Monotonicity: An Experimental Test*

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March 15, 2012

Abstract

The Axiom of Monotonicity (AM) is a necessary condition for a number of expected utility representations, including those obtained by de Finetti (1930), von Neumann and Morgenstern (1944), Savage (1954), and Anscombe and Auman (1963). This paper reports on experiments that directly test AM by eliminating strategic uncertainty, context, and peer effects. When the decision problem is simple we do not observe violations of AM; however, when it becomes a bit more obscure, we find a significant portion of subjects violating AM.

JEL codes: D9, C7, C9.
Keywords: monotonicity, dominance, disjunction effect, sure-thing principle

*We would like to thank Jorge Tarraso and Matias Vera-Cruz for valuable assistance. We have also benefited greatly from discussions with David Ahn, Andrew Caplin, Tim Cason, Jim Cox, Rachel Croson, Martin Dufwenberg, Drew Fudenberg, Konrad Grabiszewski, Debasis Mishra, Thomas Palfrey, Ariel Rubinstein, Arunava Sen, Tomas Sjöström, Ricard Torres, John Wooders, seminar participants at the Delhi School of Economics, the Indian Statistical Institute (Delhi), ITAM, University of Arizona, the Jawaharlal Nehru University and conference participants at the 2009 ESA meetings in Tucson. We are grateful for financial support provided by the Asociación Méxicana de Cultura.

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1 Introduction

An intuitively appealing postulate in decision theory is the axiom of monotonicity (AM). It stipulates that if for each state of nature, the consequence of some act f is preferred to that of another act g, then f is preferred to g. The axioms of von Neumann and Morgenstern (1944) and Savage (1954) imply AM. The axiom is explicitly taken into consideration in the representations provided by de Finetti (1930), Anscombe and Aumann (1963), Schmeidler (1989), Gilboa and Schmeidler (1995) and others (see Gilboa, 2009). AM also has broader implications that go beyond decision theory. In strategic settings, for example, AM provides an epistemic foundation for the choice of dominant strategies. In one way or another, the axiom of monotonicity is central to the way we understand individual decision making today.

In recent years experimental methods have been instrumental in putting important axioms to rigorous testing. This is probably best exemplified by Ellsberg (1961) who constructed an intuitive scenario for choice under ambiguity in which people violate Savage's Sure-Thing Principle (postulate P2 in Savage) over acts.[1] Ellsberg's experiment motivated much of the theoretical[2] and experimental[3] work on ambiguity aversion. In this paper we study another key axiom in decision theory: monotonicity.[4] This axiom is so intuitive that there would be no reason for doubting it had it not been for a series of intriguing studies that jointly establish the empirical relevance of what is known as the "disjunction fallacy" (Shafir and Tversky 1992a,b; and Croson

[1] To be precise, subjects violated P2 or/and completeness or/and transitivity.

[2] This typically involves relaxing Savage's axioms to derive, amongst others, the Choquet Expected Utility representation in Schmeidler (1989), the Maxmin Expected Utility representation in Gilboa and Schmeidler (1989) and the representation of Klibanoff, Marinacci and Mukerji (2005). It is appropriate to add here that AM is a necessary condition for all of these approaches.

[3] A horse-race testing of various theoretical explanations for ambiguity aversion has been done by Halevy (2007) and Ahn, Choi, Gale, and Kariv (2010).

[4] This should be distinguished from Savage's postulate P3 that was recently studied by Charness, Karni and Levin (2010). They explored the robustness of what Khaneman and Tversky (1983) called a "conjunction fallacy" – an observed violation of first-order stochastic dominance.

1999). These experiments suggest that substantial number of individuals (in the order of 30-35%) make choices that are inconsistent with AM. If true, this would have at least three important implications: (i) much of the current decision theory would have limited applicability; (ii) because the violation of AM is so unintuitive, it would pose a serious challenge for theoretical modeling; (iii) our understanding of subjects' choices of dominated strategies in games (e.g., the prisoner's dilemma) would have to be reevaluated.

The purpose of our experiment is to understand, in as clean a manner as possible, what causes violations of AM. To this end, we subjected violations of AM to the toughest experimental test yet. The centerpiece of our design was the choice between two binary lotteries that were resolved simultaneously by a single draw from a bingo cage. One lottery dominated the other: the dominant lottery paid higher amounts in both states. We have two treatments where we implement two different tests of AM. In the first treatment, we provided subjects with all the details of the decision problem. The environment is made transparent, such that decisions are easy to make. The idea is to have a minimal environment to test monotonicity. Violation of AM, in this setting, would pose serious questions on the validity of the axiom. At the same time, violations here would provide a lower bound on violations that could be expected to occur in the field.

The simplicity of our first treatment is unprecedented in the existing literature. Suggested violations of AM, found by other authors, have been identified in more complicated settings involving contextual or strategic uncertainty. To inquire into the origins of this behavior, in the second treatment we take a (minimal) step toward this literature. Here we implement the test in a slightly more obscure setting. Subjects are not given information about the total number of balls in the bingo cage. Thus, the environment is more "obscure." Under the axiom of monotonicity the missing information on probabilities of states is irrelevant. The dominant lottery should still be chosen. At the same time, the obscurity implanted in the environment could induce some subjects to pursue alternative heuristics.

Our results paint a clear picture. Under the transparent conditions of our first treatment, virtually all choices were consistent with AM (around 2% violations). In the second treatment, where chances of different states cannot be objectively measured, we observe around 18% violations. This suggests that AM holds in simple decision problems involving pure risk. Its validity is not as strong in the more obscure settings involving uncertainty.

It is indeed difficult to justify the choice of a dominated lottery as an act of rational choice. Yet there is a framework wherein such choices can be rationalized. Recall that AM is defined in Savage's framework, where the decision maker is provided with a state-space. In practice the decision maker would have to formulate the relevant state-space on her own. This may be a difficult task even in simple environments, such as our second treatment where the number of balls in the bingo cage is not known. The Bolker-Jeffrey (Bolker 1966, 1967; and Jeffrey 1965) approach provides a way out. Here, the decision maker is able to rationally rank acts (or lotteries) even when the underlying relevant state space is very hard to describe. She does so by deliberating directly on the likelihood of consequences (or prizes in a lottery) and disregarding the state space altogether. It is clear that in such a scenario a dominated lottery may be chosen.[5]

Gravel, Marchant and Sen (2009), provide a representation in the Bolker-Jeffrey framework. Here, the finite consequences of an act receive equal weights. In such a scenario, the decision maker never chooses a dominated lottery. Ahn (2008) provides another representation in a similar, but conceptually different, framework. But consequences there are infinite. Agastya and Slinko (2009) provide a utility representation, where a decision maker may choose a dominated finite lottery for several periods (in fictitious play). The probability that they would do so for n periods goes to zero as n goes to infinity.

Agastya and Slinko use the approach provided by Gilboa (1995, 2009). For a given act (lottery), the decision maker simply evaluates the relative

[5]Kreps (1988) provides a convincing arguement as to why this may be so.

frequency of each consequence in an act from historical data. The weighted average is then used to rank lotteries. With relative frequencies for weights, given certain histories, a dominated lottery can rank higher than a dominant lottery. However, with sufficient experimentation, this result cannot hold forever.

The rest of the paper is organized as follows. First we introduce the axiom of monotonicity (section 2) and put it in the context of the existing literature. Next we discuss the design and related hypotheses (sections 3 & 4). Results are presented in section 5 and this is followed with a discussion and conclusion (sections 6 & 7).

2 The Axiom of Monotonicity

In the world of Savage a decision maker is aware of a set of states of nature S and a set of consequences C. An act is a function from S to C and A is the set of all such functions. The decision maker has complete and transitive preference, $\succeq$ ($\succ$), over all acts in A. The decision maker also has a preference relation over the elements of C. Let us call this relation R. Anscome and Aumann (1963), and others, derive R (P) from $\succeq$ $(\succ)$. Let us denote the so derived preference as $R^{(\succeq)}$. Let an act which gives the same consequence x in all states of nature be denoted f^x.

Definition: For any two consequences x and y in C, if $f^x \succeq (\succ) f^y$ then $xR^{(\succeq)}(P^{(\succeq)})y$.

It is easy to see that $R^{(\succeq)}$ is complete and transitive.

The axiom of monotonicity (AM): We say that preference $\succeq$ satisfies the axiom of monotonicity in B, where $B \subseteq A$, if for any pair of acts f and g in B, such that $f(s)R^{(\succeq)}g(s)$ for all s in Ω, we have $f \succeq g$.

As mentioned earlier, AM is a fundamental axiom for most of decision theory and has natural implications in game theoretic settings as well. In a game, player i's pure strategy can be thought of as an act which maps opponents' pure strategy profiles (the state space) to her own payoffs. If for all such profiles (states), the payoff from strategy s_i is strictly greater than that from any other strategy, then s_i is said to be a dominant strategy. When i knows the game, and knows that strategy choice is independent across players, she knows that she can never be better off by choosing a strategy different from s_i.

Shafir and Tversky (1992b) had subjects play a sequence of various prisoner's dilemma games with randomly chosen opponents. At times, they saw their opponents move before choosing their own actions and at times they did not. Again, more than 30% of subjects who defected after observing the opponent's choice cooperated when the opponent's choice was unobservable. Croson (1999) used across-subject design to support the disjunction effect hypothesis. In her experiment, subjects often cooperated in the simple prisoner's dilemma. But when asked to state their belief about the opponent's play before choosing their own strategy, they defected at a much higher rate.

The primary focus of our paper was testing AM in the decision theoretic setting. But our design allows us to go beyond that. It can be also viewed as a screening procedure for subjects' behavioral types. We are able to identify subjects as either behaviorally consistent or inconsistent with AM. Then, we can relate these types to play in the prisoner's dilemma (PD) game. This is a point of tangency with the existing literature that finds much of the AM violations in strategic setting. A natural conjecture is that those who have violated AM are more cooperative in PD game. One should be careful though. The strategic aspect of PD adds another level of complexity. Subjects now have to form conjectures about opponents' behavior and their other-regarding preferences (as in Rabin 1993, Bolton and Ockenfels 1999, Fehr and Schmidt 2000, Dufwenberg and Kirchsteiger 2004, Andreoni and Samuelson 2006) that are typically hard to measure or control.[6]

[6]With the exception of PD game in the last task all these considerations are absent in our experiment.

3 Experimental Design and Procedures

Our objective was to create a simple, nonstrategic environment in which violation of AM could be observed directly. Each of our treatments had four tasks. The first two tasks were intended to elicit the antecedent of AM and the last two tasks to verify the implication. In task 1 the subject was asked to choose between two options: R (Right) and L (Left). If she chose R she got 85 pesos and if she chose L she got 75 pesos. Task 2 was identical except that now R paid 35 pesos and L paid 25 pesos.

Tasks 1 and 2 boil down to asking the subject if she prefers more money to less. On the surface this may seem a bit odd. Why would anyone ask such question? Don't we all prefer more money to less? We believe that it would be wrong to presume the answer. After all, there exist some rational explanations why a subject might take less money. For example, a subject may not want to appear greedy (Arad 2011), either in front of the experimenter (Andreoni & Bernheim 2010) or herself (Benabou & Tirole 2002). It could also be that the subject thinks there is more to the experiment than meets the eye. This type of subject would behave "as if" playing a different game (e.g., Halevy & Felkampf 2005); finally, it could simply be the case that the subject simply made an error (e.g., Andreoni 1995). Typically one may wish to discard such subjects from the sample. We will present our results with and without such subjects.

The key to testing AM was task 3. A subject was asked to choose between two lotteries L and R. Lottery R was constructed to be *dominant*. It paid amounts that were chosen in tasks 1 and 2. Lottery L was *dominated* and paid amounts that were left unchosen in tasks 1 and 2, i.e., see Figure 1.

To illustrate better, consider a subject who had chosen the higher amount in both tasks 1 and 2. Then the lottery R would pay \$85 in the good state and \$35 in the bad state. Accordingly, lottery L would pay \$75 and \$25.[7]

[7] Our payoffs are very similar to those used by Shafir and Tversky (1992b) and Croson (1999) who have both found a large amount of disjunct behavior in the prisoner's dilemma game with almost identical payoffs. The only difference is that in our case the lower payoffs

Figure 1: Task 3

	Lotteries L	Lotteries R
B-ball Num. > 20	\$(forgone in task 1)	\$(chosen in task 1)
B-ball Num. ≤ 20	\$(forgone in task 2)	\$(chosen in task 2)

Note: "B-ball Num." refers the number on the ball chosen from the bingo cage.

In this version of task 3 lottery R dominates L in terms of final payout. We will refer to this as the *payout-dominant version* (PDV).

Typically, one may wish to go no further. However, AM says nothing about the amount of money chosen. It is a statement on preferences. Hence we go a step further. Suppose that the subject chose the lower amount (\$75) in task 1. Now R would pay \$75 in the high state and \$35 in the low state and L would pay \$85 and \$25 respectively. In this case we have a test of AM only if we are willing to believe that the subject's choice of \$75 over \$85 was preference-driven. We will refer to this version of task 3 as the *choice-dominant version* (CDV). But what if the subject were to have simply made a mistake? Then, the appropriate test of AM would be the PDV formulation that assumes increasing preference for money. In our experiment we run both versions of task 3.

The lotteries in task 3 were resolved with the same randomization device - a bingo cage. The balls in the bingo cage were uniquely labeled with numbers between 1-40. The higher prize of the chosen lottery was paid out whenever the number on the ball drawn from the bingo cage exceeded 20. Otherwise, the subject earned the lower prize. In the first treatment (*Transparent*) we gave the subjects all the details about the bingo cage. Subjects were told that there were exactly 40 uniquely labeled balls in the bingo cage. They were all invited come to the front of the room[8] where all the balls were lined

of L and R are 25 and 35 instead of 30 and 35.

[8] This was done row-by-row.

up on display and ordered in the ascending order so they could be easily inspected. In the second treatment (*Obscure*) the only difference was that we did not reveal how many balls were in the bingo cage. The bingo-cage contained 35 balls but subjects were not invited to come up and inspect it. Thus, they could see a bingo cage loaded with balls and placed in front of the room, but they could not count the exact number of balls inside.

The fourth task of the experiment was the prisoner's dilemma. We used the same payoffs as in task 3. For this task subjects were matched randomly with one other participant. The frame of the PD is presented in Figure 2.

Figure 2: Task 4

	Left	Right
The person who you are matched with chose: Left	75 , 75	25 , 85
The person who you are matched with chose: Right	85 , 25	35 , 35

To minimize the chance of distortions due to possible peer and experimenter effects[9] we minimized the social distance by adopting a double-blind protocol. Subjects were separated from each other by blinders that provided complete privacy. In the experiment each subject was identified by a number that was inscribed on a card randomly drawn from a hat. This number was entered by the subject on the opening screen of the software. At the

[9]There is an established literature on the importance of double-blind protocol in preventing these peer-effects in experiments (Hoffman *et al.* 1996, Eckel and Grossman 1996). We have experienced these effects ourselves; in our early pilots that were run pen-and-paper in a nonanonymous classroom setting we found as much as 50% of subjects taking the lower amount in tasks 1 and 2. This stands in stark contrast with the data we obtained in the actual experiment in which we used (i) double-blind procedure and (ii) we stated clearly in the instructions that the experiment was funded by an external grant in order to mitigate the possibility that subjects think they are taking money out of our own pocket.

end of the experiment the experimenter put all payments in the respective envelopes with the corresponding numbers written on the top of them. One of the subjects was again randomly selected to hand out the envelopes to everyone else in the room.[10,11]

To get at our questions we used a within subject design in which tasks come in sequence. We control for possible order effects by randomizing the order of tasks. To preserve the natural structure of the AM implication we only randomized the order of the tasks 1 and 2 and the order of tasks 3 and 4. Furthermore, we were worried that responding to tasks may become automatic if the same column with the higher amount(s) is always associated with the same button, e.g., Right. For this reason we also randomized the assignment of columns to buttons for each task and each subject.

The experiment was run at ITAM in the computer laboratory. The software was written in Visual Basic 6.0. In total 162 subjects participated in the experiment. The Non-transparent and Transparent treatments consisted of 3 and 6 sessions respectively with 12 - 20 students per session. The students were recruited form the 1st year introductory courses offered at ITAM, i.e., they had only minimal exposure to economics. The experiment was run in Spanish.[12,13] Our assistant, a native Spanish speaker, read out the instructions, followed by a round of privately answering subjects' individual questions. The opening screen of the software contained a page of comprehension questions that had to be answered correctly by everyone before the experiment could begin. The experiment lasted for about 45 minutes. Sub-

[10]In exchange for the envelope, she collected the card with the number that matched the envelope, and then returned the cards to the experimenter.

[11]One of the major difficulties with double blind procedure is with having subjects sign the payment receipts. At that point a name and face is clearly related to the amount (and decisions) made in the experiment. We by-passed this problem by having each subject sign a payment form with the average amount earned by a subject in the experiment. This procedure was explained to subjects verbally.

[12]The instructions and the software were initially written in English, then translated to Spanish by our assistant, and consequently translated back to English by our second assistant to ensure the accuracy of the translation.

[13]The English version of the instructions can be found in the Appendix C.

jects were paid 50 pesos as a show-up fee and half of their total point earnings in the experiment. The average payment was 155 pesos.

4 Hypotheses

In the experiment we observe choices and not preferences nor the subjects' construct of the state space. Therefore, before we state our hypotheses it is necessary to define an observational equivalent of the AM for our experiment. This we call the monotonicity principle:

Monotonicity Principle: A choice satisfies the monotonicity principle if the dominant lottery is chosen in task three.

Recall that in the Transparent treatment we carefully explained to subjects all details of the randomization device in task 3. The state space can then be represented by numbers inscribed on the balls, i.e., set of states is $S = \{1, 2, ..., 39, 40\}$. Let c_t denote the alternative that was chosen in task t and let n_t the alternative that was left unchosen. Task 1 can be viewed as a choice such that the chosen amount is preferred to the alternative, i.e., $c_1 P^{(\succeq)} n_1$. Similarly for task 2 we have, $c_2 P^{(\succeq)} n_2$. For all $s \in \{1, ...20\}$ the dominant lottery gives c_1 and the dominated lottery gives n_1. For $s \in \{21, ..., 40\}$, the dominant lottery gives c_2 and the dominated lottery gives n_2. Then, by AM,[14] the dominant lottery must be preferred to the dominated lottery.

Hypothesis 1: If AM holds in transparent and simple decision problems, then we will observe no (or a negligible number of) dominated lottery choices in the Transparent treatment.

[14]To test AM through revealed preferences, in a simple setting as ours, we need a bit stronger version of AM. Namely, we say that preference $\succeq$ satisfies the strong axiom of monotonicity in B, where $B \subseteq A$, if for any pair of acts f and g in B, such that $f(s) P^{(\succeq)} g(s)$ for all s in Ω, we have $f \succ g$.

In the Obscure treatment the randomization process is not as clear. In particular, subjects do not know the number of balls in the bingo cage. What they do know is that all balls in the bingo cage are uniquely labeled. Then, the problem can be described by a natural state space partition that corresponds to the state space in the Transparent treatment: $S = \{1, 2, ..., 39, 40\}$. However, trying to give a full description of the whole state space could be a daunting task. There could be potentially up to $2^{40} - 1$ sets of relevant states.[15] If subjects find it too difficult to wrap their minds around this problem they might resort to other (perhaps experience-based) decision rules that could violate the monotonicity principle and with it the axiom of monotonicity.

Hypothesis 2: If AM holds in the obscure decision problems, then we will observe no (or a negligible number of) dominated lottery choices in both the Transparent and the Obscure treatments.

Finally, we are also interested in how a violation of AM would affect the play in the prisoner's dilemma game. Recall from our previous discussion that both Shafir and Tversky (1992, 1993) and Croson (1999) have found a large proportion of "AM violations" (in the order of 30%) in the prisoner's dilemma. They called this the disjunction effect. There is a great leap from a simple bingo-cage-type uncertainty with 50/50 chances to strategic environment of PD game. To choose a strategy in a game the player has to form a belief about the opponent's strategy and type (in terms of the opponent's other regarding payoffs). This necessarily puts the player in a situation with reduced transparency, i.e., akin to the Obscure treatment. We speculate that the same factors that are responsible for the violation of AM in the decision theoretic setting will also contribute to the choice of the dominated strategy (cooperation) in the PD game.

[15] Possibly less, because the subject is able to see the bingo cage loaded with balls, e.g., the state in which the bingo cage is empty is ruled out.

Hypothesis 3: Subjects who violated AM will cooperate in the prisoner's dilemma at higher rate than subjects who did not violate AM.

5 Results

As the first step we check the consistency of behavior in tasks (1, 2, and 4). Table 1 shows that indeed there are no differences in tasks that were unaffected by our treatment variation.[16]

Table 1: Choice frequencies in tasks 1, 2 and 4

	Treatments	
	Obscure	Transparent
Tasks 1 and 2: took the higher amount	85%	82.4%
Task 4: chose to cooperate in PD	45%	47.1%
No. of observations	60	102

Note: Task 3 is left out intentionally and is analyzed in detail in Table 2.

In tasks 1 and 2, most of the subjects revealed preference for money and took the higher amount. We refer to these subjects as *regular*. However, even despite employing strict experimental controls that ensured full anonymity, a small group of subjects (15-18%) did choose the lower amount at least once (let us call these the *irregular* subjects). How should one interpret these choices? We have already mentioned several possibilities, e.g., greed, mistakes, rule-rationality, etc. But it needs to be stressed that our experiment was not designed to understand this particular aspect of behavior. The purpose of tasks 1 and 2 was to establish the antecedent of AM; that is, to screen the subjects for their behavioral types – those who always take the higher amount and those who do not. Of course, we have a strong economic intuition for the first category. In particular, only for these subjects we are sufficiently confident that their choices are driven by preference for money, that they are not playing a game against the experimenter, and that they

[16] Fisher's exact test shows no significant differences between frequencies for Tasks 1 and 2 (p-value = 0.687) and also for Task 4 (p-value = 0.429).

understand and follow the instructions. We will therefore base our main results only on this subsample.

In what follows we will first discuss the main results regarding violations of monotonicity; then we comment on subjects who took the lower amount in at least one of the first two tasks; and thirdly, we relate AM violations to play in the PD game.

5.1 Violation of Monotonicity

Task 3 involved a choice between dominant lottery and dominated lottery. Table 2 presents the results.

Table 2: Dominated lottery choices in Task 3

	Treatments			
	Obscure	Transparent		
		Pooled	CDV	PDV
Regular subjects	17.7%	2.4%	4.4%	0%
	(9/51)	(2/82)	(2/45)	(0/39)
Irregular subjects	66.7%	-	72.7%	42.9%
	(6/9)	-	(8/11)	(3/7)
Combined	25%	-	17.9%	4.4%
	(15/60)	-	(10/56)	(2/46)

Note: The ratios in parenthesis give the actual number of observations.

Regular Subjects

Let us focus on the first row of Table 2. This is data on the regular subjects for whom all four tasks of were exactly the same. More specifically, both versions task 3 (CDV and PDV) had the same structure: in each state ("> 20" and "≤ 20") the dominant lottery paid the higher amount ($85 or $35) and the dominated lottery paid the lower amount ($75 or $25). Hence, we pooled the data. The incidence of dominated lottery choices in the Obscure treatment was as high as 17.7% and in the Transparent treatment only 2.4%. The difference is significant at 1% level with the p-value of one-sided (two-sided) Fisher's exact test[17] being 0.006 (0.008).[18] Thus, we can reject Hypothesis 2, but cannot reject Hypothesis 1. This implies that AM holds in simple and transparent decision-theoretic setting, but violations emerge in slightly more obscure settings involving conditions of uncertainty.

Irregular Subjects

The subjects who chose the lower amount in at least one of the tasks 1 or 2 account for about 15-20% of the data. This makes them less important but nonetheless a quite interesting group. Their dominated lottery choices are shown in the second row of Table 2. We implemented two different tests of AM for these subjects. One was based on the assumption that their choices in tasks 1 and 2 were preference-driven (CDV condition). The other assumed that those choices were irrelevant, e.g., mistakes. The data show that in both conditions the rate of AM violations is quite high (more than 40%).[19] This implies that the irregular subjects have significantly higher propensity to make non-monotone choices than the rest.

5.2 Play in the Prisoner's Dilemma

We only look at the data for the regular subjects who chose the higher amount in both tasks 1 and 2. The reason is the same as before: only for this subgroup are we sufficiently confident that the choice of dominated lottery violated AM. Our hypothesis (Hypothesis 3,) posits that AM violators should be more likely to cooperate in the PD game than the non-violators.[20] But we cannot quite conclude this from the data. The proportion of cooperators

[17]The p-values in the remainder of this paper are based on this test.

[18]Differences between Non-transparent treatment and CDV and PDV conditions are individually significant. One-sided (two-sided) p-values for the Non-transparent vs. CDV condition are 0.064 (0.108) and for the Non-transparent vs. PDV condition are 0.009 (0.011).

[19]We do not have sufficiently large sample on these subjects to perform meaningful statistical tests.

[20]In addition we would have to assume that the antecedent of AM is satisfied in the PD game

among regular subjects who did not violate AM is 42.8% (18/42). Among those who did violate AM the proportion is higher, at 55.6% (5/9). The data go in the right direction but the difference is not significant (the p-value is 0.45).

Thus, we can reject the Hypothesis 3, i.e., violation of AM does not imply cooperative behavior in the prisoner's dilemma game. This suggests that there may be a disconnection between violation of AM and what is known in the literature as the "disjunction effect."

6 Discussion

Several points should be noted.

1. First we would like to highlight the role of the initial two tasks in our experiment. These tasks were trivial in nature, yet about 15-18% of subjects took the lower amount. We have already commented on possible explanations but one of them is specifically worth emphasizing. In experimental work we strongly rely on subjects fully understanding and following the instructions; just like the theoretical agent is assumed to be fully knowledgeable of all details of the decision problem. Despite our best efforts explaining the experiment and running subjects through control questions it would be perhaps a bit naive to think that each and every one of our subjects was fully confident that she perfectly understood the experimental environment. There could be a small group of subjects who in spite of having a proper understanding of the experiment still feel uneasy, perhaps thinking that they have missed something in reading through the instructions. These subjects could then strategically choose the lower amount in anticipation this paying off later on. Our initial two tasks incidentally served the purpose of filtering out those subjects from our main results. Without the first two tasks we would have no way of identifying them and would have gone on to conclude that the rate AM violators is higher by about a third.

2. Related to the previous point is the question whether the trivial nature of the initial two tasks could have single-handedly led to violations of AM (see footnote 9) – perhaps via some experimenter demand effect. This is unlikely. Notice that the first two tasks do not vary between our treatments, yet violations of AM do vary.

3. Another possible concern has to do with what could be perceived as a small proportion of AM violations in the Obscure treatment. We believe that "size" is an important issue when one is concerned about the empirical validity of an axiom. But our experiment makes a slightly different point. It suggests that AM is not violated in the environment in which the state space is simple and can be easily formulated. But when this becomes more difficult, then (what can at worst be called statistically traceable) violations of AM emerge. Does this mean that AM violations would increase if the relevant state space is even more complex? Perhaps, yes. But before we even try to answer that question, we would need to have a better understanding of what complexity is and how it could be measured. Without such understanding, the best one could do is to run experiments in various contexts and see if AM holds. One has to be cautious though. In complex situations, such as a game, the antecedent of AM may be hard to verify.

4. The violations of AM in our experiment were committed by inexperienced subjects. It is quite possible they would vanish with learning and repetition, but this exercise goes beyond the scope the current paper. We believe that systematic behavior of inexperienced subjects is just as interesting as that of experienced subjects. In particular, violations of AM in our experiment could not be attributed to mistakes and hence are behaviorally meaningful. Throughout our lives we encounter myriad of brand new situations that we have to respond to on the spot. Decisions we make have an immediate impact on others – whether direct or anticipatory. For example, Shiller (1998) observes that AM violators could be causing higher volatility in financial markets. In the same vein, asset market bubbles may be fueled

by AM violators and, more importantly, by those who pretend to be AM violators. Informed and rational traders could take advantage by mimicking such types, leading to a market bubble and then jumping the gun just before the crash. This is the main idea behind the "greater fool theory," see for example Kindelberger (2000) and Allen and Gorton (1993).

5. Our experiment suggests that if subjects understand the state space well enough, there would be no violation of AM. Indeed, in almost all expected utility representations, the decision maker is assumed to be aware of the state space. Of course, there is an issue about what this awareness means. Leaving aside such concerns, it might be interesting to see whether AM violations decrease as subjects learn more in the laboratory. Such experiments would, however, be more complex than what we present in this paper. Especially if beliefs are to be elicited through incentive compatible mechanisms, some amount of clarity may have to be sacrificed.

6. In our Transparent treatment subjects could agree on the probabilities of each ball being drawn from the bingo cage. In the Obscure treatment, where the total number of balls is not known, this is not necessarily the case. Hence one could perhaps call the choice in the Obscure treatment "the choice under ambiguity." If so, then what is the relationship between our experiment and that of Ellsberg? A major and somewhat obvious difference is that in the case of Ellsberg, subjects choose between an ambiguous and a risky option, whereas in our Obscure treatment, subjects chose between two ambiguous lotteries.

7. Although Ellsberg-type experiments (e.g., Halevy 2007, Ahn *et al.* 2011) are clearly different, one may still wonder whether our findings cannot be interpreted as being driven by ambiguity aversion. In the Ellsberg experiment, subjects violated axiom P2 of Savage.[21] P2 is also known as the Sure-Thing Principle. We explicitly define P2 in the Appendix. How is P2

[21] Or completeness or transitivity.

related to AM? We deal with this question in Appendix C. We show that, in general, P2 is neither necessary nor sufficient for AM. So our point of view is that one should not invoke P2 to interpret violations of AM.

8. Another axiom of Savage that is similar to AM is P3. Again, in the Appendix, we show that P3 is neither necessary not sufficient for AM. In a sense, P3 is a restriction which makes some set of dynamic preferences (as uncertainty lessens) compatible with static preferences. AM is a restriction which makes static preferences compatible with preferences over consequences, in a different sense.

7 Conclusion

The disjunction effect literature reports on data that is inconsistent with AM. A reason for such violations is provided by the Bolker-Jeffrey approach to decision making. Here a decision maker deliberates on the probability of consequences. Such deliberations make sense when the state space is difficult to formulate. This motivated a careful test where violations of AM were recorded over two treatments. In one treatment, the state space was made transparent. In the other, the state space was more obscure. We found that in the simple environment that allowed the decision maker to formulate the state space relatively easily, AM is not violated. When the nature of uncertainty is more obscure, we find significant violations.

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Appendix

A Relationship between axioms

Here we discuss the relationship between two axioms of Savage, respectively P2 and P3, and the Axiom of Monotonicity (see below for definitions).

Let there be a finite set of consequences C with two or more elements, a finite set of states of nature Ω. Acts, which are functions $f : \Omega \longrightarrow C$. Let the set of all acts be A. Let E denote a non-null, strict subset of Ω and E^c its complement in Ω.

Savage's P2: We say that P2 is satisfied in B, where $B \subseteq A$, if for any four acts f, g, f' and g' in B and any $E \subsetneq \Omega$ such that: (i) $f(s) = f'(s)$ and $g(s) = g'(s)$ for all s in E; (ii) $f(s) = g(s)$ and $f'(s) = g'(s)$ for all s in E^c; and (iii) $f \succeq g$, we have $f' \succeq g'$.

Ellsberg's experiments showed that a sizeable proportion of people violate the behavior stated in P2. Could it be the case that AM is violated by only those who violate P2? Note from the definition that if f, g, f' and g' are not distinct acts, then P2 is trivially satisfied. Thus, if the cardinality of B is equal to 2, as in our experiment, P2 can never be violated whereas AM can. So, in a rather narrow sense, one can say that AM is not necessarily violated by only those who violate P2. The point, however, is to understand the relationship between P2 and AM in a broader sense. That is, when we take the set B to be equal to A.

An individual who violates P2 in A, need not violate AM in A. This fact is known in the literature. For example, the motivation behind most expected utility representations under ambiguity is that P2 does not hold. Yet, all these representations assume that AM holds. So it must be that a violation of P2 does not imply a violation of AM, or equivalently, AM does not imply P2. Actually, neither is it the case that P2 implies AM. Examples

1 and 2 illustrate this fact. In Example 1, P2 is violated but AM is not. In Example 2, AM is violated but P2 is not.

Example 1: Let $\Omega = \{s_1, s_2, s_3\}$; $C = \{1, 0\}$; $A = \{p, q, r, s, t, u, v, w\}$; acts map to consequences as follows (e.g. $p(s_1) = 1$):

	s_1	s_2	s_3
p	1	1	1
q	1	1	0
r	1	0	1
s	0	1	1
t	0	0	1
u	0	1	0
v	1	0	0
w	0	0	0

Preferences over acts are as follows: $p \succ q \succ r \succ s \succ t \succ u \succ v \succ w$.

By transitivity, $p \succ w$ and hence, $1\ P^{(\succeq)}\ 0$. AM is satisfied in A, by the given preferences. The way to check it is as follows. Consider q, we have $q(s_3) = 0$, otherwise $q(s) = 1$. Note that q is strictly preferred over all other acts which result in 0 in state s_3. There are no other acts listed below q which has consequence 1 in states s_1 and s_2. Similarly for other acts.

To see that P2 is violated in A, consider $E = \{s_1, s_3\}$, $E^c = \{s_2\}$. Note that: (i) $q(s) = v(s)$ and $s(s) = t(s)$ in E; (ii) $q(s) = s(s)$ and $v(s) = t(s)$; (iii) $q \succ s$. However, $t \succ v$.

This example shows that when the cardinality of Ω is greater than two, then it is not the case that (in A) AM implies P2. We now show, by example, that neither is it the case that P2 implies AM. In the example below, AM is violated in A, but P2 is not.

Example 2: As before, let $\Omega = \{s_1, s_2, s_3\}$; $C = \{1, 0\}$; $A = \{p, q, r, s, t, u, v, w\}$; acts map to consequences as follows:

	s_1	s_2	s_3
p	1	1	0
q	1	1	1
r	1	0	1
s	0	1	1
t	0	0	1
u	0	1	0
v	1	0	0
w	0	0	0

Preferences over acts are as follows: $p \succ q \succ r \sim s \sim t \sim u \sim v \succ w$.

By transitivity, $q \succ w$. Hence, $1\ P^{(\succeq)}\ 0$. Now note that AM is violated in A as $q(s) \succeq p(s)$ for all s, but $p \succ q$. We now come to the tedious part of showing that P2 is not violated. Because of our earlier observation, we shall consider only distinct acts.

Let $E = \{s_1, s_2\}$. Note that the set $\{p, q, s, u\}$ satisfies the antecedent of P2. To see this, rename $f \equiv p$, $f' \equiv q$, $g' \equiv s$, $g \equiv u$. Check that: (i) $f(s) = f'(s)$ and $g(s) = g'(s)$ for s in E; (ii) $f(s) = g(s)$ and $f'(s) = g'(s)$ in E^c; (iii) $f \succ g$. Since $f' \succ g'$, P2 is satisfied.

Let $\{p, q, t, w\} \equiv \{f, f', g', g\}$ (i.e. p is renamed f etc.). Check that: (i) $f(s) = f'(s)$ and $g(s) = g'(s)$ for s in E; (ii) $f(s) = g(s)$ and $f'(s) = g'(s)$ in E^c; (iii) $f \succ g$. Since $f' \succ g'$, P2 is satisfied. From now, we simply state the set of acts and rename them. The "check" remains the same as above.

$\{p, q, r, v\} \equiv \{f, f', g', g\}$; $\{r, s, u, v\} \equiv \{f, g, g', f'\}$; $\{r, t, v, w\} \equiv \{f, g, f', g'\}$ (in (iii) we get $f \sim g$, while $f' \succ g'$, which does not violate P2); $\{s, t, u, w\} \equiv \{f, g, f', g'\}$ (here in (iii) of the antecedent we get $f \sim g$, while $f' \succ g'$, which does not violate P2).

Given $E = \{s_1, s_2\}$, there are no more sets of acts which satisfy P2.

Let $E = \{s_2, s_3\}$. Check: $\{p, u, q, s\} \equiv \{f, f', g, g'\}$ (here in (iii) of the antecedent we get $f \succ g$, while $f' \sim g'$); $\{p, u, r, t\} \equiv \{f, f', g, g'\}$ (here again in (iii) of the antecedent we get $f \succ g$, while $f' \sim g'$); $\{p, u, v, w\} \equiv \{f, f', g, g'\}$; $\{q, s, r, t\} \equiv \{f, f', g, g'\}$ (again in (iii) of the antecedent we get $f \succ g$, while $f' \sim g'$); $\{q, s, v, w\} \equiv \{f, f', g, g'\}$; $\{r, t, v, w\} \equiv \{f, f', g, g'\}$.

Let $E = \{s_1, s_3\}$. Check: $\{p, v, u, w\} \equiv \{f, f', g, g'\}$; $\{p, v, s, t\} \equiv \{f, f', g, g'\}$ (here in (iii) of the antecedent we get $f \succ g$, while $f' \sim g'$); $\{p, v, q, r\} \equiv \{f, f', g, g'\}$ (here in (iii) of the antecedent we get $f \succ g$, while $f' \sim g'$); $\{q, r, s, t\} \equiv \{f, f', g, g'\}$ (in (iii) of the antecedent we get $f \succ g$, while $f' \sim g'$); $\{q, r, u, w\} \equiv \{f, f', g, g'\}$; $\{s, t, u, w\} \equiv \{f, f', g, g'\}$ (here in (iii) of the antecedent we get $f \sim g$, while $f' \succ g'$).

This ends the example.

Note that the structure of both the examples is such that it is easy to extend them to cases where the cardinality of Ω is strictly greater than three.

The reader may assert that in our experiment, the relevant state space is of cardinality two. This is because, in both lotteries, all consequences are the same for balls drawn with numbers greater than 20 and similarly, all consequences are the same for balls drawn with numbers less than or equal to 20. But this is so only for the narrow sense, i.e. when comparisons are made over B (the subset of acts) which has only two elements. And we have already seen that in such cases, P2 always holds. On the other hand, in our experiment C (the set of consequences) has four elements. So in the the set A (the set of all acts) we have acts for which the set of all states cannot be meaningfully partitioned into two. For example, the act which has consequences \$25, \$35, \$75 and \$85, when the ball drawn has numbers belonging to sets $\{1, .., 10\}$, $\{11, .., 20\}$, $\{21, .., 30\}$, and $\{31, .., 40\}$ respectively. The reader may still want to know about the relationship between AM and P2 when the cardinality of Ω is two. Here, the surprising result is that AM implies P2.

Proposition 1 *Let $\Omega = \{s_1, s_2\}$ and the cardinality of C be at least four; then AM implies P2.*

Proof. Let c_1, c_2, c_3, c_4 be consequences in C. P2 is trivially satisfied unless (i) $f(s_1) = f'(s_1) = c_1$ and $g(s_1) = g'(s_1) = c_2$; (ii) $f(s_2) = g(s_2) = c_3$ and $f'(s_2) = g'(s_2) = c_4$. So suppose (i) and (ii). By completeness, either $c_1 R c_2$ or $c_2 R c_1$. Without loss of generality, let $c_1 R c_2$. Since, $f(s_1) = c_1$, $g(s_1) = c_2$ and $f(s_2) = g(s_2) = c_3$, then, by AM, we have $f \succeq g$. Now, $f'(s_1) = c_1$, $g'(s_1) = c_2$ and $f'(s_2) = g'(s_2) = c_4$. Therefore, by AM, we have $f' \succeq g'$. ■

This result says that all those who violate P2 also violate AM. So the number of people who violate P2 cannot be greater than the number who violate AM. In this sense, the number of people who violate P2 provides a lower bound for the number who violate AM.

We now state P3:

Savage's P3: Let f, g, f' and g' be four acts in B, where $B \subseteq A$, such that $f'(s) = x$ and $g'(s) = y$ for all s in Ω and $f(s) = f'(s)$ and $g(s) = g'(s)$ for all s in E. We say that P3 is satisfied in B when: $f' \succeq g'$ iff $f \succeq_E g$.

The fact that AM is neither necessary nor sufficient for P3 should be clear. Nevertheless, we provide two examples. In example 3, P3 is violated but AM is not. In example 4, AM is violated but P3 is not.

Example 3: $\Omega = \{s_1, s_2, s_3\}$; $C = \{1, 0\}$; $B = \{p, q, r\}$, where $B \subset A$.

	s_1	s_2	s_3
p	1	1	1
q	0	0	1
r	0	0	0

Preferences over acts: $p \succ q \succ r$.

A conditional preference: $q \sim_E r \succ_E p$, where $E = \{s_1, s_2\}$.

Here $p \succ r$ implies $1 \; P^{(\succeq)} \; 0$. It is easy to see that AM is satisfied by the given preference over acts. P3 is violated due to $q \succ_E p$.

P3 is violated because of "event dependent preferences." To see this, let s_1 and s_2 stand for heavy and medium showers and s_3 stand for a sunny day. Let 0 stand for an umbrella and 1 for a hat. A decision maker wants to go out for a walk the next day. Today, she has to choose between a hat and an umbrella (or a lottery which gives her a hat tomorrow irrespective of the state, and a lottery which gives her an umbrella tomorrow irrespective of the state). Suppose the decision maker believes that it is unlikely to rain tomorrow. It makes sense for her to choose a hat. Next day she finds out that it is raining, though she still does not know whether rains would be heavy or medium when she goes out for the walk. She might now prefer an umbrella over a hat. The point here is that preferences conditional on some event may be quite at odds with unconditional preferences. P3 is a way to relate conditional and unconditional preferences. AM is a way to relate unconditional preference over acts to preference over consequences (or prizes). These are quite different restrictions.

The next example violates AM but not P3.

Example 4: $\Omega = \{s_1, s_2, s_3\}$; $C = \{1, 0\}$; $B = \{p, q, r\}$, where $B \subset A$.

	s_1	s_2	s_3
p	1	1	1
q	0	0	1
r	0	0	0

Preferences over acts: $q \succ p \succ r$.

Conditional preference with $E = \{s_1, s_2\}$, $E = \{s_1\}$, $E = \{s_2\}$: $p \succ_E q \sim_E r$.

Conditional preference with $E = \{s_2, s_3\}$, $E = \{s_1, s_3\}$, $E = \{s_3\}$: $p \succ_E q \succ_E r$.

P3 is not violated here because it puts no restrictions on the unconditional preference between p and q, while AM does. The only unconditional preference on which P3 puts a restriction on is that between p and r.

B Instructions

Below is the English version of the instructions. The instructions below are those used in the Transparency treatment. In the Non transparency treatment the changes were that the text in [] was added and the text in { } was deleted.

Instructions

Welcome to the experiment. From this moment on no talking is allowed. If you have a question after we finish reading the instructions, please raise your hand and the experimenter will approach you and answer your question in privacy.

The experiment consists of 4 tasks that will be presented to you in sequence (one after another). In each task you will be asked to make a single decision.

Earnings

The amount you earn in this experiment will be paid to you in cash at the end of the experiment. The funding for this experiment was provided by an external grant from Asociation Mexicana de Cultura.

You will be paid according to the following rule:

50 pesos for coming on time to the experiment + $\frac{1}{2}$ * (the points that you earn in each of the four tasks of the experiment).

Privacy

In this experiment you are completely anonymous. The experimental procedure that will be described to you in detail insures that NO ONE including the experimenters will be able to know which decision was made by you.

Tasks and Decisions

You will be seated at the computer terminal which is shielded by blinders to insure your complete privacy. In front of you there is a folded card with a number which will identify you throughout the experiment. You will use this number to make your decisions and also to redeem your payment.

After we finish reading these instructions and answer any questions that you may have, you will be asked to follow the instructions on the computer screen. The software will guide you through the tasks of the experiment.

When we start the experiment you will see the following screen:

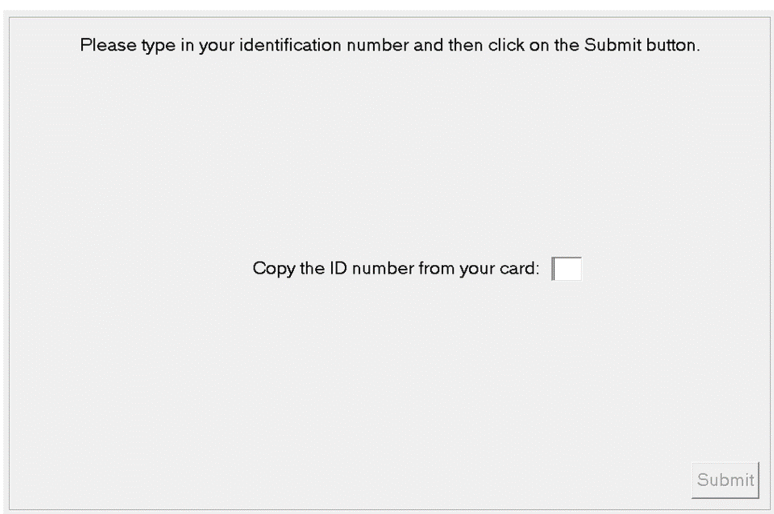

Figure 1

Please enter your identification number which is written on the card in front of you. Make sure that you copy the number correctly. If you make a mistake we will not be able to pay you your earnings.

Next you will be asked to complete a series of comprehension questions. These questions ensure that you have properly understood the instructions. You will not be allowed to proceed with the experiment unless you have answered all questions correctly. Once you have answered the instructions the Task 1 of the experiment begins.

TASK 1: The task is very simple. On the screen (Figure 2) you see two boxes each containing a single number.

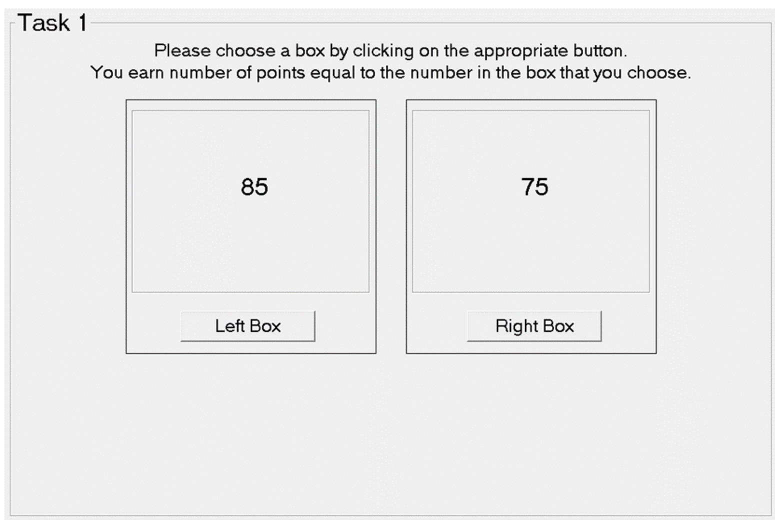

Figure 2

This screen is just an example. In the experiment the numbers in boxes may be switched.

All you have to do is to choose a box (left or right) by clicking on the appropriate button labeled either “Left” or “Right.” The number inside of the box that you choose represents the number of points that you earn in this task.

TASK 2: The instructions for task 2 are exactly the same as for task 1. The only difference between tasks 1 and 2 is the numbers in the two boxes.

TASK 3:

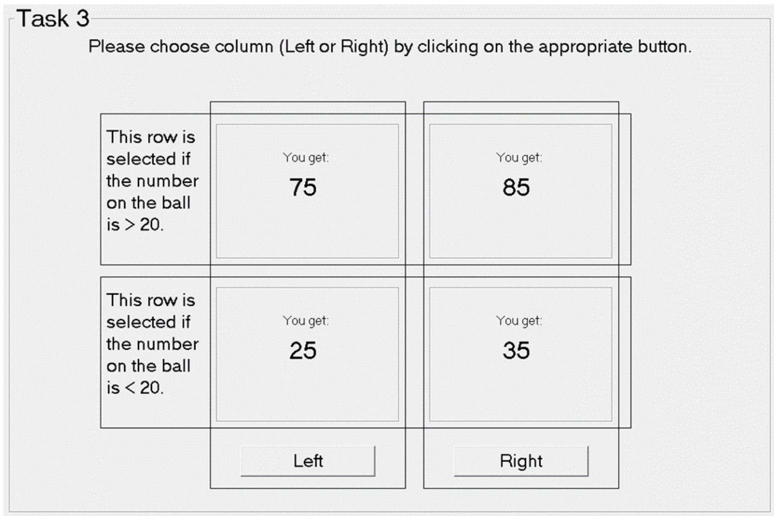

Figure 3

This screen is just an example. In the experiment the numbers in boxes may be switched.

In this task (Figure 3) you see 4 boxes. The boxes are grouped horizontally into two rows and also vertically into two columns.

You are asked to choose a column (left or right) by clicking on the appropriate button labeled either “Left” or “Right.”

The number of points you earn is equal to the number in box which is

(i) inside of the column that you have selected and also

(ii) inside of the row which will be decided randomly at the end of the experiment by a draw of a single ball from a bingo cage. This is done in the following way:

The bingo cage in front of the room contains [forty] balls that are labeled with numbers between 1 and 40. No two balls have the same number. {The number of balls in the bingo cage is decided by the experimenter.} After everyone has completed the experiment one of the participants will be randomly selected to spin the bingo cage and draw a single ball. If the number on the ball greater than 20 then the top row is chosen. If the number on the ball is less than or equal to 20 then the bottom row is chosen.

TASK 4:

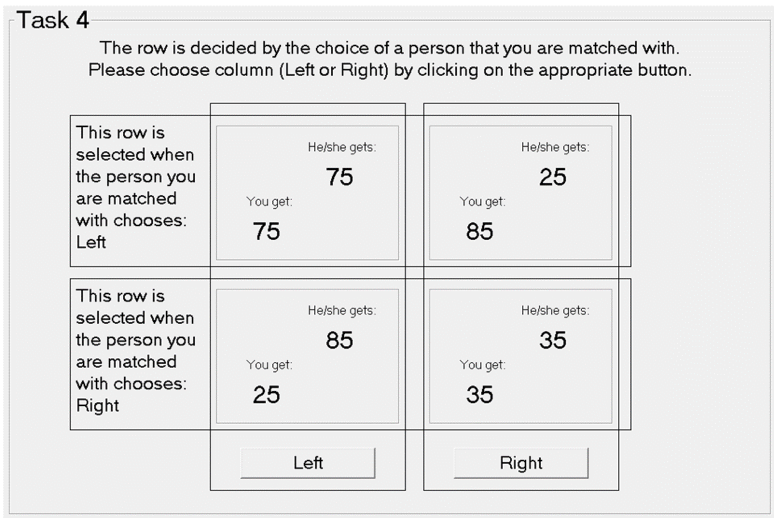

Figure 4

This task is similar to task 3. The difference is that now you are randomly matched with one other person in this room. In Figure 4 you see 4 boxes. The boxes are grouped horizontally into two rows and also vertically into two columns. Each box contains two numbers. The number labeled "You earn:" represents the number of points that you earn when that box is selected. Similarly, the number labeled "He/she earns:" represents the number of points that the person you are matched with earns when that box is selected.

Which box is selected depends on your decision as well as on the decision of the other person that you are matched with. Both you and the other person simultaneously choose a column (left or right) by clicking on the appropriate button labeled either "Left" or "Right." The box which is selected for payment lies

(i) inside of the column that you have chosen and also

(ii) it is inside of the row which depends on what the other person has done: if he/she chose left, then the top row is selected; and if he/she chose right, the bottom row is selected.

The order of tasks

In the experiment you will first complete tasks 1 and 2 but the order in which they appear is decided randomly. This means that you may encounter task 2 as first and task 1 as second. Then you complete tasks 3 and 4. Again their order is decided randomly and you may complete task 4 before you complete task 3. When everyone is finished with all four tasks you will be asked to fill out a short questionnaire. After that one randomly chosen participant will draw a ball from the bingo cage to determine which row is played in the Task 3.

Payment

When everyone is finished with the experiment the experimenter will put earnings of each student into a separate envelope and write the student's identification number on the top of the envelope. Then, one of the students in the room will be randomly selected to distribute the envelopes to everyone else in the room. To receive your earnings you will be asked to exchange the card with your identification number for the envelope which contains your earnings and has the same number written on the top of it. When you get your envelope you may leave the room.