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How Large Looms the Ghost of the Past? State Dependence versus Heterogeneity in Coordination Games

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In games with multiple, Pareto-rankable equilibria and repeated play, does a history of playing an inefficient equilibrium make it harder for players to reach the efficient equilibrium? In other words, can people "get stuck" in bad equilibria? Previous studies have found support for this, but they have relied on naturally occurring variation in precedent. I implement randomized control to establish that precedent effects are important, but that naturally occurring variation exaggerates the importance of precedent. I present evidence that some of the endogeneity of naturally occurring precedents is due to variation in risk attitudes. This is because in the coordination games used, the inefficient equilibrium is associated with a safe strategy. Understanding the causal effect of precedent is important since many development problems are viewed as coordination games. Moreover, an appreciation of the way in which potential heterogeneity may interact with the policy is essential when trying to lift groups out of bad precedents.

JEL Classification: D02, D8, O43

1. Introduction

In coordination games, can people "get stuck" in bad equilibria? Coordination games have been used as a metaphor for a wide variety of social interactions, such as the establishment of a new institution, the implementation of a new technology that has network externalities, or even the overthrow of a dictatorial regime.[1] An understanding of the effect of bad precedents is critical to policies that try to lift groups out of inefficient equilibria. The persistence of poor institutions has been blamed for the inferior economic performance of many countries and the often complementary difficulty in transiting to democracy.[2]

In this article, I demonstrate the way in which use of naturally occurring data to estimate the effect of bad precedents can lead to large biases. I do this by comparing estimates from naturally occurring data to estimates from data where I randomly induce a bad precedent. The bias arises because the inefficient equilibrium is associated with the safest strategy. People who

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[1] See Tullock (1974), Chong (1993), Hardin (1995), Lichbach (1995), Moore (1995), Weingast (1997), Ginkel and Smith (1999), Goldstone (2001), Weber (2006), and Acemoglu and Robinson (2006). Also see Devetag and Ortmann (2007) for a review of the coordination literature.

[2] Acemoglu and Robinson (2001), North (2005), and North, Wallis, and Weingast (2009).

		Median of choices	
		1	*0*
Your choice	*1*	5	1
	0	3	3

Figure 1. The *N*-player stag hunt. The number in each cell is your payoff. Payoffs are symmetric.

naturally arrive at the inefficient equilibrium are more likely to remain there not only due to a precedent effect but also due to their systematic aversion to the riskiness of the efficient equilibrium.

Variations of the stag hunt have been used to study the issue of precedent. Figure 1 is the *N*-player stag hunt (also referred to as the average opinion game). Its key features are: (i) There are two Pareto-rankable pure strategy Nash equilibria, (1, 1) and (0, 0), and (ii) the choice corresponding to the inefficient equilibrium is the safest (it has the highest guaranteed payoff).[3]

Suppose that this game is repeated and players are informed about the median at the end of every period. A precedent effect occurs when a median in period $t - 1$ causally increases the likelihood of that same median occurring in period t.

As part of their pioneering work on coordination games, Van Huyck, Battalio, and Beil (1991) used a version of the *N*-player stag hunt with more strategies (see Figure A1 in the Appendix).[4] *Every* time the game was played, they found that the median in the first period was perpetuated for the remaining periods—statistically seemingly the most acute possible precedent effect. Similar results were reported by Cachon and Camerer (1996) and Blume and Ortmann (2007). Van Huyck, Battalio, and Beil (1991) concluded that "subjects select an equilibrium that is determined by the historical accident of the initial median" (p. 898).

Precedent effects are a plausible explanation for the perpetuation of inefficient equilibria. Schelling's (1960) focal point theory, and its formalization by Bacharach and Bernasconi (1997), argues that in multiple equilibrium games, players search for salient equilibria (focal points). In repeated play, the preceding median can be quite salient, especially when it is one of the few pieces of information delivered to players after each round.

Returning to Van Huyck, Battalio, and Beil's (1991) data, was the initial median actually a historical accident? Perhaps not. Serial correlation can be explained by state dependence, such as the precedent effect, but it can also be the result of heterogeneity in unobservable explanatory variables that persists over time (Heckman 1991). Whatever generated the first-round median of 0 may also directly increase the likelihood of a subsequent median being 0. In the case of the stag hunt, heterogeneous risk attitudes could be an example of persistent unobserved heterogeneity: Risk-averse groups of players generate a median of 0 *every* round.

[3] In a 2-player version of the game, Harsanyi and Selten (1988) describe the property as risk-dominance.

[4] For the weakest link game, a different class of coordination game, see Van Huyck, Battalio, and Beil (1990).

Rather than being "stuck" in the inefficient equilibrium due to historical forces, they are continually drawn to it for ex ante reasons.

In response to potential variation in unobserved explanatory variables, I build upon the observations of Van Huyck, Battalio, and Beil (1991) by randomizing the precedent. I do this by running a variant of the *N*-player stag hunt that uses computer players as confederates. To act as a natural variation benchmark, I also run a version with only human players.

I find that there is a precedent effect, but that it is substantially overestimated by relying on natural variation. By collecting data on risk preferences, I present evidence that naturally occurring variation in risk preferences can partially explain the endogeneity of naturally occurring precedents.[5]

The wide application of coordination games suggests that it is important to fully understand precedent effects. A significant step toward dissecting the precedent effect is being able to estimate it accurately. More generally, policymakers should be aware of the possibility of an interaction effect between whatever got a group into a bad equilibrium in the first place and the outcome of an intervention.

The remainder of this article is organized as follows. Section 2 is the experimental design. Section 3 is the empirical results. Section 4 is the discussion.

2. Experimental Design

Procedure

I use the stage game in Figure 1 with a 1-point to $1 exchange rate. All features of the game, including those about to be described, are common knowledge.

There are seven players and three periods. After each period, players see what the median was in that period. After the second period, the seven players are randomly divided into two subgroups of three players with one player eliminated. The remaining six players then proceed to play the last period in their subgroup. There is no communication during the session.[6]

In the *human treatment*, all seven players are humans. Consequently, the precedent taken into periods 2 and 3 is naturally generated.

In the *computer treatment*, four of the starting seven players are computers, and the remaining three players are human. Computers play according to the following rules (recall that these are common knowledge):

(i) Within each period, all the computers choose the same action: They behave as a bloc.
(ii) The computers choose the same action in all periods: They repeat their first-period choice.
(iii) The computers' common first-period choice is determined by chance.

[5] Many others have studied the relationship between riskiness and play in coordination games, e.g., Berninghaus and Ehrhart (1998), Rankin, Van Huyck, and Battalio (2000), Clark and Sefton (2001), Schmidt et al. (2003), and Goeree and Holt (2005). However, these articles do not consider the way in which this issue relates to the endogeneity of estimating the causal effect of precedent, which is the main issue in this article.

[6] The experiment was computer based. It was programmed using z-Tree, software created by Urs Fischbacher.

Subjects are not given any information about the probabilities of the computers' choices. After the second period, the four computer players are eliminated, and the three human players play the third period together in a subgroup of size 3.

In each of the first two periods of the computer treatment, the common computer choice determines the median. Consequently, the precedent taken into periods 2 and 3 is exogenous.[7]

Human sessions have 14 human subjects, and computer sessions have 12 or 15 human subjects. All matching is anonymous, and payments are private.

After playing the stag hunt, subjects participate in a Hey-Orme risk preferences test (Hey and Orme 1994).[8] This is an individually completed task. During each period, the subject is faced with a choice between two lotteries, each over the same four outcomes ($0, $10, $20, $30). The subject chooses which she prefers (or expresses indifference). The subject does this for 20 pairs.[9] To generate incentives for truthful revelation, subjects are informed that one of the pairs will be selected at random at the end, and each subject will play out the lottery for which she declared a preference.[10]

Using maximum likelihood estimation (see Harrison and Rutstrom 2008; Andersen et al. 2010; see Wilcox [2011] for a new microeconometric model of risk attitudes), one can use the choice data to estimate the parameter K in the constant relative risk aversion (CRRA) von Neumann-Morgenstern utility function $u(m) = m^K$, where m denotes $ wealth, and K is a measure of risk-lovingness.[11]

Hypotheses to Be Tested

Let X_{it} be the choice of player i in period t, and let $\bar{X}_t$ be the median of X_{it} in period t. The causal effect of interest is the effect of an exogenously "good" versus "bad" precedent on the

[7] It may be tempting to conclude that in the computer treatment, at the start of period 3, the players know nothing about each other's choices in period 2, since the computer players solely determine the precedent. This is essentially incorrect. Assuming it is common knowledge that the players prefer more to less and that they understand the rules of the game, it follows that in period 2, all the players will make the same choice as the computer players (which actually always occurred in the data I collected), and this will be common knowledge at the start of period 3. This is why I had 2 periods of play prior to the final period; 2 periods is the least number necessary for ensuring that everyone plays according to the exogenous precedent for at least 1 period.
However, it is worth pointing out that, in principle, the fact that the existence of computer players is made common knowledge potentially limits my ability to compare the human and computer treatments in the desired manner, e.g., by affecting the players' choice heuristics.

[8] The instructions (see the Appendix) are adapted from a set provided by Glenn Harrison.

[9] Subjects do not know how many pairs they will have to ponder.

[10] This procedure determines the earnings for the risk preferences task. Subjects are only paid for one out of the stag hunt and the risk preferences task, as determined by the outcome of a coin flip.

[11] In principle, a valuable additional control would be the subjects' beliefs. I decided against collecting these data for several reasons. First, identification of beliefs while allowing for differences in risk preferences is not trivial and requires substantial structural assumptions; most of the existing elicitation methods are based on the assumption of risk neutrality, which is clearly inappropriate in this setting (see Offerman et al. [2009] for a joint identification method). Second, measurement of beliefs during play is quite disruptive and, in my opinion, changes the way in which subjects play the game substantially. I believe it raises two possibilities of experimenter demand effects that are important for my analysis: Subjects may pay more attention than is naturally warranted to their expectations, and they may make more effort to make decisions that are consistent with their beliefs to avoid appearing irrational. Third, the main goal of this article is to demonstrate the endogeneity of precedent. Identifying the source of the endogeneity is a secondary goal. Heterogeneity in risk preferences is a sensible avenue to explore given the theoretical literature on the stag hunt; however, there are many other naturally varying potential sources of endogeneity, rendering definitive inferences on the role of any individual source precarious. That is why my conclusions concerning risk preferences are tentative, and why controlling for beliefs would be of limited value.

probability of a good median:

$$\Pr(\bar{X}_t = 1|\bar{X}_{t-1} = 1) - \Pr(\bar{X}_t = 1|\bar{X}_{t-1} = 0). \tag{1}$$

Prediction 1

Using exogenous variation in precedent ($\bar{X}_{t-1}$), there is a positive causal effect of precedent.

In other words, a precedent effect occurs when a median of x in period $t - 1$ causally increases the likelihood of that same median occurring in period t:

$$\Pr(\bar{X}_t = 1|\bar{X}_{t-1} = 1) - \Pr(\bar{X}_t = 1|\bar{X}_{t-1} = 0) > 0. \tag{2}$$

Van Huyck, Battalio, and Beil (1991) and subsequent studies found the largest possible precedent effect—perfect persistence of the median: $\Pr(\bar{X}_t = x|\bar{X}_{t-1} = x) = 1$ and $\Pr(\bar{X}_t = x|\bar{X}_{t-1} \neq x) = 0$.

Playing $X_{it} = 1$ carries some risk, whereas $X_{it} = 0$ is riskless. If risk-averse players are more likely to generate a precedent $\bar{X}_{t-1} = 0$, then they are also more likely to generate a median of 0 in subsequent rounds independently of any precedent effect. This means that natural variation in the precedent $\bar{X}_{t-1}$ is positively correlated with an unobservable variable (risk-lovingness; K_i) that is also positively correlated with $\bar{X}_t$. This is classic endogeneity.

Prediction 2

Estimating the causal effect using natural variation in precedent leads to a higher estimated causal effect than using exogenous variation in precedent.

Prediction 3

The probability that player i chooses $X_{it} = 1$ is increasing in her risk-lovingness (K_i).

3. Empirical Results

All sessions were conducted at the Interdisciplinary Center for Economic Sciences at George Mason University in spring of 2009. Subjects were recruited from a database of students who had declared an interest in participating in experiments. Sessions lasted 30 minutes, and average earnings per subject were \$20. There were seven sessions (99 subjects). Unless otherwise stated, all statistical tests that follow are Mann-Whitney tests.

Result 1

Using exogenous variation in precedent, there is a positive causal effect of precedent. There were 19 groups in the computer treatment (three human + four computer players). Of the nine where I induced a good precedent, *all* nine sustained the good equilibrium in the third period. Of the 10 where I induced a bad precedent, only three sustained the bad equilibrium in

Table 1. Regression and Probit Results

Estimation Method Dependent Variable	Model 1 Regression Median in Period 3	Model 2 Regression Choice in Period 3	Model 3 Probit Choice in Period 3
Constant	0.00	0.17*	–
Standard error	(0.11)	(0.09)	–
Computer session	0.70***	0.53***	0.50***
Standard error	(0.14)	(0.11)	(0.14)
Median from last period	1.00***	0.78***	0.70***
Standard error	(0.16)	(0.12)	(0.11)
Computer session × Median from last period	−0.70***	−0.59***	−0.63***
Standard error	(0.21)	(0.16)	(0.20)
R^2/pseudo-R^2	0.67	0.37	0.30
Observations	31	93	93

"Computer session" is a dummy variable that takes the value 1 for computer sessions. In the probit (model 3), the reported figure is the estimated marginal effect. Asterisks denote statistical significance.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

the third period.[12] The point estimate of the causal effect of precedent is therefore 30%. This difference is statistically significant (p-value = 8%).

One can also test this using the 57 individual choice observations. Subjects in the good precedent were 19% more likely to play $X_{i3} = 1$ than subjects in the bad precedent (p-value = 8%).

Result 2

Estimating the causal effect using natural variation in precedent leads to higher estimated causal effect than using exogenous variation in precedent.

There were six groups of seven humans, which divided into 12 groups of three humans (recall that 1 subject in each seven is excluded from the third period). Of the six subgroups that naturally generated a good precedent, *all* six sustained the good equilibrium in the third period. Of the six subgroups that naturally generated a bad precedent, *all* six sustained the bad equilibrium in the third equilibrium. The point estimate of the causal effect of precedent is therefore 100% (p-value < 1%), replicating what Van Huyck, Battalio, and Beil (1991) and subsequent studies find in their version of the stag hunt.

To formally compare the precedent effect across the data types, I run a regression with the pooled data (model 1 in Table 1). The dependent variable is the median in period 3. The model contains a dummy for computer sessions, a dummy for the median from period 2, and an interaction variable for these dummies. The interaction term confirms that the precedent effect

[12] Due to the simplicity of the data, tables of summary statistics do not add to the presentation of the results, and I therefore relegate them to the Appendix.

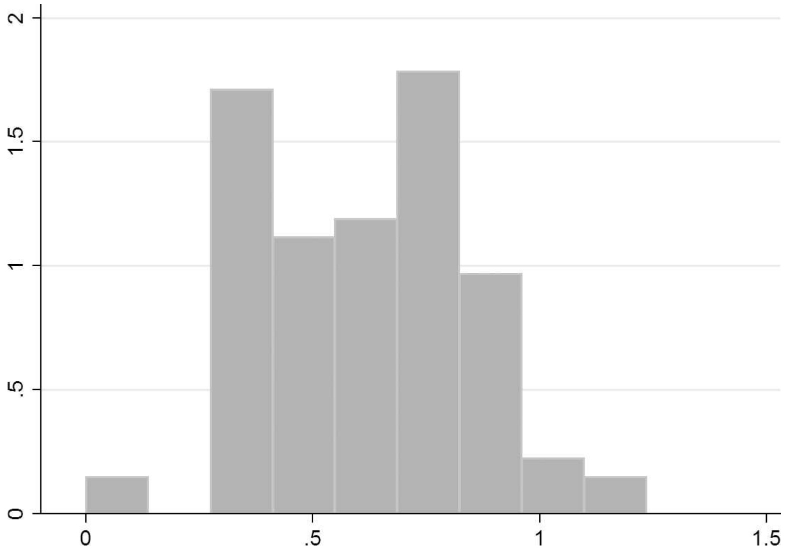

Figure 2. Histogram of estimated risk-lovingness. Estimated risk-lovingness comes from fitting data from a Hey-Orme test to the CRRA utility function $u(m) = m^K$, where m denotes \$ wealth, and K is the risk-preferences parameter.

is 70% higher in the human sessions (where the precedent is based on natural variation) and that this figure is statistically significant.[13]

Using individual data generates analogous results. Subjects in the good precedent are 78% more likely to select $X_{i3} = 1$ than subjects in the bad precedent (p-value $< 1\%$). Models 2 (regression) and 3 (probit) in Table 1 use an interaction term to compare the precedent effect in the computer sessions to that in the human sessions. In both models, the precedent effect is about 60% higher in human sessions, and this figure is statistically significant.[14]

Result 3

The probability that player i chooses $X_{i1} = 1$ is increasing in her risk-lovingness (K_i).

Recall that I use the Hey-Orme test to estimate i's parameter K_i in the CRRA utility function $u(m) = m^{K_i}$, where m denotes \$ wealth. $K_i = 1$ implies risk neutrality, while higher (lower) values denote risk-lovingness (risk-aversion). Figure 2 is a histogram of the recovered estimates of the risk-lovingness parameter.[15] The sample mean of K_i is 0.62, and the sample standard deviation is 0.23.

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[13] A probit is not possible because of the perfect correlation in the human treatment.

[14] Note that I have designed the experiment to permit a "restart" effect of jumping to the good equilibrium in both computer and human treatments. The manner in which the game "restarts" in period 3 is analogous in the two treatments.

[15] The Hey-Orme test was done after the coordination game. A t-test (p-value = 39%) and MW test (p-value = 21%) on the recovered risk-lovingness parameters confirm that session type (human vs. computer) did not affect behavior in the Hey-Orme test. Further, a t-test (p-value = 77%) and MW test (p-value =38%) on the recovered risk-lovingness parameters confirm that exogenous precedent in the computer treatment did not affect behavior in the Hey-Orme test, allaying fears of consistency bias from the two tasks occurring one after the other.

For the 21 subjects who played 0 in the first round of the human sessions ($X_{i1} = 0$), the mean risk-lovingness was 0.52, while for the 20 who played 1, the mean was 0.67.[16] The difference is statistically significant (p-value = 4%). In the computer sessions, the mean risk-lovingness for the 37 who played $X_{i1} = 0$ was 0.59, and for the 20 who played $X_{i1} = 1$, it was 0.71. The difference is also statistically significant (p-value = 4%).[17] An ancillary result is that of the 30 subjects in the computer treatment who are induced into the bad precedent, the 21 who play $X_{i3} = 1$ have a risk-lovingness parameter that is 0.14 higher than the nine who played $X_{i3} = 0$ (p-value = 10%).

4. Discussion

In line with Van Huyck, Battalio, and Beil (1991) and the remainder of the literature, I have demonstrated a precedent effect.[18] As mentioned in the introduction, Schelling's (1960) focal point theory offers a compelling explanation. At the end of each round, the median choice is reported to all subjects, and this is common knowledge; therefore, it is particularly salient.

Bacharach and Bernasconi's (1997) formalization can further flesh out the mechanism. In their theory, players look at the strategies, and they generate frames for describing them, e.g., "even-numbered strategies," or "strategies that correspond to my favorite number." These frames differ in their likelihood of being generated, known as their availability. They also differ in the extent to which they narrow down the strategy space, known as their rareness. For example, if two strangers are trying to coordinate on a European country in the absence of communication, then the frame "countries with English as the first language" is very rare, since only the UK and Ireland satisfy it. It is probably highly available, especially if it is common knowledge that the players are from the UK. In contrast, if the players are well traveled, then "countries that player 1 has been to" is neither rare nor available. Bacharach and Bernasconi (1997) argue that frames that are highly available and rare are the best targets for coordination.

In the stag hunt, the preceding median is highly available. It is also perfectly rare. In a stage game with only two strategies, rareness is essentially a nonissue. However, in the K-strategy version considered by Van Huyck, Battalio, and Beil (1991), a group focused on trying to climb out of the worst equilibrium might not know whether to play the next best equilibrium or to just jump to the best equilibrium in one go. In the face of such uncertainty, the preceding median likely becomes even more operationally focal.

I have also demonstrated the possibility of overestimating the precedent effect if identification is based on natural variation in the precedent. Risk attitudes clearly affect choices in the stag hunt. People do not purely randomly find themselves in the bad precedent—they

[16] The total observations sum to less than a multiple of 7 because I lost the risk-lovingness data for one of the subjects.

[17] Similar results are obtained using regressions and probits where X_{i1} is the dependent variable and K_i is the explanatory variable.

[18] In addition to the studies mentioned in the introduction, there are several other studies of precedent. Meyer et al. (1992) and Schotter and Sopher (2003) study games with multiple equilibria, but all are efficient, and so precedent is not as damaging as it can be in games with Pareto-rankable equilibria. In some of the other experiments reported in Van Huyck, Battalio, and Beil (1991), in addition to Knez and Camerer (1994), Devetag (2005), and, in some sense, Weber (2006), a weaker form of precedent is studied. A game where it is "easy" to coordinate is followed by a game where it is more difficult to coordinate. The strategies are labeled such that continuing the choices of the first game in the second game yields the efficient outcome. These studies report mixed results on the strength of weak precedent.

		Smallest choice	
		1	*0*
Your choice	*1*	5	1
	0	3	3

Figure 3. The *N*-player weakest-link game. The number in each cell is your payoff. Payoffs are symmetric.

often end up there because their group members are risk averse. The same distaste for risk will push them toward the inefficient equilibrium in subsequent rounds independently of any precedent effect.

The payoff structure of the stag hunt means that risk attitudes are an important determinant of choices, and in turn, they could be a source of endogeneity. However, if one modifies the payoffs, heterogeneity in social preferences, intelligence, or in many other dimensions can generate endogeneity.[19] One should note that my design permits only suggestive evidence about risk preferences as a source of endogeneity. This is because I rely on natural rather than induced variation in risk attitudes.

Coordination games have been used to study a wide variety of decisions where we observe naturally occurring data that are consistent with a strong effect of precedent. As mentioned in the introduction, these include legal and political systems. My goal was *not* to generate an externally generalizable estimate of the endogeneity bias. Rather, I wanted to demonstrate that the bias can be large: In this study, naturally occurring variation in precedent leads to an overestimation of the precedent effect by a factor >3.

In the case of policies designed to lift a group out of a bad equilibrium, the importance of this finding is twofold. First, if the intervention attempts to diminish the precedent effect, then predictions about its productivity may be biased. Second, and more importantly, policymakers need to consider the possibility of an interaction between the intervention and whatever got the group into the bad equilibrium in the first place.

As an example, consider a community trying to manage a common property resource, a problem that has stag hunt coordination aspects (Ostrom 1990). Reaching and maintaining the good equilibrium is easier for groups whose members have social preferences, since playing 1 in the stag hunt has a positive externality. Therefore, to some extent, communities that are more successful at coordinating may be communities with above average social preferences. Consequently, transplanting good coordination to the communities that demonstrate poor coordinators may prove fruitless. This would be even more likely if the stag hunt took the weakest-link form (Figure 3; Van Huyck, Battalio, and Beil 1990), which is an even riskier version.

All it takes is *one* player choosing 0 for all the players choosing 1 to earn \$1 (in the average-opinion version, a majority is required). In *K*-strategy versions, choices quickly unravel

[19] See Jones (2008) for the way in which intelligence affects decision-making in the related issue of cooperation.

toward inefficient equilibria, even when high minima are achieved in early periods (Van Huyck, Battalio, and Beil 1990).

In conclusion, state dependence is a plausible explanation of the serial correlation observed in coordination games, but just like in labor market settings, an investigation of heterogeneity is critical when considering policy interventions.

Appendix 1: Average Opinion Game in Van Huyck, Battalio, and Beil (1991)

This game can also be described as the *K* strategy version of the stag hunt (Figure A1). The number in each cell is the payoff. Payoffs are symmetric.

Playing 3 yields the highest guaranteed payoff (0.50), though it constitutes an inefficient equilibrium.

		Median choice						
		7	*6*	*5*	*4*	*3*	*2*	*1*
Your choice	*7*	1.30	1.15	0.90	0.55	0.10	-0.45	-1.10
	6	1.25	1.20	1.05	0.80	0.45	0.00	-0.55
	5	1.10	1.15	1.10	0.95	0.70	0.35	-0.10
	4	0.85	1.00	1.05	1.00	0.85	0.60	0.25
	3	0.50	0.75	0.90	0.95	0.90	0.75	0.50
	2	0.05	0.40	0.65	0.80	0.85	0.80	0.65
	1	-0.50	-0.05	0.30	0.55	0.70	0.75	0.75

Figure A1. The Average Opinion Game in Van Huyck, Battalio, and Beil (1991)

Appendix 2: Data Tables

In every session, the median in period 2 is equal to the median in period 1 ($\bar{X}_1 = \bar{X}_2$) (Tables A1 and A2).

Table A1. Group and Individual Choice Data from Computer Sessions

		Median in period 3	
		0	1
Median in period 2	0	3	7
	1	0	9

		Choice in period 3	
		0	1
Median in period 2	0	9	21
	1	3	24

Table A2. Group and Individual Choice Data from Human Sessions

		Median in period 3	
		0	1
Median in period 2	0	6	0
	1	0	6

		Choice in period 3	
		0	1
Median in period 2	0	15	3
	1	1	17

Appendix 3: Experimental Instructions

Welcome to our study in decision-making. If you pay attention and make good decisions, you may earn a considerable amount of money. At the end of the experiment, you will be paid your earnings privately and in cash.

Just for showing up, you have earned \$7. Today you will do 2 tasks. In each task, the choices that you make will determine your earnings. However, you will only be paid the earnings that correspond to one of the 2 tasks. After you have completed the tasks, I will flip a coin to determine which of the 2 tasks will be used to determine your earnings. Whatever you earn from the task will be in addition to the \$7 that you got for showing up.

For the remainder of this experiment, please refrain from any communication with other participants.

Task 1 (Human Treatment)

You will be put into groups randomly. You will never know the identity of the other members of your group. You will only interact with the members of your group. Each group will be composed of 7 players. Each round, every player makes one decision: They choose between A and B.

The amount you earn for a round depends on your choice.

- If you choose A, then you earn exactly \$3, regardless of what other players pick.
- If you choose B, then:
  - If the most popular choice is B, then you earn \$5.
  - If the most popular choice is A, then you earn \$1.

You will play this game for 3 rounds.

- For the first 2 rounds, you will be in a group of 7.
- After each round, everybody will find out what the most popular choice was in that round.
- In the third round, your group of 7 will be divided randomly into 2 groups of 3 with 1 person left over on their own.
  - The person left on their own will do nothing for the third round. They will receive a fixed payment.
  - The rest of you will play the third round in your groups of 3. Remember that these 3 people were all together in the starting group of 7.

Your total earnings will be the sum of your earnings from each round.

Task 1 (Computer Treatment)

You will be put into groups randomly. You will never know the identity of the other members of your group. You will only interact with the members of your group. Each group will be composed of 3 human players and 4 computer players. Each round, every player makes one decision: They choose between A and B.

The amount you earn for a round depends on your choice.

- If you choose A, then you earn exactly \$3, regardless of what the other players pick.
- If you choose B, then:
  - If the most popular choice is B, then you earn \$5.
  - If the most popular choice is A, then you earn \$1.

Computer players choose according to the following 3 rules:

(i) They all choose the same as each other every round. In other words, they behave as a block.
(ii) What they pick in the first round is the same as what they pick in all other rounds.
(iii) Chance determines what they pick in the first round.

Therefore, after chance determines the common choice that they all make in the first round, they will all just continue to make that choice in all later rounds. Recall that there is 1 more computer player than human players. This implies 2 things:

- In each round, whatever common choice the computer players make will determine the most popular choice.
- Knowing what the most popular choice in the first round tells you nothing about what the other human players picked in the first round.

You will play this game for 3 rounds.

- For the first 2 rounds, you will be in a group of 3 human players and 4 computer players.
- After each round, everybody will find out what the most popular choice was in that round.
- In the third round, the computer players will be kicked out, and you will play the third round with only the human players in your group.

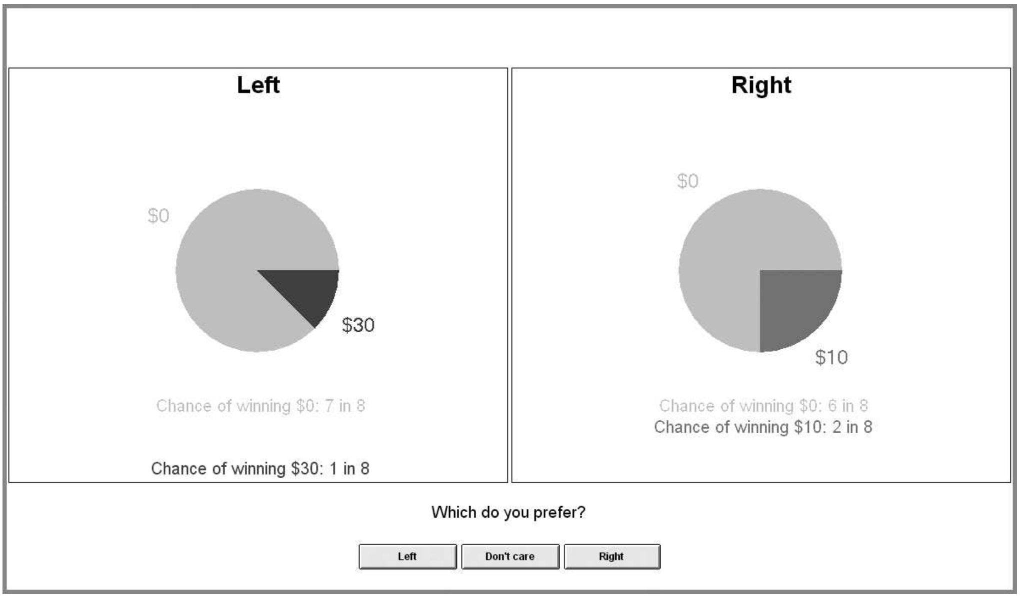
Your total earnings will be the sum of your earnings from each round.

The first task is finished. We will now do the second task. Recall that you will only receive earnings for one of the two tasks. After you complete the second task, we will flip a coin to determine which.

Task 2

In this task, you will make decisions alone, and your earnings will not depend upon the decisions of others. You will be given a series of choices between 2 games of chance. For each pair of games, you should indicate which of the 2 games you prefer to play. If task 2 is chosen for your earnings, you will actually get the chance to play one of the games of chance you choose, so you should think carefully about which games of chance you prefer.

Here is a pair of games of chance like the ones you will see on your screen, although the display on your screen will be bigger, easier to read, and in color.



The outcome of the games of chance will be determined by a random number between 1 and 8. Each number between (and including) 1 and 8 is equally likely to occur. In fact, you will be able to roll the number yourself using an 8-sided die.

In the above example, the left game pays nothing ($0) if the random number is between 1 and 7, and it pays $30 if the random number is 8. Notice that the size of the pie slices shows you the chances of each possible outcome.

In the above example, the game on the right pays nothing ($0) if the random number is between 1 and 6, and pays $10 if the random number is between 7 and 8. As with the game on the left, the pie slices represent the fraction of the possible numbers that yield each payoff.

Each pair of games is on a separate screen on the computer. On each screen, you should indicate which of the games you prefer to play by clicking on one of the three boxes beneath the games. You should click the "Left" box if you prefer the game on the left, the "Right" box if you prefer the game on the right, and the "Don't care" button if you do not prefer one or the other.

You should approach each pair of games as if it were the only pair of games you are considering, because if task 2 is chosen for your earnings, you are only going to play one of the many games. If you chose "Don't care" in the games that we play out, we will pick one for you using a coin flip.

If task 2 is chosen for your earnings, then after you have worked through all of the pairs of games, we will roll a die to determine which pair of games has been chosen to play. If you picked "Don't care" for that pair, we will flip a coin to decide which one you will play. Then, we will let you roll the die to determine the outcome of the game you chose (or that was selected for you based on the coin flip).

For instance, suppose you picked the game on the left in the above example. If your die roll was 6, you would win nothing; if it was 8, you would get $30. If you picked the game on the right and rolled a 6, you would win nothing; if it was 8, you would get $10.

Therefore, if task 2 is selected for your earnings, then your earnings are determined by three things:

(i) which pair of games of chance is chosen at random to be played out;
(ii) which game you chose for the pair selected to be played; and
(iii) the outcome of the game when you roll a die.

This is not a test of whether you can pick the best game in each pair, because none of the games is necessarily better than the others. The game you prefer is a matter of personal taste. The people next to you may have different tastes, so their responses should not matter to you. Please work silently, and make your choices by thinking carefully about each game.

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