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Endogenous Entry and Exit in Common Value Auctions

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Abstract

We develop and experimentally test a model of endogenous entry, exit, and bidding in common value auctions. The model and experimental design include an alternative profitable activity (a "safe haven") that provides agent-specific opportunity costs of bidding in the auction. Each agent chooses whether to accept the safe haven income or forgo it in order to bid in the auction. Agents that enter the auction receive independently-drawn private signals that provide unbiased estimates of the common value. The auctioned item is allocated to the high bidder at a price that is equal to the high bid. Thus the market is a first-price sealed-bid common value auction with endogenous determination of market size.

Keywords: auctions, bidding theory, market equilibrium, experimental economics

JEL Classification: C72, C92, D44, D82

1. Introduction

We develop and experimentally test a model of endogenous entry/exit and bidding in common value auctions. A common value auction is one in which the unknown (random) value of the auctioned item is the same for all bidders. Endogenizing market entry and exit captures the phenomenon that *not* every potential buyer attends every auction. The model and the experimental design include an alternative profitable activity that provides buyer-specific opportunity costs of bidding in the common value auction. Making the opportunity costs buyer specific incorporates the concept that market entry and exit decisions depend on individual economic circumstances. The model that we develop is an *equilibrium* model in two senses. It provides Bayesian-Nash equilibrium strategies for auction market entry/exit and bidding. It also determines the equilibrium size of the auction market resulting from opportunity costs of entry.

The model and the experimental design have the following features. The number of agents is public information. Agents independently draw their low risk alternative (or "safe haven") incomes from a known distribution. These safe haven incomes are private information. Each agent chooses whether to accept the safe haven income or to forego it in order to bid in the auction. The number of agents that choose to bid in the auction is public information. Each agent that chooses to bid in the auction receives an independently-drawn private signal that provides an unbiased estimate of the unknown common value. The auctioned item is allocated to the high bidder at a price that is equal to the high bid; thus the market is a first-price sealed-bid common-value auction. The testable implications of the model include predictions of which agents will enter the auction, and how much they will bid, as functions of variables that represent the agents' opportunity costs of bidding and their information about the uncertain value of the auctioned item.

Our extension of common value auction experiments to include endogenous entry and exit is motivated by considerations that are external and internal to the literature on auctions. First, bidders in non-laboratory auctions have low-risk alternatives to bidding such as investing in Treasury bills. Such bidders' responses to the relative expected profits from bidding and their best alternatives endogenously determine the number of bidders in an auction. In contrast, most previous theoretical and experimental research on auctions has not endogenized the auction market entry decision. Secondly, previous experiments with common value auctions have found that bidders often incur losses from bidding and that many of them go bankrupt.[1] Bankrupt subjects, within a theoretical and experimental environment that assumes an exogenously-determined number of bidders, forces an experimenter to either: (a) dismiss them and decrease market size, in violation of the presumed environment; or (b) replace them artificially by a criterion external to the presumed environment; or (c) let then continue bidding under conditions where their marginal economic incentives are non-existent. The data, therefore, beg for a more general model, with *endogenous* entry and exit, within which common value auctions can be studied. Furthermore, economic theory teaches that any market is not isolated, and that the number of agents in any activity is endogenous as the agents self-select among alternative activities depending upon individual tastes and opportunity costs. Therefore, the extension of bidding theory and experiments to include endogenous determination of the number of bidders is a natural progression for research in this area.

Common value auctions are commonly believed to exhibit the "winner's curse," which can be explained as follows. Assume that bidders do not know the common value when they submit their bids but that each of the N bidders has an independent sample (or "signal") that provides an unbiased estimate of the common value. If all bidders use the same monotonic strategy, then the high bidder is the one with the highest estimate of value. But the highest of N unbiased estimates is biased upwards. If bidders do not allow for this "order-statistic property" in choosing their bids, then the high bidder may submit a bid that exceeds the expected value of the auctioned item *conditional* on his or her estimate being the highest of N estimates. For example, if each bidder were to submit a bid equal to his or her *unconditional* expected value for the auctioned item, then the high bidder would have an expected loss from "winning" the bidding competition. A winning bid that exceeds the expected value of the auctioned item, conditional on the winning bidder's value estimate being the highest value estimate, is one of the traditional concepts of the winner's curse. In the following pages, we shall refer to it as the expected-value (or EV) winner's curse.[2]

There has been much disagreement in the literature about whether the winner's curse characterizes bidding in the markets for oil leases, book publishing, baseball free agents, and corporate takeovers. But the field data are hard to interpret because the values of the auctioned items, and the information that the bidders had about those values at the time of the auction, are usually unclear even after the auction. Experimental research can contribute to our understanding of the winner's curse phenomenon because, in the laboratory, the actual values of the auctioned items and the information that the bidders have about those values are controlled by, and hence observable by, the experimenter. Thus there need be no ambiguity in the laboratory about whether bidders experience a winner's curse, and the research can focus on the conditions under which it occurs.

The present paper focuses on opportunity cost (entry) conditions under which experimental subjects suffer from the EV winner's curse and those under which they enjoy an EV "winner's blessing" in which a bid is less than the expected value of the auctioned item conditional on the bidder's own signal being the highest signal. Whenever there occurs an EV winner's blessing, as we have defined it, the bidder will have positive expected profit from bidding in the common value auction.

2. Endogenous determination of the number of bidders

The detailed bidding and price formation rules that characterize various types of auctions have led to the development of Bayesian-Nash equilibrium bidding models with rich sets of testable implications. These testable implications, for both individual bidding behavior and market allocations, have been the topics of numerous experimental investigations. Although this theoretical and experimental literature is quite extensive, for the most part it has *not* included complete models of endogenous determination of the number of bidders in a market. Thus, a typical equilibrium bid function would be written in the form,

$$b_i = b(x_i, N), \quad i = 1, 2, \ldots, N, \tag{1}$$

where b_i is agent i's bid, x_i is agent i's value signal, and N is the exogenously-determined number of bidders.[3]

The present paper reports the development and experimental tests of an equilibrium bidding model in which the number of bidders is *endogenously* determined. Thus, in place of statement (1), the new theory takes the form,

$$\begin{aligned} b_i &= b(x_i, n), \quad \textit{for } i \in B \\ B &= \{i = 1, 2, \ldots, m \mid a_i \leq a^*\} \\ a^* &= h(m, F(a), G(x_o, x_i)) \\ n &= \#B, \end{aligned} \tag{2}$$

where: b_i and x_i are defined as above; B is the set of agents that enter the auction (i.e., the set of bidders); m is the exogenously-determined number of agents (or the maximum possible number of bidders); a_i is the bidding opportunity cost of agent i that is independently drawn from the distribution with c.d.f., $F(a)$; a^* is the reservation opportunity cost; $G(x_o, x_i)$ is the c.d.f. of the joint distribution of common value, x_o and signal, x_i; and n is the actual number of bidders (the number of elements in the set B, $\#B$).

Endogenous determination of the number of bidders may enable them to overcome the winner's curse through an elementary ability to respond to comparative profit signals. However, since earlier auction experiments were not designed to let the number of bidders be endogenously determined, the implications of any behavioral differences for equilibrium market configurations is unknown. Our experimental design with an alternative safe haven activity will reveal whether EV winner's curse strategic errors have implications for equilibrium market configurations or can be interpreted as a disequilibrium phenomenon.

Including a "safe haven" alternative to bidding permits us to examine the following types of questions about entry, exit, and bidding in common value auctions:

A. Do bidders enter and exit the auction in response to varying opportunity costs?
B. Are the individuals with relatively low opportunity costs, who are predicted to be in the auction, actually observed to be in the auction?
C. Is the observed number of bidders in the auction larger or smaller than the predicted number of bidders?
D. With inclusion of a safe haven alternative to bidding, do we observe a significant incidence of winner's curse bidding in experiments? If not, does this depend upon the alternative providing a *positive* return?

The endogenous entry and exit extension of the economics of auction markets leads to various insights, including a more fundamental understanding of the winner's curse. In turn, applying the theory to the winner's curse makes clear the importance of making the number of bidders endogenous.

3. Bidding theory and experimental design

This section explores relations between the design of our experiments and Bayesian-Nash equilibrium theory. Bidding experiments with endogenous entry are conducted as follows. First, each subject $i = 1, 2, \dots, m$ draws an alternative (safe haven) income, a_i. Then each subject decides whether to participate in the bidding or retreat to the safe haven. The number of subjects who decide to participate in the bidding, n, is announced. Then individual bidders draw value signals and submit bids.

We introduce the safe haven into bidding theory by letting the subjects' alternative incomes, $a_i, i = 1, 2, \dots, m$, be independently drawn from the uniform distribution on $[0, \bar{a}]$. We now construct beliefs that support the symmetric risk neutral Bayesian-Nash equilibrium of entry/exit and bidding strategies. Let agent j believe that any rival agent i will enter the auction if agent i's alternative income, a_i is less than or equal to a^* and will choose the safe haven if $a_i > a^*$. Then agent j believes that the probability that $n - 1$ out of the $m - 1$

other agents will enter the auction is

$$
\begin{aligned}
P(n-1) &= {}_{m-1}C_{n-1}\left(\frac{a^*}{\bar{a}}\right)^{n-1}\left(1-\frac{a^*}{\bar{a}}\right)^{m-n} \\
&= \frac{(m-1)!}{(m-n)!(n-1)!}\left(\frac{a^*}{\bar{a}}\right)^{n-1}\left(1-\frac{a^*}{\bar{a}}\right)^{m-n} \\
&\qquad\qquad \text{for } n-1 = 0, 1, \ldots, m-1.
\end{aligned} \tag{3}
$$

Let $\pi(n)$ be the expected profit per bidder if there are n bidders in the auction (e.g., agent j and $n-1$ other bidders). Then the expected profit to agent j from entering the auction is

$$v_j(a^*) = \sum_{n=1}^{m} P(n-1)\pi(n). \tag{4}$$

Agent j will enter the auction if her alternative income, a_j is less than or equal to her expected profit from entering the auction, $v_j(a^*)$ and will choose the safe haven if $a_j > v_j(a^*)$. Note that a^* is the unique symmetric equilibrium critical realization from the safe haven if all m agents use it as a cutoff for making their entry and exit decisions; that is, a^* satisfies the symmetric equilibrium property if and only if[4]

$$v_j(a^*) = a^*, \quad j = 1, 2, \ldots, m. \tag{5}$$

Derivation of other properties of the model requires specification of the bidders' information about the value of the auctioned item. Let n subjects communicate that they have decided to forego their safe haven alternative incomes in order to bid in the auction. The computer then draws (but does not announce) the common value, x_0 from the uniform distribution on $[\underline{x}, \bar{x}]$. Then the signals, x_i, $i = 1, \ldots, n$, are independently drawn from the uniform distribution on $[x_0 - \theta, x_0 + \theta]$ and each signal is announced privately to each of the n bidders. If $n = 1$ then the dominant strategy is to bid the minimum amount that would not be rejected by the seller. In our experiments, the minimum acceptable bid was 1, which was less than $\underline{x}$; hence one part of the equilibrium bid function, $b(x_i, n)$ is $b(x_i, 1) = 1$, for all x_i. The equilibrium bid function for two or more risk neutral bidders in a common value auction like ours is contained in Kagel and Levin (1986). Putting the parts together yields the risk neutral Nash equilibrium (RNNE) bid function for our experiments:

$$
b(x_i, n) = \begin{cases}
1, & \text{for } n = 1,\ \underline{x} - \theta \le x_i \le \bar{x} + \theta \\
\underline{x} + (x_i - \underline{x} + \theta)/(n+1), & \text{for } n > 1,\ x_i < \underline{x} + \theta \\
x_i - \theta + Z_i, & \text{for } n > 1,\ \underline{x} + \theta \le x_i \le \bar{x} - \theta \\
\text{no closed form}, & \text{for } n > 1,\ x_i > \bar{x} - \theta,
\end{cases} \tag{6}
$$

where

$$Z_i = 2\theta \exp[-(n/2\theta)(x_i - \underline{x} - \theta)]/(n+1). \tag{7}$$

If all of the n bidders submit bids according to statements (6) and (7), then the expected profit of the high bidder would be $-1 + (\underline{x} + \bar{x})/2$, for $n = 1$, and it would be approximately $2\theta/(n + 1)$, for $n > 1$.[5] The probability that any one of the n bidders will receive the highest signal, and hence (with all bidders using (6) and (7)) submit the highest bid, is $1/n$. Therefore, the expected profit per bidder in the common value auction is approximately

$$\pi(n) = \begin{cases} -1 + (\bar{x} + \underline{x})/2, & \text{for } n = 1 \\ 2\theta/n(n+1), & \text{for } n > 1. \end{cases} \tag{8}$$

The (real number) solutions of Eqs. (3)–(8) provide symmetric equilibrium entry/exit and bid functions for the common value auction with private information conditions.

The set B of agents that are predicted to enter the auction, conditional on a realization of their m alternative incomes, is

$$B = \{i = 1, 2, \ldots, m \mid a_i \leq a^*\}. \tag{9}$$

Hence the number of bidders that are predicted to enter the auction, conditional on their alternative incomes, is the number of elements of the set B:

$$n = \#B. \tag{10}$$

The (unconditional) expected number of bidders in the auction is

$$\begin{aligned} E(n) &= \sum_{n=1}^{m} {}_mC_n \left(\frac{a^*}{\bar{a}}\right)^n \left(1 - \frac{a^*}{\bar{a}}\right)^{m-n} n \\ &= \sum_{n=1}^{m} \frac{m!}{(m-n)!(n)!} \left(\frac{a^*}{\bar{a}}\right)^n \left(1 - \frac{a^*}{\bar{a}}\right)^{m-n} n = m\frac{a^*}{\bar{a}}. \end{aligned} \tag{11}$$

4. Experimental procedures

Our experiments were conducted in the Economic Science Laboratory at the University of Arizona. The subjects were undergraduate students recruited from economics and business classes. They were paid \$3–10 upon signing in and then each was randomly assigned to a computer terminal. When all subjects were ready, the experimenter instructed them to enter their names, social security numbers, and telephone numbers into their computers and to begin reading the instructions on their computer screens.

Most experiments were run in 3-2-1 sets as portrayed in figure 1. First, 3 experiments with 3 separate groups of inexperienced subjects were run, all with the same design parameters. Second, the same design parameters were run with 2 separate groups of subjects who had acquired experience in one of the inexperienced sessions. Finally, 1 experiment with the same design parameters was run with subjects who had participated in both inexperienced and once-experienced sessions. The subjects were regrouped in the second and third sessions so that no subject would ever bid against the exact same set of rival bidders in more than one

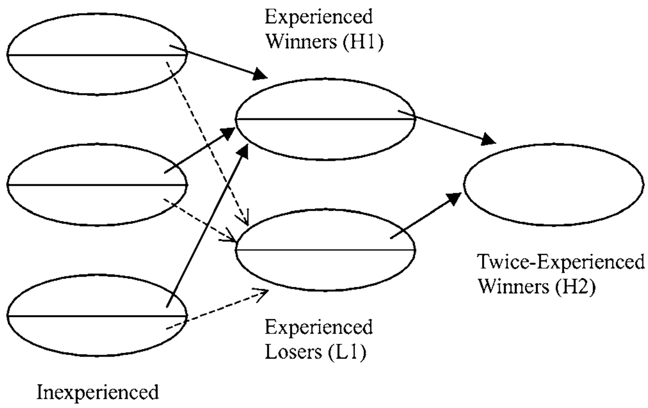

Figure 1. Flow of subjects between experience levels.

session. The criterion for regrouping was relative profit performance in the immediately-preceding session. Thus one group of experienced subjects consisted of those who had made the most money in the first session (the "experienced winners"); these subjects correspond to the winner's curse "survivors" in the economy. The other group was a control group that consisted of those who had made the least money in the first session (the "experienced losers"). The 1 experiment in the third session was run with subjects who had made the most money in the second session (the "twice-experienced winners"). When subjects participated in the inexperienced and once-experienced sessions, they did not know that there would be later experiments of the same type. Each experimental session included 30 periods of bidding for monetary payoffs; these are the data-recording periods.

At the end of an experiment, each subject was called individually to the door of the laboratory control room to be paid in private. At that time, each subject was asked whether he or she would be willing to return to participate in another experiment of the same type. The next subject was called to be paid, and asked about willingness to return, after the preceding subject had exited from the laboratory. Subjects that indicated a willingness to return were told they would be contacted later by telephone if their participation was needed in a subsequent experiment. The subjects were never informed of the (profit) criterion that was used in regrouping for a subsequent session. All experiments with the same experience-level used the same seeds for the pseudo-random number generators, thus eliminating a possible non-behavioral cause of variability across subject groups.[6] The seeds were changed between experience-levels in order to eliminate any possibility that subjects might remember some sequences of pseudo-random numbers. There were some variations of this basic 3-2-1 design that will be explained below.

Each experiment included 30 auction periods with the same design parameters. Monetary amounts were displayed to the subjects in "experimental dollars" that had an announced exchange rate into U.S. dollars. The support for the uniform probability distribution of common values (in experimental dollars) was $[\underline{x}, \bar{x}] = [2500, 22500]$. The width of the support for private signals was $2\theta = 3600$ experimental dollars. Other design parameters that varied across experiments are presented in Table 1. Those parameter values were chosen

Table 1. Design parameters.

Market size (No. of subjects)	Positive-income safe haven	$E(n)$, predicted average number of bidders	$\bar{a}$, highest possible haven income in E$	Exchange rate	W.C. exp. loss in US$
6	Yes	4.2	280	360	$3.08
6	No	6	0	360	$3.57
8	Yes	5.1	200	200	$6.05
8	No	8	0	200	$7.00
12	Yes	8	77	81	$17.28
12	No	12	0	59	$25.81

as follows. The highest possible haven income, $\bar{a}$, was chosen such that the *unconditional* average number of bidders, $E(n)$, was 4.2 in a 6-bidder experiment, 5.1 in an 8-bidder experiment, and 8.0 in a 12-bidder experiment.

Experiments lasted one to two hours. The exchange rates were chosen such that the theoretically-predicted average profit per subject in an experiment from bidding and choosing the safe haven was $21. This $21 figure does not include the sign-in payment or the initial capital balance. The implications of the exchange rates for salient incentives can be described by measuring the expected cost to a subject in U.S dollars of committing the winner's curse, as explained in the following two paragraphs.

We measure the magnitude of the EV winner's curse by the difference between the amount of the bid, b_i and the expected value of the auctioned item conditional on the bidder's own signal, x_i being the highest signal (i.e., equal to the realization of the highest order statistic, y_n). Thus, the measure is

$$EVCurse = b_i - E(x_0 \mid x_i = y_n). \tag{12}$$

When the *EVCurse* measure is positive, the bid exhibits the winner's curse. When the *EVCurse* measure is negative, the bidder has an expected positive profit from bidding, and we say that the bid exhibits a "winner's blessing." For the experimental design with x_0 distributed uniformly on $[\underline{x}, \bar{x}]$ and signals distributed uniformly on $[x_0 - \theta, x_0 + \theta]$, one has

$$E(x_0 \mid x_i = y_n) = \begin{cases} \underline{x} + A, & \text{for } x_i < \underline{x} + \theta \\ B, & \text{for } \underline{x} + \theta \le x_i \le \bar{x} - \theta \\ B/D - C^n[\bar{x} + 2\theta C/(n+1)]/D, & \text{for } x_i > \bar{x} - \theta \end{cases} \tag{13}$$

where:

$$A = (x_i - \underline{x} + \theta)/(n+1); \tag{14}$$

$$B = x_i - (n-1)\theta/(n+1); \tag{15}$$

$$C = (x_i - \bar{x} + \theta)/2\theta; \tag{16}$$

$$D = [(2\theta)^n - (x_i - \bar{x} + \theta)^n]/(2\theta)^n. \tag{17}$$

Suppose that a naïve subject ignores the order statistic property and submits a bid equal to his signal, which is the unconditional value of the auctioned item:

$$b_i = x_i = E(x_0 \mid x_i). \tag{18}$$

In the event that this subject's bid is the winning bid and his signal is the highest signal, his expected loss equals the difference between the unconditional and conditional expected values of the auctioned item, $E(x_0 \mid x_i) - E(x_0 \mid x_i = y_n)$. For 82% of the possible signals, those in $[\underline{x} + \theta, \bar{x} - \theta]$, the subject's loss is given by Eqs. (13), (15), and (18) as

$$E(x_0 \mid x_i) - E(x_0 \mid x_i = y_n) = (n-1)\theta/(n+1). \tag{19}$$

Equation (19), together with the experimental design parameter, $\theta = 1,800$ experimental dollars and the exchange rates in the fifth column of Table 1, imply the U.S. dollar (or US $) losses in the sixth column of Table 1. Thus the monetary incentives for the subjects to avoid the winner's curse are significant in these experiments. As reported in the sixth column of Table 1, the expected loss from a single naïve winning bid varies from $3.08 to $25.81.

During an experiment, a subject's computer screen displayed graphs of the prior probability distribution of the common value and the Bayesian-updated (on the subject's signal) posterior distribution of the common value for each auction period.[7] The information in these graphs had been explained, in non-technical language, in the instructions. After bids were submitted in a data-reporting period, the following information was displayed on each subject's computer screen: (a) the common value; (b) the amount of the winning bid; and (c) the amount of the subject's bid.

We conducted a total of 66 experiments that included four procedural treatments crossed with the positive-income safe haven, market size, experience, and high- and low-income treatments. The treatments are summarized in Table 2. The 66 experiments consisted of: 39 experiments with inexperienced (INX) subjects; 9 experiments with once-experienced, relatively low income (L1) subjects; 9 experiments with once-experienced, relatively high

Table 2. Treatments.

Environment	Pos. income safe haven	Market size	Initial capital balance	Subject experience (Number of experiments)	Bankrupts dismissed
Baseline	yes	8	1,000	INX(6), L1(2), H1(2), H2(2)	no
Baseline	no	8	1,000	INX(4), H1(1)	no
Baseline	yes	12	1,000	INX(3), L1(1), H1(1), H2(1), H3(1)	no
Smaller group experience	yes	6, 6 then 12	1,000	INX(5), L1(1), H1(2), H2(1)	no
Liability	yes	8	$2,000 \times 30$	INX(3), L1(1), H1(1), H2(1)	NA
Liability	no	8	$2,000 \times 30$	INX(3), L1(1), H1(1), H2(1)	NA
Specialist	yes	6 then 12, 12	1,500	INX(7), L1(1), H1(1), H2(1)	yes
Specialist	no	6 then 12, 12	1,500	INX(8), L1(1), H1(1), H2(1)	yes

income (H1) subjects; 8 experiments with twice-experienced, relatively high income (H2) subjects; and 1 experiment with thrice-experienced, relatively high income (H3) subjects. The four procedural treatments had the following features.

A. *Baseline treatment*

In the baseline experiments, each subject was given an initial capital balance of 1,000 experimental dollars to cover possible losses from bidding in the auction. As explained below, the capital balance was 1,500 in the specialist treatment and 2,000 *per period* in the liability treatment. In the baseline experiments, we let negative balance ("bankrupt") subjects continue bidding as part of the process of allowing them to obtain experience. As explained below, in the specialist treatment bankrupt subjects were dismissed from further bidding (and not replaced). Beyond the baseline experiments, we expanded our study to include factors that we expected would reduce or increase the incidence of the winner's curse. The treatments we introduced that we expected would reduce the winner's curse focused on subject experience and limited liability. We also expanded the population of bidders from 8 to 12 to provide more demanding environments.

B. *Smaller group experience*

We hypothesized that the relative value of experience in the different market size experiments could be responsible for differences in the EV winner's curse. That is, there could be a lower incidence of learning through losses on winning bids in the larger group inexperienced experiments than in the smaller group ones. We explored this question with our "small group experience treatment" by running a series of experiments in which the subjects first gained experience in two 30-period experiments with 6 subjects, after which two such groups were combined to participate in a 12-subject experiment.

C. *Limited liability: Create deep pockets*

In our baseline experiments, a subject could accumulate a negative balance of sufficient magnitude to make it impossible to offset during the remaining periods of the experiment. A few subjects' choices change quite noticeably when their capital balances become negative. One such pattern is for a subject to adopt a practice of bidding at or near her signals. The apparent rationale for this strategy is to win many auctions in the hope of "hitting a long shot" high payoff and recouping earlier losses. A rare but much more perverse response to plunging into red ink is for a subject to start bidding amounts that exceed the maximum possible conditional value ($x_i + \theta$) of the auctioned item and even amounts that exceed the maximum possible value ($\bar{x}$). The objective of this behavior could be to prevent other subjects from making money in the auction. Both of these patterns of negative-balance bidding increase the EV winner's curse.

One condition of the uncontrolled behavior of a few subjects in the baseline experiments is that subjects had limited liability and, therefore, did not have to pay to the experimenter any negative balances. Hansen and Lott (1991) hypothesized that the winner's curse would disappear if the subjects had unlimited liability. We designed experiments to ascertain the *quantitative* significance in our experimental design of the implications of limited liability that were analyzed by Hansen and Lott.[8] Thus, in our "liability treatment" we adopted their recommendation of reinitializing the subjects' capital balances every period. We used a capital balance of 2000 (experimental dollars) per subject. Since the subjects' private signals, x_i, were drawn from the uniform distribution centered on the common value, x_0, such that $x_0 - 1800 \leq x_i \leq x_0 + 1800$, the distribution of x_0 conditional on x_i assigns zero probability to values of x_0 that are not contained in the interval $[x_i - 1800, x_i + 1800]$. Thus the capital balance of 2000 was sufficient to ensure that a subject could not go bankrupt in an experiment even if he bid amounts equal to (or up to 200 above) his signals. In order not to have the gift of 2000 per period make these experiments prohibitively expensive, we randomly selected 3 out of the 30 periods for payoff at the end of an experiment.

D. Specialization and performance

Bankruptcy is an important part of the self-selection process that creates specialized professional managers in any industry. Therefore, in defining a "specialist treatment" we invoked a bankruptcy termination rule in which subjects knew in advance that they would be excused if in any period their working capital balances became negative.

An instructional device widely used in business to sharpen specialized decision skills is to allow managers to experience the consequences of their actions through practice or "gaming" exercises.[9] In our specialist treatment, subjects participated in eight practice trials with complete-information feedback: after each practice trial, subjects would see the common value and all of the signals, bids, and safe haven payments.[10] After the practice trials were completed, any amounts won or lost were canceled and each subject's capital balance was restored before beginning the data-reporting rounds of the experiment. In the data reporting rounds of the specialist treatment, the subjects were given the same type of partial information feedback as in the other treatments; that is, they were informed only of the amounts of the winning bid and the common value.

Specialization, experience, and survival are also related to capitalization. Greater initial capital endowments increase the likelihood that bidders can learn to adapt their bidding behavior before triggering the bankruptcy termination condition. Accordingly, in our specialist treatment we increased each subject's initial capital endowment by 50%, from 1000 to 1500 experimental dollars.

5. Market entry and individual bidding behavior

A. Market entry and exit

Table 3 lists the average predicted and observed entries for experienced, non-bankrupt subjects for each market size and treatment condition using a positive-income safe haven. Entry predictions are derived from expected profits from Nash equilibrium bidding. The numbers of predicted entries reported in Table 3 are conditional on realizations of the safe

Table 3. Entry behavior.

Market size (No. of subjects)	Design treatment	Predicted entries	Observed entries	% Observed entries predicted
6	Baseline	3.9	2.9 (1.0)	91
8	Baseline	4.9	3.9 (1.3)	90
8	Liability	4.9	4.6 (1.3)	83
12	Baseline	7.7	4.8 (1.5)	84
12	Specialist	7.7	5.4 (1.8)	95

Note: sample standard deviations in parentheses.

haven incomes and the bidders' signals; hence they differ from the unconditional predictions reported in Table 1 for the design parameters.

In every experiment, we observe fewer entries than predicted.[11] Where the predicted entries are in the range 3.9 to 4.9 (market size 6 to 8), the corresponding observed entries vary from 2.7 to 4.6. Where the predicted number of entries is 7.7, we observe entries of 4.8 to 5.4 bidders per period. The actual number of entries is significantly different than the predicted number by a two-tailed t-test at 5 percent significance for only one design treatment, the baseline design with market size 12. But the Fisher sign test leads to rejection of the hypothesis that the observed number of entries is equally likely to be greater or smaller than the predicted number of entries ($p = .03$) in favor of the alternative that observed entries are lower.

As seen from the fifth column in Table 3, the model succeeds quite well in using Nash equilibrium expected profits for predicting which subjects enter the auction: the percentage of observed entries that are predicted entries varies from a low of 83 percent to a high of 95 percent. Thus the self-selection process of entering or not entering the auction follows a pattern that is generally consistent with the model: individual subjects usually exit from the auction market in periods when their safe haven opportunity costs are above the expected profit from Nash equilibrium bidding.[12] It is noteworthy that the market-size-12 specialist treatment has the high of 95 percent observed entries that are predicted.

One observes from Table 3 that a feature of the more demanding environment of market size 12 is an increased flight to the safe haven. With a positive-income safe haven, the average number of entries shakes down to less than 6 in all of our treatments. This underlines the importance of the primary motivation for this study: making auction market entry behavior endogenous. Our observations tell us that subjects adjust profitably to the winner's curse, not only by learning to bid less aggressively, but also by self-selecting to enter less frequently. Given these observations, it is relevant to note that in bidding for offshore oil leases the number of active bidders on individual tracts averages about three although there are many more bidders in the auction.

The hypothesis that observed auction market entry and exit are independent of the safe haven opportunity costs is rejected by a binomial test at a significance level less than 1%. The null hypothesis is that entries occur with a probability p_i in each experiment. The test of independence asks whether good entries are more likely than the assumed probability under the null hypothesis. If bidders were to play asymmetrically, where some always bid and some never bid, this would be not the case. Under the null hypothesis, when p_i is chosen to be the sample probability, the samples are sets of experiments with the same a^* and $\bar{a}$. A bidder enters with probability p_i. This will be a good entry with probability $a^*/\bar{a}$ on average. The expected value of good entries would then be $p_i a^*/\bar{a}$. The test is a "binomial" test because each observation is a Bernoulli trial. The point of this test is to summarize the fact that bidders are taking into account their safe haven payments even if they are not employing them to execute a time-invariant monotonic entry strategy.

B. *Individual bidding behavior*

We here report the results from econometric estimation of bid functions with data for individual experienced, non-bankrupt subjects. The four-line bid function in statement (6) presents some problems for estimation which we handled in the following way. The dominant strategy bid of 1 when there is only one bidder in the auction (i.e., $n = 1$) that is independent of the bidder's signal, x_i is clearly a separate regime from the rest of the bid function. Observations with $n = 1$ were deleted from the sample in estimating the parameters of the three-part bid function that applies when $n \geq 2$ and Nash equilibrium bids are given by a monotonically increasing function of the bidder's signal.[13]

The part of the increasing bid function that applies to very high signals does not have a closed form. Our approach here was to find a numerical solution for this part of the bid function, for each value of n in the data, and then find the quadratic equation that provided the closest approximation of the solution (in the sense of minimizing the sum of squared residuals). This approach yields the following equation for determining theoretical bids for high signals ($x_i > \bar{x} - \theta$):

$$W_{it} = \hat{\alpha}_0(n_t) + \hat{\alpha}_1(n_t)x_{it} + \hat{\alpha}_2(n_t)x_{it}^2, \tag{21}$$

where the $\hat{\alpha}_i(n_t)$, $i = 0, 1, 2$, are the parameters that provide the closest quadratic approximation of the numerical solution for n_t number of bidders.

We use OLS to estimate parameters with a spline function approach using data from the baseline and specialist treatments with experienced subjects. First define the dummy variables:

$$D_{lt} = \begin{cases} 0, & \text{if } x_{it} < \underline{x} + \theta \\ 1, & \text{otherwise;} \end{cases} \tag{22}$$

$$D_{ht} = \begin{cases} 0, & \text{if } x_{it} > \bar{x} - \theta \\ 1, & \text{otherwise.} \end{cases} \tag{23}$$

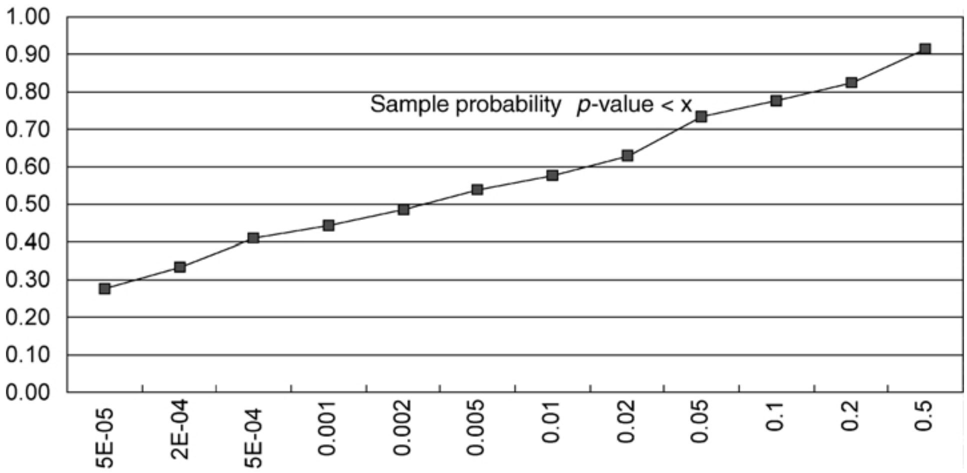

Figure 2. Cumulative distribution of p-values by subjects for $\underline{x} - \theta < x_i < \bar{x} + \theta$.

The estimating equation is

$$b_{it} = (1 - D_{lt})\beta_0 + (1 - D_{lt})\beta_1 \frac{x_{it} - \underline{x} + \theta}{n_t + 1} + D_{lt} D_{ht} \beta_2 (x_{it} - \theta) + D_{lt} D_{ht} \beta_3 Z_{it} + (1 - D_{ht})\beta_4 W_{it} + \varepsilon_t, \tag{24}$$

where Z_{it} is defined by Eq. (7), W_{it} is defined by Eq. (21), and ε_t is an error variable. The testable theoretical restrictions on the parameters are

$$H_o : \hat{\beta}_0 = \underline{x}; \quad \hat{\beta}_j = 1, \quad j = 1, \ldots, 4. \tag{25}$$

Figure 2 presents the cumulative distribution of p-values for F-tests of the parameter restrictions in statement (25). Note that the parameter restrictions in (25) are rejected at 5% confidence for 75% of the subjects. We conclude that the risk neutral Nash equilibrium model does not provide a good quantitative explanation of bidding in common value auctions by the majority of subjects.

We next report the implications for the winner's curse of endogenous entry and exit, together with our various treatments.

6. Experiment treatment effects

In all data analysis involving the magnitude of a winner's curse or blessing, we omit data from those periods in which there was only one bidder in the auction because its inclusion would bias the analysis against finding a winner's curse. In order to understand this, consider the effect of applying the *EVCurse* measure in Eq. (12) to an observation where the only bidder in the auction bid 1 experimental dollar and the conditional expected value of the auctioned item equaled the mean of the unconditional distribution, 12,500 experimental dollars.

The estimation strategy used to measure treatment effects is appropriate for data that include repeated measures of decisions made by subjects. A linear mixed-effects model is used for estimation. The fixed effects are associated with the experimental treatments while the random effects are associated with each subject as well as each experiment. The mixed-effects estimation is implemented using the S statistical program (Pinheiro and Bates, 2000).

A. *Using data from all of the experiments*

Table 4 presents estimated treatment effects for the positive income safe haven (SH), liability (L), market size 12 (MS12), specialist (S), subject experience (EX), first round high income (H1), and second round high income (H2) treatments. The first row of Table 4 reports estimation of the experimental treatment effects using data from all of our experiments. The fitted model relates the EV Curse associated with the bid made in period k by subject j in experiment i to dummy variables for the treatments as follows.

$$EVCurse_{ijk} = \beta_0 + \beta_1 MS12_i + \beta_2 SH_i + \beta_3 L_i + \beta_4 S_i + \beta_5 EX_i + \beta_6 H1_i + \beta_7 H2_i + u_i + v_{ij} + \varepsilon_{ijk}, \tag{26}$$

with the assumed error term distributions:

$$u_i \sim N(0, \sigma_1^2);\ v_{ij} \sim N(0, \sigma_2^2);\ \varepsilon_{ijk} \sim N(0, \sigma_3^2). \tag{27}$$

Note that at 5% significance, or even 1% significance, all of the treatments except the safe haven and first- and second-high income status had significant effects on the EV Curse.

Table 4. Treatment effects.

Dependent variable	Independent variables							
	Const.	*SH*	*L*	*MS*12	*S*	*EX*	*H*1	*H*2
	All data with $N > 1^a$							
EV Curse	621	−166	751	708	−581	−704	−258	−249
	(<.0001)	(.1479)	(.0006)	(<.0001)	(.0086)	(.0003)	(.1802)	(.1988)
	Fully-controlled experiments with experienced subjects and $N > 1^b$							
EV Curse	−590	297	684	421	−155	—	−108	−14
	(.0084)	(.0862)	(.0027)	(.0573)	(.4791)	—	(.5349)	(.9358)

[a] 10,403 observations of 528 subjects in 66 experiments.
[b] 3,871 observations of 226 subjects in 25 experiments.
Note: p-values in parentheses.

The dummy variable, *SH*, equals 1 when the safe haven pays positive income and equals zero when the safe haven pays zero income. Observe that the addition of positive income to the safe haven treatment (SH) does not have a significant effect on the EV Curse.

The dummy variable L equals 1 for data from a liability treatment experiment. The liability treatment *increases* the EV winner's curse. This test result is inconsistent with the Hansen and Lott hypothesis that limited liability increases the winner's curse.

$MS12$ is a (market-size) dummy variable for the 12-subject experiments. Note that the EV Curse is larger in the market size 12 experiments.

The dummy variable S equals 1 for experiments with the specialist treatment involving full information practice rounds, higher initial capital balance, and the bankruptcy termination procedure. $S = 0$ in all other experiments. The specialist treatment significantly reduced the EV Curse.

The *EX* variable equals 1 for data from experiments with experienced subjects and it equals 0 for data for inexperienced subjects. Note that subject experience significantly decreased the EV Curse. $H1$ equals 1 for data from experiments with once-experienced, high-profit subjects and $H2$ equals 1 for data from twice- or thrice-experienced, high-profit subjects. The coefficients for $H1$ and $H2$ are not significant. Thus, previous common value auction experience significantly decreases the EV Curse but it makes no difference whether that experience included relatively profitable or unprofitable bidding.

Recalling the definitions of the dummy variables, we observe that the significant constant in the first row of Table 4 indicates that inexperienced bidders in the market size 6 or 8 experiments with a zero income safe haven and limited liability had an expected loss from bidding in the auction. In contrast, experienced bidders with limited liability in six- or eight-bidder market sizes, with or without the positive income safe haven, and with or without the specialist treatment had an expected profit from bidding in the auction. Thus the winner's curse appears to result mainly from subject inexperience although it is more of a problem in the market size 12 treatment.

B. *Using data from experiments with experienced, fully-controlled subjects*

A few of the subjects in our treatments that did not include the bankruptcy termination procedure adopted bidding patterns that suggest a loss of control of their incentives. Thus, upon accumulating a negative balance, some subjects started bidding amounts equal to or above their signals or even amounts greater than the maximum possible conditional expected value of the auctioned item. Such behavior does not appear to be controlled by local profit incentives. In order to check on whether our estimates of the treatment effects are affected by such behavior, we estimate treatment effects using only data from experiments with experienced subjects, none of which adopted the above-described negative-balance bidding patterns. In other words, we now delete data for an entire experiment if even one subject adopted a negative-balance, high-bid pattern of bidding behavior. We also delete all data from experiments with inexperienced subjects. The fitted model is the same as that in statements (26) and (27) except it excludes the dummy variable for experience. The resulting coefficients are reported in the second row of Table 4.

At 5% or 1% significance, there are two differences from row 1 results: the market size 12 and specialist treatments are no longer significant. If we increase the cutoff significance level to 10%, then the market size 12 coefficient is significantly positive, as it is in row 1, but there is one other difference between row 2 and row 1 results: the positive income safe haven has a significantly positive coefficient in row 2.

Recalling the definitions of the dummy variables, we observe that the significant constant in the second row of Table 4 indicates that experienced bidders in the market size 6 or 8 experiments with a zero income safe haven and limited liability had an expected profit from bidding in the auction; that is, they experienced a winner's blessing not a winner's curse. This absence of the winner's curse is less clear in market size 12 experiments with a positive income safe haven. As before, the results from the liability treatment are inconsistent with the Hansen and Lott hypothesis.

7. Conclusion

We extended bidding theory and experimental design to include endogenous entry into the auction market. This introduces some salient characteristics of general economic reasoning and of naturally-occurring auctions: (a) that entry into an auction market is endogenous and comes at an opportunity cost of foregone earnings from the next-best alternative activity; and (b) auction market equilibrium configurations include determination of the number of active bidders as well as their bidding strategies. We tested the model's predictions for auction market entry and individual bidding behavior. The hypothesis that market entry is independent of opportunity cost is rejected; thus subject bidders do endogenously determine auction market size according to relative profit opportunities within and outside the auction. But the observed number of entries is consistently lower than the number predicted using Nash equilibrium profit as the measure of expected profit from entry. This behavioral difference from theoretical predictions is consistent with a rational adjustment of entry decisions because average observed profits from bidding are lower than Nash equilibrium profits.

Our several experimental treatments permit us to explore the conditions under which a winner's curse or a winner's blessing occurs. The winner's curse occurs among inexperienced bidders and can be a problem for experienced bidders in experiments with as many as 12 potential bidders.

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Notes

1. See, for example, Bazerman and Samuelson (1983), Dyer et al. (1989), Kagel and Levin (1986), Kagel et al. (1989).
2. Several distinct concepts of the winner's cures have appeared in the literature. Conditions under which they are and are not consistent with bidding theory are explored in Cox and Isaac (1984, 1986). The EV (concept of

the) winner's curse requires concave utility as a maintained hypothesis because the *optimal* bid of a risk-loving agent could exceed this *expected value*.

3. There are a few papers that do not assume that the number of bidders is an exogenously-determined constant. Matthews (1987) and McAfee and McMillan (1987b) develop bidding models that incorporate a random number of bidders. But a model with a random number of bidders that is drawn from an exogenous distribution is quite different from a model in which the number of bidders is endogenously determined. McAfee and McMillan (1987a) examine private value auctions with entry. Harstad (1990), Hausch and Li (1989), and Levin and Smith (1994) model common value auctions with entry.
4. For all $n \geq 1$, $\frac{\Delta\pi(n)}{\Delta n} < 0$. $\frac{\partial P(n-1)}{\partial a^*} \gtrless 0$ as $[(n-1) - (m-1)\frac{a^*}{\bar{a}}] \gtrless 0$. Therefore $\nu'(a^*) < 0$. Since $\nu(a^*)$ is monotonically downward sloping, it can only intersect an upward sloping function in a single point. Thus $\nu(a^*) = a^*$ for at most one a^*.
5. The equilibrium bid function for $n > 1$ has three parts. The expression, $2\theta/(n+1)$, is an "approximation" because it assumes that the second part of the bid function applies to all of the data; thus it does not reflect the deviations of the first and third parts from the second part of the bid function. The θ, $\underline{x}$, and $\bar{x}$ design parameters used in our experiments imply that 82% of the signals are expected to be in the domain of the part of the bid function in the third line on the right-hand-side of Eq. (6).
6. This control was compromised in some of the early experiments with inexperienced subjects when the same pseudo-random number generator was used both to "break" tied high bids and to generate common values and signals. Most of the experiments did maintain this control because they were conducted after a separate "tie-breaker" pseudo-random number generator was added to the network software.
7. The displayed posterior distributions were derived using the property that each subject's signal was an unbiased estimator of the common value. These posterior distributions were not based on the assumption that a subject's signal was the highest of n signals. Thus the computer did not solve the order statistic calculation (an alleged source of the winner's curse) for the subjects.
8. Hansen and Lott (1991) critiqued Kagel and Levin's (1986) experimental design and conclusions. Kagel and Levin (1991) advanced counter-arguments to the critique and re-analyzed their data using an approach that included the subjects' cash balances. Lind and Plott (1991) provided new empirical evidence on the winner's curse from experiments with a seller's auction in which limited liability is not an issue.
9. Recent examples of the use of mock training auctions by firms for management training include communication companies preparing for the FCC spectrum auctions, and electric utilities preparing for projected spot and hedge contract markets in electric power.
10. Kagel and Levin (1986) gave their subjects complete-information feedback during the *data-reporting* periods of their experiments. Such information is theoretically irrelevant; i.e., bidders already have all the information required by the model.
11. Although it is tempting to conjecture that this could be a consequence of risk aversion, the latter enters in a complex way and requires careful consideration. Risk aversion enters into bidding behavior conditional on entry, which in turn affects the expected utility of bidding profits, and finally the decision to enter based on a comparison of the expected utility of bidding profits with the utility of safe haven income.
12. We did *not* usually observe a subset of the subjects fleeing to the safe haven and staying there. There was only one exception: out of all the subjects, one subject did flee to the safe haven and remained there.
13. In all but one case, solo bidders followed the dominant strategy of bidding the minimum acceptable bid, which was 1 experimental dollar.

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