*SHORT-TERM MEMORY FOR RESPONSES:
THE "CHOOSE-SMALL" EFFECT*

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Pigeons' short-term memory for fixed-ratio requirements was assessed using a delayed symbolic matching-to-sample procedure. Different choices were reinforced after fixed-ratio 10 and fixed-ratio 40 requirements, and delays of 0, 5, or 20 s were sometimes placed between sample ratios and choice. All birds made disproportionate numbers of responses to the small-ratio choice alternative when delays were interposed between ratios and choice, and this bias increased as a function of delay. Preference for the small fixed-ratio alternative was also observed on "no-sample" trials, during which the choice alternatives were presented without a prior sample ratio. This "choose-small" bias is analogous to results obtained by Spetch and Wilkie (1983) with event duration as the discriminative stimulus. The choose-small bias was attenuated when the houselight was turned on during delays, but overall accuracy was not influenced systematically by the houselight manipulation.

Key words: fixed-ratio discrimination, delayed symbolic matching to sample, memory, response bias, subjective shortening, time-order error, key peck, pigeons

In a recent series of experiments, Spetch and colleagues (e.g., Spetch, 1987; Spetch & Rusak, 1989; Spetch & Wilkie, 1982, 1983) have provided detailed observations of pigeons' performance on delayed-discrimination tasks with event duration as the discriminative stimulus. In the initial experiments (Spetch & Wilkie, 1982, 1983), pigeons were trained on a delayed symbolic matching-to-sample (DSMTS) task in which the duration of a prior event (access to food or presentation of a houselight) served as the discriminative stimulus. Different choice responses were reinforced after short- and long-sample events. When delays (i.e., retention intervals) were interposed between the sample event and choice, Spetch and Wilkie observed that their animals showed a bias towards the short-sample choice alternative. Especially at test delays greater than 10 s, subjects tended

to choose the short-sample alternative more frequently than the long-sample alternative. This bias produced a pattern of results in which accuracy following short-sample events remained approximately constant with increasing delay, whereas accuracy following long-sample events decreased markedly with lengthening delays, in many cases reaching levels that were well below chance. Logically enough, Spetch and Wilkie labeled this phenomenon the "choose-short" effect.

This effect, which has proven remarkably robust (e.g., see Spetch, 1987), was originally explained as resulting from a process called subjective shortening (Spetch & Wilkie, 1983). According to this account, remembered durations undergo a foreshortening over time through the delay interval; the longer the delay, the greater the foreshortening such that the remembered duration of a long sample would seem subjectively equivalent to a short sample presented without a delay. The subjective-shortening hypothesis has a number of appealing features. It accounts for the data very well. It is compatible with time-order effects prevalent in studies of human (e.g., Allan, 1979) and pigeon (e.g., Fetterman & Dreyfus, 1986) duration discrimination. It is also consistent with notions about a time-dependent foreshortening of events that lie at the core of many views of memory (e.g., James, 1890; W. Roberts & Grant, 1976; Staddon, 1984).

The subjective-shortening hypothesis appears to be further strengthened by the finding that animals tend to respond short when the

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choice alternatives are presented without a prior sample duration (Church, 1980; S. Roberts, 1982; Spetch & Wilkie, 1983). This result could be due to a temporal generalization effect whereby no-sample (i.e., 0-s) test trials are perceived as more similar to short- than to long-duration samples. However, the no-sample result is also compatible with alternative interpretations of the choose-short effect (e.g., see Killeen & Fetterman, 1988; Kraemer, Mazmanian, & Roberts, 1985).

The Spetch and Wilkie (1982, 1983) model was developed to account for biased forgetting with duration tasks. It holds that forgetting occurs along the temporal dimension, and that temporal memories are retrospective. However, the choose-short result could reflect more general causal factors, perhaps revealing a basic property of memorial psychophysics with prothetic (quantitative) stimuli. The choose-short effect bears a close resemblance to the negative time-order errors commonly observed in human psychophysical experiments (Hellström, 1985). Negative time-order errors reflect a tendency to underestimate the first stimulus of a sequential pair relative to the second. For example, when two equal durations are presented in succession, the first typically is judged shorter than the second. This result is obtained routinely in human timing experiments (e.g., Allan, 1977) and has recently been observed in pigeons (Fetterman & Dreyfus, 1986). Negative time-order errors are not restricted to tasks involving temporal stimuli, however; they are obtained with a number of stimulus dimensions and are widely viewed as evidence for processes common to a variety of perceptual judgments (Hellström, 1985).

There are different explanations for time-order effects (see Hellström, 1985, for a review), but one popular view (e.g., Jamieson & Petrusic, 1975) involves memory processes that have much in common with the subjective-shortening model. In this view, negative time-order errors result from the fact that, when the stimuli to be judged are separated in time (i.e., presented sequentially), the perception of the second stimulus must be compared against the faded memory of the first. The hypothesized changes in the remembered value of the first stimulus are akin to subjective shortening.

The present experiment was motivated by questions about the generality of the choose-

short result. We chose as our task a DSMTS procedure in which pigeons were trained to discriminate between two fixed-ratio (FR) schedules. One choice was reinforced following a small FR requirement, and the alternate choice was reinforced after a large FR requirement. This task is not difficult for pigeons to learn (e.g., Hobson, 1975; Pliskoff & Goll-diamond, 1966), and it involves a discrimination between stimuli extended over time, as with duration tasks. We were concerned primarily with the effects of delayed testing on discrimination. Specifically, we asked whether pigeons might show a tendency to respond to the small-ratio choice alternative (a "choose-small" effect) when delays were placed between ratios and choice.

Maki, Moe, and Bierley (1977) used a DSMTS task after training pigeons to discriminate between FR 1 and FR 20 ratio requirements. Accuracy decreased as a function of delay, but Maki and associates did not observe a bias to respond small with increasing delay. However, Maki et al. employed a fixed-delay procedure whereby sessions contained only a single delay value, and the choose-short effect is typically not obtained with fixed-delay procedures (Spetch & Wilkie, 1983). We tested our animals under conditions identical (except for the stimuli) to those used in the initial report by Spetch and Wilkie (1982) and assessed the effects of delayed testing on bias for the different choice alternatives. The main question asked was whether systematic biases would appear at long test delays.

METHOD

Subjects

The subjects were 4 adult Silver King pigeons maintained at 80% of their free-feeding weights. These pigeons had extensive experience (approximately 120 sessions) on a psychophysical task involving discrimination of response number. All had been trained to make different choices after completing a smaller (e.g., 10 responses) or larger (e.g., 20 responses) FR requirement. The birds experienced a series of conditions in which different FR training pairs were used; probe ratios intermediate to the values of the training stimuli were introduced under each of the conditions.

The experiment was concerned with issues related to the psychophysics of response counting¹ and is not directly relevant to the present study except insofar as the prior experience facilitated training and ease of obtaining stable performance. None of the birds had experience with delayed-discrimination procedures.

Apparatus

The experimental space was a four-key pigeon chamber. Three keys were arrayed in a row spaced 8 cm apart, about 22 cm above the chamber floor. The fourth key was located directly above the center key of the array, about 30 cm above the floor. This key remained dark and inoperative throughout the experiment. A houselight was mounted above the left key, 32 cm above the floor. The feeder opening measured 5 cm² and was located directly below the center response key; the bottom of this opening was 10 cm above the chamber floor. White noise served to mask extraneous sounds; additional masking and ventilation was provided by a fan attached to the chamber wall. A TRS MOD 1[®] computer controlled the experiment and recorded events.

Procedure

All birds had received extensive training (described above) on the basic FR discrimination task, so no special training was required. The task was a discrete-trials discrimination procedure in which the number of responses on the center key served as a conditional cue for subsequent choice responses. Trials began with the center key lit white. The sample consisted of an FR requirement on the center key. On small-sample trials, the requirement was FR 10; on large-sample trials, the requirement was FR 40. The two sample ratios occurred with equal probability and in random order. Completion of the sample FR darkened the center keylight and illuminated the side keys with red and green lights (0-s delay). The position of red and green varied randomly across trials. For 2 birds (Pigeons 31 and 33), a response to the green key was correct following the small FR and a response

to the red key was correct following the large FR. This arrangement was reversed for the other 2 birds (Pigeons 32 and 34). Correct responses turned off the keylights and produced 2-s access to mixed grain, followed by a 25-s intertrial interval (ITI) during which all lights were off. Incorrect responses darkened the choice-key stimuli and initiated the ITI directly. A noncorrection procedure was used, and no data were collected until five reinforcers had been obtained (to allow for warm-up effects). Sessions ended after 50 reinforcers had been obtained.

Subjects were trained on this 0-s delay FR discrimination task for 15 sessions, by which point discrimination performance appeared stable and asymptotic (95% correct or better for all birds during the last five sessions of training). Delay testing was begun immediately after 0-s delay training. During this phase, delays were sometimes placed between sample FRs and the choice period such that the final ratio response turned off all lights for the duration of the delay period, and the choice keylights were illuminated at the end of the delay interval. Delays of 0, 5, and 20 s were intermixed within sessions. The 0-s delay occurred on half of the trials in each session; the remaining trials contained a 5-s or 20-s delay, and each delay occurred with equal probability. Data were not recorded nor were nonzero delays arranged until five reinforcers had been obtained, and sessions ended after 50 reinforcers had been obtained. As above, a noncorrection procedure was used. The initial delay series lasted 15 sessions.

The houselight was off during trial periods, ITIs, and delays for the first 15 sessions of delay testing. Pilot studies led us to believe that the choose-small bias might be restricted to situations in which ITI and delay conditions were similar (i.e., when both were dark). We examined the possible contribution of this factor with an ABAB design in which performance was assessed over blocks of five sessions with and without houselight illumination during delays. During the first and third blocks of sessions (houselight off), the houselight was always off; during the second and fourth blocks (houselight on), the houselight was turned on at the beginning of the delay interval and remained on throughout the delay. This manipulation, presumably, made ITIs and delays

¹ Fetterman, J. G., & MacEwen, D. (1988, November). *Response counting: Both time and number matter*. Paper presented at the meetings of the Psychonomic Society, Chicago.

Fig. 1. Percentage of correct responses as a function of delay between sample ratio and choice. Each panel shows performance for a single subject. Filled circles show accuracy for the first five (of 15) sessions; unfilled circles show accuracy for the last five sessions. The horizontal lines indicate chance performance.

easily discriminable from one another. Delays of 0, 5, and 20 s were intermixed within sessions in the proportions described above. Note that, because the houselight was presented only during delays, conditions during 0-s trials were identical for the houselight-off and houselight-on conditions.

Two probe sessions were conducted after houselight testing was completed. Each probe session contained nine no-sample trials in which the choice stimuli were presented at the beginning of the trial; the center keylight did not come on and the animals did not emit a sample ratio. A response to either choice key turned off the keylights and initiated the ITI. We recorded the animals' choices on these no-sample trials, but responses were never reinforced because, of course, neither response was appropriate. Probe trials occurred every fifth trial until nine such trials had been presented; they were intermixed with 0-s delay trials in which the two sample ratios were arranged

with equal probability. Sessions ended after 50 reinforcers had been obtained.

RESULTS

Figure 1 presents accuracy data for all birds at each delay for the first and last five sessions of delay testing, which we treat as replications. Accuracy decreased to near chance level at the 5-s delay; there was very little difference in accuracy at 5-s and 20-s delays. There were no systematic differences in performance between the first and last five sessions of delay testing. A two-way analysis of variance (ANOVA) with delay and replication as the variables confirmed what the figure shows. The main effect of delay was significant, $F(2, 6) = 204.13$, $p < .05$, and post-hoc comparisons (Tukey HSD) revealed that accuracy at the 0-s delay was significantly greater than accuracy at both the 5-s and 20-s delays; accuracy

Fig. 2. Percentage of correct responses after small (triangles) and large (circles) samples as a function of delay between sample ratio and choice. Each panel shows performance for a single subject. Symbols connected by dashed lines show accuracy for the first five (of 15) sessions of delay testing; symbols connected by solid lines show accuracy for the last five sessions. The horizontal lines indicate chance performance.

scores at the 5-s and 20-s delays were not significantly different. The main effect of replications was nonsignificant, as was the interaction term.

Figure 2 shows each pigeon's accuracy scores after small (FR 10) and large (FR 40) samples at each delay value. All birds showed evidence of a choose-small bias at the 20-s delay; accuracy after small samples was clearly higher than accuracy after large samples. The same pattern is evident, but to a lesser degree, at the 5-s delay. None of the animals showed evidence of a response bias at the 0-s delay. The

choose-small effect appears to be a relatively stable phenomenon; there were no systematic differences over the first and last five sessions of delay testing.

The data in Figure 2 were analyzed with a three-way ANOVA with ratio value, delay, and replication as the variables. The main effects of delay, $F(2, 6) = 174.44$, $p < .05$, and ratio value, $F(1, 3) = 13.72$, $p < .05$, were significant; the replications were not significantly different from one another. The interaction between ratio value and delay approached significance, $F(2, 6) = 4.98$, $p < .06$;

Table 1

Sensitivity and bias measures for all birds at each delay for the first and third five-session blocks of delay testing.

	Block 1			Block 3		
	0-s delay	5-s delay	20-s delay	0-s delay	5-s delay	20-s delay
Bird 31						
log <i>d</i>	—	0.102	0.291	—	0.450	0.157
<i>A'</i>	0.990	0.603	0.703	0.996	0.821	0.638
<i>p</i> correct	0.970	0.541	0.657	0.992	0.720	0.561
log <i>b</i>	—	0.259	1.06	—	0.105	0.921
Bird 32						
log <i>d</i>	—	−0.226	−0.088	1.176	0.046	0.046
<i>A'</i>	0.957	0.136	0.386	0.966	0.550	0.550
<i>p</i> correct	0.922	0.426	0.467	0.937	0.527	0.500
log <i>b</i>	—	0.410	0.324	—	0.385	0.284
Bird 33						
log <i>d</i>	1.470	0.083	−0.063	—	−0.088	0.122
<i>A'</i>	0.977	0.587	0.421	0.996	0.387	0.621
<i>p</i> correct	0.954	0.541	0.467	0.993	0.449	0.575
log <i>b</i>	—	0.083	0.220	—	0.008	0.240
Bird 34						
log <i>d</i>	1.405	0.197	0.173	—	0.050	0.098
<i>A'</i>	0.979	0.681	0.664	0.992	0.554	0.600
<i>p</i> correct	0.960	0.594	0.573	0.986	0.523	0.555
log <i>b</i>	—	0.124	0.153	—	0.125	0.274

none of the remaining interactions approached significance. Because the interaction of ratio value and delay was very nearly significant, simple effects were analyzed. At the 0-s delay, the simple main effect of ratio value was non-significant, $F < 1$, whereas the simple main effects of ratio value were significant at both the 5-s, $F(1, 3) = 41, p < .05$, and 20-s, $F(1, 3) = 144.5, p < .05$, delays.

Figures 1 and 2 present the data in terms of percentage correct measures. Although the patterns are clear, and this format is consistent with prior research (e.g., Spetch & Wilkie, 1982), some researchers regard percentage correct measures as less than ideal because such measures confound changes in the discriminability of the stimuli with changes in response bias (e.g., see Davison & McCarthy, 1988; Davison & Tustin, 1978; Wright, 1974). Accordingly, independent measures of sensitivity and response bias were calculated for the data of Figures 1 and 2. We used the indices of Davison and Tustin (1978), which embody the traditional signal-detection theory separation between the discriminability of the stimuli and the influence of nonsensory variables (see McCarthy & White, 1987, and White &

McKenzie, 1982, for applications to delayed-discrimination tasks). The discriminability measure, log *d*, provides a bias-free measure of accuracy, and is calculated as below:

$$\log d = 0.5 \log(B_w B_z / B_x B_y), \tag{1}$$

where B_w and B_z represent the number of correct choices following small- and large-ratio samples, respectively, and B_y and B_x denote incorrect choices of the small- and large-ratio alternatives, respectively. The measure of response bias, log *b*, is calculated as below:

$$\log b = 0.5 \log(B_w B_y / B_x B_z), \tag{2}$$

where B_w, B_x, B_y , and B_z are as defined in Equation 1. For the log *d* measure, positive values indicate above-chance performance and negative values indicate below-chance performance; a value of zero represents chance performance. For the log *b* measure, positive values indicate a bias for the small-ratio alternative and negative values indicate a bias for the large-ratio alternative. A value of zero indicates no bias.

Table 1 shows the results of this analysis. Discriminability and response bias measures are shown across delays for each bird for the

first and third five-session blocks of delay testing. Log b scores are not presented for the 0-s delay data because the measure is variable when accuracy is "close to the ceiling"; small changes in performance produce disproportionately large changes in the bias measure. It does not seem appropriate to speak of bias when absolute error rates are extremely low, as at the 0-s delay, especially when measures of bias are highly sensitive to very small changes in the distribution of error frequencies. Because so few errors were made at the 0-s delay, performance was, practically speaking, unbiased. Some of the log d measures are missing at the 0-s delay because values of zero were contained in one of the error cells (B_X or B_Y), which forces a denominator of zero. To provide additional information about performance at all delays, Table 1 also shows A' , a nonparametric index of sensitivity (Grier, 1971), and the probability of a correct response, the standard measure of discrimination accuracy.

Accuracy measures decreased markedly from the 0-s to the 5-s delay. The changes from the 5-s to the 20-s delay were nonsystematic, however; accuracy measures decreased in some cases and increased in others. Comparing log d scores at the 5-s and 20-s delays, there were three instances in which the measures decreased, four in which there was an increase, and one instance of no change. The changes in response bias were more systematic. For the log b measure, in seven of eight cases there was a bias to respond small at the 5-s delay, and in six of eight comparisons the magnitude of this choose-small bias increased at the 20-s delay.

Figure 3 presents the data from no-sample trials in which the choice alternatives were presented at trial onset, such that a sample ratio was not emitted prior to a choice response. We presented 18 such trials per bird over two probe sessions. Figure 3 shows the results as a percentage of trials in which the animals responded to the small-ratio alternative; each bar shows this percentage for a single bird. All pigeons showed a strong preference for the small-ratio choice alternative; very few choices of the large-ratio alternative were made (a total of 8 out of 72 choices for all birds).

The effects of houselight illumination during delays were assessed with an ABAB design (A, houselight off; B, houselight on; see Procedure). There is, of course, substantial literature documenting the disruptive effects of

Fig. 3. Percentage of choices of the small-sample choice alternative on trials in which no prior sample was presented. Each bar shows the performance of a single subject. See text for details.

delay-interval events, such as houselight illumination, on delayed-discrimination performance (e.g., Grant & Roberts, 1976). These effects are usually interpreted as evidence for retroactive interference with memory for a prior sample event. Maki et al. (1977) obtained such effects on a task involving delayed discrimination of response number, very much like the task used here. Illumination of the houselight during delays decreased accuracy and, interestingly, produced a bias to respond to the large-ratio alternative. We assessed the influence of houselight illumination during delays with respect to both overall accuracy and response bias. Because conditions during 0-s delay trials were identical for the houselight-off and houselight-on conditions (see Procedure), data are presented only for trials with nonzero delays (i.e., 5-s and 20-s delays). Performance at the 0-s delay was comparable for the two houselight conditions, as expected.

The influence of houselight illumination on overall accuracy (A') was somewhat complex. Illumination of the houselight during delays produced a reliable decrease in accuracy, but only at the 20-s delay and only for the first determination of the houselight-on condition. The effects of houselight condition for the second determination were nonsystematic. This pattern is reflected in the results of a three-way ANOVA conducted on the A' scores with replication, delay, and houselight condition as

Fig. 4. Log b scores as a function of delay with (triangles) and without (circles) houselight illumination during the delay interval. Each panel shows performance for a single subject. Filled and unfilled symbols represent the first and second determination of a condition, respectively.

the variables. The three-way interaction was significant, $F(1, 3) = 21.62$, $p < .05$; the remaining interactions and all main effects were nonsignificant.

The more interesting and interpretable effects of the houselight condition were observed with respect to response bias. Figure 4 shows these results over the different delays and conditions in terms of the log b measure (see Equation 2 above). Positive values of log b indicate a bias to respond small whereas negative values indicate a bias to respond large. Log b scores are plotted for each subject as a function of delay for the first and second replications of

the houselight-off and houselight-on conditions.

The houselight manipulation produced systematic, reversible effects on response bias for Pigeons 31, 32, and 33. The choose-small bias was attenuated or eliminated by the houselight-on condition, but the bias returned when the houselight-off condition was reinstated and was attenuated again when the houselight was reintroduced. These effects were greatest at the 20-s delay.

The performance of Pigeon 34 was less orderly with respect to these manipulations. There was very little difference in performance

for the first determination of each houselight condition, but for the second determination, the pattern for Pigeon 34 was very similar to that seen for the other 3 birds: The choose-small effect was attenuated when the houselight was turned on during delays.

The log *b* scores were analyzed with a three-way ANOVA. The main effect of houselight condition was significant, $F(1, 3) = 16.29$, $p < .05$; the main effects of delay and replication were nonsignificant. There was a significant interaction between houselight condition and delay, $F(1, 3) = 18.35$, $p < .05$; none of the remaining interactions was significant. Post-hoc comparisons (Tukey HSD) of the two houselight conditions at each delay value showed that the houselight factor was significant only at the 20-s delay.

DISCUSSION

Pigeons trained to discriminate between small and large FR requirements emitted disproportionate numbers of responses to the small-sample alternative when delays were placed between sample ratios and choice (Figure 2) and when the choice alternatives were presented without a prior sample ratio (Figure 3). This choose-small bias is comparable to results reported by Spetch and Wilkie (1983) and others with event duration as the discriminative stimulus. The differences in performance at 0-s and 5-s delays resulted from changes in both sensitivity and response bias, whereas comparisons of performance at 5-s and 20-s delays indicated a change in response bias only (Table 1). Illumination of the houselight during delay intervals attenuated the choose-small effect by comparison to delay periods during which all lights were off (Figure 4), but delay illumination did not systematically affect overall accuracy.

We shall first consider the remarkable similarities between our results and those of Spetch and Wilkie (1982). The similarities suggest common underlying processes, but the question is how general these processes might be. Are these shared processes specific to (so-called) counting and timing tasks, or, as suggested above, do they reflect more general causal factors? In our discussion of this issue, we shall remain agnostic with respect to a formal definition of counting tasks. However, as used in

Table 2

Means (*M*) and standard deviations (*SD*) of the time (in seconds) taken to complete the small and large fixed-ratio requirements for the first and third five-session blocks of delay testing.

	Block 1		Block 3	
	Small	Large	Small	Large
Bird 31				
<i>M</i>	2.30	18.02	2.31	21.07
<i>SD</i>	1.05	4.97	1.45	5.11
Bird 32				
<i>M</i>	3.16	20.94	2.06	11.10
<i>SD</i>	2.11	5.53	0.50	3.47
Bird 33				
<i>M</i>	3.06	12.39	3.53	12.47
<i>SD</i>	1.12	2.87	0.88	3.01
Bird 34				
<i>M</i>	3.66	16.75	3.44	12.31
<i>SD</i>	0.99	3.40	0.77	2.67

this article, the term *counting* may be construed as a discrimination of numerosness ("many" vs. "few"). Davis and Memmott (1982) further restrict the term to situations involving the enumeration of individual items in an array.

Counting and timing may be mediated by the same mechanism. Both involve a discrimination between stimuli extended over time, and in many cases time and number are highly correlated with one another. With the FR discrimination, for example, ratio time varied directly with ratio value. Our subjects could have based discriminations upon the time spent responding rather than number of responses emitted (or conversely, animals could discriminate time on the basis of behavior; see Killeen & Fetterman, 1988, and Stubbs, 1979, for discussions of this issue). Table 2 provides data that bear on this point by presenting the means and standard deviations of the times taken to complete the small and large FR requirements. These data were obtained from the first five and last five sessions of delay testing, and thus correspond to the performances summarized in Figures 1 and 2. The measures represent the "run time," or the time from the first to the last ratio response. The time measures do not include the latency from trial onset until the first ratio response, primarily because the latency data were highly variable, and because

in the absence of pecking it was not possible to be certain whether the birds were attending to the sample stimulus.

It is clear from the data of Table 2 that the birds could have based discriminations on the time spent responding rather than (or in addition to) the number of responses emitted. The large (FR 40) ratio took more time to complete than the small (FR 10) ratio, and pigeons can easily discriminate time differences on the order of those shown in Table 2 (e.g., Stubbs, 1968). The ratio times were somewhat variable, as indicated by the standard deviations, so ratio time was a less reliable predictor of the correct choice than ratio value. The variability of the ratio times is potentially informative, however. The birds might have "confused" one ratio with the other when an individual ratio time was considerably shorter or longer than average. For example, subjects might have been more likely to respond inappropriately small on trials in which the ratio time was shorter than the average time for large ratio trials. Inspection of our data did not reveal any evidence for such confusions, however. Errors were related to the length of the delay interval and to ratio value, but not to the variability of the times required to produce individual ratios.

Several points should be made in connection with our analysis of temporal cues. First, the substantial difference in ratio values (FR 10 vs. FR 40) resulted in a correspondingly large difference in ratio times (see Table 2); there was very little overlap in the two distributions (e.g., large ratio times two standard deviations below the mean were, in all cases, longer than small ratio times two standard deviations above the mean). Perhaps this is why confusions were not related systematically to ratio time. Second, the data of Rilling (1967) suggest that responses mediate timing (cf. Killeen & Fetterman, 1988), the converse of the notion entertained here. Rilling trained pigeons to discriminate between fixed-ratio or fixed-interval schedules and found that number of responses was the better predictor of choice than time for both schedule types. He did not address the question of how the animals discriminated number. In our view, the matter should not be treated in an either-or way. There is ample evidence demonstrating that animals are sensitive to time *and* number cues, and it is probably the case that both contribute to per-

formance (see Fetterman, Stubbs, & Dreyfus, 1986, for a demonstration and discussion of this point).

The time data show that temporal cues could have contributed to the discrimination, but we cannot conclude that temporal cues influenced performance independently of their correlation with ratio value. This finding does not rule out the possibility that time and number discriminations have a common basis, however. If choices in the two situations are based on factors common to both tasks, we would expect manipulations such as those used in the present and related timing experiments (e.g., Spetch & Wilkie, 1982) to produce similar outcomes. Our results are in accord with this prediction. Additional support is provided by Meck and Church (1983), who found that rats bisected both time- and number-based dimensions at the geometric mean of the training values (but see Fetterman, Dreyfus, & Stubbs, 1985, and Fetterman et al., 1986, for different results with response counting and stimulus counting tasks), and that methamphetamine produced comparable shifts in psychometric functions for time and number. They took these results as evidence that performance in both situations was mediated by a single internal clock mechanism (see also Meck, Church, & Gibbon, 1985, for a similar conclusion).

Honig and Spetch (1988) trained pigeons to discriminate between two different rates of alternation (fast vs. slow), a task that incorporates features of both time- and number-discrimination procedures. When delays were placed between samples and choice, their birds showed a bias to respond to the alternative correlated with the fast-rate sample (a "choose-fast" effect; see Meck, Church, & Olton, 1984, for other similarities between duration and rate discrimination tasks). The similarities in results from duration, number, and rate discrimination procedures provide a *prima facie* case for common mechanisms.

We should also consider the possibility that these results tell us something about delayed stimulus control that is not restricted to timing, counting, and rate discrimination tasks. Perhaps, as with time-order errors, these results reveal something more general about delayed stimulus control by prothetic dimensions. Kraemer et al. (1985) offered an explanation of the choose-short effect that could be applied to a variety of discrimination tasks. Their ex-

planation holds that durations are "labeled" immediately after their termination, and that the labels are forgotten during the delay. These labels might be overt (e.g., Blough, 1959) or covert (e.g., Roitblat, 1982), but they bear an arbitrary relation to the stimuli to be discriminated. The tendency to respond short results from generalization whereby the absence of information about a prior sample is deemed more similar to the short than to the long duration "code." Because this process involves a transformation of the nominal stimulus, this account should be applicable to a variety of discrimination tasks. For example, animals trained to discriminate the loudness of two sounds and then tested with a DSMTS procedure should "choose soft," a result similar to what has been observed in studies of comparative judgments of loudness with humans (Hellström, 1978).

We have been struck by the similarity between "biased-forgetting" effects and the time-order errors of human psychophysics (Hellström, 1985), which are obtained under a variety of circumstances. Our results suggest that biased forgetting is not restricted to duration tasks, but whether its generality is comparable to that of time-order effects remains to be established. One approach to this question would be to carry out DSMTS experiments with different stimulus dimensions (e.g., sound or light intensity) and observe whether biases similar to those found with duration and number tasks occur. Another approach involves manipulating factors known to influence time-order errors using DSMTS procedures. For example, stimulus magnitude is known to influence the direction of time-order errors; most often, time-order errors are positive for low levels of stimulation and negative for high levels (e.g., Jamieson & Petrusic, 1975), a result that has recently been replicated with pigeons (Dreyfus, Fetterman, Smith, & Stubbs, 1988). In the context of DSMTS with duration stimuli, we would expect animals to choose long when the durations were short (e.g., 0.5 vs. 1.0 s) but to choose short when durations were long, as in the experiment of Spetch and Wilkie (1982).

Biased forgetting may indeed reflect the operation of underlying cognitive mechanisms, as implied by subjective-shortening and coding accounts, but other factors need to be considered. One such factor is the distribution of

reinforcers for the different choice responses. Reinforcers were arranged for each correct response; with this procedure, any bias that develops alters relative reinforcement for the two choices. When, for example, pigeons choose small more often than large, small responses are reinforced more often. Relative reinforcement rates affect response bias in duration discrimination (e.g., Stubbs, 1976) and other tasks and could contribute to the observed effects.

Although we were aware of this positive-feedback relation, we elected not to use a controlled-reinforcer procedure because we wanted to replicate the essential features of the Spetch and Wilkie (1982) experiment. The question is one of cause and effect. Biased forgetting might be fundamental and alter choices with changes in relative reinforcement as the outcome. Or, some temporary disruption favoring small responses might alter relative reinforcement rates, which would then maintain a lasting change in response bias. We favor the former interpretation for the following reasons: First, the changes in relative reinforcement rate that resulted from the choose-small bias were fairly small. Relative reinforcement rates computed with respect to the small-ratio choice alternative and averaged across the first and third five-session blocks of delay testing were .58, .60, .48, and .52 for Pigeons 31, 32, 33, and 34, respectively. Second, the biased-forgetting phenomenon appears quite robust. An account based solely upon obtained reinforcement does not explain the consistency of preferences for short and small from the very beginning of delay testing. Third, this reinforcement account does not explain the no-sample result (Figure 3) because no-sample choices were never reinforced. In sum, we do not believe these effects can be attributed solely to reinforcer bias, but we also believe that this and related experiments (e.g., Spetch & Wilkie, 1982) should be repeated with controlled-reinforcer procedures.

We have thus far considered reasons why biased forgetting occurs in delayed discriminations of stimulus duration, stimulus rate, and response number. The conditions under which biased forgetting does *not* occur are equally informative, however, and could provide insight into causal mechanisms. One such factor is the manner in which delays are arranged. Spetch and Wilkie (1983) established that a stable choose-short effect obtains only

when different delays are intermixed *within* sessions (the so-called variable-delay procedure). When but a single delay is used (fixed-delay procedure), the choose-short effect diminishes with repeated exposure to a given delay (Spetch & Wilkie, 1983; Experiment 4); typically, the effect "washes out" within three sessions. Similarly, comparing our results with those of Maki et al. (1977), it appears likely that the fixed- versus variable-delay distinction could account for the discrepant findings (discussed above). Other studies have also found the scheduling of delays in delayed-discrimination tasks to be a relevant variable. White and Bunnell-McKenzie (1985) compared DMTS performance under fixed- and variable-delay procedures and found that matching accuracy (measured as $\log d$) was higher overall with variable delays (see also Carter & Werner, 1978, and Honig, 1987, for similar results).

How might the scheduling of delays influence response bias? Assume that the memory of each sample (small/short and large/long) fades with increasing delay; these changes in remembered magnitude might be hyperbolic (e.g., Harnett, McCarthy, & Davison, 1984) or exponential (e.g., McCarthy & White, 1987). Assume further that each of the decremented magnitudes is compared against a criterion value established in reference memory (Honig, 1978) during training. With the fixed-delay procedure, the criterion could be established in such a way that choices would be unbiased. With the variable-delay procedure, however, the criterion would represent a conflation of the appropriate criteria at each of the variable delays. Such an arrangement would result in a bias to respond small at long delays, and, depending on how the composite criterion was formed, there could be a choose-large effect at short delays. The latter (choose-large) effect was not observed in the present experiment, however, probably because of the high proportion (.50) of 0-s delay trials included within sessions. This criterion hypothesis predicts that the distribution of delays would influence performance. For instance, increasing the probability of a long delay should eliminate the choose-small/short effect.

A second boundary condition on the biased-forgetting phenomenon is indicated by the influence of the houselight manipulation. Illumination of the houselight during delays did

not produce a consistent decrement in accuracy, but did attenuate or eliminate the choose-small bias. We were somewhat surprised that houselight illumination did not consistently affect accuracy, because this result is contrary to a substantial literature (e.g., Grant & Roberts, 1976). Furthermore, Maki et al. (1977) used a task very much like ours and found that houselight illumination during delays impaired delayed discriminations of response number. There may be a simple explanation for the discrepancy, however. Inspection of Figure 1 and Table 1 reveals that accuracy was close to chance at the 5-s and 20-s delays without the houselight, so the birds could not do much worse when the houselight was turned on. This floor effect probably accounts for the discrepant finding.

The differences in bias with and without houselight illumination are potentially more interesting, and suggest at least one very simple account of the biased-forgetting phenomenon. M. L. Spetch (personal communication, May 1988) obtained similar results with an event-duration task, and Santi (1984) has shown that the commonly observed relation between accuracy and the $\log$ ITI/delay ratio with DMTS obtains only when both delays and ITIs are dark. One explanation (suggested by M. L. Spetch) for the lack of bias in the houselight-on condition is that the added illumination disrupts the discrimination such that the birds respond randomly instead of basing choices on the (distorted) memory of the prior sample. Such disruption would not be evident in overall accuracy measures because overall accuracy was close to chance in both the houselight-off and houselight-on conditions.

A second interpretation is that the birds confused ITIs and delays, and that choose-short and choose-small effects are obtained only when ITIs and delays are potentially confusing, as in the houselight-off condition, in which all lights were off during both intervals (or, presumably, also if the houselight was on during ITIs and delays are potentially confusable, as result from the temporal similarity of the 25-s ITI and the 20-s delay intervals; animals may sometimes become disoriented with respect to their location in the stream of experimental events. This hypothesis could account for the no-sample result; our subjects might have confused choices offered at the end of the 25-s ITI (i.e., at the start of the trial) with those offered

after a 20-s delay and, as observed, responded small in both instances. This interpretation is supported by the finding that houselight illumination attenuated bias only at the 20-s delay (i.e., the significant interaction of houselight condition and delay). Maki et al. (1977) offered a similar explanation for their houselight manipulation, which produced a bias to respond large, except that their account focused on the potential confusion of delays and trial periods, not ITIs (see Maki et al. for details).

Our confusion hypothesis is very speculative, but it is clear that we need to pay close attention to contextual factors such as those discussed here. It is also clear that we need to consider our procedures from the subject's point of view; our formal task descriptions may not always mesh with the subject's perceptions of the task structure, a point often overlooked due to anthropocentric biases.

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