# OPTIMIZATION THEORY: CLOSE BUT NO CIGAR

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ABSTRACT

In his critique, Houston argues that optimization theory provides a plausible account of how animals behave in operant choice experiments. He fails, however, to take into consideration two critical findings. First, there is no systematic relationship between the reinforcement rate maximization predictions and performance across different procedures. Second, the results from variable-interval choice procedures are at best ambiguous, because quite different choice proportions produce nearly maximal reinforcement rates. That is, in variable-interval schedules, it is virtually impossible for subjects not to produce relatively high reinforcement rates. In addition, there are several logical or mathematical problems in Houston's description of the Heyman and Luce model of variable-interval choice.

Key words: optimizing; matching; reinforcement; evolution.

Houston's critique (1982) is one of several recent papers on whether animals maximize reinforcement rate in operant psychology experiments (see, e.g., Rachlin, Battalio, Kagel and Green, 1981; Staddon, 1980). According to several investigators, subjects in operant experiments acquire the response rate that maximizes overall reinforcement rate (e.g., Rachlin, Green, Kagel and Battalio, 1976) or overall net gain (rate of food intake less the costs of response effort, e.g., Rachlin et al., 1981). Other researchers, however, write that the maximizing theory predictions do not describe how subjects behave (e.g., Heyman and Luce, 1979; Prelec, 1982). One reason for the disagreement is that the relationship between prediction and behavior is complex. In the most widely used procedure, the independent concurrent variable-

interval variable-interval schedule, conc. VI VI, the maximizing predictions approximate the experimental results, while in other procedures, the predictions differ sizeably from the subject's behavior (see, e.g., Herrnstein and Heyman, 1979). Consequently different portions of the literature may suggest different conclusions. For example, Houston (1982) says that the discrepancy between the maximizing predictions and responding in the independent conc. VI VI procedure should be ignored because it is so small. In contrast, Prelec (1982) writes that the results from the conc. VI VI schedule do not provide a decisive test for maximizing theory, but that the results from other procedures, for example single schedule experiments, show that maximization theory fails to predict operant behavior.

One way to include all the data is to test whether the maximizing predictions and behavior covary across experimental procedures. Systematic covariation, even with large absolute deviations between predictions and results, would suggest that animals are in some sense maximizers (for example, maximizers with a limited capacity for learning the contingencies), whereas the failure to find systematic covariation across procedures would suggest that the approximation in the independent conc. VI VI procedure was a fortuitous good fit. Consequently, a description of the correlation between maximizing theory and behavior in different choice procedures will provide the basis for evaluating optimization theory. Consider first, though, a few basic findings and issues.

I. INDEPENDENT CONC. VI VI SCHEDULE PERFORMANCE

The experimental result that has motivated theoretical interest in conc. VI VI schedules and other operant choice procedures is the relationship between the distribution of behavior and the distribution of reinforcements. There are two manipulandums, and the subject typically responds to each according to the overall reinforcement proportions. For example, if an experimental session provided 60 reinforcers at one response lever and 20 reinforcers at the other response lever, the subject would switch from lever to lever (the reinforcers occur unpredictably) but spend about 75% of the session time at the 60 reinforcement lever. This is called matching (Herrnstein, 1970) and because of its generality across species and experimental procedures, it is considered an empirical law, written:

$$B_1/(B_1 + B_2) = R_2/(R_1 + R_2) \qquad (1)$$

where $B_i$ is either total responses or total time at schedule $i$, and $R_i$ is total reinforcements earned at schedule $i$. Importantly, since delivery of a reinforcer depends primarily on the passage of time, the reinforcement proportions are approximately constant. This means that it is possible for time and response proportions to deviate substantially from the reinforcement proportions. However, the matching relation, Equation 1, generally holds, and reinforcement maximizing

theory is one possible explanation of why Equation 1 applies, and why it applies so generally.

Note that the matching relation is described in terms of reinforcement proportions, $R_1/(R_1 + R_2)$. In contrast, according to maximization theory, behavior stabilizes at the choice proportion that produces the greatest overall reinforcement rate, the sum $R_1$ plus $R_2$. Therefore, if matching is normative, the matching time allocation proportion must be the one that maximizes overall reinforcement rate. (For most procedures, reinforcement rate depends more on time allocation than it does on response allocation.) Or put somewhat differently, animals maximize reinforcement rate, and a by-product is the matching relation (see, e.g., Shimp, 1969).

Heyman and Luce used a mathematical model of the conc. VI VI procedure to test whether the time allocation proportion that maximized reinforcement rate was the same as the time allocation proportion that matched the reinforcement proportions. They found that maximizing and matching (Equation 1) predicted different choice proportions, although the difference was small for typical response rates. It is not clear whether Houston accepts this result. He has co-authored a paper that shows that matching and maximizing are different (Houston and McNamara, 1981), but now writes, "maximizing on independent conc. VI VI schedules does imply matching." Apparently, Houston means 'imply' in the sense that the discrepancies between the maximizing and matching predictions (Equation 1) were not important, for elsewhere he writes, "it is possible that although, say, maximizing does not strictly imply matching it results in a very good approximation to it — good enough to be called matching if it was the behavior produced by an animal." Houston is saying that although maximizing may not be equivalent to the formal representation of the empirical results, Equation 1, it approximates the results, and it does so with the same degree of accuracy as Equation 1. There are several reasons for rejecting this conclusion.

First, maximization theory (e.g., Houston and McNamara, 1981) predicts time allocation proportions that are somewhat more extreme than the matching law predictions (i.e., toward 1.0). The results, however, do not follow this pattern of deviation. Instead variation is centered about the matching law (Baum, 1979). This difference is consistent with the view that matching and maximizing are different processes. Second, for the moment accept Houston's assumption that the differences between the maximizing and matching predictions are too small to be detected experimentally. This could mean, as Houston suggests, that matching and maximizing are equally good accounts of how subjects behave. Alternatively, it could mean that the experimental procedure was not sensitive enough to detect theoretically significant differences. Therefore, even if Houston's assumption is accepted, it is not possible to conclude that maximization implies matching. Third, the good fit of maximizing theory to behavior in the independent procedure may be fortuitous. The following overview of the literature evaluates this hypothesis.

In choice procedures that use two interdependent rather than independent variable-interval schedules (e.g., Fantino, Squires, Delbruck and Peterson, 1972; Stubbs and Pliskoff, 1969) the time allocation proportion that maximizes reinforcement rate differs from the matching law prediction in the direction of indifference, 50% (see Heyman and Luce, 1979; Staddon, Hinson and Kram, 1981). For example, in a VI 160-sec VI 480-sec interdependent schedule with a 1.5-sec changeover delay (Findley, 1958), a time allocation proportion of about 65% maximizes reinforcement rate, whereas matching predicts a time allocation proportion of 75% (see Table 1 below). This difference is experimentally detectable, and Baum's review (1979) shows that the matching equation leads to the more accurate account.

In procedures that give the subject a choice between a response requirement and an interval requirement, concurrent VI VR schedules, the matching rule and maximizing predictions generally differ, with the magnitude of the difference depending on the particular combination of schedules. For the schedules used in one study (Herrnstein and Heyman, 1979), the subjects should have spent more than 50% of the session time on the response key associated with the variable-ratio schedule in order to maximize the overall reinforcement rate. Instead, the subjects spent more than 50% of the session time on the schedule associated with the variable-interval schedule, and time allocation proportions matched reinforcement proportions.

There are at least two studies in which the experimenter explicitly arranged a contingency that made it impossible for subjects simultaneously to maximize overall reinforcement rate and to match response proportions to reinforcement proportions (Heyman, 1977; Mazur, 1982). In both studies, maximizing failed to predict the experimental results, and in Mazur's experiment, the subjects continued to match response proportions to reinforcement proportions, and in Heyman's experiment there was a tendency for time proportions to continue to match. (Also see Vaughan, 1981.)

This brief overview supports three general conclusions: (1) matching is not the same as reinforcement rate maximization in choice procedures that use at least one variable-interval reinforcement schedule; (2) the maximizing predictions and time allocation measures do not covary across experimental procedures; (3) consequently, the differences, however small, between the time allocation proportions that maximize overall reinforcement rate and the time allocation proportions that match the reinforcement proportions in independent conc. VI VI schedules should not be ignored. Indeed, the differences prove to be the theoretically significant finding; they signal the fact that animals do not maximize reinforcement rate in operant choice experiments.

Houston's second argument in favor of the maximizing hypothesis is that subjects earn close to the maximum reinforcement rate in concurrent interval experiments. He writes, "an alternative to asking if matching results in maximizing is to

investigate how close the obtained reinforcement rate is to the maximum rate." Note that Houston's question about reinforcement rates is somewhat different than the question of the time allocation proportion that maximizes reinforcement rate, because a given deviation from the maximizing time allocation will produce a large or small reinforcement loss depending on the contingency. Thus in some contingencies, small departures from maximizing will be costly, while in other contingencies, large departures will produce nearly maximal reinforcement rates. For concurrent interval schedules, Houston argues that maximizing does a good job at predicting behavior because subjects obtain close to the maximum reinforcement rate: "on the independent conc. VI VI, the best maximizing strategy does very well..." However, this argument is only effective if some choice patterns produce low reinforcement rates and subjects adjust their behavior in the direction of higher reinforcement rates. But this, surprisingly, is not the case. In concurrent schedules, a wide range of choice distributions produces nearly maximal reinforcement rates. For example, Table 1 shows that subjects get about the same reinforcement rate for indifference, a 50% time allocation, as they do for matching, a 75% time allocation. Nevertheless, subjects match, and Table 1 suggests that they do so independently of the overall reinforcement rates.

TABLE 1

CHOICE AND OVERALL REINFORCEMENTS PER HOUR IN CONCURRENT VI VI SCHEDULES

The calculations were based on Houston and McNamara's model of the independent conc. VI VI schedule and Heyman and Luce's model of the interdependent schedule. The schedule values are VI 160-sec and VI 480-sec, with a 1.5-sec changeover delay. The changeover rate parameter was set to 5 sec (see Heyman and Luce, 1979). The equations for the overall reinforcement rates are in Heyman (1982) and Houston and McNamara (1981).

| Choice: $T_1/(T_1 + T_2)$ | Reinforcement %[c] | Reinforcements/hour: independent VI VI | Reinforcements/hour: interdependent VI VI |
|---|---|---|---|
| 25% | 74.0% | 28.815 | 28.368 |
| 50% | 74.7% | 29.495 | 29.190 |
| 60% | 74.9% | 29.608 | 29.283 |
| 65%[a] | 75.0% | 29.647 | 29.296 |
| 70% | 75.1% | 29.676 | 29.283 |
| 75% | 75.2% | 29.694 | 29.236 |
| 78%[b] | 75.3% | 29.696 | 29.183 |
| 80% | 75.3% | 29.695 | 29.133 |
| 90% | 75.9% | 29.578 | 28.477 |

[a]65% is the choice proportion that maximizes reinforcement rate in the interdependent procedure.
[b]78% is the choice proportion that maximizes reinforcement rate in the independent procedure.
[c]The expected reinforcement proportions are for the independent procedure. The expected reinforcement proportion for the interdependent procedure is 75% $(V_2/(V_1 + V_2))$ for all choice proportions between 0.0 and 1.0.

INTERDEPENDENT CONCURRENT INTERVAL SCHEDULES

Heyman and Luce pointed out that the time allocation proportion that maximizes reinforcement rate in concurrent schedules with interdependent timers predicts sizeable undermatching, whereas subjects approximate matching. Houston does not directly dispute the discrepancy from maximizing theory, but he does suggest that there are errors in Heyman and Luce's model and discussion of the experimental results. The criticism of the model has to do with the two different ways of arranging interdependent schedules.

The purpose of the interdependent procedure is to hold the obtained reinforcement proportions precisely constant. This is arranged by (a) using two variable-interval timers and stopping both whenever one sets up a reinforcer (e.g., Fantino et al., 1972), or by (b) using a single timer and assigning a reinforcer to one of the response levers when the interval elapses (Stubbs and Pliskoff, 1969). The two contingencies differ in only one way. In the single timer contingency, the reinforcement establishes new intervals at both response manipulandums with expected durations of $pV$ and $(1 - p)V$, where $p$ is the probability of assignment and $V$ is the mean timer interval (see Staddon et al., 1981, for details). In the two variable-timer procedure, a reinforcement does not reset the timer interval at the unattended response manipulandum. This difference appears to be without theoretical significance, since the maximizing solutions for the two procedures are virtually identical (compare Heyman and Luce, 1979, with Staddon et al., 1981).

The interdependent schedule is relevant to maximizing theory because of its relationship with the independent conc. VI VI schedule: the reinforcement contingencies are different but the response requirements are identical. Consequently, maximizing theory must predict that subjects will behave differently in the two procedures, with undermatching the prediction for the interdependent. Houston suggests that, contrary to Heyman and Luce's discussion, the maximizing predictions fit the data. However, the most comprehensive review to date (Baum, 1979) shows no such trend: matching implies a slope of 1.0 for the line describing the correlation between reinforcement ratios and time allocation ratios (Baum, 1974). The median slope (by study) of the regression line is 0.96 in the independent procedure, and it is nearly the same, 0.98, in the interdependent procedure (Baum, 1979). This does not mean that all aspects of performance in the two procedures are the same; however, no systematic difference relevant to maximizing theory has been reported.

CONCURRENT VARIABLE-INTERVAL VARIABLE-RATIO SCHEDULES

Most researchers agree that maximizing theory is incompatible with performance in conc. VI VR schedules (but for an exception, see Rachlin et al., 1981). For example, if Houston's model of response cost (Appendix 3) is combined with the

results reported by Herrnstein and Heyman (1979), maximizing theory leads to the unrealistic prediction that the subjects were obtaining negative reinforcement rates. (This is because the departures from the maximizing time allocation proportions were so large that it was necessary to assume relatively high response costs in order to rationalize the obtained time allocation proportions as an instance of maximizing.) Possibly a plausible maximizing account of conc. VI VR performance will appear, but for reasons given elsewhere, this seems unlikely (Baum and Nevin, 1981; Herrnstein, 1981).

THEORETICAL ISSUES

*Mathematical models*

In Appendix 1 and Table 1, Houston compares the Staddon and Motheral (1978) and the Heyman and Luce models of conc. VI VI performance. This is of some theoretical importance, because Houston writes that there are conditions in which the two models make identical predictions, even though the authors claim opposite conclusions.* Houston's comparison, however, requires conditions that either do not occur or are not logically possible. For example, Houston assumes that local response rate varies with changeover rate, yet it is well established that the two measures are independent: changeover rate varies with time allocation, whereas local response rate remains approximately constant with changes in time allocation (e.g., Stubbs and Pliskoff, 1969). The details of this and related problems are described in Appendix 1, below.

*Evolutionary accounts of matching*

At present there are several different maximizing theories of operant behavior. The original one was that animals maximize overall reinforcement rate. However, most researchers now acknowledge that this is incorrect (see, e.g., Baum, 1981; Staddon and Motheral, 1978). One response has been to suggest an evolutionary theory of operant behavior that entails an optimization principle (e.g., Staddon, 1980). Matching, it is said, evolved because it was the optimal pattern of responding in the animal's natural environment. Thus, in experiments that provide conditions that are similar to the animal's niche, matching will produce high reinforcement rates, but in experimental settings that do not mimic the animal's environment, matching will not be the adaptive behavior.

Houston criticizes Heyman and Luce for not discussing evolutionary accounts of matching. However, at the time that we wrote our paper it was believed that ‐

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*In an earlier paper (Heyman, 1979), I stated that the Staddon and Motheral (1978) and Heyman and Luce (1979) models predicted the same reinforcement rates in the special condition that the subject switched sides after each response. This is incorrect, as Staddon and Motheral (1979) and now Houston (1982) point out.

maximizing overall reinforcement rate and matching were equivalent (e.g., Rachlin et al., 1976) and there were no alternative theories to test. Moreover, now some four years later, the evolutionary theory is still more a suggestion than a testable hypothesis. Note that there are two important assumptions: matching is the product of selective pressures and matching evolved because it was optimal. However, the logic of evolutionary processes allows for biological phenomena that are not the product of adaptation, and even with selective forces, an optimal solution is by no means necessary. Whatever works best given the alternatives is most likely to persist.

It is, I believe, fair to say that matching remains a mystery. All species yet tested choose between two reinforcement schedules according to the symmetry described by Equation 1. However, why organisms should do so remains unexplained.

APPENDIX 1

Houston states that there are conditions in which the Staddon and Motheral (1978) and the Heyman and Luce models of conc. VI VI schedules predict the same reinforcement rates. These conditions, however, include relations that are contradicted by the data and are logically impossible. Consequently his claim is incorrect. First, consider Houston's extension of the Heyman and Luce model. Houston writes that the Heyman and Luce model implies that the local response rates and changeover rates are related by the expression, $LR_i = 1/r_i + u_i$, where $LR_i$ is the local response rate at manipulandum $i$, $r_i$ is the mean local interresponse time at manipulandum $i$, and $u_i$ is the mean rate of switching from manipulandum $i$. Note that Houston's modification assumes a correlation between local response rate, $LR_i$, and changeover rate, $u_i$. However, there is no logical connection between these two measures, and, as mentioned above, there is no empirical connection either.

Second, in keeping with the data, the Heyman and Luce model says nothing explicit or implicit about the relationship between local response rate and changeover rate. The model was derived for procedures that do not use a response manipulandum, which is equivalent to a local response rate of 0.0, and if the model is extended to include a representation of a response requirement then it is necessary to estimate the expected time from the termination of a timer interval to the next response. The pattern of responding determines this delay period. For example, for an exponential distribution of interresponse times, the expected delay is the local interresponse time, $r_i = t_i/b_i$, where $t_i$ is total time at manipulandum $i$ and $b_i$ is total responses at manipulandum $i$. For a discussion of the delay for other interresponse time distributions, see Heyman (1982).

Third, Houston's modification of the Heyman and Luce model leads to logical contradictions. Throughout the paper, Houston uses the notation $r_i$ to indicate the mean interresponse time at manipulandum $i$, where $r_i$ is defined as above. Then he modifies the Heyman and Luce model with the definition that the local response rate is $1/r_i$ plus the probability of switching, $u_i$. This makes no sense since by definition

$r_i$ already includes all responses and all time spent at manipulandum $i$. Another way to see that this definition is logically faulty is to consider the relationship between the average number of responses per visit to a manipulandum and the total number of responses at the manipulandum. This relationship is simply $B_i = (C/2)S_i$, where $B_i$ is total responses at the manipulandum, $C$ is total changeovers, and $S_i$ is the average number of responses per visit. In other words, the average number of responses per visit is precisely proportional to the overall response totals: $B_1/B_2 = S_1/S_2$. However, according to Houston's modification of the Heyman and Luce model there are $1/r_i u_i + 1$ responses per visit to a manipulandum, and this leads to the erroneous conclusion that the average number of responses per visit is not proportional to the total number of responses. (To determine the average number of responses per visit from Houston's modification of the Heyman and Luce model solve the equation $a_i r_i/(1 + r_i u_i) = 1/u_i$, where $a_i$ is the average number of responses per visit and the other notation is as previously defined. Also note that it is generally the case that $r_1 = r_2$.)

The Staddon and Motheral model (1978) describes reinforcement rate as a function of the overall response rates. Houston extends the model to include definitions of the local response rates and changeover rates. His modifications are similar to those made by Staddon and Motheral (1979) in a rejoinder to a critique of their original model. These modifications lead to logical contradictions.

Houston writes that according to Staddon and Motheral (1978, 1979) the expected visit times at a manipulandum are related to the overall response rates, as follows. Let $x$ and $y$ be the two overall response rates. For example at manipulandum 1, the overall response rate is $x = b_1/(t_1 + t_2)$, where $b_1$ is total responses at the manipulandum and the denominator is the entire session time. Next, it is stated that the average time per visit to a manipulandum, $t_i$, is equal to the reciprocal of the overall response rate at the other manipulandum (see Houston, Appendix 1 and Table 6). For example, Houston says that the average time per visit to side 1 is $1/y$ (where $y$ is the overall response rate at side 2). Finally, the local response rates, $b_i/t_i$ are equal to the sum of the two overall response rates, $x + y$, which is in accord with the generally accepted assumption that $1/r_1 = 1/r_2$.

Now recall that in a concurrent schedule contingency a reinforcement can occur either during a visit, at the end of a local interresponse time, or it can occur after a changeover, at the end of a visit time to the other manipulandum. These are the only two possibilities. Therefore according to Houston's description of the Staddon and Motheral model, the expected time between reinforcements at manipulandum 1 is $V_1 + 1/(x + y)$ if the reinforcement sets up during a visit or it is $V_1 + 1/x$ if the reinforcement sets up while the subject is at manipulandum 2, where $V_1$ is the mean VI timer interval. But this contradicts Staddon and Motheral's definition that the expected time between reinforcements at side 1 is $V_1 + 1/x$. The problem is that the Staddon and Motheral model incorrectly identifies the visit time at one manipulandum, $t_i$, with the reciprocal of the overall response rate at the other manipulandum.

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