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THE GENERALIZED MATCHING LAW AS A DESCRIPTION OF MULTIPLE-SCHEDULE RESPONDING

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The literature was examined to determine how well the generalized matching law (Baum, 1974) describes multiple-schedule responding. In general, it describes the data well, accounting for a median of 91% of the variance. The median size of the undermatching parameter was 0.46; the median bias parameter was 1.00. The size of the undermatching parameter, and the proportion of the variance accounted for by the equation, varied inversely with the number of schedules conducted, with the number of sessions conducted per schedule, and with the time within a component. The undermatching parameter also varied with the operanda used to produce reinforcers and with the reinforcer used. The undermatching parameter did not vary consistently with component duration or with several other variables. Bias was greater when fewer rather than more schedules were conducted, when two rather than one operanda were used, and when White Carneaux rather than homing pigeons served as subjects. These results imply that the generalized matching law may describe both concurrent and multiple-schedule responding, but that the same variables do not always influence the bias and undermatching parameters in the same way for the two types of schedules.

Key words: generalized matching law, multiple schedules of reinforcement, local rates, overall rates, undermatching, bias

Baum (1974) proposed that Equation 1 describes concurrent-schedule responding:

$$\left(\frac{P_1}{P_2}\right) = a\left(\frac{R_1}{R_2}\right)^b. \quad (1)$$

P_1 and P_2 are the rates of responding emitted during the two components of the concurrent schedules with rates of reinforcement equal to R_1 and R_2, respectively. a and b are parameters estimated from the data; the former is labeled "bias" and the latter is labeled "undermatching," when the values are less than 1.0. In recent discussions, some authors have referred to this exponent as "sensitivity to reinforcement" because it indicates the amount of change in behavior that will be produced by a given change in reinforcement (e.g., Davison & Ferguson, 1978). Although this term is acceptable, we shall use the original "undermatching" term because it may be less susceptible to misinterpretation.

Recently, other authors have argued that Equation 1 also describes multiple-schedule responding (e.g., Charman & Davison, 1982). If true, this would be important for at least two reasons. First, the equation would provide a mathematical description of multiple-schedule responding. Second, the similarity between the mathematical descriptions of concurrent- and multiple-schedule responding might suggest that behavior on the two types of schedules could be described by a single type of formal statement. This would simplify theories of schedule-controlled behavior, and it would support theories that provide the same explanation for concurrent- and multiple-schedule responding (e.g., Herrnstein, 1970; Williams, 1983), at the expense of theories that do not (e.g., Rachlin, 1973).

In order to test the capability of Equation 1 to describe multiple-schedule responding, we searched the literature for studies that used standard multiple-schedule procedures and that provided the data necessary to test Equation 1. Studies were not included if they provided more than two components within a single multiple schedule, if they did not vary the rate of reinforcement, or if they altered response rates through reinforcement of specified interresponse times, through signaling the availability of reinforcers, or through differential reinforcement of high or low rates of responding.

Studies that provided ratio schedules in both components also were ignored, for two reasons: First, most of our information about both concurrent and multiple schedules comes from

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the investigation of interval schedules. Therefore, ratio schedules can be ignored with little loss of information. Second, Equation 1 is usually applied to the rates of reinforcement obtained by the subjects rather than to the rates programmed by the schedules. But, ratio schedules provide an uninteresting test of Equation 1 when the obtained rates of reinforcement are used. Ratio schedules require that the obtained rate of reinforcement be proportional to the rate of responding. Therefore, Equation 1 must provide a good description of the results of these studies when obtained rates are used.

METHOD AND RESULTS

Equation 1 was fitted to the data by taking the log (base 10) of both sides of the equation and using a linear curve-fitting technique to determine the a and b parameters and the goodness of fit. Table 1 contains this information. It provides estimates of the bias and undermatching parameters, their standard deviations, and the proportions of variance accounted for by Equation 1, for all subjects and for the mean of all subjects included in each study.[1]

Several things should be noted about Table 1. First, some studies failed to report the data necessary to calculate all of the statistics; blank spaces appear in the table when the necessary data were not reported. Second, some studies did not report the obtained rates of reinforcement. The nominal rates of reinforcement specified by the schedule were used for those studies. Third, Table 1 contains some studies that presented the data in figures rather than in tables. Some data for those studies may be slightly inaccurate because of difficulty reading the figures. Finally, Table 1 contains the results of Shimp and Wheatley (1971) even though the data from that study do not provide a good test of Equation 1. Shimp and Wheatley provided data for only 2 days for each of two schedules for most component durations. These data have been included, however, because they provide within-study data that test the theoretically important idea that the undermatching parameter approaches 1.0 as components become shorter (e.g., Williams, 1983). Footnotes in Table 1 indicate where each of these problems occurs.

Figure 1 contains the relative frequency distributions of the sizes of the bias and undermatching parameters, and of the proportion of the variance accounted for by Equation 1, for all studies reported in Table 1. Although Table 1 reports results for individual component durations and for the mean of all component durations for Charman and Davison (1982), only the results for individual component durations have been included in Figure 1. Figure 1, and all of the statistics to follow, also include only data for individual subjects. The results for the mean of all subjects have been included only for studies that did not report data for individual subjects.

Table 1 and Figure 1 show that Equation 1 provided a good description of the data. The median proportion of the variance accounted for by Equation 1 was .91, and the equation accounted for 80% or more of the variance in 72% of the cases. Figure 1 also shows that the median size of the undermatching parameter was 0.46. The median size of the bias parameter was 1.00.

Table 2 presents some of the details of the procedures used in the studies presented in Table 1. Most of the headings of this table are self-explanatory. The number of operanda listed under the heading of "response operandum" includes only the operanda that were instrumental in producing reinforcers. Therefore, only one operandum would be listed for a signal-key multiple schedule (e.g., Schwartz, 1978) even though a second response key might have been used to display the discriminative stimuli. If more than one experiment was conducted, or if there was

[1] Davison and McCarthy (1981) argued that linear regression techniques may underestimate the size of the undermatching parameter because these techniques incorrectly assume that there is no error variance in the independent variable. Linear regression techniques have been used here in spite of this problem because this paper will compare the results obtained with multiple schedules with those obtained with concurrent schedules, and most of the concurrent-schedule literature has used linear regressions to estimate the size of the undermatching parameter. The present conclusions will also be based on comparisons of the relative sizes of the undermatching parameters rather than on their absolute sizes, reducing the severity of the problem. However, it should be noted that the sizes of the undermatching parameters reported here may be systematic underestimates of the true sizes of these parameters. Therefore, no conclusions should be drawn from the absolute sizes of these parameters.

Table 1

Undermatching and bias parameters, standard deviations of these parameters, and the proportion of the variance accounted for by Equation 1.

Subject	Undermatching Parameter	Undermatching *SD*	Bias Parameter	Bias *SD*	r^2
Charman & Davison (1982)					
Experiment 1[a]					
5- and 25-s components					
151	0.75	0.09	1.35	0.04	0.95
152	0.45	0.05	1.02	0.02	0.95
153	0.42	0.09	0.89	0.05	0.86
154	0.72	0.24	1.00	0.10	0.74
155	0.43	0.06	1.17	0.02	0.94
156	0.36	0.15	0.83	0.07	0.66
Mean	0.52	0.16	1.02	0.07	0.85
10- and 50-s components					
151	0.47	0.08	0.83	0.03	0.91
152	0.41	0.09	0.79	0.04	0.87
153	0.31	0.09	0.55	0.04	0.78
154	0.36	0.05	0.71	0.02	0.95
155	0.33	0.05	0.83	0.02	0.92
156	0.35	0.05	0.85	0.02	0.95
Mean	0.37	0.06	0.76	0.07	0.89
30- and 150-s components					
151	0.47	0.01	1.02	0.00	0.99
152	0.41	0.06	0.85	0.04	0.92
153	0.74	0.20	0.93	0.08	0.81
154	0.36	0.03	0.74	0.02	0.97
155	0.32	0.07	0.91	0.03	0.87
156	0.61	0.18	0.81	0.08	0.79
Mean	0.49	0.13	0.87	0.04	0.89
120- and 600-s components					
151	0.45	0.03	1.00	0.01	0.99
152	0.42	0.06	0.72	0.03	0.94
153	0.60	0.15	0.52	0.06	0.83
154	0.33	0.05	0.95	0.02	0.94
155	0.31	0.04	0.95	0.02	0.95
156	0.29	0.04	0.83	0.01	0.95
Mean	0.40	0.10	0.81	0.10	0.93
Mean of all component durations					
151	0.54	0.10	1.05	0.07	0.96
152	0.42	0.03	0.83	0.06	0.92
153	0.52	0.16	0.69	0.11	0.82
154	0.44	0.16	0.83	0.06	0.90
155	0.35	0.05	0.95	0.05	0.92
156	0.40	0.12	0.83	0.01	0.83
Mean	0.45	0.14	0.85	0.09	0.89
Experiment 2					
One-key schedules					
151	0.61	0.15	1.07	0.07	0.82
152	0.48	0.07	0.95	0.04	0.92
153	0.49	0.07	0.95	0.03	0.93
154	0.44	0.15	1.07	0.06	0.69
155	0.29	0.07	1.00	0.03	0.81
156	0.27	0.07	0.91	0.03	0.81
Two-key schedules					
151	0.90	0.20	1.35	0.09	0.87
152	0.59	0.09	0.43	0.04	0.93
153	0.48	0.16	1.17	0.06	0.74
154	0.36	0.08	0.87	0.04	0.82
155	0.37	0.07	1.55	0.03	0.89
156	0.39	0.07	1.78	0.02	0.92
Davison & Ferguson (1978)					
Part 1: Key vs. lever[b]					
161	0.64		3.47		0.94
162	0.41		3.72		0.75
163	0.50		2.82		0.73
164	0.41		8.32		0.90
165	0.04		5.25		0.23
166	0.48		12.59		0.71
Part 2: Lever vs. lever					
161	0.57		1.07		0.96
162	0.46		1.07		0.96
163[c]	0.30		0.95		0.94
164	0.23		0.89		0.72
165	0.09		1.12		0.17
166	0.42		1.05		0.85
Part 3: Key vs. key					
161	0.78		0.93		0.92
162	0.51		1.26		0.88
163	0.42		0.98		0.89
164	0.62		0.93		0.94
165	0.30		0.89		0.81
166	0.53		1.05		0.71
Dysart, Marx, McLean, & Nelson (1974)[d,e]					
Mean	0.62	0.01	1.10	0.01	1.00
Hamilton & Silberberg (1978)—Experiment 1, master subjects[e]					
M0	−0.32	0.38	1.30	0.31	0.42
M1	0.76	0.24	1.08	0.17	0.91
M2	0.60	0.19	1.39	0.17	0.91
M3	0.07	0.25	1.23	0.20	0.07
M4	0.30	0.02	1.32	0.02	1.00
M5	0.43	0.06	1.26	0.05	0.98
M6	−0.10	0.01	1.33	0.00	0.99
M7	0.32	0.62	1.77	0.72	0.21
Mean	0.30	0.05	1.31	0.04	0.97
Lander & Irwin (1968)[d]					
P12	0.26	0.01	1.02	0.01	0.98
P14	0.41	0.07	1.02	0.10	0.78
Lobb & Davison (1977)—Figure 7[d]					
21	0.55	0.03	1.09	0.03	0.99
22	0.37	0.07	1.11	0.07	0.91
23	0.47	0.08	0.90	0.07	0.92
24	0.48	0.14	1.21	0.15	0.80
25	0.70	0.11	1.15	0.10	0.93

Table 1 (*Continued*)

Subject	Undermatching Parameter	Undermatching *SD*	Bias Parameter	Bias *SD*	r^2
26	0.12	0.08	0.94	0.06	0.46
Mean	0.41	0.04	1.06	0.03	0.98
McLean & White (1981)					
P12	0.44		1.00		0.98
P17	0.37		0.89		0.95
P24	0.34		0.89		0.94
P26	0.53		0.89		0.98
McSweeney (1978)					
Experiment 1					
99	0.50	0.05	1.15	0.05	0.92
61	0.40	0.16	0.95	0.13	0.48
1442	0.47	0.11	1.01	0.11	0.71
1473	0.54	0.15	0.90	0.13	0.65
Mean	0.50	0.07	1.03	0.06	0.88
Experiment 2					
6443	0.75	0.07	1.03	0.06	0.99
3174	0.85	0.81	0.54	0.46	0.52
1530	0.24	0.45	1.22	0.61	0.23
60	0.85	0.84	0.46	0.43	0.51
Mean	0.60	0.52	0.77	0.41	0.56
McSweeney (1980)					
Experiment 1					
2455	1.05	0.68	1.09	0.06	0.44
2560	0.39	0.24	0.95	0.06	0.48
2457	0.16	0.51	1.35	0.14	0.03
2454	0.35	0.41	1.05	0.03	0.20
Mean	0.41	0.99	1.06	0.02	0.05
Experiment 2					
2455	0.61	0.11	0.74	0.14	0.94
2560	0.21	0.02	0.94	0.03	0.98
2457	0.48	0.22	1.04	0.53	0.70
2454	0.46	0.10	0.75	0.12	0.91
Mean	0.40	0.08	0.85	0.11	0.93
Experiment 3					
2455	0.49	0.00	0.89	0.00	1.00
2560	0.36	0.05	0.84	0.07	0.96
2457	0.35	0.13	1.12	0.25	0.80
2454	0.20	0.05	0.94	0.08	0.87
Mean	0.34	0.04	0.90	0.06	0.98
McSweeney (1982)					
6443	0.76	0.06	1.22	0.06	0.99
3174	0.12	0.02	0.59	0.00	0.97
1530	−0.28	0.07	0.65	0.02	0.94
60	0.68	0.04	1.13	0.03	1.00
Mean	0.24	0.02	0.82	0.00	0.99
McSweeney, Dougan, & Farmer (in press-a)					
6311	0.12	0.71	1.89	1.28	0.01
11131	0.82	0.42	0.46	0.20	0.66
902	0.39	0.29	1.24	0.33	0.48
404	1.79	0.41	0.46	0.20	0.90
Mean	0.74	0.15	0.91	0.12	0.92
McSweeney, Dougan, & Farmer (in press-b)					
15	0.42	0.07	0.69	0.09	0.95
17	0.14	0.04	0.86	0.07	0.83
18	0.07	0.02	0.93	0.04	0.79
Mean	0.18	0.05	0.83	0.07	0.88
Reynolds (1963)					
Experiment 1[d]					
37	0.51	0.03	1.05	0.05	0.99
52	0.45	0.03	1.01	0.04	0.99
53	0.30	0.07	1.05	0.10	0.84
Mean	0.39	0.02	1.04	0.03	0.98
Experiment 2					
37	0.60	0.08	0.86	0.11	0.93
52	0.46	0.10	0.94	0.16	0.85
53	0.38	0.05	0.91	0.08	0.93
Mean	0.45	0.04	0.90	0.05	0.97
Experiment 3					
37	0.38	0.07	1.12	0.06	0.88
52	0.61	0.10	1.13	0.09	0.90
53	0.10	0.03	1.02	0.03	0.67
Mean	0.26	0.03	1.07	0.02	0.95
Schwartz (1975)					
Stimuli on instrumental key[e]					
P1	1.06	0.06	1.07	0.03	1.00
P2	0.90	0.09	0.95	0.04	0.99
P3	0.67	0.18	0.90	0.09	0.93
P4	0.56	0.11	0.94	0.05	0.96
Mean	0.79	0.08	0.96	0.04	0.99
Stimuli on signal key					
P1	0.51	0.00	0.96	0.00	1.00
P2	0.36	0.04	0.95	0.02	0.99
P3	0.45	0.10	1.02	0.06	0.95
P4	0.58	0.05	1.00	0.03	0.99
Mean	0.47	0.01	0.98	0.00	1.00
Schwartz (1978)					
Instrumental pecks[d,e]					
P1	0.24	0.05	1.14	0.03	0.93
P2	0.75	0.04	1.24	0.03	1.00
P3	0.55	0.06	0.96	0.03	0.98
P4	0.51	0.09	1.08	0.05	0.94
Mean	0.47	0.04	1.09	0.02	0.99
Signal-key pecks					
P14	0.27	0.63	0.99	0.44	0.09
P24	0.29	0.44	1.11	0.25	0.18
P34	1.40	0.59	1.51	0.64	0.74
P44	0.78	0.02	1.97	0.03	1.00
Mean	0.68	0.32	1.34	0.27	0.69
Shimp & Wheatley (1971)					
2-s components[f]					
13	0.71	0.14	1.14	0.25	0.93
15	0.88	0.09	1.51	0.25	0.98

Table 1 (*Continued*)

Subject	Undermatching Parameter	Undermatching *SD*	Bias Parameter	Bias *SD*	r^2
20	0.86	0.06	1.16	0.13	0.99
Mean	0.82	0.07	1.23	0.14	0.98
5-s components					
13	0.84	0.10	0.86	0.16	0.89
15	0.82	0.11	0.98	0.16	0.87
20	0.88	0.07	1.27	0.14	0.95
Mean	0.91	0.05	1.06	0.07	0.97
10-s components					
13	0.82	0.19	0.58	0.17	0.90
15	0.54	0.12	1.39	0.25	0.91
20	1.13	0.32	0.75	0.31	0.86
Mean	0.90	0.20	0.84	0.21	0.91
30-s components					
13	0.68	0.09	0.81	0.06	0.96
15	0.32	0.22	1.11	0.33	0.52
20	0.62	0.16	1.13	0.19	0.88
Mean	0.69	0.14	0.93	0.12	0.92
60-s components					
13	0.85	0.22	1.19	0.23	0.88
15	0.26	0.15	0.92	0.13	0.61
20	0.75	0.06	1.06	0.06	0.99
Mean	0.67	0.05	1.02	0.05	0.99
180-s components					
13	0.53	0.04	0.89	0.06	0.99
15	0.16	0.03	1.10	0.05	0.95
20	0.52	0.06	0.95	0.10	0.98
Mean	0.46	0.00	1.01	0.00	1.00
Silberberg & Schrot (1974)					
Experiment 1					
9905	0.73	0.86	0.90	0.22	0.27
11838	0.81	0.03	0.69	0.01	1.00
Experiment 2					
11917	0.75	0.40	1.45	0.13	0.63
10868	−0.03	0.45	0.90	0.18	0.00
White & Redman (1983)					
First subinterval					
C1	0.69		0.91		0.81
C2	0.15		1.15		0.91
C3	0.61		0.83		0.95
C4	0.76		0.72		0.83
Second subinterval					
C1	0.23		0.85		0.82
C2	0.16		1.05		0.83
C3	0.22		1.00		0.87
C4	0.38		0.95		0.83
Fourth subinterval					
C1	0.08		0.98		0.63
C2	0.02		1.00		0.06
C3	−0.03		1.00		0.55
C4	−0.06		0.98		0.03
Zimmerman, Hanford, & Brown (1967)[d]					
77	0.51	0.08	1.84	0.15	0.88
23	0.48	0.09	1.09	0.09	0.81
40	0.34	0.07	1.12	0.08	0.79
76	0.30	0.04	1.12	0.04	0.91
Zuriff (1970)					
Series 1[e,g]					
275	0.52	0.08	0.54	0.04	0.93
258	0.50	0.06	0.51	0.03	0.95
456	0.53	0.02	0.52	0.00	1.00
470	0.55	0.12	0.72	0.08	0.87
Mean	0.51	0.02	0.57	0.01	1.00
Series 2					
275	0.39	0.02	0.61	0.02	1.00
470	0.27	0.06	0.89	0.07	0.92

[a] The standard deviations reported for this study are those of the logarithms of the bias parameters, rather than of the bias parameters themselves.

[b] Bias is measured towards the component with the key-peck response.

[c] This subject is labeled 164 on the figure from which these data were taken.

[d] These data were read from a figure.

[e] Nominal rates of reinforcement were used for this study.

[f] For all component durations except the 5-s duration, the data are from only 2 days and two different schedules. Reinforcement rates may also be too high for this study because magazine time was not subtracted from the time denominator before calculating these rates.

[g] Bias is measured towards the variable-interval component.

more than one part to an experiment, the procedural details for the different parts or experiments have been indicated by roman numerals.

The data reported in Table 1 were analyzed according to procedural detail to determine whether any of those details altered the goodness of fit or the size of the parameters. Results were analyzed when the procedural detail varied within studies, as well as when it varied between studies. Sign tests and Friedman two-way analyses of variance by ranks were used to analyze the related data obtained when the effect of a variable was assessed within a study (Siegel, 1956). Sign tests were used when only two points were compared; Friedman tests were used for more than two points. The median test and the ex-

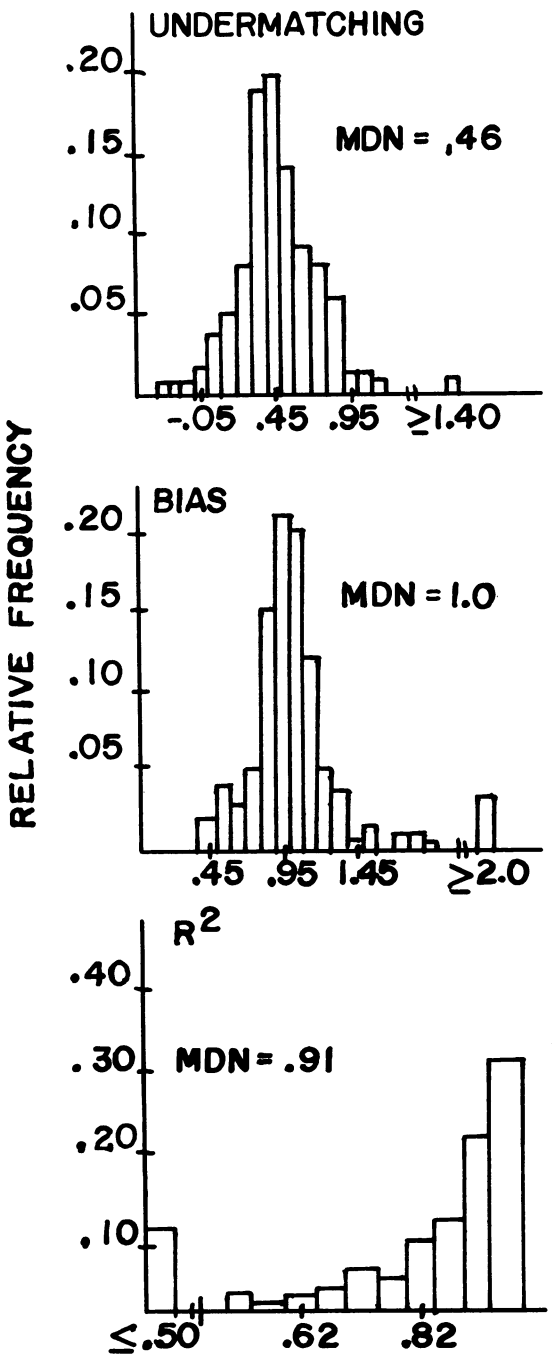

Fig. 1. Relative frequency distributions of the size of the undermatching and bias parameters, and of the proportions of the variance accounted for by Equation 1, for all studies included in Table 1.

tension of the median test were used for the independent data collected across studies (Siegel, 1956). The median test was used when two points were compared; the extension of the median test, when more than two points were compared. Results were considered significant when $p < .05$.

This analysis of the data should be treated with caution. When the procedural detail varied within studies, the conclusions are limited because they are usually based on the results of only one or two experiments. When the detail varied across studies, the conclusions are limited because variables other than the one identified may have actually produced the differences between results. For example, the data were analyzed to determine the effect that the number of schedules conducted had on the bias and undermatching parameters, but conclusions based on this analysis might be incorrect if some other variable, such as the number of sessions conducted per schedule, varied systematically with the number of schedules conducted. In an attempt to minimize this problem, data are analyzed across studies only when at least three studies contributed to the data for each point. However, the problem cannot be entirely ignored, and the relationships described below are subject to these reservations.

Proportion of the Variance

Table 1 indicates that few procedural details altered how well Equation 1 described the data. One variable that did alter the fit was the number of sessions conducted per schedule. The proportion of the variance accounted for by Equation 1 decreased as the number of sessions conducted increased. The median proportion of the variance accounted for was .93, .92, .79, and .83 for studies conducting 15 to 19, 20 to 24, 25 to 29, and more than 30 sessions per schedule on the average, respectively. These medians differ significantly according to the extension of the median test [χ^2 (3) = 12.04].

The proportion of the variance accounted for may have also decreased with time within the component in the one study that reported it (White & Redman, 1983). The median proportions of the variance were .87, .83, and .31 for the first 15 s of a component, the second 15 s, and the fourth 15 s, respectively. These differences were marginally significant by a

Table 2

Procedures used in studies reported in Table 1.

Study	Subjects	Component schedules	Response operandum	Reinforcer
Charman & Davison (1982), Exp. 1	naive homing pigeons, 80%	VI VI, alternate, duration varied	1 key	3-s wheat
Charman & Davison (1982), Exp. 2	experienced homing pigeons, 80%	VI VI, alternate, 5-s duration	1 or 2 keys	3-s wheat
Davison & Ferguson (1978)	naive homing pigeons, 80%	VI VI, alternate, 2-min duration	I:1 key & 1 lever II:1 lever III:1 key	3-s wheat
Dysart et al. (1978)	naive White Carneaux pigeons, 80%	VI VI, mixed order, 3-min duration	1 key	4-s mixed grain
Hamilton & Silberberg (1978), Exp. 1	naive & experienced White Carneaux pigeons, 80%	RI RI, alternate, 27-s duration	1 key	5-s mixed grain
Lander & Irwin (1968)	experienced homing pigeons, 80%	VI VI, alternate, 3-min duration	1 key	2.5-s mixed grain
Lobb & Davison (1977), Fig. 7	experienced homing pigeons, 80–85%	VI VI, alternate after first reinforcer	1 key	3-s wheat
McLean & White (1981)	experienced homing pigeons, 80%	VI VI, alternate, 90-s duration	1 key	3–s wheat
McSweeney (1978)	experienced homing & White Carneaux pigeons, 80–85%	VI VI, alternate, 2-min duration	2 treadles	5-s mixed grain
McSweeney (1980)	experienced homing pigeons, 80–85%	VI VI, alternate, 90-s duration	1 key	5-s mixed grain
McSweeney (1982)	experienced homing & White Carneaux pigeons, 80–85%	VI VI, alternate, 60-s duration	1 key	5-s mixed grain
McSweeney, Dougan, & Farmer (in press-a)	experienced White Carneaux pigeons, 80–85%	VI, VI, alternate, 60-s duration	2 treadles	5-s mixed grain
McSweeney, Dougan, & Farmer (in press-b)	experienced homing pigeons, 80–85%	VI VI, alternate, 90-s duration	1 key	5-s mixed grain
Reynolds (1963)	White Carneaux pigeons, 80%	VI VI, alternate, 3-min duration	1 key	3-s mixed grain
Schwartz (1975)	naive Silver King & White Carneaux pigeons, 80%	VI VI, alternate, 100-s duration	1 key	4-s mixed grain
Schwartz (1978)	experienced White Carneaux pigeons, 80%	VI VI, alternate, 20-s duration	1 key	4-s mixed grain
Shimp & Wheatley (1971)	experienced White Carneaux pigeons, 80%	VI VI, alternate, duration varied	1 key	2-s mixed grain
Silberberg & Schrot (1974)	naive White Carneaux pigeons, 80%	yoked to concurrent VI VI	I:1 key II:2 keys	4-s mixed grain
White & Redman (1983)	experienced homing pigeons, 80%	VI VI, alternate, 60-s duration	1 key	2.5-s wheat
Zimmerman et al. (1967)	experienced White Carneaux pigeons, 80%	VI VI, alternate, 24-min duration	1 key	conditioned reinforcers
Zuriff (1970)	experienced White Carneaux, 80%	VI VR, alternate, 1-min duration	1 key	4-s mixed grain

Friedman analysis of variance by ranks [χ_r^2 (2) = 6.13, $p \approx .05$].

The number of data points considered, which excludes extinction components and occasional components for which inadequate data were provided, also altered the fit of Equation 1 when the extremes were compared. The median proportions of the variance accounted for were .97, .92, .91, and .88 for studies that conducted 3, 4, 5, or 6 schedules, respectively. These statistics were not significantly different [χ^2 (3) = 6.40] by the extension of the median test. But the difference between the largest and smallest numbers of points conducted, 3 and 6, was significant by the median test [χ^2 (1) = 5.12].

The effect of component duration had equivocal effects, at best, on the proportion of variance accounted for by Equation 1. Shimp and Wheatley (1971) varied component duration and found that the proportion of the variance was greater for very short and very long component durations than it was for components of intermediate length. The median proportions of the variance were .98, .89, .90, .88, .88, and .98 for the 2-, 5-, 10-, 30-, 60-, and 180-s component durations, respectively. These differences are significant by a Friedman analysis of variance by ranks [χ_r^2 (5) = 6.48].

However, the effect of component duration on the proportion of the variance accounted for was not significant for Charman and Davison's (1982) study, which used two different durations for the two components of each multiple schedule. The median proportions of the variance accounted for were .90, .92, .90, and .95 for the 5- versus 25-s, 10- versus 50-s, 30- versus 150-s, and 120- versus 600-s component durations, respectively. These results were not significant by a Friedman analysis of variance by ranks [χ_r^2 (3) = 2.85].

Changes in component duration also failed to produce statistically significant changes in the proportion of the variance accounted for when the data were examined across studies. The median proportions of the variance accounted for were .88, .87, .91, .78, and .92 for studies that used 5-, 60-, 90-, 120-, and 180-s component durations, respectively. These results were not significant [χ^2 (4) = 2.36] by the extension of the median test. Neither the differences between the extreme, 5- and 180-s component durations, nor the differences between the 5- and 90-s component durations, reached significance using the median test.

Undermatching

The undermatching parameter decreased as time within the component increased in the one study that examined it (White & Redman, 1983; this finding was subsequently confirmed in a study by White, Pipe, & McLean, 1984, which appeared after completion of the statistical analyses reported here). The median size of the undermatching parameter was 0.65, 0.23, and −0.01 for the first 15 s, the second 15 s, and the fourth 15 s of a component, respectively. The changes in undermatching were significant by a Friedman analysis of variance by ranks [χ_r^2 (2) = 6.50].

The undermatching parameter decreased with increases in the number of sessions conducted per schedule. The median sizes of the undermatching parameters were 0.53, 0.44, 0.44, and 0.36 for the data obtained from studies that conducted 15 to 19, 20 to 24, 25 to 29, and more than 30 sessions, respectively. These differences were significant [χ^2 (3) = 10.85] by the extension of the median test.

The size of the undermatching parameter changed with changes in the response requirement in the one study that examined this variable (Davison & Ferguson, 1978, χ_r^2 (2) = 7.00, Friedman analysis of variance by ranks). The undermatching parameter was largest when key pecks produced reinforcers in both components (median = 0.52) and smallest when lever presses produced reinforcers in both components (median = 0.36). The parameter was of intermediate size when pecks produced reinforcers in one component and presses produced reinforcers in the other (median = 0.45).

The undermatching parameter changed significantly with the number of schedules conducted, but only when the extremes were compared. The median size of the undermatching parameter was 0.51, 0.53, 0.43, and 0.42 for studies conducting 3, 4, 5, or 6 schedules, respectively. These parameters were not significantly different by the extension of the median test [χ^2 (3) = 0.59]. The median test was significant, however, when it was applied to the data from the studies that conducted the fewest (3) and largest (6) number of schedules [χ^2 (1) = 4.20].

The undermatching parameter was also smaller when wheat served as the reinforcer than when mixed grain served [χ^2 (1) = 4.28, median test]. The median undermatching parameter was 0.42 for studies using wheat and 0.50 for studies using grain.

The effect of component duration was equivocal. The undermatching parameter decreased significantly with increases in component duration in the experiment by Shimp and Wheatley (1971). The median sizes of the undermatching parameters were 0.86, 0.84, 0.82, 0.62, 0.75, and 0.52 for the 2-, 5-, 10-, 30-, 60-, and 180-s component durations, respectively. These changes are significant by a Friedman analysis of variance by ranks, χ_r^2 (5) = 10.24. However, undermatching did not change significantly with changes in component duration in the experiment by Charman and Davison (1982) or when it was examined across studies. The median sizes of the undermatching parameters reported by Charman and Davison were 0.44, 0.36, 0.44, and 0.38 for the 25- versus 5-, 50- versus 10-, 150- versus 30-, and 600- versus 120-s component durations, respectively. A Friedman analysis of variance by ranks applied to these parameters was not significant [χ_r^2 (3) = 6.95]. The median sizes of the undermatching parameters found in studies using 5-, 60-, 90-, 120-, and 180-s component durations were 0.46, 0.39, 0.37, 0.48 and 0.43, respectively. These parameters were not significantly different by a Friedman analysis of variance by ranks [χ_r^2 (4) = 0.39]. A median test applied to the extreme 5- and 180-s component durations was also not significant [χ^2 (1) = 0.17].

The following variables may have also influenced the size of the undermatching parameter, but there were too few data to support strong conclusions. The type of component schedule may have influenced undermatching. Most of the studies reported in Table 1 used variable-interval schedules in both components. The two exceptions produced slightly different undermatching parameters. The median size of the undermatching parameter was 0.31 in the experiment by Hamilton and Silberberg (1978), which used random-interval schedules in both components; the median undermatching parameter was 0.51 in the experiment by Zuriff (1970), which used a variable-interval schedule in one component and a variable-ratio schedule in the other component. Both of these undermatching parameters may differ from the median size of the undermatching parameter obtained for all studies (0.46), but there were too few data to assess the statistical significance of the differences.

The undermatching parameter may have been larger when the components were presented randomly than when they alternated. The components alternated in all studies except that by Dysart, Marx, McLean, and Nelson (1974). The median size of the undermatching parameter was 0.46 for studies in which components alternated and was 0.62 in that by Dysart et al.

The undermatching parameter appeared to be smaller for instrumental responses emitted during signal-key multiple schedules than during other schedules. Schwartz (1975) found an undermatching parameter of 0.79 when the discriminative stimuli appeared on the response key, and one of 0.40 when the stimuli appeared on a signal key. The median size of the undermatching parameter was 0.36 for all studies using a signal-key procedure, and 0.46 for all studies, most of which used more standard procedures. Unfortunately, there are not enough data to assess the statistical significance of these differences.

The undermatching parameter did not appear to be different for responses on the signal key (median = 0.54) and responses on the instrumental key (median = 0.53) in the one study that reported these data (Schwartz, 1978). Again, however, there are too few data to assess the significance of this effect.

Bias

It is more difficult to assess the effect of different variables on the size of the bias parameter. The problem arises because most of the studies represented in Table 1 provide no obvious rationale for assigning one component to Component 1 and the other to Component 2 when entering data into Equation 1. The components are usually distinguished only by the discriminative stimuli that signal them. The choice of which data to enter in the numerator and which in the denominator does not affect the undermatching parameter, but it does affect the size of bias. The bias parameter obtained with a component in the numerator will be the inverse of the bias parameter that would have been obtained had that

component been assigned to the denominator. Therefore, it makes little sense to argue that a bias parameter of 2.0 obtained in one study is larger than a bias parameter of 0.5 obtained in another. The two studies would have yielded identical bias parameters had a different choice of numerator and denominator been made.

The problem can be eliminated, however, if the absolute values of the logarithms of the bias parameters are examined instead of the bias parameters themselves. The absolute value of the log of a number and its inverse are identical. Therefore, this statistic, which is insensitive to the choice of numerator and denominator, is used here in examining the factors that influence the amount of bias. All statistical analyses were performed on medians of the absolute values of the logs of the bias parameters (MAVLB).

Amount of bias was affected by the use of different operanda to produce reinforcers in different components. For example, in Davison and Ferguson's (1978) experiment, the MAVLB was 0.032 when key pecking produced reinforcers in both components, it was 0.029 when lever pressing produced reinforcers in both components, and it was 0.645 when a key peck produced reinforcers in one component and a lever press produced them in the other. (The bias was always towards key pecking.) These differences were significant when assessed by a Friedman analysis of variance by ranks [χ_r^2 (2) = 9.25].

All studies reported in Table 1 used pigeons as subjects, so present results cannot assess the effect of many subject characteristics on bias. However, one subject characteristic did alter bias. The MAVLB was 0.067 when White Carneaux pigeons served as subjects and it was 0.051 when homing pigeons served. This difference was statistically significant by the median test [χ^2 (1) = 4.30].

Bias decreased significantly with increases in the number of schedules conducted. The MAVLBs were 0.089, 0.060, 0.064, and 0.041 for studies conducting 3, 4, 5, or 6 schedules, respectively. These differences between medians were statistically different [χ^2 (3) = 9.83], as assessed by the extension of the median test.

Several other variables also may have altered the amount of bias but the statistical significance of their effect is difficult to assess. Presenting the components randomly in time may have produced less bias than that obtained when components alternated. Components alternated in all studies but that of Dysart et al. (1974), who found a MAVLB of 0.041 that may deviate significantly from that of 0.052, found for all studies.

Bias may have been greater for responses on the signal key of signal-key multiple schedules than it was for responses on the instrumental key. Schwartz (1978) found a MAVLB of 0.122 for signal-key responses and of 0.045 for instrumental-key responses, but there are too few data to assess the significance of this difference.

Bias may have been greater for responding during multiple schedules in which the discriminative stimuli appeared on the instrumental key (a normal multiple-schedule procedure) than it was for responding on the instrumental key of a signal-key procedure. Schwartz (1975) found a MAVLB of 0.028 for responses on a normal multiple schedule and of 0.014 for responses on a signal-key procedure. Comparing bias across schedules also confirmed this result. The MAVLB was 0.052 for normal multiple schedules across all studies and 0.034 for signal-key multiple schedules.

The type of simple schedules provided by the components also may have altered bias. Most studies reported in Table 1 used multiple variable-interval schedules and found a MAVLB of 0.052. Hamilton and Silberberg (1978) used multiple random-interval random-interval schedules and obtained a MAVLB of 0.117. Zuriff (1970) used multiple variable-interval variable-ratio schedules and found a MAVLB of 0.237 (with bias always towards the variable-ratio component).

Variables That Did Not Produce Significant Effects

Many procedural variables did not produce a significant change in the proportion of the variance accounted for by Equation 1, or in the undermatching and bias parameters. These variables are summarized in Table 3. When conclusions are based on comparisons within a study, the study that forms the basis for the conclusion is given after the result. When conclusions are based on the results of several studies, only the medians are given. Statistical tests could not be used to assess the significance of the effect of some variables because

Table 3

Procedural variables that did not produce significant effects on the proportion of the variance accounted for by Equation 1, the undermatching or bias parameter.

Variable	Result
Proportion of the variance	
Subjects	naive, median = 0.91; experimentally experienced, median = 0.90; χ^2 (1) = 0.44, median test
Subjects	homing pigeons, median = 0.87; White Carneaux pigeons, median = 0.91; χ^2 (1) = 1.44, median test
Reinforcer duration	3-, 4-, or 5-s access to food, medians = 0.90, 0.94, 0.85, respectively; χ^2 (2) = 4.85, extension of the median test
Method of programming interreinforcer intervals	Catania & Reynolds (1968, Appendix 2), Fleshler & Hoffman (1962), or arithmetic progression, medians = 0.89, 0.95, 0.89, respectively; χ^2 (2) = 0.36, extension of the median test
Holding over or canceling uncollected reinforcers	held over, median = 0.89; canceled, median = 0.91; χ^2 (1) = 0.41, median test
Type of reinforcer	wheat, median = 0.88; grain, median = 0.92; χ^2 (1) = 0.98
Component schedules	random interval, median = 0.91, Hamilton & Silberberg (1978); variable interval and variable ratio, median = 0.94, Zuriff (1970); variable interval, median = 0.91
Random or alternating components	random, median = 1.00, Dysart et al. (1974); alternating, median = 0.91
Operanda	treadles, median = 0.90; keys, median = 0.89; one treadle and one key, median = 0.74; Davison & Ferguson (1978), sign test
Signal-key procedure (instrumental responses)	signal key, median = 0.99; usual procedure, median = 0.98, Schwartz (1975); signal key, median = 0.95; usual procedure, median = 0.91
Signal-key procedure (signal-key responses)	signal-key responses, median = 0.46; instrumental key responses, median = 0.96, Schwartz (1978)
Undermatching parameter	
Subjects	naive, median = 0.47; experimentally experienced, median = 0.48; χ^2 (1) = 0.40, median test
Subjects	homing pigeons, median = 0.42; White Carneaux, median = 0.53; χ^2 (1) = 2.49, median test
Holding over or canceling uncollected reinforcers	held over, median = 0.45; canceled, median = 0.39; χ^2 (1) = 1.18, median test
Reinforcer duration	3-, 4-, or 5-s access to food, medians = 0.44, 0.53, 0.40; χ^2 (2) = 5.32, extension of the median test
Method of programming interreinforcer intervals	Fleshler & Hoffman (1962), Catania & Reynolds (1968, Appendix 2); arithmetic progression, medians = 0.43, 0.52, 0.45; χ^2 (2) = 2.24, extension of the median test
Bias parameter	
Subjects	naive, MAVLB = 0.050; experimentally experienced, MAVLB = 0.052; χ^2 (1) = 0.21, median test
Component duration	2-, 5-, 10-, 30-, 60-, and 180-s components, MAVLB = 0.064, 0.066, 0.143, 0.053, 0.036, 0.041, Shimp & Wheatley (1971); χ_r^2 (5) = 7.97, Friedman analysis of variance by ranks; 25- vs. 5-, 50- vs. 10-, 150- vs. 30-, and 600- vs. 120-s component durations, MAVLB = 0.060, 0.092, 0.056, and 0.052, Charman & Davison (1982); χ_r^2 (3) = 0.68, Friedman analysis of variance by ranks; 5-, 60-, 90-, 120-, and 180-s component durations, MAVLB = 0.051, 0.084, 0.051, 0.050, and 0.025; χ^2 (4) = 7.64, extension of the median test
Time within component	first 15 s, second 15 s, fourth 15 s, MAVLB = 0.071, 0.022, 0.005, White & Redman (1983); χ_r^2 (2) = 5.38, Friedman analysis of variance by ranks
Number of sessions per schedule	15 to 19, 20 to 24, 25 to 29, more than 30, MAVLB = 0.049, 0.051, 0.070, 0.052; χ^2 (3) = 2.08, extension of the median test
Holding over or canceling uncollected reinforcers	held over, MAVLB = 0.065; canceled, MAVLB = 0.049; χ^2 (1) = 2.91, median test
Reinforcer duration	3-, 4-, or 5-s access to food, MAVLB = 0.051, 0.048, 0.087; χ^2 (2) = 5.10, extension of the median test
Method of programming interreinforcer intervals	Fleshler & Hoffman (1962), Catania & Reynolds (1968, Appendix 2), arithmetic progression, MAVLB = 0.051, 0.064, 0.056, respectively; χ^2 (2) = 5.32, extension of the median test
Type of reinforcer	wheat, MAVLB = 0.051; grain, MAVLB = 0.053; χ^2 (1) = 0.54, median test

too few data were provided. When this is true, no statistical test is reported.

Table 3 does not provide a strong basis for assessing effects of either subject type or type of reinforcer. All studies used pigeons as subjects, so only the effects of using White Carneaux or homing pigeons and of using naive or experimentally experienced subjects could be assessed. All studies except that of Zimmerman, Hanford, and Brown (1967) used food as reinforcers, so only the effect of using wheat instead of mixed grain could be assessed.

Other Studies

Baum (1974) argued that Equation 1 could be applied to aspects of reinforcement other than rate, by substituting the ratio of the value of the reinforcers for the ratio of the rates of reinforcement on the right side of Equation 1. Table 4 contains the undermatching and bias parameters, with their standard deviations, and the proportion of the variance accounted for by Equation 1, for studies that varied some aspect of reinforcement other than rate. These data and statistics were calculated as for Table 1. Table 4 shows that Equation 1 described the data well, accounting for a median of 92% of the variance in the data. The equation accounted for slightly more of the variance when reinforcer duration or sucrose concentration was varied (medians = 0.96 and 0.97, respectively) than when reinforcement rate was varied (median = 0.91). The equation accounted for a smaller proportion of the variance in data from studies that added punishment to one component (median = 0.14) or that provided another simple schedule that was concurrently available with the multiple schedule (median = 0.81). The equation accounted for approximately the same proportion of the variance when the components supplied negative reinforcers and the ratios of the rates of responding were plotted as a function of the ratios of shock-frequency reduction (median = 0.91).

The undermatching parameters obtained in the studies that varied reinforcer duration (median = 0.40) appear to be similar to those obtained in studies that varied rate of reinforcement (median = 0.46). All other variables seemed to produce undermatching parameters that differ from those reported for rate of reinforcement. The median undermatching parameter was 1.13 when sucrose concentration varied. It was 0.21 when punishment was added to a component. It was 0.92 when the ratio of the rates of responding were plotted as a function of the ratio of shock-frequency reduction, and it was 0.36 when a simple schedule was concurrently available with the multiple schedule.

One variable did not seem to produce bias differing from that reported in Table 1. The median absolute value of the bias parameter (MAVLB) was 0.056 when the ratios of the rates of responding were plotted as a function of reinforcer duration. It was 0.052 for the studies reported in Table 1.

Four other variables did appear to produce bias parameters that differed from those in Table 1: The MAVLB was 0.036 when the ratios of the rates of responding were plotted as a function of sucrose concentration; it was 0.029 when they were plotted as a function of the ratio of the shock-frequency reduction obtained by responding; it was 0.395 when punishment was added to a component (the bias being away from the punished component); and it was 0.090 when another simple schedule was concurrently available with the multiple schedule.

DISCUSSION

The present paper applied Equation 1 to multiple-schedule responding. Because Equation 1 describes concurrent-schedule responding well (e.g., Baum, 1974, 1979), the results for concurrent and multiple schedules will be compared. Before doing so, however, two cautionary notes should be stated.

Limitations of the Present Comparisons

First, the limits of the present analysis should be recognized. As argued earlier, when a procedural detail varied within a study, the conclusions about its effect are based on the results of only one or two studies. When the detail varied across studies, variables other than the one identified, or even some interaction among variables, may have actually produced the result. For example, studies using wheat as the reinforcer produced significantly smaller undermatching parameters than

Table 4

Undermatching and bias parameters, their standard deviations, and proportions of the variance accounted for by Equation 1 for unusual cases.

Subject	Undermatching Parameter	Undermatching *SD*	Bias Parameter	Bias *SD*	r^2
Studies that varied reinforcer duration					
Hamilton & Silberberg (1978, Experiment 2)[a]					
M8	0.30	0.00	0.69	0.00	1.00
M9	0.49	0.36	1.44	0.33	0.65
M10	0.57	0.09	0.88	0.05	0.98
M11	0.47	0.01	1.04	0.00	1.00
M12	−1.10	2.13	19.85	37.76	0.21
Mean	0.41	0.27	1.15	0.20	0.71
Merigan, Miller, & Gollub (1975)					
One-lever schedules[a]					
P1	0.30	0.04	1.07	0.06	0.94
P2	0.54	0.07	1.15	0.11	0.96
P3	0.40	0.11	0.99	0.15	0.82
P5	0.41	0.05	1.01	0.08	0.96
Mean	0.42	0.04	1.04	0.06	0.97
Two-key schedules—5-s component duration					
P1	0.36	0.04	0.88	0.04	0.95
P2	0.88	0.11	0.95	0.13	0.94
P3	0.81	0.06	1.09	0.08	0.98
P5	0.84	0.08	0.96	0.09	0.96
Mean	0.70	0.05	0.95	0.05	0.98
Two-key schedules—2-min component duration					
P1	0.35	0.00	0.84	0.00	1.00
P2	0.31	0.04	0.88	0.06	0.98
P3	0.52	0.02	0.63	0.02	1.00
P5	0.41	0.01	0.88	0.02	1.00
Mean	0.38	0.02	0.81	0.02	1.00
Shettleworth & Nevin (1965)[a]					
4	0.30	0.03	1.03	0.03	0.96
7	0.34	0.05	0.94	0.05	0.91
Mean	0.32	0.04	0.98	0.04	0.94
Studies that varied sucrose concentration					
Flaherty & Sepanak (1978)					
Mean	1.13		0.92		0.97
Studies that added punishment					
Lattal (1970)[b]					
P12	0.16	0.25	1.84	0.34	0.16
P14	0.28	0.57	3.34	1.74	0.11
Mean	0.21	0.36	2.39	0.69	0.15
Studies that used negative reinforcement					
de Villiers (1974), Experiment 2					
Response ratios as a function of shock-frequency reduction					
R7	0.81	0.10	1.07	0.08	0.89
R9	0.92	0.06	0.95	0.04	0.97
R11	1.00	0.15	1.13	0.07	0.91
Studies in which a simple schedule was concurrently available					
Lobb & Davison (1977)					
Figure 4[b]					
21	0.34	0.05	0.99	0.10	0.90
22	0.37	0.06	1.08	0.11	0.89
23	0.23	0.06	1.13	0.11	0.73
24	0.16	0.08	1.36	0.18	0.43
25	0.58	0.04	1.23	0.08	0.98
26	0.34	0.04	0.71	0.05	0.92
Mean	0.31	0.04	1.04	0.06	0.94
Figure 9[b,c]					
21	0.40	0.11	1.04	0.08	0.93
22	0.66	0.16	1.18	0.29	0.81
23	0.53	0.14	1.58	0.37	0.78
24	0.36	0.10	1.40	0.19	0.75
25	0.58	0.08	1.26	0.17	0.92
26	0.30	0.07	1.00	0.10	0.82
Nevin (1974)[d]					
58	0.32	0.14	2.07	0.14	0.56
59	−0.27	0.53	2.58	0.64	0.06
60	0.86	0.23	1.03	0.11	0.78
Mean	0.11	0.29	2.08	0.29	0.04

[a] Nominal reinforcer durations were used.
[b] These data were read from a figure.
[c] Bias is measured towards the component without punishment.
[d] Nominal rates of reinforcement were used.

those using mixed grain. However, because all of the studies that used wheat were conducted by Davison and his colleagues, some other procedural difference between Davison's studies and those of other authors may have actually produced the difference. Therefore, the present results should be interpreted as giving an overview of how well Equation 1 describes multiple-schedule responding and of how concurrent- and multiple-schedule responding may be similar or different. The present conclusions should be verified in experiments specifically designed to examine the effects of the particular variables in question before strong conclusions are drawn.

Second, the measures used to apply Equation 1 to concurrent- and multiple-schedule responding are different. Equation 1 has been applied in the present paper to the local rates of responding and reinforcement because local rates are the measures that are usually reported for multiple schedules. Equation 1 is

applied to overall rates when studying concurrent schedules. Local rates are calculated by dividing the number of responses emitted or the number of reinforcers obtained during a component by the time spent responding on that component. Overall rates are calculated by dividing the number of responses or reinforcers by total session time.

Although the specific measures used for concurrent and multiple schedules are different, the implications of this difference are not clear. First, it might be argued that although the measures are calculated differently, they may nevertheless describe similar aspects of behavior. For example, the two different rates are typically used because Herrnstein (1970) argued that the appropriate time base for calculating rates is the time during which the component is available. Both components are available for the total session time during concurrent schedules. Each component is available for only part of the session during multiple schedules. According to Herrnstein's argument, local rates for multiple schedules and overall rates for concurrent schedules both describe the same aspect of the animal's behavior: the number of responses emitted per time of component availability.

Second, it might be argued that for most multiple schedules there will be little difference in the way Equation 1 applies to the overall and local rates. Therefore, the present results will be similar to those that would have been obtained had Equation 1 been applied to the overall rates, the same measure used for concurrent schedules. According to this argument, the total session time is the same for both components of the multiple schedule. Therefore, the time bases should cancel when Equation 1 is applied to the ratio of the overall rates. In most multiple schedules, the components are available for approximately equal lengths of time. (The only study in Table 1 that did not provide equal-duration components is that by Charman and Davison [1982]. However, even in that study, the duration of one component was a constant multiple of the other.) Therefore, the time bases will also cancel when Equation 1 is applied to local rates. Because the differences in time bases between local and overall rates cancel when ratios are taken, Equation 1 should describe overall rates in the same way as it describes local rates.

Alternatively, it may be that the differences between the measures used for concurrent and multiple schedules are symptomatic of fundamental differences between responding on the two schedules. For example, McSweeney, Melville, Buck, and Whipple (1983) found that the local rates of reinforcement obtained from the components of concurrent schedules are approximately equal for most schedules and that the local rates of responding are equal for many others. If this is so, then Equation 1 does not have an interesting application to the local rates of responding and reinforcement during concurrent schedules. The fact that Equation 1 applies to local rates during multiple schedules but not during concurrent schedules may suggest that different principles govern responding during the two schedules.

Consistent with this argument, Herrnstein and Vaughan (1980) theorized that subjects distribute their behavior between the components of concurrent schedules in such a way that the local rates of reinforcement obtained from the components will be equal. This theory is consistent with the results of McSweeney et al. (1983), but, because it would be difficult to apply the theory to multiple schedules, what may be implied is that concurrent and multiple-schedule responding are fundamentally different.

The differences between measures used to describe concurrent and multiple-schedule responding require further study. On the one hand, the differences may be more superficial than real, or they may be easily removed by settling on a common measure. On the other hand, they may eventually prove to be fundamental differences between concurrent and multiple-schedule responding.

Comparisons Based on the Parameters of Equation 1

Equation 1 describes the present data for multiple schedules approximately as well as it describes the concurrent-schedule literature, accounting for a median of 91% of the variance for the present studies and for a mean of 90.3% of the variance for concurrent schedules (Baum, 1979). However, there are slightly more multiple-schedule studies that Equation 1 does not describe well than there are concurrent schedules. Equation 1 accounted for less than 80% of the variance in the data for

28% of all studies reported in Table 1; it accounted for less than 80% of of the variance in the data in only 11% of the concurrent schedule cases reported by Baum (1979).

The undermatching parameters reported in Table 1 were considerably smaller than those usually reported for concurrent schedules. Baum (1979) reported that the undermatching parameter varied from 0.38 to 1.50, with most values falling between 0.80 and 1.00, for concurrent schedules. Most of the undermatching values in Table 1 fell between 0.00 and 1.00, with a median size of 0.46.

The median of the absolute values of the log bias parameters in the present study was slightly smaller than that reported for concurrent schedules. The MAVLB reported in the concurrent-schedule studies reviewed by Baum (1979) was approximately 0.060. It was 0.052 for the multiple schedules reviewed here.

Interpretations of Parameters

The present data cannot evaluate whether Baum's interpretations of the factors that should change the size of the undermatching parameter for concurrent schedules, also influence these parameters for multiple schedules. Baum (1974) argued that changeover-delay (COD) duration, the level of discrimination, and the level of deprivation for the programmed reinforcer can alter undermatching. Baum (1979) also argued that the use of different simple schedules as components should alter undermatching.

The effect of COD duration on the undermatching parameter cannot be assessed because multiple-schedule studies do not typically employ CODs. The effect of deprivation cannot be assessed because all of the results reported in Table 1 were produced by pigeons maintained at 80 to 85% of their free-feeding weights. The effect of using different simple schedules cannot be assessed because too few data were collected. The undermatching parameter for results from the two studies that did not use variable-interval schedules differed from the corresponding parameter from studies that used variable-interval components. However, too few data were collected to statistically evaluate this difference.

The fact that the undermatching parameters found in the present analysis are smaller than those typically reported in the concurrent-schedule literature might support Baum's argument that discrimination influences the size of the undermatching parameter. Discrimination might be better during concurrent schedules because the two components are constantly available for comparison; discrimination might be poorer in multiple schedules because the two components are never simultaneously available. However, this argument relies on an assumption about the factors that influence discrimination and should be supported by independent measures of discrimination before it is accepted.

Baum's speculations about factors that alter the bias parameter are more easily assessed using the present data. Baum argued that bias would vary if the operanda that produced reinforcers differed between the components. The present results did find different bias parameters when two different operanda were used than when only one was used. The fact that the median of the absolute values of the log bias parameters was sightly smaller for the present multiple schedules than for concurrent schedules may also support this claim. In multiple schedules, reinforcers in both components are usually contingent upon responses on the same operandum. Many concurrent schedules require responding on two different operanda. Therefore, it is more likely that the operanda used in the two components will differ in some minor ways—such as the force required to operate them—for concurrent schedules than for multiple schedules. If this is so, then bias would be greater for concurrent than for multiple schedules, which is the observed result.

Baum also argued that the use of two qualitatively different schedules in the two components would alter the bias parameters. As predicted, the bias parameters appeared to be different for the one study that used two different simple schedules (Zuriff, 1970, MAVLB = 0.242) than they were for other studies that used the same simple schedules in the two components (MAVLB = 0.052). The one study in which punishment was added to a component may also support Baum's argument: It resulted in bias parameters (MAVLB = 0.395) somewhat different from those reported in most studies (MAVLB = 0.052).

The present studies could not evaluate Baum's argument that differences between scheduled and obtained reinforcers, as well as qualitative differences between the reinforcers

scheduled in the two components, alter the size of the bias parameters. None of the studies reported in the Tables addressed these questions.

Comparisons of Multiple and Concurrent Schedules

The present undermatching parameters did not always change as a function of the same variables that produce changes in the undermatching parameters for concurrent schedules. However, two variables did produce the same effect. First, Table 1 may show an effect of the type of component schedule on the undermatching parameter. Likewise, several studies have shown that the choice of simple schedules influences the size of the undermatching parameter for concurrent schedules (LaBounty & Reynolds, 1973, as reanalyzed by Baum, 1974; Lobb & Davison, 1975; Nevin, 1971; Rider, 1981; Trevett, Davison, & Williams, 1972). Second, Todorov, Oliveira Castro, Hanna, Bittencourt de Sá, and Barreto (1983) reported that in the case of concurrent schedules, the size of the undermatching parameter decreased with increases in the number of schedules conducted. The present study found the same trend, at least when the extremes were compared.

In contrast, three variables appear not to produce the same effect on undermatching for concurrent and multiple schedules. First, the present study found that the size of the undermatching parameter decreased with increases in the number of sessions conducted per point. This trend is opposite to that found by Todorov et al. for concurrent-schedule responding. Second, Todorov et al. suggested that the use of naive versus experienced subjects may have altered the undermatching parameter describing performances on concurrent schedules. According to this argument, the use of naive subjects may have produced the higher undermatching parameters usually reported in studies conducted by Baum; the use of experimentally experienced subjects may have produced the lower undermatching parameters typically reported by Davison. However, the present study found no difference in the size of the undermatching parameters for naive and experienced subjects when multiple schedules were used. Third, the undermatching parameters found for studies that varied the rate of reinforcement (Table 1) may have been more similar to the undermatching parameters found for studies that varied the duration of reinforcement (Table 4) than is typically found in studies of concurrent schedules. The median undermatching parameter was 0.46 when rate of reinforcement was varied, and 0.40 when the duration of reinforcement was varied in the studies examined here. The direction of this difference is the same as that found for concurrent schedules, which typically also report larger undermatching parameters for rate than for duration or amount of reinforcement (Fantino, Squires, Delbrück, & Peterson, 1972; Schneider, 1973; Todorov, 1973; Todorov, Hanna, & Bittencourt de Sá, 1984; Walker, Schnelle, & Hurwitz, 1970). However, the difference between the sizes of the undermatching parameters seems to be much larger for concurrent schedules than for the current multiple schedules. For example, Todorov et al. (1984) found undermatching parameters of 0.84 and 0.97 for their 2 subjects when they varied rate of reinforcement, and undermatching parameters of 0.32 and 0.62 when they varied duration of reinforcement. These are larger differences than the difference between 0.46 and 0.40 for the present studies. Only future experiments will determine whether this apparent difference in the size of the undermatching parameters for rate and duration represents a real difference between concurrent and multiple-schedule responding or whether it is produced by other differences between the studies that determine the parameters.

The variables that alter the bias parameter were more similar for concurrent and multiple schedules. Only one factor produced a difference between the two sets of data. The absolute values of the logs of the bias parameters decreased for multiple schedules with increases in the number of schedules conducted. Todorov et al. (1983) found no effect of the number of schedules conducted on the bias parameter for concurrent schedules.

Two other variables produced similar results for concurrent and multiple schedules. First, the use of two different simple schedules for components seemed to alter the absolute values of the bias parameters reported in Table 1. Likewise, several studies have shown that the use of two different schedules alters

the bias parameters for concurrent schedules (Bacotti, 1977; Herrnstein, as cited in Baum, 1974; Lobb & Davison, 1975; Nevin, 1971; Rider, 1981; Trevett et al., 1972).

Second, the present study found no effect of the number of sessions conducted per schedule when subjects responded on multiple schedules. Likewise, Todorov et al. (1983) failed to find an effect of this variable on the bias parameter when subjects responded on concurrent schedules.

These similarities between the bias parameters for concurrent and multiple schedules may be more apparent than real, however. The factors that alter the bias parameters for concurrent schedules have rarely been studied. Further studies might reveal that many more parameters alter bias differently for concurrent than for multiple schedules, contradicting the present results.

Implications for Theory

The fact that Equation 1 describes well the data for concurrent and multiple schedules might be taken to support theories that attribute responding on both schedules to the same theoretical explanation (e.g., Herrnstein, 1970; Williams, 1983), at the expense of theories that do not (e.g., Rachlin, 1973). However, the current data caution against this conclusion for three reasons.

First, it seems reasonable to assume that an adequate account should describe behavior better when more complete information is available than when less complete information is available, but the opposite was found for Equation 1 as a descripton of multiple-schedule responding. The proportion of variance accounted for by Equation 1 decreased with increases in the number of schedules conducted and with increases in the number of sessions conducted per schedule.

Second, the present data question one of the bases for supporting Equation 1 as a unifying theory of concurrent and multiple-schedule responding. It has been argued that concurrent schedules are really multiple schedules with very short and unequal component durations. In support of this theory, it has been argued that the undermatching parameters for multiple schedules become larger and approach those found for concurrent schedules as components become shorter (e.g., Williams, 1983). The present results question this conclusion, however. The undermatching parameter did become larger as components became shorter for Shimp and Wheatley (1971), but this study provided only 2 days of data for each of two schedules for all but one component duration and involved very few sessions per schedule (mean = 11.4). Component duration had no systematic effect on the size of the undermatching parameter in the much more extensive Charman and Davison (1982) study, or when the results were analyzed across studies. It could also be argued that the Charman and Davison study provides a more relevant test of the similarity of concurrent and multiple schedules than that provided by Shimp and Wheatley. Charman and Davison, unlike Shimp and Wheatley, provided components of unequal duration, and it is argued that concurrent schedules resemble multiple schedules with unequal component durations. Therefore, the conclusion that multiple schedules with short-duration components resemble concurrent schedules because their undermatching parameters approach those found for concurrent-schedule responding becomes less secure. There is little evidence that responding on multiple schedules with short and unequal components approaches responding on concurrent schedules.

Third, if the present data are interpreted as supporting Equation 1 as a theory of multiple-schedule responding, then, paradoxically, they may also weaken Equation 1 as a theory of concurrent-schedule responding. Equation 1 drew some of its initial acceptance as a theory of concurrent responding from the fact that Baum (1974) provided an interpretation for each of the parameters (Timberlake, 1982). If these parameters had been seen as curve-fitting parameters, functioning only to make the equation better fit the data, there would have been nothing special about Equation 1. It would have gained acceptance as a model of behavior only after it was shown that it described the data better than did alternative two-parameter models. Instead, Equation 1 has been accepted as a theory of concurrent-schedule responding with little testing of the alternatives.

The present data may open Equation 1 to attack because they may question Baum's interpretation of his parameters in two ways.

First, different variables altered the bias and undermatching parameters for concurrent and multiple schedules. If these parameters have a theoretical interpretation, as Baum argues, then it seems more likely that they should change as a function of the same variables for both schedules. Second, several variables altered both the bias and undermatching parameters for responding on multiple schedules. Both parameters changed significantly with changes in the responses required for reinforcement and both decreased significantly with increases in the number of schedules conducted. Although the statistical significance of other effects could not be assessed, the present data also suggest that the random presentation of the components, the use of a signal-key procedure, and the use of different simple schedules as components also altered both parameters, although not always in the same direction. Changes in both parameters with changes in the same variables may be puzzling to Baum's interpretation, which assumes that the parameters represent different, independent effects on behavior.

This apparent weakening of Equation 1 as a general behavior theory might be mitigated by arguing that Equation 1 does not apply to multiple-schedule responding. Or it might be argued that the factors that alter bias and undermatching really are independent but that the crude procedural variables studied here alter factors that affect both parameters. But this argument creates problems of its own. It puts Equation 1 in jeopardy until pure procedures, which alter only one parameter, are developed. In this case again, Baum's interpretation of his parameters, and, therefore, the special status of Equation 1 as a theory of behavior, is questioned.

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