

*CHOOSING AMONG NATURAL STIMULI*

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Pigeons worked on concurrent variable-interval, variable-interval schedules with the alternatives signaled by slides either containing trees or not. The schedules were designed to hold both overall and relative rates of reinforcement within narrowly constrained limits, and slides were quasi-randomly ordered each day. Responding to the two alternatives was well described by the generalized matching equation with substantial undermatching. Using an adaptation of the matching law, we estimated that the subjects were correctly classifying 82% to 95% of exemplars. The matching performance transferred to new exemplars of trees and nontrees with only slight generalization decrement. The pigeons appeared to be discriminating among exemplars even when the alternatives provided equal rates of reinforcement and the average relative performances were close to 50%.

*Key words:* categorization, concepts, natural categories, choice, matching law, reinforcement, stimulus discrimination, pigeon

When choice is studied within the experimental analysis of behavior, the experimental chamber generally is extremely simplified. In the standard procedure, subjects usually choose between alternative but topographically similar ways of getting the same reinforcer, and are kept at constant levels of deprivation. Reinforcers are delivered on schedules typically differing in only one parameter. Although it has been argued (e.g., Kamil & Yoerg, 1982) that more natural environments better preserve "ecological validity," it can also be argued that once functional relations have been worked out within these simplified environments, it is possible to explore profitably more complex situations, including (but not limited to) those that more resemble the natural environment. Indeed, some experimenters now pit qualitatively different reinforcers against each other (e.g., Hursh, 1978), or they use schedules varying in basic structure, not just in a parameter (e.g., Herrnstein & Heyman, 1979). Other experiments mimic the natural environment to some extent by allowing the subject's, rather than the experimenter's, behavior to regulate deprivation levels or the reserves of varying qualities of forage (e.g.,

Collier & Rovee-Collier, 1981; Lea, 1982; Snyderman, 1983a, 1983b).

One additional simplifying constraint in the study of choice was shed in the research reported here. In the standard procedure, alternatives are signaled by unambiguously different stimuli—lights of distinct color or position are almost universal in laboratory research on choice. In nature, however, the animal is likely to be choosing among alternatives that belong to classes for which the membership criteria are probably open-ended and polymorphous (Dennis, Hampton, & Lea, 1973; Herrnstein, 1984), depending therefore on no necessary or sufficient features. These natural categories have been brought into the laboratory recently, but are not yet within the framework of research on choice.

We know that animals in nature, when choosing between alternative foraging locations, must deal to some extent with open-ended polymorphous classes. Moreover, we can assume that if a law of choice, which has been shown to hold in the laboratory, holds as well in nature, it must be with respect to instances of such polymorphous classes, because that is what nature confronts animals with. What we examine here is choice under controlled laboratory conditions, but stripped of the artificiality of precisely specified stimuli.

Using photographs of natural scenes as discriminative stimuli, Herrnstein and Loveland (1964) showed that pigeons could respond dif-

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ferentially to the presence or absence of persons in pictures seen for the first time. The technique has been extended to other classes of pictures—trees, water, letters of the alphabet, and so forth (see review in Herrnstein, 1984). In the present experiment, pigeons were required to choose between two concurrently available variable-interval schedules of reinforcement. Rather than the usual colors or positions identifying the alternatives, one schedule was accompanied by 35-mm slides containing trees, and the other schedule by slides not containing trees. Choice proportions established with one set of trees and nontrees were tested, without the schedules being changed, for generalization to tree and nontree slides never seen before. Such a procedure provides for at least the possibility of an integration of the domains of choice and categorization. In addition, as we suggest in the Discussion, it may lay a foundation for distinguishing between a capacity for merely categorizing and the presumably more abstract capacity to form concepts (a distinction first formulated for animals subjected to this discrimination procedure by Lea, 1984).

## METHOD

### *Subjects*

The subjects were experimentally naive, female, White Carneaux pigeons. They were run at approximately 80% of their free-feeding weights.

### *Apparatus*

A standard pigeon chamber was modified to allow the back-projection of 35-mm slides onto a centered screen key, 0.044 m high and 0.064 m wide. A standard pigeon key (changeover key) was mounted 0.051 m to the left of the screen key; below the screen was a hopper for the delivery of mixed grain. Reinforcement consisted of access to the grain and was produced intermittently by pecks on the screen key. The changeover key allowed the pigeons to advance from one slide to the next.

### *Procedure*

*Preliminary training.* The pigeons were first trained to peck on the screen key. As soon as they were pecking consistently, pecks were reinforced when the screen showed a yellow triangle and not when it showed a purple

square. The triangle-square phase of training, which was included to ensure that the pigeons were attending to stimuli on the screen, lasted two sessions and was immediately followed by sessions using tree and nontree slides.

For five sessions, the same 40 tree and 40 nontree slides were presented in quasi-random order (see below). Because it is possible that a pigeon would not be attending to the screen when the slide first went on, responses were not recorded during the first 2 s of a slide's presentation. For the next 10 s, pecks on the slide key were recorded for each slide but had no further consequence. Following this 10-s period, a VI 10 s (variable interval of 10 s) started. For tree slides, a peck after the end of a programmed interval was reinforced (2-s access to grain) only if it occurred within 2 s of the preceding peck. The slide projector was shut off at the end of reinforcement and the slide tray advanced to the next position; 4 s after the projector turned off, it came on again. For nontree slides, pecks were unreinforced; instead, after a programmed interval on the VI 10 s, the first 15-s period without a peck turned the projector off and advanced the slide tray to the next position.

The pigeons learned rapidly to discriminate between tree and nontree slides. Then, for the next five sessions, the discrimination was reversed: Pecking produced food in the presence of nontrees and not in the presence of trees. Performance rapidly switched accordingly. For the last two sessions of preliminary training, pecking in the presence of all slides was reinforced as above, so as to begin the experiment proper with a tendency to respond to both classes.

*Experimental procedure.* At the start of a session, only the changeover key (CO key) was illuminated. A single peck turned it off and advanced the slide tray, followed 1 s later by the illumination of the slide projector. The first peck on the screen key reilluminated the CO key, but was not recorded and had no other consequence. This was done simply to ensure that birds attended to each slide. At this point a CO-key response extinguished the CO-key light and the slide projector, the slide tray advanced, and 1 s later the slide projector came on. The procedure thus resembles Findley's (1958) changeover procedure, except that tree and nontree stimuli (and the availability

of their correlated schedules) did not strictly alternate, but did so quasi-randomly (see below).

After the first peck on the screen key following a slide-tray advance, each subsequent peck was added to the response tally for the class of slide present, and initiated a timer that ran until either (a) 2 s elapsed without another peck on the screen key, (b) reinforcement was produced, or (c) the CO key was pecked. Operation of the timer drove two concurrent variable-interval schedules, one programming reinforcements to be delivered in the presence of tree slides, and the other in the presence of nontree slides. No peck at the screen key could be reinforced within 1 s of starting a stopped 2-s timer. The 2-s differential-reinforcement-of-high-rate (DRH) contingency probably discouraged monkey-pecking behavior that might become adventitiously associated with key pecking. To the extent that it did so, the contingency increased average rates of key pecking. For purposes of analysis, rates of responding and reinforcement in the presence of a slide are calculated with respect to time during which the 2-s timer ran.

Both VI schedules operated whenever the 2-s timer did. Programmed reinforcers were stored, up to an overall maximum of three, but the schedules operated continuously, whether or not reinforcers were stored. It was thus possible to lose a reinforcer if three had been stored and a fourth was programmed. However, inasmuch as there were few occasions when reinforcers were missed because of the limit of the storage capacity, the overall rate of reinforcement was largely independent of the pattern of behavior. Reinforcers from the two schedules could be collected only in the order in which they set up. Like Stubbs and Pliskoff's (1969) procedure, this one also guarantees that the obtained relative frequency of reinforcement closely approximates the programmed relative frequency. The present procedure was thus designed to keep both overall and relative rates of reinforcement close to their programmed values, except in the vicinity of exclusive preference.

Except as indicated below, the same set of 40 tree and 40 nontree slides were in the tray daily, reordered for each session. The slide orders were random except that no more than 4 tree or 4 nontree slides could appear consecutively, and each of the 8 groups of 10 slides

Table 1  
Experimental procedure.

Session	Slide set	Session	VI schedule (s)	
			Trees	Non-trees
1-14	1	1-21	60	60
15-39	2	22-52	36	180
40-80	3	53-94	120	40
81-119	4	95-127	40	120
120-156	5	128-166	180	36
157-166	1			

in a tray of 80 contained 5 tree and 5 nontree slides. Sessions terminated after 40 reinforcements.

Five pairs of VI schedules were run, with the ratio of programmed reinforcements in the presence of trees to that of nontree set at 1:1, 5:1, 1:3, 3:1, and 1:5, in that order. The programmed overall rate of reinforcement was 120 per hour under all conditions. The first slide set seen was the same as that used during preliminary training. At least seven sessions before changing schedule values, the set of 80 slides was replaced with another set, so as to assess transfer in the absence of a change in schedules. During the final reinforcement schedule, the replacement set was the one used at the beginning of the experiment; otherwise, each replacement set comprised only slides never seen before by the subjects. All slides were drawn from a large library; an informal attempt was made to choose instances of trees and nontrees that, to a human observer, seemed fairly easy to identify as such. Because the pigeons controlled the rate of presentation of slides, the number seen each session varied, but, in general, pigeons saw at least one full revolution of the tray per session. Table 1 summarizes the number and order of sessions for each condition and for each set of slides.

## RESULTS

### *Transfer of Preference*

Subjects on concurrent variable-interval schedules tend to equalize, or match, the ratio of responses (or the ratio of times spent responding) to the ratio of reinforcements received from the alternatives. A general version of this principle was formulated by Baum (1974), as follows: Let  $P_T$  and  $P_N$  represent

pecks to tree and nontree slides, and  $R_T$  and  $R_N$ , reinforcements from the two alternatives. Baum's generalized matching law states:

$$\frac{P_T}{P_N} = b \left( \frac{R_T}{R_N} \right)^a \quad (1)$$

In the case of "normative" matching, the parameters  $a$  and  $b$  equal 1.0. Values of  $b$  other than 1.0 indicate bias toward one alternative or the other. Values of  $a$  below 1.0 are termed *undermatching*, and values above 1.0 are termed *overmatching*. Undermatching may occur for various reasons, including some degree of failure in discrimination between the alternatives. The generalized matching law for times spent responding replaces the  $P$  terms in Equation 1 with  $T_T$  and  $T_N$  for time spent in the presence of tree and nontree slides, respectively.

Figure 1 shows the log of the ratio of times spent responding,  $T_T/T_N$ , as a function of the log of the ratio of reinforcements received,  $R_T/R_N$ , for each pigeon. Equation 1 with parameters equal to 1.0 would plot as lines of unit slope passing through the origin (0,0). Each of the five open points in a plot is an average of the final five sessions in the presence of the original slide tray correlated with each pair of schedule values. These sessions came just prior to replacement with a new set of slides. The filled points are averages for the five sessions just prior to a change in the reinforcement schedule, which, as mentioned above, always occurred at least seven sessions after the slide trays were replaced. Best fitting lines were estimated by the method of least squares for the logarithm of Equation 1 for each set of five data points separately—dashed lines for the open points, solid lines for the filled points. Numbers next to each data point identify the set of slides in effect at the time.

All the fitted functions resemble each other in major respects. Table 2 lists the slopes, intercepts, and variances accounted for, or  $r^2$ 's, for each of the eight lines in Figure 1. For every subject, the slope was below 1.0, indicating undermatching, and the intercept was close to 0, indicating virtually no bias. Variances accounted for were .94 or higher in all cases. Fitting the logarithm of Equation 1 to pecks, for the five last sessions with each replacement set, results in essentially the same equation. For pecks, the average slope was

.63, the average intercept, .02, and the average  $r^2$ , .97; for time spent responding, the corresponding values were .64, .02, and .97.

The degree of undermatching in Figure 1 was slightly greater (the slopes were lower) for the replacement set than for the original set of slides for every subject, averaging .64 and .72, respectively. This small difference may have arisen because transfer of preference to new exemplars of trees and nontrees was less than complete. This possibility is directly examined in Figure 2. Here, the ratio of times spent responding,  $T_T/T_N$ , is shown for the five sessions just prior to a replacement of the slide tray and for the first five sessions with the replacement tray, for each pigeon and for the five such replacements. There were no changes in reinforcement schedules at these transitions (see Table 1).

If there had been no transfer of preference at all, then the first session with a new tray should have approximated .5 on the ordinate. With perfect transfer, these first and subsequent sessions should have fallen on the line established by the preceding five sessions with the other tray. There is a small but consistent tendency for the initial sessions with new slides to regress toward .5, indicating incomplete transfer.

#### *Frequency Distributions of Exposure Durations*

In this procedure, the pigeon controls the exposure duration of slides. From Figure 1, we already know that the average durations were controlled in such a way that Equation 1 fit closely. However, Figure 1 contains no information about the distributions of exposure durations for a given class of slide. That information is provided in Figure 3, which gives frequency distributions of exposure durations for tree and nontree slides in class intervals of 1 s. The points in the figure are averages, across the 4 subjects (which did not differ systematically), of the five sessions prior to replacements of the slide tray. The top left panel shows the case in which tree slides were correlated with a VI 180-s schedule (Sessions 152 to 156) and the case in which nontree slides were correlated with the same schedule (Sessions 35 to 39). In the case of VI 60-s schedules (Panel C), the two schedules were present simultaneously (Sessions 10 to 14). To reconstruct the four other concurrent schedules, combine the tree or nontree curve of panel

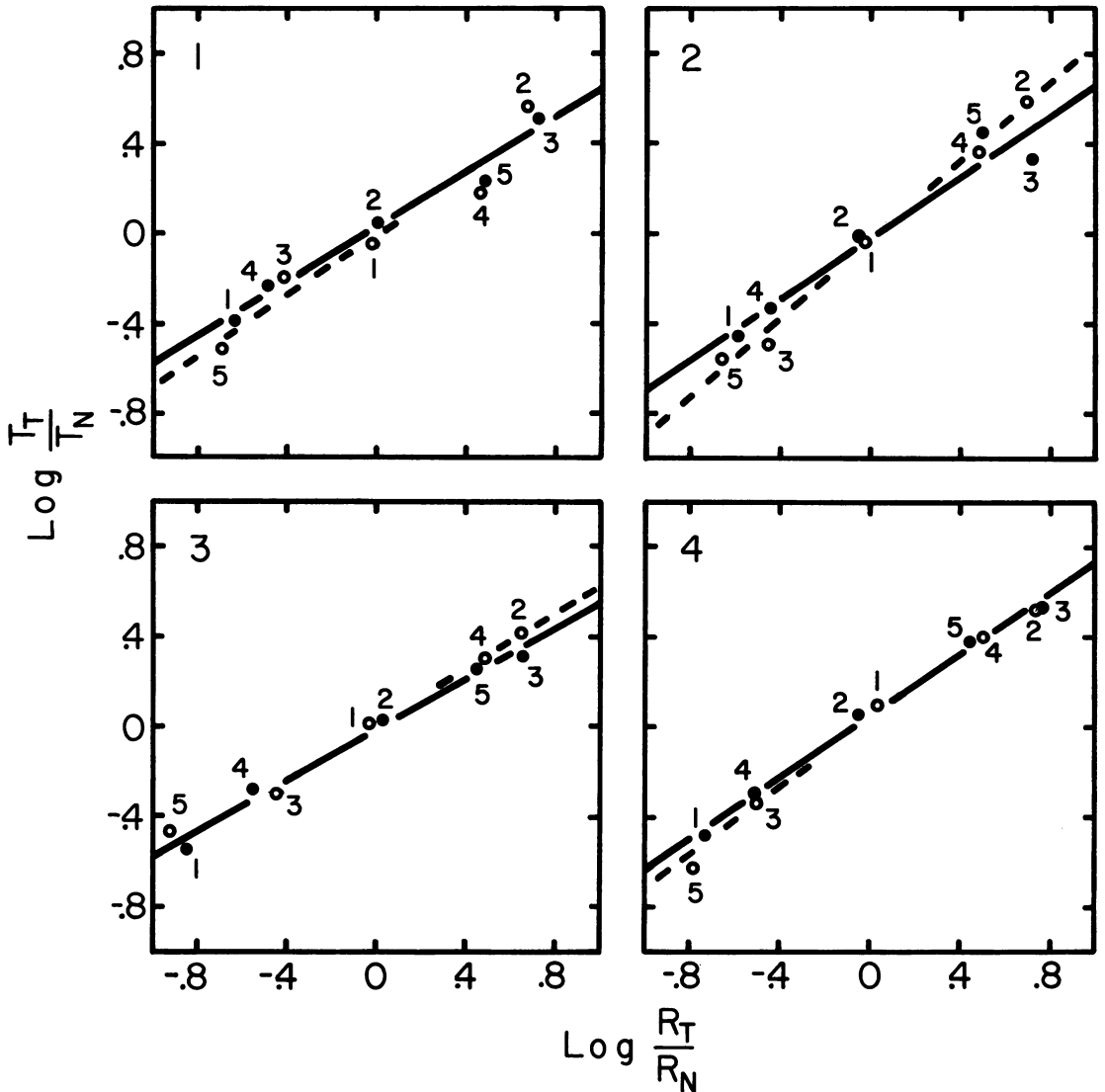


Fig. 1. Log ratio of times in the presence of trees to times in the presence of nontrees, as a function of log ratio of reinforcements for those two classes. Open points (and the least square dashed-line fit) correspond to the means from the five sessions just prior to changing the slide set; closed points (and the least square solid-line fit) correspond to the means from the five sessions just prior to changing the schedules in effect. The number next to each data point indicates the slide set in effect at the time.

A with the complement in panel E, and the tree or nontree curve of panel B with the complement in panel D.

The richer the reinforcement schedule correlated with a category of stimuli, the more time the pigeon was likely to spend with exemplars of it, as expected. However, Figure 3 shows that the exposure durations overlapped significantly across the range of reinforcement

rates provided by VI 36 s to VI 180 s. The leaner schedules had the effect of narrowing the frequency distributions and shifting them toward zero. For any given reinforcement rate, the distribution of exposure durations for trees did not differ systematically from that for nontrees. This similarity not only confirms the absence of bias in Figure 1 and Table 2, but adds a further dimension, inasmuch as two

distributions can have equal means while being dissimilar in overall shape. Figure 3 shows that the distributions, and not just the means, are unbiased with respect to trees and nontrees.

### Concordance

The pigeons clearly discriminated between exemplars of tree and nontree, as the foregoing results summarize. But were they consistently distinguishing among exemplars of trees and among exemplars of nontrees? To the extent that they were, the spread of exposure durations illustrated in Figure 3 must have been based on characteristics of the photographs, rather than on random variability. We used a measure of concordance, Kendall's coefficient of concordance  $W$  (Siegel, 1956), within and across subjects, to assess the consistency with which exemplars were ranked with respect to time spent in their presence. The time values were means for a session. Table 3 shows the results. The upper part of the table gives within-subject concordances for the final four sessions of each of the five reinforcement schedules; the lower part, across-subject concordances for the 4 subjects, for each of the final four sessions (the last one is identified as 4). Four sessions, rather than five, were used so as to keep measures between and within pigeons comparable. It is necessary to calculate concordances within the categories of tree and nontree taken separately, so that the calculations are uncontaminated by the degree to which responding to the two categories was being pulled apart by the contingencies of reinforcement.

Table 3 includes only concordances significant at  $p$  levels of .05 or smaller.<sup>1</sup> A given pigeon's rankings were more alike over consecutive sessions than were the rankings of the 4 pigeons on a given session. Only when the reinforcement schedules were maximally different—with reinforcement rates differing by a factor of five—did the between-subject concordances ever reach statistical significance.

<sup>1</sup> Significance levels for the coefficient of concordance are conceptually analogous to those for coefficients of correlation. The null hypothesis is not, however, a value of 0 for  $W$  (as it is for standard measures of correlation), but a value between 0 and 1.0, depending on sample size and range of ranks.

Table 2  
Best fitting lines in Figure 1.\*

Subject	Slope		Intercept		$r^2$	
	Original	Replacement	Original	Replacement	Original	Replacement
1	.68	.61	.00	.03	.94	.98
2	.87	.68	-.03	-.01	.99	.94
3	.58	.57	.03	-.01	.99	.99
4	.74	.68	.02	.05	.99	.99

$$* \text{Log}(T_T/T_N) = a \text{log}(R_T/R_N) + \text{log } b.$$

The large number of significant values in the upper half of the table indicates that specific characteristics of the photos must have influenced their rank. The much smaller number in the lower half further suggests that the controlling characteristics were mainly, though not entirely, idiosyncratic. The idiosyncratic rankings tended to give way to a consensus among the pigeons as the schedules became maximally different. The Discussion considers why the only significant concordances among subjects were for the most extreme schedules.

## DISCUSSION

These results extend the range of Baum's (1974) generalized matching law as a descriptive framework, to choice between open-ended categories of the sort presumably found in nature. They are a partial response to the complaint (e.g., Kamil & Yoerg, 1982) that the artificiality of laboratory experiments of choice severely limits the applicability of the matching law. In terms of the  $r^2$ s (see Table 2), the matching here was as good as or better than that shown by pigeons looking at the artificial stimuli of the standard choice procedure.

Although many researchers in animal behavior stress the desirability of maintaining ecological validity, their reasoning makes sense only if rather implausible conditions hold: that animals bring to bear within artificial situations behavioral principles that are both uninteresting in and of themselves and that bear little resemblance to what occurs in nature. There is the added difficulty that some law must govern when such principles are brought to bear: Is this a law that operates in natural situations, artificial situations, or both? It

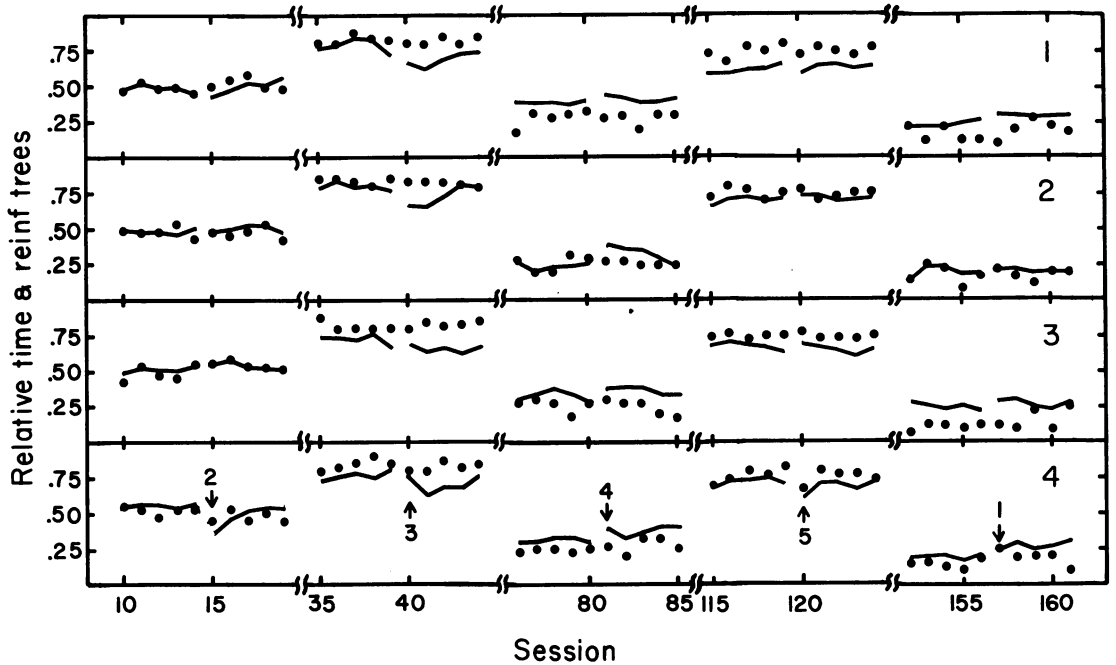


Fig. 2. Relative time in the presence of tree slides (points) and relative reinforcements for tree slides (solid lines) for the five sessions just prior to changing the slide set and for the five sessions just after. Session numbers are indicated on the abscissa; numbered arrows indicate the slide set that was introduced. (Adapted from Herrnstein, 1985.)

seems to us more likely that the same basic behavioral principles manifest themselves in both settings, and that those principles can best be originally discerned within a deliberately contrived and simplified situation. Experiments such as the present one can then access whether in fact the same principles continue to hold (or how they are modified) as the situation becomes more like the natural environment.

All subjects undermatched with all sets of stimuli, as would be expected if some exemplars did not fall unequivocally into one category or the other. A direct test of this explanation for the undermatching would require replacing the tree and nontree slides with unambiguous (i.e., artificial) stimuli, a test we have not performed here. However, Herrnstein and Vaughan (1980, Figure 5.7) discuss data from such an experiment using conventional stimuli (except that there was a third class of slides in the presence of which pecking was never reinforced and that in which only two reinforcers could be stored). Of the 4 pigeons in that study, 2 undermatched only

slightly and 2 overmatched slightly. On the average, they matched normatively.

If we assume that the undermatching here was due to stimulus ambiguity rather than to the various other possible sources, we can adapt the basic matching equation so as to estimate incorrect classifications into the two categories. The lack of bias suggests that confusions were symmetrical—that is to say, a tree was as likely to be classified as a nontree as vice versa. Below, we give a more rigorous argument for symmetry. Let us define a term— $h$ —that expresses the probability of correctly classifying slides, and whose complement,  $1 - h$ , is the probability of misclassifications. (In the absence of symmetry, we would postulate two values for  $h$ , one for each of the two categories.) This analysis does not permit us to say whether given slides were being consistently misclassified, whether all slides had an equal probability of error, or whether there was a distribution of probabilities for the different exemplars. In any case, the reinforcements correlated with each type of behavior would be the sum of the appropriate rein-

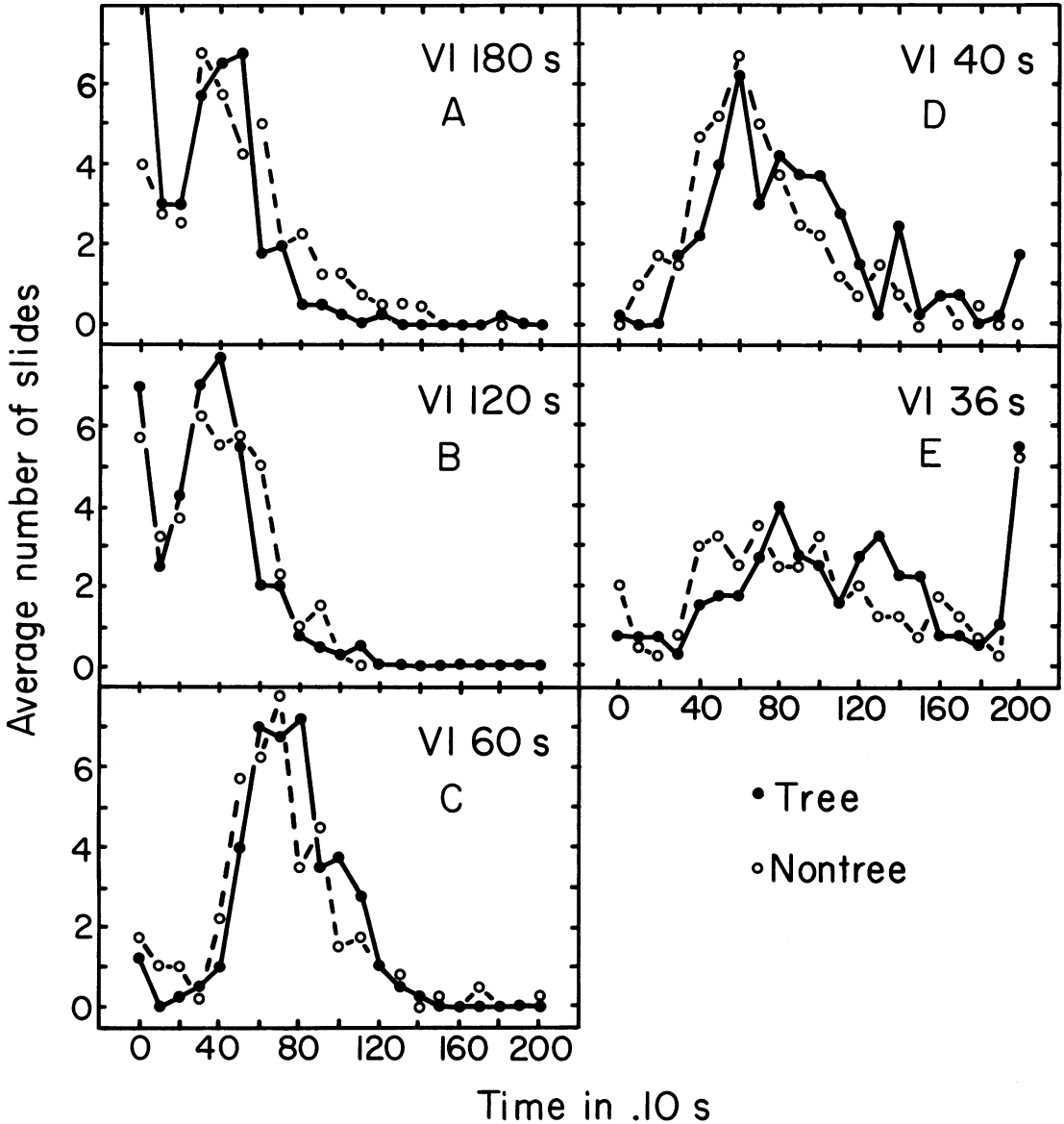


Fig. 3. Distributions of visit times (i.e., times between changeovers) in the presence of tree slides (solid lines) and in the presence of nontree slides (dashed lines). Each panel shows the distributions from the two cases in which the same VI schedules were in effect. For panel C, those two cases were also concurrently available; for the others, they were not. These functions are the averages of the 4 pigeons, which did not differ in any substantial way.

forcements weighted by  $h$  and the inappropriate reinforcements weighted by  $1 - h$ . The normative matching equation (i.e., no bias and exponent of 1.0 on the reinforcement terms) then becomes:

$$\frac{B_1}{B_2} = \frac{hR_1 + (1 - h)R_2}{hR_2 + (1 - h)R_1}$$

$$= \frac{R_2 - h(R_2 - R_1)}{R_1 - h(R_1 - R_2)} \quad (2)$$

Equation 2 differs analytically from Baum's generalized matching law, but the two would be impossible to discriminate empirically for the present study, given the small number of data points. (For approaches to the stimulus-



Table 3  
Concordance. Reinforcement ratios—tree : nontree.

		1:1		5:1		1:3		3:1		1:5	
		Tree	Non-tree	Tree	Non-tree	Tree	Non-tree	Tree	Non-tree	Tree	Non-tree
Subjects across 4 sessions	1	.60 <sup>a</sup>	.48 <sup>a</sup>	.51 <sup>a</sup>	.60 <sup>a</sup>	.65 <sup>a</sup>	.48 <sup>a</sup>	.41 <sup>b</sup>	.54 <sup>a</sup>	.68 <sup>a</sup>	.52 <sup>a</sup>
	2	—	.42 <sup>b</sup>	.48 <sup>a</sup>	.35 <sup>c</sup>	.72 <sup>a</sup>	.53 <sup>a</sup>	.53 <sup>a</sup>	.68 <sup>a</sup>	.65 <sup>a</sup>	.47 <sup>a</sup>
	3	—	.46 <sup>a</sup>	.52 <sup>a</sup>	—	.56 <sup>a</sup>	.60 <sup>a</sup>	.55 <sup>a</sup>	.62 <sup>a</sup>	.58 <sup>a</sup>	.52 <sup>a</sup>
	4	.68 <sup>a</sup>	.71 <sup>a</sup>	.53 <sup>a</sup>	.50 <sup>a</sup>	.62 <sup>a</sup>	.44 <sup>a</sup>	.44 <sup>a</sup>	.52 <sup>a</sup>	.66 <sup>a</sup>	.76 <sup>a</sup>
Sessions across 4 subjects	1	—	—	.43 <sup>a</sup>	—	—	—	—	—	.39 <sup>c</sup>	.35 <sup>c</sup>
	2	—	—	—	—	—	—	—	—	—	—
	3	—	—	.35 <sup>c</sup>	—	—	—	—	—	—	—
	4	—	—	—	—	—	—	—	—	.37 <sup>c</sup>	—

<sup>a</sup>.005  $\geq p$ .

<sup>b</sup>.01  $\geq p \geq .005$ .

<sup>c</sup>.05  $\geq p \geq .01$ .

— n.s.

discrimination problem that are based on generalized matching, see Davison & Tustin, 1978, and White, Pipe, & McLean, 1984.) A single value of the exponent in Equation 1 corresponds almost exactly to a single value of  $h$  for the range of reinforcement ratios observed here. Indeed, we have not plotted the best fitting functions for Equation 2 because the difference between the two equations would fall within the width of a pencil line in Figure 1. Conceptually, however, Equation 2 seems a more reasonable way to formalize the undermatching due to stimulus confusions.<sup>2</sup> (Other sources of undermatching, such as undermatching due to psychophysical scaling of reinforcement variables, may more properly be represented with exponents below 1.0.) With Equation 2, we can specify the proportion of misclassifications that would yield a given exponent for undermatching in Equation 1 by fitting a value of  $h$  that most closely approximates the observed undermatching. For example, to obtain the degree of undermatching expressed by an exponent of .70 in Equation 1,  $h$  would approximately equal .88; for an exponent of .60,  $h$  would approximately equal .83.

Table 4 presents the values of  $h$  corresponding to the slopes in Figure 1 and Table

2, for each of the 4 pigeons responding to the original and replacement slide sets. Table 4 says, in effect, that if the pigeons had matched normatively except for classification errors, they would have correctly classified between 82% and 95% of the slides.<sup>3</sup> The replacement sets had, for 3 of the pigeons, slightly smaller proportions of correct classifications, possibly because transfer was imperfect and relearning was incomplete. In earlier experiments (Herrnstein, 1979; Herrnstein, Loveland, & Cable, 1976), in which trees and nontrees were correlated with reinforcement and nonreinforcement,  $\rho$  (a measure of discrimination based on the probability of ranking positive exemplars of a category above negative exemplars) ranged between .85 and .91, averaged over groups of subjects. The average value of  $h$  is here .87.

To return briefly to the assumption of symmetry—the assumption that the subjects made

Table 4  
Proportions correctly classified ( $h$  in Equation 2).

Subject	Original	Replacement
1	.87	.84
2	.95	.87
3	.82	.82
4	.90	.87

<sup>2</sup> Essentially the same equation has been applied by Burgess and Wearden (1986) to the quantification of the effects of superimposed, response-independent reinforcement, by letting  $1 - h$  (their " $p$ ") represent the proportion of the superimposed reinforcements that are miscredited to the baseline response.

<sup>3</sup> The proportions of correctly classified stimuli given in the text and Table 4 are lower bounds, for if there are additional sources of undermatching besides stimulus confusions, they would also reduce the value of  $h$ .

as many errors in one direction as the other, and that, therefore, only a single value of  $h$  in Equation 2 suffices: It can be readily shown that if we allow two values for  $h$ , corresponding to differing probabilities of correct classification for the two categories, then Equation 2 implies bias in the generalized-matching-law sense. Indeed, the degree of bias could be used to estimate the degree of asymmetry. However, in the present instance, there is no evidence of significant bias (see Figures 1 and 3), and hence, no evidence of asymmetry in Equation 2.

In past experiments on natural categorization, the discrimination has been between stimuli correlated with reinforcement and with nonreinforcement. Here, both categories were correlated with nonzero levels of reinforcement. The differences in time allocated to the alternatives here must therefore depend not just on the inherent discriminability of the stimuli, but on the difference in the rates of reinforcement correlated with the stimulus categories—somewhat akin to “sensitivity” and “bias” in the usage of signal-detection theory. The parameter  $h$  measures sensitivity or discriminative accuracy, based on the allocation of behavior across the range of schedules, whereas the relative responding in a given schedule can be thought of as a partial analogue of bias (in the signal-detection, rather than the matching-law, sense), attributable to the prevailing reinforcement ratio. That is to say, the matching law itself quantifies the extent to which a difference in reinforcement rate “biases” responding toward one alternative or the other.

When the two schedules were equal, subjects divided their time equally between the two categories. Was this a sign of neutral or of dynamic equilibrium? It might have been supposed that discrimination would vanish entirely when the reinforcement ratio was at 1:1, for the resulting performance might appear to signify indifference between the alternatives, which is one possible (average) outcome of neutral equilibrium. However, there is a genuine meliorizing contingency (Herrnstein & Vaughan, 1980) in all the reinforcement schedules, including 1:1. Matching implies that the alternatives reinforce at equal local rates, whatever the programmed variable-interval schedules. Deviations from matching always produce a difference in local

reinforcement rates. If the precondition for discrimination between stimuli is a correlated difference in local reinforcement rate, then discrimination would be sustained in all schedules as the local reinforcement rates fluctuate around equality. Performance on the 1:1 schedule was as narrowly confined around the matching value (see Figures 1 and 2) as on the other schedules, suggesting dynamic, rather than merely neutral, equilibrium.

Further evidence that the pigeons were discriminating at least among exemplars, if not between the categories of tree and nontree, under all reinforcement ratios, including 1:1, is implicit in Table 3, presenting concordances. In order for a pigeon's rankings to be self-concordant, the slides within a category would have to be discriminated from each other. Concordance levels within subjects tended to be the smallest at the 1:1 reinforcement ratio, but even here they were still statistically significant in six of eight instances.

Across subjects (Table 3, bottom), the concordances fell short of statistical significance except when the reinforcement schedules were maximally different. By choosing slides that seemed to us to be easily sorted (see Methods), the stimulus variance within categories was truncated and, in all likelihood, the intersubject correlations consequently were reduced (see Herrnstein et al., 1976, for evidence that exemplars that seem difficult or easy to humans to discriminate are likewise difficult or easy for pigeons). However, it can be shown that the more different the schedules, the larger the discrepancy in local reinforcement rates for a given deviation from matching (Herrnstein & Vaughan, 1980). As the schedules became more different, the discrimination between trees and nontrees increasingly overrode whatever idiosyncratic distinctions a given pigeon had made among exemplars of each of the two categories. Whether or not a slide is a good or poor exemplar of trees versus nontrees for pigeons in general then came to predominate over idiosyncratic distinctions as a factor controlling responding and time allocation.

The absence of bias or other asymmetries in responding in both average (Figure 2) and distributional statistics (Figure 3) may appear to be a counterinstance to the “feature-positive effect” (Hearst, 1984; Jenkins & Sainsbury, 1970). Contrary to the implications of

such an effect, the pigeons showed no greater or lesser tendency to respond to trees or nontrees, above and beyond the tendency accounted for by Equations 1 or 2. This confirms the results of an earlier categorization experiment in which trees and nontrees were differentially correlated with reinforcement and nonreinforcement (Herrnstein, 1979). It was found that trees or nontrees served equally well as the signal for reinforcement. These are counterinstances to the feature-positive effect only if "tree" and "nontree" stand in the relation of feature and nonfeature, respectively. But there is another possible interpretation of the lack of the effect in these studies. If pigeons are forming category rules, not just for the exemplars of trees, but also for those of nontrees, then no feature-positive effect would be expected, for an instance of nontrees would contain its "features" no less than one of trees. The features of nontrees may be difficult for the experimenter to fathom, but they would presumably comprise the disjunction of trees with the sorts of things that turn up in slides—people, buildings, sky, water, and so on. Other evidence is also consistent with pigeons' behaving as if they were redundantly attempting to characterize both positive and negative categories, rather than the more efficient approach of characterizing just one of the categories, of which the absence is the other category (see Herrnstein et al., 1976).

On five occasions, one slide tray was replaced with another to test for generalization of the discrimination to new exemplars of trees and nontrees (see Figure 2). Here, generalizing meant maintaining a particular ratio of times spent with the two categories; failure to generalize meant that the ratio of times would regress toward .5. The first generalization test was not informative on this point, in that the ratio of times was about .5 to begin with. The subsequent tests each showed substantial generalization, usually with only slight and quickly fading disruption of the established performance level.

The final generalization test is of particular interest. Here, the baseline-performance level was at a ratio of about 1:3.3 (trees to nontrees) in the presence of the fifth tray of slides. With the replacement tray, performance remained almost unchanged from the first session on. The replacement set was not, however, new to the pigeons; it was the same tray used for

the first schedule, with a reinforcement ratio of 1:1. In other words, on this test the pigeons responded not as they had earlier to these very slides, but as they had to other exemplars drawn from the same categories.

In the final generalization test, the pigeons responded with respect to the categories—that is, trees and nontrees—rather than with respect to particular exemplars. Slide Set 1 had been correlated with a 1:1 distribution of behavior when last seen, but in the final generalization test the prevailing schedule of reinforcement called for more responding to nontrees than to trees. The exemplars in slide Set 1 must be more similar to themselves than to those in slide Set 5, which they were replacing in this test, yet the pigeons transferred to Set 1 the pattern of responding to Set 5, rather than reinstate the earlier pattern to Set 1. The results make sense if the 1:3.3 response tendency displaced the 1:1 tendency previously correlated with slide Set 1 by reconditioning to the *classes* that embrace the exemplars (i.e., tree and nontree) rather than to the *individual* exemplars. To the extent that a class may be construed as a concept if it is superordinate to the exemplars drawn from it, as Lea (1984) has argued it may, the present results suggest that pigeons can conceptualize, not merely categorize.

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