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# Theory and Misbehavior of First-Price Auctions: Comment

By JAMES C. COX, VERNON L. SMITH, AND JAMES M. WALKER\*

Glenn Harrison's (1989) critique of our research on first-price auctions is based on his premise that it is "more natural" to test hypotheses about bidding behavior with data on monetary payoffs (to the experimental subjects) rather than with the bid data that we used. His reconsideration of (a small part of) the evidence is implemented with tests of the risk-neutral bidding model that use three "metrics" of forgone expected payoffs. In Section I below, we explain that Harrison's metric approach to testing behavioral hypotheses with payoff data is based on an implicit assumption of unique cardinal utility. Without unique cardinal utility, calculations with his metrics yield results of arbitrary magnitude.

Although measures of forgone payoffs cannot provide metrics for testing hypotheses derived from utility theory, they can provide useful heuristics to aid in experimental design. However, Harrison's metrics do not provide useful heuristics for our auction experiments, because they do not measure forgone payoffs in informative ways. This is also explained in Section I.

Harrison's concern with the motivational implications of subjects' monetary payoffs is a valid and widely shared concern among experimental economists. A major part of our research program has been devoted to exploring motivational questions. However, the critiques by Harrison (1989) and John Kagel and Alvin Roth (1992) create the impression that we have not examined motivational questions. In fact, our research program has provided and continues to provide clear empirical evidence on the motivation and opportunity-cost questions that

Harrison attempted to address with his metrics. This is explained in Section II.

Kagel and Roth (1992) reformulate Harrison's critique of our first-price auction research. In doing so, they implicitly reject both Harrison's metrics and his premise that it is "more natural" to test hypotheses about bidding behavior with payoff data rather than bid data. Kagel and Roth implicitly accept our explanation that it is necessary to examine *bid* data for evidence on the strength or weakness of subjects' motivation, and they present some bid data that are relevant for testing risk-neutral bidding theory and its generalizations. However, Kagel and Roth make crucial errors in interpreting and applying both Harrison's critique and our bidding theory. As a consequence, their central conclusions are untenable. We explain this in Section III below.

## I. Harrison's Critique

In deriving his "opportunity cost" for "misbehavior," Harrison (1989 p. 751) first reproduces the linear part of the Nash equilibrium bid function for the constant-relative-risk-aversion model (CRRAM) that is derived in Cox, Bruce Roberson, and Smith (1982):

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

In equation (1),  $b_j^*$  is agent  $j$ 's theoretically optimal bid,  $v_j$  is agent  $j$ 's independent private value for the auctioned object,  $1-r_j$  is agent  $j$ 's coefficient of constant relative risk aversion,  $N$  is the number of bidders in the auction, and  $\underline{v}$  is the lower bound on the support of the uniform probability distribution of auctioned object values.

Harrison also reproduces the following expected-utility function from Cox, Rober-

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son, and Smith (1982):

$$(2) \quad U(b_j) = \gamma^{N-1} (b_j - \underline{v})^{N-1} (v_j - b_j)^{r_j}$$

where

$$(3) \quad \gamma = \frac{N-1 + E(r)}{(N-1)(\bar{v} - \underline{v})}$$

and where  $1 - E(r)$  is the mean coefficient of constant relative risk aversion in the population of bidders and  $\bar{v}$  is the upper bound on the support of the uniform probability distribution of auctioned object values.

Harrison (1989 p. 751) explains his use of equations (1)–(3) as follows: “The question considered here, however, is how  $\tilde{U}$  differs from  $U^*$  as  $b_j$  deviates from  $b_j^*$ .” In other words, Harrison evaluates the expected-utility opportunity-cost function,

$$(4) \quad L(b_j) = - [U(b_j) - U(b_j^*)].$$

Harrison uses observed bids by experimental subjects and theoretical bids for risk-neutral ( $r_j = 1$ ) and risk-averse ( $r_j = 0.7$ ) agents to calculate the differences in his metrics. Specifically, his metric 1 calculates  $L(b_j)$  using the assumptions that  $r_j = 1$ , for  $j = 1, 2, \dots, N$ , and  $E(r) = 1$ . His metric 2 calculates  $L(b_j)$  using the assumptions that  $r_j = 1$ , for all  $j$ , and that  $E(r) = 0.7$ . In his table 3, Harrison extends his series of metrics to include the case in which  $r_j = 0.7$ , for all  $j$ , and  $E(r) = 0.7$ .<sup>1</sup>

Harrison reports only the median values of these calculated figures and ignores all other data. On the basis of these medians,

<sup>1</sup>Note that the calculation of Harrison’s metrics places specific inconsistent requirements on the risk characteristics of bidders. In metric 2, where  $r_j = 1$ , for all  $j$ , and  $E(r) = 0.7$ , Harrison has devised a set of conditions which, given his use, violate the conditions of the Nash model. He pools bid data across all individuals within an experiment and across experiments. From his “metric,” it would follow that *all* individuals are risk-neutral and yet they believe that *all* competitors are risk-averse. Clearly, this is an assumption that is inconsistent with the requirements of a Nash equilibrium.

Harrison concludes that the expected-utility differences are small and suggests that we were wrong in rejecting the risk-neutral model. However, as is well known, expected-utility theory does not attach any meaning either to comparisons of preference differences or to any particular cardinal measure (“metric”) of utility.<sup>2</sup>

First consider difference comparisons. Expected-utility functions are derived from binary relations over pairs of prospects, not quaternary relations over pairs of pairs (or differences) of prospects. Thus the numerical difference between two expected-utility numbers does not measure preference difference. There do exist preference axioms for which preference differences are meaningful. Furthermore, one can find in the literature a version of difference preferences that can be represented by a function  $\tilde{U}(\cdot)$  that has the (expected-utility) functional form of linearity in the probabilities (Rakesh Sarin, 1982). However, that approach would not salvage Harrison’s metrics because the “utility” function  $\tilde{U}(\cdot)$  would still only be unique up to a positive affine transformation. Thus, if  $\tilde{U}(\cdot)$  represents difference preferences, then so does  $\alpha + \beta \tilde{U}(\cdot)$ , for any  $\alpha$  and any positive  $\beta$ . Therefore, a difference-preference opportunity-cost function would be correctly written as

$$(5) \quad \tilde{L}(b_j) = -\beta [\tilde{U}(b_j) - \tilde{U}(b_j^*)].$$

However,  $\beta$  is a positive number of arbitrary magnitude. Hence, it is not possible to use (5) to make any valid statement that some  $\tilde{L}(b_j)$  is “small.” In order to make such a statement, one needs unique cardinal utility. Furthermore, since Harrison (1989 table 3) applies his metrics to data for *groups* of subjects, he requires cardinal utility that is interpersonally comparable.

Harrison (1989 p. 761) goes beyond applying his utility metrics to data from bidding

<sup>2</sup>Explanations of these properties of expected-utility theory are widely available. The most accessible expositions are in textbooks such as Hal Varian (1984) and David Kreps (1990).

experiments and ends his article with the following methodological statement: "In principle the experimental method has an advantage over other empirical techniques if one can design *controlled* experiments that induce subjects' utility functions. In this case the experimentalist can say just 'how large' the discrepancies are between observed and predicted behavior in a natural metric from the perspective of theory."

Actually, the advantage of experimental methods over other techniques does not depend in any way on one's ability to induce subjects' unique cardinal utility in the sense of being able to say "how large" is the utility of discrepancies between observed and predicted behavior. Experimentalists have not found a miraculous way to cardinalize ordinal utility uniquely, and—except for Harrison—have made no such claim.

The substance of Harrison's argument is that one cannot reject the hypothesis of risk-neutral bidding behavior in the experiments he analyzes when one uses his metrics to measure the importance of expected forgone payoffs. Harrison's behavioral hypothesis test does require a unique cardinal utility interpretation for the following reasons: (a) payoffs (and forgone payoffs) are worth considering only if the subjects care about them; and (b) Harrison's test involves specific assumptions about *how much* the subjects care about forgone payoffs of any given size. Harrison's formula for a behavioral hypothesis test embodies the specific cardinal utility function (or "metric") that is tested with the payoff data.

Although measures of forgone payoffs are not useful for testing behavioral hypotheses, they can provide useful heuristics to aid in experimental design.<sup>3</sup> However, Harrison's

metric calculations are not useful for this purpose because he only reports median values and ignores the rest of the data. Median forgone payoffs are associated with median bids; but median bids have very small (Nash equilibrium) *ex ante* calculated probabilities of winning in a first-price auction in which only the highest bid can win. These low probabilities are the reason why Harrison's metric calculations yield such "small" numbers. Indeed, as noted by Daniel Friedman (1992 p. 1375): "...Harrison's reasoning would lead to the conclusion that subject misbehavior is 'statistically negligible' even when four or more subjects all bid *randomly* between own valuation and zero. ... The underlying problem is that Harrison's statistical procedures can hide many large losses under more numerous small losses." Friedman proceeds to introduce a measure of forgone payoffs that, unlike Harrison's, may be a useful heuristic to aid in experimental design.

The hypothesis tests that originally led us to reject the risk-neutral model do not use median bids. Instead, the tests in Cox, Roberson, and Smith (1982) use market prices, that is, only the highest bids. Thus, we found that winning bids were significantly greater than those predicted by the risk-neutral model. We also found significant differences across winning bidders in their bid:value ratios. These results are inconsistent with both the risk-neutral model and the models in which bidders are assumed to all have the same risk-averse preferences. This led us to construct the first heterogeneous-bidders model, CRRAM, before analyzing individual bidding data.

## II. Our Investigations of Subject Motivation

Our critique (and those of Friedman [1992] and Kagel and Roth [1992]) of Harri-

<sup>3</sup>Harrison's critique may suggest to the reader that the salient monetary payoffs in our experiments were insignificant. However, examination of our data reveals the following. Define  $P_{RN}$  as the profits from theoretical risk-neutral bidding. Define  $P_A$  as the actual subjects' profits in an experiment. Then  $C_A = P_{RN} - P_A$  measures the actual forgone payoff from higher than risk-neutral bidding. What are the  $C_A$  values in our experiments? The series-4 experiments in Cox et al. (1988) have average values of  $P_{RN} = \$48.42$  and  $P_A = \$22.48$ . Thus, if these subjects were actually risk-neu-

tral then the payoff loss ( $C_A$ ) from nonoptimal bidding in the series-4 experiments would on average exceed \$25 per experiment. The five-bidder, 20-period enhanced-payoff experiment in Cox et al. (1988) has  $P_{RN} = \$119.28$  and  $P_A = \$42.30$ ; that is,  $C_A = \$76.98$ .

son (1989) should not be construed to imply that experimental economists can ignore questions about subject payoffs in designing their experiments. In fact, experimental economists and experimental psychologists have, for decades, been concerned with the relation between monetary payoffs and subject motivation. Sidney Siegel and Lawrence Fouraker (1960) examined the effect of increasing the differences in payoffs between the predicted (Pareto-optimal) quantity outcome and two adjacent discrete quantity outcomes in bargaining experiments. Their results showed a substantial decrease in the variance of outcomes with increased forgone payoff, but the predictions of the theory were supported under both payoff conditions. Other studies such as Siegel (1961), Fouraker and Siegel (1963), Morris Fiorina and Charles Plott (1978), David Grether and Plott (1979), Grether (1980, 1981), Cox et al. (1983a,b, 1984, 1985b, 1988), and Detlof von Winterfeldt and Ward Edwards (1986), have examined the effects of changing payoff differences on subjects' behavior.

A large part of our research program on auctions has been devoted to exploring questions of subject motivation. This part of our research program has been extensive, partly because the nonunique measurement properties of preference theory require that we examine decisions in message (or bid) space for evidence on the strength or weakness of motivation. We have used two approaches to study the relation between subjects' behavior and monetary payoffs. One of these involves the introduction of monetary payoff transformations as experimental treatments and observations of whether behavior changes as a result. The other approach consists of the development and testing of new bidding models in response to various anomalies arising in tests of CRRAM. Some of these models incorporate such things as income thresholds, utility (of the event) of winning the auction, and other nonstandard motivational assumptions. It cannot be overemphasized that, if forgone payoff makes a difference in subject decisions (and we believe that it can and does), this necessarily implies that decision-makers are motivated by other

things besides monetary rewards. This follows because standard theory predicts that decision-makers will choose so as to maximize their gain *however flat is the payoff hill*. Consequently, as has been emphasized repeatedly in the past (Siegel, 1961; Smith, 1976, 1982), other things in the utility function are keys not only to understanding motivation from the perspective of the subject, but also to obtaining a theoretical treatment of the intuition that forgone payoff matters. By ignoring the long experimental history of these considerations, Harrison's empirically justified concern with payoff motivation fails to provide any new insights.

#### A. Experiments with Payoff Transformations

Our research program on auctions has examined the effects of payoff transformations as experimental treatments. We used such transformations to obtain empirical evidence on subject motivation and in constructing alternative tests of CRRAM. We have used multiplicative transformations, power-function transformations, and stochastic (or lottery payoff) transformations. Our experiments with multiplicative transformations long ago provided theoretically meaningful evidence on the motivational questions that Harrison attempted to analyze with his metrics. The nature of this evidence can be understood from the following.

As in Section I above, let  $v_j$  be the induced (monetary) value of the auctioned object for agent  $j$ . Let  $b_j$  be the (monetary) amount of agent  $j$ 's bid. If  $b_j$  is the winning bid, then agent  $j$  receives the monetary payoff,  $v_j - b_j$ . With CRRAM as the maintained hypothesis, the utility to agent  $j$  of bidding  $b_j$  can be represented by the expected-utility function in equation (2) above. Now rewrite equation (2) as

$$(6) \quad U(b_j) = \alpha + \beta \gamma^{N-1} (b_j - \underline{v})^{N-1} (v_j - b_j)^{r_j}$$

for arbitrary  $\alpha$  and any positive  $\beta$ . State-



ment (6) drops the normalization  $(\alpha, \beta) = (0, 1)$ , which is contained in statement (2), to make it clear that the results in the following discussion are invariant to positive affine transformations of utility functions.

Experiments with multiplicative transformations of payoffs are conducted as follows. Instead of paying a winning bidder  $v_j - b_j$  dollars, we pay him or her  $\lambda(v_j - b_j)$  dollars. We then vary  $\lambda$  and observe whether the subjects' bidding behavior changes. With CRRAM as the maintained hypothesis, it is easy to understand the questions that are addressed with this payoff treatment.

With the multiplicative-payoff treatment, expected-utility function (6) can be rewritten as<sup>4</sup>

$$(7) \quad U_\lambda(b_j) = \alpha + \beta \gamma^{N-1} (b_j - \underline{v})^{N-1} [\lambda(v_j - b_j)]^{r_j}.$$

Statements (6) and (7) imply

$$(8) \quad U'_\lambda(b_j) = \lambda^{r_j} U'(b_j).$$

Now assume that we are modeling difference preferences [replace  $U(\cdot)$  with  $\tilde{U}(\cdot)$ ] as in Sarin (1982). Then, preference interpretations are defined for

$$(9) \quad \tilde{U}'_\lambda(b_j) = \lambda^{r_j} \tilde{U}''(b_j)$$

and

$$(10) \quad \tilde{U}'_\lambda(b_j^*) - \tilde{U}'_\lambda(b_j) = \lambda^{r_j} [\tilde{U}(b_j^*) - \tilde{U}(b_j)].$$

<sup>4</sup>The term  $\gamma^{N-1}(b_j - \underline{v})^{N-1}$  is the probability that agent  $j$ 's bid is the winning bid if all of his or her rivals bid according to the CRRAM equilibrium bid function in statement (1). Statement (8) implies that agent  $j$ 's optimal bid in the presence of the multiplicative payoff transformation is given by statement (1) if  $G(b) = \gamma^{N-1}(b_j - \underline{v})^{N-1}$ . Therefore, bid function (1) has the Nash equilibrium best-reply property for all positive values of  $\lambda$ . An alternative (longer but more rigorous) derivation of this result would follow the logic of the derivation in Cox, Smith, and Walker (1982).

Note that the multiplicative payoff treatment has three related effects. Increasing  $\lambda$  provides a scale increase in subjects' monetary payoffs. Statement (9) shows that increasing  $\lambda$  increases the curvature of expected "utility" functions and thus addresses the "flat-maximum" property discussed by Harrison (1989). Alternatively, statement (10) shows that increasing  $\lambda$  provides a scale increase in the forgone expected "utility" from any given bid deviation,  $b_j^* - b_j$ . Finally note that statement (8) indicates that CRRAM predicts that varying  $\lambda$  will have no effect on bids (if  $b_j^*$  equates the first derivative to zero for any  $\lambda > 0$ , it does so for all  $\lambda > 0$ ). In fact, one can show that power-function (or log-linear) utility is necessary for this invariance; hence, the multiplicative-transformation treatment discriminates between CRRAM and other models.

We reported experiments in which  $\lambda$  was increased from 1 to 3 in Cox, Smith, and Walker (hereafter CSW) (1983a,b, 1984, 1985b, 1988). Experiments in which  $\lambda$  is increased from zero to as high as 20 are reported in Smith and Walker (1992b). All of these experiments produced auction market prices and individual subjects' bids that are significantly greater than risk-neutral ones. The earlier ( $\lambda = 1, 3$ ) experiments revealed no significant effect on bids from changing  $\lambda$ . The later experiments with higher values of  $\lambda$  revealed insignificant effects on bids, in the direction of further movement *above* risk-neutral bids (Smith and Walker, 1992b).

Thus our research program had previously provided an empirical answer to the (empirical) question that Harrison attempted to address with his metric calculations. The answer was that low opportunity cost, or low expected-utility-function curvature ("flat maxima"), did *not* explain the consistent tendency of subjects to bid in excess of risk-neutral bids.

If low opportunity cost cannot explain higher-than-risk-neutral bids, then what can? Bidder risk aversion is one explanation, since Nash equilibrium bidding theory predicts that a risk-averse bidder will bid more than a risk-neutral bidder in a first-price buyers' auction.

When we began our research program on auctions, the only available Nash-equilibrium alternative to the risk-neutral model was the identical-bidders concave (risk-averse or risk-neutral) model developed by Charles Holt (1980), John Riley and William Samuelson (1981), and Milton Harris and Artur Raviv (1981). Early in our research program, however, we found significant differences among the bid-value relationships for individual bidders, a result that is inconsistent with the identical-bidders concave model. As a response to these falsifying observations, we developed a heterogeneous-bidders model, CRRAM, as an alternative generalization of the risk-neutral model (CSW, 1982). We subsequently developed the log-concave model as a generalization of the risk-neutral model, the identical-bidders concave model, and CRRAM (CSW, 1988).

Although CRRAM does a surprisingly good job (for a parametric model) in organizing the data from first-price auctions, there are several tests that are inconsistent with it (CSW, 1984, 1985b, 1988).<sup>5</sup> Most of these CRRAM-inconsistent observations are consistent with the log-concave model; but some of these observations appear to be related to questions about subjects' motivation and preferences. As a response, we developed and tested bidding models specifically designed to study these questions.

### *B. Bidding Models with Nonstandard Motivation and Preference Hypotheses*

We have introduced bidding models that incorporate systematic violations of Bayes' rule (CSW, 1983b), nonlinear subjective expected values (CSW, 1985b), utility of the (event of) winning the auction (CSW, 1983b, 1988), and income-threshold utility (CSW, 1988). In each case, development of the new model was a response to observations of bidding behavior by some subjects that could

not be rationalized by bidding models based on standard hypotheses. Each model has testable implications, and we designed many experiments to test them. The interested reader is directed to our cited papers for a complete development. We will here review one of our models that was developed specifically for investigating the motivational implications of subjects' payoffs.

We explained above that we developed the CRRAM and log-concave models in order to provide equilibrium bidding models that are consistent with the robust tendency of subjects' bids to (a) exceed risk-neutral theoretical bids and (b) exhibit heterogeneous (across subjects) bid/value relationships. CRRAM attributes higher than risk-neutral bids to risk aversion. In contrast, the general M-parameter log-concave model can attribute such bids to risk aversion or any other bidder characteristics that are consistent with log-concavity and M-parameter representability. Early in our research program, we also developed alternative (to CRRAM) specific models that attribute high bids to utility of (the event of) winning the auction rather than to risk aversion. One such model was introduced in Cox, Roberson, and Smith (1982) and tested in CSW (1983b).

One of the models that we developed in CSW (1988) was the parametric model that we dubbed CRRAM\*. It is a testable model that (a) provides an alternative (to risk aversion) explanation of higher than risk-neutral bidding and (b) is consistent with the "throwaway bid" phenomenon. CRRAM\* attributes higher than risk-neutral bids to risk aversion or utility of the event of winning the auction (as distinct from utility of the monetary payoff from winning). This model attributes "throwaway bids" to subject preferences that exhibit income thresholds.

What are "throwaway bids"? Some subjects in first-price auction experiments, upon drawing a low value from a discrete uniform distribution, enter a bid at (or near) zero, or less frequently bid at, near, or even (intentionally) above value. Our interpretation from the beginning has been that at low values (below 20 percent of maximum value, or \$2 on a scale of \$10) this minority of

<sup>5</sup>Some further anomalies result when CRRAM is extended to multiunit discriminative auctions (CSW, 1984, 1985a).

subjects simply do not take the auction seriously. They are unlikely to win or lose much in any case, so they may bid lower or higher relative to value than when they receive values above this cut-off point. We present examples of such behavior in CSW (1988). Of course, low values will occur, but standard models predict that subjects will bid the same, relative to value, right down to values near zero. Although a large majority of subjects in fact do this, a minority do not. In these cases it is obvious that the theory must be generalized to account for the broader range of observed demand behavior than can be predicted from standard models. Accordingly, we hypothesized (CSW, 1988 pp. 90–4) that, for some subjects, there were two special utility effects: (a) a utility of winning, parameterized by  $w_j \geq 0$ , which is distinct from the utility of monetary payoff; and (b) a threshold income, parameterized by  $t_j \geq 0$ , below which a utility loss occurs. In CRRAM\*, utility can be written in the CRRA form:

$$(11) \quad u_j(v_j - b_j + w_j - t_j) = \begin{cases} -[-(v_j - b_j + w_j - t_j)]^{r_j} & \text{for } v_j - b_j + w_j - t_j < 0 \\ (v_j - b_j + w_j - t_j)^{r_j} & \text{for } v_j - b_j + w_j - t_j \geq 0. \end{cases}$$

The derivation of a Nash equilibrium bid function would require agents to form expectations on the new parameters ( $w_i, t_i$ ) in addition to  $r_i$  for all of their rivals. The resulting model is not tractable (except by numerical methods). However, if agent  $j$  believes that all  $i \neq j$  will bid  $b_i = \beta_i v_i$ , where  $1/\beta_i$  has the cumulative density function  $\Phi(1/\beta)$  on  $(1, N/[N-1])$ , then the optimal bid rule for  $j$  is

$$(12) \quad b_j = \begin{cases} 0 & \text{for } v_j + w_j - t_j < 0 \\ \frac{N-1}{N-1+r_j} (v_j + w_j - t_j) & \text{for } v_j + w_j - t_j \geq 0. \end{cases}$$

Hence, if  $w_j - t_j > 0$ , the bid function has a positive intercept, and if  $w_j - t_j < 0$ , the bid function predicts zero bids for values less than the threshold,  $t_j - w_j$ .

A procedure, such as the one articulated above, is an appropriate way to proceed when subjects reveal via their message choices that their motivation diverges from that postulated by the original model. The new bid-prediction equation (12) is testable. We have designed experiments in which we add a lump-sum reward,  $w'_j$ , for winning to the cash reward,  $v_j - b_j$ , or alternatively deduct a lump-sum fee,  $t'_j$ , from the reward,  $v_j - b_j$ , when  $j$  wins the auction. We do this in an environment in which we control for the parameters ( $w_i, t_i$ ), for  $i \neq j$ , by using robot (simulated) bidders, as explained in Walker et al. (1987). The robots use the bid function  $b_i = \beta_i v_i$ . In some experiments  $\beta_i$  is the slope of the risk-neutral bid function,  $(N-1)/N$ , whereas in other experiments  $\beta_i$  is randomly drawn from the sample of estimates,  $\hat{\beta}_i$ , for all previous human subjects. In CSW (1988), we report tests of the predictive consequences of CRRAM\*, namely, that when  $w'_j > 0$  and  $t'_j = 0$ , the intercept of the observed bid function will increase relative to the baseline, and when  $w'_j = 0$  and  $t'_j > 0$ , the intercept of the observed bid function will fall relative to the baseline. Overall, we find that 35 of 45 cases conform to the predictions of CRRAM\*.

Our development and testing of CRRAM\* provides an example of how message-space observations that suggest subject characteristics that are not in standard theory can be studied in an internally consistent research program. This approach is in sharp contrast to that followed by Harrison. In implementing what he believes are the implications of his 1989 paper, Harrison has adopted the practice of testing theories by applying informal notions of "perceptive thresholds" to calculations of expected incomes. Consider, for example, the search experiments reported in Harrison and Peter Morgan (1990). They begin with predictions of several search models that do *not* include income thresholds. Experiments are then carried out to test the predictions of these standard models. How-



ever, in analyzing the data the predictions of *informal* threshold models are substituted for the predictions of the formal models. Harrison and Morgan (1990 p. 483) explain their procedure as follows: "We now consider how close the subjects came to the theoretically optimal search strategy for each problem. ... To give a meaningful response, we pose this question: how poor do our subjects' perceptual abilities need to be in order to reconcile the observed and theoretically predicted behaviours?" The problems with this informal approach to theory modification are apparent. The predictions of the formal search models are, in fact, not tested. The informal modifications of the models that are tested are based on an implicit assumption that income or perceptual thresholds produce *additive* differences in the predictions of search models. However, search models with thresholds have not been developed, and there is no reason to believe that their predictions would consist of additive deviations from the search models without thresholds. Hence, what one has in Harrison and Morgan (1990) is simply a method of data analysis that is inappropriate for drawing any conclusions about the search models that are ostensibly being tested.

### III. Kagel and Roth's Critique

Kagel and Roth (1992) argue that we attribute "overbidding" (meaning higher than risk-neutral bidding) entirely, to risk aversion. They also assert (p. 1379) that "...data inconsistent with risk-averse bidding are largely ignored in CSW (1988)." These allegations are false, because in CSW (1988) we develop and test a model that provides an alternative explanation of overbidding; it is the utility-of-winning model described in Section II above, which was directly motivated by anomalies we observed in testing CRRAM. Kagel and Roth also claim (p. 1380) that "...behavior satisfies CSW's theory when no change is required, but does not satisfy it when extensive changes are required..." This conclusion apparently reflects their belief that: (a) the data they present in their section III are

inconsistent with our theory; and (b) we have not conducted tests that require adjustments in behavior to satisfy the theory. In fact, Kagel and Roth's data analysis contains several crucial errors; this is explained below. Furthermore, we have presented results from experiments that require adjustments in behavior to satisfy the theory in five of our published papers; the references and a partial listing of the many experimental treatments are also given below. In the following subsections, we will respond to the comments in sections I–III in the Kagel and Roth (hereafter KR) paper.

#### A. KR's Section I

KR begin their Section I with a description of Harrison's metric calculations and reassert the conclusions that Harrison states about the experiments that he analyzes. We have already explained in Section I why the metrics cannot be used to test behavioral hypotheses. Friedman (1992) has explained why Harrison's metrics do not provide useful heuristics to aid in experimental design; but we are puzzled by KR's acceptance of Harrison's critique, as we will now explain.

Harrison's critique is based on his metric calculations with *median* bids. Our original tests that lead to rejection of the risk-neutral model used market prices, that is, only the *highest* bids (Cox, Roberson, and Smith, 1982). Further evidence that the highest bids consistently exceed those predicted by the risk-neutral model is presented in CSW (1988 pp. 69–71). It seems very strange that KR, at this point in their argument, accept Harrison's utility-metric evaluation of median bids, and reject our message-space evaluation of the highest bids, when in the balance of this section of their paper they excoriate us for the claimed sin of not using high bids.

KR (1992 p. 1381) assert that our "...investigation of CRRAM is based on private valuations for which the expected cost of deviating from equilibrium is the lowest." This is said to follow from the fact that "...as much as 25 percent of the private valuations drawn could be excluded in the analysis of CRRAM bid functions..."

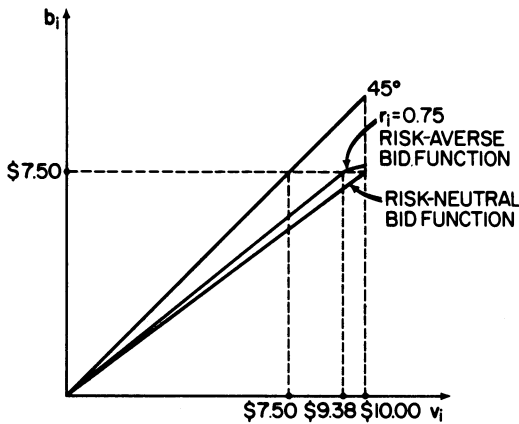


FIGURE 1. THE CRRAM RISK-NEUTRAL ( $r_i = 1$ ) AND RISK-AVERSE ( $r_i = 0.75$ ) BID FUNCTIONS FOR  $N = 4$

and to imply that "...Harrison's critique applies with special force to the CRRAM model." These statements simply reflect KR's misunderstanding of CRRAM, as we will now explain.

KR support their statements by claiming that the highest 25 percent of values and associated bids would be excluded when values are drawn from  $[\$0.01, \$10.00]$  and there are four bidders. Our Figure 1 shows the CRRAM risk-neutral ( $r_i = 1$ ) bid function for the  $N = 4$  bidders case and KR's correct identification of  $b^*$  (the highest bid of a risk-neutral bidder) as \$7.50. It also shows the CRRAM risk-averse bid function for  $r_i = 0.75$ . KR assert that the  $v_i$  between \$7.50 and \$10.00 would not be used in estimation of the linear part of the bid functions. In fact, only the  $v_i$  between \$9.38 and \$10.00 would not be used in estimating the linear part of the  $r_i = 0.75$  bid function.

In general, if the most risk-tolerant bidder is risk-neutral, then all values less than or equal to  $(N - 1 + r_i)\bar{v}/N$  would be used in estimating the linear part of the CRRAM bid function for the bidder with risk-aversion parameter  $r_i$ .<sup>6</sup> We have previously ex-

plained the correct procedure for use of the data in CSW (1988 table 4) and in Walker et al. (1987 p. 241; 1990 p. 12, footnote 3, and tables 1, 2, and 4). That explanation immediately implies the following: if values are drawn from the uniform distribution on  $[0, \bar{v}]$  and the most risk-tolerant individual has risk parameter  $\bar{r}$ , then the percentage of observations excluded from the linear part of the bid function for bidder  $i$  is

$$(13) \quad \frac{100}{\bar{v}} \left( \bar{v} - \frac{N-1+r_i}{N-1+\bar{r}} \bar{v} \right) \\ = 100 \frac{\bar{r} - r_i}{N-1+\bar{r}}.$$

For the case considered by KR ( $N = 4$ ,  $\bar{v} = \$10.00$ ), 12.5 percent of the observations would be excluded if the revealed values of  $r_i$  were uniformly distributed on  $[0, 1]$ . Of course, the revealed values of  $r_i$  are not so distributed and, as a result, fewer observations are excluded from the linear part. For the  $N = 4$  experiments reported in Walker et al. (1990 table 2), 5.7 percent of the observations were excluded. With the  $N > 4$  experiments (reported in our other papers) a smaller percentage of the observations were excluded because the risk-neutral and risk-averse bid functions move closer together as  $N$  increases. Furthermore, whenever some of our observations were excluded (as required by the theory) in estimating linear bid functions, we reestimated them with all of the data as a heuristic check on whether the estimates of the slope parameters  $(N - 1)/(N - 1 + r_i)$  and, hence, of the risk-aversion parameters  $r_i$  were affected in any notable way. They were not so affected. Cox and Ronald Oaxaca

cient of constant relative risk aversion for the least risk-averse (or most risk-preferring) individual in the population. Then, the CRRAM equilibrium bid function has a linear part,  $b_i = (N - 1)v_i / (N - 1 + r_i)$ , and a nonlinear part that does not have a simple closed form (CSW, 1982). The linear part applies to  $v_i \leq (N - 1 + r_i)\bar{v} / (N - 1 + \bar{r})$ , and the nonlinear part holds for larger values.

<sup>6</sup>Let auctioned-object values be drawn from the uniform distribution on  $[0, \bar{v}]$  and let  $\bar{r}$  be the coeffi-

(1992) develop and apply a spline-function technique that uses *all* of the data in CSW (1988) to estimate the parameters of both parts of CRRAM bid functions. The results indicate that CRRAM organizes all of the data quite well. This finding provides yet further evidence of the error in KR's claim that exclusion of data implies that Harrison's critique applies with special force to CRRAM.

In our nonparametric test of the disputed risk-neutral special case of CRRAM, we do not exclude any of the bid data (CSW, 1988 pp. 70–2). Some observations were excluded only when we estimated the parameters of the linear part of CRRAM bid functions. Furthermore, in most of our pre-1988 papers the analysis was based solely on auction market prices (i.e., only the highest bids). Therefore, KR's assertion that our analysis of bid data uses only bids by the least-motivated subjects is false.

We now turn our attention to KR's table 1 and their accompanying discussion. They report mean proportionate absolute bid deviations (from risk-neutral bids) for the lowest, middle, and highest 20 percent of the bids. They observe that there appears to be an inverse relationship between  $v_j$  and their mean-deviation measure and conclude that this provides support for their reformulation of Harrison's critique. However, the fact that the proportionate bid deviations are somewhat lower in the highest 20 percent of the values than in the middle 20 percent of the values is consistent with a concave nonlinear upper segment for the CRRAM bid function (see Fig. 1). These observations support (rather than violate) CRRAM, and KR are to be commended for proposing this test. The comparison between the lowest and the middle 20-percent figures is inconsistent with CRRAM and all other bidding models based on standard assumptions. This reflects the "throwaway" bid and utility-of-winning phenomena that we (CSW, 1988 pp. 77–8) and other experimentalists, have previously reported. In fact, we have not only reported the phenomena, but also have extended bidding theory to accommodate them and then tested the ex-

tended theory (CSW, 1988 pp. 90–6);<sup>7</sup> see Section II, above.

### B. KR's Section II

This section again incorrectly imputes to us the position that risk aversion is the only factor in bidding above the risk-neutral Nash equilibrium (RNNE) bid function. Now the evidence that is provided to dispute this "straw man" is in the form of bidding in excess of value in dominant-strategy auctions, where risk aversion is not a factor. This is a phenomenon on which we have reported extensively in CSW (1985a). (We have reported experiments both with and without a restriction on bidding above value.) Again, as in first-price auctions, this anomaly could be addressed with a utility-of-winning model (or a decision-cost model as in Smith and Walker [1992a]). Of particular interest would be the introduction of a utility of winning in first-, second-, and third-price auctions with the objective of getting a stronger test of this hypothesis than we were able to construct for first-price auctions alone. (We have proposed this extension to Kagel and Dan Levin for their interesting working paper [Kagel and Levin, 1988].)

<sup>7</sup>In Smith and Walker (1992a), based on a simple effort-cost model of decision-making, it is shown that

$$\frac{d \text{Var} \varepsilon_j(t)}{d v_j(t)} < 0$$

where  $\text{Var} \varepsilon_j(t)$  is the variance of individual  $j$ 's bid in auction  $t$  relative to the CRRAM risk-averse optimal bid,  $b_j^*(t)$ , given by (1) when value is  $v_j(t)$ . Thus, independently of any income-threshold phenomena, individual error variance around  $b_j^*(t)$  declines with higher values for given  $\lambda$ . This is because, in the model, the higher expected utility when values are higher results in increased opportunity cost of a nonoptimal decision; thus, more decision effort is expended, and decision error is reduced. Consequently, proportional bid deviations from the RNNE are predicted to decline with value in KR's table 1 for the linear portion of the CRRAM bid function. (Joyce Berg and John Dickhaut [1990] also use an error-incentives model to account for the preference-reversal phenomenon.)

Next, KR challenge our interpretation of bidding above RNNE when the risk-neutralizing lottery procedure is used to pay subjects. First, it should be noted that this was a joint test of CRRAM and the lottery procedure. Second, we carefully screened subjects so that they were especially consistent with CRRAM (insignificant intercepts, high  $R^2$ ) as a means of giving the lottery procedure its best shot. The results were quite decisive and very disappointing for the lottery procedure, so we next went with inexperienced subjects. They did better, with 50 percent of the subjects bidding in a way that was not significantly different from RNNE; this is, more than twice the baseline number with monetary rewards (Walker et al., 1990 p. 15). Furthermore, those bidding as if they were risk-averse were shifted toward risk-neutrality. This compares fairly well with results reported by Thomas A. Rietz (1991) for inexperienced subjects. However, when we retested most of the subjects they did not stay relatively risk-neutralized, whereas with monetary rewards 80 percent of the subjects remain stable when retested.<sup>8</sup> In fact, many bid as if they are risk-preferring, an extremely rare occurrence with monetary rewards. (To our knowledge no one else has retested subjects with the lottery procedure.) These treatment effects from the lottery procedure relative to baseline suggest that the lottery procedure is in question, not CRRAM, which yielded more stable comparisons between inexperienced and experienced subjects.

What accounts for the deviant behavior induced by the lottery procedure? Since the lottery procedure adds an additional compounding of elementary gambles, and since there is independent evidence that the predictive power of expected utility deteriorates markedly with higher compounding of

<sup>8</sup>Smith and Walker (1992b) vary experience through four levels with a relatively large sample (154 subject-experiments of 25 auctions each). Second and higher levels of experience increase the bid levels relative to inexperienced subjects, but not significantly by all measures.

gambles (Colin Camerer and Teck-Hua Ho, 1990), it is natural to suspect that what is failing when the lottery procedure is introduced is the compound-lottery axiom. KR argue (p. 1383) instead that "... these data are inconsistent with the risk-aversion hypothesis... [because]... for *expected-utility maximizers* the binary lottery technique *must* be capable of controlling risk preferences." KR's statements are inconsistent with the fact that expected-utility theory can be well-defined and empirically valid on a set of simpler gambles but fail empirically on a set of more complex gambles. (KR argue the untenable case that if utility theory fails on any part of the domain of gambles, it necessarily fails on all.) Literally, what fails is the conjunction of CRRAM with an additional compounding of payoff uncertainty. We leave it at that for future experimentation and examination, and for the reader to contemplate. We regret that the results were not more favorable to this procedure because, if it did work, it would be without question an important experimental tool.<sup>9</sup> Roy Radner and Andrew Schotter (1989) tried the procedure in conjunction with their

<sup>9</sup>Roth (who has used the procedure extensively) has never, to our knowledge, tested the procedure; he has only applied it on the assumption that it would risk-neutralize subjects (see Roth and Michael W. K. Malouf [1979] and the references in KR to Roth's work in bargaining). Roth's introduction of the procedure (first proposed by Cedric Smith [1961]) to experimental bargaining was a very innovative contribution since it permitted the (utility) information conditions that are believed to underlie the Nash bargaining model to be satisfied. If Roth had conducted the experiments both with lottery payoffs and without them (e.g., with direct monetary rewards) he could have learned whether the lottery treatment made any difference. Since earlier work had used money, and Roth reports similar findings with the lottery procedure, one must suppose that the procedure has no treatment effects in the context of bargaining. This is in line with the sealed-bid bargaining result of Radner and Schotter (1989). When we introduced lotteries in first-price auctions they were employed under those conditions most favorable to CRRAM, not under the conditions for which we had reported that CRRAM fails (the multiple-unit discriminative auctions in CSW [1984]; significantly nonzero bid-function intercepts and the use of quadratic and square-root monetary transformations in CSW [1988]).

model of sealed-bid bargaining and found that it did not yield the predictions of risk-neutral bargainers. Rietz (1991) reports more favorable comparative results when he first “trains” subjects in second-price auctions using the procedure before exposing them to first-price auction rules (but note that Rietz does not permit bidding above value, which is a procedure that KR [1992 p. 1382] criticize in our work). Rietz’s samples are very small and have not been replicated, but his procedures show promise and deserve further examination. For example, Rietz’s procedure of not allowing bids above value (Rietz, 1991 p. 33) in both his first- and second-price auctions, with monetary and with lottery payoffs, may be crucial to his result (which is not a criticism).

We have not conducted exercises comparing bids above RNNE with any of the other well-known methods for measuring risk aversion because of the extensive and expensive study of this matter by W. V. Harlow (1988). He compared the following five instruments for individual (*not* pooled) measures of risk aversion using a large sample of subjects: bid deviations from RNNE, choice between risky gambles, two psychological survey tests, and a measure constructed from a biochemical blood-sample test. These measures are significantly and positively correlated, showing that CRRAM “as if risk-averse” subjects are similarly risk-averse by other measures. Results from this research program are also reported in Harlow and Keith Brown (1990a, b).

KR accuse us of a methodology that exhibits a strong prior belief in CRRAM—so strong that they allege that we are not sensitive to contrary evidence. Yet it is precisely the evidence contrary to CRRAM that led us to formulate two extensions of CRRAM and the generalized log-concave model (CSW, 1988). It is correct, however, for KR to maintain that their methodology and data analysis stand in sharp contrast with ours. We welcome the opportunity to continue their discussion of this contrast, and we will present our viewpoint under three heads.

1. *Data Analysis*.—Throughout KR’s paper and in almost all the auction work of

Kagel (the exception is Kagel and Levin [1985]), data across individuals are pooled; data sets by individual are not analyzed and tested so that one can say something about distributions of individual decision-making characteristics relative to the predictions of a theory. However, propositions about individual behavior do not apply to *distributions* of individuals unless all individuals are identical. RNNE theory assumes that all individuals are identical. Thus, in first-price auctions, RNNE theory predicts not only that  $j$  will bid  $b_j = (N-1)v_j/N$ , but that this is true for all  $j$ . Therefore, as in CSW (1988), we answer two priority questions: Do subjects bid as if they are all risk neutral? If not, do they bid differently from each other? We answer these questions with nonparametric tests of individual bid deviations from risk-neutral behavior and report data on distributions of these statistics across individuals (CSW, 1988 pp. 71–4). These tests do not assume the special properties of CRRAM, which invites other more specific tests. As a result the reader can determine what proportion of the subjects deviate from RNNE strategies and in which direction (CSW, 1988 table 2) and whether the individuals in each experiment are bidding as if they use the same strategies or distinct strategies (CSW, 1988 table 3). Only after these tests are reported do we test the special properties of CRRAM. The remaining anomalies led us to explore theoretical variations on it.

In contrast with our approach, KR’s tables 1–4 present pooled data, from which one cannot determine whether (a) most bidders are close to the RNNE strategy, but aggregate deviations are large because of a few outlying subjects, or (b) most bidders deviate from RNNE bidding. Indeed, the pooled data might have been close to the RNNE, with individuals bidding much higher than RNNE offsetting those bidding much lower. Proper analysis of individual data must allow for the possibility that observed bid deviations from the prediction are of the form  $\hat{d}_j = \hat{b}(v_j, \theta_j) - b^*(v_j)$ , where the observed bid of  $j$ ,  $\hat{b}$ , depends not only on value  $v_j$ , but on some individual



characteristic  $\theta_j$ . Consequently the distribution of  $\hat{d}_j$  depends upon the distribution of  $\theta_j$  in the sample of bidders and not only on the distribution of  $v_i$ .

2. *Perceptual Errors*.—KR (1992) have no explanation of why we, they, Harrison, and others, all observe the well-documented deviations of subject bids from the RNNE, other than to attribute it to “perceptual errors” (p. 1382) or “bidding errors...of one sort or another” (p. 1384). These phrases provide neither a theory nor an explanation, but rather only names for the observed phenomenon. How do they propose to model these perceptual errors? Why are perceptual errors always predominately positive? We have offered (CSW, 1982, 1988) a class of models that predict deviations from RNNE in terms of risk aversion (log-concave model and CRRAM) and models that predict them in terms of a utility of winning and threshold utilities (CRRAM\* and CRRAM\*\*). As we have documented, these modifications account for some but not all of the anomalies.

3. *Theory Modification in the Light of Experiment*.—In our view, the function of empirical research is to motivate the modification of theory in the light of evidence. Theories typically account for some tests but fail to account for others. Thus, both Newton’s and Einstein’s theories were “born refuted” in the sense that falsifying observations existed when the theories were originally constructed (Irme Lakatos, 1980 pp. 5, 39). Newton’s theory was not immediately abandoned simply because (as was known in 1687) it was inconsistent with the observed orbit of the moon; it was, instead, replaced by relativity theory when the latter produced a more progressive research program (Lakatos, 1980 Ch. 1). Relativity theory has not been abandoned in response to falsifying observations because of the absence to date of a substitute theory leading to a more progressive research program.

In order to modify economic theory appropriately, one needs to know not only where the theory fails, but also where the theory does not fail. In this way one knows which predictive consequences of the theory need to be preserved and which need to be

altered.<sup>10</sup> Our judgment is that private-value auction theory is doing well enough relative to the data that it would be premature to replace it without further attempts to modify it in testable ways so as to reduce the anomalous cases. As explained by Lakatos (1980 p. 35), “There is no falsification before the emergence of a better theory.” The choice is necessarily between better and worse theories, not between some specific theory and the void. KR want us to reject auction theory because—as with all theories—there are anomalies, but they are not offering us a coherent substitute theory.

Our log-concave model (CSW, 1988) is consistent with a wide range of possible bidding patterns. The model allows bidders to differ from each other in any way that can be represented by a finite number of parameters. Thus, if  $\theta_i$  is the  $(M - 1)$ -vector of agent  $i$ ’s characteristics then agent  $i$ ’s utility of money income  $y_i$  can be written as  $u_i(y_i) = u(y_i, \theta_i)$ . The curvature of  $u(y_i, \theta_i)$  in  $y_i$  is restricted only to be log-concave. Thus, bidders can be risk-averse, risk-neutral, or risk-preferring. Furthermore, a given bidder can be risk-averse for some gambles and risk-neutral or risk-preferring for others. If this model has a problem, it is that it is consistent with too much data; it is so general that it has few testable implications. However, it provides a general framework in which equilibrium bid functions are known to exist for more restrictive models with more testable implications. We have developed and tested some more restrictive models, including CRRAM. If KR do not approve of those models, then the log-concave model provides them with a general framework in which to search for alternative formulations.

<sup>10</sup>An example of this methodology is illustrated by Berg and Dickhaut (1990), who use an error-incentive model to show how decision error in the preference-reversal phenomenon is decreased when incentives are increased. They show empirically that, as incentive-induced forgone expected utility increases, error rates decrease. They also show that this modification of standard theory better accounts for the data than alternative approaches that depart more fundamentally from preference theory.

C. KR's Section III

KR (1992) analyze bid data from the first-price auction experiments reported in Douglas Dyer, Kagel, and Levin (hereafter DKL) (1989) and in Raymond C. Battalio, Carl A. Kogut, and Jack Meyer (hereafter BKM) (1990). KR (p. 1386) state that "The primary point of Harrison's (1989) paper is that... bidders with lower resale values have sharply reduced financial incentives..." KR (p. 1386) argue that this "primary point" is illustrated by data from the DKL experiment which shows that some bidders with relatively low resale values may bid zero, full resale value, or even more than full resale value. We do *not* agree with KR's assertion of Harrison's "primary point." We do agree that some subjects bid differently when they receive resale values from the low end of the support of a known distribution than when they receive higher resale values. We documented such behavior in CSW (1988) under the rubric, "the throw-away bid phenomenon." More importantly, as explained in Subsection II-B above, we extended bidding theory to accomodate this type of behavior and then tested the extended theory in CSW (1988).

The DKL and BKM experiments differ from Harrison's experiments by using larger values for  $\bar{v}$  (the highest possible induced value) while keeping  $\underline{v}$  (the lowest possible induced value) approximately the same (KR, 1992 p. 1384 and table 1). The implication of this procedure for bidder opportunity costs can be understood by substituting from our equation (3) into (6) and then finding the expression for the difference,

$$\begin{aligned}
 (14) \quad & \tilde{U}(b_j^*) - \tilde{U}(b_j) \\
 &= \beta \left[ \frac{N-1 + E(r)}{(N-1)(\bar{v} - \underline{v})} \right]^{N-1} \\
 & \times \left\{ (b_j^* - \underline{v})^{N-1} (v_j - b_j^*)^{r_j} \right. \\
 & \quad \left. - (b_j - \underline{v})^{N-1} (v_j - b_j)^{r_j} \right\}.
 \end{aligned}$$

Statement (14) has a preference interpretation if we use a difference preference approach, as in Sarin (1982). Note that the right-hand side of (14) is a decreasing function of  $(\bar{v} - \underline{v})$ ; that is, increasing  $(\bar{v} - \underline{v})$  *decreases* the opportunity cost of any given bid deviation,  $b_j^* - b_j$ . Thus, KR are mistaken in thinking that the DKL and BKM experiments are responsive to Harrison's conjecture about low opportunity cost of bid deviations (or "flat maxima"). In fact, these experiments have by far the flattest maxima of all those being discussed in the present controversy and are contrary to Harrison's implication that flatter maxima increase deviations from RNNE.

KR's tables 2 and 3 report bid deviations and forgone expected income for median bidders and high bidders. The first thing to note about these tables is that the reported forgone-expected-income figures are incorrect. This follows from the fact that these auctions were part of "dual-market" experiments in which a subject simultaneously enters bids in two or more markets and then coin flips or other random devices select one market for monetary payoff. KR explain why they did not report the actual expected-income figures in their footnote 12. This footnote states that tests show "...no systematic behavioral difference under dual-market as compared to single-market procedures in private-value auctions." That statement has an interesting implication that KR do not mention. All expected-income and expected-forgone-income figures are decreased substantially by introduction of the random market-selection procedure. If this has no effect on bidding behavior, then this provides a direct test which yields results that are inconsistent with Harrison's conjecture.

Another implication of the random selection procedure is that the forgone-expected-income figures reported in KR's tables 2 and 3 are greatly overstated because they do not reflect the market-selection probabilities. Whatever one may think about the usefulness of such calculations, it is clear that any accurate comparison of expected-forgone-income figures across experiments must be based on the *actual*

probabilities that the monetary payoffs will be received. BKM (1990 p. 99) reported that the probability that one of their  $n = 5$  dual markets would be selected for monetary payoff was 0.25. KR (1992 footnote 12) state that the dual-market approach was used in experiment A&M<sub>1</sub>, that it was not used in A&M<sub>2</sub>, and that it was used in some periods of A&M<sub>3</sub>. Therefore, the median forgone-expected-income figures in the fourth (A&M<sub>1</sub>) rows of KR's tables 2 and 3 should be multiplied by 0.25; the figures in the fifth (A&M<sub>2</sub>) rows should not be changed; and the figures in the last (A&M<sub>3</sub>) rows should be adjusted by an unknown proportion. DKL (1989 p. 270) reported that in their experiments the high bidder would earn profits under only one of two bidding procedures, to be determined by a coin flip, after which one of two markets would be selected for monetary payoff by a second coin flip. Thus, the probability that any subject's decision would have monetary payoff implications in a DKM experiment was 0.25. This implies that the median forgone-expected-income numbers in the first three rows of KR's tables 2 and 3 must be multiplied by 0.25 to get the actual figures. Making these corrections yields the following figures in place of the ones reported in the first five rows of the median-forgone-income column of KR's table 2: 0.076, 0.036, 0.059, 0.071, and 0.045. These median expected-forgone-income figures are about the same as those reported by Harrison (1989 table 3) for his experiments; they are about 33 percent of the corresponding figures for the  $N = 4$ ,  $\lambda = 3$  experiments of CSW (1983a, b, 1984, 1985b, 1988); and they are about 5 percent of the corresponding figures for the  $N = 4$ ,  $\lambda = 20$  experiments of Smith and Walker (1992a, b). Thus, the experiments used by KR have median expected forgone incomes that are among the lowest of all those being discussed in the present controversy.

KR claim that the numbers reported in their table 4 are inconsistent with CRRAM. In fact, there is nothing in their paper that supports that conclusion. The reason for this is that KR use an incorrect procedure in analyzing their data. Their analysis is based entirely on the use of a

linear bid function (KR, 1992 footnote 15). However, their data include unedited observations from the upper part of the bid function. More importantly, their conclusions are about *expected* profits, corresponding to observed average profits, but KR have not carried out an expected-profit calculation. The simple formula that they use to analyze their data (see their footnote 15) has no valid implication for this question. Correct expected-profit calculations for these bid functions involve the use of order statistics, as explained in Cox, Roberson, and Smith (1982 pp. 12–13) and CSW (1985 pp. 184–8).

KR (1992 p. 1388) concede that the tests they report in their table 4 "...are a bit informal..." and then continue by stating that "...more formal tests...yield the same result (Kagel and Levin, 1985)." Note that the "informal" tests in KR's table 4 are concerned with the effects of varying  $N$ , the number of bidders, on the ratio of observed to risk-neutral theoretical profits; but all of the experiments reported in Kagel and Levin (1985) are for  $N = 6$ . Thus, it is impossible for this claim about "more formal tests" to be true.

KR (1992 p. 1387) claim that "evidence" is provided in Kagel and Levin (hereafter KL) (1985) and Kagel, Ronald M. Harstad, and Levin (hereafter KHL) (1987) that subjects act as if they are relatively more risk-averse when there are higher profits to be earned as a consequence of increasing  $V_H$  (or, in our notation,  $\bar{v}$ ), the upper bound of the support from which the values of auctioned objects are drawn. We have not been able to find the "evidence" relating profits to relative risk aversion which they assert is in KL (1985) and KHL (1987). The only test results relating profits to relative risk aversion that we can find in KHL (1987) are those in their table VI. That table reports on tests that use data on the mean of the uniform distribution of values ("public information") to test the special case of CRRAM in which all bidders are assumed to have the same risk-aversion parameter. They refer to this special case as the "CRRAM model" and state the following (KHL, 1987 pp. 1293–4): "Comparing the predicted impact on revenue in Table VI with the risk-

neutral predictions in Table IV shows that the revenue-enhancing effects of public information are sharply curtailed on the basis of the degree of risk aversion observed. In fact there is a slight difference, averaged across all auction periods, between the average predicted revenue increase of 15 cents per auction period under the CRRA bidding model and the observed increase of 22 cents per auction." This conclusion in KHL (1987) seems to be inconsistent with KR's assertions. We cannot find any other test results on relative risk aversion in KHL (1987).

KL (1985) do present other test results on CRRAM. KL use data from the "public-information" periods of five out of the seven first-price auction experiments reported in KHL (1987). In all of these experiments, the number of bidders,  $N$ , equals six. The experimental treatments within the public-information periods are random variation in  $v_0$ , the mean of the value distribution, and time-trended variation in  $\varepsilon$ , which is half the range of the support of the value distribution. Thus, the support  $[\underline{v}, \bar{v}]$  for the uniform distribution of values can be written as  $[v_0 - \varepsilon, v_0 + \varepsilon]$ , and the linear part of the CRRAM bid function in equation (1) above can be rewritten as

$$(15) \quad b_j = v_0 - \varepsilon + \frac{N-1}{N-1+r_j}(v_j - v_0 + \varepsilon).$$

KL (1985) use ordinary least squares to estimate

$$(16) \quad b_j = \alpha_0 + \alpha_1 v_j + \alpha_2 v_0 + \alpha_3 \varepsilon$$

and then test the parameter restrictions implied by (15):  $\alpha_0 = 0$ ,  $\alpha_2 + \alpha_3 = 0$ ,  $\alpha_3 + 1 - \alpha_1 = 0$ . They report  $F$ -test results with a high percentage of the estimated parameters not satisfying the parameter restrictions.

There are two flaws in this KL (1985) test of CRRAM: (a) an incorrect application of bidding theory; and (b) a time-trended treatment variable in the experiments. We will explain both of these flaws.

KL (1985) use three different assumptions about the relative risk-aversion parameter of the least risk-averse bidder in the population,  $\bar{r}$ . An assumed value of  $\bar{r}$  and the highest possible value,  $v_0 + \varepsilon$ , imply the amount of the highest bid that the (assumed) least risk-averse bidder will ever make,  $b_{\bar{r}}^*$ . KL then truncate their sample by deleting all observations such that  $v_j > b_{\bar{r}}^*$ . Thus, KL (1985) make the same mistake in testing CRRAM that KR (1992) do in discussing it. See Subsection III-A, above, for an explanation of why this is an incorrect use of the data.

The time-trended treatment variable in the experiments analyzed in KL (1985) and KHL (1987) creates serious problems for interpreting the data from these experiments. In some experiments, the subjects learn from experience and adjust their behavior as the experiment is in progress. If different treatments are introduced during the experiments, their effects can be confounded with learning and sequencing effects. Experimentalists have devised various ways to deal with this potential problem, including the use of ABA designs (to see whether the subjects "return to baseline"), and inverting or randomizing treatment orders. None of these controls or checks for learning and sequencing effects were used in the experiments reported in KL (1985) and KHL (1987). This is a problem, because their key treatment variable  $2\varepsilon$ , the range of the value distribution, is time-trended. In two out of their seven first-price auction experiments,  $\varepsilon$  weakly monotonically increases from 6 to 24 throughout the experiments. In the other five experiments,  $\varepsilon$  weakly monotonically increases from 6 to 24 during the first 22 or 23 periods and then drops to 12 in the last two periods. There are only two observations for each subject for the final  $\varepsilon = 12$  treatment level; hence it is not possible to test whether the subjects returned to baseline (bid the same as in the earlier  $\varepsilon = 12$  periods). This means that the effects of the  $\varepsilon$  treatment are confounded with any learning and sequencing effects in these experiments. This produces serious interpretation problems. For example, when Cox and Oaxaca (1992) used the simple



check of introducing a time-trend variable (period number) into a KL (1985) regression equation, large numbers of subjects were transferred from the apparent CRRAM-inconsistent category to the CRRAM-consistent category.<sup>11</sup>

KR end their section III by asserting that the results of our (earlier) multiplicative-payoff experiments are not inconsistent with Harrison's (later) conjecture about low forgone payoffs. The support they offer consists of (a) a claim that Harrison's critique postulates unspecified "uncontrolled factors" that we did not vary (KR, 1992 p. 1388) and (b) a story about a 300-pound gorilla who plays golf (KR, 1992 footnote 16). First, Harrison (1989 pp. 751-4) did *not* put forth vague arguments about unspecified "uncontrolled factors"; instead, his critique is clearly about some specific measures of forgone payoffs in experiments. In addition, the reader can easily see from our equation (10) above that our multiplicative-payoff experiments varied all increasing measures of forgone payoffs. Second, KR's interpretation of the gorilla story is not helpful for understanding scientific method. If one were to apply that interpretation in physics he would conclude that the famous Michaelson-Morley experiment is of little or no value in discriminating between ether theory and relativity theory because the latter predicts no change in the experiment and none was observed.<sup>12</sup>

<sup>11</sup>Cox and Oaxaca (1992) explain that KL's experimental design for testing CRRAM is an unusual type of "boundary experiment" (Smith, 1982 p. 942). Ordinarily, in testing an *equilibrium* theory, experimentalists hold constant the demand, supply, or bid functions for several periods in order to give the subjects time to "find" the equilibrium. In contrast, KL shift the bid function (in bid-value space) every period through their random variation of  $v_0$  and time-trended variation of  $\epsilon$ . This procedure maximizes the chances of observing disequilibrium "hysteresis effects" (Douglas Davis et al., 1991) in the experiments. KL's test then requires that the subjects respond such that the estimated parameters for  $v_0$  and  $-\epsilon$  are not significantly different from each other.

<sup>12</sup>KR's gorilla story does not contain the salient characteristics of a bidding experiment. To see this, consider the following alternative. A gorilla will play  $T$  holes of golf. The fairway on which she plays each hole

We want to end by making sure that any readers who are unfamiliar with our papers are not misled by the last three paragraphs in KR's (1992) section III and the fourth paragraph in their section IV. All of that discussion is based on KR's (p. 1389) assertion that their informal tests of CRRAM, based on varying  $N$  and  $\bar{v}$  (or  $V_H$ ), are "more demanding" than our tests based on multiplicative payoffs because the former "requires substantially greater adjustments in behavior to remain faithful to CRRAM." KR claim that the alleged difference between their "more demanding" tests and our tests reflects "an important methodological difference" (p. 1389). We are pleased that KR approve of tests based on varying  $N$  and  $\bar{v}$ , since that was a principal focus of the first paper in our research program on auctions (Cox, Roberson, and Smith, 1982). That paper reports experiments in which  $N = 3, 4, 5, 6,$  and  $9$ , and  $\bar{v} = \$4.90, \$8.10, \$12.10, \$16.90,$  and  $\$36.10$ . Furthermore, data from those  $(N, \bar{v})$  treatments, or other experimental treatments that require adjustments in behavior to remain faithful to CRRAM, are used in tests reported in CSW (1984, 1985b, 1988) and in Walker et al. (1990).

#### IV. Concluding Remarks

Harrison's (1989) metric approach to testing behavioral hypotheses involves an assumption of unique cardinal utility. Without unique cardinal utility, claims that mea-

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is randomly drawn, with replacement, from a population of fairways of *different lengths*. The expected monetary payoff to the gorilla is an increasing function of her accuracy in getting the ball close to the pin. There is a water trap in front of each green. If the ball lands in the water trap, the gorilla receives zero monetary payoff on that hole. The gorilla is observed consistently to adjust the length of her drive to varying fairway length. In addition, the gorilla consistently hits the ball farther than the distances that would maximize her expected monetary payoff. One model predicts that the gorilla will continue to use the same consistent strategy (of adapting her drive to varying fairway length) if monetary payoffs are tripled. Alternative models and conjectures predict that the gorilla's strategy will change. Monetary payoffs are tripled, and it is observed that the gorilla's strategy does not change.



sured utility differences are “too small” are arbitrary assertions. This is an empirical question, and Harrison has offered no experiments showing that increases in such differences cause decisions to move closer to RNNE predictions. Harrison’s claim that experimental economists can induce subjects’ utility functions in a way that supports valid statements based on a metric of forgone utility is wrong, and his metrics do not provide useful heuristics to aid in experimental design because they do not measure forgone payoffs in informative ways.

Both the Harrison critique and the Kagel and Roth (1992) comment create the impression that our research has not examined motivational questions. In fact, a major part of our research during the past decade has been directed to such questions. Here, we have described two of the approaches we have used. Our multiplicative-payoff experiments specifically addressed the “flat-maximum” property discussed by Harrison. These experiments were reported in several of our articles, beginning in 1983. The empirical results are clearly inconsistent with Harrison’s conjecture about risk-neutral bidding and flat maxima. Our formulation and testing of “income-threshold” and “utility-of-winning” (nonstandard) models of bidding behavior provide an internally consistent approach to studying motivational questions. This contrasts with the internally inconsistent approach that has been used by Harrison.

Kagel and Roth insist that inconsistencies with theoretical predictions that result from introduction of the compound lotteries implied by the use of the binary lottery payoff technique must be interpreted as inconsistencies with “the risk-aversion hypothesis” rather than the compound-lottery axiom. What about the well-known predictive failures of expected-utility theory in Allais-paradox, preference-reversal, and compound-lottery experiments (see e.g., Grether and Plott, 1979; Daniel Kahneman and Amos Tversky, 1979; Soo Hong Chew and William Waller, 1986; Cox and Seth Epstein, 1989; Camerer and Ho, 1990)? Must they all be interpreted as inconsistencies with “the risk-aversion hypothesis” by KR’s

reasoning? The fact is that some specific axiom (e.g., the compound-lottery axiom) can be of crucial importance in one context (e.g., lottery-payoff experiments) but of little importance in accurately predicting outcomes in another context. Kagel and Roth’s assertion does not justify their ignoring the results from empirical tests of the reliability of the lottery payoff procedure.

Kagel and Roth (p. 1389) state that “...the superior fit of the CRRAM model in first-price auctions results from its larger number of free parameters than the RNNE or homogeneous risk-aversion alternatives and thus does not directly test the core hypotheses of risk aversion and equilibrium bidding.” It is obvious that if the data are inconsistent with the RNNE or homogeneous-bidders risk-averse models then a more general model (perhaps one with “more free parameters”) should be developed and tested. We did this when we introduced CRRAM and the log-concave model into the literature. After introducing the new models, we reported results from multiplicative-payoff experiments in which CRRAM predicts no change in behavior and all non-log-linear equilibrium bidding models predict that behavior will change. We also reported several experiments, in five of our papers, in which CRRAM predicts that behavior will change in response to the experimental treatments (Cox, Roberson, and Smith, 1982; CSW, 1984, 1985b, 1988; Walker et al., 1990). All of these tests are clearly direct tests of core hypotheses.

Kagel and Roth (p. 1389) conclude that “...data from these same [DKL and BKM] experiments show that subjects act as if they are relatively more risk-averse when there are higher expected profits to be earned, which directly violates predictions based on the assumption of constant relative risk aversion underlying CRRAM.” We have shown that Kagel and Roth’s conclusion is wrong. First, correcting the figures reported in Kagel and Roth’s tables 2 and 3 reveals that their data come from experiments with lower, not higher, expected profits than other experiments involved in the present controversy. Second, Kagel and Roth’s

within-experiment calculations reported in their table 4 and footnote 15 are based on a simple formula that cannot support conclusions about expected profits in bidding experiments; correct analysis of data to draw conclusions about theoretical predictions of expected profits from bidding requires the use of order statistics, as explained in Cox, Roberson, and Smith (1982 pp. 12–13) and CSW (1985 pp. 184–8). Third, Kagel and Roth's conclusion is contradicted by the valid test results in Smith and Walker (1992b), in which a 20-fold increase in expected payoffs (and expected forgone payoffs) does not cause a significant increase in revealed subject risk aversion.

Kagel and Roth maintain that their methodology and data analysis stand in sharp contrast with ours. This is correct, and we continue their discussion of the contrast. They analyze only data that are pooled across individuals; hence, they cannot learn anything about individual decision-making characteristics relative to the predictions of theory. In contrast, we conduct both nonparametric and parametric tests on individual-subject data. They attribute deviations of pooled bids from RNNE to undefined "perceptual errors" (KR, 1992 p. 1382) or "bidding errors ... of one sort or another" (p. 1384). In contrast, we have developed and tested models that predict deviations from RNNE in terms of risk aversion, utility (of the event) of winning, utility thresholds and decision costs. Kagel and Roth want us to reject auction theory because, as with all theories, there are anomalies. In contrast, we maintain, as does Lakatos (1980), that there is no falsification by negative evidence until we or others provide substitute theories within a progressive research program.

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