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The Empirical Claims of Maximization Theory: A Reply to Rachlin and to Kagel, Battalio, and Green

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Kagel, Battalio, and Green (1983) and Rachlin (1983) claim to find errors in Prelec's (1982) critique of maximization theory. They present new evidence, which, they suggest, demonstrates that maximization accounts better for equilibrium behavior on schedules of reinforcement than does matching. On closer examination the allegations of mathematical and conceptual errors are found to be without substance and the new data do not differentially favor maximization or matching. Hence, the conclusions drawn in Prelec (1982) do not at this time need revision.

The utility-maximization hypothesis combines a mathematically sophisticated theory of value with a minimal theory of schedule performance. The subject, it assumes, maximizes a utility function defined over all psychologically distinct states of his environment, which in the molar version of the theory are the long-run distributions of responding and reinforcement. Schedules influence performance in only one way—by making some states of the environment physically possible and others not.

Thus at its heart this theory contains a conception of operant behavior that is extraordinarily simple and well worth testing. To do so, one turns to the equations that define the maximum of any utility function, $U(r_1, \ldots, b_1, \ldots, b_n)$, having rates of reinforcement (r_i), responding (b_i), and nonresponding (b_n) as arguments. Maximization requires that at equilibrium the net marginal benefit (NMB) for responding is the same for each activity, scheduled ($i < n$) and unscheduled ($i = n$); see, e.g., Intriligator, 1971, chap. 4:

$$\mathrm{NMB}_i \equiv \frac{\partial U}{\partial r_i}\frac{dr_i}{db_i} + \frac{\partial U}{\partial b_i} \leqq \mu, \quad i = 1, \ldots, n; \tag{1}$$

$$\sum_{i=1}^{n} b_i(\mathrm{NMB}_i - \mu) = 0. \tag{2}$$

According to the familiar first-order conditions set forth in Equation 1, the actual NMB for responding on any schedule is computed by first multiplying the marginal rate of reinforcement (MRR) on that schedule (dr_i/db_i, or MRR_i) with the marginal utility of its reinforcer ($\partial U/\partial r_i$) and then adding the marginal utility of responding ($\partial U/\partial b_i$), which may be negative. A schedule that cannot match the common $\mathrm{NMB} = \mu$ offered by its competitors will, by Equation 2, drop out of the picture; more formally, $\mathrm{NMB}_i < \mu$ implies that $b_i = 0$. One should keep in mind that the inferred terms $\partial U/\partial r_i$ and $\partial U/\partial b_i$ are not constants but functions, providing the theory with a great deal of flexibility. To expose the empirical claims of maximization theory, one must search for situations where these functions—ordinarily acting as free parameters—cancel out.

In my article (Prelec, 1982) I stated that the matching response rule differs from the maximizing scheme in only one essential way: It replaces the marginal term dr_i/db_i with a local or average rate of reinforcement, r_i/b_i. Starting from that initial modification, one can proceed to construct a matching theory with as complex a value structure as one likes. As with maximization theory, one simply makes provision for more and more elaborate rescaling of the physical reinforcement and response variables. Aside from a few brief comments about the so-called *reinforcement value functions* (pp. 195, 202–203), this is not something I had attempted to do in the article. Rather, I devoted a large part of it to a careful examination of two special versions of matching and maximizing theories—versions that kept rescaling to a minimum. In retrospect, it appears that the attention I devoted to these special theories obscured one fundamental point (but see the discussion on pp. 218–222): that the difficulties with the maximization hypothesis have very little to do with the restrictive assumptions of linear utility used in the special theory but carry over, without modification, to the general utility-maximization model.

The Single-Key, Variable-Interval Paradox

A critical problem with maximization theory arises out of the orders-of-magnitude difference between typical variable-interval (VI) and variable-ratio (VR) marginal rates of reinforcement. As an example of this, I selected a bird that was executing

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72 pecks per minute on a VI 5.4-minute schedule yielding a MRR of less than 1/150,000 in Catania and Reynolds's (1968) experiment. I suggested that this same subject—if it was indeed a maximizer—should respond at that same rate, at least, when placed on a VR 150,000-response schedule. Rachlin (1983) and Kagel, Battalio, and Green (1983) question this inference, for somewhat different reasons, and believe that in reaching it I have made a number of logical and interpretive errors. Before replying to their arguments, I want to attend to the specific claim, made by Kagel et al. (1983, p. 381) that my conclusion here "is blatantly incorrect even for the simple linear utility function," which creates an impression that I have committed a mathematical error of some kind. According to the special maximization model (Prelec, 1982, pp. 193–204), the subject in a single-schedule VI d' experiment maximizes a total utility function that looks like this:

$$U(b_1, b_n) = \frac{b_1}{db_1 + 1} + \frac{b_n}{d_n b_n + a_n}; \qquad (3)$$

whereas in the single-schedule VR a experiment he maximizes

$$U(b_1, b_n) = \frac{b_1}{a} + \frac{b_n}{d_n b_n + a_n}. \qquad (4)$$

The first term in both Equations 3 and 4 gives the obtained rate of reinforcement if response rate is b_1; the second term gives the utility derived from *not* responding at rate b_n—utility of leisure, if you will.[1] Now let b_1° and $b_1^{\circ\circ}$ be the rates of responding that maximize utility in the two experiments, subject to the usual constraints, that is, that rates of response and nonresponse sum up to k and do not fall below zero. If the ratio requirement in the second experiment happens to equal the MRR on the VI, evaluated at the point (b_1°, b_n°) where Equation 3 attains a maximum,

$$a = \frac{dU}{db_1}(b_1^\circ, b_n^\circ) = \frac{1}{(db_1^\circ + 1)^2},$$

then $b_1^{\circ\circ}$ will equal b_1°. This is a simple mathematical fact about which nothing more need be said.

Granted, the special model does not contain a response-cost term nor does it allow for diminishing marginal returns to reinforcer consumption. Let me examine what happens when these problematic restrictions are removed. I assume, now, that the utility function has the form

$$U(b_1, b_n) = U_r(r_1(b_1)) + U_b(b_1) + U_n(b_n), \qquad (5)$$

where U_r is the utility of reinforcement, U_b is the cost of responding, and U_n, as before, is the utility of leisure. The marginal utility of food and leisure is presumed decreasing, $U_r'' < 0$ and $U_n'' < 0$, whereas the marginal cost of responding, as Kagel et al. (1983) suggest, is increasing, $-U_b'' > 0$. These are perfectly conventional assumptions, consistent with the utility models that have so far appeared in the maximization literature (e.g., the power function by Rachlin, Kagel, & Battalio, 1980, the constant elasticity of substitution [CES] utility function by Green, Kagel, & Battalio, 1982, etc.). Maximizing utility in the VI experiment requires that the NMB for responding (see Equation 1) equals the marginal benefit of resting;

$$\frac{U_r'(r_1(b_1^\circ)}{150{,}000} + U_b'(b_1^\circ) = U_n'(b_n^\circ).$$

(Remember that $dr_1/db_1 = 1/150{,}000$ at $b_1^\circ = 72$.) Replacing the VI by a VR 150,000 schedule makes the NMB for responding, at that same rate of response, higher than before,

$$\frac{U_r'(72/150{,}00)}{150{,}000} + U_b'(b_1^\circ) > U_n(b_n^\circ),$$

because the marginal utility of food is greater at the now exceedingly low rate of consumption, $r_1 = 72/150{,}000$. The optimal rate of response on the VR, $b_2^{\circ\circ}$, must be a consequence rise above the VI optimum rate of 72 responses per minute. Therefore, allowing for nonlinear utility aggravates this particular difficulty with the maximization hypothesis.

Kagel et al.'s (1983) implication of the average rate of reinforcement (ARR) is entirely inappropriate here and is at odds with a well-known theorem in mathematical economics (Malinvaud, 1972, p. 312) that states that the marginal conditions, in the extended sense of Equations 1 and 2, are both necessary and sufficient to identify the maximum of any concave utility function over a convex budget set. All utility functions proposed by maximizing theorists, including Green et al. (1982), have up to now been concave. Maximization can predict zero response rates on some schedules, but the criteria for "shutting down" are strictly marginal (e.g., Equation 2).

It is, of course, totally implausible that "3 seconds of access to a food hopper . . . every 4.2 hours is sufficient to cover the cost of continuously key-pecking over this time interval," (Kagel et al., 1983,

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[1] Rachlin (1983) incorrectly specified the *linear* utility model. In the special theory, the hyperbolic function does double duty, first as a feedback function for schedules and second as a utility function for an unspecified number of background, leisure activities (Prelec, 1982, pp. 193, 201–202). The aggregation theorems (Prelec, 1982, pp. 207–209) permit compression of all leisure activities into a single one (as is shown here by Equations 3 and 4). The utility function for leisure is thus *nonlinear,* hyperbolic.

p. 381), which is why I brought up this example in the first place. The question is not whether the pigeon will quit on the VR 150,000 but, rather, why he tolerates that same MRR on the VI. If the reader is wondering whether there is something intrinsically peculiar about VI schedules or about this subject who responded 72 times per minute on the VI 5.4-minute schedule, I should like to point out that the paradox dissolves when one uses the matching rule to predict which VR schedule ought to maintain a comparable rate of response. The matching subject who pecks 72 times per minute on the VI 5.4-minute schedule would also do so on a VR with a ratio requirement equal to his current ARR. The exact value here is $db_1 + 1$, or approximately 400 responses, which accords well with the VR data presented in Figure 1 of Kagel et al. (1983).

Rachlin (1983) countered this paradox by drawing a distinction between responding continuously on the VR 150,000 and gambling away one response for a 1/150,000 chance of food. There is no warrant for making this distinction because the molar-maximizing (or molar-matching) theory is concerned only with equilibrium behavior; that is, behavior that is stable from session to session. Comparison of Equations 3 and 4 shows, with perfect symmetry, that the same response rate, maintained session after session, is a utility maximum in both the VI 5.4-minute schedule and VR 150,000 experiments. A similar misunderstanding appeared in Rachlin's two footnotes in which he argued that my construal of maximization theory requires that it hold at the molecular level, that is, that maximizing has to specify certain properties of responding that matching theory can safely ignore. This is not correct. To repeat, both matching and maximizing theories are defined with respect to molar, long-run measures of responding and reinforcement (b_i and r_i). It is not possible to produce *any* claims about local, molecular changes in response by merely subjecting these molar variables to various mathematical operations (differentiation, integration, etc.). The term dr_1/db_1, for example, refers to a small change in the long-run rate of reinforcement that would be induced by a small change in the long-run rate of response and not to any local contingency.

Linear VI Schedules

As early as 1976, Vaughan (1976, 1982) demonstrated, by means of linear VI schedules, that the MRR on VI schedules is not a significant factor in performance. Kagel et al. (1983) are puzzled by linear VI schedules—they wondered if, perhaps, I am not confusing them with variable-time (VT) schedules. The operation of the linear VI, as Vaughan (1982) described it, is straightforward: The schedule can be likened to a mechanism that deposits rewards into a container at a constant rate irrespective of responding. Reaching into the container is the response that, as one would expect, collects a single reward if the container is not empty. As long as one reaches into the container at a faster rate than the rate at which rewards are deposited, one will collect all rewards. Therefore, for all rates of response greater than the scheduled rate of reinforcement, the asymptotic MRR is zero, exactly.

However, Kagel et al. (1983) are right to direct attention to the VT schedule, because this schedule indeed reaches the heart of the matter. The generic difference between the VT schedule and all standard schedules, including the linear VI, is that the VT gives rewards freely, even when there is no responding. For any reinforcement theory this difference is critical because the VT schedule does not *reinforce* behavior in the traditional sense of that word. Matching theory, being essentially a quantification of reinforcement principles, requires that events entering into the reinforcement-rate measure, r_i, actually be reinforcements, that is, events conditional on the responses that are being cumulated into the associated response-rate measure, b_i (Prelec, 1982, p. 195). Therefore, matching predicts that of the three schedules, VI, linear VI, and VT; the first two will maintain similar rates of responding, whereas responding on the third one (VT) should, in the absence of reinforcement, hover close to zero. Maximization, on the other hand, must hope to locate the cause of the difference between VI and VT responding in the difference between the positive VI MRR and the zero VT MRR. This much Kagel et al. readily admit. But, now, they and other maximization theorists should reflect carefully on the significance of Vaughan's (1976) linear VI procedure: If, as they believe, only the margin matters, then the linear VI, offering no molar incentives to responding above the minimal collection rate, should fall in with the VT and drive the response rate down to a very low level. As it turns out, behavior on the linear VI is not noticeably different from behavior on an ordinary VI schedule.

The Concurrent VI–VR Experiment

When two concurrent schedules deliver the same reinforcer and require an equivalent response, then the maximizing subject ought to maximize a utility function in which the two kinds of responses and reinforcements are perfect substitutes:

$$U = U(r_1 + r_2, b_1 + b_2, b_n). \qquad (6)$$

In my article, I observed (Prelec, 1982, pp. 219–221) that at the optimal response distribution the subject equalizes the MRR on the two schedules, irrespective of the precise mathematical specification of U. For concurrent VI–VR schedules this implies biased matching, with a bias coefficient approximately equal to the square root of the response requirement on the VR schedules (Theorem 2); hence, for the VR 100 we should find a bias of 10, and so forth. This does not happen. What one does observe (Herrnstein & Heyman, 1979) is matching with a bias of about 2 if b_1 and b_2 are measured in response rates and a bias slightly less than 1 (i.e., in favor of the VI) if they are measured in time units.

One should appreciate that Equation 6 makes it possible to accommodate fully the impact of leisure and response cost on total utility. The function $\partial U/\partial b_n$ gives the marginal utility of leisure at different rates of responding, reinforcement, and resting, and the function $\partial u/\partial(b_1 + b_2)$, presumably negative, the marginal disutility, that is, cost of responding. Nothing requires these functions to be especially simple, let alone linear. Thus the claim by Kagel et al. (1983, p. 382), that I have assumed that "the marginal cost of responding is constant," is a misreading of the argument.

The crucial assumption that *is* contained in Equation 6 is that the subject cares only about the total sum of responses and the total sum of reinforcers and not about how these two totals are distributed between the schedules. As I remarked (Prelec, 1982, p. 221), a reconciliation of utility-maximization theory with the concurrent VI–VR findings, in whatever guise, has to entail the abandonment of perfect substitutibility of the two kinds of responses and reinforcements. Now I examine the proposed explanation of Kagel et al.

By allowing for increasing marginal costs of responding, they argue, maximization theory can drive a wedge between the marginal rates of return on the VI and VR schedules. On the surface this is a very odd claim, for nothing in the utility specification (Equation 6) is incompatible with increasing marginal cost (i.e., with $-\partial^2 U/\partial(b_1 + b_2)^2 > 0$). It is apparent that Kagel et al. have in mind a specification:[2]

$$U = r_1 + r_2 - U_b(b_1) - U_b(b_2), \tag{7}$$

in which the response-cost function, U_b, is applied to each rate of response individually, rather than to the total response output. Why not write, more plausibly,

$$U = r_1 + r_2 - U_b(b_1 + b_2)? \tag{8}$$

Unlike Equation 7, Equation 8 retains substitutibility and is therefore incapable of separating the VI–VR margins. With Equation 7, however, substitutibility is quietly abandoned—it is this feature, rather than the innocuous assumption of increasing marginal cost, that is crucial. According to Equation 7, not only are responses costly, but what is especially costly is to have an unequal division of responses between the two schedules. If the cost of responding is marginally increasing, $U_b'' > 0$, then the total cost, $U_b(b_1) + U_b(b_2)$, of the same total response output $(b_1 + b_2)$ will increase with departures from indifference (i.e., departures from $b_1 = b_2$). Given sufficiently sharp second derivatives on U_b, the utility specification (Equation 7) can account for any response distribution located somewhere in between indifference and the nominal reinforcement-rate maximum at the point of equal margins. But, surely, an explanation is wanting here. Why should preference for indifference, per se, enter into the utility function?

Objections to Matching

Rachlin (1983) and Kagel et al. (1983) present some new data that, although interesting, do not help differentiate between the matching and the maximizing rules. Figure 1 of Kagel et al. shows that the VR response function is bitonic. In my article (Prelec, 1982), I showed that when utility/value is linear in reinforcement rate, then both matching *and* maximizing predict monotonic decreasing VR response functions (pp. 214–215). The maximization models that predict bitonic functions do so at the cost of nonlinear rescaling of reinforcement rates. If the reinforcement value functions, which carry out the rescaling within a general matching theory (p. 203), allow for reinforcement value to decrease sharply with total consumption, then matching, too, can predict bitonic VR functions. One certainly cannot claim that the privilege of rescaling the physical variables to which a theory applies is a privilege uniquely granted to maximization theory. It may be, as Kagel et al. (1983) suggest, that reinforcement value does not stay constant beyond, say, 300 reinforcements per hour—but that is hardly relevant to the matching–maximizing issue.

In Figure 1, Rachlin (1983) presents new data that are not compatible with matching, but he neglects to say how these data reflect on the maximization hypothesis. The feedback functions for the novel schedules used in that experiment are, in fact, hyperbolas, as long as responding is random in time.[3] Applying the usual results on concurrent

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[2] This is the specification that is provided in an earlier draft of their article.

[3] Schedule B is a regular VR schedule. On Schedule A, with response requirement equal to N, reinforcement sets up after $(N - 1)$ pecks of either kind; thus the average *scheduled* interreinforcement interval, so to speak, is equal to $(N - 1)/k$, where k is the total rate of response, $b_A + b_B$. Using the usual derivation for VI feedback functions (Prelec, 1982, pp. 198–201), we obtain the hyperbolic form,

$$r_A = \frac{b_A}{\left(\frac{N-1}{k}\right)b_A + a_A}.$$

If responding is random, then a_A, the VI response requirement, equals one.

VI–VR experiments (Theorem 2) shows that maximization would be consistent with a bias equal to the square root of the ratio requirement on the straight VR schedule (Schedule B). In Green's experiment (Rachlin, 1983) this parameter takes on four different values, yielding bias coefficients of $\sqrt{15}$, $\sqrt{30}$, $\sqrt{60}$, and $\sqrt{90}$, which are clearly not found in Figure 1. These data could have just as well been plotted as deviations from maximization, in which case they would appear to support the matching hypothesis.

One minor point remains to be cleared up. Kagel et al. (1983, p. 383) feel that my explanation of the VI–VR bias (shown also in their Figure 2) introduces a free parameter, "with no coherent explanation," into matching theory. As I explained (Prelec, 1982, pp. 193–195), each behavioral measure, b_i, needs to be scaled against its maximum value, k_i, before either the matching or the maximizing equations can be applied. This is because both theories deal with behavior allocation after the vagaries of topography have been neutralized. That there exists a topographical difference between VI and VR pecks, with VR pecks being faster—more like rapid bursts—is a straightforward empirical hypothesis; if it turns out to be false, then some fundamental assumptions about matching may have to be revised. As preliminary evidence favoring this hypothesis, let me point to (a) the difference in local rates of response on concurrent VI–VR schedules and (b) the difference in apparent response ceilings on single-key VI and VR schedules (see, e.g., Figures 2 and 4 in Prelec, 1982).

Substitutibility

It is true, as Rachlin observes, that the special matching theory does not directly apply to experiments with qualitatively different reinforcers, such as food and water. This theoretical restriction of range is consistent with a long tradition in operant work that holds that a comprehensive understanding of performance should be pursued by means of experiments in which the motivational variables either cancel out (as in the concurrent schedule case) or are kept under tight control. I do not see anything eccentric in setting aside the complex problems of reinforcer interaction during the initial stages of theory construction, and the present note is certainly not the place to start a discussion of how a framework for treating these problems can be constructed from matching like response rules.[4] Rachlin's interpretation of the economic postulates may indeed contribute to our comprehension of these interactions in the sense that the response- and reinforcement-rescaling operations may prove derivable from internal preference orderings of some kind; however, as a theory of schedule performance, the molar-maximization hypothesis will not do, even if one places at its disposal the full flexibility of the economic theory of value.[5]

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[4] A draft article entitled "A matching-based theory of value, and its normative implications" that formally addresses these issues can be obtained on request from the author.

[5] Those who are still in doubt about this point may wish to look at Mazur (1981).

References

Catania, A. C., & Reynolds, G. S. A quantitative analysis of the responding maintained by interval schedules of reinforcement. *Journal of the Experimental Analysis of Behavior,* 1968, *11,* 327–383.

Green, L., Kagel, J. H., & Battalio, R. C. Ratio schedules of reinforcement and their relationship to economic theories of labor supply. In M. Commons, R. J. Herrnstein, & H. Rachlin (Eds.), *Quantitative Analysis of Behavior* (Vol. 2). Cambridge, Mass.: Ballinger, 1982.

Herrnstein, R. J., & Heyman, G. M. Is matching compatible with reinforcement maximization on concurrent variable-interval variable-ratio? *Journal of the Experimental Analysis of Behavior,* 1979, *31,* 209–223.

Intriligator, M. D. *Mathematical optimization and economic theory.* Englewood Cliffs, N.J.: Prentice-Hall, 1971.

Kagel, J. H., Battalio, R. C., & Green, L. Matching versus maximizing: Comments on Prelec's paper. *Psychological Review,* 1983, *90,* 380–384.

Malinvaud, E. *Lectures on microeconomic theory.* Amsterdam–London: North-Holland, 1972.

Mazur, J. E. Optimization theory fails to predict performance of pigeons in a two-response situation. *Science,* 1981, *214,* 823–825.

Prelec, D. Matching, maximizing, and the hyperbolic reinforcement feedback function. *Psychological Review,* 1982, *89,* 189–230.

Rachlin, H. How to decide between matching and maximizing: A reply to Prelec. *Psychological Review,* 1983, *90,* 376–379.

Rachlin, H., Kagel, J. H., & Battalio, R. C. Substitutibility in time allocation. *Psychological Review,* 1980, *87,* 355–374.

Vaughan, W., Jr. *Optimization and reinforcement.* Unpublished doctoral dissertation, Harvard University, 1976.

Vaughan, W., Jr. Choice and the Rescorla-Wagner model. In M. Comons, R. J. Herrnstein, & H. Rachlin (Eds.), *Quantitative analysis of behavior* (Vol. 2). Cambridge, Mass.: Ballinger, 1982.

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