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THE MATCHING LAW

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The matching law may be viewed either as an empirical generalization, and therby subject to disproof, or as part of a system of equations used to define the utility ("value") of a reinforcer. In the latter case it is tautologous, and not subject to disproof within the defining context. A failure to obtain matching will most often be a signal that the independent variables have not been properly scaled. If, however, the proper transformations have been made on the independent variables, and matching is not obtained, the experimental paradigm may be outside the purview of the matching law. At that point, reinterpretations or revisions of the law are called for. The theoretical matching law is but one of many possible ways to define utility, and it may eventually be rejected in favor of a more useful definition.

Rachlin (1971) suggested that the matching relation "is not an empirical law but a restatement of assumptions made prior to empirical test". The first section of the present paper argues that the matching relation may be seen as one of several "working assumptions" involved in a particular model of choice behavior. The second section analyzes the applications of that model with special reference to the claims made by Rachlin for the matching law. The final section notes that while assumptions such as the matching law are necessary, they must be continually reevaluated in the light of new data.

I

Rachlin begins his argument with Herrnstein's (1961) early formulation of the matching relation: when pigeons are submitted to concurrent variable-interval (VI) schedules, the ratio of response rates on the two schedules equals the ratio of their reinforcement rates. Rachlin modifies this equation: "A more general measure of behavior than pecks would be time as it is distributed among the alternatives". For the right side of the equation, Rachlin suggests the ratio of the values of the reinforcers, which is equal to the product of the ratios of amount, frequency, and immediacy of reinforcement, and the ratio of other parameters of reinforcement that "act multiplicatively in the same way". In mathematical form (Rachlin's Equation 3),

$$\frac{T_L}{T_R}=\frac{R_L}{R_R}\cdot\frac{A_L}{A_R}\cdot\frac{I_L}{I_R}\cdot\frac{X_I}{X_R}=\frac{V_L}{V_R} \tag{1}$$

Where,

T equals time spent on the schedule
R equals rate of reinforcement
A equals amount of reinforcement
I equals immediacy of reinforcement
X equals a parameter of reinforcement other than rate, amount, or immediacy
V equals value of reinforcement

The subscripts denote the schedule with which each variable is associated.

Rachlin takes this equation as a general statement of the matching law, and interprets it as saying that "preference for one alternative over another equals the reinforcement [value?] consequent upon choosing that alternative as a fraction of the reinforcement [value?] consequent upon choosing the other". But this "equation" is actually three equations:

$$\frac{T_L}{T_R}=\frac{R_L}{R_R}\cdot\frac{A_L}{A_R}\cdot\frac{I_L}{I_R}\cdot\frac{X_L}{X_R}, \tag{2}$$

$$\frac{T_L}{T_R}=\frac{V_L}{V_R}, \tag{3}$$

and

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$$\frac{V_L}{V_R}=\frac{R_L}{R_R}\cdot\frac{A_L}{A_R}\cdot\frac{I_L}{I_R}\cdot\frac{X_L}{X_R}. \quad (4)$$

All three of these formulae assert an equality; which one is the matching law? It certainly cannot be Equation 2, which is the type of relation we typically think of as matching. Catania (1963) reported, for instance, that the proportion of time pigeons spend on a schedule is equal to the proportion of reinforcers they receive there:

$$\frac{T_L}{T_R}=\frac{R_L}{R_R}. \quad (2a)$$

This empirical matching relation cannot be an instance of the law that "is not subject to empirical test", for Catania might conceivably have found:

$$\frac{T_L}{T_R}=\frac{R_L{}^2}{R_R{}^2} \quad (2b)$$

But if we are to take either Equation 3 or Equation 4 as the theoretical matching law, the remaining equation will have to be modified. Those two equations give us two independent ways of defining the intervening variable "value", and we cannot presume that they will always generate the same number. For instance, if Catania had found Equation 2b to be true, rather than Equation 2a, we would have to conclude either that value ratios equal the square of reinforcement ratios, or that time ratios equal the square of value ratios. Alternatively, we might invoke the action of "hidden reinforcers" (*i.e.*, "X") to account for discrepancies. But such hypostatization is considered repugnant, unless it is followed by an experimental demonstration and control of the hypothetical reinforcers.

Rachlin selects Equation 3 as the theoretical matching law, starting that in some instances it may be necessary to modify Equation 4, perhaps by postulating a relationship such as

$$\frac{V_L}{V_R}=\log\frac{X_L}{X_R} \quad (4a)$$

in order to retain the quality of time ratios and value ratios. If we are to be permitted such latitude in postulating transformations, however, they should be represented in our formulae. Equation 4 should be:

$$\frac{V_L}{V_R}=\frac{f_1(R_L)}{f_1(R_R)}\cdot\frac{f_2(A_L)}{f_2(A_R)}\cdot\frac{f_3(I_L)}{f_3(I_R)}\cdot\frac{f_4(X_L)}{f_4(X_R)} \quad (5)$$

This equation resembles a preference structure already studied in detail by decision theorists. Equation 5 is the special case of an additive difference model of utility known as the additive model (Tversky, 1969). This can be seen more easily by taking logarithms of each side. The more general additive difference model requires additional transformations of each of the ratios. The acceptance of Equation 5 (or its more general form) does not, however, dictate the acceptance of Equation 3. Time ratios may be related to utility ratios (and thus, to the right side of Equation 5) in some more complex way than equality. But equality is the simplest relation, it has been analyzed in considerable depth (Luce, 1959), and it seems a reasonable working assumption.

The independent variables in Equation 5 are measured on arbitrary physical scales, each of which may be transformed according to different function rules. These transformed physical scales may be thought of as "subjective scales". In some cases, the function rules may be quite simple: Catania's data suggest that a similarity transformation might be adequate for reinforcement frequency.

How are we to discover the correct transformations on each of the dimensions of reinforcement? If the validity of Equation 3 is assumed, the obtained time ratios will dictate the necessary transformations. This is what Rachlin meant when he stated that Equation 1 "is not an empirical law, but a statement of how value is measured". The notion of a functional definition of reinforcers is thus extended to the functional definition of reinforcing strength. Our animals not only nominate their reinforcers, they mensurate them as well.

The present analysis separates empirical matching relations (*e.g.*, Equation 2a) from Rachlin's theoretical matching law (Equation 3). It shows that that matching law is indeed a tautology (or, more properly, an analytic statement), because time ratios define value ratios. But this tautology serves only to generate a redundant intervening variable. Since time ratios always equal value ratios, the former could be substituted for the latter wherever they occur, thus letting both "value" and the matching law drop from our vocabulary. The generality of Rachlin's matching law is uninteresting, for it can be obtained in any situation where one cares to postulate an intervening variable equal to the data in question.

If we do substitute time ratios for value ratios in a formula such as 5 (call it "5a"), we are left with a somewhat more interesting "matching law". Such a formula suggests that it is possible to predict choice behavior by a particular concatenation of subjective scales. As before, these scales are defined in terms of behavior, by transforming independent, physical measures of a stimulus so that Equation 5a is satisfied. In the context in which each scale is derived, Equation 5a will be analytic, that is, true by definition. But when interpolations or extrapolations are made, or when several subjective scales are combined, we begin to generate information not involved in the definition of those scales. It is then that we begin to make statements about the world, rather than about our use of words, it is then that prediction becomes possible, and it is then that an equation such as 5a becomes falsifiable.

Whenever we call anything by a name, we are positing an equivalence, a matching, between thing and name. But it would be misleading to set an equality sign between the thing and the name, and consider the result a "matching law". What is left out of Equation 5a is a name for the concatenation of subjective scales. "Value" may be introduced as such a name, if one takes care not to thereby infer a *new* "matching relation" between "value" and either side of Equation 5a. For such a relation would be true by definition, and its assertion would be confusing, especially in a scientific field where "matching" has other meanings.

II

The present analysis differs from Rachlin's in several ways:

A. *Assumptions.* The use of an equation such as 5a as a model of choice behavior assumes that the additive utility model is a good representation of the way in which different dimensions of reinforcement combine to determine behavior. Interactions among the subjective scales are ruled out. Other assumptions are possible, and if this equation does a poor job of predicting choice behavior, they will have to be considered.

Rachlin believes that Equation 3 is "derivable from the assumption that an organism choosing between alternatives is under no constraints except those the contingencies of reinforcement impose". I fail to see how such constraints limit behavior in the manner expressed by Equation 3.

B. *Transformations.* Rachlin's reluctance formally to introduce the transformations on physical scales that is accomplished in Equation 5 forces him to introduce them implicitly. This he does by extending the "obtained–programmed" distinction.

Organisms whose responses are reinforced on, say, concurrent VI 1-min VI 4-min schedules will often receive more than four times as many reinforcers on the shorter schedule than on the longer. It seems reasonable to use as the independent variable the obtained ratio of reinforcers, rather than that which was programmed, because it is presumably the former that controls the organism's subsequent choice behavior. Similarly, a pigeon can obtain more than twice as much food during a 4-sec hopper presentation as during a 2-sec presentation, since the bird will require almost 1 sec to move its head from the response key to the hopper. The actual eating might be estimated more accurately with a photocell in the hopper, and these obtained values used as the independent variable in Equation 5a. In both cases, it is urged that the physical measures be made as close as possible to the point of contact with the organism, but before the dimensions are transformed into subjective scales. Rachlin *equates* the transformation into subjective scales with the obtained–programmed distinction. He argues that the best measure of obtained reinforcement is that which is derived from the time ratios. Such a strategy was employed by Chung and Herrnstein (1967), who "postdicted" a 1.6-sec delay between grain presentation and the beginning of its consumption, this being the value that minimized the deviation from matching.

I would argue that it is preferable to restrict the usage of "obtained" to denote the measured dimensions of events on some arbitrary physical scale, rather than employ it as a synonym for reinforcement value. There are several justifications for this position.

1. There will be less error in our predictions if we use the actual values of the independent variables in Equation 5a, than if we use the programmed values with a correction found appropriate in other experiments (*e.g.*, subtract

1.6 sec from all hopper durations). We cannot do this if we insist on equating obtained reinforcement with reinforcement value, and relating that to programmed reinforcement with a fixed "latency-to-eat" parameter.

An alternative is to postdict this parameter in every experiment. Chung and Herrnstein's correction for latency of eating was a free parameter added onto a model similar to Equation 2a. But, if all this parameter in fact did was estimate the latency of eating, the model would be strengthened by removing it and using the actual amount or immediacy of reinforcement as the independent variable. Not only does this approach keep more variables "outside" of the organism, it prevents "latency-to-eat" from absorbing some of the variance possibly due to errors elsewhere in the model (*e.g.*, an improper transformation on amount or immediacy, or a failure of additivity).

2. While an equation such as 5a may be employed to discover the appropriate transformations on the dimensions of reinforcement, the transformed dimensions are not the same as value. Value is the product of the ratios of several of these transformed dimensions, any one of which can be ignored only when its ratio is unity. This distinction is ignored by Rachlin when he states: "When different substances are chosen between, (*e.g.*, grape juice *vs* orange juice) relative obtained reinforcement value will not equal relative amount consumed. Here, the notions of obtained reinforcement and reinforcement value are the same." Relative obtained reinforcement value will not equal relative amount consumed, but that is because a new aspect of reinforcement (*i.e.*, "type") has been introduced. It will be necessary to scale this aspect in terms of time rations, just as it was necessary to scale the amount dimension in terms of time ratios. "Type" is difficult to represent as an independent variable, because it is itself multidimensional. Until those dimensions are themselves sorted out (*e.g.*, amount of fructose, amount of citric acid, *etc.*) and reduced to independent dimensions (*i.e.*, those between which there is no interaction, so that they satisfy the assumptions of Equation 5) it may be best to treat this "dimension" of reinforcement as a nominal scale, and assign scale values directly to certain "standard" reinforcers.

C. *Conceptual status.* In the context of some standardized concurrent design, the matching relation (*i.e.*, Equation 5a) may be used as a formula for defining subjective scales of reinforcement. Because it generates scales that are consistent with our intuitions (*e.g.*, a monotonic function between amount of reinforcement and its value), we feel confident in extrapolating its use to "dimensions" of stimuli whose reinforcing value we cannot so easily intuit (*e.g.*, orange *vs* grape juice). This exercise has several justifications. Our new scale of value may help us predict behavior in different situations, such as discrimination learning or straight-alley running. This has yet to be shown. Or the transformations necessary to generate scales of value may give us new insights into the nature of reinforcement. Dalton's theory that atoms could combine only in whole-number ratios led chemists to change the way they recorded their data. "Chemists stopped writing that the two oxides of, say, carbon contained 56% and 72% of oxygen by weight; instead they wrote that one weight of carbon would combine either with 1.3 or 2.6 weights of oxygen. When the results of the old manipulations were recorded this way, a 2:1 ratio leaped to the eye . . . [Kuhn, 1970, p. 134]".

Viewed as a defining relation, matching deserves the label of "law" just as does the defining relation between voltage, resistance, and current that we speak of as Ohm's Law. It may, however, be prudent to let the matching relation retain the status of "working assumption" or "hypothesis" a while longer. Very few thorough parametric tests have been made of Equation 5a, and the limits of the concurrent procedure have not really been explored.

D. *Limitations.* "It is as difficult to imagine a system that violates the first law of thermodynamics as to imagine an unconstrained choice not strictly governed by reinforcement contingencies [Rachlin, 1971, p. 251]." "Newton's second law of motion, though it took centuries of difficult factual and theoretical research to achieve, behaves for those committed to Newton's theory very much like a purely logical statement that no amount of observation could refute [Kuhn, 1970, p. 78]." Newton's law of motion, the laws of thermodynamics, and the matching law have more in common than face validity, however. They are all paradigms, invaluable in ordering data, spotlighting anomalies, and generating the bulk of problems that occupy researchers in periods of "normal science". And, outside of the context in which they were derived, they are all falsifiable. Harré (1960) noted that: "It is only when we begin to apply the law to cases further and further removed from those cases upon which it was based that we begin to run serious risk of a contrary case appearing and disconfirming the law. If a contrary case does occur there are three possibilities open to us [p. 154]." These are:

1. We may uphold the law and merely note the contrary case. "This will be the appropriate action when there are various doubts in the investigator's mind about the control of extraneous factors in the experimental set up [p. 154]."

2. We may merely state the limitations (*e.g.*, "Boyle's Law does not hold at very high pressures"), or postulate ideal substances (*e.g.*, frictionless bodies).

3. We may attempt to "formulate a new generalization which will include all the results obtained under all sets of conditions that have been investigated [p. 155]." Herrnstein (1970) accomplished such a generalization when he extended the matching law to single operandum designs.

E. *Heuristic applications.* In a situation quite different from the one in which the matching law was articulated, Baum and Rachlin (1969) set out to measure how pigeons spend their time when spending it on different sides of a chamber was concurrently reinforced. Their hesitation to manipulate parameters of the situation to achieve matching vanished when they realized that a mismatch would not affect their assumptions about the validity of matching, but rather their assumptions about the absence of asymmetrical reinforcers in the situation. "The more our results approximated [matching], the surer we were that we had eliminated or balanced extraneous reinforcers in the situation."

While the presumption of the matching law did circumscribe their search for hidden reinforcers, weaker presumptions would have been adequate. Let v(x) be the value of some reinforcer "x". Then, for v to be a function on x, v(x) must equal v(y) if x equals y (see, *e.g.*, Halmos, 1963, p. 30). When two reinforcers are identical, their values must be equal, and animals must be indifferent between them. Such indifference is the precondition for any functional measurement, and manipulations to obtain it do not require the matching law as a rationalization, nor do they prejudice further tests of that law. Had manipulation of the changeover delay, discriminative stimuli, chamber size, *etc.*, been confined to the 50:50 condition, data collected at points other than indifference would indeed bear on the validity of the matching law for their experimental design. Thus, while the matching rule was indisputably a valuable discriminative stimulus for their precursive behaviors, those behaviors could have been emitted under the control of stimuli that required fewer assumptions about the data in question.

III

What is the place of the Matching Law in a science already burdened with theoretical assumptions (*e.g.*, the Law of Least Effort), methodological assumptions (*e.g.*, the Desirability of Recovering Baseline), and technical assumptions (*e.g.*, the use of auditory response feedback for pigeons)? Like hypotheses, assumptions facilitate further experimentation, but unlike hypotheses, they are themselves seldom the object of investigation. Assumptions permit us to get on with the business of science, mapping the functional relations and discovering the new phenomena that are necessary for a retrospective evaluation of those assumptions. Without enormous amounts of data, for instance, we cannot simultaneously generate subjective scales of reinforcement, concatenate them according to 5a, and test the adequacy of an additive utility model. By assuming the validity of 5a, however, it should be possible to approximate a consistent set of transformations in the context of which subsequent revision of 5a would be feasible.

But such assumptions grow on us. It may be true, for instance, that "no reputable student of animal behavior has ever taken the position . . . 'that species differences are insignificant, and that all responses are about equally conditionable to all stimuli' " [Skinner, 1969, p. 173]. Yet Garcia, McGowan, and Green (1971) felt that enough psychologists were taken by that position to characterize it as "a theoretical trend in American Psychology which has virtually taken the organism out of learning." The longer such assumptions go without questioning, the harder it is to revise them when they are found wanting. One of the greatest difficulties confronting Miller (1969) in his studies of operant conditioning of the viscera and glands "was the strength of the belief that the instrumental learning of glandular and visceral responses is impossible. It was extremely difficult to get students to work on this problem [p. 435]." Some fundamental assumptions may go for years without being tested. Upon demonstrating the relativity of the reinforcement relation, Premack (1963) poignantly noted that: "Since the present case appears to represent the first direct test of the [transituationality of reinforcers] assumption, the previous success of the assumption may rest largely upon a failure to have tested it [p. 88]." After a period of consolidation, assumptions must be evaluated, and continually thereafter reevaluated, in light of what they have purchased us in systematic relationships, and what they have cost us in scope.

We are not freed from the task of evaluation because an assumption deals with a functionally defined aspect of behavior. Functional definitions supply a convenient label for some concatenation of independent variables that has a predictable effect on behavior. Sometimes the effect on behavior is more obvious or easily measured than the independent variables that bring about the effect. In these cases it is tempting to nominate an event as a punisher, conditioned reinforcer, or primary reinforcer of a particular utility, because of its effects, and to use it as such without always having a complete understanding of the conditions necessary for its efficacy. Our measurement criteria become an escape latency rather than a voltmeter reading, a response rate rather than a particular pairing of stimulus, a behavior ratio rather than a deprivation period. Functional definitions are inherently no more circular than behavior-independent definitions, since within the definitional context they are both tautologous. But to the extent that every new experimental context is an occasion for redefinition of constructs such as punishers, to that extent every occasion of their use is circular, and to that extent do they become redundant with the particular independent variables used in each experiment. It is only when transituational validity of such constructs is demonstrated that they convey information. And such transituationality is often the rule, else the shock-generator, pellet feeder, and stimulus display manufacturers would long since have gone out of business. If someone suggests a new construct, say, stimulus salience, the measurement operations will be pragmatic, and the construct informative, to the extent that it can be used to make valid predictions in new situations. And some definitions (although all are tautologous) will demonstrate more transituationality than others, and thus be more useful for our science.

It was the success of equations such as 2a—empirical matching relations—that brought the attention of the operant community to the power of the concurrent operant technique, and to the advantages of a relative measure of behavior. It is inevitable that counter-instances to empirical relations such as matching will be found, at which point it will be proper to rescale the independent variables. It is also quite probable that better models of choice behavior than Equation 5a will be found, at which point it will be proper to reject the theoretical Matching Law.

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