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REANALYSIS OF THE EQUATION FOR SIMPLE ACTION

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De Villiers and Herrnstein (1976) have shown that the equation for simple action, derived from the matching law, predicts change in behavioral output for some 40 experiments in which the value of a single source of reinforcement has been varied. Using only the positive-reinforcement studies they cite that used five or more different reinforcement values, we found the high percentage of variance they report accommodated by this equation (94%) is predicated on instances of averaging rates of behavioral output before making a least-squares fit of the equation. In our reanalysis, which minimizes rate averaging, only 78% of the data variance is accommodated. This diminished data-variance accommodation can be improved by adding parameters that permit the equation's scaling constant to change as a function of reinforcement. Although these parameters permit acceptable levels of accommodation of data variance, they correspond to no obvious psychological processes. These findings support the view that the equation for simple action is an inadequate model for behavioral output.

Key words: equation for simple action, behavioral output, matching law, response rate, reinforcement rate

The matching law states that animals adjust their operant output between two alternatives (R_1 and R_2) so that, when expressed as a proportion, it equals the proportion of reinforcers (r_1 and r_2) each alternative provides. This law is usually written in either of two equivalent forms:

$$\frac{R_1}{R_1 + R_2} = \frac{r_1}{r_1 + r_2} \tag{1}$$

or

$$\frac{R_1}{R_2} = \frac{r_1}{r_2}\,. \tag{2}$$

Equation 1, which has been successful in describing large portions of the data on choice, has been modified by Herrnstein (1970) to account for response rates to single schedules. Central to this extension has been the interpretation that even single-schedule procedures involve choice—between an explicit, experimenter-arranged reinforcement schedule (r) and the reinforcements (r_e) endogenously available to an animal (e.g., exploration, defecation, etc.) that are outside of experimenter control. Called the equation for simple action (de Villiers & Herrnstein, 1976), this equation takes the form:

$$R = \frac{kr}{r + r_e}, \tag{3}$$

where k is a scaling parameter that converts the matching-equation fraction $r/(r + r_e)$ to units representative of response-rate measures (e.g., responses/min).

Equations 1 through 3 have as their virtue the ability to organize large portions of the literature on concurrent- and single-schedule procedures by means of similar equations with a unified conceptual base—that of viewing all behavioral output in terms of choice. Buttressing this conceptual parsimony is the finding that these equations do an excellent job of accounting for the choice and single-schedule data to which they are applied (Baum, 1979; de Villiers & Herrnstein, 1976).

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Regarding experiments with explicit choice procedures, Baum (1979) assessed the portion of the data variance accommodated by Equation 2 when its composite ratios were expressed in their logarithmic equivalents. To increase the generality of this functional form, he added two parameters, *a* and *b*, so that his regressions could readily accommodate variations in slope and intercept within individual data sets. Consequently, Equation 2 was modified to the following form:

$$\log \frac{R_1}{R_2} = a \log \frac{r_1}{r_2} + b. \qquad (4)$$

Although the matching law (Equation 2) predicts that *a* and *b* should equal one and zero, respectively, Baum often found different values when he calculated the values offering the best fit with Equation 4. However, although these parameters did vary from their idealized values, he showed that their inclusion permits the matching equation to account for 90.4% of the data variance in 103 data sets.

Baum's work complements the earlier efforts of de Villiers and Herrnstein. Their concern was to assess the predictive adequacy of Equation 3 in accounting for total behavioral output. Toward this end, they collected response-strength measures from 40 experiments. Using individual-subject data except where only group data were available, they calculated the values of k and r_e by means of a computer program that searched for the smallest mean squared deviation between the obtained response rates and those predicted by Equation 3. In Table 7 of their study, they compared Equation 3 with two other arbitrarily selected, negatively accelerated, monotone-increasing functional forms—the exponential and power functions. Comparing fits for all data sets where more than four different reinforcement values were used, they found that Equation 3 accounted on average for 93.4% of the variance, while the exponential and power functions accounted for only 89.4% and 89.9%, respectively. Based on its superiority of fit and its derivation from the matching law, they argued that Equation 3 offers the best account of behavioral output on single schedules.

In the present report we test the predictive adequacy of Equation 3 using the data sets originally used by de Villiers and Herrnstein. We will show that:

1. The 94% of the data variance they found that Equation 3 accommodates is due to instances of rate averaging before calculating the percent-data-variance-accommodated statistic. If rate averaging is minimized, only 78% of the variance in behavioral output is accommodated.

2. The predictive adequacy of Equation 3 can be improved if k, the asymptote of behavioral output, is permitted to vary monotonically with reinforcement levels. This adequacy can be improved still further if bitonic variation in k is permitted.

3. k varies not only with scheduled reinforcement levels, but also with endogenous reinforcement levels (r_e). In the data sets used by de Villiers and Herrnstein, significant correlations between these putatively unrelated variables can be noted.

Although the predictive success of the equation for simple action is noteworthy, its adequacy can be questioned on both conceptual and empirical grounds (e.g., see Timberlake, 1982). For example, one problem is its assumption of reinforcer commensuration between the explicit schedule (r) and the endogenous schedules (r_e): Despite the probability that these schedules' reinforcers differ in kind, Equation 3 scales them in equivalent terms.

This assumption of equivalence between qualitatively different reinforcers permits the application of the matching-equation fraction from Equation 3 [$r/(r + r_e)$] to the rate data obtained. However, this application is probably inappropriate because the matching law sometimes does not hold when choice is between different reinforcers. For example, Hursh (1978) found that when monkeys chose between food and water (what Hursh called "complementary" reinforcers), increasing rates of food reinforcement led to *increasing* rates of water-reinforced responding (see also Rachlin, Kagel, & Battalio, 1980). This outcome, which is incompatible with matching, only occurs between reinforcers that differ in kind. Unfortunately, that is typical of situations where Equation 3 is to be applied.

In addition to this problem, concern must be expressed with using the percent-data-variance-accommodated statistic as a means of testing a model's adequacy. Anderson (1977) has shown that additive models can sometimes account for very high portions of the variance in nonadditive data sets. Based on this outcome, he questioned the inferential power of correlational analysis in evaluating the adequacy of a model. Moreover, he raised the possibility that the same type of problem may be present in de Villiers and Herrnstein's use of this approach to test Equation 3 (see Anderson, 1978).

To date, little research has addressed these problems with the simple action equation. In particular, these problems have not been reflected in tests of the predictive adequacy of Equation 3, and it is this that motivates the present report. To anticipate our first conclusion, we show below that Equation 3 does less well in modeling behavioral output than has been argued by de Villiers and Herrnstein.

TESTING THE ADEQUACY OF THE EQUATION FOR SIMPLE ACTION

In order to test the adequacy of Equation 3 to accommodate variance in single-schedule procedures, we make use of de Villiers and Herrnstein's efforts by using as our data base all studies using positive reinforcement from Table 7 of their study. This table includes only those studies that used five or more different values of the reinforcement variable, and hence can provide an adequate test of a two-parameter model such as Equation 3.

Many of the data from the studies in this table had to be estimated from graphs. As a consequence, there are unavoidable errors in our estimates not only from their real values, but also from de Villiers and Herrnstein's estimates of their values. One way to test the comparability of our data estimates with de Villiers and Herrnstein's is to compare the percentage of data variance Equation 3 accommodates in our data estimates with theirs. If our estimates are close to theirs, the variance accommodated should be approximately the same. To maximize comparability, we, like de Villiers and Herrnstein, used a nonlinear least-squares fit program (Wetherington & Lucas, 1980) to determine the values of parameters in Equation 3.

As will be discussed, we handle some of the data sets from Table 7 of de Villiers and Herrnstein differently from the way they did. For the purpose of assessing the comparability of our data and curve-fitting program with theirs, we present in Table 1 those data sets that we and de Villiers and Herrnstein interpreted in the same way. Columns 1 through 4 of the table present, respectively, the study (and, where appropriate, the condition and subject) from which the data are derived, the percentage of data variance accommodated from de Villiers and Herrnstein, the same statistic from our analysis, and the difference between these two measures. With the exception of monkey Allen from the Schrier (1965) study, all the differences in variance between their analysis and ours are small. Excluding this outlier, we account on average for 1.5% less of the data variance than do de Villiers and Herrnstein.

The analysis in Table 1 demonstrates that in terms of scoring data from the graphs of different studies and fitting Equation 3 to those data, we closely approximate de Villiers and Herrnstein. However, as we see in Table 2, the way we handle these data prior to fitting Equation 3 to them affects the judgment we reach regarding the predictive adequacy of this equation.

Columns 1 and 2 of Table 2 present all the positive-reinforcement data sets from Table 7 of the de Villiers and Herrnstein article, and the percentage of the data variance accommodated by our analysis. Averaging the data in Column 2 yields a mean percent-variance-accommodated statistic of 78.1% — a percentage substantially lower than the 92.7% found in the section of Table 7 that related to positive reinforcement from de Villiers and Herrnstein. This difference is due to three differences in data-handling methods in the two studies. First, de Villiers and Herrnstein averaged

Table 1
Comparison of Variance Accommodation

	% variance accommodated from:		
Data Set	*de Villiers & Herrnstein*	*Present Report*	*Difference*
Crespi	99.5	99.5	0
Zeaman	82.1	82.0	0.1
Keesey (1964)			
low shock	96.6	98.4	−1.8
high shock	98.6	95.1	3.5
Guttman			
sucrose	93.7	93.6	0.1
glucose	98.7	98.6	0.1
Schrier (1963)	95.4	95.4	0
Logan			
high drive	99.1	97.0	2.1
low drive	92.8	92.3	0.5
Pierce, Hanford, & Zimmerman			
Rat 1	80.8	73.3	6.7
Rat 2	98.3	98.9	−0.6
Rat 3	78.9	79.7	0.2
Rat 4	97.4	97.1	0.3
Schrier (1965)			
.33 cc			
Monkey Ruth	94.3	93.4	0.9
Monkey Karen	95.2	97.7	−2.5
Monkey Joan	92.6	91.7	0.9
Monkey Ken	98.6	98.7	−0.1
Monkey Leo	98.6	97.5	1.1
Monkey John	85.7	80.1	5.6
Monkey Allen	70.1	9.0	61.1
Monkey Mae	85.4	80.9	4.5
.83 cc			
Monkey Ruth	84.9	85.3	−0.4
Monkey Karen	93.3	86.1	7.2
Monkey Joan	98.3	90.7	7.6
Monkey Ken	97.7	97.5	0.2
Monkey John	97.0	96.6	0.4
Monkey Allen	90.8	86.5	4.3

redetermined data points from the Catania and Reynolds (1968) study and fit Equation 3 through these data. We did not average redetermined points. Second, for several studies (Conrad & Sidman, 1956; Keesey, 1962; Silver & Pierce, 1969) they used group-mean data throughout their report despite the availability of individual-subject data. Whenever possible, we fitted Equation 3 to performances of the individual subjects, not to the group means. Third, even when they had individual-subject data in prior tables, de Villiers and Herrnstein calculated a group-mean rate for each data point and fitted Equation 3 to this set of averaged data for inclusion in Table 7. In all cases, the percent-variance-accommodated scores were higher when based on averages of response rates rather than on averages of individual subject's percent-variance scores. Obviously, by presenting individual-subject variances, we did not do this.

It is apparent that one's assessment of the predictive adequacy of Equation 3 depends on how response-rate data are handled. Analyses that emphasize averaging response rates will account for more of the data variance than those that do not. We believe this procedural dependency diminishes the credibility of de Villiers and Herrnstein's demonstration that Equation 3 accounts for behavioral output in single-schedule procedures.

REVISING EQUATION 3

What is needed are methods of improving the statistical fit of Equation 3. One useful technique is to compare the fit of any equation with a family of alternative functional forms for which the originating equation is a special case. This approach has been used successfully by Baum (1979) in generalizing the matching law to many data sets, and holds the promise of doing so with Equation 3.

Toward this goal we first note the two parts of Equation 3: the proportionality hypothesis of the matching law $[r/(r + r_e)]$ and its scaling constant k. In principle, we could add parameters to permit variation in either or both of these elements. For example, the proportionality hypothesis could be altered to make it equivalent to Baum's broadened matching law by adding two parameters. However, our attention focuses on k, largely because we question the central assumption that total behavioral output is fixed, which is necessary to justify treating k as a constant (see Herrnstein, 1974).

We question the assumption of constant behavioral output, for there are good reasons for supposing that the relation between aggregate behavioral output and aggregate reinforcement rate should appear graphically as an inverted U (see Hanson & Timberlake, 1983; Rachlin & Burkhard, 1978; Staddon, 1979). To illustrate this expectation, imagine

Table 2
% Data Variance

(Col. 1)	*(Col. 2)*	*(Col. 3)*	*(Col. 4)*	*(Col. 5)*	*(Col. 6)*
	Equation 3 — Nonlinear Regression (Unadjusted $R^2 \times 100$)	*Equation 5 — Linear Regression on Reciprocals (Unadjusted $R^2 \times 100$)*	*Linear Regression on Reciprocals (Adjusted $R^2 \times 100$)*		
Data Set			*Equation 5*	*Equation 8*	*Equation 11*
Catania & Reynolds[a]					
Pigeon 118	72.8	80.7	75.8	84.2	95.0
Pigeon 121	75.1	57.1	42.8	77.0	54.2
Pigeon 129	62.3	74.4	70.1	78.9	80.9
Pigeon 278	78.5	86.4	84.1	88.3	85.6
Pigeon 279	96.4	98.6	98.4	98.3	98.6
Pigeon 281	56.5	76.5	70.6	60.9	42.0
Crespi					
rats (group mean)	99.5	98.3	97.8	100.0	100.0
Zeaman					
rats (group mean)	82.0	69.1	61.4	93.7	96.9
Keesey (1962)[b]					
— pulse frequency of brain stimulation					
Rat 26	87.3	94.7	93.4	91.3	95.1
Rat 27	82.7	96.7	95.9	99.6	95.2
Rat 28	96.8	98.7	98.4	98.7	98.4
Rat 29	94.8	98.7	98.4	98.0	98.5
Rat 33	87.4	71.3	64.1	90.0	98.6
Rat 34	84.2	71.2	64.0	74.3	96.9
Rat 36	89.7	86.6	83.2	88.5	99.9
Rat 39	92.4	85.0	81.3	94.3	97.9
Rat 32	44.1	---[c]	---[c]	---[c]	---[c]
Rat 38	89.0	95.9	94.9	93.3	95.4
— pulse duration					
Rat 26	84.4	94.9	93.6	97.6	99.7
Rat 27	81.4	97.1	96.4	97.8	99.3
Rat 28	94.2	49.8	37.2	77.5	99.9
Rat 29	83.9	91.4	88.5	87.3	90.9
Rat 33	87.9	80.0	75.0	84.7	99.5
Rat 34	81.0	76.6	70.7	68.2	89.1
Rat 36	78.5	83.2	79.0	88.7	97.1
Rat 39	91.4	90.6	88.3	92.3	97.2
Rat 32	30.2	---[c]	---[c]	---[c]	---[c]
Rat 38	84.5	64.4	46.6	91.1	---[c]
— pulse intensity					
Rat 26	57.8	90.0	87.5	85.0	96.9
Rat 27	79.4	91.3	89.1	85.7	99.8
Rat 28	44.8	92.4	90.5	87.9	96.4
Rat 29	77.2	93.6	92.0	90.5	100.0
Rat 33	94.1	95.6	94.5	99.5	99.3
Rat 34	86.0	58.1	47.6	84.5	92.2
Rat 36	81.0	89.8	87.3	96.1	94.2
Rat 39	66.0	82.3	71.3	99.3	77.9
Rat 32	78.9	75.0	68.7	89.4	99.3
Rat 38	86.4	75.4	69.2	79.2	98.1
Keesey (1964)					
— low intensity of brain stimulation rats (group mean)	98.4	95.7	94.3	91.4	97.5
— high intensity rats (group mean)	95.1	84.9	79.8	92.2	83.0

Table 2 Continued

(Col. 1)	*(Col. 2)*	*(Col. 3)*	*(Col. 4)*	*(Col. 5)*	*(Col. 6)*
	Equation 3— Nonlinear Regression (Unadjusted $R^2 \times 100$)	*Equation 5— Linear Regression on Reciprocals (Unadjusted $R^2 \times 100$)*	*Linear Regression on Reciprocals (Adjusted $R^2 \times 100$)*		
Data Set			*Equation 5*	*Equation 8*	*Equation 11*
Guttman					
—sucrose rats (group mean)	93.6	89.0	86.8	94.3	97.9
—glucose rats (group mean)	98.6	97.9	97.4	98.4	99.6
Conrad & Sidman[b]					
—48-hr deprivation					
Monkey 8	66.2	85.5	81.9	89.0	95.7
Monkey 7	56.0	49.4	36.8	61.8	60.8
Monkey 6	18.1	8.9	−13.9[d]	− 7.0[d]	11.0
—72-hr deprivation					
Monkey 8	40.1	43.2	29.0	27.4	30.4
Monkey 7	4.5	6.1	−17.4[d]	46.8	35.6
Monkey 6	4.0	4.2	−19.7[d]	25.1	1.8
Schrier (1963)					
monkeys (group mean)	95.4	97.0	96.0	99.4	99.8
Schrier (1965)					
—.33 cc sucrose					
Monkey Ruth	93.4	95.6	94.2	97.2	99.9
Monkey Karen	97.7	95.7	94.3	91.4	97.5
Monkey Joan	91.7	91.6	88.8	96.8	94.0
Monkey Ken	98.7	99.4	99.3	99.0	99.7
Monkey Leo	97.5	98.0	97.2	94.3	99.5
Monkey John	80.1	82.0	76.0	81.7	88.3
Monkey Allan	9.0	9.0	−21.3[d]	−70.6[d]	73.4
Monkey Mae	80.9	92.0	89.4	87.5	84.8
—.83 cc sucrose					
Monkey Ruth	85.3	91.6	88.9	89.9	98.9
Monkey Karen	86.1	84.9	79.8	92.2	83.0
Monkey Joan	90.7	98.6	98.2	98.1	99.6
Monkey Ken	97.5	98.8	98.4	99.3	99.8
Monkey John	96.6	97.0	96.9	97.9	96.3
Monkey Allan	86.5	90.4	87.2	81.0	98.3
Logan (p. 58)					
—low drive rats (group mean)	97.0	97.5	96.6	99.6	99.9
—high drive rats (group mean)	92.3	97.4	96.6	95.8	94.1
Silver & Pierce[b]					
Rat 1	64.0	90.6	87.5	98.6	98.2
Rat 2	67.5	98.2	97.6	97.2	94.9
Rat 3	88.2	97.7	97.0	96.6	95.9
Rat 4	93.2	91.6	88.8	99.3	99.8
Rat 5	76.5	81.9	75.9	75.2	100.0
Rat 6	50.2	97.5	96.6	95.0	99.5
Pierce, Hanford, & Zimmerman					
—cue light only					
Rat 1	73.3	93.7	91.7	100.0	100.0
Rat 2	98.9	89.0	85.4	98.5	100.0
Rat 3	79.7	93.1	90.8	99.9	99.9
Rat 4	97.1	99.7	99.6	99.8	99.8
MEAN	78.1	82.5	77.7	85.0	89.8

[a] de Villiers & Herrnstein averaged redetermined data points before calculating R^2. We did not.
[b] de Villiers & Herrnstein present group mean data only. We present individual subjects.
[c] Zero rates in their reciprocal cause regression to be unsolvable.
[d] Adjusted R^2 can be negative.

reinforcer levels for all activities as "infinitesimal," "intermediate," and "infinite." At infinitely small or large reinforcer levels, aggregate behavioral output should be very low—in the former case because high response rates are unproductive of reward and in the latter case because only minimal responding is necessary to produce reinforcer levels in excess of the animal's needs. Ony when reinforcement levels are "intermediate" do we have reason to expect consistent reinforcement-related responding from an animal. Thus, we predict a bitonic function: upward sloping in the reinforcer range infinitesimal to intermediate, and downward sloping in the range intermediate to infinite.

The bitonic functional form is inconsistent with the constancy of k. Yet fixed values for this parameter have characterized not only Equation 3, but also many of the other theoretical accounts that model behavioral output (e.g., Killeen, 1981; Rachlin, 1978; Staddon, 1977, 1979). Only McDowell and Kessel's (1979) linear system analysis (see also McDowell, 1980; McDowell & Wood, 1984) posits an account in which k varies; yet, in their account, k is limited to increasing monotonically with reinforcement amplitude.

Our attempt to improve the statistical fit of Equation 3 takes into account our expectation that k must vary. Our approach in this search is both systematic and general. In our first set of evaluations we let k vary monotonically, either increasing or decreasing with reinforcer levels. This will improve the statistical fit to the extent that the experimental data have been generated preponderantly over either the increasing or decreasing range for k. In our second set of evaluations of functional forms we relax the monotonicity constraint, thus allowing an improved fit to data generated over both the increasing and decreasing ranges of k. In both sets of evaluations, the relative predictive adequacy of Equation 3 is assessed because this equation is always a special case of the family of functions being evaluated.

To permit k to vary monotonically with reinforcer levels, we first restate Equation 3 in inverse form, because this makes the equation more mathematically tractable:

$$1/R = 1/k + r_e/kr = 1/k\,(1 + r_e/r). \tag{5}$$

Because k represents total output, it should be expressed most generally as a function of both r_e and r. However, inasmuch as r_e cannot be observed directly, we defer testing for any relationship between k and r_e until a later section of the paper, and assume for the present that r_e is a random variable independent of r. We can then express k as a function of scheduled reinforcement only. Since we have no a priori expectations as to the precise functional form of the (k, r) relationship, we choose, for ease of estimation:

$$1/k = \beta_0 + \beta_1/r. \tag{6}$$

In this equation, k approaches $1/\beta_0$ as a limit as r approaches infinity; k is an increasing function of r when $\beta_1 > 0$, and a decreasing function of r when $\beta_1 < 0$. These two possibilities in $(1/k,\ 1/r)$ space are shown in Figure 1.

Obviously, the constancy of k requires $\beta_1 = 0$. To determine whether this result obtains, we substitute Equation 6 into Equation 5:

$$1/R = (\beta_0 + \beta_1/r)(1 + r_e/r) = \\ \beta_0 + (\beta_1 + \beta_0 r_e)(1/r) + \beta_1 r_e(1/r)^2. \tag{7}$$

The estimating equation is thus

$$1/R = \alpha_0 + \alpha_1(1/r) + \alpha_2(1/r)^2, \tag{8}$$

where $\alpha_0 = \beta_0$, $\alpha_1 = \beta_1 + \beta_0 r_e$ and $\alpha_2 = \beta_1 r_e$.

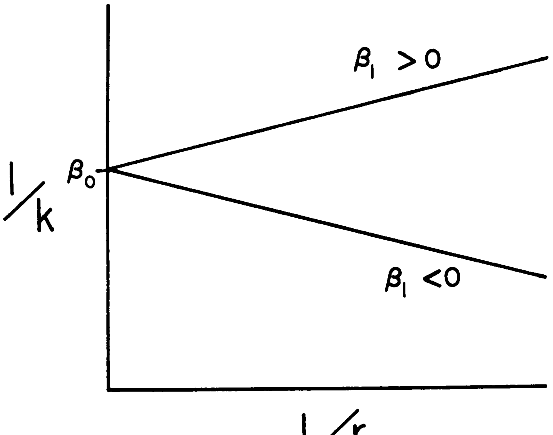

Fig. 1. Portrayal of k as an increasing or decreasing function of reinforcement value (r) when variables are stated in their reciprocals for Equation 6.

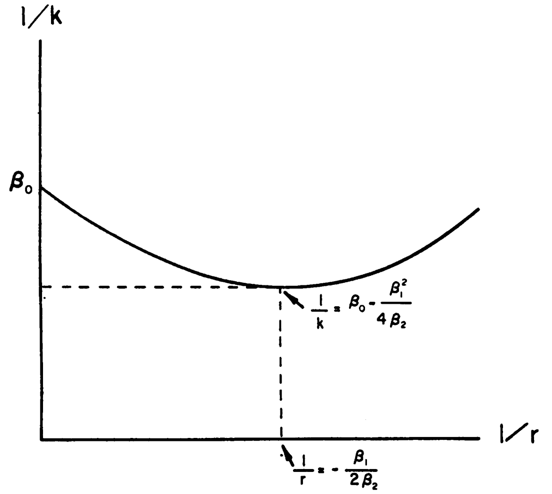

Fig. 2. Portrayal of *k* as an increasing then decreasing function of reinforcement value (*r*) when variables are expressed in reciprocal form for Equation 9.

Equation 8 generalizes Equation 3 by allowing *k* to be either an increasing ($\beta_1 > 0$) or a decreasing ($\beta_1 < 0$) function of *r* over the range of (*k*, *r*) data available, but it does not allow *k* to be an increasing and then a decreasing function of *r* over different ranges of the (*k*, *r*) data. Such flexibility would be an important capability to have if, as suggested earlier, behavioral output is a bitonic function of reinforcement. Therefore, we generalize Equation 8 to allow this, at the cost of an additional degree of freedom, by assuming:

$$1/k = \beta_0 + \beta_1/r + \beta_2/r^2. \qquad (9)$$

For example, if the relationship between $1/k$ and $1/r$ is as illustrated in Figure 2 (i.e., $\beta_0, \beta_2 > 0$, $\beta_1 < 0$), then *k* is an increasing function of *r* up to a maximum at $r = -2\beta_2/\beta_1$ and then falls, approaching $1/\beta_0$ asymptotically as a limit as *r* approaches infinity.

Substituting this function into Equation 5, we get

$$1/R = (\beta_0 + \beta_1/r + \beta_2/r^2)(1 + r_e/r) = \beta_0 + (\beta_1 + \beta_0 r_e)(1/r) + (\beta_2 + \beta_1 r_e)(1/r)^2 + \beta_2 r_e(1/r)^3. \qquad (10)$$

The estimating equation for Equation 10 is, therefore,

$$1/R = \alpha_0 + \alpha_1(1/r) + \alpha_2(1/r)^2 + \alpha_3(1/r)^3, \qquad (11)$$

and the hypothesis that *k* is a constant requires α_2 and α_3 to equal zero.

To recapitulate, we are seeking a replacement for Equation 3 that improves the percentage of variance accommodated in the data from the studies presented in Table 2. Although such a goal could be approached by testing any arbitrary functional form, we have restricted our search to equations for which Equation 3 is a special case. Two versions are offered, presented in order of greater generality. Expressed as estimating equations, they are Equations 8 and 11. Both of these equations permit *k* to be monotone increasing or decreasing with reinforcement rate; Equation 11 also permits bitonicity.

TESTING ALTERNATIVE FUNCTIONAL FORMS

The next question is whether these more general functional forms improve the fit between predicted and observed rates of behavioral output. This requires dealing with two issues.

The first issue is the alternative weighting of error terms inherent in redefining Equation 3 in reciprocal form (Equation 5). To illustrate the problem, assume we have a relationship that can be written in two or more forms—for example, a power function:

$$R = \alpha_0 r^{\alpha_1} \text{ vs } \log R = \log \alpha_0 + \alpha_1 \log r,$$

or Equations 3 and 5:

$$R = kr/(r + r_e) \text{ vs } 1/R = 1/k + r_e/kr.$$

Although the two forms of the power function are mathematically identical, the values of α_0 and α_1 estimated using the nonlinear form may differ systematically from the values arrived at when the equation is estimated in log-linear form. Because the estimating procedure chooses values for α_0 and α_1 that minimize the sum of the squared deviations from the predicted values, estimating in log form will reduce the weight given to errors associated with larger values of *r* and *R*. Similarly, estimating Equation 5 involves minimizing the sum of the squared deviations between actual and predicted $1/R$ rather than actual and predicted *R*, and thus once again reduces the weight given to the errors at high (*r*, *R*) combinations. Which weighting procedure is preferable depends on the assumed nature of the error term: If measurement error is proportional to the size of the observation, a formulation such as a log-linear or reciprocal, which deflates large error terms, is preferable to formulations such as the nonlinear power form or the nonlinear Equation 3 because these latter formulations would place too much emphasis on minimizing errors at the higher range of observations.

Although Wetherington and Lucas argue that this issue has been resolved in favor of the nonlinear method, the proper test—determining the nature of the error term—requires more data points per subject than are available. In any case, we need not be concerned with the answer such a test would provide, because only ordinal accuracy is needed to discriminate among our alternative models. For these purposes, estimation in the reciprocal form of the equation for simple action is sufficient.

A second issue is how to determine whether or not the addition of extra terms to an estimating equation significantly improves its predictive ability. There are several possible criteria, but we present here only two of the most commonly used.

The first approach is to compare the R^2 statistics achieved under the different functional forms. As the reader is no doubt aware, however, the unadjusted R^2 must increase when additional terms are added unless these additional variables are perfectly uncorrelated with the dependent variable. Ultimately, of course, as the number of independent variables equals the number of observations, $R^2 = 1$. Thus the usual procedure is to adjust the R^2 for additional terms in the following way:

$$\bar{R}^2 = 1 - (1 - R^2)\,[(N - 1)/(N - K)],$$

where N = the number of observations and K = the number of estimated coefficients including the intercept (see Pindyck & Rubinfeld, 1976, p. 59).

Columns 3 and 4 of Table 2 present the percentage variance accommodated ($R^2 \times 100$) on an unadjusted and adjusted basis, respectively, for linear regressions based on Equation 5. Columns 5 and 6 present the adjusted $R^2 \times 100$ for Equations 8 and 11, respectively. Comparisons among these data show two things. First, the linear regressions of Equation 5 (Column 3), on average, account for 4.4% more of the data variance than do the nonlinear regressions of Equation 3 (Column 2): 82.5% versus 78.1%, respectively. Second, with few exceptions, Equation 8 does a better job of accounting for the data variance than does Equation 5, and Equation 11 is superior to either of these alternative equations. Averaged over all experiments, the mean adjusted percentage variance explained by Equations 5, 8, and 11 are, respectively, 77.7%, 85.0%, and 89.8%.

The second approach is to compute the F statistic for the null hypothesis that all the coefficients of additional terms are zero (i.e., that $\alpha_2 = 0$ in Equation 8 or that $\alpha_2 = \alpha_3 = 0$ in Equation 11) and then determine the associated significance level (i.e., the probability that we falsely reject the hypothesis that $\alpha_2 = 0$ or $\alpha_2 = \alpha_3 = 0$). If this probability is sufficiently small (less than .05), we customarily reject the hypothesis that k is a constant.

One advantage of this second approach is that it facilitates dealing with a major problem with the data: the low degrees of freedom in the data sets derived from Table 7 of de Villiers and Herrnstein. When the numbers of observations are as few as those in most cases from this table, the data will not allow differentiation between any but the simplest of hypotheses. The traditional solution to this problem is to pool data sets. However, for these data sets pooling cannot be performed directly because each animal is assumed to have its own parameter values. Therefore, we cannot pool to estimate a "population" r_e or k function. We can, however, perform some joint tests. In particular, we can define the probability that each individual animal's k is indeed a constant by performing a joint F test on any set of experiments that are believed to have a good deal in common. Thus we can take the sum, over these experiments, of the difference between the sum of the squared errors with Equation 5 (equivalent to Equation 8 with α_2 restricted to equal zero, or to Equation 11 with α_2 and α_3 restricted to equal zero) and the sum of the squared errors with Equations 8 or 11, and compute the F statistic:

$$F_j = \frac{\sum_{i=1}^{n} \mathrm{SSE}_{i\,(\text{restricted})} - \sum_{i=1}^{n} \mathrm{SSE}_{i\,(\text{unrestricted})}}{\sum_{i=1}^{n} \mathrm{SSE}_{i\,(\text{unrestricted})}} \cdot \frac{\sum_{i=1}^{n} df_i}{nj},$$

where $\mathrm{SSE}_{i(\text{restricted})}$ = sum of the squared errors (i.e., deviations of actual from predicted values) in Experiment i when the specification is restricted so that $\alpha_2 = \alpha_3 = 0$ (i.e., Equation 5); $\mathrm{SSE}_{i(\text{unrestricted})}$ = sum of the squared errors when the specification allows $\alpha_2 \neq 0$, or α_2 and $\alpha_3 \neq 0$; n = number of experiments in the group; j = number of restrictions: $j = 1$ when Equation 8 is restricted to become Equation 5 ($\alpha_2 = 0$); $j = 2$ when Equation 11 is restricted to become Equation 5 (α_2 and $\alpha_3 = 0$); and df_i = degrees of freedom for the regression equation in the ith experiment.

Table 3 presents the results of the F tests for the hypothesis that $\alpha_2 = 0$ (comparing Equation 8 with Equation 5), and α_2 and $\alpha_3 = 0$ (Equation 11 vs. Equation 5). For 7 of 19 comparisons, the hypothesis that $\alpha_2 = 0$ could be rejected at the .05 confidence level; and for 11 of 19 comparisons of Equation 11 with Equation 5, we can reject the hypothesis that α_2 and $\alpha_3 = 0$.

The results from Tables 2 and 3 make clear that permitting monotonic and bitonic change in k improves the capacity of Equation 5 to accommodate data variance across the many studies catalogued by de Villiers and Herrnstein. This conclusion is not only incompatible with their equation for simple action, it also contradicts data they present in support of the constancy of k (see p. 1151 from de Villiers & Herrnstein).

Table 3

Significance Tests for Equations 8 and 11 versus 5

Data Set	*# of Observations*	*Significance for Test of:* $\alpha_2 = 0$	α_2 *and* $\alpha_3 = 0$
Catania & Reynolds	40	< .001 (F = 5.90)	< .001 (F = 7.75)
Crespi	5	.002 (F = 422.14)	.015 (F = 2148.67)
Zeaman	6	.019 (F = 33.52)	.040 (F = 23.76)
Keesey (1962)			
pulse frequency	54	.311 (F = 1.24)	.028 (F = 2.53)
pulse duration	47	.001 (F = 5.06)	< .001 (F = 19.88)
pulse intensity	60	< .001 (F = 7.57)	< .001 (F = 42.60)
Keesey (1964)			
low intensity	5	.643 (F = .29)	.351 (F = 3.56)
high intensity	5	.162 (F = 4.73)	.190 (F = 13.31)
Guttman			
sucrose	7	.051 (F = 7.56)	.031 (F = 13.85)
glucrose	7	.122 (F = 3.82)	.031 (F = 13.54)
Conrad & Sidman			
48-hr deprivation	18	.318 (F = 1.35)	.287 (F = 1.62)
72-hr deprivation	18	.104 (F = 2.75)	.349 (F = 1.39)
Schrier (1963)	5	.056 (F = 16.45)	.145 (F = 23.22)
Schrier (1965)			
.33 sucrose	40	.775 (F = .59)	.122 (F = 2.26)
.83 sucrose	30	.564 (F = .84)	.015 (F = 6.60)
Logan			
low drive	5	.044 (F = 21.11)	.110 (F = 41.13)
high drive	5	.567 (F = .45)	.758 (F = .37)
Silver & Pierce	30	.065 (F = 2.74)	.030 (F = 4.95)
Pierce, Hanford, & Zimmerman			
cue light only	20	< .001 (F = 41)	< .001 (F = 132.56)

Table 4

Correlation Between k and r_e

Data Set	*log* k	*log* r_e	*Correlation Coefficient*	*Significance Level*
Catania & Reynolds				
Pigeon 118	2.02	0.79		
Pigeon 121	1.91	2.34		
Pigeon 129	1.86	0.99		
Pigeon 278	1.89	0.74		
Pigeon 279	1.84	1.08		
Pigeon 281	1.82	0.71		
			.07	.89
Keesey (1962)				
–pulse frequency				
Rat 26	1.31	1.52		
Rat 27	1.48	1.92		
Rat 28	2.40	3.37		
Rat 29	2.50	3.50		
Rat 33	1.53	2.29		
Rat 34	1.37	2.00		
Rat 36	1.46	2.09		
Rat 39	1.51	2.14		
Rat 32	1.61	3.55		
Rat 38	1.84	3.29		
			.79	$< .01$
–pulse duration				
Rat 26	1.37	0.22		
Rat 27	1.35	0.18		
Rat 28	1.79	0.94		
Rat 29	1.43	0.60		
Rat 33	1.24	−0.10		
Rat 34	1.17	−0.22		
Rat 36	1.26	−1.00		
Rat 39	1.37	−0.10		
Rat 32	0.76	1.09		
Rat 38	2.24	2.11		
			.54	.11
–pulse intensity				
Rat 26	1.39	0.04		
Rat 27	1.34	0.04		
Rat 28	1.26	0.15		
Rat 29	1.41	0.57		
Rat 33	1.13	0.26		
Rat 34	1.81	0.18		
Rat 36	1.12	−0.40		
Rat 39	1.18	−0.05		
Rat 32	2.41	2.08		
Rat 38	1.44	1.09		
			.90	$< .01$
Schrier (1965)				
–.33 cc sucrose				
Monkey Ruth	2.03	1.20		
Monkey Karen	1.92	0.92		
Monkey Joan	2.18	1.84		
Monkey Ken	1.97	1.36		
Monkey Leo	2.05	1.66		
Monkey John	1.79	0.13		
Monkey Allan	1.71	−1.00		
Monkey Mae	0.84	1.48		
			.11	.80
–.83 cc sucrose				
Monkey Ruth	1.91	0.72		
Monkey Karen	1.84	1.13		
Monkey Joan	2.78	3.15		
Monkey Ken	1.93	1.28		
Monkey John	1.88	0.78		
Monkey Allan	1.78	0.85		
			.97	$< .01$
Silver & Pierce				
Rat 1	0.99	1.15		
Rat 2	0.93	1.46		
Rat 3	1.21	1.62		
Rat 4	0.80	1.24		
Rat 5	0.90	1.20		
Rat 6	0.96	1.56		
			.62	.19

k AS A FUNCTION OF r_e

As noted above, we expect k to be a function of total reinforcement—that is, of both r and r_e. Unfortunately, since we cannot observe r_e directly, we cannot test for any relationship between k and r_e using data from individual experiments. Suppose, however, that we have a sample of animals that, for whatever reasons, have different rates of r_e. For any subset of experiments that have a great deal in common, it is not unreasonable to assume that this r_e distribution is uncorrelated with other determinants of k. A cross-sectional regression between r_e and k will then provide an unbiased estimate of the relationship between r_e and k at the mean values of the other determinants of k.

We regressed log k against log r_e for all studies in Table 2 where k and r_e were defined for five or more subjects. A log-log regression was chosen because we would expect the relationship between k and r_e to be proportional rather than linear. Table 4 presents the studies and subject names from which the data are derived (Column 1), the logs of k and r_e defined by Wetherington and Lucas' nonlinear least-squares method applied to Equation 3 (Columns 2 and 3), and the correlation coefficient between log k and log r_e and its associated significance level for the pooled data within each study (Columns 4 and 5). As is apparent, k often does not show the constancy predicted by the equation for simple action. Instead, it tends to increase with increases in r_e, and this relationship is highly significant in three of seven cases (see also McSweeney, Melville, & Whipple, 1983).

GENERAL DISCUSSION

This report reevaluates de Villiers and Herrnstein's demonstration that the equation for simple action accounts for virtually all variation in behavioral output in studies in which reinforcer value has been parametrically manipulated. To summarize our major findings:

1. The high proportion of the data variance de Villiers and Herrnstein found accommodated by Equation 3 (94%) is predicated on particular ways of averaging rate data. When the same data sets are not averaged prior to calculating the R^2 statistic, the percent variance accommodated is much lower (78%).

2. To improve the predictive adequacy of Equation 3, we generalized it by permitting its scaling constant, k, to vary. In one form (Equation 8), k was permitted either to increase or decrease with scheduled reinforcement in monotone fashion. In a still more flexible form (Equation 11), k could not only increase, but could subsequently decrease as reinforcement was increased. For both of these latter equations, Equation 3 could be derived as a special case. When Equation 5 (which is Equation 3 in reciprocal form) was compared with these more general functional forms, it was inferior in accommodating data variance. The best job was done by the equation allowing bitonic change (Equation 11), followed by the equation permitting monotonic change (Equation 8). If the matching law is assumed to be correct, these results are incompatible with the definition of k being a constant as stated in Equation 3.

3. Because total output can be expected to be a function of endogenous as well as scheduled reinforcement levels, we tested for a rela¼tionship between k and rv_e using cross-sectional data from homogeneous groups of experiments. As anticipated, the results showed a consistently positive, and in several cases highly significant, relationship between k and r_e.

The analysis we offer here is reminiscent of one offered by Baum (1979). In his report he showed that the matching law (Equation 1) could effectively accommodate variation in choice data only if it were generalized by the addition of two parameters—one to permit variation in slope and the other to permit variation in intercept (Equation 4). Like Baum, we have added parameters to a matching-derived function to improve its fit to existing data. However, we cannot offer a rationale for the use of these parameters other than the observation that they maintain the integrity of the equation they generalize. Particularly troublesome is the variation in the value of k. Because k equals the total amount of behavior an animal can produce, scaled in terms of the *scheduled* operant, its variation violates the formal properties of its derivation (see Herrnstein, 1974). With a variable k, Equation 3 becomes an arbitrary and inadequate formulation of rate effects.

Our finding that k varies with both scheduled and endogenous reinforcement rates is a critique not just of one particular form of an equation for simple action (i.e., one where k is assumed constant), but of the rationale for extending the matching law into any equation for simple action. An equation for simple action consists of two parts: an assumption about choice proportions (in this case the matching law) and an assumption about total output. Thus, any tests of this equation are really joint tests of the matching law and of a particular functional form for the relationship between k and the levels of r and r_e. But inasmuch as we have no theory that implies a particular functional form for this relationship, the question of which form performs best seems of little interest.

We have shown the empirical successes of the equation for simple action to be limited. Its central assumption of the constancy of k is empirically violated and, in its orginial form, this equation does a less adequate job in accommodating variance for individual subjects' data than was previously believed. Because of these problems, we favor no longer applying this matching-based equation to the prediction of single-schedule rate effects. Such a distinction would acknowledge the growing body of evidence (e.g., Duncan & Silberberg, 1982; Hursh, 1978) that matching principles contribute little to the explanation or prediction of behavioral output.

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