Pitfalls of Organismic Concepts: "Learned Laziness"?

Engberg *et al.* (*1*) reported performance differences among three groups of pigeons in an autoshaping situation. The important variable was the prior experience these groups had received: One group learned to press a treadle for grain reinforcement before autoshaping began (treadle group); a second group received an equal number of reinforcements on a noncontingent basis (hopper group); and a third group effectively received no prior treatment (control group). The hopper group required the greatest median number of trials to reach a learning criterion; the control group, an intermediate number; and the treadle group, the smallest number. An analysis of variance revealed that the three groups were not drawn from the same population. The authors discussed their findings in terms of "learned laziness" and possibly learned "industriousness." We wish to comment on two aspects of this report.

Let us first consider the findings themselves. Although reliable differences among the three groups were reported, pairwise comparisons based on the authors' published data were disappointing. The small difference between the treadle group and the control group was not significant (Mann-Whitney $U = 23$, $P > .10$; $t = 0.862$, $P > .20$). We feel that the number of subjects involved in this study is sufficiently large to reveal major effects; our intuitive scan of the data—independent of formal statistical analysis—does not lead us to believe that the data were suggestive of a real effect. Thus, the question of whether training on an operant prior to autoshaping has any facilitative effect on autoshaped key-pecking still seems empirically open. The other effect—reduction in key-pecking performance owing to a history of noncontingent reinforcement—was only marginally significant (Mann-Whitney $U = 20$, $P > .06$; $t = 0.35$, $P > .30$). Since an impairment effect was previously reported by us (*2*), using somewhat similar procedures, we feel that the empirical status of this effect is considerably more secure.

Further empirical analysis must be carried out, however, before the effect can provide a clear basis for inference of the sort made by Engberg *et al.* It has been traditional in quantitative studies of learning to distinguish between rate of learning (for instance, the rate parameter of a growth function) and the asymptote of performance ultimately reached. We reported (*2*) that prior experience with noncontingent reinforcement produced low asymptotic rates of autoshaped pecking. So far as we can determine with our sample size (*3*), there is no effect on the rate of approach to the asymptote of performance. It appears that Engberg *et al.* believe that their effect represents an influence on the process of acquisition itself ("the hopper birds responded less because they anticipated a continuation of the noncontingency that existed in their first stage of training"). Data pertaining to "trials to criterion" cannot unambiguously support such an inference, since they confound rate of learning and asymptote of performance. (Relative to the performance of a control group, an increased number of trials to criterion might reflect either a slower rate of approach to the same asymptote, or the same rate of approach to a lower asymptotic value, or both.)

The dismissal of a competing response explanation of the results was likewise overstated. According to Engberg *et al.*, "the treadle group was explicitly trained to make a response that could compete with key-pecking." Stimulus control over this "competing" behavior must have been governed by the presence of the treadle. Yet the treadle was removed before the autoshaping sessions, thus removing the major source of stimulus control over the "competing" behavior. On the other hand, whatever competing behavior may have developed in the hopper group could continue undisturbed during the autoshaping sessions. Consequently, the data of the treadle group cannot be taken as evidence for or against a competing response explanation of the main effect. Moreover, while we agree with Engberg *et al.* that development of an ordinary (arbitrary) superstition is unlikely in their situation, we feel that demonstration of the systematic development of "interim" and "terminal" nonarbitrary activities in situations of this sort (*4*) provides a rational basis for considering a response-oriented, behavioristic interpretation. Thus, the results reported by Engberg *et al.* are consistent with many interpretations, and provide clear support to none.

Our second observation concerns the nature of the formulation Engberg *et al.* have proposed. The data from which their formulation arises were obtained from a "transfer of training" paradigm. We raise the issue of how transfer effects are most usefully characterized. Terms such as learned "laziness" and "industriousness" refer to characteristics induced in the organism, and not to particular alteration of a behavioral repertoire. The difference between these two characterizations can be easily illustrated.

A specific prior learning experience can have effects on subsequent test performance that vary from facilitative to detrimental. The magnitude and sign of such effects is a function of the similarity between stimuli and responses in the training and test situation (*5*). Thus a single "treatment" can facilitate performance on test A and be detrimental on test B. While this is easily encompassed in a behavioral framework based on the degree of similarity between stimuli and responses, it is embarrassing for an organismic approach. The treatment appears to induce both "learned brilliance" and "learned stupidity."

In the case of the "learned helplessness phenomenon" (*6*), the possibility of specific transfer effects due to response learning was examined experimentally and rejected (*7*), and it may be that the "helplessness" effects reported in dogs do in fact reflect an altered state of the organism. Engberg *et al.* did not report comparable tests, and their generalization therefore seems premature.

In shifting to inferences about the state of the organism, the experimental burden is expanded in still another way, because the question of what is learned must be addressed at a level of analysis far removed from the specifics of behavior. Thus, in discussing the logic of the concept of "helplessness," Rachlin (*8*) pointed out that the designation "learned omnipotence" might be applied with equal propriety, on the basis of experimental evidence. A similar point can be made here: Perhaps the pigeon has learned to anticipate that anything he does may produce food ("omnipotence") rather than to anticipate "a continuation of the noncontingency" by which action has no consequences at all ("learned laziness"). We do not wish to take a position on the relative merits of these two interpretations—we simply wish to emphasize that organismic concepts require a far more extended experimental and theoretical program than behavioristic concepts do, if appropriate standards of scientific inference are to be maintained.

Because of the greater demands of

establishing such concepts, considerable care is required in the naming of them. We feel that Engberg *et al.* have been hasty in using the term laziness, which has more than one connotation in the English language. Since Engberg *et al.* did not define "learned laziness," the term could be used in a number of different ways. In order to examine the applicability of the term to the data, let us assume that "laziness" is "resistance to exertion" (*9*) since Engberg *et al.* claim that the subjects in the hopper group learned 'to not respond." To the contrary, our experience with pigeons in similar situations is that they are very active even when not key-pecking and seldom in a quiescent state. Second, all the pigeons peck at the key, despite the fact that reinforcement is delivered for "doing nothing." Is this laziness? Or are we simply overlooking large segments of behavior by concentrating on a specific response class? Finally, if "laziness" is readily learned, why do rats (*10*) and pigeons (*11*) seem to prefer to respond for food rather than eat equally available "free food" in some conditions?

Whatever the ultimate explanation of the data of Engberg *et al.*, their use of an undefined term with obvious moralistic connotations was unfortunate. It is almost an invitation to overextrapolation to social systems by legislators, nonexperts, and indeed by the authors themselves (*12*).

Engberg *et al.* point out the possibility that animals can learn about response-reinforcer contingencies and show positive or negative transfer on this basis. This is obviously of great interest, as is the possibility that certain kinds of experience have general effects on the organism over and above those reflected in the behavioral repertoire. A detailed study of these possibilities, and of the laws by which they develop, would be an important contribution. We hope this comment has contributed to the sound exploration of such possibilities by helping to clarify the nature of the issues and pitfalls involved, and by pointing out some of the requirements for their effective experimental analysis. The use of organismic traits (laziness, industriousness) has a long and unhappy history in psychology, and has been bedeviled by difficulties of measurement and operational definition in the usual human context. It is possible that behavioral models based on animal experimentation will prove useful in developing and exploring phenomena in this realm. If animal models are to succeed, however, it seems likely that the possibilities of careful definition and complex experimental analysis will lie at the root of their contribution. Work in this direction is relatively new, and it seems timely to ask that it proceed with the experimental caution already characteristic of the simpler and more directly behavioral levels.

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12. An earlier version of the report by Engberg *et al.* (*1*) was presented by D. R. Thomas at the 13th annual meeting of the Psychonomic Society, November 1972, in St. Louis, Missouri, under the title "The pigeon in a welfare state."
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Gamzu *et al.* (*1*) have criticized our report (*2*) with regard to procedural characteristics, interpretation of the results, and "moralistic connotations" of the interpretation. They have raised some important issues. We shall first consider specifics of their criticisms of our experiment and its analysis.

Gamzu *et al.* dispute our use of a criterion measure, requiring at least one response in eight out of ten successive key illuminations, on the grounds that it confounds growth parameters to asymptotic performance and absolute response rates at asymptote. The level of responding required to meet our criterion was selected to assure that key-pecking was reliably established; however, it fell far short of asymptotic response rates in this experimental situation. It would be interesting to determine whether the number of trials necessary for the occurrence of reliable key-pecking in autoshaping is related to asymptotic parameters of performance, but this question remains open to empirical investigation.

With regard to the statistical evaluation of our results by Gamzu *et al.*, we feel that their use of *t*-tests is unjustified in view of the skewness of the scores in each group. An overall Kruskal-Wallace analysis of variance was appropriate, particularly since the three groups were ordered in the predicted direction. Certainly, the evidence is stronger for interference with performance in the hopper group than for facilitation in the treadle group. The major significance of the performance of the treadle group may be that one might expect maximal response competition and thus poorest performance from this group, but this clearly did not happen.

Gamzu *et al.* claim to have previously demonstrated an impairment effect using "somewhat similar procedures" to those employed with our hopper group. The conditions under which they observed low asymptotic rates of autoshaped pecking (*3, 4*) are not comparable to the ones under which we observed retarded initiation of key-pecking. Their "noncontingent reinforcement" (*1*) condition consisted of "nondifferential" (*3, 4*) pairings of the key stimuli and grain deliveries. Thus, their subjects had the opportunity to learn something about the lack of a temporal relation between presentations of key stimuli and grain. Indeed, Gamzu *et al.* (*4*) speculate that "autoshaping depends on the *informativeness* of key illuminations with respect to reinforcement" (*4*, p. 321). Our hopper group was not exposed to key illuminations during noncontingent initial training, a crucial distinction. Consequently, any learning in this group must have been restricted to the relation (or lack of one) between their behavior and grain deliveries. When birds were placed in the autoshaping condition, the "information" conveyed by key illuminations with respect to grain deliveries was operationally equal for all three experimental groups in our study.

Gamzu *et al.* reject our "organismic" (that is, cognitive) interpretation of our data, invoking instead the principles of "response competition" and stimulus and response similarity in transfer of training. First of all, the treadle group was explicitly trained to make a response that could compete with key-pecking. Gamzu *et al.* (*1*) argue that removal of the treadle during autoshaping most likely eliminated the source of stimulus control over treadle-pressing, and, as a consequence, the explicitly conditioned competing responses (treadle-pressing movements and body orientations toward the previous treadle location) were no longer probable during autoshaping. However, such responses occasionally were observed in autoshape training, but the treadle group acquired key-pecking most rapidly.

Gamzu *et al.* suggest that superstitious behavior may have developed in our hopper group which would persist into autoshape training and interfere with key-peck acquisition. They refer to a most informative paper by Staddon and Simmelhag (*5*), who reported "nonarbitrary" superstitious behaviors developing under conditions similar to those employed in initial training for our hopper group. The response topographies most frequently occurring in "terminal" superstitious responding were facing and pecking at the magazine wall. It seems reasonable to expect that such behaviors would facilitate rather than impede the acquisition of key-pecking, as the pigeons would have been positioned so as to receive maximal visual stimulation by the key illuminations and would already be experienced with pecking at objects other than grain in the general locale of the response key. The hopper group, however, was slowest in acquiring key-pecking.

We agree with Gamzu *et al.* that ours was a transfer design and that the role of stimulus and response similarity should be considered. Theoretically, maximal positive transfer should occur when both stimuli and responses in pretraining and test situations are highly similar (*6*). If we assume that our hopper subjects acquired superstitious behaviors similar to those observed by Staddon and Simmelhag, then the condition of high stimulus and response similarity was more closely approximated for the hopper than for the treadle group. In the latter, an important stimulus element (the treadle) was removed, and the measured response of key-pecking was very different from that explicitly trained in the previous stage. The fact that the treadle group performed the best and the hopper group the worst suggests to us that a peripheralistic stimulus-response transfer analysis of these results is necessarily inadequate. In addition, stimulus and response similarity are typically evoked analytically under experimental conditions in which contingencies of reinforcement are similar for the two stages of a transfer design. Transfer from a condition of explicitly defined contingencies to one of no experimenter-defined contingency in our treadle group obviously complicates application of this interpretation.

The principal difficulty with response competition interpretations is that competing responses are often invoked post hoc and neither observed nor measured. As such they can "account for" facilitation or impairment in transfer tasks and still have no predictive value. In the absence of an account of the compatibility relations among response topographies in the pigeon, the concept of competing responses does not seem a priori preferable to those of "learned laziness" or "industriousness." The preference of Gamzu *et al.* for a response competition interpretation seems to reflect a peripheralistic ideological commitment and is not compelled by the data.

With regard to the use of the term "learned laziness," it was our intention to emphasize the relation between our results and those designated "learned helplessness" (*7*). The term was used in a descriptive sense only. Further, our interpretation does not require additional concepts such as "learned brilliance" and "learned stupidity." We assume that both the hopper and treadle groups learned something about the relation between their behavior and grain deliveries. The hopper group learned that grain deliveries were not dependent on their behavior, whereas the treadle group learned that food delivery was dependent upon their behavior. Thus the groups differed in what was learned about contingencies in initial training and in the influence of this learning upon initiation of the key-peck response in autoshaping.

The issue of the moralistic connotations of our use of language and the social relevance of our work is a fitting place in which to conclude. We recognize the danger of "overextrapolation" of basic research but also acknowledge its opposite, the aloof unwillingness of certain scientists to even consider the possible practical applications of their work. Whether a particular application is premature or overzealous is ultimately subject to the same rules of empirical verification as is the basic research itself. We have made no claims for the applicability of our findings to human social systems. We feel, however, that there are times in which scientists should be encouraged to seek appropriate extrapolations of their basic research, rather than being discouraged from doing so.

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