Barking Up the Right Tree: Are Small Groups Rational Agents?

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Abstract

Both mainstream economics and its critics have focused on models of *individual* rational agents. But most important decisions are made by small groups such as families, management teams, boards of directors, central bank boards, juries, appellate courts, and committees of various types. Little systematic work has been done to study the behavior of small groups as decision-making agents in markets and other strategic games. This may limit the relevance of both economics and its critics to the objective of developing an understanding of how most important decisions are made. In order to gain some insight into this issue, this paper compares group and individual economic behavior. The objective of the research is to learn whether there are systematic differences between decisions made by groups and individual agents in market environments characterized by risky outcomes. A quantitative measure of deviation from minimally-rational decisions is used to compare group and individual behavior in common value auctions.

1. Introduction

A central feature of mainstream twentieth century economics is its reliance on models of individual rational decision-making. Criticism of economics usually focuses on conclusions about empirical failure of rational agent models. Much of this criticism comes from cognitive psychology or from economists with backgrounds in cognitive psychology or behavioral science. Indeed, critical discourse on rational choice models is often framed as a contest between economic theory and the falsifying evidence from cognitive psychology. An example that illustrates this point is provided by Hogarth and Reder (1987), the published record of a conference on economics and psychology.

While they differ in their conclusions about economics, and on how to conduct experiments yielding insights into the usefulness of economic theory, both Hogarth and Reder and their critic, Smith (1991) agree that the proper designation of "decision-maker" is an individual person. Thus, Hogarth and Reder (1987, p. 3) write as follows:

> It is important to note that the rational choice paradigm refers to individuals … In most applied work, the paradigm is … about the conjectured behavior of a hypothetical "representative" individual … To add credibility to the story, appeal is often made to everyday intuition concerning individual behavior.

In his critical review, Smith (1991, p. 878) writes as follows:

> I believe that the basic problem stems … from two unstated premises on which there is implicit agreement between psychology and mainstream theory: (1) rationality in the economy emanates and derives from the rationality of individual decision makers in the economy, and (2) individual rationality is a cognitively intensive, calculating process of maximization in the self-interest. A third, shared tenet, which is a correlate of points 1 and 2, is that (3) an acceptable and fundamental way to test economic theory is to test directly the economic rationality of individuals isolated from interactive *experience* in economic institutions.

Smith argues that "presumptions" (1) – (3) above are false, and presents a cogent development of the rationale for using market experiments to study the usefulness of rational agent theory in economics. But he implicitly agrees with Hogarth and Reder that the decision-making entities in experiments should be individuals: note that presumptions (1) – (3) are stated for individuals and that none of the experiments Smith reviews involves groups as decision-making agents.

Thus a problem shared by economics, its critics, and its defenders is that they all appear to be "barking up the wrong tree" in focusing on the use of, or criticisms of, models of *individual* rational agents.[1] This statement follows from observing that most important economic, political, legal, scientific, cultural, and military decisions are made by groups. Decision-making groups have many forms including families, management teams, boards of directors, central bank boards, juries, appellate courts, committees of various types, and legislatures. Decision-making responsibility may be assigned to groups, rather than individuals, because of a belief that (a) important pieces of information are possessed by different individual members of groups and/or (b) groups are inherently more rational than individual decision-makers. Whether groups make better decisions than individuals in some or all environments and, if they do, whether this reflects an advantage from having more information available, are empirical questions.

Some insights from psychology may indeed be important to economics. But a case can be made that it is the subject matter of social psychology, rather than cognitive psychology, that is important to the economics of decision-making because most important decisions are made by groups.

Many researchers in psychology and management science have previously studied group decision-making. Our research involves three important departures from previous work in that we conduct the experiments in an environment in which (a) groups compete in strategic market games, (b) the distinct information possessed by individual group members is varied as an experimental treatment, and (c) the extent to which a group's decisions depart from rationality is quantitatively measured.

We compare group and individual decision-making in the context of bidding in common value auctions. Auction market bids are commonly decided by groups. For example, oil companies typically use committees of geologists and managers to formulate bidding strategies (Capen, Clapp & Campbell, 1971; Hoffman, Marsden, & Saidi, 1991). Many general contractors bring several people together to build a bid package (Dyer and Kagel, 1996). In this strategic decision-making environment, are there grounds for expecting groups to perform any better (or perhaps worse) than individuals?

In a common value auction, the value of the auctioned item is the same to all bidders but the bidders do not know that value at the time they make their bids. If all N bidders in the market have unbiased estimates of the item's value, and use the same monotonically increasing bidding strategy, then the high bidder will be the one with the most optimistic estimate. But the highest of N unbiased estimates is biased upwards and, if bidders do not take into account this property of order statistics, then winning bidders can on average pay more for auctioned items than they turn out to be worth. This phenomenon is known as the "winner's curse" (Capen, Clapp & Campbell, 1971).

The winner's curse cannot occur if all bidders are rational (Cox and Isaac, 1984, 1986), and therefore evidence of the winner's curse in market settings is considered by Thaler (1988, 1992) to be an anomaly. However, Thaler also recognizes that acting rationally in a common value auction is not an easy task. Fully rational bidders must be able to distinguish between the expected value of the auctioned item, conditioned only on the prior information available (their value estimates), and the expected value of the item conditioned on winning the auction. Moreover, even if bidders grasp this basic distinction, the winner's curse can still occur if they do not adjust sufficiently to compensate for the presence in the market of the total number of other bidders.

Authors of numerous papers on experiments with common value auctions (e.g., Thiel, 1975; Bazerman and Samuelson, 1983a,b; Kagel and Levin, 1986; Kagel, Levin, Battalio, and Meyer, 1989; Lind and Plott, 1991; Cox, Dinkin, and Swarthout, 2001) have reported that bidders often make systematic errors and, as a result, suffer from the winner's curse. But all of these previous experiments have involved bids decided by individuals. The implications of this research for group bidding behavior are unknown.

One reason why firms and other organizations bring several people together to decide on a bid may be their conjecture that group members have different information available about the auctioned item or, if the information is common to all group members, that it may be open to individual interpretations. Alternatively, firms may have bids decided on by groups because of a belief that groups are more rational than individuals in the strategic environment of the common value auction. If groups have more information than individuals, and groups utilize their information no less rationally than do individuals, then it would obviously follow that group performance would be better than individual performance in auctions. But the actual relative performance of groups and individuals in common value auctions is an empirical question that we address with our experiments.

We report results from experiments designed to provide comparisons of individual and group bidding behavior. If group bidding differs from individual bidding, this could result from groups having more information or, alternatively, better or worse judgment. This distinction is examined by crossing an information density (signal sample size) treatment with the bidding entity (groups or individuals) treatment in the experimental design.

2. Previous Research on Group vs. Individual Decisions

Kerr, MacCoun, and Kramer (1996) reviewed the social psychology literature on consensus decisions of small, task-oriented groups in an attempt to find out whether such groups are less (or more) subject to judgmental errors than individuals. Their conclusion was that there is no simple and general answer to this question. Rather, the experimental evidence indicates that group decisions can attenuate, amplify, or simply reproduce the judgmental biases of individuals. Kerr, et al. (1996) suggested that these inconsistent findings can be explained by considering the nature of the judgmental problem under study. In certain kinds of tasks, groups can be expected to do better than individuals whereas in other tasks groups are expected to do worse.

Classification of tasks starts with a basic distinction between intellective tasks and judgmental tasks (Laughlin, 1980; Laughlin & Ellis, 1986; McGrath, 1984). A task is defined as intellective if there is a clear (i.e., normative) criterion for evaluating the quality of performance, and as judgmental if no such criterion exists. Intellective tasks are further divided on the basis of their "demonstrability," which is the degree to which the knowledge of the normative principle pertaining to the task is shared by group members and the degree to which this principle, once voiced, is accepted by them as valid (Gigone and Hastie, 1993; Laughlin & Ellis, 1986).

Highly demonstrable tasks are those where the argument prescribed by the normative model is self-evidently correct. Such tasks are sometimes called "eureka" problems. For these tasks, all it takes for a group to choose the correct solution is for there to be a single individual who advocates this solution (Laughlin, et al., 1976; Laughlin & Ellis, 1986; Davis, 1992). This social decision scheme, referred to by Steiner (1972) as "truth wins," defines a clear baseline for group performance (Lorge and Solomon, 1955). If an individual problem-solver is sampled with probability p (and a non-solver with probability 1-p), the probability of a correct group solution is $P=1-(1-p)^K$, with *K* being the number of group members. When the conditions for demonstrability are not fully met, advocates of the correct solution may require some social support to prevail. This decision rule is referred to as "truth-supported wins" by Laughlin & Earley (1982).

The distinctive feature of the group decision schemes associated with demonstrable tasks is said to be their asymmetry: factions favoring the correct alternative are supposed to be more likely to prevail than comparable factions favoring an incorrect alternative. This leads to two testable predictions: (1) groups will outperform individuals; and (2) the superiority of group performance will become more pronounced as group size increases. Several review papers (Davis, 1992; Hastie, 1986; Hill, 1987; Laughlin & Ellis, 1986; McGrath, 1984) have concluded that, on average, groups do outperform individuals on demonstrable tasks, although they typically fall short of the Lorge and Solomon (1955) baseline.

However, as stressed by Kerr, et al. (1996), the "truth" is likely to win only when there is a reasonably clear and widely shared consensus among group members about what is and is not correct. When the principles of rational choice do not appear applicable, or do not appear compelling (that is, when these principles are not demonstrably correct), there is no reason to expect group members to favor the normatively correct alternative over incorrect ones. A similar argument was made by Shafir, Simonson, & Tversky (1993, p.34), who asserted that "...the axioms of rational choice act as compelling arguments, or reasons, for making a particular decision when their applicability has been detected, not as universal laws that constrain people's choices."

When the truth has no special standing, the decision problem can be considered, for all practical purposes, as a judgmental task. The most likely decision scheme for arriving at a group decision in such tasks may be some kind of a majority or plurality rule (Davis, 1992). The result is said to be a polarization of the initial individual propensity as the popular response becomes more popular and the unpopular response less popular. Thus, good (i.e., unbiased, normative) individual performance is said to result in even better group performance, whereas poor individual performance is said to lead to even poorer performance at the group level. The larger the group the stronger is said to be the polarization effect of group decision-making (Isenberg, 1986).

The effect of the group decision rule on group polarization has been demonstrated on a wide domain of judgmental tasks. Much of this research involved risk-taking behavior. In the early sixties social psychologists concluded that groups adopt more risky decisions on average than individuals responding privately (Stoner, 1961; Wallach, Kogan, & Bem, 1962). This phenomenon was initially called the "risky shift." However, when subsequent research (e.g., Nordhoy, 1962) revealed that groups can also shift toward greater caution, the individual vs. group difference was more generally labeled as "choice shift."

Similarly to Kerr, et al., Davis (1992) described the distribution of group decisions as essentially an exaggeration of the individual distribution resulting from the decision rule adopted by the group. Davis theorized that, depending upon the skewness of the initial individual distribution, a majority or plurality rule is sufficient to produce a "shift" in group choice. He showed that, applying such rules to randomly-composed k-person groups from the original individual distribution, produced distributions of group choice that were a close description of the observed distributions.

The social psychological research summarized above focused primarily on group and individual decisions in non-strategic settings. Little systematic research has been done to compare group with individual performance in strategic games. There are, however, some exceptions that are relevant to the current investigation (Bornstein, 1992; Bornstein, et al., 1996). A recent study by Bornstein and Yaniv (1998) compared the behavior of individuals with that of three-person groups in the context of the ultimatum game. Group members had an opportunity for a short face-to-face discussion in order to decide, as a collective, on a proposed division or on whether to accept or reject a proposal. The results showed that groups behave more "rationally" than individuals in the sense that they offer less and are also willing to accept less. Bornstein and Yaniv (1998) suggested that one explanation for this result was that groups had a better understanding of the strategic structure of the ultimatum game and, in particular, of the strategic advantage associated with the allocator's position. Specifically, they raised the possibility that the sub-game-perfect equilibrium argument ("player 2 should accept any positive offer since anything is better than nothing"), when voiced, had a decisive effect on group decisions.

Although the group behavior experiments cited in the preceding paragraph do involve strategic games, they do not involve market institutions. Hence this body of work is not responsive to Smith's (1991) argument for the importance of studying market behavior in order to assess the applicability of models of rational behavior in economics.

3. Rational Bidding in Common Value Auctions

There may be various reasons why groups have responsibility for making decisions, not the least of which is the possibility that each individual will bring some distinct information to the process. We shall develop a more formal representation of the presumed advantage to bidding groups if they do have more information about the value of the auctioned item than do individual bidders. But first consider the information environment commonly used in previous research on common value auctions with individual bidders.

3.1 Implications of an Individual Estimate of Value

Consider an auction market where the bidders do not know the value of the item being sold when they submit their bids and the value, v of the item is the same for all bidders. Consider a first-price sealed-bid auction in which the high bidder wins and pays the amount of its bid for the auctioned item. Further envision that each bidder receives an independent signal, s_i that provides an unbiased estimate of the object's true value. The expected value of the auctioned item conditional on the bidder's signal is denoted by $E(v \mid s_i)$. The expected value of the auctioned item conditional on the bidder's own signal being the highest of N signals (i.e., equal to the highest order statistic, y_N) is denoted by $E(v \mid s_i = y_N)$. For bids by $N > 1$ bidding entities, one has

(1) $$E(v \mid s_i) = s_i > E(v \mid s_i = y_N)$$

by well-known properties of order statistics. Thus if bidders naively submit bids equal to their common value estimates, s_i they will have an expected loss from winning the auction; that is, they will suffer from the winner's curse.

Consider the case where the common value of the auctioned item is uniformly distributed on the interval, $[v_l, v_h]$ and each individual agent's signal is independently drawn from the uniform distribution on $[v-\theta, v+\theta]$. For this case, one has

(2) $$E(v \mid s_i = y_N) - E(v \mid s_i) = -\frac{N-1}{N+1}\theta,$$

for all $s_i \in [v_\ell + \theta, v_h - \theta]$. Thus, if bidders naively submit bids equal to their signals then the cursed winning bidder will have an expected loss in the amount $\theta(N-1)/(N+1)$. This expected loss is increasing in the number of bidders, N.

3.2 The Presumed Advantage from More Information

Now assume that *each member* of each group has a signal that is independently drawn from the uniform distribution on $[v-\theta, v+\theta]$. Under these conditions, groups of size $G > 1$ have a signal sample size of $G > 1$ on which to base their estimates of the common value. Because the signals are drawn from a uniform distribution, the signal sample midrange, m_i provides an unbiased estimate of the value of the auctioned item. The expected value of the auctioned item conditional on the bidder's signal sample midrange is denoted by $E(v \mid m_i)$. The expected value of the auctioned item conditional on the bidder's own signal sample midrange being the highest of N signal sample midranges (i.e., equal to the highest order statistic of sample midranges, z_N) depends on the sample's range, r_i and is denoted by $E(v \mid r_i, m_i = z_N)$. With signals drawn from the uniform distribution, one has

(3) $$E(v \mid r_i, m_i = z_N) - E(v \mid m_i) = -\frac{N-1}{N+1}(\theta - \tfrac{1}{2}r) .$$

Comparison of statements (2) and (3) reveals that groups with size $G > 1$ signal samples will have a smaller expected loss from naively bidding their signal sample midranges, than will individuals from naively bidding their signals, except in the improbable extreme outcome in which all of the signals in the sample with the highest midrange have the same value (and, hence $r_i = 0$). In the other improbable extreme outcome, in which the signal sample with the highest midrange has a range equal to 2θ, there will be zero expected loss from bidding an amount equal to the sample midrange. But, of course, in this case the bidder knows the auctioned item's value with certainty.

Note that equation (3) suggests a quantitative criterion for determining the extent of deviation from minimally-rational bidding in common value auctions. The magnitude of the winner's curse that is exhibited by winning bidder *i*, in a market with *N* bidders, is

(4) $$EVCurse = b_i^w - E(v \mid r_i^w, m_i^w = z_N)$$

where b_i^w is the winning bid, *v* is the common value of the auctioned item, r_i^w is the winning bidder's signal sample range, m_i^w is the winning bidder's signal sample midrange (or signal, for signal sample size 1), and z_N is the *Nth* order statistic of sample midranges (or signals, for signal sample size 1). Note that *EVCurse* is the magnitude of the expected loss (or profit, if it is negative) from winning the auction.

In order not to have an expected loss from winning, a bidding group or individual must discount it's naive estimate of the common value (its signal or its signal sample midrange) by at least the amount $(\theta - \frac{1}{2}r)(N-1)/(N+1)$, where it is understood that r = 0 for signal samples of size 1. Furthermore, the size of this minimum rational discount is independent of m_i so long as $m_i \in [v_\ell + \theta, v_h - \theta]$. These conditions are essentially always satisfied by the signals drawn in our experiments in which $\theta = 1800$ and $[v_\ell, v_h] = [2500, 22500]$. Therefore, deviations from minimally-rational bidding can be measured by linear regressions relating winning bids to the signals or signal sample ranges and midranges of the winning bidders, as we do in section 7. Bids that yield zero expected profit are given by the following equation when $m_i \in [v_\ell + \theta, v_h - \theta]$:

(5) $$b^{Zero} = -\frac{N-1}{N+1}\theta + m_i + \frac{N-1}{2(N+1)} r_i .$$

4. Experimental Design: Signals, Bidders, and Markets

The design of our experiments addresses the effects of: (a) group vs. individual decision-making; and (b) varying sample size for the information signal about the common value of the auctioned item. We are motivated by the view that in order to really look at the effect of group decision-making, one has to hold the information constant but vary the decision-making unit, or hold the decision-making unit constant but vary the information. This design accomplishes both of these things and, in addition, varies the size of the market from 3 (group or individual) bidders to 7 bidders. The groups consist of 5 individual subjects who become once- and twice-experienced.

4.1 Rationale for Varying the Signal Sample Size

The reason for varying the signal sample size is that one can argue for conditions under which all individual members of a bidding group would have the same value estimate and other conditions under which each individual would have a distinct estimate of the value of the auctioned item. Consider a standard example of a common value auction, the market in Outer Continental Shelf oil and gas leases. Some of the bids in this auction are submitted by individual companies, while many are submitted by groups of companies. Bids submitted by individual companies are typically prepared by groups of (geologist and management) employees. We acknowledge that in some instances these groups are hierarchical, i.e., recommendations are made to a single decision-maker. However, at this time we explore peer groups only. In either or both of these situations, do the individual members of a group (of individuals or companies) have the same or distinct estimates of the value of a lease? One could argue that the individual employees of one company might all have the same estimate of the value of an auctioned item. In contrast, it is difficult to argue that the employees of different companies have the same value estimate; even if the companies have the same seismic (and other) data, it is unlikely that their staffs interpret that data identically. In any case, full experimental exploration of the differences between individual and group bids requires that one separate the effects of the composition of the bidding unit *per se* from the effects of signal sample size. We "cross" the bidding unit composition treatment with the signal sample size treatment.

4.2 Rationale for Varying the Market Size

We also "cross" a market size treatment (3 or 7 bidding units) with the bidding unit composition and signal sample size treatments. The reason for varying the market size in this way is that previous researchers have found that once- or twice-experienced *individual* bidders in markets of size 3, where each individual has a signal sample size of 1, do *not* suffer the winner's curse whereas in markets of size 7 individual bidders with the same experience and signal information do suffer from the winner's curse so badly that most of them go bankrupt. We address the following questions with our experimental design: (1) Do group bidders fare the same or differently than individual bidders in the 3-bidder and 7-bidder market sizes? (2) Does the answer to question (1) depend on whether the group bidders have the same or larger signal sample sizes than the individual bidders? (3) Do group and/or individual bidders with larger signal sample sizes fare the same or differently than they do with a signal sample size of 1 in the 3-bidder or 7-bidder market sizes?

4.3 Rationale for Using Groups Consisting of Five Subjects

The research literature has often questioned whether a 3 person group is large enough to elicit true "group" behavior. In order to avoid potential criticisms that we did not use large enough groups in our experiments, we need to use groups of at least size 4. Since some of our groups might try to make decisions by voting on proposals, we want to avoid a group size that is an even number so to always admit the possibility of a decisive majority vote. Recently, Gjolberg and Nordhaug (1996) proposed some theory to guide group size selection for investment committees. They recommended odd-sized committees no larger than 20. We use groups of size 5.

4.4 Rationale for Using Experienced Subjects

Reports of results from previous common value auction experiments with individual bidders have focused on the behavior of experienced subjects, where "experience" means having participated in one or more previous common value auction experiments. The reason for this is that most subjects fall victim to the winner's curse in *all* experimental treatments when they are first-time bidders but such inexperienced behavior is not considered to be very interesting. We use subject experience as a treatment to allow comparison of our results with those in the literature.

4.5 Information and Repetition

Both individuals and groups will have opportunities to learn from experience about the implications of not discounting their signal sample estimates of the common value. This will occur both within experiments and between experiments at different subject experience levels. Within-experiment learning can occur because experiments consist of at least 30 rounds of bidding with information feedback. After each round of bidding is concluded, all subjects are informed of the amount of the winning bid and the common value of the auctioned item, and therefore about the profit or loss realized by the high bidder.

4.6 Summary of the Experimental Design

As we have explained, we use a 2 *X* 2 *X* 2 design, with the cells shown in Figure 1.

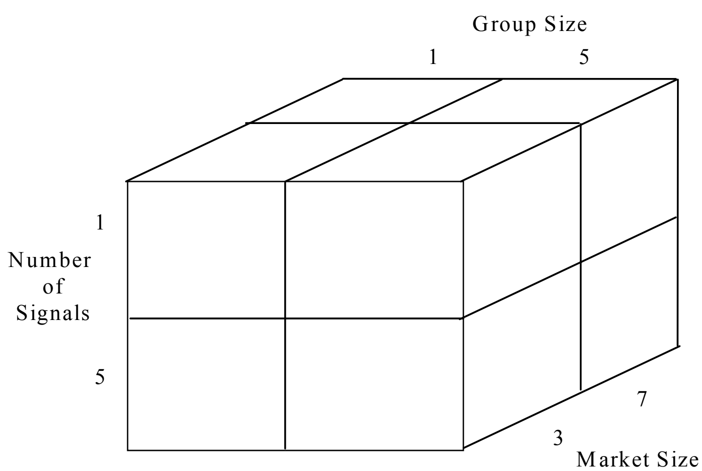

Figure 1: Experimental Design

5. Experimental Procedures

Experiments with individual bidders were conducted in the Economic Science Laboratory (ESL). Experiments with group bidders were conducted in the breakout rooms of the Decision Behavior Laboratory (DBL). ESL and DBL are adjacent laboratories in McClelland Hall at the University of

Arizona. Each group or individual had its own personal computer that was connected to a local area network running customized auction software. Subjects were recruited from the undergraduate student population.

Experiments were run in three-day sequences of two-hour blocks. The subjects were informed when they were recruited that they would be paid a $20 participation fee if they participated in all three days of the experiment. They were also informed that they would be paid all of their positive earnings from three-days' bidding in the auctions at the end of the experiment on the third day. They were further informed that anyone who failed to participate in all three days' experiments would forfeit both the participation fee and all of their positive earnings. The subjects were reminded of these procedures immediately before the experiment began and asked whether the procedures were acceptable to them. If anyone had indicated that the procedures were not acceptable, they would have been paid $5 immediately and invited to sign up for alternative one-day experiments. It turned out that none of the subjects requested this option. These payoff procedures were adopted in order to give the subjects a strong incentive to return on the second and third days of the experimental sequence. This was considered important because subject experience is known to be an important treatment in common value auction experiments and because many bidders do not make any money from bidding in the auctions on day one.

Subjects were allowed to self-select into groups as they moved into the DBL. This was intended to increase group cohesiveness and perhaps introduce even more incentive (from group loyalty) to return for the entire experiment. The subjects read instructions on their computer monitors that described bidding procedures in the first-price sealed-bid auctions. The instructions contained a detailed description of the information environment of the common value auctions. Thus, subjects were informed in non-technical terms that in each auction round the computer would draw a value for the auctioned item from the discrete uniform distribution on the integers greater than or equal to 2,500 experimental dollars and less than or equal to 22,500 experimental dollars. They were informed that the common value would not be revealed but that it would be the midpoint of a uniform distribution from which their value estimates, or signals, would be independently drawn. They were informed in non-technical terms that, after the computer drew a common value, v, for a round, it would draw all signals independently from the uniform distribution on [v-1800, v+1800]. Information about how the signals would be drawn was presented to the subjects both on their computer monitors and orally by the experimenters. The oral presentation used the analogy with bidding on oil leases and interpreted the signal(s) as estimates of the value of an oil lease by geologist(s). Neither the computerized instructions nor oral instructions contained any discussion of the order statistic property that is conventionally thought to underlie the winner's curse. These instructions contained only non-technical explanations of how the common values and subjects' signals were generated and the rules of the first-price sealed-bid auction.

Experiments were run in three-day sequences. On day 1, the inexperienced subjects first participated in 10 periods of practice auctions. After each practice auction, the subjects' computer monitors displayed the common value, all subjects' bids, and the amount won or lost by the high bidder. The subjects were each given a capital endowment of 1,000 experimental dollars in order to allow them to make at least one sizable overbid without becoming bankrupt. At the end of the practice rounds, the subjects' profits and losses were set to zero and they began the 30 monetary-payoff rounds with new 1,000 experimental dollar capital endowments. The actual number of monetary-payoff rounds to be completed was not announced. During the monetary-payoff rounds the information reported at the end of each auction included only the common value and the high bid, not the bids by other bidders. We decided not to report all bids in order to make collusion more difficult and to adopt procedures that correspond to minimal reporting requirements in non-laboratory auctions.

The procedures were the same on day 2 as on day 1. Procedures were the same on day 3 except that only the common value and high bid (rather than all bids) were reported in the practice rounds. This procedure was adopted during the first group bidding experiment after one of the experimenters overheard a discussion outside the laboratory between members of different bidding groups about plans to use the practice rounds to implement a collusive bidding scheme. Although the performance of such schemes would be an interesting research question, it was not the focus of the present research. On the second day, subjects also completed a short survey instrument to allow us to determine demographics, group network centrality and cohesion, among other constructs. These data will not be reported here.

Signals were presented to the subjects on sheets of paper. In experiments with signal sample size of 1, each subject was given a single sheet of paper with signals for 10 practice rounds and 40 monetary-payoff rounds. Thus, in the group-bidding experiments with signal sample size of 1, each member of a group had a sheet of paper with signals that were identical to the signals of other members of the same group but distinct from the signals of members of other groups. In the experiments with signal sample size of 5: (a) each subject in an individual bidder experiment had 5 sheets of paper with independently-drawn signals; and (b) each subject in a group bidding experiment had 1 sheet of paper with independently-drawn signals. Since the groups each had 5 members, each group in an experiment with signal sample size 5 had access to the same information as an individual subject in the individual-bidder experiments with the same signal sample size. The groups were informed that they were free to use their signal sample sheets within their own breakout rooms during an experiment in any way that they wanted to.

Signals, common values, and bids were denominated in experimental dollars, with a clearly specified exchange rate into U.S. dollars, for two reasons. The less important reason was that this made it easy to require that all bids be integers, thus avoiding the bid entry errors produced by subjects' having to use decimal points in their responses. The more important reason was to eliminate extraneous sources of variation across treatments. We held constant the experimental-dollar capital endowments, sequence of common values, and (the relevant) signals across the market size, signal sample size, and group/individual bidder experimental treatments. Thus the only source of variability across treatments came from the behavioral responses to the treatments. Without use of a variable exchange rate from experimental dollars into U.S. dollars, the monetary incentives would have been different for individual subjects in the individual-bidder and group-bidder experiments and the (Nash equilibrium) expected profits of bidders would have varied inversely with the square of the market size.

The U.S. dollar incentives in these experiments were non-trivial. Consider for example, a group bidder experiment with market size 7 and signal sample size 1 (treatment (5, 1, 7) in section 6 and Tables 2 and 3). The exchange rate was 9 experimental dollars per 1 U.S. dollar. If in some auction round all bidders were to have bid naively and submitted bids equal to their signals, then the group with the high bid would have had an expected loss of 150 U.S. dollars from just this one "winning" bid.

Conducting these experiments posed a substantial logistical problem. In order to have enough subjects for at least two three-day series of experiments in each of the cells in Figure 1, more than 300 subjects had to be recruited. Furthermore, these subjects had to have schedules that made it possible for them to participate on three different days within one week and, of course, they had to be willing to do this. Given the large effort required to recruit the subjects, we attempted to avoid ever turning away subjects who showed up at the laboratory. This was especially a potential problem because of the nature of group experiments. If 35 subjects are needed to run a market size 7 experiment with groups of 5 bidders each, and only 34 subjects show up, then the experimenter had better "have a plan B" if he is to avoid turning away 34 subjects, paying them each a show-up fee, and obtaining no data that week. Our procedure was to always schedule simultaneous group-bidder experiments in the Decision Behavior Laboratory (DBL) and individual-bidder experiments in the Economic Science Laboratory (ESL) on the first day of a three-day experiment. Furthermore, three experiments were almost always set up and ready to start individually or simultaneously on the ESL local area network. Then if fewer subjects showed up than needed to run a scheduled group experiment in DBL, we used them in individual experiments in ESL. If more than the enough subjects showed up to run a scheduled group experiment, we used the excess in individual experiments. Since the smallest market size was 3, we avoided ever having to turn away more than 2 subjects. One result of this procedure is that we ran more experiments in some of our treatment cells than in others. This is not a problem for the type of data analysis that we use. Of course, this procedure was used only on the first day of an experimental week; once subjects began participating in a group experiment they continued with the same group on the following days.

Several bidders made data entry errors during the experiments. There were two types of such errors. One type was when bidders entered bids thinking that they were in a different market period. The other type of error consisted of typing errors such as excluding the last digit or including an extra digit in a bid. Subjects who made these errors often immediately brought them to the experimenters' attention. Such errors were usually obvious because they produced bids that were too low or too high by a multiple of 10. We forgave losses resulting from data entry errors in subjects' payoffs when they immediately came to our attention. Bidders were grateful for this and we believe that they continued to behave as before the error. Observations with data entry errors are excluded from our data analysis.

Many inexperienced individual and group bidding entities made winning bids that turned out to be so high that they attained large negative cumulative payoffs. A non-trivial number of once- and twice-experienced subjects also incurred negative balances. When a bidder's cumulative payoffs were negative at the end of an experiment the loss was forgiven (the bidder was permitted to "go bankrupt"). Allowing bidders to continue bidding after they have attained a negative cumulative balance can be a problem because the experimenter can lose control of their incentives. But dismissing negative-balance subjects from further participation was not an option in our experiments because: (a) we needed to keep market size constant, and therefore could not simply dismiss negative balance subjects; and (b) experience was a treatment, and hence we could not substitute new subjects for bankrupt ones during an experiment. We handled this problem in the data analysis reported in section 6 by deleting observations containing bids made by bidders after they had attained a negative cumulative balance.

6. Data Analysis

The salient monetary rewards earned by subjects on day 2 (once-experienced) and day 3 (twice-experienced) are reported in Tables 1 and Table 2, as are the number of bankrupt bidding entities and the experimental/U.S. dollar exchange rates used in the experiments. Earnings from the day 1 experiments, together with the \$20 participation fees and the earnings reported in Tables 1 and 2, were all paid after the end of the day 3 experiment. There were large differences between the lowest and highest earnings in all treatments. Only non-bankrupt bidder averages are shown in the second columns of Tables 1 and 2. As explained in section 5, our experimental design involves manipulation of the exchange rate so as to keep the monetary incentives *per individual* approximately the same, regardless of market or group size. Table 1 reveals one unplanned deviation from this design feature in treatment (5, 5, 3). As it turned out, the data analysis reported below in Tables 3 and 4 reveals essentially no difference between the winning bid, signal regressions for the (5, 5, 3) experiments run with the two different exchange rates.

Inspection of bid and signal data from the experiments indicates that both individuals and groups are very subject to the winner's curse when they are inexperienced. The more interesting data are for once- and twice-experienced subjects. Tables 3 and 4 report results from analyzing data from experiments with once- and twice-experienced subjects. The reported results are from random effects regressions with estimating equations of the form,

(6) $$b_{jt} = \alpha + \beta m_{jt} + \gamma r_{jt} + \mu_j + \varepsilon_{jt},$$

where b_{jt} is the bid by bidder *j* in period *t*, m_{jt} is bidder *j*'s signal sample midrange in period *t*, and r_{jt} is bidder *j*'s signal sample range in period *t*. In experiments with a signal sample size of 1, m_{jt} is the bidder's signal and r_{jt} equals zero. The estimated coefficients will be compared those in the zero-expected-profit bid function reported in equation (5). The estimation uses winning bids (market prices) and the associated right-hand variables.

The first column of Table 3 or 4 reports the experimental treatment: (G, S, N) denotes the size of the bidding "Group" (1 for individuals or 5 for five-person groups), the "Signal" sample size (1 or 5), and the "Number" of bidders (3 or 7). The second column reports the "minimum rational discount," which is the intercept in the zero-expected-profit bid function. The third, fourth, and fifth columns report the estimated parameters and their standard errors (in parentheses). The sixth column reports the R^2's. The right-most column of Table 3 or 4 reports the Chi-squared statistics for the Wald joint test on significance of the estimated parameters and their *p*-values [in brackets].

6.1 Comparison of the Performance of Individuals and Groups with Once-Experienced Subjects

Table 3 reports the random effects regression results for once-experienced subjects. First note that for every treatment the R^2 is very high and the *p*-value for the Wald test is 0.00. The linear regression equation explains almost all of the variability in the winning bids. The first two rows report the central comparison for market size 3 (*i.e.* N = 3): experiments using individual bidders (*i.e.* G = 1) and signal sample size 1 (*i.e.* S = 1) are compared with experiments using group bidders (*i.e.* G = 5) and signal sample size 5 (*i.e.* S = 5). Comparison of the estimated intercepts with the minimum rational discount and the estimated coefficients on slopes with a slope of 1 provides a measure of the departure from rational bidding by the high bidders in an experiment. The intercept for treatment (1, 1, 3) is –1,048, which is obviously not greater than the minimum rational discount of –900, and the slope is 1.005, which is not significantly different from 1.000. In contrast, the intercept for treatment (5, 5, 3) is –527, which is significantly greater than the minimum rational discount of –900 by a one-tailed *t*-test at the 5% confidence level, and the slope is 0.994, which is not significantly different from 1.000 by a two-tailed *t*-test at the 5% confidence level. Therefore, groups with more information than individuals are less rational than the individuals in this comparison of bidding in markets of size 3.

The third and fourth rows of Table 3 compare the performance of winning bidders in individual and group bidding treatments with a market size of 7. The intercept for individual bidders with signal sample size of 1 (treatment (1, 1, 7)) is –1,281, which is not significantly greater than the minimum rational discount of –1,350. The slope for treatment (1, 1, 7) is 1.014, which is not significantly different from 1.000. The intercept for group bidders with signal sample size of 5 (treatment 5, 5, 7)) is 1,026, which is significantly greater than the minimum rational discount of –1,350. The slope is 0.990 for treatment (5, 5, 7), which is not significantly different from 1.000. Therefore, groups with more information than individuals are also less rational than the individuals in this comparison of bidding in markets of size 7.

The last four rows of Table 3 report the treatments that "cross" the design so as to make it possible to be able to separate the effects of group or individual composition of the bidding entity from the effects of signal sample size. Consider treatment (1, 5, 3). The intercept for individual bidders with signal sample size of 5 in markets of size 3 is –1,174, which is obviously not greater than the minimum rational discount of –900. The slope of 0.999 for treatment (1, 5, 3) is not significantly different from 1.000. The intercept for groups with signal sample size 1 in markets of size 3 (treatment (5, 1, 3)) is –708, which is not significantly greater than the minimum rational discount of –900 and the slope of 0.984 is not significantly different from 1.000. Thus, in these treatments neither individuals with more information nor groups with less information deviate significantly from rational bidding.

The last two rows of Table 3 report the market-size-7 results for individuals with signal sample size of 5 and groups with signal sample size of 1. The intercept for the individual bidders (treatment (1, 5, 7)) is –105, which is significantly greater than the minimum rational discount of –1,350, but the slope of 0.981 is not significantly different from 1.000. In contrast, the intercept for the groups (treatment (5, 1, 7)) is –1,069, which is not significantly greater than the minimum rational discount of –1,350, and the slope of 0.997 is not significantly different from 1.000. Thus, in this comparison individuals with more information are less rational than groups with less information in markets of size 7.

Comparison of the first and second rows, respectively, with the sixth and fifth rows holds constant the signal sample size but varies the composition of the bidding entity in markets of size 3. Observe that individuals with signal sample size of 1 (treatment (1, 1, 3)) are not significantly more rational than groups with the same signal sample size (treatment (5, 1, 3)). In contrast, groups with signal sample size of 5 (treatment (5, 5, 3)) are significantly less rational than individuals with the same signal sample size in markets of size 3 (treatment (1, 5, 3)).

Comparison of the third and fourth rows, respectively, with the eighth and seventh rows holds constant the signal sample size but varies the composition of the bidding entity in markets of size 7. Observe that individuals with signal sample size of 1 (treatment (1, 1, 7)) are not significantly more rational than groups with the same signal sample size (treatment (5, 1, 7)). Observe that individuals with signal sample size of 5 (treatment (1, 5, 7)) are not significantly more rational than groups with the same signal sample size (treatment (5, 5, 7)). In fact, the individual-bidder intercept of –105 in treatment (1,5,7) is much too high, and the signal-sample-range coefficient, $\hat{\gamma}$ is significantly different from its theoretical value (0.375).

6.2 Comparison of the Performance of Individuals and Groups with Twice-Experienced Subjects

Table 4 reports the regression results for twice-experienced subjects. First note that for every treatment the R^2 is very high and the *p*-value for the Wald test is 0.00. The first two rows report the central comparison for market size 3: treatments with individual bidders and signal sample size 1 are compared with treatments with group bidders and signal sample size 5. The slope for treatment (1, 1, 3) is 0.990 which, with the very small standard error (.0005), is significantly different from 1.000. The intercept for treatment (1,1,3) is –859, which is not significantly different from the minimum rational discount, but the –360 intercept for treatment (5,5,3) is significantly greater than –-900. Therefore, in markets of size 3, twice-experienced groups with more information than individuals appear to be less rational than the twice-experienced individuals.

The third and fourth rows of Table 4 compare the performance of winning bidders in individual and group bidding treatments with a market size of 7. The intercept for individual bidders is –1,059, which is not significantly greater than the minimum rational discount of -1,350. In contrast, the intercept for the group bidders is 196, which is significantly greater than the minimum rational discount. The 0.956 slope for treatment (5, 5, 7) is significantly different from 1.000 by a *t*-test. Therefore, groups with more information than individuals are also less rational than the individuals in this comparison of bidding in markets of size 7. Furthermore, the group-bidder slope of 196 in treatment (5,5,7) is much too high, and the signal-sample-range coefficient, $\hat{\gamma}$ is not only significantly different from its theoretical value (.375) but has the wrong sign.

The last four rows of Table 4 report the other, "design-crossing" treatments. Consider treatment (1, 5, 3). The intercept for individual bidders with signal sample size of 5 in markets of size 3 is –1,161, which is obviously not higher than –900. The intercept for groups with signal sample size 1 in markets of size 3 (treatment (5, 1, 3)) is –806, which is not significantly greater than the minimum rational discount of –900, but the slope of 0.981 is significantly different from 1.000. Thus, in this comparison groups with less information are not more rational than individuals with more information.

The last two rows of Table 4 report the market-size-7 results for individuals with signal sample size of 5 and groups with signal sample size of 1. There is a significant departure from rational bidding by the individuals with signal sample size of 5 but not by the groups with single signals.

Comparison of the first and second rows, respectively, with the sixth and fifth rows holds constant the signal sample size but varies the composition of the bidding entity in markets of size 3. Observe that individuals with signal sample size of 1 (treatment (1, 1, 3)) are not more rational than groups with the same signal sample size (treatment (5, 1, 3)). With signal sample size of 5, individuals (treatment (1,5,3)) are more rational than groups (treatment (5,5,3)).

Comparison of the third and fourth rows, respectively, with the eighth and seventh rows holds constant the signal sample size but varies the composition of the bidding entity in markets of size 7. Observe that individuals with signal sample size of 1 (treatment (1, 1, 7)) are *not* more rational than groups with the same signal sample size (treatment (5, 1, 7)). The results for individuals and groups with signal sample size 5 in markets of size 7 (treatments (1, 5, 7) and (5, 5, 7)) show significant departures from rational bidding for both individuals and groups.

7. Summary and Conclusions

Are groups more or less rational than individuals in common value auctions? Our research suggests that the answer depends upon the defining characteristics of groups. Assume that groups are characterized as decision-making entities consisting of more than one individual with distinct information. Then comparison of results from treatments involving groups, with signal sample size of 5, with treatments involving individuals, with signal sample size of 1, supports the conclusion that groups are less rational than individuals. On the other hand, if we assume that groups consist of individuals that have common information then the answer changes. Comparison of results from treatments involving groups, with signal sample size of 1, with treatments involving individuals, with signal sample size of 1, does *not* support a conclusion that groups are less or more rational than individuals.

We think that it is a surprising feature of our results that *more* information about the value of the auctioned item causes both individuals and groups to deviate *further* from rational bidding and that this "curse of information" is worse for groups than for individuals. What accounts for deterioration in bidding performance when bidders have more information about the value of the auctioned item? Why is more information especially a problem for groups?

Our experiment does not support some analyses that have appeared in the previous literature. For example, Hoffman, et al. (1991) explained the prevalence of joint bids for offshore oil leases as *not* resulting from anti-competitive efforts to reduce competition in the auction but, instead, as a result of firms' efforts to avoid the winner's curse because "…with several signals to compare, firms can better identify and filter out exceptionally high signals" (p. 103). Our experimental results contradict this analysis in that we find that our groups' performance deteriorates significantly when they have "several signals to compare." Authors of several review papers have concluded that groups outperform individuals on demonstrable intellective tasks (Davis, 1992; Hastie, 1986; Hill, 1987; Laughlin & Ellis, 1986; McGrath, 1984). Formulating a strategy for bidding so as to avoid the winner's curse in common value auctions is clearly an intellective task that is demonstrable by an economist: see section 3 above. But is this a task that is demonstrable by experiment subjects? Group bidders in our experiment did not outperform individuals when both had signal sample sizes of 1 but the majority of both types of bidding entities performed quite well (when once- or twice-experienced). Both individual and group performance deteriorated when they received more information and, furthermore, group performance was worse than individual performance. Does possession of more information make rational bidding strategies less demonstrable?

Endnotes

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1. It is important to point out in this context that all of the first author's previous papers involving rational agent models have used individuals as decision-makers.

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Table 1. Salient Earnings for Once-Experienced Subjects

G, S, N[a]	Exch. Rate	Average[b]	Low	High
1, 1, 3	200	$ 5.91	$ 0	$ 13.04
5, 5, 3	400 (80)	$ 3.99 ($19.97)	$ 0 ($ 0)	$ 8.95 ($44.74)
1, 1, 7	43	$ 24.67	$ 0	$ 53.60
5, 5, 7	45 (9)	$ 21.88 ($109.42)	$ 0 ($ 0)	$ 32.42 ($162.11)
1, 5, 3	200	$ 10.79	$ 0	$ 15.13
5, 1, 3	200 (40)	$ 15.95 ($73.83)	$ 0 ($ 0)	$ 19.61 ($98.07)
1, 5, 7	43	$ 19.20	$ 0	$ 43.19
5, 1, 7	45 (9)	$ 29.47 ($147.36)	$ 0 ($ 0)	$ 44.02 ($220.11)

a. G, S, N = Group size, Signal sample size, Number of bidders.
b. Figures in parentheses are earnings for the whole group.

Table 2. Salient Earnings for Twice-Experienced Subjects

G, S, N[a]	Exch. Rate	Average[b]	Low	High
1, 1, 3	200	$ 17.64	$ 0	$ 38.33
5, 5, 3	150 (30)	$ 17.09 ($85.43)	$ 0 ($ 0)	$ 29.26 ($146.33)
1, 1, 7	43	$ 32.72	$ 0	$ 54.67
5, 5, 7	45 (9)	$ 23.24 ($116.18)	$ 0 ($ 0)	$ 39.66 ($198.33)
1, 5, 3	200	$ 17.08	$ 0	$ 28.70
5, 1, 3	200 (40)	$ 25.28 ($126.40)	$ 0 ($ 0)	$ 39.58 ($197.90)
1, 5, 7	43	$ 24.91	$ 0	$ 58.47
5, 1, 7	45 (9)	$ 46.94 ($234.71)	$ 0 ($ 0)	$ 123.63 ($618.44)

a. G, S, N = Group size, Signal sample size, Number of bidders.
b. Figures in parentheses are earnings for the whole group.

Table 3. Random Effects Regressions for Once-Experienced Subjects
(standard errors) [*p*-values]

G,S,N[a]	Rnl. Disc.[b]	$\hat{\alpha}$	$\hat{\beta}$	$\hat{\gamma}$	R^2	Wald Test
1,1,3	-900	-1,048 (111)	1.005 (0.006)		.999	33,250 [0.00]
5,5,3	-900	-527* (145)	0.994 (0.006)	0.154 (0.051)	0.998	33,241 [0.00]
1,1,7	-1,350	-1,281 (213)	1.014 (0.013)		0.991	6,565 [0.00]
5,5,7	-1,350	-1,026* (133)	0.990 (0.006)	0.359 (0.046)	0.997	35,073 [0.00]
1,5,3	-900	-1,174 (221)	0.999 (0.009)	0.305 (0.069)	0.993	11,884 [0.00]
5,1,3	-900	-708 (228)	0.984 (0.013)		0.988	5,567 [0.00]
1,5,7	-1,350	-105* (278)	0.981 (0.010)	0.206# (0.083)	0.984	8,863 [0.00]
5,1,7	-1,350	-1,069 (329)	0.997 (0.020)		0.985	2,404 [0.00]

a. G, S, N = Group size, Signal sample size, Number of bidders.
b. Rnl. Disc. = minimum rational discount
* Denotes significantly greater than the minimum rational discount by a one-tailed 5% *t*-test.
# Significantly different than the theoretical value by a two-tailed 5% *t*-test.

Table 4. Random Effects Regressions for Twice-Experienced Subjects
(standard errors) [*p*-values]

G,S,N	Rnl. Disc.	$\hat{\alpha}$	$\hat{\beta}$	$\hat{\gamma}$	R^2	Wald Test
1,1,3	-900	-859	0.990#		.995	34,988
		(199)	(0.005)			[0.00]
5,5,3	-900	-360*	0.998	0.095	0.993	13,493
		(285)	(0.009)	(0.081)		[0.00]
1,1,7	-1,350	-1,059	0.995		0.989	5,018
		(289)	(0.014)			[0.00]
5,5,7	-1,350	196*	1.014	-0.158#	0.997	17,055
		(220)	(0.008)	(0.080)		[0.00]
1,5,3	-900	-1,161	0.996	0.351	0.998	48,134
		(198)	(0.005)	(0.056)		[0.00]
5,1,3	-900	-806	0.981#		0.996	22,402
		(147)	(0.007)			[0.00]
1,5,7	-1,350	-649*	0.995	0.196#	0.996	23,722
		(169)	(0.007)	(0.057)		[0.00]
5,1,7	-1,350	-1,331	1.007		0.993	21,035
		(188)	(0.007)			[0.00]

a. G, S, N = Group size, Signal sample size, Number of bidders.
b. Rnl. Disc. = minimum rational discount
* Denotes significantly greater than the minimum rational discount by a one-tailed 5% *t*-test.
# Significantly different than the theoretical value by a two-tailed 5% *t*-test.