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Focusing Failures in Competitive Environments: Explaining Decision Errors in the Monty Hall Game, the Acquiring a Company Problem, and Multiparty Ultimatums

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ABSTRACT

This paper offers a unifying conceptual explanation for failures in competitive decision making across three seemingly unrelated tasks: the Monty Hall game (Nalebuff, 1987), the Acquiring a Company problem (Samuelson & Bazerman, 1985), and multiparty ultimatums (Messick, Moore, & Bazerman, 1997). We argue that the failures observed in these three tasks have a common root. Specifically, due to a limited focus of attention, competitive decision makers fail properly to consider all of the information needed to solve the problem correctly. Using protocol analyses, we show that competitive decision makers tend to focus on their own goals, often to the exclusion of the decisions of the other parties, the rules of the game, and the interaction among the parties in light of these rules. In addition, we show that the failure to consider these effects explains common decision failures across all three games. Finally, we suggest that this systematic focusing error in competitive contexts can serve to explain and improve our understanding of many additional, seemingly disparate, competitive decision-making failures. Copyright © 2003 John Wiley & Sons, Ltd.

KEY WORDS perspective taking; rules of the game; focusing; bounded rationality; Monty Hall; Acquiring a Company; ultimatums

The decision-making approach to negotiation has identified a number of systematic and important errors that negotiators make (Raiffa, 1982; Bazerman & Neale, 1982; Thompson, 2001). While influential, this work correctly has been criticized for relying on an overly narrow definition of the 'game' (Brandenburger & Nalebuff, 1996). Brandenburger and Nalebuff (1996) argue that how we define a game may be more important than how we play the game after it already has been defined. Bazerman et al. (2000) argue that Brandenburger and Nalebuff's (1996) critique suggests the need for a line of research that studies how

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decision makers understand the games they are playing. One of the few existing streams of research consistent with this critique is the social psychological study of how people construe conflicts (Keltner & Robinson, 1996; Robinson et al., 1995; Robinson & Keltner, 1996).

This paper attempts to contribute to the study of how people understand competitive environments by exploring how negotiators' limited focus of attention can lead to systematic errors in competitive contexts. Specifically, this paper shows that the failure properly to attend to the decisions of others, the rules of the game, and the interaction between these factors and the actor's decisions explains decision failures across three games that, at first glance, appear unrelated: the Monty Hall game (Nalebuff, 1987); the Acquiring a Company problem (Samuelson & Bazerman, 1985); and multiparty ultimatums (Messick, Moore, & Bazerman, 1997).

The negotiation literature is filled with prescriptions emphasizing counterintuitive behaviors that will help negotiators maximize their utility (Raiffa, 1982; Lax & Sebenius, 1986; Thompson, 2001). These prescriptions are the foundation of the negotiation courses that have proliferated in professional schools in the last two decades. Two critical pieces of this prescriptive advice relate to the structure of the competitive environment and the decisions of other parties. Game theorists encourage negotiators fully to understand the rules and structure of the game (Brandenburger & Nalebuff, 1996). Negotiators are similarly encouraged to think about the decisions of others, a tendency that has been found to be lacking (Bazerman & Neale, 1982; Davis, 1994). In fact, negotiators may fail to think through the perspective of others due to their egocentric view of the negotiation (Babcock & Loewenstein, 1997; Thompson & Loewenstein, 1992).

The current paper follows from Simon's (1957) and March and Simon's (1958) concept of bounded rationality, which suggests that people strive to be rational but are bounded by cognitive limitations. The most important development within the bounded rationality approach has occurred in the area of heuristics and biases (Hastie & Dawes, 2001; Kahneman, Slovic, & Tversky, 1982; Tversky & Kahneman, 1974). This area of research has profoundly affected the fields of psychology, economics, law, public policy, business, and medicine. Taking this research tradition in a new direction, this paper explores the types of information people focus on and shows the critical role this focus-of-attention plays in generating decision-making errors.

We argue that the rules of the game and the decisions of other parties, as well as the interaction between these factors and the actor's decisions, are pieces of information that are typically outside negotiators' focus. Consequently, individuals tend to engage in a radical simplification of the negotiation environment, acting as if a more direct path exists between their decisions and the outcomes they are likely to obtain. The typical results are a failure to attend to and analyze properly out-of-focus information, and systematic errors in competitive decision making. The failure fully to consider decision inputs leads negotiators to make suboptimal decisions, even when study participants have sufficient financial incentives to want to behave optimally (Ball, Bazerman, & Carroll, 1991).

The approach of this paper is consistent with an extensive literature in social-cognitive and cognitive psychology on mental representations of how humans define their social environment (Gentner & Stevens, 1983; Fiske & Taylor, 1984; Thompson, Loewenstein, & Gentner, 2000). More recently, there has been a growing literature on how focus-of-attention affects judgmental processes. Legrenzi, Girotto, and Johnson-Laird (1993) observe that the information on which individuals focus affects the mental models they create, which in turn affects their decisions. Gilbert, Wilson and colleagues (Gilbert & Wilson, 2000; Wilson et al., 2000) argue that individuals are affected by focalism—the biased processes in which individuals allow only select pieces of information to become part of their mental models. For example, these researchers show that individuals overestimate the magnitude and duration of their emotional response to an event. Wilson et al. (2000) argue that people focus too much on the event in question and fail to consider the impact of other events that are likely to occur in their lives.

In a parallel line of research, Schkade and Kahneman (1998) identify the focusing illusion, or the tendency of individuals making judgments to attend to only a subset of the available information, to overweight that information, and to underweight unattended information. Like much of the recent work on focalism, Schkade and Kahneman (1998) examine judgments of life satisfaction. The current paper significantly extends the notion that focusing affects judgmental processes, examining its effects on competitive decision making rather than on predictions of one's future well being. Specifically, we seek to provide a clear set of criteria that would help predict the content and type of information that is likely to be in and out of decision makers' focus in competitive settings.

Our goal is to identify the specific nature of focusing biases that are common and problematic in competitive contexts. We predict that decision makers will fail to think normatively about the relevance of the decisions of other parties and the details of the rules of the game that they are playing. We also predict that a critical determinant of rational decision making will be the rational inclusion of these inputs.

These predictions are tested through the use of protocol analysis, a process tracing methodology that analyzes participants' concurrent verbalizations while they attempt to solve the decision problems (e.g. Ericsson & Simon, 1993; van Somersen, Barnard, & Sandberg, 1994). This methodology has been used effectively in a number of recent studies to examine which decision inputs best predict the outcomes of complex decision tasks, such as mock jurors' determination of whether to award plaintiffs punitive damages in tort cases (e.g. Hastie, Schkade, & Payne, 1998).

We examine our predictions using variants of three well-studied decision problems: the Monty Hall game (Friedman, 1998; Nalebuff, 1987), the Acquiring a Company problem (Ball, Bazerman, & Carroll, 1991; Carroll, Bazerman, & Maury, 1988; Samuelson & Bazerman, 1985), and multiparty ultimatums (Messick, Moore, & Bazerman, 1997). Many other phenomena can be explained, at least in part, by the difficulty humans face as a result of excluding the decisions of others and the rules of the game from their focus of attention (Camerer & Lovallo, 1999; Ho, Camerer, & Weigelt, 1998; Costa-Gomes, Crawford, & Broseta, 2001; Moore, 2001). We chose a set of three simple games to allow for a parsimonious test of our ideas; we will return to related studies in the discussion section.

One striking feature of the three games we study is that, in each, individuals typically make the wrong decision, although the games require no complex analytical reasoning. We predict that the focusing of study participants, and specifically the tendency to leave the decisions of others and the rules of the game out of focus, is responsible for failure in all three tasks. For the sake of clarity, we present a brief background on the study of these three games.

THE MONTY HALL GAME

In a once-popular game show, host Monty Hall would ask contestants to pick one of three doors, knowing that one of the doors led to the grand prize and that the other two doors were 'zonks'—leading to small prizes or gag gifts. After the contestant picked a door, Monty would open one of the other two doors to reveal a zonk, and then offer the contestant the chance to trade their chosen door for the remaining unchosen and unopened door. Most people assume that, with only two doors remaining following the opening of one door by the host, the odds are 50 : 50, and most contestants preferred to hold to the door they originally chose.

Years after the show went off the air, statisticians, economists, and journalists (Selvin, 1975; Nalebuff, 1987; von Savant, 1990a, 1990b, 1991) noted that contestants tended to make a systematic mistake: they tended not to switch to the remaining unchosen door. That is, assuming that Monty always opened an unchosen door (we will call this the 'Monty always opens' condition) and then offered a switch, contestants should always have switched (Friedman, 1998; Nalebuff, 1987). The logic of switching is simple: when they first chose their door, the contestants had a one in three chance of winning the prize. When Monty opens one unchosen door to reveal a zonk, which he can always do, this probability does not change. There is still a one in three chance that the contestant had the winner to start with, and a two in three chance that the big prize was behind one of the other two doors. With one zonk revealed, the two in three chance is now carried by the unopened, unchosen door. The contestant should, therefore, always switch doors, to increase the odds of winning from one in three to two in three. In a laboratory analog of this problem, Friedman (1998) shows substantial failure to make the correct decision, and only limited learning through repeated trials.

The assumption that Monty always opened an unchosen door that did not contain the grand prize is, of course, a critical element in this analysis. Under that assumption, the correct response was to switch doors. One could make a very different assumption regarding Monty's behavior, however, assuming a 'mean Monty'—one who knows where the grand prize is located and who wants to minimize the contestant's chance of winning. Imagine that, after the contestant picks a door, 'Mean Monty' could either declare the game over or open one door and offer a switch. Assuming that Monty wants to minimize the contestant's chance of winning the grand prize, the contestant should never accept an offer from Monty to switch. In fact, since Monty wants the contestant to lose, the fact that Monty makes the offer indicates that the contestant has already picked the winning door.

In summary, normative analysis suggests that contestants should always switch doors in the 'Monty always opens' condition, but should never switch in the 'mean Monty' condition. Our prediction is, however, that the rules of the game and the decision rules of Monty, even when carefully spelled out, will be out of focus to contestants and will not be normatively evaluated. In addition, we predict that the consideration of these typically out-of-focus pieces of information is crucial to arriving at the correct answers to these problems.

ACQUIRING A COMPANY

Samuelson and Bazerman (1985) adapted Akerlof's (1970) 'lemons' problem to create a takeover game in which people systematically make offers with negative expected values (mistakes). Samuelson and Bazerman's problem is provided in Appendix A. In this game, one firm (the Acquirer) is considering making an offer to buy out another firm (the Target). Participants play the role of the Acquirer and are uncertain about the ultimate value of the Target. They know that its value under current management is between US$0 and US$100 per share, with all values equally likely. In addition, they know that the firm is expected to be worth 50 percent more under the Acquirer's management than under the current ownership. Thus, it appears to make sense for a transaction to take place. While the Acquirer does not know the actual value of the firm, the Target knows its current worth exactly. In the game, the Acquirer makes one take-it-or-leave-it offer, the target responds, and the game ends. What price should the Acquirer offer for the Target?

The dominant range of responses across a number of studies is between US$50 and US$75 (Ball, Bazerman, & Carroll, 1991; Carroll, Bazerman, & Maury, 1988; Grosskopf & Bereby-Meyer, 2001). Using protocol analyses to identify underlying cognitive patterns, Carroll, Bazerman, and Maury (1988) argue that the most common explanation for the $50 to $75 range is, 'on average, the firm will be worth $50 to the Target and $75 to the Acquirer; consequently, a transaction in this range will, on average, be profitable to both parties.'

In fact, it turns out that the correct answer is $0—making no offer at all. This is true because all offers have a negative expected value, with twice the chance of losing than of winning, and the possibility of losing twice as much as the largest possible gain. The negative expected value of any positive number is clarified by the following analysis of an offer of $60 per share (Bazerman, 2002):

> If I offer $60 per share, the offer will be accepted sixty percent of the time—whenever the firm is worth between $0 and $60 to the Target. Since all values between $0 and $60 are equally likely, the firm will, on average, be worth $30 per share to the Acquirer, resulting in a loss of $15 per share ($45 to $60). Consequently, a $60 per share offer is unwise.

Similar reasoning applies to any positive offer. On average, the Target is worth 25% less than the price the Acquirer pays when its offer is accepted. If the Acquirer offers $X and the Target accepts, the current value of the company is worth anywhere between $0 and $X. Any value in that range is equally likely, and the expected value of the offer is therefore equal to $X/2. Since the company is worth 50% more to the Acquirer, the Acquirer's expected value is 1.5(\$X/2) = 0.75(\$X), or 75% of its offer price. Thus, for any value of \$X, the best the Acquirer can do is not make an offer (\$0 per share).

The game sets a trap: even though in all circumstances the firm is worth more to the Acquirer than to the Target, any offer above \$0 generates a negative expected return to the Acquirer. Yet, the vast majority of people bid positive values that can be systematically explained. Replications with accounting firm partners, CEOs, investment bankers, and many other skilled groups have produced similar results (Bazerman, 2002). In addition, participants who were paid based on their performance, and given multiple trials to foster learning, exhibited similar patterns (Ball, Bazerman, & Carroll, 1991; Grosskopf & Bereby-Meyer, 2001).

Using protocol analysis, Carroll, Bazerman, and Maury (1988) concluded that the failure correctly to solve this problem was predictable based on the study participants' consideration of the decisions of the Target—or, the tendency to 'ignore the cognitions of others.' The current study seeks to extend this process-tracing approach and to explore whether better predictions are possible by considering both the decisions of the Target and the rules of the game.

MULTIPARTY ULTIMATUMS

One of the most common games studied by experimental economists is the ultimatum game (Guth, Schmittberger, & Schwarze, 1982; Roth, 1991). In the ultimatum game, Player 1 divides a known, fixed sum of money any way he chooses by filling out a form stating, 'I demand X.' Player 2 either accepts the offer and receives her portion of the money as allocated by Player 1 or rejects the offer, leaving both parties with nothing. Models that assume profit maximizing by both actors predict that Player 1 will offer Player 2 only slightly more than zero and that Player 2 will accept any offer greater than zero. These models fail to account for the fairness considerations that individuals who participate in ultimatum games incorporate into their offers and choices. Across studies, the average demand by Player 1 is commonly less than 70% of the funds, while individuals in the role of Player 2 often reject profitable but unequal offers (Ochs & Roth, 1989).

Messick, Moore, and Bazerman (1997) studied a multiple-party ultimatum game. In this version of the game, six participants were assigned to the roles of A, B, C, D, E, and F. Player A was given \$60 dollars to allocate to the six parties. The offers to B, C, D, E, and F had to be equal and had to be an integer. B, C, D, E, and F each recorded the minimum amount that they would accept. The key manipulation was the decision rule for the game. In one variation, if the amount that A offered to B to F was equal to or greater than the smallest amount requested by B, C, D, E, or F, the allocation of A went into effect, and if it was not, all parties received 0 (we will call this condition 'dividing the pie—smallest'). In the other condition, if the amount that A offered to B to F was equal to or greater than the largest amount requested by B, C, D, E, or F, the allocation of A went into effect, and if it was not, all parties received 0 (we will call this condition 'dividing the pie—largest').

Consistent with the two-party ultimatum game, a bimodal response pattern emerges from the demands of players B to F. Many B to F players will take \$1, since \$1 is better than the \$0 that they get by turning the offer down. Another large group of players B to F demand \$10—they want their 'fair' share. As we know from Tversky and Kahneman (1974), individuals underestimate disjunctive events and overestimate conjunctive events. In the present context, this phenomenon leads to the prediction that 'A' players will underestimate the likelihood of how easy it is to get at least one out of five people to accept \$1 and overestimate the likelihood of all five individuals accepting anything less than \$10. Empirically, Messick, Moore, and Bazerman (1997) found that the profit maximizing strategy for player A would be to divide the money 55–1–1–1–1–1 in the 'dividing the pie—smallest' condition, and to divide it 10–10–10–10–10–10 in the 'dividing the pie—largest' condition. In fact, in the latter version, any allocation other than 10 invariably led to 'A' players receiving \$0.

While the empirically best strategy for player A diverged dramatically between the two conditions (offers of \$1 vs. \$10), the actual behavior of 'A' players was much closer across the two conditions. On average, 'A' players allocated \$8.15 to the other players in the 'dividing the pie—smallest' condition, while allocating \$8.47 to the other players in the 'dividing the pie—largest' condition. Many 'A' players in the 'dividing the pie—largest' condition were missing an easy opportunity to collect \$10, and 'A' players in the 'dividing the pie—smallest' condition were foregoing a significant profit opportunity. While part of the failure of 'A' players to maximize their expected value can be explained by fairness, and/or very strong risk aversion in the 'dividing the pie—smallest' condition, we will test the possibility that much of the failure is due to 'A' players being insensitive to the decision rule and to the heterogeneity of players B to F. The current experiment will adapt the Messick, Moore, and Bazerman (1997) tasks to rule out fairness and risk aversion as alternative explanations.

Collectively, the three problems—Monty Hall, Acquiring a Company, and the multiparty ultimatum—comprise our experimental study. As far as we know, these problems have never been cross-referenced in earlier work. In studying these problems together, we seek to highlight a common behavioral phenomenon underlying participants' decision failure in these problems. To provide a more direct measurement of the participants' focus and the nature of the analysis they engage in while solving the problems, we collected verbal protocols using methods that are well-established in cognitive psychology generally and in decision-making research specifically (e.g. Biggs, Rosman, & Sergenian, 1993; Ericsson & Simon, 1993; Ford et al., 1989; Payne, Bettman, & Johnson, 1993; Svenson, 1989a, 1989b; van Somersen, Barnard, & Sandberg, 1994).

We predict that an analysis of participants' verbal protocols and their decisions will reflect their tendency to not consider fully the decisions of others and the rules of the game. In addition, we predict that this failure will be related to a lack of focus on the rules of the game, the decisions of the other party, and the interaction between these effects. After providing these empirical tests, we will return to the broader literature to speculate more generally on how the failure to think through focusing errors in a competitive environment can explain a variety of anomalies that have been discovered by economists, psychologists, and behavioral-decision researchers.

METHODS

Participants

Eighty-two graduate and undergraduate students from Boston-area universities were recruited by advertisements offering participation in a decision-making study for pay, consisting of a base fee of \$10 for participation, an additional \$5 early show-up incentive, and a \$4 fee for every correct answer they would give in each of five experimental tasks, with a potential maximum total of \$35.

Procedure

Participants were instructed that the computerized study should take them about one hour to complete. They were asked to 'think aloud' as they worked on the problems, saying out loud every thought while solving the problems, and were told that they would be recorded by the microphone attached to their computer station. The instructions also informed the participants that they could take as long as they required to solve the problems, and that because similar-looking problems might actually be different from one another, they should pay close attention.[1]

To help the participants feel comfortable talking aloud while solving problems, they were given a practice session that included three different problems. The practice problems closely resemble problems that have been previously used as warm-up tasks in verbal protocol studies (e.g. Ericsson & Simon, 1993; van Somersen, Barnard, & Sandberg, 1994). When participants momentarily forgot to verbalize their thoughts during the practice problems, they were given a neutral prompt by an experimenter, such as 'please

[1]The exact wording of the complete instructions can be obtained from the authors.

keep talking.' After completing the three practice tasks, the participants were informed that the practice session had ended and that the experimental tasks would begin.

Each participant was given a series of five tasks comprised of two versions of the Monty Hall (MH) problem, two versions of the Dividing the Pie (DP) task, and one version of the Acquiring a Company (AAC) problem (see Appendix for the AAC problem).

As clarified earlier, the correct answer to the AAC problem was zero—to make no offer.

The two versions of the Monty Hall problem were the 'Monty Always Opens' (MAO) version and the 'Mean Monty' (MM) version. Both were presented on the computer screen, and it was made clear to the participant that the computer would play the role of box opener (i.e. the computer would be in the role of Monty). The general text for both is provided below, with the unique information for the MAO version in bracketed italics and the unique information for the MM version in bracketed bold:

> In a recent study, college students were given the following question:
>
> In this problem, you will be given a choice of boxes X, Y, or Z. One of these three boxes has a valuable prize in it. The other two boxes are empty. After you pick one of the boxes, the computer [*will definitely*] [**may**] open one of the other two boxes, show you that this unchosen box does not have the prize, and offer you to trade your chosen box for the unopened unchosen box. [**The computer will make its decision whether to open a box and offer you a switch with the goal of minimizing the likelihood that you get the prize.**]
>
> For example, if you were to choose box X, the computer [*would*] [**might decide to**] open one of the two other boxes (e.g. Y) [*and show you it's empty. The computer would then*] [**show you it's empty and**] offer you the opportunity to switch your choice from X to Z.\
>
> A student who participated in the study picked box Y. The computer then opened box Z, showed the student it was empty, and offered the student to trade box Y (which the student chose) for box X (the remaining unopened, unchosen box).
>
> Please state whether the student should have traded box Y for box X or not, in order to have the best chance of winning the prize.
>
> Answer: Yes ____ No ____

Based on the logic presented above, the correct answer to the MAO version is 'Yes' (to switch), and the correct answer to the MM version is 'No' (not to switch).

Similarly, the two versions of the Multiparty Ultimatum game included a 'Dividing the Pie—Smallest' version (DPS) and a 'Dividing the Pie—Largest' version (DPL). The general text for both is provided below, with the unique information for the DPS version in bracketed italics, and the unique information for the DPL version in bracketed bold:

> In this exercise, six people will be randomly assigned to the roles A, B, C, D, E, and F. A will be randomly selected, and given $60 to allot among A, B, C, D, E, and F. The amounts given to B, C, D, E, and F must be equal, but this amount may be different from the amount that A allocates to A (herself/himself). B, C, D, E, and F will be asked to specify the minimum amount that they would accept. If the amount offered by A to each of B, C, D, E, and F is equal to or greater than the [*smallest*][**largest**] amount specified by B, C, D, E, or F, the $60 will be divided as specified by A. If, however, [*all*][**any**] of the amounts specified by B, C, D, E, and F are larger than the amount offered by A, all six parties will receive $0.
>
> Please specify the allocation from A that would maximize A's average dollar payoff:
>
> (Use whole numbers not decimals/fractions)
>
> A: $____ B: $____ C: $____ D: $____ E: $____ F: $____

Based on Messick, Moore, and Bazerman (1997), the correct answer to DPS was either 55–1–1–1–1–1 or 50–2–2–2–2–2 (these two answers were very close in maximizing expected value in Messick, Moore, & Bazerman, 1997), and the correct answer to DPL was 10–10–10–10–10–10.

The participants were randomly assigned to one of four counterbalanced orders of the 5 tasks:

Order A: MAO, DPS, AAC, MM, DPL
Order B: DPS, MAO, AAC, DPL, MM
Order C: MM, DPL, AAC, MAO, DPS
Order D: DPL, MM, AAC, DPS, MAO

The four orders were created to maximize the distance between the pairs of problems (between MAO and MM and between DPL and DPS), and to have MAO, MM, and DPS, and DPL in each of the 1st, 2nd, 4th, and 5th spots. Keeping AAC in the third spot allowed for this counterbalancing.

After completing the experiment, participants were debriefed and given an opportunity to ask further questions about the experiment.

Protocol analysis

The analysis followed recommended procedures for analyzing verbal protocols (e.g. Ericsson & Simon, 1993; van Somersen, Barnard, & Sandberg, 1994). As in other recent studies that used protocol analysis to examine complex decisions (e.g. Hastie, Schkade, & Payne, 1998), the protocols were transcribed and divided into segments representing, to the extent possible, a single thought or idea. When a participant developed a more complex idea without pause, the statement was not broken but rather retained as one unit in its original form. After the transcription was completed, the verbalizations for each of the five problems for the 82 participants was coded by two independent coders in accordance with a coding scheme that was prepared in advance.

The coding scheme was developed to allow for an examination of our main hypotheses: that participants: (1) will fail to fully consider the rules of the game and the decisions of other parties; and (2) that this failure will be related to outcome failure in each of the problems. Specifically, participants' statements were divided into four substantive categories and one miscellaneous category. Based on a simple characterization of competitive contexts, the four substantive categories included 'Rules' (R), 'Actor' (A), 'Other' (O), and 'Interaction' (I), with statements earning their designation as belonging to one of the categories based on their subject matter and the perspective they reflected. The fifth general code was for miscellaneous statements (M) that did not fit in one of the four general code categories.

A statement discussing the actor's interest or strategy in the game was designated an A statement. Often, these statements included how the actor connected with the rules of the game. A statement about the other parties was designated an O statement. Again, these statements often included how the other parties connected to the rules of the game. A statement just about the rules of the game was coded as an R statement. And, a comment that talked about how the actor interacted with other parties and the rules of the game (intuitively, creating a three-way interaction) was coded as an I statement.

Within each of the four general codes (Actor, Other, Rules, Interaction), participants could be making correct or incorrect statements. We therefore introduced subcategories within each of these four general codes: 'normative analysis' (na), 'mistaken' (w) and 'miscellaneous' (m), for each of the primary categories. In the case of 'Rules' statements, an additional category—'normative reiteration' (nr)—was added to allow for statements that repeated important parts of the task without any additional analysis. Consequently, the scheme included five main categories, and fourteen categories overall (Rna, Rw, Rm, Rnr, Ana, Aw, Am, Ona, Ow, Om, Ina, Iw, Im, and M), each of which is separately defined in Table 1.[2]

[2]Examples of each of these codes or the complete protocols are available from authors.

Table 1. Summary of coding scheme

Code	Description
Rules (R)	Statements predominantly relating to the rules of the problems.
Rna	Statements predominantly relating to the rules of the problems involving a correct analysis of some aspect of the rules.
Rw	Statements predominantly relating to the rules of the problems involving a mistake in understanding or analysis.
Rnr	Statements predominantly relating to the rules of the problems involving a reiteration of some important component of the rules.
Rm	All other statements predominantly relating to the rules of the problems.
Actor (A)	Statements predominantly relating to the focal decision maker (actor) and his interaction with the rules.
Ana	Statements predominantly relating to the focal decision maker and his interaction with the rules involving a correct analysis of some aspect of this interaction.
Aw	Statements predominantly relating to the focal decision maker and his interaction with the rules involving a mistake in understanding or analysis of this interaction.
Am	All other statements predominantly relating to the focal decision maker and his interaction with the rules.
Other (O)	Statements predominantly relating to the other parties and their interaction with the rules.
Ona	Statements predominantly relating to the other parties and their interaction with the rules involving a correct analysis of some aspect of this interaction.
Ow	Statements predominantly relating to the other parties and their interaction with the rules involving a mistake in understanding or analysis of this interaction.
Om	All other statements predominantly relating to the other parties and their interaction with the rules.
Interaction (I)	Statements predominantly relating to the interaction between the actor and the other parties (in light of the rules of the problems).
Ina	Statements predominantly relating to the interaction between the actor and the other parties involving a correct analysis of some aspect of this interaction (in light of the rules of the problem).
Iw	Statements predominantly relating to the interaction between the actor and the other parties involving a mistake in understanding or analysis of this interaction (in light of the rules of the problem).
Im	All other statements predominantly relating to the interaction between the actor and the other parties (in light of the rules of the problems).
Miscellaneous (M)	All other statements not included in any of the previous categories.

Our focus in the rest of the paper will be limited to the conceptually interesting codes: Rna, Rw, Ana, Aw, Ona, Ow, Ina, and Iw. As a reminder, we expected codes Rna, Ona, and Ina to be positively related to performance, and Rw, Ow, and Iw to be negatively related to performance.

Once it was established that the transcribed protocols could be coded according to the scheme, a subsample of eight protocols was randomly selected. Two coders reviewed these protocols independently, coded them according to the scheme, compared their results and discussed the statements' possible designations until consensus was reached. This led to a convergence between the two coders concerning how each type of statement would be coded. All protocols were then independently coded by the two coders. Each statement received one of the 14 possible codes. After completing their independent coding, the two coders compared the codes they designated for each statement in the protocols. In each case of divergence between the codes designated for a statement by the coders, they discussed and agreed on a final coding. Interrater reliability for the two coders was 84.9% on a statement by statement ('counts') basis.

RESULTS

Correct responses

Consistent with prior research, participants' overall task performance was poor. None of the 82 participants solved all five problems correctly, 5 participants (6.1%) solved four problems correctly, 14 participants (17.1%) solved three problems correctly, 43 participants (52.4%) solved two correctly, 18 participants (22.0%) solved only one of the problems correctly, and 1 participant (1.2%) solved no problem correctly.

Viewing the results on a task-by-task basis, 8.5% of the participants provided the correct answer to the Acquiring a Company (AAC) problem, 56.1% provided the correct answer to the Dividing the Pie—Largest (DPL) problem, 17.1% provided the correct answer to the Dividing the Pie—Smallest (DPS) problem, 41.4% provided the correct answer to the Monty Always Opens (MAO) problem, and 79.3% provided the correct answer to the Mean Monty (MM) problem. The mean offer in AAC was \$65.84 per share, the mean allocation to others in the DPL problem was \$8.12, and the mean allocation to others in the DPS problem was \$7.02. The AAC mean is relatively high in comparison to previous one-shot AAC studies. The means for the two Dividing the Pie problems are roughly consistent with (although slightly lower than) the results of Messick, Moore, and Bazerman (1997).

Since we predicted that focusing errors would prevent many participants from differentiating MAO from MM and DPL from DPS, we also employed an alternative definition according to which a correct answer requires a correct response to both halves of these problems—viewing the five problems as creating only three distinct tasks (MH-pair, AAC, DP-pair). Using this definition, the results are even more striking: only 6 participants (7.3%) answered both parts of DP correctly and only 20 (24.4%) answered both parts of MH correctly. In sum, with an overall success rate measured on a scale of 0–3, none of the participants solved all three problems correctly, 5 participants (6.1%) answered two out of three correctly, 28 (34.1%) answered one out of three correctly, and 49 participants (59.8%) made no correct responses at all. Thus, on a 0–3 scale, the modal success rate was zero.

There was little consistency in task performance across our five tasks. The correlations among correct responses for participants for the five problems range from a low of -0.42 (MAO–MM) to a high of 0.21 (AAC–DPS), with a mean intertask correlation of -0.04. The other task pair (DPL–DPS) similarly showed a negative correlation (-0.19). Thus, for the two task pairs, the correlations were negative reflecting the large proportion of participants who failed to analyze the two versions differently.

As further evidence of the participants' lack of ability to distinguish between the two versions of MH, we found that 58 participants (70.7%) gave the same response to both MH tasks (thus answering only one of them correctly) and 4 participants (4.9%) incorrectly answered both MH tasks. The DP tasks showed a similar pattern, with 49 participants (59.8%) giving the same response twice and 27 participants (32.9%) giving two mistaken responses. In other words, in both MH and DP, a majority of participants did not vary their response, giving the same answer to the two versions of the tasks.

Protocol analysis

Table 2 shows the percentage of participants who had a code show up, compares the participants who solved the problems correctly with those who were incorrect, and reports the χ^2 statistic with its associated levels of significance for each of our eight conceptually relevant codes.

Participants who responded correctly systematically exhibit higher rates of the normative analysis codes (Rna, Ana, Ona, and Ina) and lower rates of the mistaken understanding and analysis codes (Rw, Aw, Ow, and Iw). The opposite pattern holds for the incorrect participants. Thus, the 'na' codes tend to be positively correlated with correct responses and the 'w' codes tend to be negatively correlated with correct responses. In fact, of the 56 comparisons in the table (8 codes $\times$ 7 tasks/task pairs), only 9 comparisons violate this pattern. Moreover, across all tasks and task pairs, this pattern is never violated for the Rw, Ona, Ina, and Iw codes.

Table 2. Code show-up percentages by task, for correct vs. incorrect responses (and χ^2 statistic)

Code	Task													
	AAC ($n=82$)		DPL ($n=81$)		DPS ($n=82$)		DP-pairs ($n=81$)		MAO ($n=82$)		MM ($n=82$)		MH-pairs ($n=82$)	
	Correct ($n=7$)	Incorrect ($n=75$)	Correct ($n=46$)	Incorrect ($n=35$)	Correct ($n=14$)	Incorrect ($n=68$)	Correct ($n=6$)	Incorrect ($n=75$)	Correct ($n=34$)	Incorrect ($n=48$)	Correct ($n=65$)	Incorrect ($n=17$)	Correct ($n=20$)	Incorrect ($n=62$)
Rna	0.0%	0.0%	6.5%	2.9%	14.3%	4.4%	16.7%	10.7%	8.8%	4.2%	4.6%	5.9%	15.0%	8.1%
	(na)		(0.57)		(1.98)		(0.20)		(0.75)		(0.05)		(0.83)	
Rw	28.6%	30.7%	28.7%	40.0%	21.4%	26.5%	50.0%	53.3%	14.7%	20.8%	20.0%	35.3%	25.0%	41.9%
	(0.01)		(1.23)		(0.15)		(0.02)		(0.50)		(1.77)		(1.84)	
Ana	0.0%	2.7%	0.0%	0.0%	14.3%	5.9%	0.0%	8.0%	38.2%	22.9%	21.5%	11.8%	45.0%	41.9%
	(0.19)		(na)		(1.21)		(0.52)		(2.26)		(0.82)		(0.06)	
Aw	0.00%	37.3%	4.4%	20.0%	0.0%	16.2%	0.0%	24.0%	2.9%	2.1%	3.1%	0.0%	5.0%	4.8%
	(3.97)**		(4.93)**		(2.62)		(1.85)		(0.06)		(0.54)		(0.00)	
Ona	14.3%	12.0%	28.7%	11.4%	42.9%	1.5%	33.3%	26.7%	14.7%	4.2%	49.2%	29.4%	75.0%	38.7%
	(0.03)		(3.40)*		(25.47)***		(0.12)		(2.83)*		(2.14)		(7.99)***	
Ow	0.0%	16.0%	28.6%	21.7%	28.6%	48.5%	33.3%	56.0%	11.8%	4.2%	7.7%	23.5%	35.0%	12.9%
	(1.31)		(0.50)		(1.87)		(1.15)		(1.69)		(3.46)*		(4.94)**	
Ina	28.6%	6.7%	37.0%	0.0%	64.3%	2.9%	83.3%	28.0%	17.7%	0.0%	50.8%	17.7%	80.0%	37.1%
	(3.93)**		(16.37)***		(37.61)***		(7.80)***		(9.14)***		(6.00)***		(11.16)***	
Iw	42.9%	94.7%	41.3%	91.4%	21.4%	98.5%	50.0%	98.7%	73.5%	100.0%	52.3%	88.2%	95.0%	100.0%
	(19.52)***		(21.41)***		(55.24)***		(28.03)***		(14.27)***		(7.23)***		(3.14)*	

*$p<0.10$; **$p<0.05$; ***$p \leq 0.01$.

Thus, in AAC, successful participants exhibited an Aw code significantly less frequently than their incorrect peers; the former, in fact, never exhibited this code (0.0% vs. 37.3%; $\chi^2_{(1)} = 3.97$; $p < 0.05$). The incorrect participants were, therefore, more likely to limit their focus and state, for example:

> 'I will bid \$75 because it is around the middle of my expected value.'

Successful participants also exhibited significantly less Iw than did incorrect participants (42.9% vs. 94.7%; $\chi^2_{(1)} = 19.52$; $p < 0.01$). Incorrect respondents were more likely to reason erroneously, stating, for example:

> 'You could offer anything up to \$149 and make some profit.'

Successful participants, on the other hand, exhibited significantly more Ina than did incorrect participants as well (28.6% vs. 6.7%; $\chi^2_{(1)} = 3.93$; $p < 0.05$), being more likely to conclude that:

> 'Any positive value offer I can make will be unprofitable because [the target] knows [its true value] when they decide [whether to accept the bid].'

In DPL, correct participants showed significantly less Aw (4.4% vs. 20.0%; $\chi^2_{(1)} = 4.93$; $p < 0.05$) than did incorrect ones. The latter were therefore more likely to state, for example:

> 'A obviously will take more for himself.'

Successful participants also exhibited somewhat more Ona (28.7% vs. 11.4%; $\chi^2_{(1)} = 3.40$; $p < 0.10$) than did incorrect ones, more frequently making statements such as:

> 'Some of the others will want \$10.'

Correct respondents also showed far more Ina than did incorrect participants, who, in fact, showed no Ina (37.0% vs. 0.0%; $\chi^2_{(1)} = 16.37$; $p < 0.01$). The correct respondents were, therefore, far more likely to reason that:

> 'A should give \$10 to everybody' cause someone is definitely going to want \$10.'

Last, successful participants showed far less Iw (41.3% vs. 91.4%; $\chi^2_{(1)} = 21.41$; $p < 0.01$) than did incorrect participants, who were thus more likely to conclude that:

> 'A should keep all of the \$60 to herself.'

A similar pattern appeared in DPS, where correct participants were much more likely to make Ona statements (42.9% vs. 1.5%; $\chi^2_{(1)} = 25.47$; $p < 0.01$), such as:

> 'Somebody is going to want only \$1.'

These participants were also more likely to make Ina statements (63.4% vs. 2.9%; $\chi^2_{(1)} = 37.61$; $p < 0.01$) such as:

> 'A could give as low as \$1 [because at least one of the others will probably demand only that sum].'

In addition, successful participants were far less likely than their incorrect counterparts to make Iw statements (21.4% vs. 98.5%; $\chi^2_{(1)} = 55.24$; $p < 0.01$), such as:

> 'It wouldn't really benefit A to be greedy and take more for herself, because [the others] will all want \$10.'

In MAO, successful participants more often made Ona statements than did incorrect participants (14.7% vs. 4.2%; $\chi^2_{(1)} = 2.83$; $p < 0.10$), and were thus more likely to realize that:

> 'The computer knows where the prize is.'

Correct respondents were similarly more likely to make Ina statements than were incorrect participants, who made no such statements (17.7% vs. 0.0%; $\chi^2_{(1)} = 9.14$; $p < 0.01$). For example, one correct respondent said:

> 'The box the student chose will have the prize only 1 out of 3 times, but the remaining box will have it 2 out of 3 times, so the student should switch.'

Correct respondents also made Iw statements significantly less frequently than the incorrect subjects—all of whom made such a statement—did (73.5% vs. 100.0%; $\chi^2_{(1)} = 14.27$; $p < 0.01$). For example, one incorrect respondent said:

> 'The odds of the two remaining boxes are the same, so it does not matter if the student switches.'

The MM task provided a similar picture. Successful participants were less likely than incorrect ones to make Ow statements (7.7% vs. 23.5%; $\chi^2_{(1)} = 3.46$; $p < 0.10$); incorrect participants were more likely to state, for example:

> 'The fact is that the computer will always show you an empty box.'

Correct participants were also more likely to show Ina codes (50.8% vs. 17.7%; $\chi^2_{(1)} = 6.00$; $p = 0.01$), exemplified by the following protocol:

> 'You're guaranteed to lose if the computer shows you an empty box and the remaining and offers to switch, because it will only do that when you have no option of prizes in that box.'

Incorrect participants, on the other hand, were more likely to show Iw codes (52.3% vs. 88.2%; $\chi^2_{(1)} = 7.23$; $p < 0.01$), stating for example that:

> 'The odds stay the same from the beginning and [whatever the computer does] doesn't affect your chance of winning.'

Much like the five individual tasks, the two task pairs showed a relationship between the participants' focus of attention and their task performance. To evaluate whether the code showed up or not in task pair, we coded that it did show up if it was mentioned in either or both of the task pairs.[3] In the DP-pairs, successful participants had a significantly higher likelihood of an Ina code show-up (83.3% vs. 28.0%; $\chi^2_{(1)} = 7.80$; $p < 0.01$) and a significantly lower likelihood of an Iw code show-up (50.0% vs. 98.7%; $\chi^2_{(1)} = 28.03$; $p < 0.01$) than did participants who answered the task pair incorrectly. In the MH-pair, successful participants had a significantly higher rate of an Ona code show-up (75.0% vs. 38.7%; $\chi^2_{(1)} = 7.99$; $p < 0.01$), but also a significantly higher rate of an Ow code show-up than that of their incorrect peers—in the opposite direction from that predicted (35.0% vs. 12.9%; $\chi^2_{(1)} = 4.94$; $p < 0.05$). Both interaction codes showed the expected pattern, however, with successful participants being significantly more likely to exhibit an Ina code (80.0% vs. 37.1%; $\chi^2_{(1)} = 11.16$; $p < 0.01$) and somewhat less likely to exhibit an Iw code (95.0% vs. 100%; $\chi^2_{(1)} = 3.14$; $p < 0.10$) than incorrect participants.

Table 2 reveals that the interaction codes showed a repeatedly strong and highly significant effect across all the tasks. 'Other' and 'Actor wrong' statements also showed significant effects for a number of the tasks. Given that multiple codes may be additively contributing to the prediction of correct answers and to significant levels of multicolinearity between many of the codes, better tests of how negotiator focus affects performance could be obtained by multivariate analyses. We test our hypotheses with both logistic regression and multiple regression. The former is used to test the significance of code show-ups in predicting the

[3]This criterion was chosen to account for the tendency of many participants to skip a detailed verbal analysis when solving the second task in the pair and simply relate back to their earlier protocol.

Table 3. Logistic regression predicting participants' task-by-task correct responses

Variable	Task						
	AAC	DPL[a]	DPS	DP-pairs	MAO[a]	MM	MH-pairs[a]
Intercept	−0.21	1.54***	1.25	−0.59	2.26**	2.14***	−1.94
	(0.74)	(na)	(1.13)	(1.70)	(na)	(0.78)	(na)
Ina show-up	2.11*	2.66***	3.01**	1.69	2.43**	1.27*	1.83***
	(1.18)	(na)	(1.29)	(1.25)	(na)	(0.71)	(na)
Iw show-up	−3.35***	−2.26***	−4.79***	−3.40***	−3.02***	−1.67**	0.36
	(1.01)	(na)	(1.29)	(1.37)	(na)	(0.81)	(na)
χ^2 for log likelihood	14.50***	27.77***	48.39***	14.88***	21.43***	12.01***	31.17***
'Pseudo' R^2	0.30	na	0.65	0.35	na	0.14	na
Nagelkerke R^2	0.37	na	0.74	0.41	na	0.21	na

*$p < 0.10$; **$p < 0.05$; ***$p \leq 0.01$.
[a]In tasks where quasi-complete separation of data points existed, exact conditional regression was used, and we report exact parameter estimates and the score statistic for overall model fit (Agresti, 1996; Hosmer & Lemeshow, 2000).

task-by-task success and failure of participants. The latter is used to test the significance of the code show-ups in predicting participants' overall success across tasks.

The binary nature of the dependent variable in logistic regression limits the number of independent variables that can be used to conduct a test with sufficient power to the smallest number of observations per level of the dependent variable (i.e. correct/incorrect). In the present case, as reported in Table 2, the smallest number of observations ('*n*') appear in those 'correct' cells that had as few as six and seven observations per cell for DP-pair and AAC, respectively. These small *n*-values required us to choose as few predictors in the logistic regression equation as possible (Agresti, 1996; Hosmer & Lemeshow, 2000).

Given this limitation, we made our choice based on both theoretical and statistical considerations. We expected the interaction codes—Ina and Iw—to be highly predictive, since any analysis of the interaction between the parties implies either attention to, or neglect of, the others' decisions, the rules, and the interaction among them. This selection was further supported by the between-group comparisons in Table 2; these codes systematically showed the highest levels of significance and were the only codes that were significant across all tasks and task-pairs, without exception.

Table 3 reports the results of seven logistic regressions in which the Ina and Iw codes serve as independent variables and the binary variable of correct/incorrect response, per task or task pair, is the dependent variable. The table reports the parameter estimates with their standard errors (in parentheses) and the associated χ^2-distributed significance levels. We also report the model fit statistics of χ^2 for log likelihood with its associated significance levels, 'pseudo' R^2, and Nagelkerke R^2. In those tasks where quasi-complete separation of data points existed due to the extremely high predictive power of an independent variable, exact conditional analysis was used instead (Agresti, 1996; Hosmer & Lemeshow, 2000), and we report exact parameter estimates and the score statistics for overall model fit.

Both of the substantive interaction predictors are significant for all five individual tasks and one of them is highly significant for each of the task pairs. Moreover, the predictor coefficients always bear the expected sign—positive for Ina, which is therefore associated with an increase in the likelihood of a correct response, and negative for Iw, which is thus associated with a decrease in the likelihood of a correct response. The significance of only one of the two independent variables in predicting success in each of the two task pairs is potentially due to multicolinearity and small sample sizes for some of the independent variable combinations (see Table 4).

To provide practical insight about the magnitude of the predictor coefficients in Table 3, Table 4 allows an examination of the likelihood of a correct response from participants who exhibit: (a) only an Ina code; (b) an

Table 4. Counts and percentages of correct responses for code show-up combinations, by task

Code show-up	Task						
	AAC	DPL	DPS	DP-pairs	MAO	MM	MH-pairs
Ina only							
Correct responses	1/1	13/13	8/8	3/4	2/2	20/20	1/1
(%)	100.0	100.0	100.0	75.0	100.0	100.0	100.0
Both Ina and Iw							
Correct responses	1/6	4/4	1/3	2/22	4/4	13/16	15/38
(%)	16.7	100.0	33.3	9.09	100.0	81.3	39.5
No Ina, no Iw							
Correct responses	3/7	14/17	3/4	0/0	7/7	11/13	0/0
(%)	42.9	82.4	75.0	0.0	100.0	84.6	0.0
Iw only							
Correct responses	2/68	15/47	2/67	1/55	21/69	21/33	4/43
(%)	2.9	31.9	3.0	1.82	30.4	63.6	9.3

Ina and an Iw code; (c) neither an Ina nor an Iw code; and (d) only an Iw code. In all of the individual tasks, participants exhibiting the Ina code alone—with no Iw—were correct 100.0% of the time, while in the task pairs they were correct 75.0% of the time in the DP-pairs, and 100.0% of the time in the MH-pairs. On the other hand, participants exhibiting Iw codes only—with no Ina—responded correctly only 2.9% of time in AAC, 31.9% of the time in DPL, 3.0% of the time in DPS, 30.4% of the time in MAO, and 63.6% of the time in MM. In the task pairs, these participants were correct only 1.82% of the time in the DP-pairs, and 9.3% of the time in the MH-pairs.

The overall model fit for the logistic regressions was highly significant for all of the tasks as well, as shown by the model fit row of Table 3. The 'pseudo' R^2 values range between 0.14 and 0.65 and the Nagelkerke R^2 values range between 0.21 and 0.74 (where exact conditional analysis was not used), indicating that while the models have a highly significant predictive power, other factors not appearing in the models contribute to the prediction of correct responses as well.

Table 5 reports the results of two hierarchical multiple regressions in which the substantive code show-ups serve as predictors of the participants' overall success. By creating a numerical dependent variable of overall

Table 5. Hierarchical multiple regression predicting participants' overall success rate

Predictor variables	Sum correct A: 0–3 scale		Sum correct B: 0–5 scale	
	Step 1	Step 2	Step 1	Step 2
Rna show-up	0.04	0.06	0.08	0.06
Rw show-up	−0.19	−0.18*	−0.09	−0.02
Ana show-up	−0.02	0.04	0.02	0.12
Aw show-up	−0.07	0.01	−0.19*	−0.03
Ona show-up	0.24*	−0.04	0.35***	−0.14
Ow show-up	0.10	0.10	−0.03	0.01
Ina show-up	—	0.35**	—	0.54***
Iw show-up	—	−0.30***	—	−34***
Adjusted R^2	0.09**	0.25***	0.15**	0.42***
Δ adjusted R^2	—	0.16***	—	0.27***

The table reports standardized regression coefficients.
$*p < 0.10$; $**p < 0.05$; $***p < 0.01$.

success rate and with the further aid of the hierarchical procedure we were able to run all of the substantive codes as predictors and obtain a more comprehensive view of their relative contribution to correct responses. Additionally, the new dependent variables used in these regressions provide an across-tasks perspective on participants' performance.

The table reports two hierarchical regressions: the first treats each of the task pairs DP and MH as a single task (i.e. a participant may obtain a sum score ranging from 0, for no correct responses, to 3, for all correct responses), while the second treats each one of the five tasks as a discrete task (i.e. with a dependent variable score ranging from 0–5). All of the eight substantive codes serve as independent variables, with each variable obtaining a sum score based on the number of tasks in which the code showed up, with scores ranging from 0–3 for sum correct A, and 0–5 for sum correct B. To test the independent contribution of the Interaction codes, beyond that of the other substantive codes, we entered the code variables in two steps. Step 1 included a block of the six non-Interaction code variables, with the Interaction code variables being added in Step 2. For each of these regressions, the table reports the standardized regression coefficients and their associated significance levels for all of the substantive codes, as well as the adjusted R^2 for overall model fit and the change in adjusted R^2 from Step 1 to Step 2, each with its associated levels of significance.

A look at the results in Table 5 reveals a strong relationship between the interaction codes and participants' overall success in the tasks, with both the Ina ($\beta_{\text{Ina}} = 0.35$; $t_{(1)} = 2.45$; $p < 0.05$ for sum correct A, and $\beta_{\text{Ina}} = 0.54$; $t_{(1)} = 3.88$; $p < 0.001$ for sum correct B) and the Iw ($\beta_{\text{Iw}} = -0.30$; $t_{(1)} = -2.86$; $p < 0.01$ for sum correct A, and $\beta_{\text{Iw}} = -0.34$; $t_{(1)} = -3.17$; $p < 0.01$ for sum correct B) coefficients obtaining high levels of significance. The other codes did not obtain significance when all predictor variables were included in Step 2, except for the Rw coefficient in the sum correct A model, which was marginally significant ($\beta_{\text{Rw}} = -0.18$; $t_{(1)} = -1.69$; $p < 0.10$). Overall model fit was highly significant as well, with both models explaining a significant proportion of the variance in participants' success scores (adjusted $R^2 = 0.25$; $p < 0.001$ for the three-task dependent variable, and adjusted $R^2 = 0.42$; $p < 0.001$ for the five-task dependent variable).

Importantly, the hierarchical regressions further reveal how, for both sum correct models, the Interaction predictors contribute independently and significantly, increasing the adjusted R^2 values almost three-fold (Δ adjusted $R^2 = 0.16$; $p < 0.001$ for the three-task dependent variable, and Δ adjusted $R^2 = 0.27$; $p < 0.001$ for the five-task dependent variable) beyond that of the respective Step 1 models, which include all other substantive code categories (i.e. R, A, and O). Finally, the Step 1 models also indicate that when the Interaction codes are not taken into account, other code categories—most notably, Ona—sometimes reach significance ($\beta_{\text{Ona}} = 0.24$; $t_{(1)} = 1.88$; $p < 0.10$ for sum correct A, $\beta_{\text{Ona}} = 0.35$; $t_{(1)} = 2.76$; $p < 0.01$ for sum correct B, and $\beta_{\text{Aw}} = -0.19$; $t_{(1)} = -1.73$; $p < 0.01$ for sum correct B).

DISCUSSION

This paper offers a unique empirical investigation into the systematic focusing errors of competitive decision makers in psychologically defining the game (Bazerman, Curhan, & Moore, 2000). Prior decision research on negotiation has focused on faulty use of a specified set of data. We suggest that taking into account the data in the mental models of competitive decision makers may provide a new source of insight into common errors in competitive contexts. Our current research developed a coding system that tested and confirmed our hypotheses that: (1) humans tend to exclude a full consideration of the decisions of others and the rules of the game from the decision-maker's focus of attention in competitive contexts; and (2) this failure is tied to sub-optimal decisions made in the competitive context. In addition, we have provided evidence of the generalizability of this focusing framework by showing parallel results across three different games, which until now have been viewed separately in the literature.

From a pedagogical perspective, this work suggests that negotiation courses should move beyond training in how to play a defined game, and provide insight into the definition of the game

(Brandenburger & Nalebuff, 1996). In addition, such training should not be limited to structuring the negotiation. Rather, we should help negotiators to focus on the full set of variables that allow them to maximize their effectiveness.

At a more basic research level, this paper supports work by Schkade and Kahneman (1998) and Wilson and colleagues (Gilbert & Wilson, 2000; Wilson et al., 2000), arguing that much of what goes wrong in human judgment has to do with how we define the decision context, rather than simply with how we use the information that is in our cognitive representation. While the topics of mental models (Gentner & Stevens, 1983) and scripts and schemes are not new (Fiske & Taylor, 1984), the current paper is consistent with recent efforts that show such mental representations can predict systematic and predictable judgmental errors (Medin & Bazerman, 1999). In the past, 'decision errors' were the domain of decision researchers; cognitive and social-cognitive psychologists shied away from the label 'error.' Yet we believe that this label allows us to use descriptive knowledge to help people make decisions that will maximize their desired results.

A central finding of our work is that the codes for the interaction between the parties and the rules of the game were the most important predictors of success. Admittedly, this was a surprise to us. We have expected main effects for the independent variables of the rules of the game and decisions of the other parties, but it was the interaction that dominated. It is useful to compare this finding to the protocol results of the Acquiring a Company problem reported by Carroll et al. (1988), which showed failure to be well predicted based on whether study participants considered the decisions of the other party. Carroll et al. (1987) did not code for the rules of the game or the interaction between the rules of the game and the decisions of the other party. The coding category ('generalized hypothetical') they found predictive of success, however, would belong squarely within our 'Interaction—normative analysis' category. By separating out codes for the other party from the three-way interaction, our results emphasize that many decision failures occur not because decision makers ignore a specific category of information, but because they fail to think through how that category of information logically connects with other variables.

Successful and unsuccessful decision makers may therefore differ, at times, not in *how much* they think about the different aspects of the competitive situation, but in *how* they think about the decisions of others and the rules of the game. Our findings also raise the intriguing possibility that a further study of the decision-making *process* in competitive settings may reveal systematic characteristics that distinguish successful decision makers. We made a first step in this direction by revealing, contrary to our own expectations, the dominating importance of the interaction codes in success and failure alike. Our coding scheme, however, was designed only to identify the *components* of correct and incorrect competitive decision making within our novel framework. More studies will be required to examine whether successful decision makers also approach competitive problems, or any of their components, differently from unsuccessful ones.

Additionally, following Stanovich and West (1998), the low intertask correlations in our data may suggest that success and failure in these tasks result more from participants' systematic computing of non-normative rules or their adoption of mistaken task construal than from their cognitive abilities. Stanovich and West (1998) found this to be the case for some other well-studied decision problems, but not for many rational thinking problems. Such an interpretation would support the argument that focusing failures lead to the erroneous definition of competitive games.

Our empirical investigation focused on the three simplest decision problems that we could identify as potential examples of the predicted focusing failure—Monty Hall, Acquiring a Company, and multiparty ultimatums. We nevertheless believe our framework has far greater explanatory power. For example, Ho, Camerer, and Weigelt (1998) study a game where each player chooses a number from 0 to 100. The winning entry is the number closest to half of the mean of the entries. Totally ignoring the decisions of others and nuances of the rules of the game, 50 emerges as a naïve yet common submission. Ho, Camerer, and Weigelt (1998) note that even the simplest logic should lead people to think that if the average were 50, a better submission would be 25—but this logic requires attention to the rules of the game. Notice, however, that when you consider the decisions of other players, it becomes clear that others will follow this same logic; thus, the mean might be 25, so you should submit 12.5. But, if others figure out *this* logic, you should submit 6.25, and so on, down to 0—the equilibrium solution. While the winning answer is typically greater than 0, the interesting observation from our standpoint is the prevalence of simple numbers such as 50 and 25, which derive from not fully considering the rules of the game and the thoughts of other players.

In an experimental study of market entry, Camerer and Lovallo (1999) found that participants were insensitive to the quality of their competition. Labeling this phenomenon 'reference group neglect,' they essentially found that participants ignore how the quality of competitors interacts with the rules of the competition to affect the wisdom of entering their experimental market. Their result, like the results of the current paper, is that the focusing failure leads to decisions with negative expected values. In a related paper, Massey and Wu (2001) show that study participants exhibit 'system neglect,' undervaluing the importance of the more general context in which they are making their decision.

Moore (2000) provides fascinating evidence of judgmental failure in the context of negotiation deadlines. He creates a very simple negotiation between a buyer and seller in which, if no agreement is reached, both parties get 0 payoff. He then gives one of the parties a publicly known deadline, which intuitively puts that party at a disadvantage. Notice, however, that if one party has a deadline, so does the other! While the deadline affected the two parties symmetrically, negotiators falsely believed that a deadline put them at an asymmetric disadvantage.

Moore proceeded with another experiment, in which one party incurs time-related costs, while the other does not. This, of course, gives the party without time-related costs an advantage. He then offers the party with time-related costs the opportunity to impose a firm deadline on the negotiations, eliminating their own time-related costs and creating symmetric costs for the failure to reach agreement. Most negotiators passed on this option, despite the strategic benefit it would create. Again, most negotiators failed to think through how the rules of the game would affect the other party, and suboptimized as a result.

Moore (2001) explains his results, and those of many of the other studies that we have reviewed, in terms of the failure to think through contingencies. Expanding on and modifying this conceptualization, we have considered the contingencies we believe to be most common in competitive environments—the decisions of others and the rules of the game. In addition, we believe that focusing failures exist even when no contingencies exist. For example, one of the most interesting examples of a focusing failure is the lack of concern exhibited by citizens of the United States of campaign-finance reform as a means of curbing the political influence of special-interest groups (Bazerman, Baron, & Shonk, 2001). If you ask citizens whether they support and care about this issue, the majority says 'yes.' However, when asked to rank campaign-finance reform against other issues (such as taxes or education spending), they give it a very low rank. Why? Bazerman, Baron, and Shonk (2001) speculate that voters undervalue campaign-finance reform because of a limited focus. That is, the only reason that people should care about such reform is that it would affect virtually every other issue (and its effects could be enormous). But people do not tend to think through this process; instead, they value issues that are more clearly seen as end states or outcomes, rather than a using broader focus that would direct attention towards a set of important outcomes (Bazerman, Baron, & Shonk, 2001).

If our conceptualization of failures in competitive contexts is accurate, it may be possible to train participants on one or more competitive tasks and expect them to show improvement on a seemingly unrelated task. Our post-hoc analysis suggests that such attempts might include methods designed to make decision makers more likely to avoid their natural Actor-only focus. Of course, debiasing has a long and difficult history (Fischhoff, 1982; Hastie & Dawes, 2001). However, recent evidence suggests that developing conceptual understanding through analogical reasoning has some debiasing potential (Thompson, Loewenstein, & Gentner, 2000). Optimistically, successful transference across tasks would provide powerful support for the generalizability of the claims that: (1) individuals fail to fully consider the decisions of others and the rules of the game; and (2) this failure leads to suboptimal decisions.

APPENDIX: ACQUIRING A COMPANY

In this exercise you represent Company A (the Acquirer), which is currently considering Acquiring Company T (the Target) by means of a tender offer. You plan to tender in cash for 100% of Company T's shares but are unsure how high a price to offer. The main complication is this: the value of Company T depends directly on the outcome of a major oil exploration project it is currently undertaking. Indeed, the very viability of Company T depends on the exploration's outcome. If the project fails, the company under current management will be worth nothing ($0 per share). But if the project succeeds, the value of the company under current management could be as high as $100 per share. All share values between $0 and $100 are considered equally likely. By all estimates, the company will be worth considerably more in the hands of Company A than under current management. In fact, whatever the ultimate value under current management, the company will be worth 50% more under the management of A than under Company T. If the project fails, the company is worth $0 per share under either management. If the exploration project generates a $50 per share value under current management, the value under Company A is $75 per share. Similarly, a $100 per share value under Company T implies a $150 per share value under Company A, and so on.

The board of directors of Company A has asked you to determine the price they should offer for Company T's shares. This offer must be made now, before the outcome of the drilling project is known. From all indications, Company T would be happy to be acquired by Company A, provided the price is profitable. Moreover, Company T wishes to avoid, at all costs, the potential of a takeover bid by any other firm. You expect Company T to delay a decision on your bid until the results of the project are in, then accept or reject your offer before the news of the drilling results reaches the press. Thus, you (Company A) will not know the results of the exploration project when submitting your price offer, but Company T will know the results when deciding whether or not to accept your offer. In addition, Company T is expected to accept any offer by Company A that is greater than the (per share) value of the company under current management.

As the representative of Company A, you are deliberating over price offers ranging from $0 per share (this is tantamount to making no offer at all) to $150 per share. What price offer per share would you tender for Company T's stock?

My tender price is $______ per share

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