential selection into the *return* treatment.

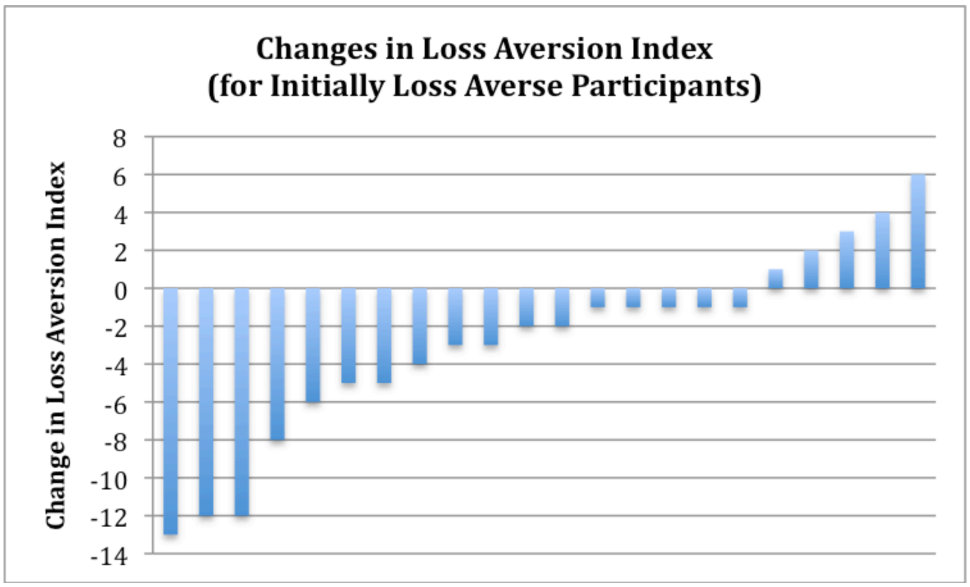
Table 6: Selection into *Return*

VARIABLES	(1)
Risk Aversion Index	-0.08
	(0.167)
Final Initial Payment	0.06
	(0.069)
isFemale	-0.86
	(0.572)
Loss Aversion Index	0.12*
	(0.064)
Treatment	0.12
	(0.573)
Constant	1.06
	(1.220)
Observations	53
Pseudo R-Squared	0.19

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The model presented in Table 6 presents the estimated coefficients from a probit regression of variables which might predict the decision to return, on a dependent dummy variable indicating whether or not participants returned for a second session. This dummy variable I call *return* and set equal to 1 if the participant returned. The statistical insignificance of the coefficients on gender (*isFemale*), Initial Risk Aversion Index (R_i), Initial Final Payment (the amount of money earned in session 1), and *Treatment* (a dummy variable to control for *simulation* vs. *baseline* initial treatment) suggest that none of these variable are a predictor of an individual's decision to return for a second session. While, in a perfect world, the initial amount of loss aversion a subject displayed would not be a significant indicator of the decision to return, we can be reasonably assured that given that 47 of the initial 53 participants chose to return, the significance of initial loss aversion index at the 10% level doesn't fundamentally alter the interpretation of our *return* results.

Figure 8: Change in Loss Aversion Index
(Sample restricted to initially loss-averse participants)



The success of the *return* treatment can be clearly seen in Figure 8, which shows the change in the loss aversion indices for each initially loss-averse participant. While only providing an informal interpretation of the results, the graph shows a clear and visible reduction in initial loss aversion index levels (A_i) for the majority of those who were initially categorized as loss-averse. Of the 22 who were initially loss-averse *and* participated in a *return* session, 17 showed a reduction in loss aversion index after their second session.

Although aesthetically pleasing, the graph above has little predictive value for the change in risky choice (and loss aversion) between treatments. While it is useful to note the overall reduction in mean loss aversion index (A_i) over sessions, the primary goal in analyzing the data from this section is to determine whether or not it is possible to predict risky choices in the *return* treatment by controlling for behavior in the initial session. With this goal in mind, it is most appropriate to use the ordered probit function.

The most straightforward measurement in comparing choices in the same decision problem across sessions is to create a variable *Δchoice(i_j)*[22] equal to the difference in the risky choice index across treatments. In this subsection, *Δchoice(i_j)* is used as the dependent variable in all regressions.

The control variables selected for the model are designed to provide a complete summary of initial session behavior. The session 1 controls include a dummy for initial treatment,[23] initial loss[24] and participant's initial loss aversion indices.

In addition to the session 1 treatment controls, I also include various variables to control for the independent differences in return session lotteries.[25] Given that all *return* treatments include a simulation stage, it is also appropriate to include controls for cumulative win and loss rates.

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[22] Calculated at choice(2, j) – choice(1,j) (i.e. the index of the risky choice a participant made when most recently confronted with the specific lottery minus the index of the risky choice the participant made initially)

[23] The corresponding variable is "wasSimulation," which is 0 if the participant's initial session was the *baseline* treatment and 1 otherwise.

[24] This is calculated relative to the $20 endowment. If a participant left the first session with $17, they have an initial loss equal to $3. (Note: losses are coded as positive and gains are coded as negative, allowing the model to better control for the effect of a large loss on second round behavior.)

[25] As before, these include: a dummy for loss round (or in some cases, status-quo round), period, variance, and gender.

Table 7: *Return* Treatment Ordered Probit Estimation

Ordered Probit Dependent Variable: *Δchoice(i,j)* VARIABLES	(1) All Return	(2) All Return	(3) ILA Return	(4) Not ILA Return
Period	0.00	-0.00	-0.01	0.01
	(0.014)	(0.014)	(0.023)	(0.020)
IsLossRound	-0.27*	-0.36***	-0.05	-0.20
	(0.144)	(0.133)	(0.248)	(0.408)
Variance	-0.02**	-0.02**	-0.01	-0.02**
	(0.007)	(0.007)	(0.009)	(0.010)
isFemale	-0.26**	-0.26**	-0.40**	-0.20
	(0.128)	(0.132)	(0.197)	(0.186)
wasSimulation	0.04	0.04	-0.23	0.37**
	(0.146)	(0.151)	(0.249)	(0.149)
InitialLoss	-0.03***	-0.03***	-0.03	-0.03**
	(0.011)	(0.011)	(0.019)	(0.016)
Cumulative Loss Rate	1.03	0.98	0.82	1.33
	(0.706)	(0.731)	(1.207)	(0.814)
Cumulative Win Rate	0.57	0.57	0.53	0.77
	(0.657)	(0.674)	(0.948)	(0.703)
Initial Loss Aversion Index	-0.02**	0.04*	0.04	-0.01
	(0.011)	(0.019)	(0.051)	(0.051)
Initial Loss Aversion Index_islossround		-0.11***	-0.17***	-0.07
		(0.025)	(0.058)	(0.064)
Constant	-1.30**	-1.45***	-1.58**	-0.84
	(0.513)	(0.525)	(0.772)	(0.658)
Observations	517	517	242	275
Pseudo R-Squared	0.03	0.05	0.08	0.04

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Model (1) of Table 7 shows the estimated coefficients from an ordered probit regression of *Δchoice*(*i_j*) (choices over initial and return sessions) while controlling for first round behavior (*wasSimulation*, *InitialLoss* and the initial value of the loss aversion index ($A_{i,1}$)), constant characteristics (*isFemale*) and the independent characteristics of second session lotteries (*variance*, *period,* loss round (*islossround)*, loss rate (*CumLossRate)*, and win rate). The negative significant coefficient on the initial loss aversion index in model (1) suggests a correlation between higher initial loss aversion and a decrease in risky choices in the return session. Meanwhile the negative significant efficient on loss round also suggests that participants in the return session made less risky choices over loss rounds.

Model (2) is almost identical to model (1), except for the addition of and interaction term between loss round and initial loss aversion index ($A_{i,1}$). The negative significant coefficient on this interaction term, in tandem with the positive coefficient on initial loss aversion index implies that when a participant returns for a second session we can predict a negative significant change in their risky choice in loss rounds if they were initially loss-averse. While it makes sense intuitively that a larger variance be associated with a lower level of risk taking, there is no existing intuitive explanation for the negative significance of being female on return session behavior. The results of models (1) and (2) do not rule out the possibility that all participants make less risky choices when they return for a second session. In order to determine whether the return session has a distinct effect on those who were initially loss averse, I run models (3) and (4).

When I restrict the sample to only those participants who returned and were initially loss averse (as in model (3)), different results arise. Specifically, the results of model (3) suggest that while initially loss-averse participants make riskier choices in loss rounds, when they return, they make less risky choices in these same loss rounds. These results are consistent with those of models (1) and (2).

The most important results of Table 7 are shown in model (4). In model (4) I limit the regression to those participants who were *not* initially loss averse. I do this in order to test whether there is a difference between the *return* session behaviors of initially loss averse and non loss-averse participants. The relatively insignificant coefficient on *islossround* in this model suggests that in loss rounds, non-loss-averse participants take do not predictably alter their risky choices. This adds significance to the main results, which would have had much less interpretive value if all participants had reduced their risky choices in loss rounds.

Given that the *simulation* results are indicative of a simulation treatment effect, it is necessary to examine the *return* results for *baseline* and *simulation* participants individually. In these models, I use the same general algorithm as in Table 7, but restrict the observations according to initial treatment. If returning has an effect on initially loss averse participants from both initial treatment groups we should expect to see a negative coefficient on the interaction term between initial loss aversion index and loss round. The estimated coefficients of these models are presented in Table 8.

Table 8: *Return* Treatment Ordered Probit Estimation by Initial Treatment

Dependent Variable: *Δchoice(i,j)*	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Baseline	Baseline ILA	Baseline *Not* ILA	Simulation	Simulation ILA	Simulation *Not* ILA
Period	-0.00	-0.03	0.07	0.00	0.04	-0.01
	(0.022)	(0.025)	(0.050)	(0.019)	(0.052)	(0.021)
isLossRound	-0.17	-0.01	3.05***	-0.22	-0.67*	-0.44
	(0.204)	(0.295)	(0.833)	(0.235)	(0.402)	(0.431)
Variance	-0.00	-0.00	0.01	-0.03***	-0.03*	-0.03***
	(0.008)	(0.011)	(0.017)	(0.010)	(0.017)	(0.012)
isFemale	-0.17	-0.33		-0.31*	-0.38	-0.20
	(0.203)	(0.246)		(0.161)	(0.352)	(0.187)
InitialLoss	-0.05**	-0.06**	-0.05*	-0.02	0.03	-0.03*
	(0.025)	(0.026)	(0.029)	(0.013)	(0.020)	(0.018)
CumLossRate	0.14	0.51	1.82	1.45*	3.36	1.28
	(1.252)	(1.597)	(1.726)	(0.859)	(2.578)	(0.887)
CumWinRate	0.02	0.10	2.63**	0.55	-0.23	0.53
	(1.306)	(1.489)	(1.264)	(0.715)	(0.850)	(0.794)
Initial Loss Aversion Index (A_i)	0.06**	0.03	-0.39***	-0.02	-0.01	0.01
	(0.029)	(0.049)	(0.059)	(0.022)	(0.051)	(0.055)
A_i _isLossRound	-0.15***	-0.19***	0.37***	-0.04	0.28***	-0.09
	(0.038)	(0.064)	(0.123)	(0.044)	(0.087)	(0.072)
Constant	-1.92*	-2.30**	3.07*	-1.24**	-0.11	-1.74***
	(0.988)	(1.068)	(1.573)	(0.533)	(1.338)	(0.616)
Observations	220	165	55	297	77	220
Pseudo R-Squared	0.09	0.12	0.14	0.04	0.08	0.04

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Model (1) of Table 8 shows estimated coefficients of an ordered probit regression on the difference in risky choices between return and initial sessions (*Δchoice*(*i_j*)) while controlling for first round behavior, gender, and the independent characteristics of second session lotteries. The negative significant coefficient on initial loss aversion index suggests that higher initial loss aversion is a predictor for riskier choices in status quo decision periods during the return session. Meanwhile the negative significant efficient on the interaction term between initial loss aversion index and loss round (A_i*_isLossRound*) also suggests that participants in the return session made less risky choices over loss rounds. The negative significant coefficient on initial loss in model (1) (and also in models (2) and (3)) is consistent with the notion that participants who left their initial session with less than $20 incurred a disutility which led them to make less risky choices in their second session.

In model (2) I restrict the sample only to *baseline* participants who were initially loss averse. Again we estimate a negative significant coefficient on the interaction term between initial loss aversion index and loss round (A_i*_isLossRound*). Qualitatively, this allows us to further restrict the results of model (1), in showing that initially loss averse participants make less risky choices in loss rounds, and the degree to which this is true depends on the value of the initial loss aversion index.

In model (3) I estimate the same model using data for *baseline* participants who were not initially loss averse. The positive coefficient on loss round is interesting, and suggests that participants who were not initially loss averse made riskier choices in loss rounds during their return session. The positive coefficient on cumulative simulation win rate (*CumWinRate*) suggests that while the number of winning simulations was not a predictor of risky choice for initially loss averse participants, those participants who were not initially loss averse made riskier choices if good outcomes were randomly generated during the preceding simulation stage. The negative coefficient on initial loss aversion index in model (3) suggests that closer a participant's initial loss aversion index was to zero, the more likely they were to make less risky choices in return session status quo rounds. Similarly, the positive significant coefficient on the interaction term between initial loss aversion index and loss round suggests that participants will loss aversion indices closer to zero made riskier choices in *return* treatment loss rounds. The results of models (1), (2) and (3) are consistent with the results of Table 7, and provide evidence that *baseline* participants reduced their tendency towards loss aversion in the *return* treatment.

When interpreting the *return* treatment effect for baseline participants, the result from section 6.3 which suggest that simulations have a decreasing effect on risk aversion cannot be ignored. Recall that the difference in risk aversion indices between *baseline* and *simulation* treatments was statistically significant, but the difference is Holt Laury scores was not. These results were interpreted as evidence that while overall risk preferences do not differ across treatments, there is some aspect of the *simulation* causing participants to seek more risk in status quo decision periods. This result is directly refuted when looking at the difference in risk aversion indices of *baseline* participants between initial and return treatments. Specifically, while the mean risk aversion indices of *baseline* participants was 3.59 in their initial session and 3.2 in their second session, the difference was not statistically significant. (Prob > |z| = 0.2251) Additionally, there was no cross session difference in Holt Laury risk aversion score either. These results are indicative of the noisiness inherent in the risk aversion survey.

In Models (4), (5), and (6) of Table 8 the sample is restricted to participants whose initial treatment was *simulation*. All of these models estimate a negative significant coefficient on variance. While there is little intuition as to why variance (of the low-risk gamble) is only significant for participants whose initial treatment was *simulation*, the negative coefficient is nonetheless consistent with risk aversion. A greater variance of the low risk gamble is a predictor for less risky choice.

In model (4) the negative significant coefficient on gender (*isFemale*) suggests that women make less risky choices in the return session. This result is consistent with the models (1), (2) and (3) of Table 7. The positive significant coefficient on cumulative loss rate suggests that a higher cumulative loss rate is indicative of riskier choices.

In model (5) I restrict the data to only include the choices for the 7 participants who were initially loss averse in the *simulation* treatment. While I estimate a positive significant coefficient on the interaction term between initial loss aversion index and loss round, I also estimate a negative significant coefficient on loss round. That is, while initially loss averse participants made less risky choices over loss rounds when they returned, participants who were initially more loss averse actually made riskier choices in return session loss rounds. Interestingly, in model (6) I do not estimate any significant change in loss round risky choices for participants who were not initially loss averse. Unlike in model (3), in model (6) I do not estimate a significant coefficient on loss round for participants who were not initially loss averse.

The results of Table 8 both reaffirm the effectiveness of the *simulation* treatment and weakly support the success of the *return* treatment. Ideally, I would have been able to run a return session for baseline participants in which I did not treat them with simulations. However, the existence of a weak return effect for initially loss averse participants is encouraging particularly because of the extremely small sample size (only 7 return participants from the simulation treatment were initially loss averse).

Section 7: Discussion

This experiment was run in order to test the saliency of reference dependent loss aversion with two different forms of experience. The results suggest that both types of experience can lead to less loss aversion. I find strong support that the hypothetical simulation of outcomes associated with a risky gamble make participants less likely to seek an uncharacteristic amount of risk over losses. While the results regarding the *return* treatment are less decisive, they also support the hypothesis that loss aversion can be reduced. Taken together the combined results of this paper suggest that the risk preference structure of prospect theory may not be stable over time.

Before embarking on a more contextual discussion of the results, it is worth emphasizing several key aspects of the experiment design which made possible the repeated study of preferences over losses. In general, studying behavior over losses is not an easy task. Because of this difficulty, experiments dealing with losses have, until now, largely been carried out in a hypothetical context. In these 'hypothetical' choice experiments, participants were asked to indicate their preferences between two outcomes involving losses, but were not paid on the basis of their decisions. Notably, there has been one experiment which gave participants the option to experience true losses (and gains), but few participants opted for this option. Thaler and Johnson (1990). In their experiment, they asked participants to play with their own money even though there was the potential to make a loss. While Thaler and Johnson took steps to encourage participants to participate, only 1/3 of their participants were willing to play for real money[26], and these 1/3 proved to be decidedly risk seeking over all choices. Eventually the authors elicited greater amount of participation, but to do so they had to drastically reduce the possibility of a loss, making choices over losses less reliable. My experiment presents a clear improvement to previous designs by giving participants an initial endowment, convincing them it is theirs, and then asking to make decisions in the case that some of the endowment has been taken away. Furthermore, by paying participants on the basis of one randomly chosen period, the choices made are more likely to reveal true preferences.

Part of the difficulty in studying behavior over losses, and why it has been largely absent in laboratory experiments, is the psychological nature of reference dependent loss aversion. For my experiment to be successful in eliciting true preferences, it was absolutely necessary to engender an endowment effect amoung participants. The key steps which aided in this process were key to the success of my experiment. By giving participants their endowment initially, asking them to count the money and asking them to detail their expected use of the money, I ensured that the $20 endowment had been factored into their status quo wealth at the beginning of the experiment. To ensure that their perception of the status quo remained consistent throughout the repetition of choices over induced losses, these loss round decisions were sandwiched between decisions with an expected value equal to $20, and every decision period began with a reminder of the $20 initial endowment. Not only did this set up yield choices which were largely consistent, showing that the choices made were indicative on true preferences rather than the random

[26] The other 2/3 made decisions over outcomes on a hypothetical basis.

selection of options, but, in the case of the 47 participants who returned for a second session, the experiment design enabled me to do this *twice*. It would be difficult to determine which of these measures was most effective, however, the result remains, that when taken together, the measures served to elicit an endowment effect and allowed for a laboratory study of true preferences over losses.

Given that the risk preference disparity of loss aversion is inextricably linked to the value disparity of the endowment effect, the results of this study provide support for List's (2003) results that market experience attenuates the endowment effect. As List found, traders in the sports card market increased their trade frequency as experience increased. However, List himself admits that he is unable to positively determine whether those who chose to continue their participation in the market were somehow different from those who exited the market. In other words, List's experiment had no way of exogenously imposing an experience treatment. In designing an experiment which allowed for treatments to be randomly imposed, I was able to overcome the bias of self-selection. Furthermore, my work can be thought of as a supplement and extension of List's work, because I was able to show that not only does experience reduce the tendency towards uncharacteristic risk in loss rounds, but, for those who weren't initially loss averse, the treatment had little effect.

List and Haigh (2005) apply the results of List (2003) in an experimental test on the differences in myopic loss aversion between experienced traders and students. Myopic loss aversion is a combination of mental accounting (where people weigh the present and discount the future) and loss aversion. Myopic loss aversion (MLA) results in decreased risk taking when long-term assets are evaluated with respect to short-term profits or gains.[27] Lower feedback frequency and longer length of financial commitment have been show to decrease MLA, and increase an investor's willingness to invest in a risky asset. [Langer and Weber (2006); Fellner and Sutter (2009)] In testing the impact of experience on MLA, List and Haigh find support for the claim that professional traders exhibit *more* MLA than students. The authors mistakenly interpret their results as evidence that experience has little effect on myopic loss aversion. Could it not be simply that experience in one realm is not easily transferred to another context? While the traders are able to minimize loss aversion in a professional context, does this imply that they should be able to realize sunk costs and be unaffected by losses in all contexts?

While my experiment is a supplement to List (2003) and List and Haigh (2005) and other papers on the topic of loss aversion and experience, there is ample room for further experiments in this field. Further experiments should be conducted to test the cross-contextual application of experience on risk-taking over losses. Although List and Haigh (2005) seems to suggest that perhaps learning cannot be transferred from one realm to another, further experiments should test this hypothesis specifically in relation to the endowment effect and divergent risk preferences over losses. The lack of a cross-contextual understanding however, should not overshadow the results presented herein. This paper has shown that experience can attenuate the risk-preference disparity in a closed laboratory environment. Had this not been the case, there would be little hope that experience in one realm, can have an effect on choices made in a separate context.

[27] The concept of myopic loss aversion (MLA) was first introduced by Benartzi and Thaler (1995) as a way to explain the equity premium puzzle. The author's argue that the disproportionate return on equity in the stock markets can be explained through a combination of mental accounting (weighting the short term more than the longer term) and disproportionate aversion to losses. In recent years, their work has been expanded and refined by several authors who argue that while MLA accounts for part of the equity premium puzzle, several other factors are missing from their original model. [Barberis et al. (2001); Barberis and Huang (2001); Grune and Semmler (2008)]

Section 8: Conclusion

In recent decades, there have been many behavioral studies surrounding changes in behavior which occur when people are faced the prospect of a loss. Loss realization aversion, the endowment effect, and increased risk taking in order to avoid a loss, are examples of such behaviors, all of which are incongruous with expected utility theory. While economists have succeeded in explaining these behaviors in the context of a reference dependent model of utility, the stability of these reference dependent preferences have not been adequately investigated.

The experiment described herein, fills a distinct gap within the existing literature, and provides evidence that with experience, people can learn to accept their losses and forgo unnecessary risk-seeking behavior when faced with a loss. Through both the simulation and repetition of risky choices over losses, otherwise risk-averse subjects became less likely to exhibit risk-seeking behavior when faced with a loss.

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Appendix A: Procedural Forms and Documents
Item 1: Subject Consent

SUBJECT CONSENT FORM

RESEARCH PROCEDURES:

This research is being conducted to study the economics of decision-making.

This experiment will last approximately 60 minutes. You will receive $20 upon your arrival to this study. $5 of this $20 is a show-up fee. Upon completion of the session you will receive an additional payment which will depend both on the decisions you make within the experiment and on chance. Earnings from decisions you make during the study will only be paid upon completion of the experiment. If you choose to withdraw early, you will keep the $5 show-up fee. You are free to ask any questions regarding the method of your payment.

Following the experiment, you will be responsible for adjusting the $20 according to the value of your earnings. The value of your earnings will be known only to you, the experimenter and the faculty supervisor.

PARTICIPATION:

You must be 18 or older to participate. Please understand that your participation is voluntary and that you may withdraw from the study at any time and for any reason.

CONFIDENTIALITY:

All electronic files will be saved confidentially on a password-protected and access-restricted local area network. No person-identifiable information will be reported in any published or unpublished work. Access to data will be restricted to Elizabeth Kapnick and Professor Robert Gazzale.

CONTACT:

This study is being conducted by Elizabeth Kapnick, under the supervision of Professor Robert Gazzale PhD. Please feel free to contact us with any questions or complaints. Elizabeth may be reached at 347 622 0698, and Professor Gazzale at 413 844 5300. You may also contact Professor Steve Sheppard, Chair of the Williams College Economics Department (x3184). This research has been received and approved by the Internal Review Board and the Economics Departmental Review Board of Williams College.

CONSENT:

If you have any questions, please ask them now. Otherwise, please read and sign the statement below. You will receive a copy of this consent.

I hereby declare that I have read the above information, and give my informed consent to participate in this study.

Signature: ______________________ Date: ______________________

Item 2: Acknowledgement of Previous Earnings Form (Sample)[28]

I Elizabeth Kapnick hereby recognize that my final profit at the end of the last session was $16.

Signature: ______________________

Date: ______________________

[28] Attached to Subject Consent Form in *Return* session

Item 3: *Simulation* and *Return* Treatment Instructions (Paragraph 5 was not included in *Baseline* Instructions)

INSTRUCTIONS:
You are about to participate in a session in which you will make a series of choices. This is part of a study intended to provide insight into certain aspects of how people make decisions. If you follow the instructions carefully, read the onscreen instructions carefully, and make good decisions, you may earn a considerable amount of money. When you complete this session, you will leave with the cash that you have earned.

During the experiment, I ask that you please do not talk to each other. If you have a question, please raise your hand and an experimenter will assist you.

In this computer-based session, you will make a series of choices between options. The $20 you received upon entering the room is your endowment with which to participate in this session.

In each of 12 decision periods, you will choose between three options regarding your $20. The payout from two of these options will depend on chance. If you choose an option whose outcome depends on chance, you will not learn the outcome of this choice until the end of the experiment.

After you have made a choice in each decision, you will be asked to make six simulations. For each simulation, you will be asked to choose one of the options whose outcome depends on chance. The computer will then randomly select one of the outcomes for that option according to the probabilities associated with each outcome. You will be asked to simulate each option whose outcome depends on chance three times. The results of these simulations in no way impact your final pay, rather these simulations provide information on the outcomes that might happen if you chose one of the options whose outcomes depends on chance.

After you have completed all twelve decision and simulation periods, you will be asked to take a brief survey intended to provide further insight into certain aspects of how people make decisions. Your earnings from this part of the session will be added to your final earnings.

The last stage of the session is the payment stage. During this stage the computer will randomly select one decision period for payment. If in this randomly selected decision period you chose an option whose outcome is determined by chance, the computer will choose an outcome in accordance with the probabilities indicated. That is, if the probability of outcome number 1 is 40%, the probability that the computer selects this outcome will be 40%. Your payment for participating in this session is the outcome of your choice in the randomly chosen decision period.

There should be $20 in the envelope you received upon entrance into the computer lab. Please open the envelope and count the money inside. If you do not have $20, please raise your hand and someone will assist you immediately.

Before we begin the experiment, please indicate how you intend on spending your $20. Your input is highly relevant to the study, so please be as detailed as possible.

My $20 will be spent on:____

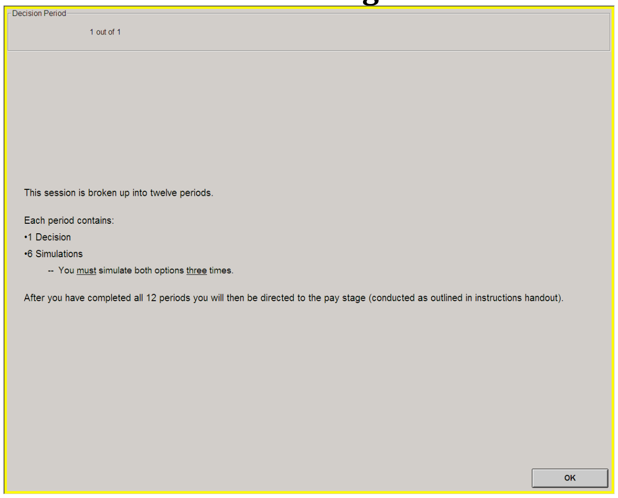
____

____

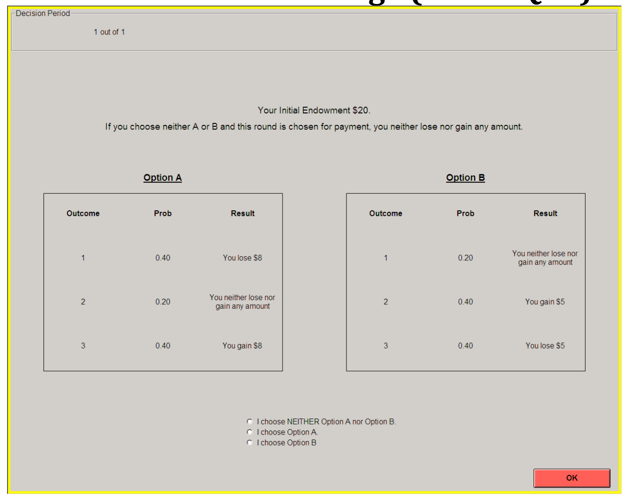
If you have any questions please ask them now. Otherwise, please direct your attention to the computer screen in front of you.

Appendix B: Screen Shot Directory

Screen 1: Instruction Stage

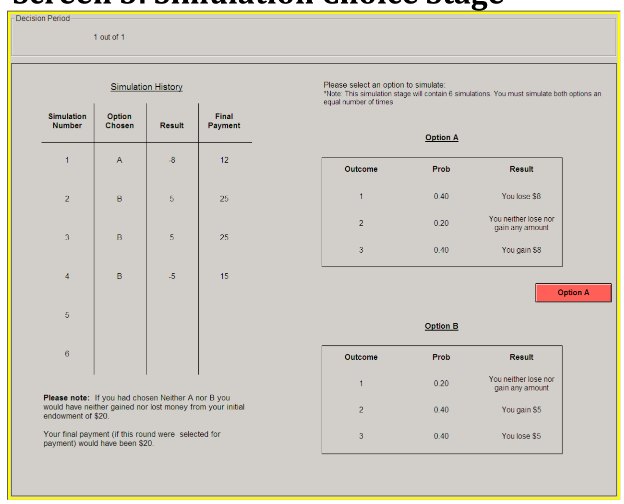


Screen 2.1: Decision Stage (Status Quo)

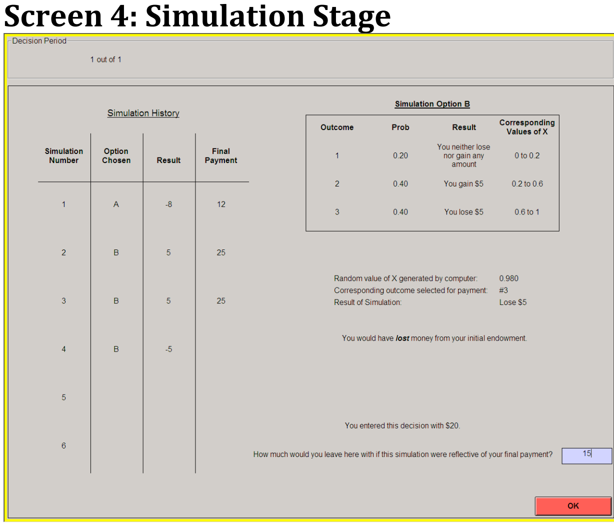


Screen 2.1: Decision Stage (Loss)

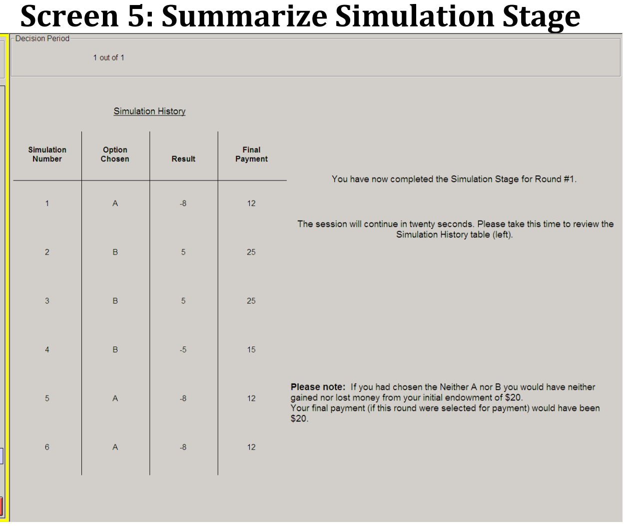
Screen 3: Simulation Choice Stage



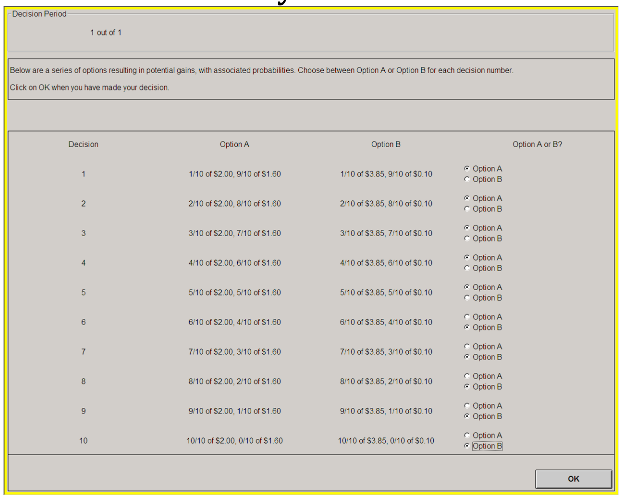
Screen 4: Simulation Stage



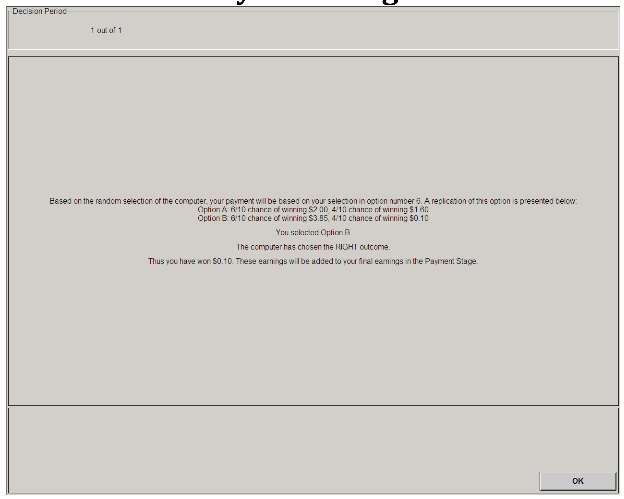
Screen 5: Summarize Simulation Stage



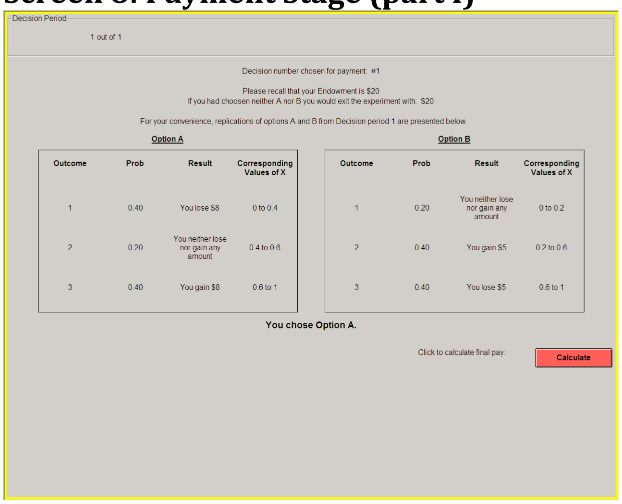
Screen 6: HL Survey



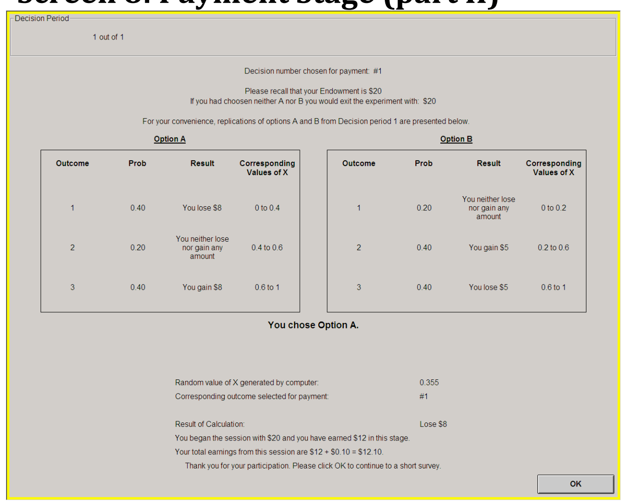
Screen 7: HL Payment Stage



Screen 8: Payment Stage (part I)



Screen 8: Payment Stage (part II)



Questions Asked on Demographic Survey:

First Name:
Last Name:
Williams ID:
Gender:
Class Year:
Age:
GPA:
Major:
Have you ever taken a course on probability or statistics?
Have you ever taken Behavioral Economics?
How clear were the instructions on a scale from 1 to 10?
What (if any) were your thoughts about the lottery choices?

Appendix C: Additional Results

Figure A: