Theory and Misbehavior of First-Price Auctions: Comment

By ANTONIO MERLO AND ANDREW SCHOTTER*

In a recent paper in this *Review*, Glenn Harrison (1989) has raised an interesting and significant point concerning the methodology of experimental economics. Specifically, Harrison has warned economists to take care in designing their experiments to give subjects sufficient incentives to overcome the significant calculation and decision costs that exist in experiments. If these costs are not overcome the fear is that the investigator may lose control over the actions of his or her subjects. One way that Harrison claims that control can be lost is if the payoff function faced by any subject, conditional on the others taking their equilibrium actions, is flat. Such a "flatness" would give any player little incentive to find his or her best response since, in a money metric, nonoptimal decisions would pay very little less than the truly optimal decision. Hence good experimental design would require that payoff functions be "steep" around the equilibrium or optimal action. John Kagel and Alvin Roth (1992) support this view.[1]

In this paper we try to go beyond the Harrison "payoff dominance" critique and assess when an experimental subject is likely to be sensitive to it. Hence, what we present here is a first attempt to give some operational content to the general point raised by Harrison (1989) which, while undoubtedly correct, needs to be refined. The major point of our paper is that the shape of the theoretical payoff function faced by subjects in an experiment cannot have any effect on their behavior unless they are able to perceive it either deductively before the experiment begins or inferentially using the information they gather during the experiment. Depending on when and what subjects learn, the Harrison criticism holds more or less force. For example, at one extreme assume that some experimental subjects (rightly or wrongly) believe that, after reading the experiment's instructions, they have solved the problem posed. (This would clearly be the case if they were perfectly rational subjects with sophisticated calculating abilities). In this case, we would expect such subjects (whom we call "theorists") to choose what they predict is the optimal choice and persist in that choice during the duration of the experiment. With such persistent behavior we can never expect such subjects to learn anything during the experiment about the shape of the payoff function they face. Hence, for these people Harrison's criticism would hold little force.

On the other extreme are those subjects (whom we call "experimenters") who, after reading the experiment's instructions, are relatively unsure about the shape of the payoff function they face. In order to estimate it, these subjects might be expected to make a diverse set of choices over a broad range of the payoff function's domain and observe the payoff consequences that result. Once they feel they have learned all they want to about the payoff function, this experimentation stops, and an action is cho-

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[1] The problem of offering experimental subjects sufficient incentives is already considered in Vernon Smith's (1982) paper. Smith, however, does not directly address the question of the slope of the payoff function.

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sen. When the payoff function is perceived as being flat, people will ultimately conclude that their choice has little influence on their payoff, and Harrison's criticism takes over. From then on, we can expect a wide variety of behaviors, since all sorts of psychological heuristics may take over a subject's behavior and dominate it. When the payoff function is perceived to be steep (even when it is, in fact, flat), we might expect that their choices will converge to the peak of their estimated (steep) payoff function, since steep payoff functions imply severe penalties for nonoptimal choices. The point is that the shape of the payoff function can only influence behavior after it is estimated, and even if subjects act as experimenters it is still possible that the Harrison criticism may not have full strength, since it may be observationally impossible for some subjects to realize that the payoff function they face is flat. Hence, depending upon the way the subjects experiment (and depending on the realization of the random variables if nature is a player in the experiment), the data generated will differ greatly. Empirically, given the meager data generated by the experiment, it may be impossible for subjects to conclude that in fact the payoff function they face is flat.

The complexity of the calculation problem posed by the experiment would determine how many theorists we might expect to observe and how much force we might expect the Harrison criticism to have. For example, if an experiment poses a trivial problem for subjects to solve (i.e., if there is an obvious dominant strategy for all subjects), we would expect that all subjects would solve the problem before the first round of the experiment. Hence, the payoff function's shape would not matter, and the Harrison criticism would hold little force. On the other hand, if the problem posed is extremely complex, we would expect to see many experimenters, and the strength of the Harrison criticism here would depend on how easily these experimenters could estimate the true shape of the payoff function they face, given the number of rounds in the experiment and the nature of the information feedback they get. It is one thing for an outside observer (experimental economist) with perfect knowledge of the experimental design to compare expected disequilibrium payoffs and *ex ante* equilibrium payoffs conditional on value realizations, as Harrison urges us to do, and another for experimental subjects (or even trained statisticians) to estimate these differences given the data generated by the experiment.

These factors restrict the set of experiments for which Harrison's criticism holds full force to that set for which the problem posed for subjects is not so easy as to make a deductive solution trivial or so hard as to make inferential estimation problematic. This means that, in general, we have to measure the strength of the Harrison criticism of an experiment by how many people appear to be affected by it.

In this paper we explore these ideas further using data generated both by Harrison's own experiments (Harrison, 1989) and by one of the experiments performed by Clive Bull et al. (1987).[2] We demonstrate that the shape of the theoretical *ex ante* payoff function could not have been a determining factor in the behavior of a large number of subjects in these experiments because, given the nature of the experiments and the data available to the experimental subjects, either they could not have perceived the function's shape or they could have misperceived it.

In Section I we discuss the Harrison criticism with reference to some data contained in Harrison (1989). We demonstrate that while the criticism has merit this particular experiment is a difficult one in which to apply it. In Section II we describe the Bull et al. (1987) experiments which generated the data for most of our analysis. In this section we investigate the difficulties involved in empirically assessing the curvature of the payoff function faced by experimental subjects. In Section III we offer some conclusions and comments for future work.

[2]One claim in this paper was recently criticized by Robert Drago and John Heywood (1989) using Harrison's criticism.

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I. The Harrison (1989) Experimental Data

The Harrison criticism was originally aimed at a series of papers by James Cox et al. (1982, 1983, 1985, 1988). Also, Harrison (1989) ran his own experiments to prove his point, and we will look at his data soon. Ironically, we feel that the Harrison criticism is particularly hard to apply to the experiments run by Harrison (1989) and Cox et al. (1982, 1983, 1985, 1988). The reason is that, given the nature of those experiments and the limited number of repetitions, we believe it is virtually impossible, even for mathematically talented experimental subjects, either to figure out in advance the shape of the theoretical payoff function they faced or to estimate it during the experiment. Let us explain.

The Harrison (1989) and Cox et al. (1982, 1983, 1985, 1988) experiments are auction experiments. Subjects receive random realizations each period defining their "value" for a fictitious good to be sold in that round. They observe these realizations secretly and submit a sealed bid. In each round the winner of the object and its price are announced, and payoffs are determined. As described, this is roughly a game of incomplete information, and in such games a strategy is a function from the value realization in each round to the set of bids. More importantly, the payoff function is actually a payoff functional *mapping* each bid function into an expected payoff function. Hence, assuming that the task of deductively figuring out in advance the shape of the theoretical payoff functional they face goes beyond the normal computing abilities of experimental subjects,[3] to determine whether the payoff functional is flat one would have to observe the payoffs to each bid function employed to see how they vary. This is quite impossible in an experiment with 20 rounds (or even 100 rounds for that matter). To begin, the set of bid functions is virtually unbounded. (Even limiting ourselves to linear functions leaves an unbounded set.) Furthermore, subjects could not even estimate the shape of the conditional payoff function or the function that results when we perform the experiment that Harrison asks us to (holding a subject's value constant, as well as the actions of the other $n-1$ bidders, at their Nash equilibrium actions and varying a subject's bid), since that would require that the subject get repeated observations of the same value—enough observations to estimate the payoff function conditional on a specific random realization. This was not the case in the Harrison (1989) experiment, as we will demonstrate using his data.

In the automaton experiments of Harrison (1989)[4] subjects played a repeated 20-period auction game (of the type described above) against a set of preprogrammed computerized subjects programmed to bid 75 percent of the random values they received each period. Each subject then received 20 uniquely different random values (drawn from the interval [0, 1,000]) over the course of the experiment. The 20 data points generated during the course of the experiment were all the subjects had to use in estimating the shape of the payoff functional or conditional payoff function (i.e., that payoff function conditional on a random realization for their value) they faced, unless they could figure it out deductively after reading the instructions and before the experiment began (a highly unlikely assumption). Table 1 presents the random realizations that the 30 human subjects who participated in this experimental treatment received in each of the 20 rounds, together with the number of rounds in which each subject won the auction during the experiment. (Note that each subject was given an identical 20-round sequence of random numbers.) From the description of the experiment it should be obvious that no payoff functional could be estimated within 20

[3] This seems to be true even when each subject plays against a set of computerized partners programmed to play a Nash equilibrium strategy to the game described, which is what Harrison (1989) does. We will discuss this case shortly.

[4] Experiments 3 and 3P in Harrison (1989).

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TABLE 1—HARRISON'S (1989) AUTOMATON EXPERIMENTS

A.		B.	
Round	Random values	Player	Number of wins
1	168	1	3
2	178	2	3
3	544	3	3
4	444	4	4
5	188	5	1
6	309	6	2
7	828	7	3
8	818	8	5
9	642	9	5
10	469	10	4
11	24	11	3
12	442	12	3
13	380	13	6
14	676	14	5
15	400	15	4
16	577	16	5
17	434	17	5
18	389	18	5
19	885	19	4
20	498	20	5
		21	4
		22	4
		23	5
		24	4
		25	5
		26	5
		27	4
		28	4
		29	6
		30	4
		Mean:	4.1

rounds; but the problem is even worse. The subjects could not even estimate the conditional payoff functions that Harrison uses to illustrate his point, since in order to do that they would have to receive many of the same random realizations and see how their payoffs change as they vary their bids, holding the random realization constant. As we see from the data, this would have been impossible. Each subject received a different realization in each of the 20 rounds of the experiment. Furthermore, from inspection of the data, we see that no subject was successful in actually winning the auction in more than six periods out of the 20 the experiment lasted, and some, in fact, only won once or twice. Hence, the information that subjects received during the experiment was minimal and clearly less than that needed to estimate or even intuitively sense the shape of the payoff function they were facing.[5]

It is one thing for an outside observer (experimental economist) with perfect knowledge of the experimental design to compare expected disequilibrium payoffs and *ex ante* equilibrium payoffs conditional on value realizations, as Harrison urges, and another for experimental subjects to estimate these differences given the data generated by the experiment. In fact, in this case it is impossible. This is not the case for the Bull et al. (1987) experiments to which we turn our attention next. In those experiments, like many many others, the object of choice is not a function but a scalar, and what has to be estimated by the subjects is not a functional but a simple function. Hence, let us turn our attention there.

II. "Automaton Tournaments" and the Harrison Criticism

Bull et al. (1987) present an experimental analysis of two-person rank-order tournaments. In these experimental tournaments, each subject chooses a number between 0 and 100 representing his or her effort level in the tournament. A monotonically increasing cost is associated with each effort level. After these efforts are chosen each is augmented by an additive shock or realization of a random variable drawn independently for each subject from a uniform distribution. The rules of the tournament dictate that the subject with the highest total (effort plus random realization) will be awarded a "big" monetary prize, while the one with the lowest total will be awarded a "small" monetary prize. The final payoff for subjects will be the prizes they win minus their costs of effort. Each such tournament is run 12 times for each pair of subjects who play

[5]Note that if a player loses the auction his or her payoff is zero, and that is the only information the player receives in that round.

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against each other repeatedly. Subjects are then paid the sum of their payoffs in each of the 12 rounds of the tournament. Bull et al. (1987) chose parameters for which the unique Nash equilibrium of their tournaments was 37.

The experimental treatment we focus our analysis upon is the one labeled "automaton tournament" in Bull et al. (1987), for which there were 17 subjects. In this treatment, each subject played the game described above against a computerized partner called an automaton who was programmed to play an equilibrium effort level equal to 37 (the Nash equilibrium level) in each period. This number was revealed to the subjects without telling them that it was an equilibrium level. Hence, while subjects theoretically were playing a game, subjects were in fact performing a one-person optimization, since inferential and conjectural problems about opponents were absent in this experiment. This decision problem, in fact, is exactly the one Harrison would like subjects to contemplate when they consider their actions. Hence, it is particularly relevant to the debate. The *ex ante* theoretical payoff function here is quite flat, as we will see later. Hence, this experiment furnishes a good test for the Harrison criticism, and it has already been criticized along these lines by Drago and Heywood (1989).

It is our point in this paper that the strength of the Harrison criticism of an experiment depends on the number of experimental subjects whose behavior appears to be affected by it. Clearly it applies to those experimental subjects who have experimented enough with the payoff function to discover its shape correctly. Their behavior, after that point, can be explained by any number of psychological heuristics. The criticism, however, does not apply to those subjects who, after they read the experiment's instructions, strongly believe, rightly or wrongly, that they have fully solved the problem and, except for minor adjustments, choose their perceived best choice in every round of the experiment. To demonstrate how many subjects we consider vulnerable to the Harrison criticism in the Bull et al. (1987) automaton experiment let us first look at Figures 1 and 2, which present the actual time series of number choices made by subjects in this experiment. As we can see, we have informally grouped these subjects into two groups by inspection of their choice pattern over the 12 rounds of the experiment. We call these groups the *theorists* (Fig. 1) and the *experimenters* (Fig. 2) (this distinction is formalized in Appendix 1).

Group 1 (theorists) is made up of those subjects (subjects 1, 3, 4, 6, 7, 10, and 16) whose period-to-period choices varied over an extremely small segment of the domain of the payoff function.[6] (The median of the absolute deviations [across subjects] from the individual medians of the 12 round choices was only 0.5.) It is hard to imagine that these subjects learn anything during the experiment about the shape of the payoff function they face since they make no attempt to learn it. (Subjects 7 and 10 did exhibit some variety in their choices but still not enough for us to classify them differently; see Appendix A for an examination of the robustness of a more formal classification scheme.) Group 2 is the group of subjects whom we call experimenters (subjects 2, 5, 8, 9, 11, 12, 13, 14, 15, and 17). Their period-to-period choices varied over wider segments of the payoff function's domain. (The median of the absolute deviations [across subjects] from the individual medians of the 12 round choices was 6.) These subjects generated enough data (even given the limited 12-round horizon of the experiment) to attempt to estimate the *ex ante* payoff function, but as we will see, they misestimated it.[7]

To support the idea that subjects we labeled as theorists employed drastically different choice strategies than subjects we labeled as experimenters, in Figures 3 and 4 we present histograms indicating the number of instances in which subjects assigned to each of our two polar groups chose an

[6] See Appendix A to see how this grouping relates to our formal definition.

[7] In Merlo and Schotter (1992), we run these experiments with a 75-round horizon.

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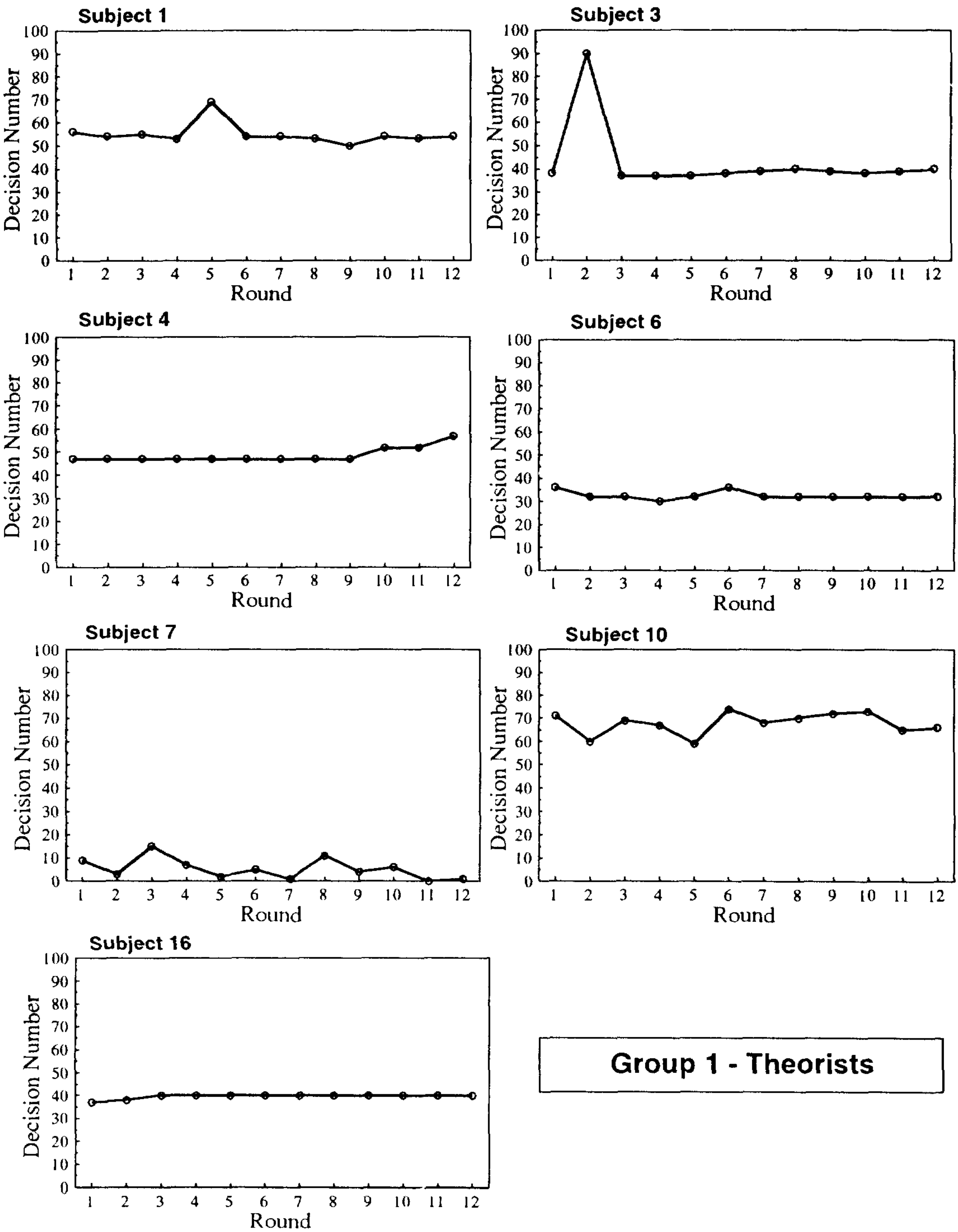

FIGURE 1. TIME SERIES OF NUMBER CHOICES IN THE BULL ET AL. (1987) AUTOMATON EXPERIMENT

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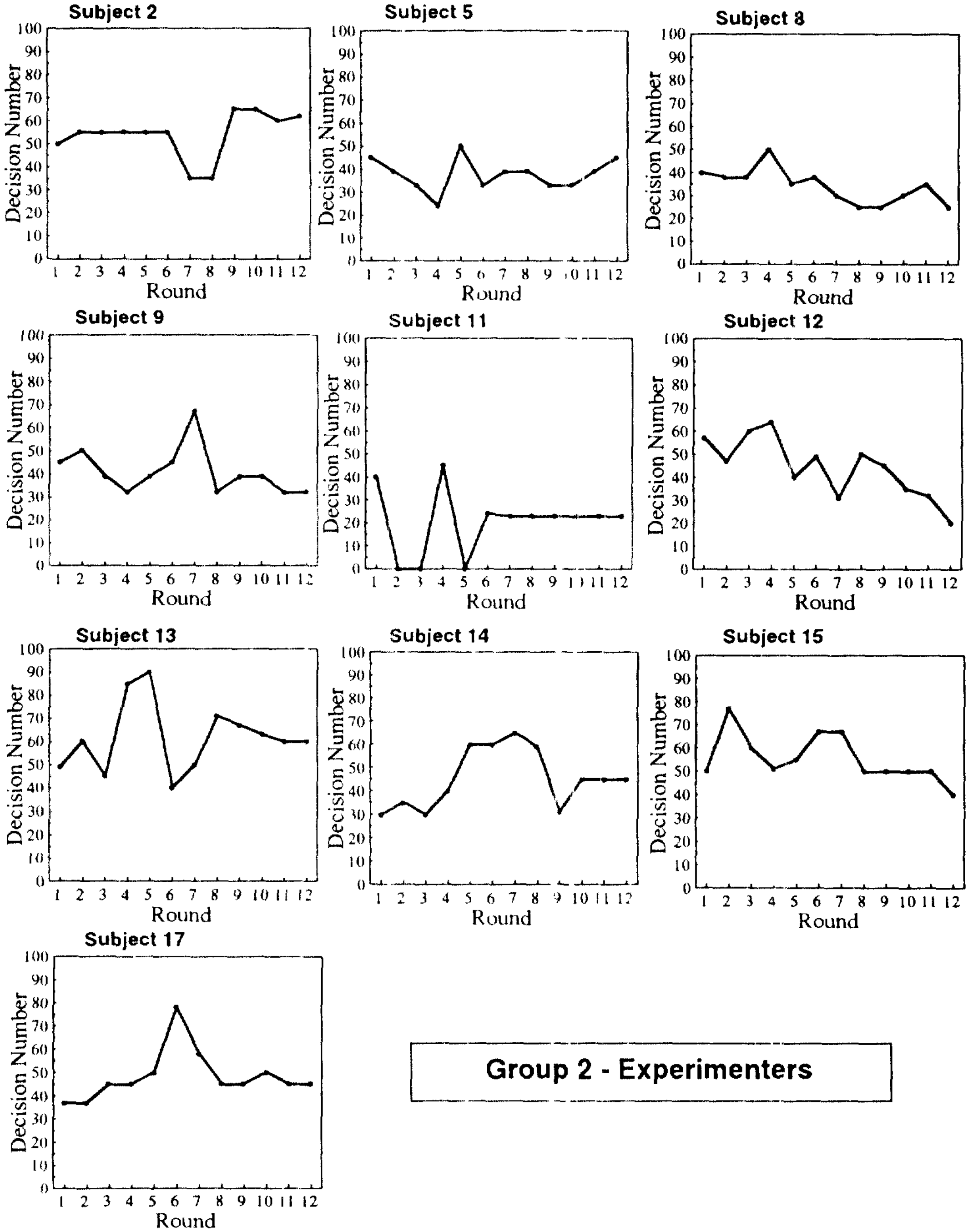

FIGURE 2. TIME SERIES OF NUMBER CHOICES IN THE BULL ET AL. (1987) AUTOMATON EXPERIMENT

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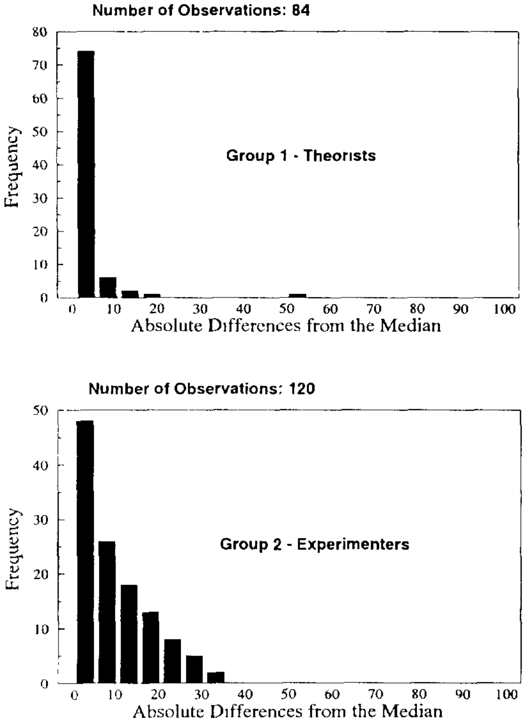

FIGURE 3. NUMBER OF INSTANCES IN WHICH SUBJECTS CHOSE AN ACTION THAT WAS A GIVEN DISTANCE FROM THE MEDIAN ACTION OVER 12 ROUNDS

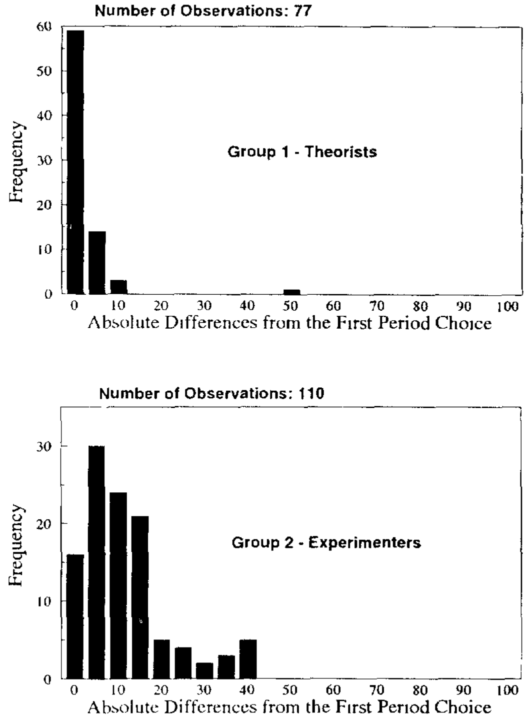

FIGURE 4. NUMBER OF INSTANCES IN WHICH SUBJECTS CHOSE AN ACTION THAT WAS A GIVEN DISTANCE FROM THE FIRST-PERIOD ACTION

action that was a given distance from their median action over the 12-round horizon of the experiment (Fig. 3) or from their first-period action (Fig. 4).[8] Nonparametric tests of the equality of the two populations support the hypothesis that these groups were different.[9] As stated above, we consider the theorists to be immune from the Harrison criticism. Let us then turn our attention to the set of experimenter subjects to see how many of them could have perceived the flatness of the payoff function they faced. To do this we present Figure 5. Here, for each of our experimenters, we draw the *ex ante* expected payoff function and the estimated payoff function based on the least-squares quadratic interpolation of the data points generated by the subject during the experiment.[10] By looking at this figure, we can observe that at most four subjects in the experiment (subjects 8, 9, 12, and 13) could be considered as candidates for the Harrison criticism in that they would have been able either to discover the true shape of the experiment's payoff function

[8] If subjects were truly theorists, their first-period actions in the experiment would indicate their theoretical best guesses of the optimal action (untainted by the experience they gain during the laboratory session).

[9] A Kolmogorov-Smirnov test of the equality of the two distributions rejects such a hypothesis with a critical probability value of 0.0001, and a Kruskal-Wallis test of the hypothesis that the two samples are drawn from the same population rejects it with a critical probability value of 0.0001.

[10] The estimates are reported in Appendix B (Table B2) together with the coefficients of the *ex ante* experiment's payoff function (Table B1).

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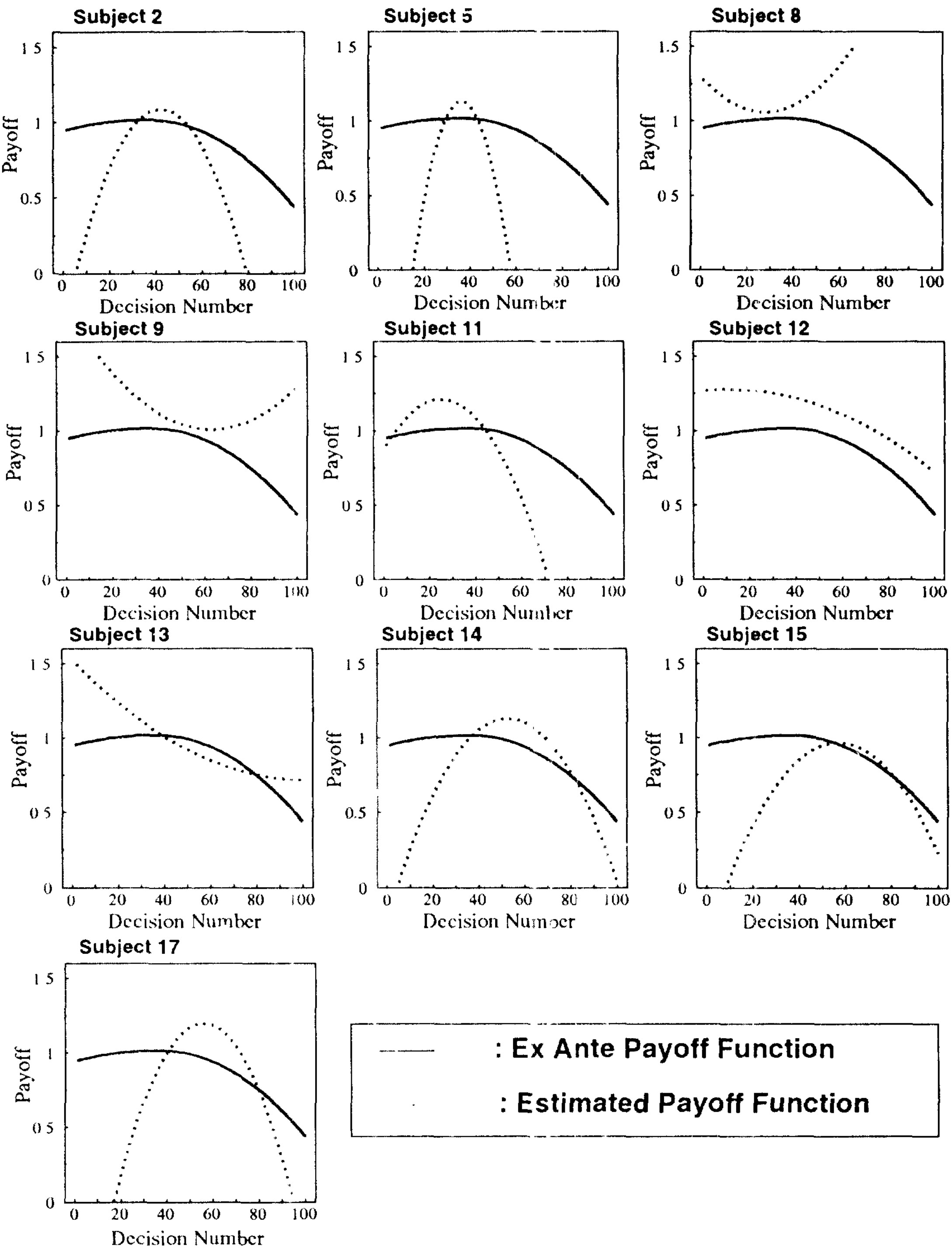

FIGURE 5. *EX ANTE* EXPECTED PAYOFF FUNCTION AND ESTIMATED PAYOFF FUNCTION BASED ON THE DATA FOR EACH SUBJECT

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(subject 12) or, at least, to conclude that it was flat (subjects 8, 9, and 13). The remaining six experimenters (subjects 2, 5, 11, 14, 15, and 17) appear to be less vulnerable to the criticism, since their estimated payoff function was more steep than the theoretical *ex ante* payoff function. This would not necessarily be evidence for our point if we did not also observe that the choices of four out of these six experimenters (subjects 5, 11, 14, and 17) moved toward the maximum of their estimated function in the later rounds of the experiment. In other words, these subjects acted *as if* they had estimated a steep payoff function and moved toward its peak as the experiment progressed. To illustrate this point we have calculated the round-by-round decision number deviations of subjects 5, 11, 14, and 17 from the peak of their estimated payoff functions over the 12 rounds of the experiment. These subjects tended to move toward the peaks of their estimated payoff functions as the experiment entered its final stages. For example, over the first three periods these subjects exhibited a mean absolute deviation from their estimated peaks of 16, while over the last three periods this mean absolute deviation was only 6. Hence, it appears that these subjects acted like subjects who were homing in on the peak of a not-too-flat payoff function. Furthermore, we can see that there is the possibility that experimenters get "trapped" in a wrong region. For example, after experimentation it appears to one experimenter (subject 11) that the payoff function he faces is steep around 23, and it appears to another experimenter (subject 14) that this function attains its maximum at 50.[11,12]

In summation, we have just seen that out of the 17 subjects who participated in this experiment only six (at most) acted in a way that we interpret as consistent with the Harrison criticism. These subjects are the four subjects whose estimated payoff function did indeed appear flat plus subjects 2 and 15 (whose behavior does not seem to have been affected by the estimated payoff function). Of course, we cannot make this claim with total certainty, since many different types of behavior are consistent with Harrison's criticism and because Harrison (1989, 1990a,b) does not provide any detailed description of what the behavior of a subject influenced by the payoff-dominance critique might look like. Still, we consider his critique to be important but suggest that the flatness of the *ex ante* payoff function in an experiment is not sufficient evidence per se to claim that an experiment has fallen under its domain. A more detailed analysis of the subject-by-subject data is required.

III. Conclusions

Before one can apply the Harrison criticism one must actually look at the data of the experiment on an individual-by-individual basis to discover whether in fact subjects acted in a manner that would have allowed them accurately to perceive the payoff function they faced. This paper has presented data that led us to question the strength of the Harrison criticism. It has been our point that the flatness of an experiment's *ex ante* expected payoff function can influence the behavior of subjects if and only if they perceive that flatness. Using data from the Harrison (1989) and Bull et al. (1987) experiments, we demonstrated that despite flat *ex ante* payoff functions the number of subjects in those experiments whose behavior

[11]Whereas the *ex ante* expected payoff function is peaked at 37.

[12]Note that the steepness of the estimated payoff functions in Figure 5 is due, in part, to realizations of exogenous random variables in the experiment and in part to the "biased" sampling strategies followed by the experimenters, both of which may influence the shape of these functions given the limited number of replications available. For example, the inference that player 11 can make about the shape of the payoff function he faces, is very likely to be affected by the fact that when he chose exactly 23 he always received the big monetary prize, whereas choices in the neighborhood of 23 were not as successful (i.e., he sometimes won the big prize and sometimes got only the small prize). Hence, in the neighborhood around 23 the payoff function appears to be steep

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could have been influenced by the payoff-dominance critique is relatively small.

APPENDIX A

Here, we formalize our distinction between theorists and experimenters. Consider a laboratory experiment run with a set of I subjects ($i = 1,\dots,I$). Let the experimental-stage game be repeated T times, and assume that the I players in the experiment have simply to choose a scalar in each period t ($t = 1,\dots,T$). Let x_{it} denote the choice of player i at time t; let $\tilde{x}_i$ denote a measure of location for the distribution of player i's choices over the T rounds of the experiment he participates in (such as the mean or median) and let ξ_i denote a measure of dispersion of such a distribution (such as the standard deviation or interquantile range). The average "dispersion" of all of our subjects' choices is

$$\bar{\xi} = (I^{-1}) \sum_{i=1}^{I} \xi_i$$

where this mean is taken by averaging the standard deviations (interquantile ranges) of the choice distributions of all players in our experiment. We will say that player i is making an experiment in period t if he chooses an action sufficiently far away from his mean (median) action over the course of the experiment. We make this operational by saying that agent i in period t is making an experiment if

$$|x_{it} - \tilde{x}_i| > q\bar{\xi} \qquad q \in [1,2].$$

In words, an experiment for player i at time t is a choice x_{it} for which the absolute deviation from his mean (median) choice over the T periods is greater than q times the average of some measure of dispersion of the individual choices across all of the players involved in the laboratory experiment we are considering. We use a mean measure of dispersion and not individual ones because if we did not do this we might run the risk of labeling a subject with a small ξ_i an experimenter, since many of his choices may be more than $q\xi_i$ units away from their mean (median) despite the fact that they are all almost identical. Likewise, our normalization allows us to avoid the problem of not labeling a subject with a large ξ_i an experimenter because, although he made widely diverse choices, his ξ_i was so large that only a few of the choices fell more than $q\xi_i$ units away from their mean (median). We will call an experimenter a player who makes at least two experiments in a T-period repeated laboratory experiment, a theorist is a player who makes fewer than two experiments in a T-period repeated laboratory experiment. Note that by defining a bound of two we are making the most severe condition necessary for a subject to be labeled a theorist.[13]

In the experiment we are considering, given that the sample distribution of players' choices is severely non-Gaussian (i.e., left skewed and leptokurtic), we focus on the median as a more reliable measure of location than the mean, and we focus on the interquantile range as a more reliable measure of dispersion than the standard deviation.[14] Note that, because of the intuitive interpretation of the interquantile range as a measure of dispersion (it identifies the range of the distribution that covers the central 50 percent of the frequency mass), by varying q we vary the size of the tails of the distribution which contain those observations we label as experiments; by increasing q we make it harder for a subject to be labeled as an experimenter (i.e., we are more likely to move subjects from group 2 to group 1 than vice versa). According to our formal definition, in Table A1 we pre-

[13] A unique deviation might in fact only be due to a mistake.

[14] Recall that for a distribution function $F(x)$ the interquantile range is defined as

$$\Delta = \xi_{0.75} - \xi_{0.25}$$

where $\xi_{0.75}$ and $\xi_{0.25}$ are quartiles defined by

$$F(\xi_p) = p \qquad p = 0.25, 0.75$$

For references, see, for example, J. S. Maritz (1981).

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TABLE A1—EXPERIMENTATION IN THE BULL ET AL. (1987) AUTOMATON EXPERIMENT

	Number of experiments		
Player	$q=1$	$q=1.5$	$q=1.8$
1	1	1	0
2	4[a]	2[a]	2[a]
3	1	1	1
4	1	0	0
5	2[a]	1	0
6	0	0	0
7	1	0	0
8	4[a]	1	0
9	2[a]	1	1
10	1	0	0
11	5[a]	5[a]	5[a]
12	7[a]	5[a]	2[a]
13	7[a]	4[a]	3[a]
14	8[a]	7[a]	1
15	5[a]	3[a]	1
16	0	0	0
17	2[a]	1	1
Mean:	3.00	1.88	1.00

[a]The subject is labeled as an experimenter.

sent the number of rounds for each subject in which his action could be labeled as an experiment when q equals 1, 1.5, or 1.8 (i.e., the size of the tails is respectively 50 percent, 25 percent, or 10 percent of the frequency mass). Note that when we set $q=1$ we exactly reproduce the intuitive classification we introduced in Section II. Moreover, even when we make it very hard for a subject to be labeled as an experimenter (by setting $q=1.8$), we do not fail to keep those subjects whom we expect to be classified as unambiguous experimenters in that category (see Fig. 1). Hence, the formal definition we propose does not fail to bear out our basic intuition and might be of some help in further investigations of this controversial issue.

APPENDIX B

The coefficients of the *ex ante* expected payoff functions plotted in Figure 5 are given in Table B1. The least-squares estimates of the payoff functions plotted in Figure 5 are given in Table B2.

TABLE B1—COEFFICIENTS OF THE *EX ANTE* EXPECTED PAYOFF FUNCTION

Payoff function: $\pi = a + b_1e + b_2e^2$, where e represents effort level			
	Estimates		
Region	a	b_1	b_2
$e \leq 37$	0.95	0.004	−0.00005
$e \geq 37$	0.85	0.010	−0.00014

Note: The probability of winning the big monetary prize conditional on e is in fact different in the two regions.

TABLE B2—LEAST-SQUARES ESTIMATES OF THE PAYOFF FUNCTION

Payoff function. $\pi = a + b_1e + b_2e^2$, where e represents effort level			
	Estimates		
Subject	a	b_1	b_2
2	−0.341301	0.067582	−0.000799
5	−2.142922	0.180567	−0.002490
8	1.295218	−0.016958	0.000303
9	1.820126	−0.025786	0.000205
11	0.869337	0.027318	−0.000548
12	1.268799	0.001564	−0.000072
13	1.525669	−0.016198	0.000081
14	−0.209029	0.050888	−0.000484
15	−0.344417	0.045930	−0.000402
17	−1.268166	0.088437	−0.000792

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