AMBIGUITY AVERSION AND ITS EFFECTS ON SAMPLE SELECTION FOR LABORATORY ECONOMIC EXPERIMENTS

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ABSTRACT:
The use of laboratory environments by economists to test and modify theory has steadily increased in recent years. Self-selection into the laboratory by subjects is one potential problem that may affect the external validity of results found in the laboratory. Walking into a laboratory environment, individuals know little information as to what will occur. In this study, I designed an experiment to determine whether ambiguity aversion can cause individuals to not participate in laboratory experiments. I find that more ambiguity averse individuals are significantly less likely to participate in laboratory sessions when sent a vague solicitation email commonly used by economists. In laboratory settings designed to estimate parameters, this selection has the potential to skew such estimations. My results suggest that economists should take careful consideration as to what may affect selection when designing their laboratory experiments.

1 INTRODUCTION

Walking into a laboratory environment, individuals usually know little information as to what will occur. Given this fact, what could play a large role in an individual's decision to attend an experiment or not is the *ambiguity* of the information presented, which according to Ellsberg (1961) can be considered "a quality depending on the amount, type, reliability and 'unanimity' of information." Specifically, ambiguity is the uncertainty about what will actually occur during the experiment created by missing information. Ellsberg (1961) proposed and demonstrated that when the information given to an individual was highly ambiguous, the otherwise reasonable individual might not take actions that conform to the axioms of the subjective expected utility model.

It seems reasonable that some individuals who are more ambiguity averse may systematically choose not to attend laboratory sessions at all. Furthermore, standard recruitment emails and other solicitation methods are generally vague as to what tasks will be performed during the experiment, what the goals of the research are, and how much money one will actually earn by participating.[1] Thus, it also seems reasonable that individuals who are more averse to ambiguity are less likely to participate in laboratory experiments when they receive an amorphous invitation to participate. I examine these important selection effects by testing the following questions: (1) are more ambiguity averse individuals less likely to participate in experiments in general than less ambiguity

[1] In the past, economists have supported their use of vague solicitation mechanisms for three main reasons: (1) to avoid selection, (2) to avoid having the research agenda affect the actions of the participants, and (3) to avoid payment expectations affect the actions of the participants. However, as this paper demonstrates, vague solicitation mechanisms can actually cause self-selection by inducing more ambiguity averse individuals to not participate.

averse individuals, (2) are more ambiguity averse individuals less likely to participate in experiments than less ambiguous averse individuals when the invitation used to solicit participation is similar to the vague invitations that economists commonly use, and (3) does self-selection affect outcomes in the laboratory?

The problem of self-selection threatens to completely derail experimentation if it indeed is a problem that cannot be controlled. While many authors have endeavored to address the problem of self-selection, few have sought to design an experiment to determine whether self-selection into the experiment does indeed happen. The experiment that I ran has two rounds to determine what, if any, effect ambiguity aversion has on the individuals who choose to participate in experiments. For this study to have meaning in regards to the use of experiments in economics, I have designed my study to closely replicate the typical laboratory experimental environment found in economics. That is, in all phases subjects' pay was tied to their actions in each session and the students were selected from the populations that economists generally draw from, both economics classes and the campus at large. In the first round of the experiment, I presented modified series of urn questions based on Ellsberg's canonical urn problem to a sampling of Williams College students using a protocol that mitigated the issue of self-selection. Working with Ellsberg's description of ambiguity averse behavior and the subjective expected utility model, I categorized individuals in Round 1 into one of three groups: (1) more ambiguity averse, (2) less ambiguity averse, and (3) individuals who responses were inconsistent with either theoretical framework[2]. The individuals in the third category did not respond in a way that could neither be considered "rational" under the subjective expected utility model nor ambiguity averse as explained by Ellsberg.

In the second round of the experiment, subjects categorized in Round 1 are sent

[2] For a full description of the subjective expected utility model I refer the reader to Ellsberg (1961). A concise description of how ambiguity aversion violates the subjective expected utility model is presented later in the paper.

one of four invitations varying in detail across two categories: (1) the amount of information presented in the email regarding how much the subjects are compensated for attending, and (2) the amount of information presented in the email regarding what tasks the subjects are asked to perform in the laboratory. Subjects were randomly placed orthogonally into each of the four email treatments. Who responded to each treatment by showing up to Round 2 is then observed. As predicted, more ambiguity averse students are significantly less likely to attend the session when they are sent the baseline ambiguous email that closely matched standard solicitations where I provide little information about the tasks attendees will perform and the exact compensation that the attendees can expect to receive. In terms of mitigating self-selection along preferences for ambiguity, my results suggest that economists performing laboratory studies should take careful consideration in drafting solicitation mechanisms. By finding that self-selection into laboratory settings occurs, this study further emphasizes the need for economists to be vigilant in considering how self-selection may affect their experiments.

This paper proceeds as follows. Section 2 describes the related research and motivation to my study. Section 3 presents my experimental design. Section 4 presents my results. Section 5 concludes.

2 BACKGROUND

Ever since its inception, the scientific method has relied on experimentation as its cornerstone. One of the very first proponents of the scientific method, Galileo described the necessity of the experiment in his own words: “in those sciences where mathematical demonstrations are applied to natural phenomena, as is seen in the case of perspective, astronomy, mechanics, music, and others, the principles once established by well-chosen experiments, become the foundations of the entire superstructure"(Settle, 1961 p. 19). Indeed, Feyman (1963) emphasizes this fact when noting that, "the principle of science, the definition almost, is the following: The test of all knowledge is experiment. Experiment is the sole judge of scientific 'truth.'"

As Galileo emphasizes, a critical maintained assumption underlying laboratory experiments is that the insights gained from such research in the laboratory can be extended to the real world, a principle come to be known as generalizability or external validity. The results in the laboratory to date when studying the physical world have supported this assumption of generalizability. That is, what occurs in the laboratory is similarly has been found to occur in the natural world, which leads to the fair statement by Shapeley (1964, p. 43) that "as far as we can tell, the same physical laws prevail everywhere."

In their pursuit of universal economic "truths," economists increasingly use the traditional experimental model of the physical sciences to understand human behavior. Holt (2005) documents that experimental economics is a "boom industry," showing that publications using the methodology were almost non-existent until the 1960s and have since grown exponentially, surpassing 50 annually for the first time in 1982, and by 1998 there were more than 200 experimental works published per year.

Yet while experimentation in the laboratory has become increasingly popular among economists, a number of criticisms have questioned the extent to which laboratory experimentation produces externally valid results. At the very minimum, for a laboratory experiment to produce externally valid results the behavior of a sample of subjects who participate in a particular experiment must be representative of the larger population, whether that larger population be a particular population of interest to the experimenter or the larger population in general. However, economists cannot force people to participate in experiments. Individuals decide whether they will participate or not. This decision is indeed a form of self-selection. The fact that individuals self-select to participate in experiments leaves open the very real possibility that they self-select systematically, which creates the possibility that they are unrepresentative of the larger population. This concern warrants testing to determine whether systematic self-selection into laboratory experiments does indeed occur.

Since the study of behavioral economics relies to a large degree on laboratory experimentation, the study of sample selection is of particular importance to this area of economics. Behavioral economists conduct games in the laboratory to determine if actual behavior departs from theoretical predictions. However, experimental and behavioral economics are not necessarily synonymous fields of study. By its very definition, behavioral economics is the combination of psychology and economics that investigates what happens in markets in which some of the agents display human limitations and complications (Mullainathan and Thaler, 2000). In general, behavioral economics refers to any study which either seeks to relax the assumptions of perfect rationality and narrowly-defined self interest as defined by classical or neo-classical economists, or tries to quantify the effects of such a relaxation. While many experimental studies fit in this framework, some are not entirely behavioral. What makes a study behavioral is a matter of debate.[3]

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[3] Indeed, many economists maintain that all economics is behavioral. However, for my purposes I maintain the definition of behavioral economics to be the field of study that seeks to relax the assumptions of perfect rationality and strict self interest, and model those relaxations, as defined by standard economic theory.

Long before laboratory experimentation was fully embraced, classical economists traditionally conceptualize the world as being populated calculating, unemotional utility maximizers dubbed Homo Economicus (Pareto, 1971). As Mullainhathan and Thaler (2000) point out, the Homo Economicus model has been defended along two main threads of logic: some claimed that the model was "right" (enough), while most others argued that it was easier to formalize and practically more relevant to understanding how most people maximize their utility in markets. Beginning with Allais, the very foundation of behavioral economics has been anchored in the belief that neither rationale was true. For example, empirical and experimental evidence has grown against the axioms of subjective expected utility theory. The controlled environment that the laboratory provides allows for insights into behavior that field-study could not have easily provided. It permits economists to answer questions that are hard to attend to in the complex, perpetually changing real world of the field.

Still, field experiments do contain advantages over laboratory study.[4] Field experiments have many advantages over laboratory experiments in regards to the fact that field experiments have a certain "realness" that laboratory experiments lack: the stakes in such experiments more closely emulate real world is just one advantage that is commonly pointed to when arguing for the benefits of field experimentation. Another advantage is that participants in field experiments are already self-selected to some extent. Those individuals who participate in certain markets are not a random sample of the population. Thus, if one were interested in say the behavior of commodities traders in commodities markets one would be better studying the traders' behavior within their market rather than bring in a random sample of individuals to the laboratory and have the sample play

[4] I define a field experiment as an experiment that uses the scientific method to measure the effects of an intervention in a naturally occurring environment. See Harrison and List (2004) for full taxonomy.

games that emulate the commodities market. While the benefits of field experimentation comes from the "realness" of such an approach, that "realness" also contributes to the many detrimental aspects of field experimentation. Indeed, the "realness" of field experimentation can often limit the extensions of the findings. Studying loss aversion in commodities markets would probably provide little insight into how loss aversion affects, doctors, teachers, or politicians. In the field, one cannot always control for or necessarily identify confounding variables that may affect the outcome of such an experiment. In the laboratory, such effects are more easily identified and controlled.

Experimentation in the laboratory is by definition controlled and artificial which creates a number of weaknesses for extrapolating real world implications from data collected by such a method. Critics have called into question whether results found in the laboratory are applicable to "real people" performing "real tasks" in the "real world" (Harrison and List, 2004; List and Levitt, 2005). Nevertheless, Smith (1976) views laboratory experiments as necessary pretests of economic theory and argues that laboratory experience suggests that all of the characteristics of "real world" behavior that we consider to be of paramount importance, such as self-interested motivation, interdependent tastes etc., arise naturally, indeed inevitably in experimental settings. Smith's *parallelism* states that theories ought to hold in environments that do not explicitly violate the underlying assumptions in the model. Thus, if theoretical predictions do not hold in the laboratory, than the underlying assumptions of the theory must need adjusting. Based on this assumption, results from laboratory research on social preferences have been widely applied outside the laboratory (see Fehr et. al., 1993 and Camerer, 2003)[5].

However, Levitt and List (2005) take issue with this claim and suggest that

[5] The definition I use for social preferences here comes from Levitt and List (2005). They define an agent with social preferences as one who has preferences that are measured over her own and others' material payoffs. Such preferences might arise due to an agent's preference for altruism, fairness, reciprocity, or inequality-aversion to name a few examples.

laboratory studies on social preferences may be a poor guide to behavior outside of the laboratory due to the fact that within the laboratory environment subject are fully aware that their behavior is being monitored and thus may be induced to behave in a way that paints them in a positive light. Indeed, List's (2005) analysis of the sports card market revealed that social preferences in the market completely disappeared even though the agents revealed strong evidence of having social preferences in laboratory environments. List's study began with having experienced sports card traders participate in a gift exchange experiment in the laboratory in which buyers make price offers to sellers for a baseball card, and in return sellers select the quality level of the good provided by buyers. In the high scrutiny environment of the laboratory, there was a positive correlation between the price offer and the card's quality. However, when the experiment was moved to the floor of a sports card show, little statistical relationship between price and quality emerged.[6]

However, List's (2003) analysis of the endowment effect in another sports card market provides strong evidence that the endowment effect found in laboratory studies persists in markets where agents have experience. Thus, as Smith (1976) prudently pointed out, laboratory results must be confirmed with field observations to build an accurate theory of behavior. But in some instances, laboratory results are all that one has to work with. In particular, in developing parameter estimations for populations, many economists rely on samples invited to laboratory sessions (Currim and Sarin, 1989; Currim and Sarin, 1990; Daniels and Keller, 1990; Kahn and Sarin, 1988). For such laboratory studies, one of the main avenues of criticism attacks the sample from which the data was collected.

Self-selection into laboratory experiments is one problem that calls into question

[6] It should be noted that Benz and Meier (2006) have found a correlation between behavior in laboratory experiments and behavior in the field. In their study, they found a correlation between how subjects behave in donation experiments in the laboratory and how subjects behave in naturally occurring charitable giving situations.

whether results found in the laboratory can be generalized to the real world (List and Levitt, 2005).[7] That is, human subjects cannot generally be compelled, or forced, to participate in experiments, but rather, select into them. Individuals observed in experiments may significantly differ from the population to which the results will be generalized (List and Levitt, 2005). Orne (1962) found that those who select into scientific experiments are more likely to be "scientific do-gooders" who are interested in the study's goals, while Rosenthal and Rosnow (1969) found that participants are generally students who readily cooperate with the experimenter and seek social approval. However, it should be noted that many of these psychological experiments lack dominance. That is, changes in subjects' utility in the experiment do not come predominantly from the reward medium. Thus economists may be concerned about whether the responses observed in such studies reveal true preferences of the subjects.

Gronou (1974) was one of the first economists to point out that selected samples are often not representative of the larger general population. Heckman (1979) devised a generalized econometric method for treating this problem. These criticisms have been largely addressed by behavioral economists by using Heckman's method in their data analysis to create samples which represent the larger population across a number of demographics but those demographics are often relatively superficial and are based on variables such as income, race, and geographic location to name a few. While such

[7] Levit and List (2005) raise four potential problems in generalizing results of laboratory economic experiments to the real world. First, subjects know that they are being watched. Humans, unlike other lab subjects, know that they are participating in an experiment. Second, the context of an experiment can affect the subject's behavior and that context is not completely controlled by the experimenter. Smith (1976), points out, that if such contextual considerations are not negligible, the experimenter loses some control over the process of induced valuation. Third, laboratory experiments have stakes that are typically quite small. Fourth, is the issue of self-selection into lab experiments. Since, my thesis deals specifically only with the fourth criticism raised by Levit and List, I refer the reader to their paper for a more detailed understanding of the multiple criticisms of lab experimentation.

variation in the sample is important, limiting sample selection to cover such variables may not necessarily create a sample that is representative of the larger population. There may be key types of people that are consistently left out of certain types of experiments depending on the method and subject of the experiment.

Nevertheless, the extent to which selection matters depends on the studies goals. If a study's goal is to present evidence of an economic anomaly that violates standard economic theory, the issue of self-selection into the laboratory may not be an issue. For instance, in demonstrating the existence of the endowment effect in the laboratory, self-selection is not an important consideration into the validity of such experiments. Since standard economic theory suggests that *any* agent's preference ought not depend on their endowment, observing the endowment effect in the laboratory effect provides clear evidence that the assumptions of the theory need adjustment. However, if an economist wanted to determine "how much" the endowment effect persists in the general population with a laboratory experiment, selection into the laboratory may be important given that the endowment effect may persist in some individuals more than others. Selection into the laboratory may also be important if an economist wanted to investigate how a particular population, say securities traders, exhibited the endowment effect. In such an instance, the economist would want to be sure that the individuals who select into the laboratory are representative of the population of interest.

One common approach to investigating self-selection biases in experiments is to examine whether professionals, or other representative agents, and students behave similarly in laboratory experiments (List and Levitt, 2005). For example, Fehr and List (2004) examine experimentally how Chief Executive Officers (CEOs) in Costa Rica behave in trust games and compare their behavior with that of Costa Rican students. They find that CEOs are considerably more trusting and exhibit more trustworthiness than students. These differences in behavior may mean that CEOs are more trusting in everyday life, or it may be that CEOs are more sensitive to the laboratory's scrutiny, or that the stakes are so low for the CEOs that the sacrifice to wealth of making the moral choice is infinitesimal (List and Levitt, 2005). But this approach does not determine whether selection bias into the student group occurred, it simply determined that the student group's behavior cannot be extended to predict all individuals' behavior.

Lazaer, Mendelsen, and Weber (2006) suggest that the common selection approach is too broad to make inferences about market outcomes. They explain that the participants are locked into the experimental environment and the specific game presented to them while non-laboratory environments, as in markets, individuals sort based on preferences, beliefs, and skills. By sorting, they mean the voluntary choice of an activity by an agent. Those individuals who choose to participate in markets are not a random sample of the population. Thus, they conclude the ability to sort contaminates inferences from experimental treatments for the field.

To support their conclusions, Lazaer, Mendelsen and Weber, find that constraining the test subjects in an experiment has a large effect on the outcomes of the experiment. Specifically, when individuals are forced to play a dictator game[8], 74% of the subjects share. (Lazaer, Mendelsen, and Weber, 2006). However, when subjects are allowed to avoid the situation, less than one-third share (Lazaer, Mendelsen, and Weber, 2006). This reversal of proportions demonstrates that the influence of sorting limits the ability to generalize experimental findings that do not allow sorting (Lazaer, Mendelsen, and Weber, 2006).

[8] In a dictator game there are two players: (1) the "proposer," and (2) the "responder." In the game, the proposer's task is to distribute an endowment between himself and the responder. The responder then must accept the allocation allotted by the proposer. The Nash equilibrium to this game is for the proposer to keep the entire endowment for themselves and the responder to receive nothing.

One could expand on the findings of Lazaer et al. to postulate that individuals who participate in laboratory experiments are already sorted. That is, there may be certain types of people who choose to participate in certain experiments. If this is in fact the case, knowing who chooses to participate in the experiments may affect the inferences that economists can make to the larger population from such studies. While it could be of great value to determine what types of people choose to participate in experiments, there is little literature on the subject. Specifically, in this study the experiments focused on how an individual's averseness to ambiguity may affect their choice to participate in a given experiment.

Ellsberg initially determined whether an individual was ambiguity averse or not by asking what has come to be known as his canonical urn problem. In Ellsberg's original problem there are two urns, one containing 50 black and 50 red balls (the known urn) and the other containing 100 balls with an unknown number of those balls being red and black (the unknown urn). Each urn represents two distinct types of uncertainty. Present in both urns is the uncertainty as to which outcome will occur: either a red or a black ball will be drawn. Present only in the unknown urn, however, is ambiguity, the uncertainty about the probability that each outcome will occur.[9] In Ellsberg's hypothetical experiments he found that after numerous individuals under non-experimental settings were offered a prize if they drew a red ball most chose the known urn to draw from; when he offered a prize to the same individuals again but this time only to those who drew a black ball most of the same individuals chose the known urn again (Ellsberg, 1961). This pattern of behavior violates the axioms of subjective expected utility theory and is termed

[9] Knight (1921) calls this distinction risk versus uncertainty. In the known urn, Knight would argue there is risk and in the unknown urn there is uncertainty. Ellberg (1961) distinguishes the two urns by stating that the known urn presents an unambiguous probability while the unknown urn presents ambiguous probability to the decision maker.

ambiguity aversion.[10]

A "rational" actor conforming to the axioms of subjective utility theory would not choose the known urn when asked both questions. Rather, they would choose one urn when betting on drawing the red ball and the other when betting on drawing the black ball. For instance, if an individual chose the known urn when betting on drawing a red ball, according to subjective expected utility theory, that same individual should chose the unknown urn when betting on drawing the black ball. This is because these choices revealed "believed" probabilities. That is, by choosing the known urn when betting on drawing a red ball, the individual is revealing she believes that there are more red balls in the known urn than in the unknown urn. Likewise, if this belief is constant, than she should also believe that there are more black balls in the unknown urn. By choosing the unknown urn when betting on pulling a black ball, this individual is acting as if she had a constant belief about the number of red and black balls in the unknown urn, subjective utility theory would argue that this individual was unaffected by ambiguity or in other words, ambiguity neutral. As mentioned in the introduction to this paper, working with Ellsberg's description of ambiguity averse behavior and the subjective expected utility model, I can categorize individuals in Round 1 into one of three groups: (1) more ambiguity averse, (2) less ambiguity averse, and (3) individuals who responses were inconsistent with either theoretical framework. The individuals in the third category did not respond in a way that could neither be considered "rational" under the subjective expected utility model nor ambiguity averse as explained by Ellsberg.

Numerous researchers, including Slovic and Tversky (1974) and MacCrimmon

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[10] Subjective expected utility theory was first explicitly described by Savage (1954), but was inspired by Ramsey (1931). Ramsey combined the von Neumann and Morgenstern (1944) expected utility theory with Finetti's (1937) calculus of subjective probabilities to create what is now known as subjective expected utility theory (Camerer, 1995). For a full list of Savage's axioms, I refer the reader to Savage's original paper, See Fishburn(1988,1989) and Camere and Weber (1992) for technical reviews of subjective utility theory.

and Larsson (1979), empirically tested Ellsberg's hypotheses about ambiguity aversion and found strong support for his hypothetical results. Becker and Brownson (1964) tested and confirmed two extensions to Ellsberg's hypotheses: (1) individuals are willing to pay money to avoid actions involving ambiguity, and (2) some people behave as if they associate ambiguity with the range of the second-order distributions of the relative frequency of an event—where the greater the range of the distribution, the greater the ambiguity implied. Note that ambiguity aversion in economics generally refers to monetary outcomes. However, since solicitations to participate in economic experiments in general are ambiguous in regards to both the tasks that will be completed during the session and the monetary outcomes that can be expected by participating, in Round 2 my experiment systematically increases information in both dimensions to see if either affects who shows up to experiments.

3 Experimental Design

This study seeks to determine whether self-selection into laboratory experiments occurs through a very simple experimental design. In the first round of the experiment, I collected a sampling of Williams College students in a manner that mitigated the self-selection issue. Those with consistent responses I classified as either more ambiguity averse or less ambiguity averse, while those with inconsistent responses were not classified but nevertheless left in the sample. All the subjects participating in Round 1 were then randomly assigned a treatment email solicitation so that each treatment sample was balanced according to the original distribution observed in Round 1. To solicit participation in Round 2, I sent out four treatment emails with each subject receiving one email. The emails varied in the amount of detail provided in regards to what tasks would be performed in the session and the compensation that the subjects could expect to receive by participating. I then recorded who responded to each treatment by showing up to the Round 2 sessions .

3.1 Round 1 Design: Measuring Baseline Ambiguity Aversion

In Round 1, I determine the ambiguity preferences of a sample of Williams College students so that it can be determined if ambiguity aversion causes students to self-select into certain experiments. Normally, economists rely on either passive solicitation mechanisms, active solicitation mechanisms, or a database of previous participants. Common passive solicitation mechanisms are posters posted around campus, sign-up sheets, or other means of announcements requiring individuals who wish to participate to actively contact the experimenter. An active solicitation mechanism would be when an economist actively approaches a potential subject, usually students in their economics classes. Both active and passive solicitation mechanisms can cause most non-field economic experiments to be conducted using students who self-select into experiments.

The typical solicitation for participation in an economic experiment by means of a mass email could not be utilized in Round 1 for that would give the students an opportunity to self-select into the sample. In creating this sample, the selection issue had to be systematically controlled. I developed a protocol so that self-selection by the students is minimized.

The protocol is simple. I solicited to participate in a manner so that their opportunity costs were low and the potential payoffs to participating were high. The first environment in which they were approached to participate was at then end of an introductory economics class. Cooperating professors allocated ten minutes at the end of their class for a single session. The students are randomly selected as to the extent they chose to take an introductory economics course, which most students at Williams College do, and they happened to be in an introductory economics taught by a cooperating professor[11]. Since in past research, economics professors have solicited participants for their experiments from their classes, the classroom seemed like the logical place to begin to build a sample of participants.[12] 94 subjects in the Round 1 sample were recruited in first-year economics classes.

At the beginning of each session, the instructions and the potential rewards to participating were explained and then the students were given the opportunity not to participate and leave the class. The students were then asked to fill out a handout (see Appendix A) with 10 of Ellsberg's hypothetical canonical urn gambles and provide some personal information. Five urn distributions are presented in the Round 1 handout beginning with 50 red balls and 50 black balls in the known urn and ending 10 red balls and 90 black balls in the known urn. Remember, at each urn distribution a subject is presented with two gambles: (1) if I were to give you $10 if you pulled a red ball on your first try from which urn would you pick and (2) if I were to give you $10 if you pulled a red ball on your first try from which urn would you pick? Thus, ten gambles are presented to the subject in Round 1. In each gamble the student will have to indicate which urn they would draw from.

The in-class sessions were supplemented by soliciting participation from students in the two major libraries on campus, Schow and Sawyer. A given area in the library was

[11] I would like to specially thank Professor Gazzale and Professor Love for generously offering their class time to further my research.
[12] Bull, Schotter, and Weigelt (1987), Ariely, Lowenstein and Prelec (2003), and Kahneman, Knetsch and Thaler (1990) are just a few of the many researchers who have used economics students in their research.

chosen for a session, and everyone in sight was approached and asked to complete the survey. The students were randomly selected from the campus community in so far as they happened to be in that given area during a given session. After the students were approached, the potential subjects were given the opportunity to not participate. As in the in-class sessions, none of the participants were told that they would be potentially contacted in the future. 103 subjects in the Round 1 sample were recruited in the library.

Out of the 203 individuals approached both in class and in the libraries, only two declined to participate. None of the students in the classroom sessions decided to opt out of participating while two students approached in the library decided not to participate. This indicates that once the students were already in close proximity to the experimenter, either the opportunity cost of participating was relatively low when compared to the potential benefits of participating in the experiment or the cost of not participating was relatively high.

At the end of all the sessions 1 out of 3 of the students were selected to potentially receive a payment. Then one of their urn answers was randomly selected to be used to determine their payment. If they selected the unknown urn, then a computer randomly chose the urn's distribution. If they draw the proper ball, determined again by a computer, according to that particular question then they will receive the payment of that hypothetical gamble.

Based on responses to the questionnaire, individuals participating in the first phase were then classified as either more ambiguity averse (MAA) or less ambiguity averse (LAA). To determine this, the responses were sorted according to when they began to deviate from general ambiguity averse behavior, as originally explained by

Ellsberg, to behavior consistent with the axioms of subjective utility theory. That is, the responses were sorted on the basis of when the subjects began to stop choosing the known urn when betting on pulling both a red and black ball. At any given known distribution, students who chose the known urn when betting on pulling both a red and black ball can be said to be ambiguity averse at that particular distribution according to Ellsberg. Subjects whose behavior could not be determined by some already established behavioral or theoretical framework, as explained earlier, were categorized as neither MAA nor LAA but were still left in the sample.

3.2 Round 2 Design

Round 2 is designed to determine (1) whether the degree to which a subject is ambiguity averse effects their participation in laboratory experiments and (2) if there is a difference in the outcomes of that experiment that is due to a difference in group composition. To achieve this, the experimental design of Round 2 has to closely simulate a typical economic laboratory experiment, from the solicitation of students to the laboratory session itself.

The first question of interest, whether certain types of students are more likely to participate in experiment, will be observed through the group composition of those persons who choose to participate in a particular session. The four treatment emails used are detailed in the table below:

Table 1
Treatment Emails

		Detail Regarding The Tasks: Ambiguous	Detail Regarding The Tasks: Detailed
Detaild Regading the Payment:	Ambiguous	Ambiguous Task, Ambigous Payment (1)	Detailed Task, Ambigous Payment (2)
	Detailed	Ambiguous Task, Detailed Payment (3)	Detailed Task, Detailed Payment (4)

Students from the original sample collected in Round 1 were randomly assigned one of four solicitation mechanisms explained in the above table. Email (1) is an ambiguous email that is based on the standard solicitation used by the economics department at Williams College that was ambiguous in terms of what tasks were to be performed in the session and what the subjects could expect to receive in compensation. Email (2) is an email that is detailed in terms of what tasks were to be performed in the session but ambiguous with terms of what the subjects could expect to receive in compensation. Email (3) is an email that is ambiguous in terms of what tasks were to be performed in the session but was detailed in terms of what the subjects could expect to receive in compensation. Email(4) is an email that is detailed in terms of both what tasks were to be performed in the session and what the subjects could expect to receive in compensation (see Appendix C for emails). The emails that contain the same amount of detail regarding either the tasks to be performed or the payment to be expected contained the exact same wording. For example, Email (1) contained the same wording in regards to what tasks were to be performed as Email (3).

In the emails that were ambiguous in regards to what tasks were to be performed, I provided no information as to what was to occur during the session. In the emails that were detailed in regards what tasks were to be performed, I told recipients that the experiment consisted of two sections. In the first section of the experiment, I told recipients that they would have the opportunity, in private, to donate part of their show up fee to a charity and that the amount they gave would be matched by the experimenter. In the second section, I told the recipients they would be asked to make a series of decisions. For each decision, they would be asked to choose one of two options where the outcome of each option is uncertain. In some decisions, they would know the probability of each outcome within each option. In other decisions, they would not know the probability of each outcome for one of the options. In the email, I told the recipients that after they made their decisions, I would randomly select some of their decisions and they will be paid according to their choices. In the emails that were ambiguous in regards to the payment, I informed the recipients that by participating in the experiment they would earn either $10 or $20 and that the session would last about 30 minutes. In the emails that were detailed in regard to the payment, I informed the recipients that by participating in the experiment they would have a 50% chance of earning $10 and a 50% chance of earning $20 and that the session would last about 30 minutes.

After being categorized as either MAA or LAA in Round 1, the subjects were randomly assigned so that the ambiguity categories would be orthogonal to the email treatments. That is, the same proportional number of MAA subjects, LAA subjects, and subjects with inconsistent responses were randomly placed in each treatment. All emails were sent out using ExLab[13] software, a standard software used for emailing mass solicitations in economic experiments.

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[13] http://exlab.bus.ucf.edu/

The instrument used in the sessions during Round 2 was designed to approach the second questions on a variety of levels (see Appendix D for instrument). Included in the instrument were a series of four tasks: a public good dictator game, a series of ordered lottery gambles, a replication of the Round 1 instrument, and finally an optimism survey. The public goods dictator game and the replication of the Round 1 instrument differ in the degree to which ambiguity theory allows for strong predictions to be made as to how MAA and LAA play may differ while the ordered lottery gambles and the survey on optimism were included to determine whether ambiguity aversion is correlated with either risk aversion or optimism.

Task 1: Public Good Dictator Game

The first task presented in the instrument was a form of the public-good dictator game. The student was given the opportunity to donate as much or as little of their $10 show up fee to two charities, one known and one ambiguous, and was notified that any donation will be matched by the experimenter. To control for nuisance variables, four versions of the public-good game were created, while each subject participating in Round 2 was randomly assigned a public-good game so that the ambiguity classifications were orthogonal to each game.

The two charities chosen for this experiment were Oxfam and Habitat for Humanity, two charities that were viewed as being close analogues operating in slightly different settings. In each instrument, one was randomly assigned as the ambiguous charity and was described in such a manner. When detailed information was provided about Oxfam in the instrument, I described Oxfam as a nonprofit organization that works to minimize poverty through relief and development work in Africa. I described Habitat for Humanity as a nonprofit organization that builds homes for those in need that has been instrumental in Hurricane Katrina relief efforts in the United States. When Oxfam was described ambiguously, I described it as a nonprofit organization that works to alleviate poverty in Africa. Similarly, when Habitat for Humanity was described ambiguously, I described it as a nonprofit organization that works to help victims of natural disasters in the United States. The order in which the known and ambiguous charities are presented in the instrument was also randomized so that any supremacy effect of one charity being viewed before the other can be controlled. Thus, subjects who returned to Round 2 were presented with one of four possible instruments. The instrument received was randomized according to the subjects' previous classification in Round 1.

In designing this first task in the Round 2 instrument, non-anonymity effects were controlled for. In the past, lack of anonymity has been found to contribute to pro-social behavior by subjects. Hoffman et al. (1994), for example, find that 22 of 48 dictators (46%) donate at least $3 of a $10 pie under normal experimental conditions, but when subject-experimenter anonymity is added, only 12 of 77 dictators (16%) give at least $3. Based on their observations, Hoffman et al. (1994, p. 371) concludes that observed "behavior may be due not to a taste for "fairness" (other-regarding preferences), but rather to a social concern for what others may think, and for being held in high regard by others." This experiment mitigated the non-anonymity effects by giving the subjects four envelopes to give the illusion of anonymity.[14] I discreetly numbered each envelope on the inside with an identification number that correlates to the paper instrument that the subjects were given. The four envelopes were (1) an envelope labeled "start" containing

[14] There was no deception in this experiment. While anonymity was an illusion, it was not explicitly guaranteed, and thus I do not know what inferences the subjects may have drawn.

the subjects initial endowment of 10 $1 bills, (2) an envelope labeled “me” which subjects will move money into to keep for them selves, (3) an envelope labeled “1” corresponding to the first charity presented in the instrument, and (4) an envelope labeled “2” corresponding to the second charity presented in the instrument. In this task, subjects moved $10 from the “start” envelope to the other envelopes as they saw fit.

Task 2: Holt-Laury Lottery Gambles

The second task presented in the instrument was included to further assess what the ambiguity aversion instrument from Round 1 was really measuring. Some students categorized as LAA may simply be more comfortable with estimating probabilities than the students categorized as MAA. A series of ordered gambles based on the gambles first developed by Holt and Laury (2002) and later modified by Jamison, Karlan, and Schechter (2006) were presented to the subjects as the second task. The series of gambles included ten binary choices between two lotteries. The exact payoffs to each gamble are shown in Appendix D part III. The first choice was between a “safe” lottery that paid $5.50 with a 10% chance and $4.40 with a 90% chance (Option A in the instrument) and a “risky” lottery that paid $10.60 with a 10% chance and $.28 with a 90% chance (Option B in the instrument). In the first choice, the first lottery was both risk-dominant and had a higher expected value. The probability of the higher payoff in each lottery increased by 10% as the choices progressed until the final choice was between $5.50 with certainty and $10.60 with certainty, so that the second lottery dominated in all respects (Jamison et. al, 2006). As the probability of the high outcome increases, a person should cross over to Option B (Holt and Laury, 2002). The number of choices that a subject chooses Option A before they choose Option B determines their tolerance for risk (Holt and Laury, 2002). That is, the longer a subject chooses Option A, the more risk averse they are.

Task 3: Modified Ellsberg Urn Gambles

The third task included in the instrument was a replication of the Round 1 instrument. The replication of this game provides a clear line of inquiry to determine if self-selection into laboratory experiments can affect laboratory outcomes. If I were to have simply run Round 1 in the laboratory, self-selection could possibly skew the observed distribution. If the distribution of responses in Round 1 differs from the distribution of responses in Round 2, then I will be able to determine whether this difference is due to any difference in group composition. Furthermore, such a replication allows me to determine if subjects are consistently ambiguity averse over time and whether the degree to which they are ambiguity averse changes. Some inconsistency is expected, due to the fact that individuals have experience with the instrument and more importantly were not paid in between Round 1 and Round 2. Thus, they may experience unnecessary regret due to possible belief that they were one of the individuals randomly selected but did not win. However, this is probably unlikely given that in Round 1 they were notified that only one in three individuals would be selected, Furthermore, if there is variation in responses between Round 1 and Round 2, given that the instrument provides an ordered scale with which I can measure ambiguity aversion, I can look to see if the responses are correlated. If the responses remain correlated, then I can infer to a certain degree how Round 1 participants who did not return to Round 2 would have responded had they participated in Round 2.

Task 4: Optimism Survey

The fourth task was included to again further assess what the ambiguity aversion instrument from Round 1 was measuring. To determine whether MAA students are simply less optimistic than LAA students and thus more averse to unknown circumstances, included in the instrument was a series of questions originally designed by Scheier, Carver, and Bridges (1994) to measure an individual's optimism. Ten questions were asked, three were positively worded, three were negatively worded, and four are filler controls. For each question the student either chooses A, I agree a lot, B, I agree a little, C, I neither agree nor disagree, D, I disagree a little, and E, I disagree a lot. An A response is graded as one point, B is graded as two points, C is graded as three points, D is graded as four points, and E is graded as five. The most pessimistic total score is 14 while the most optimistic score is 22 (see Appendix D part IV).

The students were compensated based on their responses to the first three sections of the instrument. Each subject who participated in Round 2 was given a $10 show up fee and told that they would be compensated for the other tasks presented in the experiment, though the exact amount they would receive depended on their choices, and on random chance. After the first task, the subjects kept the whatever was remaining of their $10 show up fee. Even though between the second and third task in the instrument subjects would make 20 decisions, they were notified that only one of these would end up being used to determine their payment. For each subject, an urn question was randomly selected for payment. The Round 2 experiment was designed so that the subjects would on average earn between $10 and $20 for a 30-minute session. The average payment for Round 2 was $10.19.

To avoid scheduling conflicts, three Round 2 sessions were held. Each treatment was randomly divided equally into two groups. The first group for each treatment was invited to attend the first session on Tuesday February 20th, 2007 and the second group for each treatment was invited to attend the second session on Tuesday February 27th, 2007. Finally, to further control for scheduling conflicts, all the subjects who did not attend the first two sessions were sent the same treatment email inviting them to attend the third session on Wednesday March 7th 2007.

3.3 Round 2 Hypotheses

The treatments considered in this experiment differ in the method in which I solicit experimental subjects' participation. While there are several models that depart from the neo-classical subjective expected utility model which can model decisions under ambiguity, those models are limited in so far as they predict decisions based on unknown lotteries (Kahn and Sarin, 1988). Moreover, they do not differentiate between more or less ambiguity averse individuals (Kahn and Sarin, 1988). Nevertheless, ambiguity theory allows for limited predictions as to who would respond more to the various treatment emails. Working from Becker and Brownson's (1964) first extension of Ellsberg's hypotheses that individuals are willing to pay money to avoid actions involving ambiguity, I develop four hypotheses.

Ceteris paribus, MAA subjects can be expected to be more likely to pay money to avoid actions involving ambiguity than LAA subjects since they are more ambiguity averse. This proposition leads to the first hypothesis:

> **Hypothesis 1:** Less ambiguity averse students are more likely to participate in economic experiments than more ambiguity averse students. [15]

[15] The null hypothesis for Hypothesis 1 is that there will be no difference in participation between less ambiguity averse students and more ambiguity averse students.

Despite the fact that the more detailed emails provided a great deal of information, the information is by no way complete and guaranteed with certainty. Thus, even when sent the more detailed emails, MAA would be more willing to forgo a payment opportunity by not showing up to session to avoid the ambiguous scenario of walking into the laboratory. The second hypothesis follows from the same proposition as the first:

> **Hypothesis 2:** LAA subjects will respond at a higher rate than MAA subjects to the baseline ambiguous email providing ambiguous information in regards to the task that would be completed in the session and the payment that the participants could expect to receive by participating.[16]

Since the baseline email provides little information in either dimension, MAA subjects would be more willing to forgo a payment opportunity that LAA students by not participating in Round 2. This was the second hypothesis tested.

The third hypothesis tested was the softest hypothesis regarding how subjects would respond to the treatments:

> **Hypothesis 3:** MAA subjects would respond at a different rate to the emails that were detailed in terms of what the subjects could expect to receive in compensation than to the baseline ambiguous email. [17]

I cannot make any strong predictions as whether MAA will respond at higher or lower rates to the detailed payment email than the baseline ambiguous email because I do not know for certain how MAA individuals read the information provided in regards to the

[16] The null hypothesis for Hypothesis 2 is that there will be no difference in the response rate of LAA and MAA subjects who were sent the baseline ambiguous email.
[17] The null hypothesis for Hypothesis 3 is that they will be no difference in the response rate of MAA students who were sent emails that were detailed in regards to payment and MAA students who were sent the baseline ambiguous email.

payment. MAA individuals may read the ambiguous payment information as a "certain payment" and regard the more detailed information about payment as having a higher variance. If this is the case, then working with Becker and Brownson's (1964) second extension of Ellsberg's hypothesis, the MAA students would be more likely to respond to the emails that provide ambiguous payment information.

Since ambiguity aversion theory does not address how ambiguity averse individuals treat non-monetary information, there can be no strong predictions made about how response rates will differ between the emails that contain more detailed information about the tasks the subjects will complete in the laboratory and the emails that contain more detailed information about the compensation the subjects can expect to receive by attending the session. Specifically, I cannot make any predictions based on ambiguity aversion theory as whether the response rate of MAA subjects will differ between the detailed task, ambiguous payment treatment email than to the baseline ambiguous email. Still, it seams reasonable to expect that MAA subjects would respond at higher rates to the email solicitations that provide more detailed information about the tasks to be performed during the session.

Some predictions can be made as to how MAA and LAA play during the public goods game may differ based loosely on ambiguity theory, which leads to my fourth hypothesis:

Hypothesis 4: At the very least, MAA and LAA are expected to give differently to the unknown charity due to the lack of information provided about the charity.

However, there is no literature that relates ambiguity aversion to charitable giving that allows for strong hypotheses.

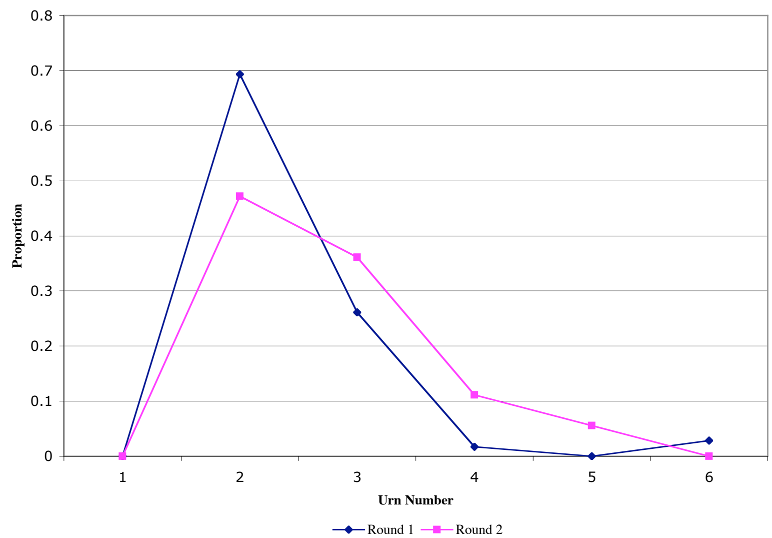
If the student responses remain consistent from Round 1 to Round 2, there are clear predictions that can be made about group composition affect the results of the Round 1 game that is replicated in the instrument of Round 2. If more LAA students attend sessions, then the average responses to the canonical urn questions will be skewed towards LAA behavior. Any change in the group composition from Round 1 to Round 2 should have some effect on the average responses. This effect could lead us to conclude that self-selection into the laboratory environment could skew the results of the laboratory study if that study was designed to estimate population parameters.

4 RESULTS

4.1 ROUND 1 RESULTS: MEASURING AMBIGUITY AVERSION

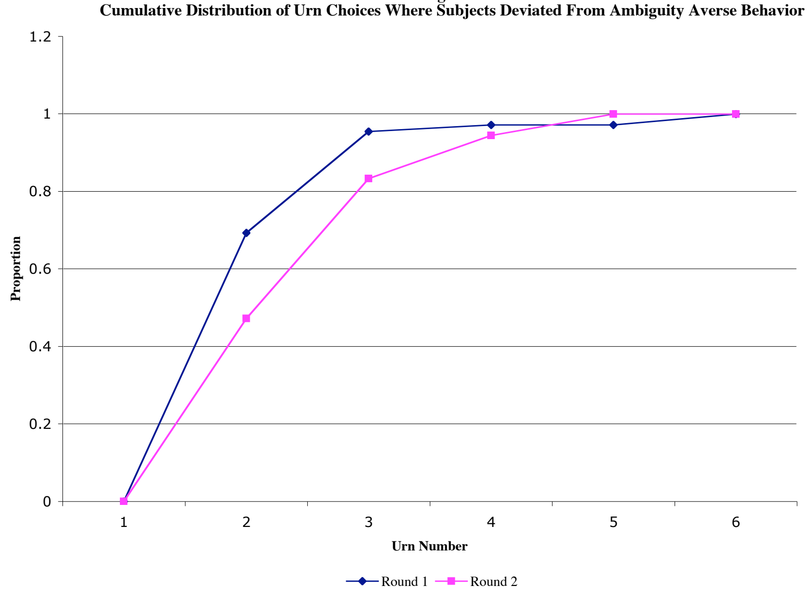
Students had the opportunity to deviate from ambiguity averse behavior at five known urn distributions. The further along in the series they deviated, the more ambiguity averse the subject is determined to be. Beginning with the 50 red balls, 50 black balls, the known distribution urns are numbered from 1 to 5. As the known urn number increased by one, the number of red balls in the known urn decreased by 10 and the number of black balls in the known urn increased by 10. Thus, known urn number 5 contained 10 red balls, 90 black balls. For each subject in Round 1, I recorded the numbered urn at which they deviated from ambiguity aversion along with their demographic data. If a particular never deviated from ambiguity averse behavior, I recorded 6 for that subject. This methodology orders ambiguity aversion from 1, ambiguity neutral, to 6, very ambiguity averse. Using that scale, I then ranked the subjects according to their ambiguity aversion. Below are two figures. The first figure compares the probability distribution of Round 1 responses with Round 2. The second compares the cumulative distribution of Round 1 responses with Round 2.

Figure 1
Probability Distribution Of Urn Choices Where Subjects Deviated From Ambiguity Averse Behavior



The number of red balls in the known urn can be calculated as follows: # of Red Balls = 50 - (Decision # - 1) * 10

Figure 2
Cumulative Distribution of Urn Choices Where Subjects Deviated From Ambiguity Averse Behavior



The number of red balls in the known urn can be calculated as follows: # of Red Balls = 50 - (Decision # - 1) * 10

Note the large proportion of students in Round 1 who deviated from ambiguity averse behavior when reaching the second known urn with 40 red balls and 60 black balls. Taking this large proportion into account, I created a rule that classified subjects as either MAA or LAA. LAA students were classified as those students who deviated from ambiguity averse behavior when arriving at the second known urn in the series (henceforth, Decision # 2), with the distribution of 40 red balls, 60 black balls. MAA students were classified as those subjects who deviated from ambiguity averse when arriving at the third urn in the series (henceforth, Decision # 3) with a distribution of 30 red balls, 70 black balls, or later. Thus, subjects who deviated from ambiguity averse behavior when arriving at the fourth known urn in the series (henceforth, Decision # 4), fifth known urn in the series (henceforth, Decision #5) or subject who never deviated (henceforth, Decision # 6) were classified as MAA.

After the students were classified, I excluded six from the Round 1 sample either because they were Economics Thesis students who found out about the study's goals or because they were not on campus when Round 2 was to be conducted. Of the 197 students in the Round 1 sample, I classified 115 classified as LAA and 54 as MAA. Table 3 provides the full distribution of Round 1 data (See Appendix B for Data).

4.2 Determinants of Round 1 Ambiguity Aversion Classification

During Round 1 I collected demographic data from each subject. To determine if ambiguity classification in Round 1 is determined by any demographic information, a series of probit models is estimated below. Marginal effects are reported for coefficients so that holding all else equal, one can determine the increased probability that a subject would participate given the stated determinants. All independent variables in the model are binary indicator variables.

Table 2
Determinants Of Ambiguity Classification
Probit

Binary Dependent Variable:	More Ambiguity Averse in Round 1 (1)
Female	0.029
	(.072)
Round 1 Conducted in Library	0.119
	(.103)
Round 1 Conducted in Gazzale's Class	0.076
	(.167)
Freshman	-0.077
	(.132)
Sophomore	
Junior	-0.087
	(.103)
Senior	-0.167
	(.092)
Economics Major	0.043
	(.118)
Psychology Major	-0.093
	(.211)
R^2	0.018

Marginal effects are reported for coefficients. Robust standard errors are in parenthesis. * indicates significance at 10 percent. ** at 5 percent. *** at 1 percent. In column one, the dependant variable equals zero if the subject did not show up to round 2 and one if the subject did show up. Variables are ommited due to collinearity. Only information from Juniors and Seniors was included in the major data since Williams students do not declare their major until the end of their Sophmore year. A probit regression that predicts being classified as Less Ambiguity Averse in Round 1 has the exact same coefficients for all variables but of the opposite sign.

As shown in Table 2, none of the demographic data predicts who was classified as either MAA or LAA. This lends evidence that my ambiguity aversion classifications are uncorrelated with any observable characteristics of the subjects, other than how they responded on the Round 1 instrument. Since subjects with inconsistent responses in Round 1 are a small proportion of my data set and because they are excluded from much of my data analysis, a probit regression was not estimated for those subjects.

4.3 Balance of Treatments in Round 2

Before looking to see if ambiguity aversion plays a role in self-selection into laboratory settings, I first show that my Round 1 sample was properly balanced across treatments. To be clear, I only attempted to balance the created ambiguity classifications across the four treatments. No attempts were made to balance any other metrics. Below is a table that investigates how well I balanced my treatments:

Table 3
Balance of Treatments
Treatment Means & Standard Deviations

Treatment:	Received Ambiguous Solicitation Email (1)	Received More Detailed Solicitation Emails (2)	t-stat: (2)<>(3) (3)	Received Ambiguous Solicitation Email (4)	Received Detailed Task and Ambigous Payment (5)	Received Ambiguous Task and Detailed Payment (6)	Received Detailed Task and Detailed Payment (7)	Prob > F (8)
More Ambiguity Adverse in Round 1	0.265	0.277	0.187	0.265	0.28	0.28	0.271	0.998
	(.063)	(.04)		(.446)	(.454)	(.468)	(.449)	
Less Ambiguity Adverse in Round 2	0.633	0.615	-0.1877	0.633	0.6	0.62	0.625	0.989
	(.07)	(.04)		(.467)	(.495)	(.49)	(.489)	
Inconsistent Responses in Round 1	0.102	0.108	0.1187	0.102	0.12	0.1	0.104	0.988
	(.044)	(.026)		(.306)	(.328)	(.303)	(.309)	
Female	0.388	0.5	1.364	0.388	0.5	0.42	0.583	0.2165
	(.070)	(.041)		(.492)	(.505)	(.499)	(.498)	
Round 1 Conducted in Library	0.489	0.534	0.532	0.5	0.64	0.44	0.52	0.2297
	(.072)	(.041)		(.51)	(.48)	(.501)	(.504)	
Round 1 Conducted in Gazzale's Class	0.347	0.264	-1.12	0.303	0.179	0.285	0.232	0.381
	(.069)	(.036)		(.464)	(.386)	(.456)	(.426)	
Number of Observations	49	148		49	50	40	48	

In columns 1 and 2 standard errors are in parentheseses. * significant at 10%; ** significant at 5%; *** significant at 1%. A subject is Less Ambiguity Averse if they began choosing the unknown urn when betting on the red ball when the known distribution was 40 red, 60 black. A subject is More Ambiguity Averse if they began choosing the unknown urn when betting on the red ball when the known distribution was 30 red, 70 black or higher. Column 3 presents the t-statistic for the two sample t-test for the data in column 1 and column 2. Column 8 provides the p-value for the F-test comparing columns 4, 5, 6, and 7.

Remember, I classified the subjects into one of three groups, MAA, LAA, and inconsistent responders based on their responses in Round 1. Looking at Table 2 we see that there is no significant difference between any of the treatments. Given the very high p-values for the F-test comparing the four treatments (greater than .98), I can be confident that the proportion of MAA, LAA, and inconsistent responders were properly balanced across the four treatments in this dimension.

However, the proportion of females and subjects who participated in Round 1 in the library does not appear to be balanced across treatment groups as well. Since the means in Table 1 correspond to proportions, the proportion of females in the baseline ambiguous email treatment is lower than in the more detailed email treatments (.38 versus .5). Also, the proportion of students that participated in Round 1 in the library varies across treatments, though the variation is not statistically significant. Similarly, while the proportion of students who participated in Round 1 in Professor Gazzale's classroom differs across treatments, the variation is not statistically significant. Still, because of these differences, I will control for observable demographics in my core analysis.

4.1 Effect of Ambiguity Aversion on Selection Into Round 2

The next step in my analysis involves looking at looking at the effect of ambiguity aversion on return rates. Of the 197 subjects Round 1 participations, 36 (18.3%) responded for Round 2.

Below, Table 4 shows that the same proportion of MAA and LAA subjects participated in Round 2:

Table 4
Summary Statistics
Means & Standard Errors

Sample Frame:	All (1)	More Ambiguity Averse in Round 1 (2)	Less Ambiguity Averse in Round 1 (3)	Inconsistent Responses in Round 1 (4)	t-stat: (2)<>(3) (5)
Panel A: Full sample					
Proportion Returned for Round 2	0.183	0.185	0.189		0.052
	(0.027)	(0.053)	(0.036)		
Proportion More Ambiguity Averse in Round 2	0.485	0.600	0.435		-0.856
	(0.083)	(0.163)	(0.106)		
Panel B: Received Ambiguous Solicitation Email					
Proportion Returned for Round 2	0.341	0.154	0.419		1.7131*
	(0.072)	(0.104)	(0.090)		
Proportion More Ambiguity Averse in Round 2	0.467	1.000	0.385		-1.6653
	(0.133)	(0.000)	(0.140)		
Panel C: Received More Detailed Solicitation Emails					
Proportion Returned for Round 2	0.1389	0.195	0.118		-1.206
	(.029)	(.0626)	(.032)		
Proportion More Ambiguity Averse in Round 2	0.55	0.5	0.5833		0.3493
	(.114)	(.1889)	(.1486)		
Panel D: Demographics and Sample Size Data					
Number of observations in full study	197	54	122	21	
Number of participants who returned in Round 2	36	10	23	3	
Female	93	28	56	9	
Freshman	74	21	50	3	
Sophomore	55	18	31	6	
Junior	37	10	21	6	
Senior	29	5	19	5	
Economics Major	22	7	13	2	
Psychology Major	4	1	3	0	
Round 1 Conducted in Class	94	24	64	6	
Round 1 Conducted in Library	103	30	58	15	

Standard errors are in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. A subject is Less Ambiguity Averse if they began choosing the unknown urn when betting on the red ball when the known distribution was 40 red, 60 black. A subject is More Ambiguity Averse if they began choosing the unknown urn when betting on the red ball when the known distribution was 30 red, 70 black or higher. Subjects with inconsistent responses were excluded in the two-sided t tests.

Earlier, Figure 1 shows non-parametrically that the distribution of ambiguity aversion appears the same in Round 1 as it does in Round 2. Furthermore, in the table above, a two sample t-test shows no significant difference between the proportion of MAA and LAA students who returned to Round 2. Given these results, I cannot reject the null hypothesis that more ambiguity averse individuals participate in laboratory experiments at the same rate as less ambiguity averse individuals.

However, LAA students were significantly more likely to participate in Round 1 when sent the baseline ambiguous email than MAA subjects. Of the subjects sent the baseline ambiguous email, 41.9% of LAA subjects participated in Round 2 while only 15.4% of the MAA chose to participate. After performing both a two-sample t-test and its non-parametric alternative, the Wilcoxon rank-sum test, this difference was found to be significant at the 10% level (p-values for both tests equal .09). On the other hand, when subjects were sent the more detailed emails, the proportion of MAA and LAA subjects who participated did not differ significantly. These two observations taken together lend evidence to the conclusion that if a laboratory experiment's email solicitation is vaguely written, the vague solicitation can lead to a biased sample of individuals participating in the experiment.

Table 5 below further separates the email treatments to determine the type of ambiguity subjects are responding to in the email: ambiguity regarding what tasks will be performed or ambiguity regarding the expected payment.

Table 5
Email Treatments
Means & Standard Errors

Sample Frame:	All	More Ambiguity Averse in Round 1	Less Ambiguity Averse in Round 1	t-stat: (2)<>(3)
	(1)	(2)	(3)	(5)
Panel A: Full sample				
Proportion Returned for Round 2	0.183	0.185	0.189	0.052
	(0.027)	(0.053)	(0.036)	
Proportion More Ambiguity Averse in Round 2	0.485	0.600	0.435	-0.856
	(0.083)	(0.163)	(0.106)	
Panel B: Baseline Ambiguous Solicitation				
Proportion Returned for Round 2	0.341	0.154	0.419	1.713*
	(0.072)	(0.104)	(0.090)	
Proportion More Ambiguity Averse in Round 2	0.467	1.000	0.385	-1.6650
	(0.133)	(0.000)	(0.140)	
Panel C: Detailed Task & Ambigous Payment Solicitation				
Proportion Returned for Round 2	0.136	0.357	0.033	-3.171***
	(.052)	(.133)	(0.033)	
Proportion More Ambiguity Averse in Round 2	0.667	0.60	1.00	
	(0)	(.2449)	(0)	
Panel D: Ambiguous Task & Detailed Payment Solicitation				
Proportion Returned for Round 2	0.133	0.071	0.161	0.809
	(.051)	(.0714)	(.067)	
Proportion More Ambiguity Averse in Round 2	0.500	1.000	0.600	
			(.245)	
Panel E: Detailed Task & Detailed Payment Solicitation Email				
Proportion Returned for Round 2	0.140	0.154	0.133	-0.174
	(.053)	(.104)	(.063)	
Proportion More Ambiguity Averse in Round 2	0.333	0.500	0.250	-0.516
	(.210)	(.25)	(0.25)	

Standard errors are in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. A subject is Less Ambiguity Averse if they began choosing the unknown urn when betting on the red ball when the known distribution was 40 red, 60 black. A subject is More Ambiguity Averse if they began choosing the unknown urn when betting on the red ball when the known distribution was 30 red, 70 black or higher. Subjects with inconsistent responses were excluded in the two-sided t tests. Of the 36 subjects who returned in Round 2, 10 were categorized as More Ambiguity Averse, 23 were categorized as Less Ambiguity Adverse, and 3 had inconsistent responses in Round 1. In Panel C and D, t-tests on Proportion More Ambiguity Averse in Round 2 could not be performed due to the fact that one variable was perfectly predictive. Nevertheless, the mean and standard errors were left in to give a sense of how individuals who recieved those two emails "switched" their responses.

Of the four treatment emails, only the baseline ambiguous email and the detailed task, ambiguous payment email elicited statistically different participation from MAA and LAA subjects. Interestingly, LAA subjects participated at the lowest rate when receiving the detailed task, ambiguous payment email. It appears that increasing the detail about the tasks to be performed in the email solicitation drives down the participation by LAA subjects. However, after further investigation of the determinants of who participates, this effect was found to be negligible.

To further investigate the determinants of who participates in Round 2, a series of probit models estimated from the data are presented in Table 6.[18] Marginal effects are

[18] Due to the lack of good data, information on majors and other demographic variables were not included in these regressions.

reported for coefficients so that holding all else equal, one can determine the increased probability that a subject would participate given the stated determinants. All independent variables in the model are binary indicator variables.

Table 6

Determinants Of Whether A Subject Shows Up To Round 2
Probit

Binary Dependent Variable:	Participated Round 2 (1)	Participated Round 2 (2)	Participated Round 2 (3)	Paricpated Round 2 (4)	Participated Round 2 (5)	Paricpated Round 2 (6)
More Ambiguity Averse in Round 1	0.0123 (.064)	0.016 (.063)	0.074 (.133)		0.074 (.133)	
Less Ambiguity Averse in Round 1				-0.074 (.133)		-0.075 (.133)
Email with Ambiguous Task and Ambiguous Payout		0.1636* (.10)	0.251** (.122)	-0.063 (.120)	0.25 (.122)	-0.062 (.120)
Email with Ambiguous Task and Detailed Payout			-0.118 (.081)	-0.134 (.106)	-0.117 (.082)	-0.134 (.106)
Email with Detailed Task and Ambiguous Payout			0.001 (.091)	0.117 (.115)	0.001 (.0915)	0.115 (.159)
Email with Ambiguous Task and Ambiguous Payout AND More Ambiguity Averse in Round 1			-0.15* (.046)		-0.15* (.046)	
Email with Ambiguous Task and Detailed Payout AND More Ambiguity Averse in Round 1			-0.115 (.082)		-0.115 (.082)	
Email with Detailed Task and Ambiguous Payout AND More Ambiguity Averse in Round 1			0.347 (.291)		0.342 (.294)	
Email with Ambiguous Task and Ambiguous Payout AND Less Ambiguity Averse in Round 1				0.37* (.261)		.37* (.262)
Email with Ambiguous Task and Detailed Payout AND Less Ambiguity Averse in Round 1				0.201 (.263)		0.202 (.263)
Email with Detailed Task and Ambiguous Payout AND Less Ambiguity Averse in Round 1				-0.164 (.055)		-0.163 (.074)
Female	-0.066 (.057)	-0.05 (.058)	-0.049 (.056)	-0.049 (.056)	-0.049 (.056)	-0.049 (.056)
Round 1 Conducted in Library	-0.187*** (.057)	-0.183*** (.058)	-0.171*** (.057)	-0.171*** (.057)	-.164** (.073)	-.164** (.073)
Round 1 Conducted in Gazzale's Class					0.009 (.073)	0.009 (.073)
R^2	0.074	0.116	0.181	0.181	0.181	0.181

Marginal effects are reported for coefficients. Robust standard errors are in parenthesis. * indicates significance at 10 percent. ** at 5 percent. *** at 1 percent. In column one, the dependant variable equals zero if the subject did not show up to round 2 and one if the subject did show up. The individuals who responded inconsistently in round 1 and therefore could not be classified as more or less ambiguity averse were excluded from this regression. Variables in column 3 are ommited due to collinearity.

Table 6 shows a number of interesting findings. First, students that participated in Round 1 in first year economics classes are significantly more likely to participate in Round 2. This finding is not surprising. Economics students already exhibit an interest in the discipline by taking courses in the major. Furthermore, the email solicitations were sent from Professor Robert Gazzale's email account to mitigate any subject's ability to link Round 1 with the Round 2 solicitations. Since more than half of the students approached in first year economics students were Professor Gazzale's students, this effect could be an artifact of Rosenthal and Rosnow (1969) findings that participants are generally students who readily cooperate with the experimenter and seek social approval. Or first year economics students may simply want to get in the good graces of a professor in the department. While Professor Gazzale was no longer the students' professor when Round 2 was performed, I performed Wilcoxon rank-sum test confirming Gazzale's students showed up more than others (p-value= .0004)[19]. However, when an indicator variable for Gazzale's students was added to my probit models, the coefficient was statistically insignificant (p-value= .90 for both models). Furthermore, when Wilcoxon rank-sum tests were performed to see if Gazzale's students who participated in Round 2 differed in their Round 1 ambiguity classification from other subjects who participated after being sent each treatment, no significant difference was found.[20]

Notice that students who received the baseline ambiguous email are more likely to participate in Round 2. This finding may encourage economists to continue to use ambiguous emails to simply keep their participation rate high. However, when a subject is sent the base line fully ambiguous email *and* they are more ambiguity averse, they are

[19] A Wilcoxon ran sum test is used here to avoid distributional assumptions regarding a data. The test is appropriate because even though it is testing a binary variable, the binary variable can still be ranked in a meaningful manner. Furthermore, there is no cause for concern that the binary variable is tied in this test. Still, a t-test confirms the difference (p-value=.0003).

[20] When comparing the Round 1 ambiguity classification of students who participated in Round 2 after receiving the fully ambiguous email, no significant difference was found between students who were recruited in Round 1 in Gazzale's class and others when performing a Wilcoxon rank sum test (p-value = .765). Similarly, using a Wilcoxon rank sum test no significant differences were found for the detailed task, ambiguous pay email treatment, ambiguous task, detail pay email treatment, and detailed task, detailed pay email treatment (p-values =.1573, .3173, .4795 respectively). Furthermore, when comparing the Round 1 ambiguity classification of students who participated in Round 2 after receiving the fully ambiguous email, no significant difference was found between students who were recruited in Gazzale's class and other first year economics students when performing a Wilcoxon rank sum test (p-value=.583). Similarly, using a Wilcoxon rank sum test no significant differences were found for the ambiguous task, detail pay email treatment and the detailed task, detailed pay email treatment (p-values= .2207 and.4795 respectively). However, of the first year economics students sent the detailed task, ambiguous email treatment in Round 2, only Gazzale's students participated.

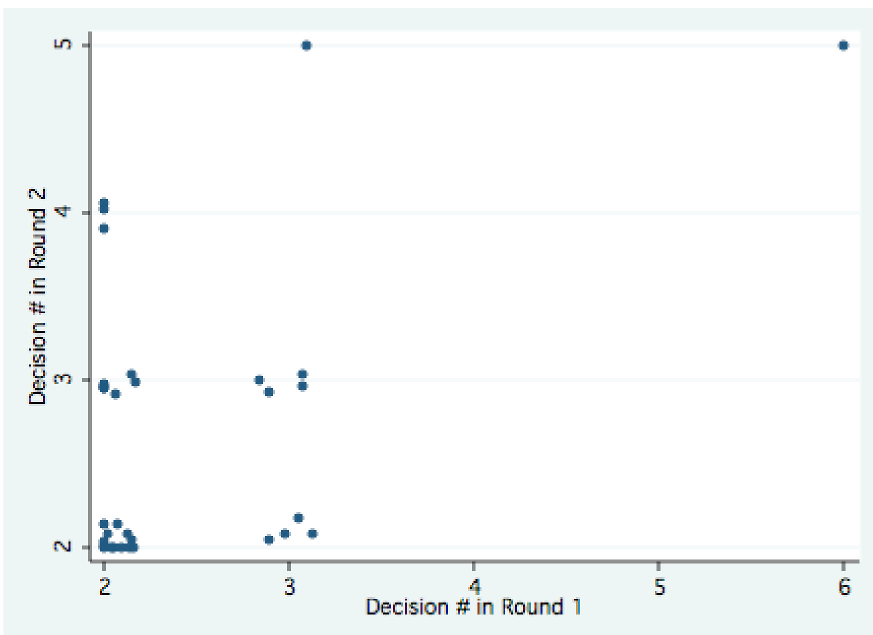
significantly less likely to participate. The significance of this interaction term in columns four and six clearly confirms my second hypothesis.

Table 6 does not confirm the previous observation that increasing the detail about the tasks to be performed in the email solicitation drives down the participation by less ambiguous averse subjects. When an individual is LAA and receives the detailed task and ambiguous payment email solicitation they are less likely to participate, but the coefficient is insignificant (p-value = .17). However, when a subject is sent the base line fully ambiguous email *and* they are less ambiguity averse, they are significantly more likely to participate. The significance of this interaction term in columns four and six further confirms my second hypothesis.

4.3 Inconsistent Results Between Round 1 and Round 2

In Round 2 I expected the subjects responses to be consistent with their Round 1 responses. However, in Round 2 there was a slightly different payment scheme. Tables 4 and 5 indicate that there was a large degree of "switching" between MAA and LAA behavior from Round 1 to Round 2. That is, subjects who were classified in Round 1 as MAA exhibited in behavior in Round 2 that classified them as LAA. Note that in Table 2 Panel A, the proportion of subjects categorized in Round 2 as MAA does not significantly differ between subjects who were categorized in Round 1 as MAA or LAA. Indeed, a 43.5 % of the subjects categorized in Round 1 as LAA who participated in Round 2 exhibited MAA behavior when completing the exact same series of modified Ellsberg urn problems. In all of my results that I have presented so far, I assumed that my classifications in Round 1 are meaningful. I made this assumption because this switching is an artifact of the very strict artificial rule that I developed to separate the Round 1 distribution into two clear groups. A more specific measure of ambiguity averseness would be to look at the decision number where the subjects started exhibiting behavior consistent with the axioms of subjective utility theory. The higher the decision number, the more ambiguity averse they are. Using this metric, I looked to see there was a correlation in responses between Round 1 and Round 2. Using a Pearson pairwise correlation, I found a positive correlation coefficient of .4461 between the Round 1 and Round 2 responses, significant at the 1 percent level (See Figure 3 below for scatter plot)[21].

Figure 3



This positive correlation between the decision number where the subject deviated from ambiguity averse behavior in Round 1 and the decision number where the subject deviated from ambiguity in Round 2 explains why there would be many subjects

[21] The scatter plot above displays jittered data points.

reclassified under my strict rule. While the Round 1 responses may not be a perfect measure of ambiguity aversion, the fact that the Round 1 and Round 2 responses are correlated allows me to safely assume that the Round 1 responses were indeed measuring ambiguity aversion.[22]

To investigate whether there were any determinants of which subjects exhibited inconsistent responses, I estimated a series of probit models below:

Table 7

Determinants of Inconsistent Responses Between Round 1 and Round 2

Probit

Binary Dependant Variable:	Inconsistent Between Round 1 and Round 2
	(1)
More Ambiguity Averse in Round 1	-0.011
	(.05)
Round 1 Conducted in Library	0.067
	(.06)
Round 1 Conducted in Gazzale's Class	-0.061
	(.069)
Female	-0.013
	(.045)
R^2	0.05

Marginal effects are reported for coefficients. Robust standard errors are in parenthesis. * indicates significance at 10 percent. ** at 5 percent. *** at 1 percent.The individuals who responded inconsistently in round 1 and therefore could not be classified as more or less ambiguity adverse were excluded from this regression. Since a binary indicatator variable for More Ambiguity Averse in Round 1 was used, a coeffificient for Less Ambiguity Averse in Round 1 would be of the same magnitude as the coefficient of More Ambiguity Averse in Round 1 but of the opposite sign.

As shown in the above table, I found no significant determinants for inconsistent responses between Round 1 and Round 2. This leads me to conclude that such behavior is idiosyncratic and random.

[22] This analysis is on only 36 observations and is thus low powered. A significant correlation with a small sample size provides solid evidence that the correlation is indeed real.

4.4 Correlation Between Ambiguity Aversion, Risk and Optimism

To be sure that my instrument is not measuring something other than ambiguity aversion, I checked to see if either risk aversion or optimism were correlated with ambiguity aversion. Remember, the Holt-Laury lottery involves a series of choices between a safe (Option A) and risky (Option B). As one moves along the series, the risky option becomes more appealing. A rational decision maker would only switch between the choices at one and only one point. Thus, the further along in the series a subject switches from the safe option to the risky option, the more risk averse that particular subject is. By recording at which lottery decision the subjects switch, the Holt-Laury lotteries provide a means to rank subjects according to their risk aversion. The Spearman rank correlation between observed ambiguity aversion and risk aversion in Round 2 yields an insignificant p-value of .59. Furthermore, a Pearson correlation yields a negative correlation coefficient of –.2012 that is insignificant with a p-value of .26. While these findings suggest that the subjects tolerance for ambiguity is uncorrelated with their tolerance for risk, they are based on only 36 observations and are thus low powered. Nevertheless, the lack of correlation also leads me to suspect that LAA subjects in Round 2 were not simply more comfortable with probabilities than MAA students.

The optimism survey conducted in Round 2 also provided a metric with which to rank subjects. The higher the subjects score on the survey, the more optimistic they are. A Pearson correlation between observed ambiguity aversion and optimism in Round 2 yields an insignificant correlation of .0063 with a p-value of .9711, leading me to believe that LAA students are not merely more optimistic about unknown outcomes than MAA students.[23]

4.3 Effects of Self-Selection on Laboratory Outcomes

The Round 2 results demonstrate that in the baseline ambiguous email treatment, MAA subjects are less likely to participate. This self-selection means little if it does not alter laboratory outcomes. I consider two scenarios. First, a researcher might not be explicitly interested in ambiguity aversion in their laboratory research but ambiguity aversion might plausibly affect subjects' behavior. I determine how selection into the laboratory based on ambiguity aversion may affect lab outcomes in this scenario by looking at the responses in the public good dictator game. Second, a researcher may want to develop parameter estimations for ambiguity aversion using laboratory sessions. By looking at the Round 2 responses to the Ellsberg urn gambles (the replication of the Round 1 instrument) I determine how selection may affect such parameter estimations

4.3.1 Public Good Dictator Game

The public-good dictator game provided a possible environment in which MAA and LAA play may differ in Round 2. However, despite the fact that non-anonymity effects were controlled, the high rate of giving observed by both MAA and LAA subjects yielded no observable differences between MAA and LAA play. Of the 36 subjects participating in Round 2, 30 subjects gave to at least one of the charities while 15

[23] As stated earlier, this analysis was based on 36 observations and thus is a low powered test. A significant correlation may be found with a larger sample.

subjects gave equally to both charities. Table 8 below provides summary statistics for giving behavior in the public good dictator.

Table 8
Giving Summary Statistics
Means & Standard Errors

Sample Frame:	All	More Ambiguity Averse in Round 1	Less Ambiguity Averse in Round 1	t-stat: (2)<>(3)	More Ambiguity Averse in Round 2	Less Ambiguity Averse in Round 2	t-stat: (5)<>(6)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Subjects Gave To At Least One Charity	0.848	0.900	0.826	-0.530	0.938	0.765	-1.382
	(0.063)	(0.100)	(0.081)		(.063)	(.106)	
Subjects Gave Equally To Both Charities	0.545	0.500	0.565	0.336	0.563	0.529	-0.185
	(.088)	(.167)	(.106)		(.128)	(.125)	
Subjects GaveTo Known Charity	0.758	0.800	0.739	-0.364	0.875	0.647	-1.540
	(.076)	(.133)	(.094)		(.085)	(.119)	
Subjects Gave To Unknown Charity	0.727	0.900	0.652	-1.473	0.813	0.647	-1.052
	(.079)	(.100)	(.102)		(.101)	(.119)	
Amount Subjects Gave in $ To Known Charity	2.606	2.700	2.565	-0.175	3	2.24	-1.102
	(.348)	(.716)	(.402)		(.508)	(.474)	
Amount Subjcects Gave in $ To Unkown Charity	2.510	3.300	2.174	-1.294	2.500	2.530	0.036
	(.404)	(.895)	(.425)		(.483)	(.654)	

Standard errors are in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. A subject is Less Ambiguity Averse if they began choosing the unknown urn when betting on the red ball when the known distribution was 40 red, 60 black. A subject is More Ambiguity Averse if they began choosing the unknown urn when betting on the red ball when the known distribution was 30 red, 70 black or higher. Subjects with inconsistent responses were excluded in the two-sided t tests.

The two-sample t-tests presented in Table 8 confirm that there is no significant difference in giving behavior between by either MAA or LAA subjects. Furthermore, Wilcoxon rank-sum tests of the amount given to both known and unknown charities confirm that there is no significant difference in giving by subjects classified in Round 1 as either MAA or LAA subjects (p-values: .93 and .33, respectively). Similarly, Wilcoxon rank-sum tests of the amount given to both known and unknown charities confirm that there is no significant difference in giving by subjects classified in Round 2 as either MAA or LAA subjects (p-values: .32 and .60, respectively). To further see if subjects classified as MAA in either Round 1 or Round 2 gave differently than LAA subjects, in Table 9 I report the results of a series of probit models:

Table 9
Determinants of Who Gives
Probit

Binary Dependent Variable: `	Subjects Gave to Unknown Charity (1) Probit	Subjects Gave to Known Charity (2) Probit	Subjects Gave Equally to Both Charities (3) Probit	Subjects Gave to At Least One Charity (4) Probit
More Ambiguity Averse in Round 1	0.238	0.042	-0.073	0.057
	(.139)	(.161)	(.191)	(.121)
More Ambiguity Averse in Round 2	0.146	0.225	0.043	0.167
	(.151)	(.142)	(.176)	(.119)
R^2	0.1402	0.0681	0.004	0.0796

Marginal effects are reported for coefficients. Robust standard errors are in parenthesis. * indicates significance at 10 percent. ** at 5 percent. *** at 1 percent. In column one, the dependant variable equals zero if the subject did not give to the unknown charity and one if the subject did. The individuals who responded inconsistently in Round 1 and therefore could not be classified as More or Less Ambiguity Averse were excluded from this regression.

No significant determinants for giving were found. Since MAA and LAA play did not differ significantly in the public goods dictator game, it provided no insight into how the group composition of the experiment due to self-selection could affect experiment outcomes in the laboratory.

Taken in the context of Eckel and Grossman's (1996)_research on public-good dictator games, there is a plausible explanation for these findings. Eckel and Grossman found that a significant increase in donations occurs when they increase the extent to which a donation goes to a recipient generally agreed to be "deserving." In this experiment, subjects most likely saw the matching donations as a signal that both the known and the unknown charities were "deserving." The fact that the experimenter was willing to match any donation made to either charity indicated to the subject that at least in the eyes of the experimenter, both charities were deserving. Since I did not vary which charity was signaled to be deserving, these results seem to confirm Eckel and Grossman's findings that a major determinant of subject giving is whether or not the charity is "deserving."

4.3.2 Replication of Round 1 Instrument

While I found no difference in giving behavior, the replication of Round 1 instrument provided a clear mode of analysis to determine if self-selection into laboratory experiments can affect laboratory outcomes. Specifically, the replication of the Round 1 instrument will allow me to determine whether parameter estimations of ambiguity aversion would have been skewed by selection into the laboratory. Since some economists use laboratory sessions to estimate parameters, this study provides evidence against the reasonableness of such a practice. If I wanted to estimate parameters for ambiguity aversion, I would have followed Kahn and Sarin's (1988) procedure by asking each subject to indicate the known probability of winning that would make him/her indifferent between the known urn and the ambiguous urn.[24] Still, for the sake of conducting comparative static analysis, my questions provide sufficient cross-sectional variation to conduct the analysis at hand.

Simply comparing responses in Round 1 to Round 2 is not appropriate because three treatment emails provided more information than standard solicitation mechanisms

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[24] Kahn and Sarin (1988) ask their subjects the following:

> There are two urns containing red and black balls. A ball will be chosen randomly from one of the urns; if the ball is red, you will win $100. Urn 1 has an unknown proportion of red and black balls totaling 200. How many red balls (total of red and black balls equals 200) would you put in Urn 2 to make you indifferent between the two urns?

Instead of replicating Kahn and Sarin's (1988) procedure, I decided to expand Ellsberg's (1961) original urn problem to a series of ordered urn questions for a number of reasons. With Kahn and Sarin's method provide a more continuous measure, it requires more complicated analytical reasoning and thus provides the opportunity for subjects to miscalculate and reveal a distribution that in fact they are not indifferent between the two urns. My method is far simpler. However, since my distributions change by 10, there is no exact point at which I can determine indifference. For instance, a subject who deviates from ambiguity averse behavior when the know urn had a distribution of 40 red, 60 black could be indifferent between the known urn and the unknown urn at any distribution from 49 red, 51 black to 40 red, 60 black.

for economic laboratory experiments. To determine whether self-selection could produce non-representative outcomes in the laboratory, I first look to see if the sample of students who participated in Round 2 after receiving the baseline ambiguous email differed from those who did not. Because all four treatments were not perfectly balanced across all demographics, I then investigate how Round 2 responses would differ from Round 1 if everyone in Round 1 were to have received the baseline ambiguous solicitation email. Unfortunately, switching introduces uncertainty as to which response for those who participated in Round 2 represent true preferences. Therefore, I perform both analyses two different ways, First, I assume that the preferences revealed in Round 1 are true preferences. Second, I assume that the preferences in Round 2 are true preferences.

When comparing the urn number where subjects deviated from ambiguity averse behavior, I found a significant difference using a Wilcoxon rank-sum test between those who participated between the subjects who participated in Round 2 and those who did not when assuming that Round 1 behavior revealed true preferences for those who attended Round 2 (p-value: .08). However, when assuming that Round 2 behavior revealed the true preferences of the subjects that participated in that round, using a Wilcoxon rank sum tests I found no significant difference in the urn number where subjects deviated from ambiguous behavior between the subjects who participated in Round 2 and those who did not (p-value: .51). This insignificant difference is due to inconsistent responses between Round 1 and Round 2 of those who participated after receiving the baseline ambiguous email solicitation.

However, this analysis is flawed because it looks only at the subjects who received the baseline ambiguous email. The fact that my treatments were not balanced perfectly across all demographics creates room for error. I perform two hypothetical analyses to answer the question, if everyone in Round 1 received the fully ambiguous email, would the distribution of responses to the modified series of Ellsberg urn questions in Round 2 significantly differed from Round 1. In both analyses I make the following assumption:

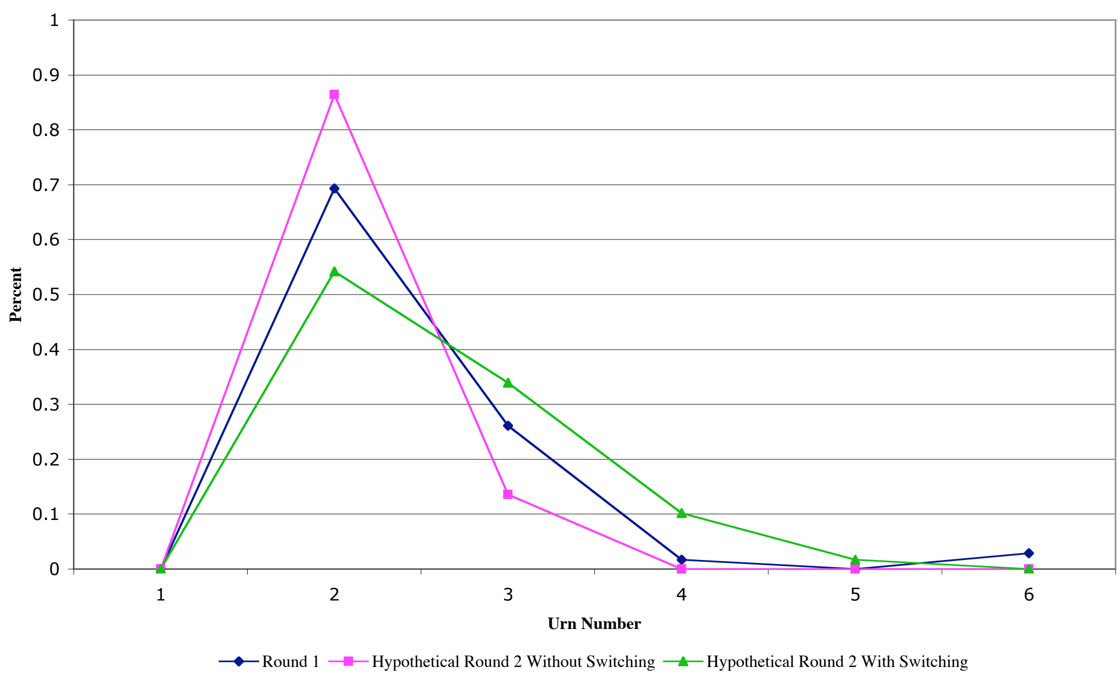
> **Assumption 1:** Subjects who deviated from ambiguity averse behavior at the same Urn number would respond at the exact same rate to the baseline ambiguous email.

The practical meaning of this assumption is explained explicitly in the context of subjects who deviated from ambiguity averse behavior at Urn # 2 as follows. I assumed that subjects who deviated from ambiguity averse behavior at Urn # 2 and were not sent the fully ambiguous email, would respond at the exact same rate as those who deviated at the same decision number and were sent the fully ambiguous email. Out of the 31 subjects who deviated from ambiguity averse behavior at Urn # 2 and were sent the fully ambiguous email, 13 returned. Thus, of the subjects who deviated from ambiguous averse behavior at Urn # 2 and were not sent the fully ambiguous email, I assumed that 42% would have returned to Round 2 had they been sent the fully ambiguous email. Given this assumption, I created a hypothetical dataset based on the observed response rates to the baseline ambiguous email in Round 2. For my first hypothetical analysis I made an additional assumption:

> **Assumption 2:** Responses between Round 1 and Round 2 will remain consistent.

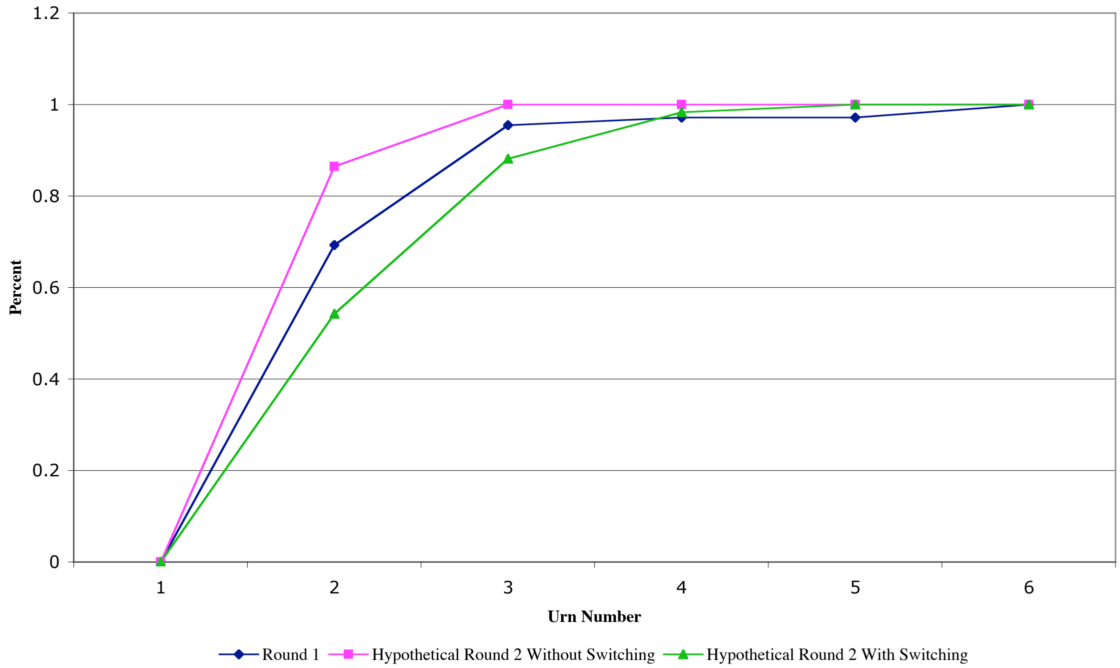
While this assumption runs contradictory to my observations in Round 2, I use it as a basis for my hypothetical analysis to determine how a hypothetical Round 2 data set might differ if all of the subjects in Round 1 were sent the baseline ambiguous email. A two-sample Wilcoxon rank-sum test comparing the hypothetical Round 2 distribution to the observed Round 1 distribution yields a p-value of .01, leading me to conclude that the two distributions are significantly different. In figures 4 and 5, I present the differences in both probability and cumulative distributions.

Figure 4
Comparison of the Probability Distribution Of Round 1



The number of red balls in the known urn can be calculated as follows: # of Red Balls = 50 - (Decision # - 1) * 10. "Hypothetical Round 2 Without Switching" refers to the distribution I created for my first hypothetical analysis while "Hypothetical Round 2 With Switching" refers to the distribution I created for my second hypothetical analysis.

Figure 5
Comparison of the Cumulative Distribution of Round 2



The number of red balls in the known urn can be calculated as follows: # of Red Balls = 50 - (Decision # - 1) * 10. “Hypothetical Round 2 Without Switching” refers to the distribution I created for my first hypothetical analysis while “Hypothetical Round 2 With Switching” refers to the distribution I created for my second hypothetical analysis.

In performing my second hypothetical analysis, I modify assumption 2 to account for the changes in responses observed with-in subjects between Round 1 and Round 2. To create a more realistic hypothetical distribution for Round 2, I assume the following:

> **Assumption 3:** Subjects who were not previously sent the fully ambiguous email in Round 2 will respond consistently with like subjects who attended Round 2.

Assumption 3 simply means that I assume that individuals who did not show up to Round 2 but deviated from ambiguity averse behavior at that same decision number in Round 1 as subjects who did participate, would deviate from their Round 1 responses in a similar manner. The assumption becomes more clear as I explain how I formulated the rules for my second hypothetical analysis.

Using assumption 3, I was able to formulate a series of simple rules as to how the hypothetical subjects attending Round 2 would change their responses from Round 1 to Round 2. To formulate the rules, I categorized the subjects who attended Round 2 according to their Round 1 responses and then looked to see the number of subjects in each category deviated and what response they deviated to. For instance, in Round 2, 23 subjects attended who original deviated from ambiguity averse behavior at Urn #2. Of those 23 subjects, 13 continued to deviate from ambiguity averse behavior at Urn #2, while 7 deviated from ambiguity averse behavior at Urn #3 and 3 deviated from ambiguity averse behavior at Urn #4. Thus, in my hypothetical distribution, 13/23 subjects who deviated from ambiguity averse behavior at Urn #2 in Round 1 would remain consistent, 7/23 would deviate at Urn #3, and 3/23 would deviate at Urn #4. Using this procedure, I created my second hypothetical Round 2 distribution. I present probability and cumulative distributions in figures 4 and 5.

A two-sample Wilcoxon rank-sum test comparing the second hypothetical Round 2 distribution to the observed Round 1 distribution yields a p-value of .03, leading me to conclude that the two distributions are significantly different. If one were to create a population parameter estimate for ambiguity aversion using the hypothetical Round 2 data, one could surely expect that the estimate would differ significantly from an estimate created in Round 1. Regardless of whether we believe Round 1 or Round 2 revealed true preferences, these analyses confirm that an experimenter trying to estimate the ambiguity aversion of the larger sample of Williams College students using laboratory sessions would have skewed results.

5 CONCLUSION

I find that ambiguity may influence selection in laboratory experiments. Specifically, I find that more ambiguity averse individuals are less likely to participate in laboratory experiments when the solicitation sent out is ambiguous in regards to the task and the payment. Since this email treatment closely emulates standard solicitation emails used by experimental economists, these results indicate the possibility of self-selection into a wide range of experimental settings.

The results observed in this study indicate that the degree to which laboratory outcomes may be affected by selection into the laboratory depends on what type of selection is occurring and what type of tasks are performed in the laboratory. In the public-good dictator game, no difference was found in play between more ambiguity averse subjects and less ambiguity averse subjects. However, this does not mean that selection caused by ambiguity aversion is unimportant to other laboratory studies.

The selection I observed has direct significance to economists who use laboratory sessions to measure population parameters. In short, I hope my results dissuade them from the practice. If one were to use a laboratory session to create a population parameter estimate for ambiguity aversion, my results suggest that they would run into a number of problems. First, there is the issue of self-selection into the experiment. If the experimenter were to use a common email solicitation that is analogous to the fully ambiguous email treatment, then self-selection that could skew the estimate is likely to occur. Second, there is the issue of inconsistent responses. I have no reason to believe that the inconsistent responses do not represent true preferences. However, a change in time, a change in venue, and a slightly different pay scheme lead to inconsistent responses between Round 1 and Round 2. These two effects taken together will most likely result in a population parameter estimation unrepresentative of the larger population.

Furthermore, laboratory study seeking to gain insight into how to model decisions under ambiguity is likely to be affected by at the very least self-selection into the experiment if such selection is not carefully considered and controlled for by the experimenter. Research into ambiguity aversion is particularly vulnerable because theory provides little insight into how an agent ought to act given their preferences for ambiguity. Kahn and Sarin (1988) modified the subjective utility model to take into consideration preferences for ambiguity. However, in the same paper, they then test their model against laboratory results without careful consideration to who was showing up to their experiment. The resulting modifications to their model and the direction in which they drove future research has the possibility to be wrought with bias and unrepresentative of behavior in general. Parameter estimation in the laboratory already has the problem that the student population, which economists largely draw from, may not be representative of the larger population. However, there is no reason to believe that the issue of selection would not affect other potential subjects in a similar way.

In terms of mitigating self-selection along preferences for ambiguity, the results suggest a simple solution: increase the detail in the solicitation mechanism. However, this suggestion must be added with a caveat. While my results suggest that increasing the detail in regards to the task in a solicitation does not drive down participation by less ambiguity averse individuals, it very well may. Remember, the p-value for the variable for students classified in Round 1 as less ambiguity averse who received the detailed task, ambiguous pay email was .17. Given the small sample size in my experiment, for that treatment in particular, less ambiguity averse individuals may very well be driven away by detail regarding the task. This caveat further complicates how economists must think about mitigating self-selection and warrants further investigation.

The main contribution of this study is showing that self-selection into the laboratory environment can and does occur. Laboratory study is a valuable tool that economists must use wisely. While this study shows that self-selection is possible along one dimension, it does not rule-out self-selection along other dimensions. Remember, economists are not physicists. As a profession, economists study people not particles. People who may superficially appear to be alike may very significantly in behavior. The degree to which this variation matters depends on the researcher's goals. Individual participation in laboratory experiments can be affected by any number of environmental factors out of the control of the experimenter. Scheduling conflicts, personal preferences, different opportunity costs, and any number of other considerations can affect who participates and who does not. To limit such systematic effects, economists must be creative in terms of controlling for self-selection into laboratory experiments while taking into account how such selection could affect their end results.

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Appendix A- Round 1 Instrument

In this experiment you will be asked to make a series of choices. In each scenario, there are two urns. Both will always contain 100 balls, each ball being either red or black. In each scenario, you will know the exact number of black and red balls in one urn, but you will not know the number of each color in the second urn, only that there are 100 balls in the second urn and every ball is either red or black. The balls are well mixed so that each individual ball is as likely to be drawn as any other.

After all questionnaires have been completed, the experimenter will select at random one-third of all questionnaires. For each questionnaire selected, the experimenter will randomly select one of the five scenarios, with each scenario as likely to be drawn as any other. The experimenter will then randomly select one of the two questions within the selected scenario, with each question as likely to be selected as the other. Finally, if your questionnaire is selected, a ball will be drawn on your behalf, with each ball as likely to be drawn as any other.

Finally, please note that <u>there are no tricks in this experiment</u>. While in each scenario there is an urn for which you do not know the number of black and red balls, the number of unknown balls of each type already has been selected at random and is on file with the Williams College Department of Economics. Likewise, the pull of a ball from a chosen urn will truly be done at random via a process overseen by the Department of Economics.

DECISION #1

URN A: 100 BALLS: 50 RED, 50 BLACK
URN C: 100 BALLS: ? RED, ? BLACK

IF I WERE TO GIVE YOU $10 IF YOU PULLED A RED BALL ON YOUR FIRST TRY, FROM WHICH URN WOULD YOU CHOOSE TO DRAW?

☐ URN A ☐ URN C

IF I WERE TO GIVE YOU $10 IF YOU PULLED A BLACK BALL ON YOUR FIRST TRY, FROM WHICH URN WOULD YOU CHOOSE TO DRAW?

☐ URN A ☐ URN C

DECISION #2

URN D: 100 BALLS: 40 RED, 60 BLACK
URN E: 100 BALLS: ? RED, ? BLACK

IF I WERE TO GIVE YOU $10 IF YOU PULLED A RED BALL ON YOUR FIRST TRY, FROM WHICH URN WOULD YOU CHOOSE TO DRAW?

☐ RN D ☐ RN E

If I were to give you $10 if you pulled a black ball on your first try, from which urn would you choose to draw?

☐ RN D ☐ RN E

Decision #3

Urn F: 100 balls: 30 red, 70 black
Urn G: 100 balls: ? red, ? black

If I were to give you $10 if you pulled a red ball on your first try, from which urn would you choose to draw?

☐ RN F ☐ RN G

If I were to give you $10 if you pulled a black ball on your first try, from which urn would you choose to draw?

☐ RN F ☐ RN G

Decision #4

Urn H: 100 balls: 20 red, 80 black
Urn I: 100 balls: ? red, ? black

If I were to give you $10 if you pulled a red ball on your first try, from which urn would you choose to draw?

☐ RN H ☐ RN I

If I were to give you $10 if you pulled a black ball on your first try, from which urn would you choose to draw?

☐ RN H ☐ RN I

Decision #5

Urn J: 100 balls: 10 red, 90 black
Urn K: 100 balls: ? red, ? black

If I were to give you $10 if you pulled a red ball on your first try, from which urn would you choose to draw?

☐ RN J ☐ RN K

If I were to give you $10 if you pulled a black ball on your first try,

FROM WHICH URN WOULD YOU CHOOSE TO DRAW?

☐ URN J ☐ URN K

PERSONAL INFORMATION:

NAME:

GENDER: M OR F

ACAMDEMIC YEAR: 2007 2008 2009 2010

MAJOR(S):

NATIONALITY:

NATIONALITY AT BIRTH:

EMAIL ADDRESS:

APPENDIX B- ROUND 1 DATA

The response to each question was recorded as either a 1, when the student selected the known urn, or a 0, when the student selected the unknown urn. Each sessions data was recorded independently and then incorporated in a master list of either ambiguity loving or ambiguity averse students. Responses in the data correlate to the 10 responses recorded in the instrument (See instrument in Appendix A). Students gave two responses per urn scenario. Response 1 correlates to the first response, Response 2 the second response etc.

Less Ambiguity Averse Master List

Student #	Response 1	Response 2	Response 3	Response 4	Response 5	Response 6	Response 7	Response 8	Response 9	Response 10
2	1	1	0	1	0	1	0	1	0	1
3	1	1	0	1	0	1	0	1	0	1
4	1	1	0	0	0	1	0	1	0	1
5	1	1	0	1	0	1	0	1	0	1
6	1	1	0	1	0	1	0	1	0	1
7	1	1	0	1	0	1	0	1	0	1
8	1	1	0	1	0	1	0	1	0	1
9	1	1	0	1	0	1	0	1	0	1
11	1	1	0	1	0	1	0	1	0	1
13	1	1	0	1	0	1	0	1	0	1
14	1	1	0	1	0	1	0	1	0	1
16	1	1	0	1	0	1	0	1	0	1
17	1	1	0	1	0	1	0	1	0	1
19	1	1	0	1	0	1	0	1	0	1
20	1	1	0	1	0	1	0	1	0	1
21	1	1	0	1	0	1	0	1	0	1
22	1	1	0	1	0	1	0	1	0	1
23	1	1	0	1	0	1	0	1	0	1
24	1	1	0	1	0	1	0	1	0	1
25	1	1	0	1	0	1	0	1	0	1
26	1	1	0	1	0	1	0	1	0	1
27	1	1	0	1	0	1	0	1	0	1
28	1	1	0	1	0	1	0	1	0	1
29	1	1	0	1	0	1	0	1	0	1
30	1	1	0	1	0	1	0	1	0	1
31	1	1	0	1	0	1	0	1	0	1
32	1	1	0	1	0	1	0	1	0	1
33	1	1	0	1	0	1	0	1	0	1
36	1	1	0	1	0	1	0	1	0	1
37	1	1	0	1	0	1	0	1	0	1
38	1	1	0	1	0	1	0	1	0	1
39	1	1	0	1	0	1	0	1	0	1
40	1	1	0	1	0	1	0	1	0	1
41	1	1	0	1	0	1	0	1	0	1
42	1	1	0	1	0	1	0	1	0	1
43	1	1	0	1	0	1	0	1	0	1
44	1	1	0	1	0	1	0	1	0	1
45	1	1	0	1	0	1	0	1	0	1
46	1	1	0	1	0	1	0	1	0	1
47	1	1	0	1	0	1	0	1	0	1
48	1	1	0	1	0	1	0	1	0	1
49	1	1	0	1	0	1	0	1	0	1
50	1	1	0	1	0	1	0	1	0	1
51	1	1	0	1	0	1	0	1	0	1
52	1	1	0	1	0	1	0	1	0	1
53	1	1	0	1	0	1	0	1	0	1
54	1	1	0	1	0	1	0	1	0	1
55	1	1	0	1	0	1	0	1	0	1
56	1	1	0	1	0	1	0	1	0	1
57	1	1	0	1	0	1	0	1	0	1
58	0	0	0	0	0	0	0	0	0	0
59	1	1	0	1	0	1	0	1	0	1
60	1	1	0	0	0	0	0	0	0	0
61	1	1	0	1	0	1	0	1	0	1
62	0	0	0	0	0	1	0	1	0	1
74	1	1	0	1	0	1	0	1	0	1
75	1	1	0	1	0	1	0	1	0	1
76	1	1	0	1	0	1	0	1	0	1
77	1	1	0	1	0	1	0	1	0	1
78	1	1	0	1	0	1	0	1	0	1
79	1	1	0	1	0	1	0	1	0	1
80	1	1	0	1	0	1	0	1	0	1
81	1	1	0	1	0	1	0	1	0	1
82	1	1	0	1	0	1	0	1	0	1
83	1	1	0	1	0	1	0	1	0	1
84	1	1	0	1	0	1	0	1	0	1
85	1	1	0	1	0	1	0	1	0	1
86	1	1	0	1	0	1	0	1	0	1
107	1	1	0	1	0	1	0	1	0	1
108	1	1	0	1	0	1	0	1	0	1
109	1	1	0	1	0	1	0	1	0	1
110	1	1	0	1	0	1	0	1	0	1
111	1	1	0	1	0	1	0	1	0	1
112	1	1	0	1	0	1	0	1	0	1
113	1	1	0	1	0	1	0	1	0	1
114	1	1	0	1	0	1	0	1	0	1
115	1	1	0	1	0	1	0	1	0	1
116	1	1	0	1	0	1	0	1	0	1
117	1	1	0	1	0	1	0	1	0	1
118	1	1	0	1	0	1	0	1	0	1
119	1	1	0	1	0	1	0	1	0	1
120	1	1	0	1	0	1	0	1	0	1
121	1	1	0	1	0	1	0	1	0	1
122	1	1	0	1	0	1	0	1	0	1
156	1	1	0	1	0	1	0	1	0	1
157	1	1	0	1	0	1	0	1	0	1
158	1	1	0	1	0	1	0	1	0	1
159	1	1	0	1	0	1	0	1	0	1
160	1	1	0	1	0	1	0	1	0	1
161	1	1	0	1	0	1	0	1	0	1
162	1	1	0	1	0	1	0	1	0	1
163	1	1	0	1	0	1	0	1	0	1
164	1	1	0	1	0	1	0	1	0	1
165	1	1	0	1	0	1	0	1	0	1
166	1	1	0	1	0	1	0	1	0	1
167	1	1	0	1	0	1	0	1	0	1
168	1	1	0	1	0	1	0	1	0	1
169	1	1	0	1	0	1	0	1	0	1
170	1	1	0	1	0	1	0	1	0	1
171	1	1	0	1	0	1	0	1	0	1
172	1	1	0	1	0	1	0	1	0	1
173	1	1	0	1	0	1	0	1	0	1
174	1	1	0	1	0	1	0	1	0	1
176	1	1	1	0	1	0	1	0	1	0
177	1	1	1	0	1	0	1	0	1	0
178	1	1	1	0	1	0	1	0	1	0
193	1	1	0	1	0	1	0	1	0	1
194	1	1	0	1	0	1	0	1	0	1
195	1	1	0	1	0	1	0	1	0	1
196	1	1	0	1	0	1	0	1	0	1
197	1	1	0	1	0	1	0	1	0	1
198	1	1	0	1	0	1	0	1	0	1
199	1	1	0	1	0	1	0	1	0	1
200	1	1	0	1	0	1	0	1	0	1
201	1	1	0	1	0	1	0	1	0	1

MORE AMBIGUITY SENSITIVITY MASTER LIST

Student #	Response 1	Response 2	Response 3	Response 4	Response 5	Response 6	Response 7	Response 8	Response 9
1	1	1	1	1	0	1	0	1	0
10	1	1	1	1	0	1	0	1	0
12	1	1	1	1	0	1	0	1	0
15	1	1	1	1	0	1	0	1	0
18	1	1	1	1	0	1	0	1	0
34	1	1	1	1	0	1	0	1	0
35	1	1	1	1	0	1	0	1	0
63	1	1	1	1	0	1	0	1	0
64	1	1	1	1	0	1	0	1	0
65	1	1	1	1	0	1	0	1	0
66	1	1	1	1	0	1	0	1	0
67	1	1	1	1	0	1	0	1	0
68	1	1	1	1	0	1	0	1	0
69	1	1	1	1	0	1	0	1	0
70	1	1	1	1	0	1	0	1	0
71	1	1	1	1	0	1	0	1	0
72	1	1	1	1	0	1	0	1	0
73	1	1	1	1	0	1	0	1	0
87	1	1	1	1	0	1	0	1	0
88	1	1	1	1	0	1	0	1	0
89	1	1	1	1	0	1	0	1	0
90	1	1	1	1	0	1	0	1	0
91	1	1	1	1	0	1	0	1	0
92	1	1	1	1	1	1	1	1	1
96	1	1	1	1	1	1	1	1	1
97	1	1	1	1	0	1	0	1	0
98	1	1	1	1	0	1	0	1	0
99	1	1	1	1	0	1	0	1	0
100	1	1	1	1	0	1	0	1	0
101	1	1	1	1	0	1	0	1	0
102	1	1	1	1	0	1	0	1	0
103	1	1	1	1	0	1	0	1	0
104	1	1	1	1	0	1	0	1	0
105	1	1	1	1	0	1	0	1	0
106	1	1	1	1	0	1	0	1	0
137	1	1	1	1	1	1	1	1	1
138	1	1	1	1	1	1	0	1	0
139	1	1	1	1	0	1	0	1	0
148	1	1	1	1	0	1	0	1	0
149	1	1	1	1	0	1	0	1	0
150	1	1	1	1	0	1	0	1	0
151	1	1	1	1	0	1	0	1	0
152	1	1	1	1	0	1	0	1	0
153	1	1	1	1	0	1	0	1	0
154	1	1	1	1	0	1	0	1	0
155	1	1	1	1	0	1	0	1	0
143	1	1	1	1	1	1	0	1	0
187	1	1	1	1	1	1	1	1	1
188	1	1	1	1	0	1	0	1	0
189	1	1	1	1	0	1	0	1	0
190	1	1	1	1	0	1	0	1	0
191	1	1	1	1	0	1	0	1	0
192	1	1	1	1	0	1	0	1	0

Session 1: October 26th, 2006 - Professor Gazzale's ECON 110 8:55 am Class

Student #	Response 1	Response 2	Response 3	Response 4	Response 5	Response 6	Response 7	Response 8	Response 9	Response 10
1	1	1	1	1	0	1	0	1	0	1
2	1	1	0	1	0	1	0	1	0	1
3	1	1	0	1	0	1	0	1	0	1
4	1	1	0	0	0	1	0	1	0	1
5	1	1	0	1	0	1	0	1	0	1
6	1	1	0	1	0	1	0	1	0	1
7	1	1	0	1	0	1	0	1	0	1
8	1	1	0	1	0	1	0	1	0	1
9	1	1	0	1	0	1	0	1	0	1
10	1	1	1	1	0	1	0	1	0	1
11	1	1	0	1	0	1	0	1	0	1
12	1	1	1	1	0	1	0	1	0	1
13	1	1	0	1	0	1	0	1	0	1
14	1	1	0	1	0	1	0	1	0	1
15	1	1	1	1	0	1	0	1	0	1
16	1	1	0	1	0	1	0	1	0	1
17	1	1	0	1	0	1	0	1	0	1
18	1	1	1	1	0	1	0	1	0	1
19	1	1	0	1	0	1	0	1	0	1
20	1	1	0	1	0	1	0	1	0	1
21	1	1	0	1	0	1	0	1	0	1
22	1	1	0	1	0	1	0	1	0	1
23	1	1	0	1	0	1	0	1	0	1
24	1	1	0	1	0	1	0	1	0	1
25	1	1	0	1	0	1	0	1	0	1
26	1	1	0	1	0	1	0	1	0	1
27	1	1	0	1	0	1	0	1	0	1
28	1	1	0	1	0	1	0	1	0	1
29	1	1	0	1	0	1	0	1	0	1
30	1	1	0	1	0	1	0	1	0	1
31	1	1	0	1	0	1	0	1	0	1
32	1	1	0	1	0	1	0	1	0	1
33	1	1	0	1	0	1	0	1	0	1
34	1	1	1	1	0	1	0	1	0	1
35	1	1	1	1	0	1	0	1	0	1

Session 2: October 26th, 2006- Professor Gazzale's ECON 110 9:55 am Class

Student #	Response 1	Response 2	Response 3	Response 4	Response 5	Response 6	Response 7	Response 8	Response 9	Response 10
36	1	1	0	1	0	1	0	1	0	1
37	1	1	0	1	0	1	0	1	0	1
38	1	1	0	1	0	1	0	1	0	1
39	1	1	0	1	0	1	0	1	0	1
40	1	1	0	1	0	1	0	1	0	1
41	1	1	0	1	0	1	0	1	0	1
42	1	1	0	1	0	1	0	1	0	1
43	1	1	0	1	0	1	0	1	0	1
44	1	1	0	1	0	1	0	1	0	1
45	1	1	0	1	0	1	0	1	0	1
46	1	1	0	1	0	1	0	1	0	1
47	1	1	0	1	0	1	0	1	0	1
48	1	1	0	1	0	1	0	1	0	1
49	1	1	0	1	0	1	0	1	0	1
50	1	1	0	1	0	1	0	1	0	1
51	1	1	0	1	0	1	0	1	0	1
52	1	1	0	1	0	1	0	1	0	1
53	1	1	0	1	0	1	0	1	0	1
54	1	1	0	1	0	1	0	1	0	1
55	1	1	0	1	0	1	0	1	0	1
56	1	1	0	1	0	1	0	1	0	1
57	1	1	0	1	0	1	0	1	0	1
58	0	0	0	0	0	0	0	0	0	0
59	1	1	0	1	0	1	0	1	0	1
60	1	1	0	0	0	0	0	0	0	0
61	1	1	0	1	0	1	0	1	0	1
62	0	0	0	0	0	1	0	1	0	1
63	1	1	1	1	0	1	0	1	0	1
64	1	1	1	1	0	1	0	1	0	1
65	1	1	1	1	0	1	0	1	0	1
66	1	1	1	1	0	1	0	1	0	1
67	1	1	1	1	0	1	0	1	0	1
68	1	1	1	1	0	1	0	1	0	1
69	1	1	1	1	0	1	0	1	0	1
70	1	1	1	1	0	1	0	1	0	1
71	1	1	1	1	0	1	0	1	0	1
72	1	1	1	1	0	1	0	1	0	1
73	1	1	1	1	0	1	0	1	0	1

Session 3: November 1, 2006- Professor Love's ECON 120 8:30 am Class

Student #	Response 1	Response 2	Response 3	Response 4	Response 5	Response 6	Response 7	Response 8	Response 9	Response 10
74	1	1	0	1	0	1	0	1	0	1
75	1	1	0	1	0	1	0	1	0	1
76	1	1	0	1	0	1	0	1	0	1
77	1	1	0	1	0	1	0	1	0	1
78	1	1	0	1	0	1	0	1	0	1
79	1	1	0	1	0	1	0	1	0	1
80	1	1	0	1	0	1	0	1	0	1
81	1	1	0	1	0	1	0	1	0	1
82	1	1	0	1	0	1	0	1	0	1
83	1	1	0	1	0	1	0	1	0	1
84	1	1	0	1	0	1	0	1	0	1
85	1	1	0	1	0	1	0	1	0	1
86	1	1	0	1	0	1	0	1	0	1
87	1	1	1	1	0	1	0	1	0	1
88	1	1	1	1	0	1	0	1	0	1
89	1	1	1	1	0	1	0	1	0	1
90	1	1	1	1	0	1	0	1	0	1
91	1	1	1	1	0	1	0	1	0	1
92	1	1	1	1	1	1	1	1	1	1
93	0	0	0	0	0	1	0	1	0	1
94	0	0	0	1	0	1	0	1	0	1

Session 4: November 14, 2006- Schow

Student #	Response 1	Response 2	Response 3	Response 4	Response 5	Response 6	Response 7	Response 8	Response 9	Response 10
95	0	0	0	1	0	1	0	1	0	1
96	1	1	1	1	1	1	1	1	1	1
97	1	1	1	1	0	1	0	1	0	1
98	1	1	1	1	0	1	0	1	0	1
99	1	1	1	1	0	1	0	1	0	1
100	1	1	1	1	0	1	0	1	0	1
101	1	1	1	1	0	1	0	1	0	1
102	1	1	1	1	0	1	0	1	0	1
103	1	1	1	1	0	1	0	1	0	1
104	1	1	1	1	0	1	0	1	0	1
105	1	1	1	1	0	1	0	1	0	1
106	1	1	1	1	0	1	0	1	0	1
107	1	1	0	1	0	1	0	1	0	1
108	1	1	0	1	0	1	0	1	0	1
109	1	1	0	1	0	1	0	1	0	1
110	1	1	0	1	0	1	0	1	0	1
111	1	1	0	1	0	1	0	1	0	1
112	1	1	0	1	0	1	0	1	0	1
113	1	1	0	1	0	1	0	1	0	1
114	1	1	0	1	0	1	0	1	0	1
115	1	1	0	1	0	1	0	1	0	1
116	1	1	0	1	0	1	0	1	0	1
117	1	1	0	1	0	1	0	1	0	1
118	1	1	0	1	0	1	0	1	0	1
119	1	1	0	1	0	1	0	1	0	1
120	1	1	0	1	0	1	0	1	0	1
121	1	1	0	1	0	1	0	1	0	1
122	1	1	0	1	0	1	0	1	0	1

Session 5: November 16, 2006- Sawyer

Student #	Response 1	Response 2	Response 3	Response 4	Response 5	Response 6	Response 7	Response 8	Response 9	Response 10
123	1	1	0	0	0	1	0	1	0	1
124	1	1	0	1	0	1	0	1	0	1
125	1	1	0	1	0	1	0	1	0	1
126	1	1	0	1	0	1	0	1	0	1
127	1	1	0	1	0	1	0	1	0	1
128	1	1	0	1	0	1	0	1	0	1
129	1	1	0	1	0	1	0	1	0	1
130	1	1	0	1	0	1	0	1	0	1
131	1	1	0	1	0	1	0	1	0	1
132	1	1	0	1	0	1	0	1	0	1
133	1	1	0	1	0	1	0	1	0	1
134	1	1	0	1	0	1	0	1	0	1
135	1	1	0	1	0	1	0	1	0	1
136	1	1	0	1	0	1	0	1	0	1
137	1	1	1	1	1	1	1	1	1	1
138	1	1	1	1	1	1	0	1	0	1
139	1	1	1	1	0	1	0	1	0	1

Session 6: November 27, 2006- Schow

Student #	Response 1	Response 2	Response 3	Response 4	Response 5	Response 6	Response 7	Response 8	Response 9	Response 10
176	1	1	1	0	1	0	1	0	1	0
177	1	1	1	0	1	0	1	0	1	0
178	1	1	1	0	1	0	1	0	1	0
179	1	0	1	1	0	1	0	1	0	1
180	1	0	0	1	0	1	0	1	0	1
181	0	0	0	1	0	1	0	1	0	1
182	0	0	0	1	0	1	0	1	0	1
183	0	1	0	1	0	1	0	1	0	1
184	0	0	1	1	0	1	0	1	0	1
185	1	1	0	0	0	0	1	0	1	1
186	0	0	0	1	0	1	0	1	0	1
187	1	1	1	1	1	1	1	1	1	1
188	1	1	1	1	0	1	0	1	0	1
189	1	1	1	1	0	1	0	1	0	1
190	1	1	1	1	0	1	0	1	0	1
191	1	1	1	1	0	1	0	1	0	1
192	1	1	1	1	0	1	0	1	0	1
193	1	1	0	1	0	1	0	1	0	1
194	1	1	0	1	0	1	0	1	0	1
195	1	1	0	1	0	1	0	1	0	1
196	1	1	0	1	0	1	0	1	0	1
197	1	1	0	1	0	1	0	1	0	1
198	1	1	0	1	0	1	0	1	0	1
199	1	1	0	1	0	1	0	1	0	1
200	1	1	0	1	0	1	0	1	0	1
201	1	1	0	1	0	1	0	1	0	1

Session 7: December 3, 2006- Schow

Student #	Response 1	Response 2	Response 3	Response 4	Response 5	Response 6	Response 7	Response 8	Response 9	Response 10
140	1	1	1	1	1	1	1	1	1	1
141	0	0	1	1	1	1	1	1	0	1
142	1	1	1	1	1	1	0	1	0	1
143	1	1	1	1	1	1	0	1	0	1
145	0	0	0	0	0	1	0	1	0	1
146	0	0	0	0	0	1	0	1	0	1
147	0	1	0	1	0	1	0	1	0	1
148	1	1	1	1	0	1	0	1	0	1
149	1	1	1	1	0	1	0	1	0	1
150	1	1	1	1	0	1	0	1	0	1
151	1	1	1	1	0	1	0	1	0	1
152	1	1	1	1	0	1	0	1	0	1
153	1	1	1	1	0	1	0	1	0	1
154	1	1	1	1	0	1	0	1	0	1
155	1	1	1	1	0	1	0	1	0	1
156	1	1	0	1	0	1	0	1	0	1
157	1	1	0	1	0	1	0	1	0	1
158	1	1	0	1	0	1	0	1	0	1
159	1	1	0	1	0	1	0	1	0	1
160	1	1	0	1	0	1	0	1	0	1
161	1	1	0	1	0	1	0	1	0	1
162	1	1	0	1	0	1	0	1	0	1
163	1	1	0	1	0	1	0	1	0	1
164	1	1	0	1	0	1	0	1	0	1
165	1	1	0	1	0	1	0	1	0	1
166	1	1	0	1	0	1	0	1	0	1
167	1	1	0	1	0	1	0	1	0	1
168	1	1	0	1	0	1	0	1	0	1
169	1	1	0	1	0	1	0	1	0	1
170	1	1	0	1	0	1	0	1	0	1
171	1	1	0	1	0	1	0	1	0	1
172	1	1	0	1	0	1	0	1	0	1
173	1	1	0	1	0	1	0	1	0	1
174	1	1	0	1	0	1	0	1	0	1
175	0	0	0	1	0	1	0	1	0	1

APPENDIX C- EMAILS

Ambiguous Email
I am writing to inform you of an opportunity to participate in an Economics Department experiment on Tuesday, February 20th from 7:00 PM until 10:00 PM in Hopkins 108. You will earn either $10 or $20 by participating in this experiment and the session will last about 30 minutes.

To sign up to participate in this experiment, please click on the link below.

Detailed Task, Ambiguous Payment Email
I am writing to inform you of an opportunity to participate in an Economics Department experiment on Tuesday, February 20th from 7:00 PM until 10:00pm in Hopkins 108. You will earn either $10 or $20 by participating in this experiment, and the session will last about 30 minutes. The experiment will consist of two sections.

In the first section, you will have the opportunity, in private, to donate part of your show up fee to a charity. The amount you give to the charity will be matched by the experimenter.

In the second section, you will be asked to make a series of decisions. For each decision, you will be asked to choose one of two options where the outcome of each option is uncertain. In some decisions, you will know the probability of each outcome within each option. In other decisions, you will not know the probability of each outcome for one of the options. After you have made your decisions, we will randomly select some of your decisions and you will be paid according to your choices.

If you are interested in participating in this study please click the link at the bottom.

Ambiguous Task, Detailed Payment Email
I am writing to inform you of an opportunity to participate in an Economics Department experiment on Tuesday, February 20th from 7:00 PM until 10:00 PM in Hopkins 108. You will earn either $10 or $20 by participating in this experiment. The experiment is designed so that you have a 50% chance of earning $10 and a 50% chance of earning $20. The session will last about 30 minutes.

To sign up to participate in this experiment, please click on the link below.

Detailed Task, Detailed Payment Email
I am writing to inform you of an opportunity to participate in an Economics Department experiment on Tuesday, February 20th from 7:00 PM until 10:00pm in Hopkins 108. You will earn either $10 or $20 by participating in this experiment. The experiment is designed so that you have a 50% chance of earning $10 and a 50% chance of earning $20. The session will last about 30 minutes. The experiment will consist of two sections.

In the first section, you will have the opportunity, in private, to donate part of your show up fee to a charity. The amount you give to the charity will be matched by the experimenter.

In the second section, you will be asked to make a series of decisions. For each decision, you will be asked to choose one of two options where the outcome of each option is uncertain. In some decisions, you will know the probability of each outcome within each option. In other decisions, you will not know the probability of each outcome for one of the options. After you have made your decisions, we will randomly select some of your decisions and you will be paid according to your choices.

If you are interested in participating in this study please click the link at the bottom.

APPENDIX D- ROUND 2 INSTRUMENT

Version 1:

Welcome to this experiment on decision-making and thank you for being here. You will be compensated for your participation in the experiment, though the exact amount you will receive will depend on the choices you make, and on random chance (as explained below). Please pay careful attention to these instructions, as a significant amount of money is at stake.

Information about the choices that you make during the experiment will be kept strictly confidential. In order to maintain privacy and confidentiality, please do not speak to anyone during the experiment and please do not discuss your choices with anyone even after the conclusion of the experiment.

This experiment has four parts. First, you will be asked to make a series of decisions regarding charitable donations. Second, you will be asked to make a series of decisions between two lotteries based on the rolling of a die (a computer will conduct the actual randomization). Then you will be asked to make a series of decisions based on drawing a ball from an urn. Finally, you will be asked a series of questions with which you will either agree or disagree with along a scale. More detailed instructions will follow in each section.

Part I:

Today you received a show up fee of X dollars. You will now have the opportunity to share part or all of that show up fee with one or both of two charities. You may donate as much or as little of your show up fee to each of these charities as you wish. Please indicate below the amount you would like to donate and sign where indicated.

1. Habitat for Humanity, a nonprofit organization that builds home for those in need which has been instrumental in Hurricane Katrina relief efforts. The experimenter will match any amount that you donate to Habitat for Humanity.

2. A nonprofit organization that works to alleviate poverty. The experimenter will match any amount that you donate to this organization.

Part II:

You will be making 10 choices, such as those represented as “Option A” and “Option B” below. The money prizes are determined by the computer equivalent of rolling a ten-sided die. Each outcome, 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, is equally likely. Even though you will make ten decisions, **only one** of these will end up being used to determine your payment. For each questionnaire, the experimenter will randomly select one of ten decisions with each decision being as likely to be drawn as the other. Finally, a computer generated “roll” for that decision will be made and you will be paid based on your decision.

Decision 1	
If you choose Option A in the row shown below, you will have a 1 in 10 chance of earning $11 and a 9 in 10 chance of earning $8.80. Similarly, Option B offers a 1 in 10 chance of earning $21.20 and a 9 in 10 chance of earning $0.55.	
Option A $11 if the die is 1 $8.80 if the die is 2-10	Option B $21.20 if the die is 1 $0.55 if the die is 2-10
☐ Option A	☐ Option B

Decision 2	
If you choose Option A in the row shown below, you will have a 2 in 10 chance of earning $11 and a 8 in 10 chance of earning $8.80. Similarly, Option B offers a 2 in 10 chance of earning $21.20 and an 8 in 10 chance of earning $0.55.	
Option A $11 if the die is 1-2 $8.80 if the die is 3-10	Option B $21.20 if the die is 1-2 $0.55 if the die is 3-10
☐ Option A	☐ Option B

Decision 3
If you choose Option A in the row shown below, you will have a 3 in 10 chance of earning $11 and a 7 in 10 chance of earning $8.80. Similarly, Option B offers a 3 in 10 chance of earning $21.20 and a 7 in 10 chance of earning $0.55. Option A: $11 if the die is 1-3; $8.80 if the die is 4-10 Option B: $21.20 if the die is 1-3; $0.55 if the die is 4-10 ☐ Option A ☐ Option B

Decision 4
If you choose Option A in the row shown below, you will have a 4 in 10 chance of earning $11 and a 6 in 10 chance of earning $8.80. Similarly, Option B offers a 4 in 10 chance of earning $21.20 and a 6 in 10 chance of earning $0.55. Option A: $11 if the die is 1-4; $8.80 if the die is 5-10 Option B: $21.20 if the die is 1-4; $0.55 if the die is 5-10 ☐ Option A ☐ Option B

Decision 5
If you choose Option A in the row shown below, you will have a 5 in 10 chance of earning $11 and a 5 in 10 chance of earning $8.80. Similarly, Option B offers a 5 in 10 chance of earning $21.20 and a 5 in 10 chance of earning $0.55. Option A: $11 if the die is 1-5; $8.80 if the die is 6-10 Option B: $21.20 if the die is 1-5; $0.55 if the die is 6-10 ☐ Option A ☐ Option B

Decision 6
If you choose Option A in the row shown below, you will have a 6 in 10 chance of earning $11 and a 4 in 10 chance of earning $8.80. Similarly, Option B offers a 6 in 10 chance of earning $21.20 and a 4 in 10 chance of earning $0.55. Option A $11 if the die is 1-6 $8.80 if the die is 7-10 Option B $21.20 if the die is 1-6 $0.55 if the die is 7-10 ☐ Option A ☐ Option B

Decision 7
If you choose Option A in the row shown below, you will have a 7 in 10 chance of earning $11 and a 3 in 10 chance of earning $8.80. Similarly, Option B offers a 7 in 10 chance of earning $21.20 and a 3 in 10 chance of earning $0.55. Option A $11 if the die is 1-7 $8.80 if the die is 8-10 Option B $21.20 if the die is 1-7 $0.55 if the die is 8-10 ☐ Option A ☐ Option B

Decision 8
If you choose Option A in the row shown below, you will have a 8 in 10 chance of earning $11 and a 2 in 10 chance of earning $8.80. Similarly, Option B offers an 8 in 10 chance of earning $21.20 and a 2 in 10 chance of earning $0.55. Option A $11 if the die is 1-8 $8.80 if the die is 9-10 Option B $21.20 if the die is 1-8 $0.55 if the die is 9-10 ☐ Option A ☐ Option B

Decision 9
If you choose Option A in the row shown below, you will have a 9 in 10 chance of earning \$11 and a 1 in 10 chance of earning \$8.80. Similarly, Option B offers a 9 in 10 chance of earning \$21.20 and a 1 in 10 chance of earning \$0.55.

Option A	Option B
\$11 if the die is 1-9	\$21.20 if the die is 1-9
\$8.80 if the die is 10	\$0.55 if the die is 10

☐ Option A ☐ Option B

Decision 10
If you choose Option A in the row shown below, you will have a 10 in 10 chance of earning \$11 and a 0 in 10 chance of earning \$8.80. Similarly, Option B offers a 10 in 10 chance of earning \$21.20 and a 0 in 10 chance of earning \$0.55.

Option A	Option B
\$11 if the die is 1-10	\$21.20 if the die is 1-10

☐ Option A ☐ Option B

Part III:
In this section you will be asked to make a series of choices. In each scenario, there are two urns. Both will always contain 100 balls, each ball being either red or black. In each scenario, you will know the exact number of black and red balls in one urn, but you will not know the number of each color in the second urn, only that there are 100 balls in the second urn and every ball is either red or black. The balls are well mixed so that each individual ball is as likely to be drawn as any other.

For each questionnaire selected, the experimenter will randomly select one of the five scenarios, with each scenario as likely to be drawn as any other. The experimenter will then randomly select one of the two questions within the selected scenario, with each question as likely to be selected as the other. Finally, if your questionnaire is selected, a ball will be drawn on your behalf, with each ball as likely to be drawn as any other.

Finally, please note that <u>there are no tricks in this experiment</u>. While in each scenario there is an urn for which you do not know the number of black and red balls, the number of unknown balls of each type already has been selected at random and is on file with the Williams College Department of Economics. Likewise, the pull of a ball from a chosen urn will truly be done at random via a process overseen by the Department of Economics.

Decision # 1

Urn A: 100 balls: 50 red, 50 black Urn C: 100 balls: ? red, ? black
If I were to give you $10 if you pulled a red ball on your first try, from which urn would you choose to draw? ☐ Urn A ☐ Urn C If I were to give you $10 if you pulled a black ball on your first try, from which urn would you choose to draw? ☐ Urn A ☐ Urn C

Decision # 2

Urn D: 100 balls: 40 red, 60 black Urn E: 100 balls: ? red, ? black
If I were to give you $10 if you pulled a red ball on your first try, from which urn would you choose to draw? ☐ Urn D ☐ Urn E If I were to give you $10 if you pulled a black ball on your first try, from which urn would you choose to draw? ☐ Urn D ☐ Urn E

Decision # 3

Urn F: 100 balls: 30 red, 70 black
Urn G: 100 balls: ? red, ? black

If I were to give you $10 if you pulled a red ball on your first try, from which urn would you choose to draw?

☐ Urn F ☐ Urn G

If I were to give you $10 if you pulled a black ball on your first try, from which urn would you choose to draw?

☐ Urn F ☐ Urn G

Decision # 4

Urn H: 100 balls: 20 red, 80 black
Urn I: 100 balls: ? red, ? black

If I were to give you $10 if you pulled a red ball on your first try, from which urn would you choose to draw?

☐ Urn H ☐ Urn I

If I were to give you $10 if you pulled a black ball on your first try, from which urn would you choose to draw?

☐ Urn H ☐ Urn I

Decision # 5

Urn J: 100 balls: 10 red, 90 black
Urn K: 100 balls: ? red, ? black

If I were to give you $10 if you pulled a red ball on your first try, from which urn would you choose to draw?

☐ Urn J ☐ Urn K

If I were to give you $10 if you pulled a black ball on your first try, from which urn would you choose to draw?

☐ Urn J ☐ Urn K

PART IV:

Please answer the below questions according to your own feelings, rather than how you think "most people" would answer. Please be as honest and accurate as you can throughout, there is no right or wrong answer. Also, please try not to let your response to one statement influence your responses to other statements. Think about each statement on its own.

A = I agree a lot
B = I agree a little
C = I neither agree nor disagree
D = I disagree a little
E = I disagree a lot

In uncertain times, I usually expect the best.	A	B	C	D	E
It's easy for me to relax.	A	B	C	D	E
If something can go wrong, it will.	A	B	C	D	E
I'm always optimistic about the future.	A	B	C	D	E
I enjoy my friends a lot.	A	B	C	D	E
It's important for me to keep busy.	A	B	C	D	E
I hardly ever expect things to go my way.	A	B	C	D	E
I don't get upset too easily	A	B	C	D	E
I rarely count on good things happening to me.	A	B	C	D	E
Overall, I expect more good things to happen to me than bad.	A	B	C	D	E

PERSONAL INFORMATION:

NAME:

EMAIL ADDRESS:

Version 2:

Welcome to this experiment on decision-making and thank you for being here. You will be compensated for your participation in the experiment, though the exact amount you will receive will depend on the choices you make, and on random chance (as explained below). Please pay careful attention to these instructions, as a significant amount of money is at stake.

Information about the choices that you make during the experiment will be kept strictly confidential. In order to maintain privacy and confidentiality, please do not speak to anyone during the experiment and please do not discuss your choices with anyone even after the conclusion of the experiment.

This experiment has four parts. First, you will be asked to make a series of decisions regarding charitable donations. Second, you will be asked to make a series of decisions between two lotteries based on the rolling of a die (a computer will conduct the actual randomization). Then you will be asked to make a series of decisions based on drawing a ball from an urn. Finally, you will be asked a series of questions with which you will either agree or disagree with along a scale. More detailed instructions will follow in each section.

Part I:

Today you received a show up fee of X dollars. You will now have the opportunity to share part or all of that show up fee with one or both of two charities. You may donate as much or as little of your show up fee to each of these charities as you wish. Please indicate below the amount you would like to donate and sign where indicated.

3. Oxfam, a nonprofit organization that works to minimize poverty through relief and development work in Africa. The experimenter will match any amount that you donate to Oxfam.

4. A nonprofit organization that works to help victims of natural disasters. The experimenter will match any amount that you donate to this organization.

Version 3:

Welcome to this experiment on decision-making and thank you for being here. You will be compensated for your participation in the experiment, though the exact amount you will receive will depend on the choices you make, and on random chance. Even though you will make twenty decisions, **only one** of these will end up being used to determine your payment. Please pay careful attention to these instructions, as a significant amount of money is at stake.

Information about the choices that you make during the experiment will be kept strictly confidential. In order to maintain privacy and confidentiality, please do not speak to anyone during the experiment and please do not discuss your choices with anyone even after the conclusion of the experiment.

This experiment has four parts. First, you will be asked to make a series of decisions regarding charitable donations. In the second and third sections, you will be asked to make a series of choices between options, where the outcome of each option is not know with certainty. Finally, you will be asked a series of questions with which you will either agree or disagree with along a scale. More detailed instructions will follow in each section.

Part I:

Today you received four envelopes: a “Start” envelope, a “Me” envelope, a “1” envelope, and a “2” envelope. In the start envelope you will find ten $1 bills and ten dollar-size pieces of blank paper. You will now have the opportunity to share part or all of the $10 with one or both of two charities. Any money that you donate to either charity will be matched; meaning every dollar you donate will result in the charity receiving two dollars. You may donate as much or as little of the $10 to each of these charities as you wish by placing dollar bills in the corresponding envelopes. At the end of the experiment, you will keep the “Me” envelope and any dollar bills you place in that envelope.

5. Envelope 1: A nonprofit organization that works to alleviate poverty in Africa.

6. Envelope 2: Habitat for Humanity, a nonprofit organization that builds home for those in need which has been instrumental in Hurricane Katrina relief efforts in the United States.

Version 4:

Welcome to this experiment on decision-making and thank you for being here. You will be compensated for your participation in the experiment, though the exact amount you will receive will depend on the choices you make, and on random chance. Even though you will make twenty decisions, **only one** of these will end up being used to determine your payment. Please pay careful attention to these instructions, as a significant amount of money is at stake.

Information about the choices that you make during the experiment will be kept strictly confidential. In order to maintain privacy and confidentiality, please do not speak to anyone during the experiment and please do not discuss your choices with anyone even after the conclusion of the experiment.

This experiment has four parts. First, you will be asked to make a series of decisions regarding charitable donations. In the second and third sections, you will be asked to make a series of choices between options, where the outcome of each option is not know with certainty. Finally, you will be asked a series of questions with which you will either agree or disagree with along a scale. More detailed instructions will follow in each section.

Part I:

Today you received four envelopes: a “Start” envelope, a “Me” envelope, a “1” envelope, and a “2” envelope. In the start envelope you will find ten $1 bills and ten dollar-size pieces of blank paper. You will now have the opportunity to share part or all of the $10 with one or both of two charities. Any money that you donate to either charity will be matched; meaning every dollar you donate will result in the charity receiving two dollars. You may donate as much or as little of the $10 to each of these charities as you wish by placing dollar bills in the corresponding envelopes. At the end of the experiment, you will keep the “Me” envelope and any dollar bills you place in that envelope.

7. Envelope 1: A nonprofit organization that works to help victims of natural disasters in the United States.

8. Envelope 2: Oxfam, a nonprofit organization that works to minimize poverty through relief and development work in Africa, committed to creating lasting solutions to global poverty, hunger and social injustice.