Ambiguous Solicitation: Ambiguous Prescription[*]

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ABSTRACT

We conduct a two-phase laboratory experiment, separated by several weeks. In the first phase, we conduct urn games intended to measure ambiguity aversion on a representative population of undergraduate students. In the second phase, we invite the students back with four different solicitation treatments, varying in the ambiguity of information regarding the task and the payout of the laboratory experiment. We find that those who return do not differ from the overall pool with respect to their ambiguity version. However, *no* solicitation treatment generates a representative sample. The ambiguous task treatment drives away the ambiguity averse disproportionally, and the detailed task treatment draws in the ambiguity averse disproportionally.

Keywords: Laboratory experimental methods; Experimental economics; Laboratory selection effects

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1 Introduction

Sample selection issues are relevant for any empirical exercise with human subjects. We study this problem directly, in the context of laboratory experiments in economics. However, the issue is at least as salient for "field experiments" (see Harrison & List 2004 for an overview). Among other issues, they still need to recruit their subjects, and thus there is the possibility for selection bias. Indeed, no sample is likely to be fully representative; Gronau (1974) is an early paper that worries about just such an effect on wage selectivity in labor markets. We discuss further in the conclusion the relevance for both field experiments and for fully naturally occurring observed data.

In order to directly assess such effects, we hypothesize in advance that a particular recruitment procedure will affect the composition of the subjects who show up to participate. We use standard laboratory protocols for comparability, but the implications are similar for either the lab or the field. In particular, we vary the amount of information (about the task to be performed and/or the expected payment) revealed at the time of recruitment. This is a dimension that varies in any case, but that is not often explicitly considered or controlled for. It also has a natural theoretical link to ambiguity-aversion: an aversion to uncertainty over states of the world about which the probabilities are unknown.[1] We hypothesize that potential subjects who are more ambiguity-averse will be less likely to choose to participate if they have less information about their possible outcomes. Although we focus on this single aspect, we stress that our concern is broader. Selection biases are likely to be present in almost all situations, along a variety of dimensions, and by definition they are unusually difficult to test for and to control for.

[1] Ellsberg (1961) provides the canonical thought experiment, suggesting that individuals, if forced to choose between two lotteries with different amounts of information available, will prefer to bet on one with a known but unfavorable probability of winning rather than on one with an unknown probability. Camerer & Weber (1992) review the early experimental evidence generally confirming this intuition. In a more recent study, Ahn et al. (2007) compare various empirical measures.

In order to test for this effect here, we begin by inducing a representative sample of undergraduates, namely almost all students in several pre-occurring groups, to voluntarily participate in the first phase of our experiment in which we measure ambiguity preferences (specific procedures are described in Section 2). This is by no means representative of the population at large, but if anything it is more homogeneous – making it more difficult for us to observe selection effects within that group. In fact, even in this case, we do find a significant selection effect when those same students are invited to participate in a follow-up experiment via a randomly varied recruitment email. In particular, none of the emails that we used successfully led to the same underlying distribution of types as existed in the base population (sample frame).

The general issue of potential bias both in subject pools and in subject behaviors has been considered by experimental psychologists for many years (Orne 1962; Rosenthal 1973). Note that there are two distinct considerations: Who volunteers to participate in an experiment to begin with? And does their behavior change relative to other settings? The latter effect is sometimes referred to as a demand characteristic, since most studies find that subjects appear to conform their behavior to that which is 'demanded' by the researcher. But of course, without good evidence as to what the baseline population looks like, it is difficult or impossible to separate these effects. Experimental economics may suffer slightly less from both effects: the first because there is always payment for participating; and the second because often we have been interested not in individual differences but rather in comparing institutions or testing theories that are supposed to apply to everyone equally. Even within economics, this potential problem was discussed quite early (e.g., Kagel et al. 1979), but it has received relatively little attention.

As it matures, however, experimental economics has become increasingly interested in behavior differences among groups, and here the selection effects are more acute. For instance, there are (or have appeared to be) robust gender differences in a variety of behaviors. One of these is risk-aversion, and in particular bidding behavior in first-price auctions. Women bid higher than men do, which is less risky, and therefore often earn less money. Chen et al. (2005) replicate this finding, but then show that, if one controls for the stage of the women's menstrual cycle, the differences disappear. There is an obvious selection story to back this up: non-menstruating woman have higher estrogen levels, leading them to be both more likely to participate in an experiment in the first place (indicative of pro-social or affiliative behavior) and to be less 'aggressive' once they do.

Of course this is not proof of a selection effect, but by definition it is difficult to test that portion of the population that tends not to participate in experiments. One indirect approach is look at sorting behavior amongst subjects who have already agreed to participate in general but who can endogenously choose what task to perform (e.g., what game to play, or indeed whether or not to play a game at all). A number of recent papers have explored this issue and found significant differences in behavior between those who were assigned to a treatment versus those who chose into it.[2] Some of these papers are also able to relate individual differences (e.g., overconfidence or risk aversion) to the choice of treatment, confirming the idea that underlying preferences affect not only people's behavior, holding the environment fixed, but also what environments people end up in. In particular, two papers have looked at endogenous entry into auctions: Reiley (2005) manipulates reserve prices in a field setting; Palfrey and Pevnitskaya (2008) studies the link between risk tolerance, auction participation, and bidding behavior. All of these clearly have implications for the interpretation of any experimental results that wish to test for differences involving exactly those underlying preferences.

Three recent papers directly address the issue of determining which subjects actually physically show up at an experiment, although of course neither of them can fully compare to the (unknowable) general population – and neither do we. The first to do this is Harrison et al. (2009), which uses a field experiment setting to look at selection effects depending on risk aversion. They find small effects overall, but a noticeable difference from the use of a guaranteed show-up fee: not surprisingly, such guarantees are relatively more attractive to those who are more risk averse. The second is Jamison et al. (2008),

[2] Some examples include Camerer & Lovallo (1999); Lazear et al. (2005); Eriksson et al. (2006); Falk and Dohman (2006) and Gaudecker et al. (2008).

which studies the effect of deception in laboratory experiments. They find that deceived women and deceived low earners are less likely to show up for a second (nominally unrelated) experiment. However, they do not focus on the specifics of the recruitment procedure itself. Finally, Malani (2008) looks at self-selection into randomized medical trials, finding a link between optimism concerning treatment efficacy (which is correlated with the treatment's effect, due to unobserved individual heterogeneity) and enrollment into the trial.

2 Experiment Design and Results

2.1 Phase 1 Design: Measuring Baseline Ambiguity Aversion

Our goal in Phase 1 was to determine the ambiguity preferences of a sample of subjects into which there would be little or no self-selection specific to our experiment. Our starting sample frame was the population from which economics experiments typically solicit: undergraduate students. We recruited subjects in two methods, both of which are particularly common in economics experiments. First, we asked students in a number of introductory economics courses to complete an ambiguity-aversion survey in the final ten minutes of a class. Second, a researcher approached every student he encountered at campus libraries and asked each student to complete the same ambiguity-aversion survey. There was almost no Phase 1 self-selection (beyond taking undergraduate economics, and frequenting the library): all 94 students in the selected economics classes completed the survey, and 109 out of 111 of the approached library students completed the survey.[3]

The researcher first asked students whether they were willing to complete a brief survey in which one in three students earns money based on her responses. All students agreeing to participate signed an informed consent form. The researcher handed each student brief instructions, read the instructions aloud, invited subjects to ask questions, and gave her a

[3] We subsequently dropped six subjects from our sample frame. Four were not on campus when we conducted Phase 2, and two learned of the study's research objectives and revealed this through informal communication with one of the researchers.

survey to complete. Subjects were not informed that they would receive a future solicitation to participate in a subsequent experiment.

The survey (Appendix A) contains ten of Ellsberg's hypothetical urn gambles and collects basic demographic data. In each of five scenarios, there is a hypothetical urn containing 100 balls whose distribution of black and red balls is clearly stated (the known urn), and a second hypothetical urn also containing 100 red and black balls in total, but whose distribution of red and black balls is clearly stated as unknown (the ambiguous urn).[4] The five known urns are offered in order: 50 red and 50 black balls; 40 red and 60 black balls; 30 red and 70 black balls, 20 red and 80 black balls; and 10 red and 90 black balls. For each scenario, we presented the subject with two gambles:

(1) If we paid you $10 for pulling a red ball on your first try, would you pick from the "known" or the "ambiguous" urn?

(2) If we paid you $10 for pulling a black ball on your first try, would you pick from the "known" or the "ambiguous" urn?

Thus, we presented ten gambles to each subject, and for each gamble asked from which urn the subject would draw. Asking each gamble twice, once for red and once for black, eliminated the chance the subject had a preference for a color or mistrusted the administrator.

2.2 Phase 1 Ambiguity Aversion Classification

Based on Phase 1 survey responses, we place all Phase 1 participants into one of three categories: more ambiguity averse (Table 1, 69%), not more ambiguity averse (27%), and unable to classify (4%). To classify subjects, we look at the first "known" urn distribution at which a subject chooses the ambiguous urn when betting on whether a red ball will be the first ball drawn.[5] We classify as "more ambiguity sensitive" those students who first choose the ambiguous urn for the red-ball bet when the known urn contains 30 red balls, and who continue to choose the ambiguous urn for the red-ball bet

[4] We inform subjects that the distribution of red and black balls in the ambiguous urn is constant for all decisions. We do not refer to urns as ambiguous or known.

[5] Recall that the first known urn contains 50 red and 50 black balls, and each subsequent known urn contains 10 fewer red balls and 10 more black balls.

when the known urn contains fewer than 30 red balls. These subjects chose the known urn for both bets when the known urn contains either 50 or 40 red.

Seven subjects switched from the ambiguous urn to the known urn even though the known urn in each round became worse. We presume these individuals were not paying attention, or were not understanding the questions well, and thus categorize them as "unable to classify" and drop them from the analysis.

We classify all other subjects as "not more ambiguity averse." Note that we do not have a category "ambiguity neutral" or "ambiguity seeking" because only nine and five subjects, respectively, would have been categorized as such. We thus categorize as "not more ambiguity averse" the combination of ambiguity seeking (5 subjects), ambiguity neutral (9 subjects) and those that switched at the first round, when the odds were 40% for the known urn (122 individuals).

In Table 2 we show, with both OLS and probit specifications, the (lack of) correlation between this classification and location of experiment, gender, year in school and major.

2.3 Phase 2 Solicitation: Observing the Decision to Participate

The real "experiment" in Phase 2 is simply the decision to respond to our email solicitation. Our goal is to determine whether ambiguity preferences affect the decision to participate in laboratory experiments, and then more specifically whether different email solicitations affect this selection decision differentially.

We randomly assigned subjects to one of four recruitment treatments (i.e., ambiguity classification is orthogonal to treatment assignment). Treatments differ only in the amount of detail provided in the invitation email. We employ a 2x2 design, with each respondent receiving either an ambiguous or detailed description of their task, and either an ambiguous or detailed description of their payout. The "standard" email sent at Williams College, and elsewhere (Davis and Holt, 1992), is closest to the ambiguous task/ambiguous pay email.

Details on each email treatment are as follows (the full text is in Appendix B):
(1) *Ambiguous task/Ambiguous pay:* "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."[6]
(2) *Ambiguous task/Detailed pay:* Here, the payout section from the ambiguous email is replaced by the following: "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."
(3) *Detailed task/Ambiguous pay:* Here each participant is also informed that they will play a game in which they will decide how much of their participation fee they want to contribute to charity, and then will play games of uncertainty, choosing between known and ambiguous urns.
(4) *Detailed task/Detailed pay:* The detailed task and detailed pay, as noted from the above.

Appendix C provides the detail on the activities in Phase 2. We do not use any of these data in this paper, since the sample size is too small for meaningful distributional analysis (to observe whether the differential selection drew in different people, which then would lead to different analytical results for the Phase 2 games themselves).

Table 3 presents the key selection results as comparison of means, and Table 4 presents them in a probit specification. First note that Table 3, Row A demonstrates orthogonality of ambiguity aversion to assignment to each email treatment, and Appendix Table 1 demonstrates the same for other known demographic variables.

We have two key hypotheses:

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[6] While this is more information than is usually provided about expected earnings, unlike the *detail pay* emails, the exact distribution of payouts is unknown.

Hypothesis #1: Those who participate in laboratory experiments do not differ with respect to ambiguity aversion from those who do not participate.

Table 3 Column 1 tests this hypothesis with mean comparison, and Table 4 Column 1 tests this hypothesis with a probit specification. We cannot reject the null hypothesis. Those who participate in Phase 2 are no more or less ambiguity averse than those who do not. Note that this is a pooled analysis, across all four treatment solicitation emails. Thus, although we cannot reject the null hypothesis, this null is under the setting of a blend of solicitation approaches. We now turn to examine heterogeneity generated by the different solicitations.

Hypothesis #2: The level of ambiguity in each solicitation does not generate differential selection on ambiguity personality characteristics with respect to who participates.

In Table 3 columns 2, 3, 5, and 6, we test this hypothesis with respect to ambiguity on both task and payout. Row A in columns 2 and 3 shows the average ambiguity aversion for those that received either the ambiguous task or the detailed task is 28.4% in both cases (the randomization was stratified on ambiguity, hence the perfect orthogonality).

However, comparing rows B and C shows that the ambiguous task generates differential selection towards those less ambiguity averse (p-value 0.079), and that the detailed task similarly generates reverse selection towards the more ambiguity averse (p-value 0.014). Similar tests for ambiguity on the payment, however, do not yield statistically significant differences (nor are they signed as predicted). We discuss this in the conclusion with conjectures as to why the ambiguous payout treatment did not generate differential selection, whereas the ambiguous task treatment did.

Table 4 shows similar results, in a probit specification. Column 2 shows that the ambiguous task generates a 6.9 percentage point higher participation rate (not statistically significant) and the ambiguous email task generates a 9.4 percentage point higher participation rate (significant at 90%). Column 3 presents the key results on heterogeneity induced by the solicitation. Here, we interact the email treatment with whether the individual is ambiguity averse or not. We find the same pattern shown in Table 3, that the ambiguous task treatment deters the ambiguity averse from participating (significant at 99%), but that the ambiguous payout treatment does not generate differential selection patterns.

3. Conclusion

We examine a new facet of selection into laboratory experiments: ambiguity aversion. We find that the method of solicitation generates potentially important heterogeneity with respect to ambiguity aversion in the sample frame that participates in experiments. Thus if ambiguity aversion could influence the choices participants make, experimentalists should note that the solicitation used could generate higher or lower participation rates, depending on how much information is given in the solicitation. Further research could shed insight on two areas. First, with a larger sample size, further analysis could be done to examine whether analytical results change depending on the solicitation method. Second, it would be useful to know whether other solicitation methods could generate a more representative population. For instance, paying more money could generate higher participation rates, or alternative wording, somewhere in between our ambiguous and detailed treatments, may yield the optimal results. In our setting, it is key to note that we found *no* treatment which drew in a representative population.

Although we find differential selection from the ambiguous *task* treatment, we do not find differential selection from the ambiguous *payout* treatment. We have two conjectures for why this may be. First, perhaps the information was simply not ambiguous enough. We stated that they would win between $10 and $20, and although this adheres to the canonical urn question fairly accurately, if read quickly it could be perceived as more informative than intended. Second, it is possible that recipients of the email did not trust the researcher (whereas in the urn questions, mistrust of the researcher should not confound the analysis by design), and thus simply assumed that this really means $10, except for very few who would win the $20.

These results are important not just for laboratory experiments; similar issues apply to field experiments. We think about two types of field experiments here, "artefactual" or "framed" field experiments, using the Harrison and List taxonomy (Harrison and List, 2004), and "natural" field experiments or for that matter any observational data collection. First, with respect to those cognizant of being "researched" (artefactual or framed), the results here are potentially just as applicable in the field as in the laboratory: *no* method of solicitation in our experiment generated a representative sample frame. Typical methods in the field could have similar issues, e.g., those who are more social, those who are more likely to think the games could lead to NGO handouts, those who are more curious, etc., are all more likely to participate, and the method of solicitation could exacerbate any of these issues.[7] Regarding "natural" field experiments or any surveying process, the issues raised here are also relevant. The fundamental idea is not new: external validity. This paper sheds insight into how the solicitation method can generate more (or, ideally, less) selection which then influences the external validity of the study.

[7] Recall Malani (2008), which finds exactly such a problem in the context of medical trials.

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Table 1

Full Distribution of Ambiguity Urn Decisions from Phase 1

	Frequency	
Chose ambiguous urn for both red and black at 40/60	5	Coded as "0" in binary variable "More Ambiguity Averse"
	2.5%	
Chose ambiguous urn for both red and black at 50/50	6	
	3.0%	
Chose ambiguous urn for either red or black at 50/50	3	
	1.5%	
Switched to ambiguous urn at 40/60	122	
	61.9%	
Switched to ambiguous urn at 30/70	46	Coded as "1" in binary variable "More Ambiguity Averse"
	23.4%	
Switched to ambiguous urn at 20/80	3	
	1.5%	
Switched to ambiguous urn at 10/90	0	
	0.0%	
Never chose ambiguous urn	5	
	2.5%	
Unable to classify (i.e., switched back and forth)	7	
	3.6%	
Total	197	

Table 2
Determinants Of Ambiguity Classification

Model:	Probit	OLS
Dependent Variable:	More Ambiguity Averse in First Experiment (1)	More Ambiguity Averse in First Experiment (2)
Female	0.038	0.039
	(0.068)	(0.068)
First experiment conducted in library	0.086	0.087
	(0.075)	(0.077)
First experiment conducted in economics class	Omitted	Omitted
Freshman	0.193	0.174
	(0.122)	(0.111)
Sophomore	0.192	0.172
	(0.125)	(0.111)
Junior	0.117	0.100
	(0.133)	(0.116)
Senior + Graduate	Omitted	Omitted
Economics major	0.093	0.089
	(0.115)	(0.109)
Psychology major	-0.073	-0.073
	(0.208)	(0.238)
R^2	0.022	0.025
Observations	190	190

Marginal effects are reported for probit specification. Robust standard errors are in parentheses. Had anything been significant statistically, then * would have indicated significance at 10 percent; ** at 5 percent; and *** at 1 percent. Information on major unavailable for freshmen and sophomores.

Table 3: Analysis of Who Participated in Second Experiment
Mean and Standard Error

		Solicitation Treatment in Second Experiment					
	Full Sample in First Experiment (1)	Ambiguous Task Solicitation (2)	Detailed Task Solicitation (3)	Chi-Square p-value (2)=(3) (4)	Ambiguous Payment Solicitation (5)	Detailed Payment Solicitation (6)	Chi-Square p-value (5)=(6) (7)
Number of observations in first experiment	190	95	95		94	96	
Number participated in second experiment	34	22	12		21	13	
Percent participated in second experiment	18%	23%	13%	0.058	22%	14%	0.114
	(0.028)	(0.044)	(0.034)		(0.043)	(0.035)	
(A) Proportion more ambiguity averse in first experiment	0.284	0.284	0.284	1.000	0.287	0.281	0.927
	(0.033)	(0.047)	(0.047)		(0.047)	(0.046)	
(B) More ambiguity averse in first experiment AND participated in second experiment	0.294	0.136	0.583		0.333	0.231	
	(0.079)	(0.075)	(0.149)		(0.105)	(0.122)	
(C) More ambiguity averse in first experiment AND did NOT participate in second experiment	0.282	0.329	0.241		0.274	0.289	
	(0.036)	(0.055)	(0.047)		(0.053)	(0.050)	
Chi-square test: p-value (B)=(C)	0.888	0.079	0.014		0.596	0.663	

Table 4
Determinants Of Whether A Subject Participated in Second Exepriment
Probit

Binary Dependent Variable:	Participated in Second Experiment		
	(1)	(2)	(3)
More ambiguity averse in first experiment	0.012	0.011	0.208
	(0.060)	(0.060)	(0.140)
Email with ambiguous task		0.069	0.164***
		(0.054)	(0.063)
Email with ambiguous payout		0.094*	0.080
		(0.054)	(0.062)
Email with ambiguous task AND recipient "more ambiguity averse" in first experiment			-0.184***
			(0.039)
Email with ambiguous payout AND recipient "more ambiguity averse" in first experiment			0.012
			(0.121)
Controls for year gender, year in school, major, and location of first experiment	yes	yes	yes
Pseduo r-squared	0.098	0.125	0.174
Number of Observations	190	190	190

Marginal effects are reported for coefficients. Robust standard errors are in parentheses. * indicates significance at 10 percent. ** at 5 percent. *** at 1 percent.

Appendix A: Phase 1 Experiment Instructions

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 **there are no tricks in this experiment**. 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.

[*We present 5 of the following questions, with M=1,2,3,4,5.*]

Decision # {M}

Urn A: 100 balls: **{50-(M-1)*10)} red, {50+(M-1)*10)} 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

Appendix B: Text of Invitation 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.

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.

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.

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.

All Four Emails:

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

Appendix C: Phase 2 Experiment Instructions

[*Italicized text in brackets details how subject instructions vary. Text in braces identifies the alternative text.*]

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 known with certainty. Finally, you will be asked a series of questions 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.

[*A subject receives one of four versions. In half, Oxfam is the known charity while Habitat for Humanity is ambiguously described, whereas in the other half, Habitat for Humanity is the known charity while Oxfam is ambiguously described. We controlled for order effects. The ambiguous description is in braces.*]

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

2. 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. {A nonprofit organization that works to alleviate poverty in Africa}

Part II:

You will be making 10 choices between two lotteries, 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. A computer generated "roll" for that decision will be made and you will be paid based on your decision.

Finally, please note that **there are no tricks in this experiment**. The roll will truly be done at random via a process overseen by the Department of Economics.

[*We present 10 of the following questions, with N=1,2,…,9,10.*]

Decision {N}	
If you choose Option A in the row shown below, you will have a {N} in 10 chance of earning \$5.50 and a {10-N} in 10 chance of earning \$4.40. Similarly, Option B offers a {N} in 10 chance of earning \$10.60 and a {10-N} in 10 chance of earning \$0.28.	
Option A	Option B
\$5.50 if the die is 1	\$10.60 if the die is 1
\$4.40 if the die is 2-10	\$0.28 if the die is 2-10
☐ 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.

[*We present 5 of the following questions, with M=1,2,3,4,5.*]

Decision # {M}

Urn A: 100 balls: **{50-(M-1)*10)} red, {50+(M-1)*10)} 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

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. Please circle your response in the table.

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