Evidence on the Equivalence of the Strategic and Extensive Form Representation of Games

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Abstract: This paper reports an experiment testing whether strategically equivalent representations of a social situation produce equivalent behavior when actually played by human subjects. The investigation was limited to representative members of the class of generic 2×2 extensive form games of perfect information, which include widely studied games in the experimental literature, and the resulting class of 2×2 strategic form games. We find a systematic difference between subjects' choices in the strategic and extensive form representations of these games. The observed behavioral difference between game forms cannot be attributed to differences in subjects' ability to do backwards induction, differences in the salience of interpersonal preferences, or differences in optimization premiums between the two game forms. Instead, subjects in the extensive form are consistently more likely to choose a branch that allows the other player to make a meaningful choice. We hypothesize that the extensive form elicits more inclusive behavior than the strategic form.

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1. Introduction

Strategic rationality as developed in the classical game theory literature provides a potentially useful theory of similarity between various social situations. It furnishes a powerful reduction of complicated situations into a neat specification of players, strategies, and preferences.[1] In particular, the concept of a pure strategy allows one to transform the extensive form representation of a situation into the strategic form representation, which is analytically more tractable. If individuals are strategically rational, they should make identical decisions in an extensive form game and the resulting strategic form game.[2] This paper reports an experiment testing whether strategically equivalent representations of a situation produce equivalent behavior when actually played by human subjects.

While experimenters have previously found evidence against the behavioral equivalence of the strategic and extensive forms,[3] this evidence is narrow, based on a few cleverly constructed examples, and does not yield any general conclusions about why differences occur between the two representations. In our experiment, we test the equivalence between the strategic and extensive forms of representative members of the class of generic 2×2 extensive form games of perfect information and the resulting class of 2×2 strategic form games. These include a number of games that have been widely studied in the literature such as Selten's (1978) chain store paradox and Krep's (1990) trust game. For each of these games, subjects are asked to make a choice for each role in both the extensive and strategic form representation of the games. The experiment is designed to give the pure strategy hypothesis its best possible chance -- the strategic and extensive form treatments only differ in how the games are presented.

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[1] See von Neumann and Morgenstern (1947) for the seminal derivation and Kuhn (1953) for the reformulation used today.

[2] The classical statement from Von Neumann and Morgenstern, *Theory of Games and Economic Behavior*, 1947, p. 79 is the following: "Imagine now that each player k = 1, ..., n, instead of making each decision as the necessity for it arises, makes up his mind in advance for all possible contingencies; i.e. that the player k begins to play with a complete plan: a plan which specifies what choices he will make in every possible situation, for every possible actual information which he may possess at that moment in conformity with the pattern of information which the rules of the game provide for him for that case. We call such a plan a strategy.

Observe that if we require each player to start the game with a complete plan of this kind, i.e. with a strategy, we by no means restrict his freedom of action. In particular, we do not thereby force him to make decisions on the basis of less information than there would be available for him in each practical instance in an actual play. This is because the strategy is supposed to specify every particular decision only as a function of just that amount of actual information which would be available for this purpose in an actual play. The only extra burden our assumption puts on the player is the intellectual one to be prepared with a rule of behavior for all eventualities -- although he is to go through one play only. But this is an innocuous assumption within the confines of a mathematical analysis."

A modern statement can be found in Kohlberg and Mertens (1986, p. 1011), "No reasonable definition of rationality could imply a different behavior for the strategist when he has to give instructions to his agents in advance of the play, as compared to the situation where he would have to carry out those instructions himself."

[3] See Schotter, Weigelt, and Wilson (1994) and Deck (2000).

Subjects only make a single choice in each role and each form, and receive no feedback between decisions. We therefore have a clean within subject test of the pure strategy hypothesis across a broad class of games.

A number of reasons exist why, ex ante, we might expect to see differences in play between the strategic and extensive form representations of a game. First, play might differ between the two game forms due to increased salience for deductive concepts such as backwards induction in the extensive form representation. This is the source of potential variation stressed by Schotter, Wilson, and Weigelt in their examination of differences between the extensive and normal forms. Another possibility is that subjects put greater weight on other-regarding preferences in the extensive form. This would seem particularly true for models that incorporate intentionality, as the extensive form representation makes it clear that a second players' decision is contingent on the first player's choice. A final possibility grows out of economists' increasing awareness that models of noisy decision-making substantially improve our ability to predict subjects' choices over models without mistakes.[4] A key assumption in developing a general model of mistakes is that the probability a player fails to give a best response is inversely related to the optimization premium. If changing the presentation of the game changes the optimization premium, differences in behavior between the two presentations should result.

We find systematic differences between subjects' choices in the strategic and extensive forms of these games. These differences cannot be explained by any of the possibilities raised above. Instead, subjects in the Player 1 role are more likely to choose a branch that allows the other player to make a choice that influences payoffs in the extensive form than in the strategic form.. We hypothesize that the extensive form elicits inclusive behavior. In particular, suppose subjects have a preference for allowing both players to have some influence over the final distribution of payoffs. If this preference is more salient when the game is presented in its extensive form, it would produce the effects seen in our data. Such a phenomenon would be quite important to economists; as such, we suggest directions for future research to investigate this new hypothesis.

2. Experimental Design

We begin this section by discussing the selection of games to be studied. We then outline the procedures used in running the experiments.

Selection of Games: To focus our analysis, we examine the class of generic 2×2 extensive form games of perfect information and the resulting class of 2×2 strategic form games (see Figure 1). The class contains

[4]See McKelvey and Palfrey (1995 and 1998) and Battalio, Samuelson, and Van Huyck (2001).

36 members,[5] including Selten's (1978) chain store game and Krep's (1990) trust game. It is the simplest class of extensive form games that maintains strategic interdependence between the players.

[Figure 1 here]

Of these 36 members, half are isomorphic transformations of other members of the class obtained by relabeling actions. We choose to maintain the assumption that behavior will be invariant to a relabeling of actions and eliminate the isomorphic transformations, leaving 18 games to be considered.

In this class of games, the second mover always has a weakly dominant strategy. The first mover has a strictly dominant strategy to pick *t* in six games and to pick *b* in six games. In this sense, these games don't involve any meaningful strategic interdependence under the assumed preferences. We select one of each type to include in the experimental design. The remaining six games all require the first mover to forecast what the second mover will do in order to determine a best response. We investigate all six making a total of eight games studied below.

Figure 2 summarizes the eight games. The subgame perfect equilibrium for each game is found by following the doubled line from the root of the tree to a terminal node. Game 1 has not been widely studied. Games 2 and 5 each have an equilibrium in dominant strategies. Game 3 is Krep's (1990) trust the kindness of strangers game. Game 4 is a team game. Like Game 3, Game 5 gives Player 2 an opportunity to reciprocate the kindness of strangers. Game 6 is Selten's (1978) chain store game. Game 7 is a strictly competitive game that has not been widely studied to our knowledge. Game 8 is Beard and Beil's (1994) trust the rationality of strangers game. Games 5,6, and 8 have two Nash equilibria, one of which is not subgame perfect.

[Figure 2 here]

Von Neumann and Morgenstern's (1947) concept of a pure strategy allows one to transform the extensive form representation of a social situation into the strategic form. A *pure strategy* assigns an action to every information set controlled by the player. For the class of extensive form games considered here the pure strategy abstraction assumption results in 2×2 bimatrix strategic form games. (See the right panel of Figure 1.)

Experimental Procedures: Human subjects made a series of choices using pen and paper. Each subject made decisions for both roles in both the strategic and extensive form representation of all eight games. The subjects made their choices in the row and column role by checking a box and in the first and second mover roles by drawing a line. All of the choices in a single role were presented on one decision page.

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[5]There are six possible orderings over the three payoffs for each of the players. Avoiding interpersonal comparisons of payoffs, this yields 36 possible pairs of orderings over the two players payoffs.

The page contained eight games plus a ninth game unrelated to the experimental design.[6] Each decision page was passed out, filed out, and collected before the next decision page was passed out. First the subjects played the Player 1 (row) role in the strategic form, then the Player 2 (column) role in the strategic form, then the Player 1 (first mover) role in the extensive form, and finally, the Player 2 (second mover) role in the extensive form.[7] The paper instrument always used green ink to show the subject's own payoffs and blue ink to show the other participant's payoffs. The subjects received no feed back until the end of the experiment.

The experiments are designed to give strategic rationality its best chance. The information available to subjects and the timing of decisions were in fact identical between the strategic and extensive form representations. The only difference between the two treatments was in how the games were presented to the subjects.[8]

Two instruments were used. In the first, the games were always presented in identical order[9] and only the subgame resulting from the first mover chosing *t* was shown on the decision page for the second mover. If subjects did not recall the earlier decision pages and did not recognize the pattern in the order of the games, then they did not know what payoffs Player 1 had foregone in allowing Player 2 to make a choice.[10] Since other regarding behavior and intentionality have become an important part of the explanation for anomalous behavior in simple games, we designed a second instrument. The second instrument, scrambled the games making it more difficult to notice the relationship between the roles and game forms and showed the whole extensive form on the decision page for the second mover.

The subjects were explicitly told they were playing against the choices made by the other participants in the session. Their earnings were determined using a mean matching protocol, that is, they received the average payoff determined by the empirical distribution of the other participant's choices. The mean matching protocol was explained to the subjects through examples. Subjects were paid in cash for all decisions they made. Their average payoff from these 32 choices was $12.75. This does not

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[6]Along with the eight strategic form games we were studying, we also included a bargaining game studied in Battalio and Van Huyck (2000) using a computer interface. This game was included to verify the consistency of our results with earlier experiments.

[7]This order was held constant in all sessions. Given that subjects received no feedback between filling out the various sheets, we feel it is unlikely that order effects could lead to the systematic differences between the strategic and extensive forms observed in the data.

[8]Schotter *et al* also use a design in which subjects receive no information about their opponent's choice until after both choices have been made. Deck's design lets Player 2s in the extensive form games see the move of the Player 1s before making a decision.

[9] Except that games five and eight were flipped on the extensive form pages.

[10] The instructions for the Player 2 forms tell the subjects "your choice will be matched with each of the other first mover choices." This implicitly tells the subjects that the same games are being played on the Player 1 and Player 2 forms. But since the Player 1 forms have already been collected, only subjects with an exceptionally good memory would know what the payoffs were if the Player 1 picked the branch leading to a terminal node.

include their earnings from any other games played in the session. Subjects were also given their choice of $4 cash or a parking pass worth $4.

Subjects made other decisions not reported in this paper. These decision pages contained variations on the dictator game with differing prices (see Andreoni and Miller, 2001). In some sessions, the 2×2 games were presented first and in some sessions the dictator games were presented first. No feedback was given until the end of the session when earnings were computed. There is no theoretical reason to believe that participating in the dictator experiment would affect play in our experiment, and indeed no statistically significant effect is found in the data.

A total of 187 subjects participated in the experiment. Three sessions with a total of 99 subjects used the first instrument in which the second mover decision page only showed the subgames resulting from a choice of *t* and the games were not scrambled, and three sessions with a total of 88 subjects used the second instrument in which the second mover decision page showed the entire extensive form and the games were scrambled.[11]

Subjects were recruited from the undergraduate population at Texas A&M University using email solicitations. Prior to the start of the experiment, a set of instructions was read out loud to all subjects. These instructions explained how to fill out the instrument and how to calculate earnings. A copy of the instructions and decision instrument one are attached to this paper as Appendix A.

3. Analytical Framework

Prior to running any experiments, we had a number of hypotheses about why differences might (or might not exist between the two treatments. The strongest of these, harkening back to von Neumann and Morgenstern's discussion of strategic equivalence of the strategic and extensive forms, is the pure strategy hypothesis.

Pure Strategy Hypothesis: Behavior in the strategic and extensive forms will be statistically indistinguishable.

If people are not strategically rational, then there are many ways in which the presentation of a social situation can influence behavior. Schotter, Weigelt, and Wilson (1994) explore the behavioral hypothesis that the presentation of a strategic situation can facilitate the use of alternative solution concepts. For example, backwards induction may be more salient in the extensive form representation,

[11] Three sessions with 75 subjects participated in the dictator games experiment prior to participating in our experiment and three sessions with 112 subjects played our experiment first.

where the timing is laid out explicitly.[12]

Backwards Induction Hypothesis: Subjects are more likely to make choices consistent with subgame perfection in the extensive form representation of the social situation.

Another possible source of difference in behavior between the two representations reflects other-regarding preferences. In recent years, experimental economists have found extensive evidence that subjects preferences over outcomes in games depend not only on their own monetary outcomes, but also on the outcomes of others and on the path of play that lead to the outcome. Prominent recent examples of this work include Bolton and Ockenfels (2000), Fehr and Schmidt (1999), and Charness and Rabin (2000). Emotions as varied as altruism, envy, reciprocity, and spite fall under the umbrella of these models. At this point in time, it would be difficult to say that there exists either a clearly defined set of empirical regularities that are in need of explaining or a definitive model of other-regarding behavior. As such, it is impossible to identify all the possible ways in which other-regarding preferences might manifest themselves. To narrow down the possible effects, we focus on a strong empirical regularity that has been observed in a wide variety of experiments, reciprocity: reward those who have treated you well and punish those who have treated you poorly. We examine the data for evidence of more (or less) reciprocity in the extensive form representation.

For Player 2s, it is clear what it means for play to be more (or less) consistent with reciprocity in the extensive form representation. In Games 1 and 6, a choice of t unambiguously harms Player 2s. If reciprocity is more (less) salient for Player 2s in the extensive form representation, we would expect to see them choosing the action that harms Player 1 more (less) frequently – R in either Game 1 or Game 6. In Games 2, 3, 5, and 8, a choice of t unambiguously helps Player 2s. If reciprocity is more (less) salient for Player 2s in the extensive form representation, we would expect to see them choosing the action that helps Player 1 more (less) frequently -- R in Games 2 and 5 and L in Games 3 and 8.[13] It is worth noting that movements towards more reciprocal behavior are often not consistent with movement towards subgame pefection (see Games 2, 3, 5, and 6). As such, there is some hope for sorting out between the backwards induction hypothesis and the reciprocity hypothesis.

Reciprocity Hypothesis: Player 2s are more likely to act reciprocally in the extensive form presentation.

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[12]Equivalently, one can think about iterated removal of weakly dominated strategies.

[13]The use of a mean matching protocol should not reduce the saliency of reciprocity for Player 2s. By the nature of the game, Player 2s know their action only affects Player 1s that have chosen t. While the ability of any one Player 2 to reward (or punish) any <u>one</u> Player 1 is small, they are able to affect <u>all</u> Player 1s who have treated them well (or poorly). Thus, the overall impact of a Player 2's choice is substantial.

This implies they should choose *R* more frequently in Games 1, 2, 5, and 6, and should choose *L* more frequently in Games 3 and 8.

Probabilistic choice models beginning with Luce (1959) can explain behavior inconsistent with backwards induction. A particularly interesting assumption to make regarding noisy choices is that better responses are more likely than inferior responses, but all responses have positive probability. If choices are sensitive to the optimization premium and optimization premiums are sensitive to how the game is presented, then behavior will violate the pure strategy hypothesis.

It is easiest to see how the pure strategy hypothesis can be violated when the "errors" players make are sensitive to the optimization premium by first focusing on Player 2: either the column player or second mover. Let $p|f$ denote Player 2's assessment of the probability Player 1 will play t subject to playing game form f, where f is either e for extensive or s for strategic. Let $E\pi_2(a, p|f)$ denote the expected payoff from action a, either L or R, against assessment p in game form f. Let $r_2(p|f)$ denote the optimization premium for Player 2 given assessment p in game form f. Then for the class of games represented in figure 1 the optimization premium for Player 2s is given by equation 1.

$$r_2(p|f) = E\pi_2(L, p|f) - E\pi_2(R, p|f) =$$
$$((p|f)\ \varphi + (1-(p|f))\ \delta) - (\ (p|f)\ \psi + (1-(p|f))\ \delta) = (p|f)\ (\varphi - \psi) \qquad \textbf{(eq. 1)}$$

To the extent that subjects' assessments of p is affected by the presentation of the game, $(p|s)$ ... $(p|e)$, we should expect see differences in Player 2s' choices between the two representations due to differences in the optimization premium.

Calculating the optimization premium for Player 1s is only somewhat more complicated. Let $q|f$ denote Player 1's assessment of the probability Player 2 will play L subject to playing game form f, where f is either e for extensive or s for strategic. Let $E\pi_1(a, q|f)$ denote the expected payoff from action a, either t or b, against assessment q in game form f. Let $r_1(q|f)$ denote the optimization premium for Player 1 given assessment q in game form f. Then for the class of games represented in figure 1 the optimization premium for Player 1s is given by equation 2.

$$r_1(q|f) = E\pi_1(t, q|f) - E\pi_1(b, q|f) =$$
$$((q|f)\ \beta + (1-(q|f))\ \gamma) - \alpha = (q|f)\ (\beta - \gamma) + (\gamma - \alpha) \qquad \textbf{(eq. 2)}$$

As above, if subjects' assessments of q is affected by the presentation of the game, $(q|s)$ ... $(q|e)$, we should expect see differences in Player 2s' choices between the two representations.

We have no way of directly observing players' assessments of p and q. However, we can observe the empirical values of the variables and the empirical optimization premiums that result. To the extent that subjects' assessments of p and q correspond to the actually empirical frequencies, changes in behavior between the two representations should be correlated with changes in the empirical optimization premiums.

Optimization Premium Hypothesis: Changes in behavior between the strategic and normal form representations are driven by changes in the (empirical) optimization premium.

The preceding discussion begs the question of why the optimization premium ought to be different between the two representations. To some extent, changes in the empirical optimization premiums give us a mechanism to capture indirect effects from the backward induction and reciprocity hypotheses. For example, suppose Player 2s are more responsive to issues of reciprocity in the extensive form representation. This changes the empirical optimization premium for Player 1s. To the extent that this change is anticipated, the behavior of Player 1s should also differ between the two representations. More generally, the error term in models like quantile response equilibrium (McKelvey and Palfrey, 1995 and 1998) is meant to capture the unmodeled parts of subjects' preferences. To the extent that the error terms are unmodeled, there is no particular reason why they can't differ between representations.

Subjects' perceived optimization premiums may change even if the empirical optimization premiums do not. For example, it is straight forward to show that the optimization premium for Player 2s in the extensive form is greater than the optimization premium for Player 2s in the strategic form *if the extensive form game is played sequentially*.[14] To extent that subjects perceive the extensive form representation as being more like the sequential game, changes in Player 2s' behavior should result due to changes in the perceived optimization premium. While such changes are a theoretical possibility, they rely on subjects' unobservable beliefs and, for the case described above, cannot easily be distinguished from the backward induction hypothesis. As such, we prefer to focus on the empirical optimization premiums.

4. Experimental Results

Table 1 summarizes the primary empirical results of the paper. The top half of the table shows data for the Player 1 role, and the bottom half shows data for the Player 2 role. Strategic rationality makes a sharp prediction for both roles in all games in both forms: either everyone plays one action or

[14]Intuitively, Player 2s know for certain if their decision is payoff relevant. This is equivalent to setting $p = 1$.

the other. The actual frequency of *t* ranged from 0.278 in the strategic form of game 5 to 0.909 in the strategic form of game 2. The actual frequency of *L* ranged from 0.187 in the strategic form of game 7 to 0.920 in both forms of game 4. The largest deviations from predicted behavior occurs in the Player 1 role of the extensive form representation of game 3, Krep's (1990) trust the kindness of strangers game, and in the Player 1 role of the strategic form representation of game 6, Selten's (1978) chain store game. In both of these cases, the majority of subjects did not choose the predicted behavior.

[Table 1 here]

There is a great deal of heterogeneity in the population. For each of the 32 choices in the experiment, we can calculate the percentage of subjects choosing the less frequent of the two possible choices. The minimum for this variable is 8%, and the median is 28%. This is extraordinary given that the column players always have a weakly dominant strategy and the row players have a strictly dominant strategy for Games 2 and 5. Even if we only consider the choices with a (weakly) dominant strategy, the median percentage of subjects choosing the dominated strategy is 19%.

Even at an individual level substantial heterogeneity is observed. The column labeled "fraction switching" in Table 1 reports the fraction of subjects who changed their choice in *either* direction between the strategic and extensive form representation of the games. (Recall that each subject played both roles of both forms of all eight games.) The choices of individual subjects often vary between the strategic and extensive form representations of a game. Looking at the 16 choices that are made in both the strategic and extensive form games, the minimum percentage of subjects switching their choice between the two forms is 10%, and the median percentage is 35%.

Just because subjects frequently switch their choices between representations, it need not follow that there is a systematic pattern to these switches. Looking for systematic differences, the column labeled "difference between forms" in Table 1 reports the difference between the actual play of *t* or *L* in the strategic and extensive forms. The next three columns of Table 1 report the results of tests for statistically significant differences between the two representations. We use McNemar's test for significance of changes, a form of chi-squared test that accounts for the paired nature of the observations (Siegel and Castellan, 1988, p. 75 – 80). The null hypothesis is that the proportion of subjects switching from top to bottom (or left to right for column players) between the two representations is the same as the proportion switching in the opposite direction. In other words, we are testing whether the difference between the representations is significantly different from zero. We report rejections of the null at the 10%, 5%, and 1% levels of significance. Of the 16 possible differences between the two representations, 7 are at least significant at the 10% level of significance. Naively treating these 16 tests as statistically independent, the likelihood of getting 7 or more false rejections out of 16 at the 10% level is 6.1×10^{-5}.

Conclusion 1: There are significant differences in play between the two representations. We reject the pure strategy hypothesis.

The economic significance of these differences is modest. The maximum difference observed is only 15% (see the Player 1 role in game 5). The differences between the two representations are more systematic for Player 1s than for Player 2s. The changes are positive in seven of the eight games for the Player 1 role, but are only positive in four of eight games for the Player 2 role. The Player 1 role changes are not only more consistently positive but also larger in absolute value. Looking at the tests of statistical significance for the Player 1s, there are significant changes in play for five of the eight games. A number of these are quite strong, with the differences for Games 4 and 5 easily significant at the 1% level and the difference for Game 7 barely missing the 1% level. In contrast, there are only significant changes in two of the eight games for the Player 2 role. Neither of the differences is even close to reaching the 1% level of significance.

Conclusion 2: There are much greater differences in play between the two representations for the Player 1 role than for the Player 2 role.

The penultimate columns of Table 1 report whether the difference between representations for the game is in the direction predicted by the backwards induction or reciprocity hypotheses. If so, this is indicated by an X. For the reciprocity hypothesis, no prediction is made for most cases. This is indicated by a mark of "---."

Only 4 of 16 differences are in the direction predicted by the backwards induction hypothesis. The extensive form does not seem much better at inducing strategic rationality than the strategic form. For three of the eight games, the play of Player 1 role is more consistent with subgame perfection in the extensive form. Looking at only the games in which the change between game forms is significant at the 10% level for Player 1, four of five have play that is less consistent with subgame perfection in the extensive form.

For the six games in which the reciprocity hypothesis makes predictions for Player 2s, three show movement towards more reciprocal behavior and three show movement towards less reciprocal behavior. Of the two games in which there are statistically significant differences between the two representations, one shows movement towards more reciprocal behavior and one shows movement away from reciprocal behavior. As such, reciprocity seems to have little to do with differences between the game forms.[15]

Conclusion 3: There is little evidence that changes in row players' behavior between the two representations is organized either by subgame perfection or reciprocity. We reject the backward induction and reciprocity hypotheses.

Instead of the backward induction or reciprocity hypotheses, we discover an unexpected shift in favor of *t* by subjects in the Player 1 role of the extensive form. For seven of the eight games, the Player 1s choose *t* more frequently in the extensive form representation. Moreover, for all five games where there are significant differences between the two representations, the row players choose *t* more frequently in the extensive form.

Conclusion 4: Subjects in the Player 1 role show a strong movement towards *t* in the extensive form representation of the games. In other words, they are more likely to make a choice that leads to a decision node for Player 2 rather than a terminal node.

Before interpreting the differences in play between the two game forms, we turn to a more detailed statistical analysis. Table 2 presents regression analysis of the experimental data. The inclusion of this analysis has three purposes. First, it allows us to aggregate the data across games. This allows us to determine if the results described previously are statistical blips that are confined to a few games or are more general in nature. Second, the regression results let us see whether any of the variations in treatments we used are responsible for the results. Finally and most importantly, the regression analysis allows us to determine whether or not the optimization premium hypothesis can explain the differences between the strategic and extensive representations.

[Table 2 here]

Given the binary nature of subjects' choices, all of the regressions reported in Table 2 use a logit specification. We include fixed effects to reduce the impact of any individual effects (Chamberlain, 1980). The regressions also include dummies for seven of the eight games (with no dummy for Game 1). While highly significant statistically, these dummies are of little direct interest and hence are suppressed in Table 2. The regressions included all observations from all subjects. Data from Player 1 roles and Player 2 roles are analyzed separately. Thus, each of the regressions includes sixteen observations, two per game, for each of the 187 subjects in the experiment. Standard errors are reported in parentheses

[15]Another type of other-regarding preference that we might have examined is some form of inequality aversion. Given that there are few observable differences for Player 2s between the two representations, it is highly unlikely that any such theory would organize our data.

below the parameter estimates. Tests of statistical significance for parameter estimates are two-tailed z-tests.

For the Player 1 regressions, the dependent variable equals 1 if Player 1 chose *t* and 0 if he chose *b.* Thus, positive coefficients indicate that Player 1 is more likely to chose *t* all else equal. For Player 2 regressions, the dependent variable equals 1 if Player 2 chose *L* and 0 if he chose *R.* Positive coefficients indicate that Player 2 is more likely to chose *L* all else equal.[16]

The independent variables are as follows:

1) Empirical Optimization Premium: This variable is calculated as the difference in expected payoffs between *t* and *b* for Player 1s and the difference in expected payoffs between *L* and *R* for Player 2s. The expected payoffs are based on the empirical frequencies. Payoff differences are calculated separately for each representation. For example, the expected payoff from choosing *t* in Game 1 in the strategic form is equal to .316*60 + .684*20 = 32.64. The payoff difference for Game 1 in the strategic form is then the expected payoff from *t* minus the expected payoff from *b* which equals 32.64 - 40 = -7.36. If subjects' choices are sensitive to the empirical optimization premium, the coefficient estimate for this variable should be positive.

2) Extensive form: This variable is coded is a 0 for observations from the strategic form representation and 1 for observations from the extensive form representation. A positive coefficient for the dummy means that Player 1s are more likely to choose *t* in the extensive form or Player 2s are more likely to choose *L* in the extensive form.

3) Interaction between extensive form and subgame perfection: We first create a dummy that is coded as one if the subgame perfect choice is *t* for a row player or *L* for a column player and is coded as zero otherwise. This variable is then interacted with the variable for the extensive form. A positive coefficient for this variable means that row players are more likely to choose *t* in the extensive form when it is the subgame perfect action and column players are more likely to choose *L* in the extensive form if it is the subgame perfect action. Suppose any differences between the representations were due to greater (lesser) consistency with subgame perfection in the extensive form. We would expect the coefficient for the interaction between extensive form and subgame perfection to be positive (negative) and we expect this coefficient to equal negative two times the coefficient for the extensive form dummy.

[16] One implication of this choice of dependent variables is that the fixed effects are capturing a subject's predilection towards using one of the strategies (e.g. *t* vs. *b*). It may instead be true that subjects have a greater or lesser predilection towards using the subgame perfect action. We have run regressions using this sort of specification, and get qualitatively identical results. We have also looked at models in which there is correlation between an individual's switches between representations. These also yield identical qualitative results.

4) Gender: The gender variable is coded as a one for male and a zero for female. This is interacted with the extensive form dummy. This variable allows us to determine if changes between the representations are tied to a subject's gender.

5) Treatments: We create dummy variables for whether a subject saw the Andreoni-Miller instrument prior to playing in our experiment, and whether they played in the treatment with scrambling and full trees as Player 2s in the extensive form representation. These variables are interacted with the dummy for the extensive form. These variables allow us to determine if changes between the representations are linked to the treatment being played.

The top half of Table 2 shows regressions on row player data. Model 1 only includes the empirical optimization premium between *t* and *b*. The coefficient estimate is large and statistically significant at the 1% level. Given that the game variables control for any other differences between the games, this suggests that subjects do respond to changes in the empirical optimization premium.

Model 2 only includes an extensive form dummy. The parameter estimate on this variable is positive and significant at the 1% level. Thus, we find general support for our conclusion that subjects are more likely to choose *t* in the extensive form.

Model 3 includes both the empirical optimization premium between *t* and *b* and the extensive form dummy. The coefficient for the extensive form is little changed in magnitude and remains significant at the 1% level. The coefficient for empirical optimization premium is roughly halved and only achieves statistical significance at the 10% level.

Conclusion 5: The results of Model 3 indicate that while subjects respond weakly to changes in the empirical optimization premium, the impact of the extensive form on Player 1s' choices cannot be attributed to changes in the empirical optimization premium. We can therefore reject the optimization premium hypothesis.[17]

Model 4 modifies Model 3 by adding the interaction between the extensive form and subgame

[17]Recall that the regressions include dummies for the games. As such, the parameter for the empirical optimization premium is identified off differences between representations, not between games.

perfection. Subjects are more likely to switch to top in the extensive form when this is the subgame perfect action, but the parameter estimate for this variable is not statistically significant at any standard level. Moreover, the linear restriction that the coefficient for extensive form interacted with subgame perfection equals negative two times the coefficient for the extensive form variable is rejected at the 1 percent level. Therefore, subgame perfection does not help explain the changes in Player 1s' choices between the two representations. This provides added support for Conclusion 3. Inclusion of the interaction between the extensive form and subgame perfection does somewhat reduce the estimate for the extensive form coefficient, and weakens its statistical significance (largely due to a sharp increase in the standard error).

The final regression, Model 5, modifies Model 3 by adding the interaction between the extensive form and gender and the interactions between the extensive form and the treatment variables. There is no evidence that our results are being driven by the differing treatments used. The interaction with gender is weakly significant; the impact of switching to the extensive form is less for men than for women. Women are almost twice as likely to change their behavior in the extensive form to allow the other player to have a meaningful choice.

The bottom half of Table 2 shows regressions on Player 2 data. These specifications are analogous to those run for Player 1s. In none of these regression is there any significant difference between the two representations. Thus, the small number of differences found for the individual games do not appear to reflect a general pattern. Once again, the regressions indicate that subjects weakly respond to the empirical optimization premium. None of the interaction terms are statistically significant.[18]

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[18]The regressions in Table 2 do not test whether the use of full game trees and scrambling affects the behavior of Player 2s in general. We therefore ran chi-squared tests separately for each of the eight games to see to see if the use of full game trees and scrambling affected the choices of Player 2s in the extensive form games. The null hypothesis being tested is that the likelihood of choosing *L* is equal between the treatment with partial trees and the treatment with full trees and scrambling. For none of the eight games could we reject the null at even the 10% level. Changing how the game trees were presented does not appear to have any impact on Player 2 decisions.

5. Conclusions

We find significant differences in behavior between the extensive and strategic form representation of social situations. These differences are not organized by changes in the saliency of backwards induction or reciprocity. Although subjects do consistently respond to changes in the empirical optimization premium, this does not appear to drive the difference between representations.

Instead, the observed difference appears in the Player 1 role of the extensive form representation of the games. Our subjects tend to choose the path that allows the second mover to influence the outcome of the game more frequently than they do in the strategic representation of the situation. We hypothesize that this effect results from a norm in favor of allowing other people to influence the outcome of social situations. We hypothesize that subjects have a preference for inclusion – all individuals affected by the outcome should be involved in selecting it – and that the extensive form representation makes this inclusion preference more salient.

It is worth noting that the existing results about differences between representations are consistent with the inclusion preference we identify in our data. Both Schotter *et al* and Deck study games that fall within the class of 2 x 2 games we study. While they give their results quite different interpretations, both of them actually find that Player 1 is more like to give Player 2 a meaningful choice in the extensive form. Thus, our results unify the results found by earlier experimenters. By looking at a broader class of games, we can for the first time reach some general conclusions about why differences occur between the strategic and extensive form representations.

Further experimental work is needed to determine the causes underlying the systematic differences we observe between the strategic and extensive form representations. If our hypothesis that subjects have preferences for procedural fairness turn out to be correct, this gives us insight into a wide variety of puzzling phenomena. For example, this bias in favor of including others may be responsible for some of the results reported in the literature on the trust game. In the trust game including the other may be misinterpreted as “trust.” Also, it is common place occurrence that organizations facing a major decision will devote a large amount of time considering how to go about making the decision and assuring that all stakeholders within the organization have input on the decision. Often these activities seem unnecessary, having little impact on the decisions actually reached. However, if individuals indeed have preferences over how decisions get made, then these apparently wasteful activities are actually rational.

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Table 1: Results Summary

Player 1 Role

Game	Predicted Play of *T*	Actual Play of *t*		Difference Between Forms	Statistical Significance			Movement Towards . . .		Fraction Switching
		Strategic	Extensive		10%	5%	1%	SGPE	More Reciprocity	
1	0.000	0.316	0.412	0.096	X				---	0.385
2	1.000	0.909	0.866	-0.043					---	0.182
3	0.000	0.583	0.674	0.091	X				---	0.401
4	1.000	0.578	0.722	0.144	X	X	X	X	---	0.401
5	0.000	0.278	0.428	0.150	X	X	X		---	0.396
6	1.000	0.380	0.439	0.059				X	---	0.455
7	0.000	0.374	0.492	0.118	X	X			---	0.406
8	1.000	0.733	0.791	0.059				X	---	0.262

Player 2 Role

Game	Predicted Play of *L*	Actual play of *L*		Difference Between Forms	Statistical Significance			Movement Towards . . .		Fraction Switching
		Strategic	Extensive		10%	5%	1%	SGPE	More Reciprocity	
1	0.000	0.283	0.294	0.011						0.342
2	1.000	0.738	0.818	0.080	X			X		0.316
3	0.000	0.401	0.460	0.059					X	0.380
4	1.000	0.920	0.920	0.000					---	0.096
5	1.000	0.674	0.572	-0.102	X	X			X	0.348
6	1.000	0.893	0.872	-0.021					X	0.150
7	0.000	0.187	0.193	0.005					---	0.273
8	1.000	0.861	0.840	-0.021						0.171

Notes: All tests of statistical significance are from McNemar's test for significance of changes (Siegel and Castellan, p. 75-80). Movement indicates the direction of change in the extensive form using the strategic form as a base. The data set includes all observations from all 187 subjects.

Table 2
Fixed Effects Logits

Row Players
Dependent Variable: Choice of Top = 1, Bottom = 0

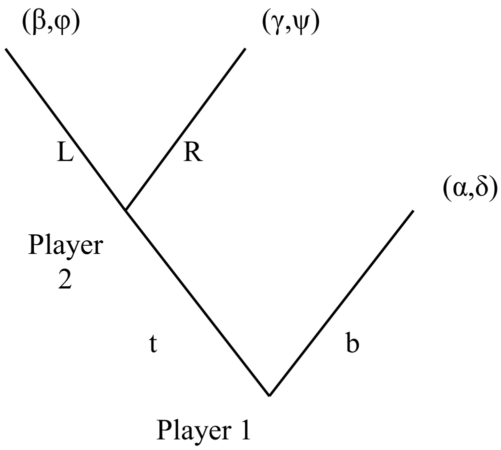
Variable Name	Model 1	Model 2	Model 3	Model 4	Model 5
Empirical Optimization Premium	0.214** (0.064)		0.126+ (0.068)	0.153 (0.107)	0.125+ (0.068)
Extensive Form		0.437** (0.084)	0.388** (0.088)	0.340+ (0.174)	0.598** (0.186)
Extensive * Subgame Perfect				0.086 (0.267)	
Extensive * Gender 0 = Female, 1 = Male					-0.296+ (0.174)
Extensive * Andreoni-Miller First					-0.189 (0.173)
Extensive * Scrambling					0.086 (0.170)
Log-Likelihood	-1325.02	-1316.89	-1315.15	-1315.09	-1312.96

Column Players
Dependent Variable: Choice of Left = 1, Right = 0

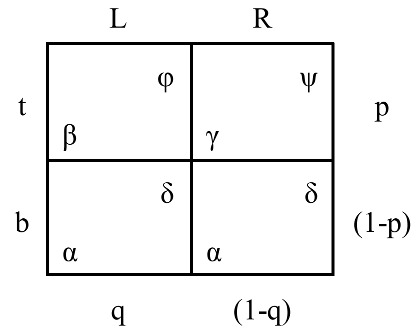
Variable Name	Model 1	Model 2	Model 3	Model 4	Model 5
Empirical Optimization Premium	0.058+ (0.033)		0.059+ (0.034)	0.070 (0.054)	0.059+ (0.034)
Extensive Form		0.008 (0.091)	-0.010 (0.092)	-0.057 (0.192)	-0.123 (0.201)
Extensive * Subgame Perfect				0.081 (0.292)	
Extensive * Gender 0 = Female, 1 = Male					0.214 (0.188)
Extensive * Andreoni-Miller First					-0.015 (0.188)
Extensive * Scrambling					-0.010 (0.186)
Log-Likelihood	-1122.68	-1124.21	-1122.67	-1122.63	-1121.98

** signficant at 1% level * significant at 5% level + significant at 10% level

FIGURE 1

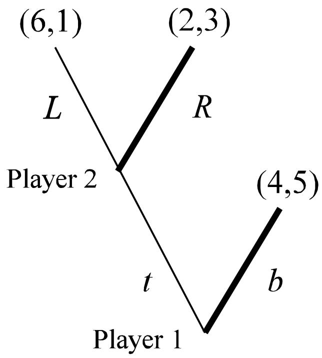


Extensive Form

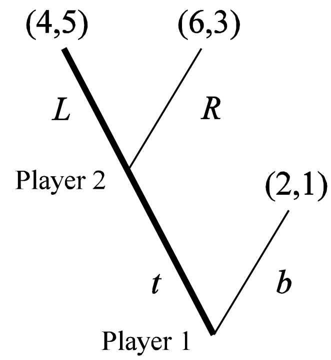


Strategic Form

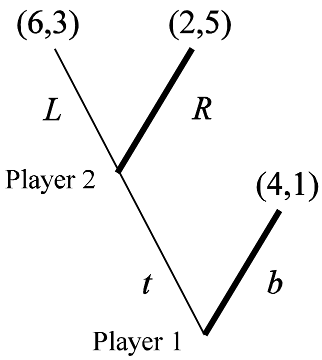
FIGURE 2



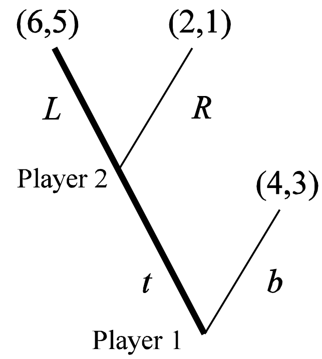
Game 1



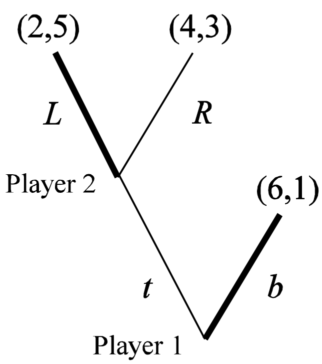
Game 2



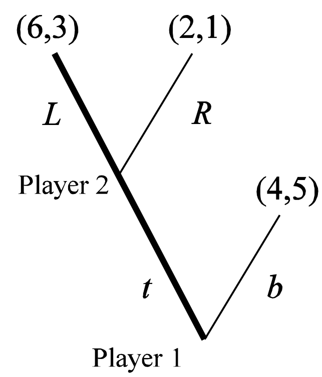
Game 3



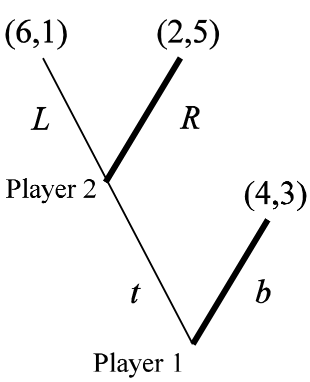
Game 4



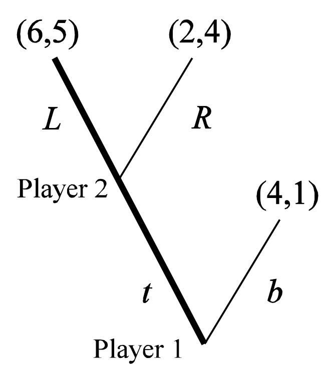
Game 5



Game 6



Game 7



Game 8

Appendix A: Instrument One

Welcome

This is an experiment about decision making. If you have any questions during this experiment, please raise your hand and an experimenter will come to you. You will be paid for participating, and the amount of money you will earn depends on the decisions that you and the other participants make. Research grants have provided the funds for this experiment. At the end of the experiment you will be paid privately and in cash for your decisions.

Session ID Number

At the top of this page is a number on an index card. This is your participant number. Each participant has a different number. You will want to verify that the number on this card is the same as the number on the top of each decision form. You will use this number to collect your cash payment. Please remove your index card now and place it face up on the desk.

Choices

You will be asked to make nine choices about your choice of t or b in an earnings table. You make choices on the row form by checking a box for one of two rows labeled "t" and "b" respectively.

The choices that you make and the choices that all of the other participants in this room make will determine your earnings. The next page provides an example of how your earnings are calculated.

Earnings

Your payoffs will always be indicated by green. Units are cents. You will be paid for all of your choices. Your earnings for each choice is going to be determined based on the choices of everyone else in the room. Your row choice will be matched against all of the column choices and you will receive the average of these earnings. For example, suppose there are 101 people including yourself and suppose that of the 100 other people in the room 75 choose column L and 25 choose column R for the example earnings table.

	L	R	
t	30 / 40	10 / 60	:
b	50 / 80	70 / 20	~

Example Choice of "t"

	L	R	
t	30 / 40	10 / 60	~
b	50 / 80	70 / 20	:

Example Choice of "b"

Earnings for example choice of "t": $\frac{75}{100}40 \% \frac{25}{100}60 ' \frac{120}{4} \% \frac{60}{4} ' \frac{180}{4} ' 45 \text{ cents}$

Earnings for example choice of "b": $\frac{75}{100}80 \% \frac{25}{100}20 ' \frac{240}{4} \% \frac{20}{4} ' \frac{260}{4} ' 65 \text{ cents}$

Now suppose 25 choose column L and 75 choose column R:

Earnings for example choice of "t": $\frac{25}{100}40 \% \frac{75}{100}60 ' \frac{40}{4} \% \frac{180}{4} ' \frac{220}{4} ' 55 \text{ cents}$

Earnings for example choice of "b": $\frac{25}{100}80 \% \frac{75}{100}20 ' \frac{80}{4} \% \frac{60}{4} ' \frac{140}{4} ' 35 \text{ cents}$

Please fill out the row choice form, taking the time you need to be accurate. When all participants are done and have turned their Instruction sheet and their row choice form face down on their desk we will collect the forms.

Row Form Participant #_____

Check the box at the end of a row to indicate your choice. Your payoffs are in green. The blue payoffs are for column participants. Units are cents. Your choice will be matched with each of the other column participant choices (but not your own). Your earnings for each choice will be determined by summing up your payoff for each match and dividing by the number of matches. Please make nine choices now.

	L	R	
t	10 60	30 20	~
b	50 40	50 40	~

Choice 1

	L	R	
t	50 40	30 60	~
b	10 20	10 20	~

Choice 2

	L	R	
t	30 60	50 20	~
b	10 40	10 40	~

Choice 3

	L	R	
t	50 60	10 20	~
b	30 40	30 40	~

Choice 4

	L	R	
t	50 20	30 40	~
b	10 60	10 60	~

Choice 5

	L	R	
t	30 60	10 20	~
b	50 40	50 40	~

Choice 6

	L	R	
t	10 60	50 20	~
b	30 40	30 40	~

Choice 7

	L	R	
t	26 46	10 9	~
b	19 11	35 31	~

Choice 8

	L	R	
t	50 60	40 20	~
b	10 40	10 40	~

Choice 9

Column Form Participant #_____

Check the box at the bottom of a column to indicate your choice. Your payoffs are in green. The blue payoffs are for row participants. Units are cents. Your choice will be matched with each of the other row participant choices (but not your own). Your earnings for each choice will be determined by summing up your payoff for each match and dividing by the number of matches. Please make nine choices now.

	L	R
t	10 60	30 20
b	50 40	50 40
	~	~

Choice 1

	L	R
t	50 40	30 60
b	10 20	10 20
	~	~

Choice 2

	L	R
t	30 60	50 20
b	10 40	10 40
	~	~

Choice 3

	L	R
t	50 60	10 20
b	30 40	30 40
	~	~

Choice 4

	L	R
t	50 20	30 40
b	10 60	10 60
	~	~

Choice 5

	L	R
t	30 60	10 20
b	50 40	50 40
	~	~

Choice 6

	L	R
t	10 60	50 20
b	30 40	30 40
	~	~

Choice 7

	L	R
t	26 46	10 9
b	19 11	35 31
	~	~

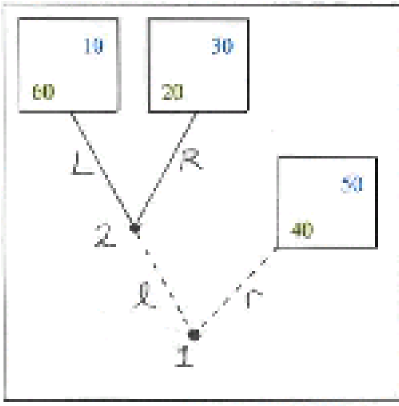
Choice 8

	L	R
t	50 60	40 20
b	10 40	10 40
	~	~

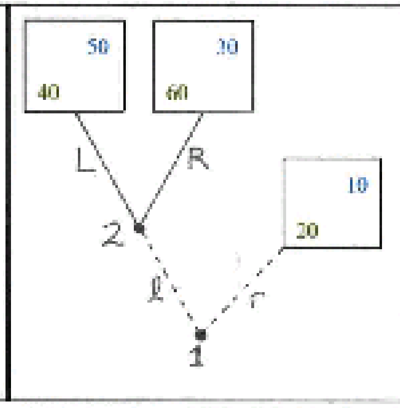
Choice 9

First Mover Form Participant #_____

Draw a line along one of the two dashed lines to indicate your choice of the left or right branch. Your payoffs are in green. The blue payoffs are for the second mover. Units are cents. Your choice will be matched with each of the other second mover participant choices (but not your own). Your earnings for each choice will be determined by summing up your payoff for each match and dividing by the number of matches. Please make nine choices now.



Choice 1



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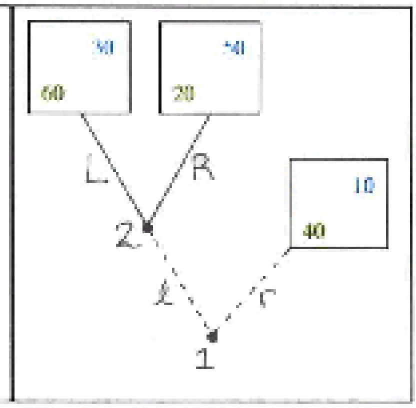
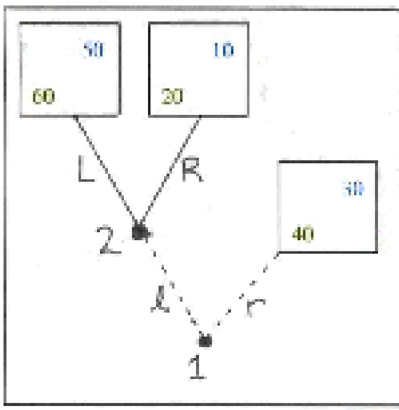
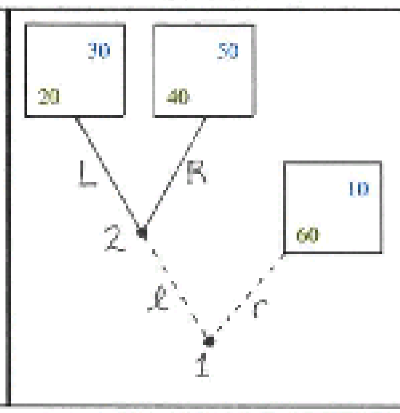


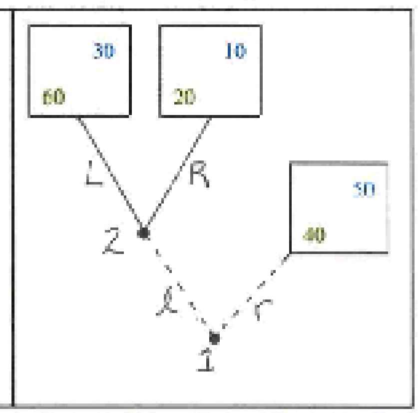
Choice 3



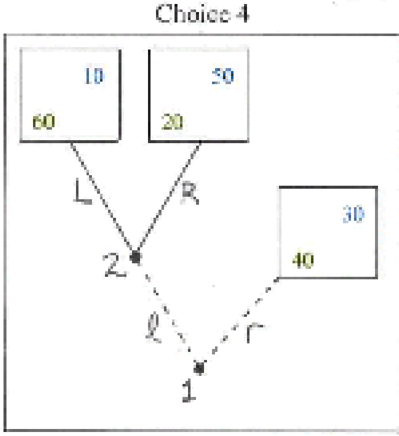
Choice 4



Choice 5



Choice 6



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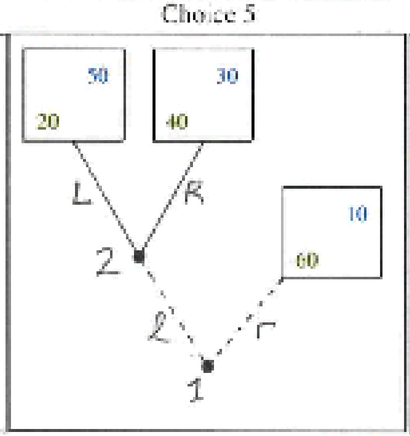
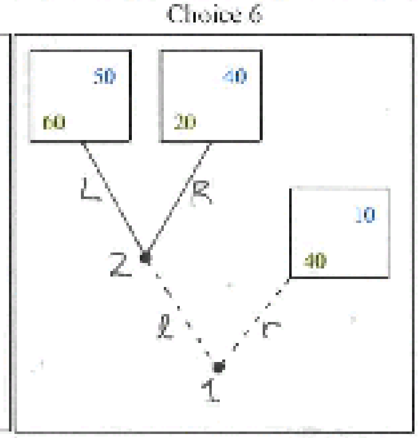


Choice 8

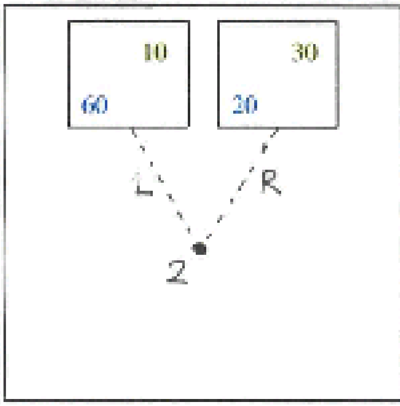


Choice 9

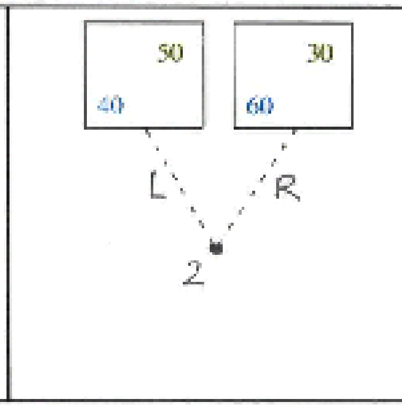
Second Mover Form

Participant #_____

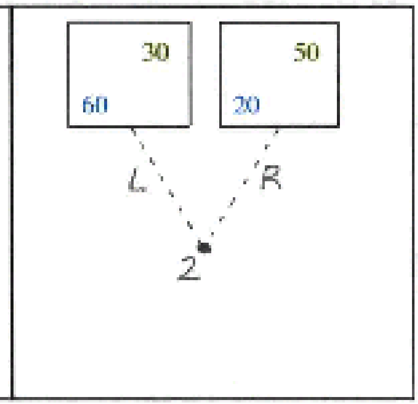
Draw a line along one of the two dashed lines to indicate your choice of the left or right branch. Your payoffs are in green. The blue payoffs are for the first mover. Units are cents. Your choice will be matched with each of the other first mover choices (but not your own). Your earnings for each choice will be determined by summing up your payoff for each match and dividing by the number of matches. Please make nine choices now.



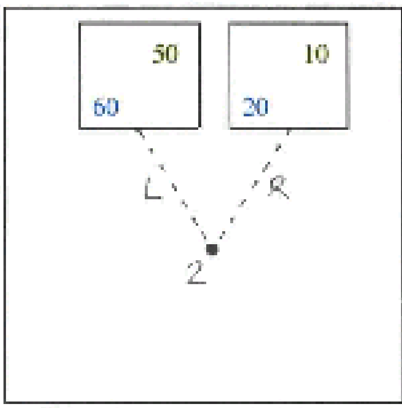
Choice 1



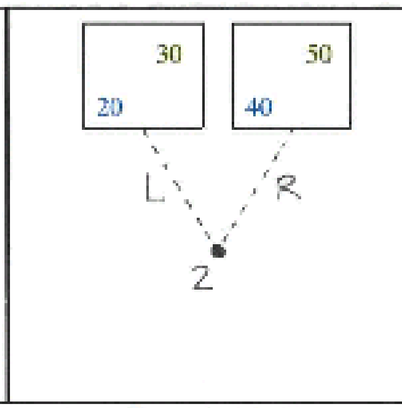
Choice 2



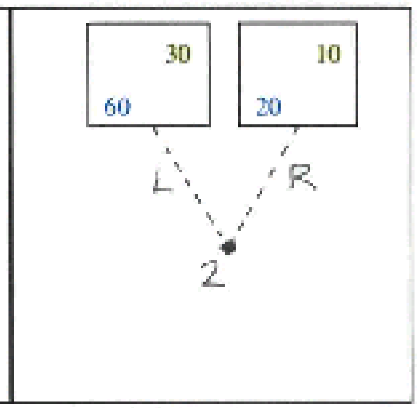
Choice 3



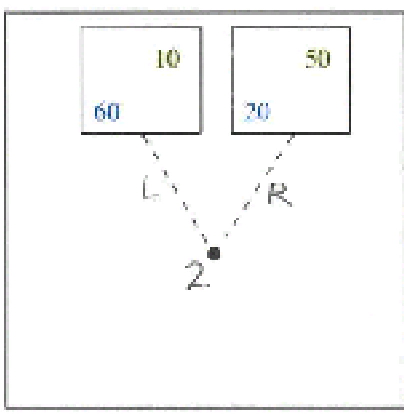
Choice 4



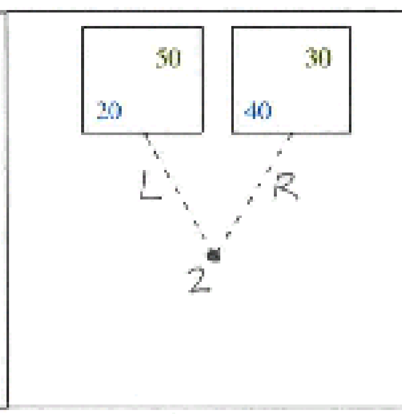
Choice 5



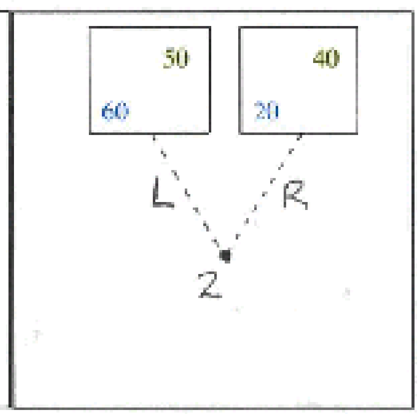
Choice 6



Choice 7



Choice 8



Choice 9