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by

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ABSTRACT

Non-cooperative bidding theory for sealed-bid auctions generally implies testable predictions that are conditioned on the risk attitudes of agents. Archetypical of this result is the Nash Equilibrium prediction for First price auctions for an object that is valued by agents in an independent and private manner. Received laboratory experiments that purport to test this theory do not generally control for the risk attitudes of subjects. Those experiments exhibit behavior inconsistent with popular bidding models that assume that agents have the same aversion to risk or are all risk neutral. In this paper we construct an explicit prior distribution for the risk attitudes of experimental subjects and reconsider the experimental results. We find that observed bidding behavior is indeed consistent with the Nash predictions when explicit prior weights are attached to alternative assumptions about subject risk aversion.

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1. INTRODUCTION

Non-cooperative bidding theory for sealed-bid auctions generally implies testable predictions that are conditioned on the risk attitudes of agents. Archetypical of this result is the Nash Equilibrium prediction for First Price auctions for an object that is valued by agents in an independent and private manner. Received laboratory experiments that purport to test this theory do not generally control for the risk attitudes of subjects. Those experiments exhibit behavior inconsistent with popular bidding models that assume that agents have the same aversion to risk or are all risk neutral. In this paper we construct an explicit prior distribution for the risk attitudes of experimental subjects and reconsider the experimental results. We find that the observed bidding behavior is indeed consistent with the Nash predictions when explicit prior weights are attached to alternative assumptions about subject risk aversion.

In Section 2 we briefly review the existing theoretical and experimental literature, noting discrepancies between the two as to what is assumed about subject risk attitudes. In Section 3 we consider a specific Nash bidding model due to Cox, Roberson and Smith [1982] that clearly illustrates the risk-sensitivity of the theoretical predictions. In Section 4 we provide independent evidence of the risk attitudes of experimental subjects in a test for risk aversion developed by Harrison [1986a]. This evidence allows us to construct an explicit prior probability density function over the coefficient of (constant relative) risk aversion employed in the specific bidding model of Section 3. In Section 5 we reconsider the evidence from the First Price and Dutch auction experiments reported in Cox, Roberson and Smith [1982] and Cox, Smith and Walker [1983a] [1983b].[1] Section 6 offers several concluding remarks.

2. EXISTING LITERATURE

Consider a Nash Equilibrium (NE) model of bidding behavior for First Price (FP) Sealed-Bid auctions in which the single object is independently and privately valued by agents. The recent theoretical and experimental literature on this topic is classified in Table 1 according to the treatment of risk aversion. Three aspects of that treatment are considered: (i) whether or not the set of active bidding agents are assumed to have homogeneous or heterogeneous attitudes to risk, (ii) whether or not agents are assumed to be risk neutral, and (iii) whether or not the distribution of risk attitudes across agents is known to all agents as common knowledge. We also distinguish between auctions designed to sell a single unit of some commodity and those designed to sell multiple units (the latter are generally referred to as discriminative auctions).

Our classification indicates that, with the sole exception of Cox, Smith and Walker [1985b], an apparent gulf exists between the theoretical models available and their experimental counterparts. On the other hand, the available experimental literature appears to be devoid of precise theoretical counterparts. This methodologically awkward state of affairs would be acceptable if the behavioral results from experiments pertaining to the final

TABLE 1

Classification of Literature

Treatment of Bidder Risk Attitudes	Theoretical	Experimental
I. Single-Unit		
I.1 Risk Neutral	Vickrey [1961]	CSW [1985b]
I.2 Homogeneous & Known RA	Holt [1980] Mathews [1980] Harris & Raviv [1981] Riley & Samuelson [1981]	None
I.3 Heterogeneous & Known RA	CRS [1982]	None
I.4 Heterogeneous & Known RA or RP	Cox & Smith [1984]	None
I.5 Heterogeneous & Unknown RA or RP	None	CRS [1982] CSW [1983a] CSW [1983b] CSW [1983c]
II. Multiple-Unit		
II.1 Risk Neutral	Vickrey [1962]	None
II.2 Homogeneous & Known RA	Harris & Raviv [1981]	None
II.3 Heterogeneous & Known RA	CSW [1982] CSW [1985a]	None
II.4 Heterogeneous & Unknown RA or RP	None	CSW [1984] CSW [1985a]

Notation: RA denotes Risk Aversion; RP denotes Risk Preferring; CRS denotes Cox, Roberson and Smith; CSW denotes Cox, Smith and Walker.

classification were in accord with predictions drawn from the more restrictive earlier classifications. In this case we could conclude that the theory is robust in a well-defined behavioral sense, viewing the experimental literature as providing a healthy series of "boundary experiments" to test the range of applicability of the theory.[2]

Unfortunately, there are some serious disparities between the behavioral properties of most of the experiments and the theoretical models. Consider the single-unit auction results in Cox, Roberson and Smith [1982] and Cox, Smith and Walker [1983b] [1985b]. One stylized fact to emerge from these experiments is that individual bids are generally too high to be consistent with risk neutral behavior, indicating that subjects behave as if averse to risk. This result obtains in experiments with more than three bidders. One obvious criticism of these results is that the experimental setting did not correspond to any known or articulated theoretical framework. Specifically, subjects did not know the probability distribution used by Nature to draw the attitudes to risk from.

3. A SPECIFIC BIDDING MODEL

Cox, Roberson and Smith [1982], hereafter CRS, present a model based on a power function utility specification for agent i:

$$(1) \quad U_i(y) = y_i^r$$

where U denotes the utility of experimental income y, and $(1-r_i)$ is the Arrow-Pratt measure of (constant relative) risk aversion. CRS restrict

r_i to lie on the closed interval [0,1], with r_i=1 corresponding to risk neutrality. They then show that the NE bidding function b_i^o for this utility specification is

$$(2) \quad b_j^o = \underline{v} + \frac{N-1}{N-1+r_j}(v_j - \underline{v})$$

where N is the number of bidders, and v_i the valuation for subject i drawn from a uniform distribution defined over [$\underline{v}$,v']. Further, they show that the expected utility of bid b_i is

$$(3) \quad U(b_j) = \gamma^{N-1}(b_j - \underline{v})^{N-1}(v_j - b_j)^{r_j}$$

where

$$\gamma = \frac{N - 1 + E(r)}{(N-1)(v' - \underline{v})}$$

and E(r) is the expected value of r_i for the group of N bidders. Two key assumptions of this model are: "...(a) each agent i chooses b_i to maximize $EU(b_i) = (v_i-b_i)^{r_i}G_i(b_i)$, where $G_i(b_i)$ is the probability that b_i is the highest of N bids; [and] (b) agent expectations are rational, $G_i(b_i) = G(b_i)$..." (Cox, Smith and Walker [1985b; p. 160].

The methodological dilemma posed by the disparity between theory and experimental behavior has been posed beautifully by Cox, Smith and Walker [1985b; p. 162]:

> We interpret the observation that subjects bid in excess of the predictions of the Vickrey model as due to heterogeneous risk-averse agents, and have used this <u>interpretation</u> to develop an improved model.

> Subsequently this model was found to be consistent with the scalar invariance tests described [... in Cox, Smith and Walker [1983a]]. Now we ask whether direct methods might be applied to examine the risk-averse interpretation of the data. Other interpretations are possible. We might assume in place of (a) that agents choose to bid to maximize $EU(b_i)=(v_i-b_i)G_i(b_i)$, and instead of (b), that $G(b_i)=[G(b_i)]^{1/r_i}$ where r_i is now a characteristic of bidder i that transforms the objective probability of winning, $G(b_i)$, into a subjective probability of winning, $[G(b_i)]^{1/r_i}$. This subjective expected value ... model is prominent in psychology [....]. It abandons Muthian rational expectations, but the resulting model yields a bid function identical with (2), and the two theories are observationally equivalent on the basis of all experimental tests to date. The methodological point is that the parameter r_i is not observable; it is a construct based on an interpretation of what is driving behavior, and other interpretations are potentially admissible. We have adopted the heterogeneous risk-averse interpretation because it is an integral part of the traditional EUT [Expected Utility Theory], while the alternative is thought to be "ad hoc". This does not mean that EUT is "true", but that it appears that there is not yet a sufficient basis for the scientific community to abandon EUT.
>
> We propose two payoff manipulation models which, based on EUT, should have a determinate effect as interpreted in terms of risk aversion. If these models are "correct", and our interpretation that subjects are risk averse is correct, the new data should be consistent with these predictions.

Unfortunately, the resulting data was not consistent with these predictions, leading Cox, Smith and Walker [1985b; p. 164-5] to question the validity of the payoff manipulation procedures.

For present purposes, however, there is one crucial point in the above methodological position: is the parameter r_i observable? More precisely, can it be observed independently of the bidding experiment being evaluated? We argue that it can be so observed, thereby allowing a re-evaluation of the existing bidding data and the CRRA interpretation of it.

The force of the earlier criticism in Section 2 is that subjects did not know r_i or E(r), as required by this theory. Note that E(r) does not enter into the NE bid function (2) while r_i does.[3] Thus one possible response to the theoretical criticism is that subjects "only need to know their own

r_i" in order to compute b_i^o without being able to compute $U_i(b_i)$ in (3), since the latter requires that they know $E(r)$. Such questions can be legitimately left begging, of course: we do not claim that subjects actually arrive at their behavioral rules by the same thought-experiment that happens to be convenient for theorists. Whether or not this response is acceptable is left to the reader's methodological tastes.

This argument, however, still leaves unresolved the criticism that the subjects did not even know their own r_i. One plausible and convincing response is that we can just assume that each subject behaves as if aware of his own tastes. Such "tastes" include one's home-grown taste for risky prospects as well as alternative deterministic consumption bundles. We shall therefore reasonably presume such knowledge, even in the absence of some experimental device for revealing it to subjects.[4]

Some perspective on the game theory underlying the CRS model may be helpful in understanding our approach. The CRS and CSW model is a Bayesian game of incomplete information in the much-cited sense of Harsanyi [1967-68; Parts I and II]. Specifically, and crucially, it is a game of incomplete but consistent information. Subjects do not know either the valuation or the risk attitude of other subjects, but they are assumed in the model to know and agree on the distribution used by "Nature" to randomly generate each. Moreover, this knowledge is common. As argued above, however, the CRS and CSW experiments do not ensure that subjects have common, let alone consistent, knowledge of the distribution of risk attitudes. These experiments are therefore games of incomplete and potentially inconsistent information in the sense of Harsanyi [1967-68; Part III].

One response to this disparity is to simply postulate that the experimental subjects behaved "as if" in a game of consistent information: see Harsanyi [1967-68; Part III, Postulate 4, p. 493]. This is the approach we adopt. We assume that subjects behave as if a certain prior distribution over risk attitudes is known to all throughout the experiment.

The issue now becomes how to control for attitudes to risk when evaluating the experimental results. One way to control for this is simply to assume some value and proceed to compare observed and predicted bids. However, this represents a test of the joint hypothesis that "subjects had an r_i equal to such and such a value <u>and</u> followed the NE bid function". Joint hypotheses such as this, with $r_i=1$, were used by CRS and others. As noted earlier, however, rejection of <u>this</u> joint hypothesis tells us nothing directly about the validity of the NE concept (although it may say a great deal about it's <u>behavioral</u> fragility).

If we <u>could</u> reject joint hypotheses of this general form for <u>all</u> values of r_i to which we attach positive prior probability then we could learn something about the validity of the NE concept.[5] Unfortunately, this ambitious avenue of escape for the NE concept is not available: as noted earlier, the CRS evidence is generally consistent with risk aversion and NE bidding. <u>Theory</u> is clearly silent on the <u>degree</u> of risk aversion of typical experimental subjects. In the absence of any <u>independent evidence</u> upon which to base our priors over r_i, we must view these tests of the validity of the NE concept as inconclusive.[6]

4. INDEPENDENT EVIDENCE ON RISK ATTITUDES

Harrison [1986a] has proposed and implemented an experimental test for risk aversion.[7] The objective of this test is to determine the utility function of a subject over an arbitrary interval of monetary gain. Each subject is informed at each of 24 (sequential) stages in the test that they face a "basic wager" with two prizes of zero and one dollar. The higher prize will be won if a subsequent random "lottery drawing", from a uniform distribution on the open interval (0,100), exceeds a specified value. These values are pre-determined and range from $0.05 up to $0.95 in different stages.

The subject is asked at each stage to state a minimum "selling price" for the wager, knowing that at each stage a random "buying price" will be determined. If the buying price exceeds the stated selling price, the subject receives the former price; if not, he gets to play the basic wager. If the subject gets to play the basic wager at any stage, the above-mentioned lottery drawing occurs.

Providing that this buying price is randomly determined and is independent of the stated selling price at any stage, the subject maximizes the expected utility of the reward by setting his selling price equal to his monetary equivalent of the basic wager. Given that the subject is indifferent between the utility of the stated selling price and the expected utility of the wager, we may define a utility function of monetary gain in terms of the expected values of the wager (i.e., we implicitly set the utility level of the zero payoff to zero, and the utility level of the dollar payoff to one). Utility values at each stage are thereby determined from the parameters of the basic wager, which are known.

Forty-six subjects were studied in Harrison [1986a] using this test.[8] Virtually all subjects were drawn from (honors) undergraduate courses in economics at the University of Western Ontario. All subjects were experienced with the experimental design.[9] Seven subjects displayed "degenerate responses" to the test by stating the same selling price (typically $0.99) for several wagers. It is intuitively obvious that these subjects did not respond to the monetary incentives of the test at all stages. We could have deleted these degenerate responses on "a priori" grounds, but chose to ignore these subjects altogether in the subsequent analysis.[10]

In order to obtain an estimate of each subject's coefficient of relative risk aversion we estimated an arbitrary linear transform of the CRS utility specification (1) using non-linear maximum likelihood procedures. The CRS bidding model explicitly assumes that subjects are drawn from a population with coefficient values in the closed unit interval. We therefore restrict ourselves to subjects with (maximum likelihood[11]) point estimates that fall in that interval. The estimated coefficients and standard errors of those twenty subjects are listed in Table A1 in Appendix A.

The pooled[12] point estimate and standard error corresponding to these twenty values is 0.698 and 0.311, respectively. Assuming that the pooled distribution for this coefficient is Gaussian[13], we can compute normalized probability values for r over the unit interval by truncating for all values greater than one. For pedagogic tractability, a coarse discrete approximation of this pooled distribution provides probability values of 0.019, 0.034, 0.057, 0.085, 0.114, 0.139, 0.153, 0.152, 0.136 and 0.111 for r equal to 0.05, 0.15, 0.25, 0.35, 0.45, 0.55, 0.65, 0.75, 0.85, and 0.95, respectively. Unless otherwise stated, we evaluated the pooled distribution at a much finer level, with values of r equal to 0.01, 0.02, ..., 0.99 and 1.00. These probability values are listed in Table A2 in Appendix A.

We do not want to be dogmatic as to the propriety of our non-diffuse prior. There are alternative procedures available to measure the attitudes to risk of experimental subjects, and the priors that they generate (for the same subject pool) might well differ from the prior we propose.[14] There are also alternative NE bidding models that make different assumptions about the risk characteristics of the underlying population, and these would require a quite different prior.[15] These alternatives are undoubtedly worth exploring in future research.

5. A RECONSIDERATION OF THE EVIDENCE

Table 2 illustrates the sensitivity of deviations from the NE prediction with respect to alternative assumed values for risk attitudes. The individual bidding data for all winning subjects from one representative CRS experiment, DFD8x, is employed. We assume that all bidders have the same aversion to risk. When r_i=1.00 bidders are assumed risk neutral and observed bids on average exceed the NE prediction by 80.7 cents. As we lower the assumed value of r_i we also increase b_i^o. When r_i=0.45 we have virtual equality between observed and predicted behavior, with a mean bid deviation of only minus $0.009.

Table 2 brings to a head the need for some prior over risk attitudes. If we adopt a diffuse (uniform) prior over the ten r_i values shown, we have a posterior mean bid deviation of $0.044. If we instead adopt the non-diffuse prior shown in Table 2 we have a posterior mean bid deviation of

TABLE 2

Illustrative Risk-Conditioned Bid Deviations for Experiment DFD8x (N=4): Winning Bids

r	Non-Diffuse Prior Probability	Unweighted Mean	Unweighted Standard Deviation	Weighted Mean	Weighted Standard Deviation
0.05	0.019	-0.788	0.525	-0.015	0.0099
0.15	0.034	-0.575	0.503	-0.019	0.0171
0.25	0.057	-0.375	0.483	-0.021	0.0275
0.35	0.085	-0.186	0.466	-0.016	0.0396
0.45	0.114	-0.009	0.451	-0.001	0.0514
0.55	0.139	0.158	0.438	0.022	0.0608
0.65	0.153	0.316	0.427	0.048	0.0653
0.75	0.152	0.466	0.417	0.071	0.0634
0.85	0.136	0.608	0.409	0.083	0.0556
0.95	0.111	0.743	0.402	0.082	0.0446
Sum	1.000	--	--	0.243	0.4352
Average	--	0.044	0.450	--	--
1.00	--	0.807	0.399	--	--

only \$0.243. By contrast, the degenerate prior that assigns all weight to the risk-neutral case (r=1.00) implies a posterior mean bid deviation of \$0.807. Note that the standard deviation of the data exceeds \$0.39 for all ten values of r_i. Thus only one of these three alternative priors leads (in this example) to bid deviations that are statistically different from zero at conventional significance levels, although all three imply quite different posterior mean bid deviations. We will employ standard two-tailed t-tests throughout our evaluation of results. No qualitative conclusions are altered, unless otherwise stated, if we employ less familiar non-parametric tests.

Tables 3 and 4 report the results of applying these three alternative weighting procedures to all of the FP and Dutch experiments reported in CRS and Cox, Smith and Walker [1983a] [1983b].[16] Table 3 evaluates the <u>winning</u> bids in the FP auctions. Table 4 evaluates the <u>winning</u> bids in the Dutch auction (no other bids are observed[17]). Each of these tables employs an evaluation of the relevant prior at a much finer level than Table 2 (the calculations tabulated in Table 2 were undertaken using the 100 values for r_i shown in Table A2).

In all of these experiments $\underline{v}$ = \$0.10. The highest valuation v' was varied with N such that expected profit per risk neutral bidder was held constant at \$0.40 per period in the NE. Specifically, v' = \$4.90, \$8.10, \$12.10, \$16.90 and \$36.10 for N = 3, 4, 5, 6 and 9, respectively. This variation in v' is particularly important to ensure comparable saliency across experiments in which N is varied.

TABLE 3

Prior-Weighted Bidding Behavior in First-Price Auctions: Winning Bids

N	Exp. ID	Number of Bids	Degenerate Prior Mean	Degenerate Prior Std. Dev.	Diffuse Prior Mean	Diffuse Prior Std. Dev.	Diffuse Prior Mean	Diffuse Prior Std. Dev.
3	DFD3	10	-0.167	0.55	-0.694	0.63	-0.550	0.61
	FDF3'	20	0.070	0.47	-0.394	0.50	-0.267	0.48
	FDF10	20	0.087	0.25	-0.404	0.32	-0.269	0.29
	DFD10'	10	0.327	0.25	-0.190	0.26	-0.048	0.25
	DFD10x	10	0.120	0.19	-0.308	0.26	-0.190	0.24
4	FDF8	20	0.830	0.47	0.164	0.38	0.338	0.39
	DFD8'	10	0.800	0.21	0.120	0.23	0.298	0.22
	DFD8x	10	0.807	0.40	0.044	0.45	0.243	0.43
	FDF8'x	20	0.822	0.32	0.099	0.30	0.288	0.29
	DFD8x*	10	0.987	0.16	0.224	0.19	0.423	0.17
	FDF8'x*	20	0.781	0.34	0.056	0.34	0.245	0.33
5	DFD9	10	1.380	0.37	0.460	0.46	0.693	0.44
	FDF9'	20	1.089	0.37	0.199	0.35	0.425	0.35
	FDF9x	20	0.994	0.45	0.084	0.52	0.315	0.50
	DFD9'x	10	1.176	0.27	0.335	0.34	0.549	0.32
	DFD9*	10	0.558	1.00	-0.329	1.04	-0.104	1.03
	FDF9'*	20	1.275	0.41	0.375	0.39	0.603	0.39
6	DFD2	10	0.797	0.82	-0.280	0.79	-0.012	0.80
	FDF2'	20	1.509	0.81	0.378	0.85	0.660	0.84
	DFD4	10	0.887	0.52	-0.250	0.60	0.033	0.57
	FDF4'	20	1.124	0.39	0.028	0.34	0.301	0.35
9	DFD5	10	1.784	0.71	0.090	0.81	0.500	0.78
	FDF5'	20	2.323	0.88	0.606	0.91	1.021	0.90

TABLE 4

Prior-Weighted Bidding Behavior in
Dutch Auctions: Winning Bids

N	Exp. ID	Number of Bids	Degenerate Prior Mean	Degenerate Prior Std. Dev.	Diffuse Prior Mean	Diffuse Prior Std. Dev.	Diffuse Prior Mean	Diffuse Prior Std. Dev.
3	DFD3	10	-0.783	0.67	-1.219	0.85	-1.093	0.80
	FDF3'	20	0.520	0.34	-0.057	0.37	0.101	0.36
	FDF10	20	0.233	0.49	-0.277	0.52	-0.137	0.50
	DFD10'	10	0.150	0.38	-0.329	0.45	-0.198	0.43
	DFD10x	10	-0.010	0.57	-0.506	0.66	-0.370	0.63
	SDS7	10	-0.333	0.51	-0.858	0.54	-0.714	0.52
	DSD7'	20	0.523	0.43	0.017	0.48	0.156	0.46
4	FDF8	20	0.890	0.28	0.224	0.32	0.398	0.30
	DFD8'	10	0.474	0.58	-0.147	0.56	0.015	0.55
	DFD8x	10	0.661	0.69	-0.050	0.70	0.135	0.70
	FDF8'x	20	0.747	0.34	-0.016	0.34	0.183	0.33
	DFD8x*	20	0.849	0.54	0.134	0.56	0.320	0.55
	FDF8'x*	10	0.560	0.52	-0.200	0.53	-0.002	0.53
5	DFD9	10	0.534	0.62	-0.342	0.59	-0.120	0.60
	FDF9'	20	1.488	0.39	0.573	0.32	0.805	0.34
	FDF9x	20	0.384	0.52	-0.437	0.66	-0.229	0.62
	DFD9'x	10	1.098	0.44	0.156	0.44	0.395	0.43
	DFD9*	20	-0.264	1.01	-1.139	1.10	-0.917	1.07
	FDF9'*	10	1.386	0.26	0.463	0.28	0.697	0.27
6	DFD2	10	1.018	0.61	-0.101	0.64	0.178	0.63
	FDF2'	20	1.423	0.64	0.316	0.61	0.592	0.61
	DFD4	10	0.583	0.63	-0.527	0.69	-0.251	0.67
	FDF4'	20	1.153	0.42	0.023	0.46	0.304	0.44
	DSD1	20	0.643	0.71	-0.436	0.81	-0.167	0.78
	SDS1'	10	0.900	1.35	-0.224	1.38	0.056	1.37
	DSD3x	20	1.513	0.91	0.355	0.94	0.643	0.93
9	DFD5	10	1.669	1.05	-0.032	1.09	0.379	1.08
	FDF5'	20	1.544	1.58	-0.150	1.67	0.260	1.64
	DSD4	20	0.250	1.49	-1.352	1.56	-0.965	1.54
	SDS4'	10	2.307	1.42	0.668	1.39	1.064	1.40

In each session a given number of bidders participated in 30 auctions, one after the other. These consisted of 10 FP, followed by 10 Dutch, followed by 10 FP (denoted FDF) or 10 Dutch, 10 FP and 10 Dutch (denoted DFD). We shall refer to each such session as "an experiment". Apart from a $3.00 payment upon simply turning up, expected profit (at the risk neutral NE) per subject per experiment was therefore $12.00; each experiment lasted about one hour. The numeral in the experiment ID denotes a particular parameter design (e.g., choice of N and v'). The "x" denotes the use of experienced subjects, typically in a different parameter design, and an asterisk indicates that subject payoffs were tripled.

In order to ensure comparability between paired Dutch and FP experiments CRS (p.20) restricted bids in the latter to be in increments corresponding to the "tick interval" of descending Dutch bids. In the N=3 case this interval was $0.20, such that the only feasible bids consisted of the set ($0.10, 0.30, 0.50,...,4.50, 4.70, 4.90). In the N=4 case the parameters listed in CRS (Table 3, p. 19) imply a set of feasible bids of ($0.10, 0.40, 0.70,...,7.60, 7.90, 8.20). However, all observed bids in these experiments appear to have been restricted to the set ($0.30, 0.60, 0.90,...,7.50, 7.80, 8.10) instead. Virtually, all of the other CRS experiments restricted bids "as advertised".[18] For simplicity we follow CRS in ignoring this restriction on the feasible set of bids when generating theoretical predictions; in any event, the error implied is negligible.

Our first conclusion is that winning bids in FP auctions (Table 3) are significantly different from the NE prediction in certain cases for two of the alternative priors, but are not significantly different for the non-diffuse prior. Consider the degenerate prior first. As reported by CRS, when N>3 we observe significant overbidding: about two standard deviations above zero. Overbidding when N=3 is not at all significant. Adopting the diffuse prior on risk attitudes forces us to change these conclusions: the only significant outcome is now the underbidding when N=3. Adopting the non-diffuse prior results in conclusions intermediate between the two other priors. The underbidding when N=3 is now no longer significant. Although the tendency to overbid when N>3 is greater with the non-diffuse prior relative to the diffuse prior, it is not generally significant.[19] Thus the observed bid deviations from the NE prediction are not significant assuming the non-diffuse prior. The use of the non-diffuse prior therefore makes a major qualitative difference to the interpretation of FP bidding behavior.

The second conclusion from our results is that the winning bids in the CRS Dutch auctions (Table 4) do <u>not</u> differ significantly from the NE prediction for the diffuse and non-diffuse priors, although they do differ significantly for the degenerate prior. Actually the results in Table 4 for the degenerate prior appear to be insignificant also[20], but this is the one case where the use of a parametric test procedure (our implicit t-test) leads to incorrect qualitative conclusions. Non-parametric tests[21] lead to the rejection of the null hypothesis, at standard significance levels, for the degenerate prior when N=3, 4, 5 and 6 (but not when N=9).

6. CONCLUDING REMARKS

From a Bayesian perspective our approach to evaluating the experimental bidding data is perfectly natural. The model that is proposed (in Section 3) as explaining how the data is generated happens to be conditional on some "nuisance" parameter, r_i. One possible, plausible, and popular interpretation of this parameter is that it is a coefficient of risk aversion for a homogeneous representative bidder (recall Table 1 and our quote from Cox, Smith and Walker [1985b; p. 162]). In order to obtain an unconditional posterior distribution over bid deviations (from the NE), we must therefore integrate out the conditional distribution of the data. To do this we need to obtain a marginal prior distribution for the nuisance parameter that is independent of the bidding data we are primarily interested in. One such data-based (or "empirical") prior was proposed (in Section 4), as were two others. The posterior that results when we employ our preferred (non-diffuse) prior leads us to conclude (Section 5) that bidding behavior is consistent with the popular class of NE bidding models proposed by Holt [1980], Riley and Samuelson [1981] and Harris and Raviv [1981].

FOOTNOTES

[1]Comparable research has also been undertaken for multiple-unit discriminative auctions by Cox, Smith and Walker [1984] [1985a], and for common value auctions by Levin, Kagel, Battalio and Meyer [1985]. Important earlier experimental studies include Coppinger, Smith and Titus [1980], Miller and Plott [1985] and Smith [1967]. Kagel, Harstad and Levin [1985] and Kagel and Levin [1985] also report several First-Price sealed bid experiments with independent and private values.

[2]See Harrison [1986b] and Smith and Williams [1983] for illustrations of such experiments.

[3]E(r) does enter into the bid function in the corresponding multiple-unit case: see Cox, Smith and Walker [1982].

[4]See Harrison [1986c] [1986d] for the explicit use of such devices in bargaining experiments. An interesting series of experiments would involve using the pre-test presented in Harrison [1986a] (discussed in Section 4 below) to <u>screen</u> for subjects that are more likely to be risk-neutral, and then drawing on this subject pool in bidding experiments. The subjects may even be told, using appropriate language, that they have been so screened.

[5]Compare Leamer [1983] on the role of "sensitivity analysis" in Bayesian inference.

[6]An alternative way to control for risk attitudes is adopted in Cox, Smith and Walker [1985b]. They attempt to <u>induce</u> risk neutral subjects by means of a modification of the medium of payment to subject. This "lottery" procedure was introduced by Roth and Malouf [1979], extended by Berg, Daley, Dickhaut and O'Brien [1984], and evaluated in common-value auction experiments by Harrison and McKee [1986]. Again, it implies a joint hypothesis that "the lottery procedure induces risk neutrality <u>and</u> subjects followed the NE bid function". As noted earlier, Cox, Smith and Walker [1985b] report rejection of this joint hypothesis.

[7]This test derives from an earlier procedure devised by Becker, DeGroot and Marschak [1964], which was shown in Harrison and Rutström [1986] to have a design flaw of some behavioral significance.

[8]There were some minor variants on the test procedures over this subject pool. Some subjects were provided the actuarial value of the wager, and some were paid only one-half of their nominal earnings. Neither treatment should theoretically affect the risk aversion of subjects, and there is some evidence that it indeed has no behavioral effect.

[9]There is some behavioral evidence that experienced subjects are more likely to be classified as risk averse or risk-loving rather than risk neutral.

[10]In any event, including these subjects does not alter our subsequent conclusions in any qualitative way.

[11]In each case we initiated the iterative (non-linear) maximum likelihood search at r=1.0, corresponding to risk neutrality. We also initiated the search at r=0.5 to provide some check that our reported point estimate was indeed a global maximum.

[12]Each subject receiving equal weight. It is possible to give each subject's estimate a different weight depending on the "explanatory power" of the estimated equation. Thus if one subject has an estimated coefficient of 0.4 in a regression that explains 99 percent of the variation of the endogenous variable, and another has a coefficient of 0.6 in a regression only explaining 1 percent of the variation, we would adopt a pooled estimate over these two values lower than 0.5. Some such weighting scheme should be employed if we were to consider (CRRA) risk-loving agents as members of the population, since there are some very high estimates of r obtained in regressions with extremely low explanatory power.

[13]Such an assumption underlies the regression procedure adopted.

[14]Binswanger [1980] [1981] has implemented a procedure with Indian peasants as subjects; for present purposes we would prefer to use a more comparable subject pool to the one used in the sealed-bid experiments (viz., university students). Holt [1985] has proposed a procedure with some appealing theoretical properties, but no applications have been reported. There are many popular procedures in the experimental psychology literature (see Sherman [1967] and Ang and Schwarz [1985] for the use of two of these in economics experiments), but it is not at all clear that these procedures measure risk aversion in the Arrow-Pratt sense (although the measures that they do obtain might be shown to be highly correlated, across subjects, with those obtained using our procedure).

[15]For example, Cox and Smith [1984] extend the CRS model to allow some bidders to be risk-preferring, which corresponds to allowing values of r greater than one. If we were evaluating the risk-sensitive predictions of such a model we would not need to exclude subjects with estimated risk coefficients exceeding one, implying a different non-diffuse prior to the one appropriate for the CRS model evaluated here.

[16]I am extremely grateful to Jim Cox, Vernon Smith and James Walker for providing complete access to this raw data, and to Shawn LaMaster and James Walker for invaluable assistance in obtaining the data.

[17]They are "observed" to the limited extent of knowing that they are no greater than the winning bid. In the light of the conclusion by CRS (p. 26) that the Dutch and FP models are not behaviorally isomorphic, it may seem inappropriate to apply NE predictions for FP auction models to Dutch auction experiments. However, Harrison [1987] reconsiders their experimental data

and argues that the two institutions are behaviorally indistinguishable. Note that the behavioral validity or otherwise of the Dutch-FP isomorphism is independent of the validity, or risk-sensitivity, of the NE predictions for either institution.

[18]The only two exceptions are DFD2 and FDF2, for which feasible bids were in increments of $0.40 counting from a base of minus $0.10 (rather than $0.10).

[19]The individual results when N=5 are quite varied, but they have a pooled mean of only 0.454 with a pooled standard deviation of 0.41. With 59 degrees of freedom the null hypothesis of a zero bid deviation can only be rejected at an ultra-conservative 35 percent significance level.

[20]The only exception is when N=5: we have a pooled mean (standard deviation) of 0.856 (0.51), implying rejection of the null hypothesis at the 10 percent level. This null hypothesis is not rejected at the 8 percent level, though. The pooled means (standard deviations) for N=3, 4, 6 and 9 are -0.067 (0.51), 0.651 (0.53), 0.963 (0.59) and 1.627 (1.23), respectively. The results for N=3, 4 and 9 are insignificant at the 20 percent level and the results for N=6 are insignificant at the 10 percent level (they are significant at the 20 percent level). It should be noted that CRS (Table 5, p. 23) reject the hypothesis of risk-neutral bidding at a 5 percent level when N=4, 5 or 6 using a non-parametric Kolmogorov statistic.

[21]Such as the Kolmogorov test statistic employed by CRS (Table 4, p. 22). They do not indicate rejection of the (comparable) null hypothesis at a 5 percent level when N=3 or 9. Comparing the (one-sided) test statistic they report with the quantiles of the Kolmogorov test statistic in Conover [1980; Table A14, p. 462] one is led to reject the null hypothesis when N=3 at the 5 percent level (the test statistic and quantile are virtually identical) and when N=9 at the 1 percent level (and not to reject it at the 2.5 percent level).

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APPENDIX A

TABLE A1

Individual Subject Estimates of Coefficient of Relative Risk Aversion

Point Estimate	Standard Error
0.857	0.115
0.017	0.156
0.772	0.077
0.864	0.200
0.891	0.177
0.741	0.090
0.592	0.383
0.980	0.383
0.222	0.342
0.960	0.580
0.818	0.033
0.841	0.613
0.994	0.375
0.801	0.142
0.391	0.225
0.379	0.096
0.341	1.362
0.672	0.303
0.996	0.389
0.840	0.188

TABLE A2

Non-Diffuse Prior for Risk Attitudes

r	Probability	Cumulative Probability
0.01	0.00135	0.00135
0.02	0.00145	0.00280
0.03	0.00155	0.00435
0.04	0.00166	0.00601
0.05	0.00178	0.00779
0.06	0.00190	0.00969
0.07	0.00203	0.01172
0.08	0.00216	0.01388
0.09	0.00230	0.01619
0.10	0.00245	0.01864
0.11	0.00261	0.02125
0.12	0.00277	0.02402
0.13	0.00294	0.02695
0.14	0.00311	0.03007
0.15	0.00330	0.03336
0.16	0.00349	0.03685
0.17	0.00368	0.04053
0.18	0.00389	0.04442
0.19	0.00410	0.04852
0.20	0.00432	0.05283
0.21	0.00454	0.05737
0.22	0.00477	0.06215
0.23	0.00501	0.06716
0.24	0.00526	0.07242
0.25	0.00551	0.07793
0.26	0.00577	0.08369
0.27	0.00603	0.08973
0.28	0.00630	0.09603
0.29	0.00657	0.10260
0.30	0.00685	0.10945
0.31	0.00714	0.11659
0.32	0.00742	0.12401
0.33	0.00772	0.13173
0.34	0.00801	0.13974
0.35	0.00831	0.14805
0.36	0.00861	0.15666
0.37	0.00891	0.16557
0.38	0.00921	0.17478

0.39	0.00951	0.18429
0.40	0.00982	0.19411
0.41	0.01012	0.20422
0.42	0.01042	0.21464
0.43	0.01071	0.22536
0.44	0.01101	0.23637
0.45	0.01130	0.24767
0.46	0.01159	0.25926
0.47	0.01187	0.27113
0.48	0.01215	0.28327
0.49	0.01242	0.29569
0.50	0.01268	0.30837
0.51	0.01294	0.32131
0.52	0.01318	0.33449
0.53	0.01342	0.34791
0.54	0.01365	0.36156
0.55	0.01387	0.37543
0.56	0.01407	0.38950
0.57	0.01427	0.40377
0.58	0.01445	0.41822
0.59	0.01462	0.43284
0.60	0.01478	0.44762
0.61	0.01492	0.46254
0.62	0.01505	0.47759
0.63	0.01516	0.49276
0.64	0.01526	0.50802
0.65	0.01535	0.52337
0.66	0.01542	0.53879
0.67	0.01547	0.55426
0.68	0.01551	0.56977
0.69	0.01553	0.58529
0.70	0.01553	0.60083
0.71	0.01552	0.61635
0.72	0.01550	0.63185
0.73	0.01546	0.64731
0.74	0.01540	0.66271
0.75	0.01532	0.67803
0.76	0.01523	0.69326
0.77	0.01513	0.70839
0.78	0.01501	0.72341
0.79	0.01488	0.73828
0.80	0.01473	0.75301
0.81	0.01457	0.76758
0.82	0.01440	0.78198
0.83	0.01421	0.79619
0.84	0.01401	0.81020
0.85	0.01380	0.82400
0.86	0.01358	0.83758
0.87	0.01335	0.85093
0.88	0.01311	0.86404

0.89	0.01286	0.87690
0.90	0.01260	0.88950
0.91	0.01233	0.90183
0.92	0.01206	0.91389
0.93	0.01178	0.92568
0.94	0.01150	0.93718
0.95	0.01121	0.94839
0.96	0.01092	0.95931
0.97	0.01062	0.96993
0.98	0.01032	0.98025
0.99	0.01002	0.99028
1.00	0.00972	1.00000