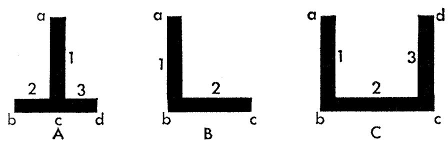

Fig. 1. Sample (*A*) and test figures (*B* and *C*). Lower-case letters identify end points and points of intersection. Numbers identify lines and were used only in the modified instructions condition—that is, group *M*.

Our guess that Hart's original procedure biased the subject against the reporting of line disappearances was clearly supported. For both test figures (whether the lines were labeled with numbers or letters), the modified instructions almost doubled the proportion of lines reported: the differences were significant ($p < .01$) for the comparison of group *M* both with group *R* and with Hart's original data.

The findings concerning point disappearances are not so easily interpreted, however. We were unable to reproduce Hart's data indicating a preponderance of fixation-point disappearances, even in group *R*, where every effort was made to duplicate his procedure. Group *R* and group *M* differed significantly ($p < .01$) from the original Hart data in the mean proportions for both fixation points and nonfixation points, and the order of these statistically reliable differences was comparable, once again, for both test figures. Instead of the preponderance of fixation-point disappearances found by Hart, our data suggest

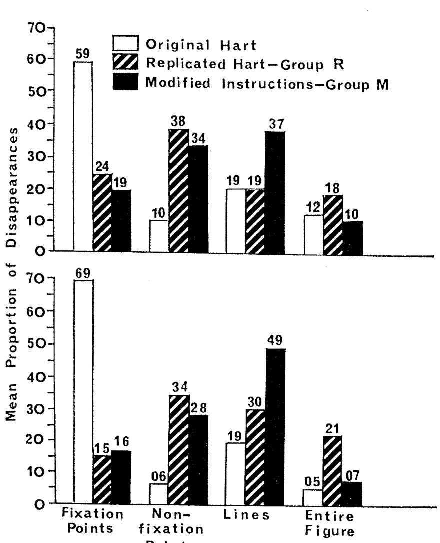

Fig. 2. The upper graph shows the mean proportions of disappearances reported by subjects viewing test figure *B*. The lower graph shows the same comparisons for test figure *C*. Decimal points have been omitted. For Hart's original data, see (*4*).

a relatively greater proportion of disappearances of nonfixation points. In group *M*, for instance, the mean proportion of fixation-point disappearances was only 0.19 for figure *B* and 0.16 for figure *C*.

The same pattern of results was obtained when the reported dimmings (rather than disappearances) were analyzed. In no category were the mean proportions for dimmings significantly different from those for disappearances.

Hart specifically noted that none of his subjects reported the disappearance of point *d*, one which was never fixated. In the present study, however, 7 of 12 subjects in group *R* and 15 of the 24 subjects in group *M* reported at least one disappearance of that point.

Even though our data for group *M* do not reveal a preponderance of fixation-point disappearances, a fixation-point effect is evident when only the disappearances of points *a*, *b*, and *c* are considered. When the proportion of fixation-point disappearances was calculated for each subject, only these three points being considered, the mean proportions were 0.54 for figure *B* and 0.60 for *C*. Both proportions are significantly greater ($p < .01$) than 0.33, the expected proportion under the assumption that the fixation point does not affect disappearances.

Our data also differed from Hart's in the frequency of reported disappearances. Considering only the total number of disappearances reported by each subject, the means for our replication were 17 for figure *B* and 20 for *C*, whereas the respective values calculated on Hart's original data were 35 and 30. Only if the reports of dimmings are pooled with those of disappearances do the values for group *R* compare to those of Hart. However, Hart's preponderance of fixation-point disappearances can not be related in any simple way to this difference in frequency, since the comparable values for our group *M* were 71 and 72, more than twice the values Hart obtained. Obviously, the modified instructions not only increased the proportion of lines reported, but also significantly ($p < .01$) increased the overall rate of reporting as well.

In general, our evidence supports McKinney (*5*) and Clarke and Evans (*6*) who suggested the possibility of two kinds of fragmentation: point-of-fixation disappearances related to receptor distribution on the retina, and more structured fragmentations having a central neural basis. Fixation-point disappearances are not the most likely to be reported, however, unless the experimenter inadvertently induces a response bias. Consequently, the fragmentation problem should continue to have important implications for current theories of perceptual organization.

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7. We thank J. T. Hart for his prompt and full reply to our requests for detailed information about his study and both Hart and J. P. McKinney for their suggestions concerning this report. This research was supported by grant GS-343 to Timothy C. Brock from NSF.

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Discriminative Avoidance Training of Rats

Hurwitz [*Science* **145**, 1070 (1964)] compares two methods for training rats to avoid shock in a lever-pressing apparatus. In both methods shock could be avoided by pressing the lever during the 7.5-sec interval between the onset of a light signal (conditioned stimulus) and the beginning of shock (unconditioned stimulus). In the first method failure to avoid resulted in a train of 0.2-sec shocks spaced at intervals averaging 13 sec. This shock was described as inescapable because a response made after the train of shocks had begun did not terminate a shock pulse if one happened to be on at the time, although it did terminate the series and turn off the light. In the second method, failure to avoid resulted in a continuous shock which had a maximum duration of 10 sec. This shock was described as escapable because a response during the shock terminated it and the light simultaneously. Hurwitz presents convincing evidence that the rats learned to avoid the inescapable but not the escapable shock. In attributing this result to the difference in escape contingency, the author contradicts the findings of many early experiments in which responses other than lever pressing were used. Assuming that lever pressing is not unique among instrumental responses, we must conclude that the interpretation either of his data or of the earlier experiments is erroneous.

I would choose the former alternative, because Hurwitz has probably confounded two important variables with the response contingency factor. The first of these is intertrial interval—the time between successive presentations of the conditioned stimulus. In his inescapable shock procedure the intertrial interval has no upper limit, whereas for the escapable shock procedure it has an average maximum of 30.5 sec. It is likely that animals trained with the inescapable shock receive many shocks per trial during the early training sessions. If as few as three shocks are delivered, the intertrial interval is sufficiently longer than the *maximum* interval in the escapable-shock situation that learning, as measured by relative frequency of avoidance, will be facilitated.

Also confounded with the response contingency is the average shock duration per trial. My experience is that, when escapable shock is used, rats reduce shock duration to a minimum determined by their reflex reaction time. Typically, they hover over the lever, usually touching it with the forepaws, throughout the intertrial interval and certainly during the interval between conditioned and unconditioned stimuli. At shock onset a whole-body contraction occurs with very short latency; this, of course, depresses the lever and terminates shock. We in this laboratory also failed to obtain avoidance learning with escapable shock, until we increased shock duration to an imposed minimum of 0.25 to 0.50 sec. Shock termination was still contingent on the response, but an immediate bar press resulted in a brief delay of shock termination, whereas a response with latency greater than the minimum resulted in immediate shock termination. Within the first few hours of training under these conditions all animals learned well to avoid shock.

In the Hurwitz experiment, therefore, it is highly probable that average shock duration and average intertrial interval are both greater with inescapable than with escapable shock. The effect of this difference in each variable is to increase rate of avoidance learning in the inescapable shock group. Hurwitz's conclusion that response contingency is the controlling factor is unwarranted unless he can show that the two procedures did not differ in intertrial interval and shock duration.

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There is no evidence from my data that rats trained under an inescapable procedure received more shock than those trained by the traditional escape-avoidance method. Brush, however, cites evidence that it is the increased shock duration which facilitates avoidance learning. His procedure of imposing a fixed minimum of 0.25- to 0.50-sec shock may have increased average shock duration somewhat, but at the same time it set up a schedule equivalent to mine, where shock was inescapable. Thus Brush's experiment adds further support to my response-contingency interpretation. Recent work by D'Amato, Keller, and Biderman (personal communication) suggests that increasing the duration of *inescapable* shock by small amounts may hinder rather than facilitate discriminative avoidance learning.

Since the object of the procedure is to establish the signal as a discriminative stimulus for lever pressing, my assumption is that a brief shock which is sufficient to elicit lever pressing would be more effective than one of longer duration. It would ensure that lever pressing elicited by shock would take place in the presence of the signal only and terminate it. A longer inescapable shock, again eliciting lever pressing, may retard avoidance learning, because now lever pressing is occasionally punished.

Brush's argument that the inescapable shock condition results in longer intertrial intervals which tend to facilitate avoidance learning would be true only where the intertrial interval is defined as the period from signal onset to signal onset, as in classical conditioning experiments. However, if the term is used to refer to the interval between termination of a signal (whether response-produced or terminated by the experimenter) and its subsequent presentation, as it is sometimes used in instrumental learning experiments, the two groups were treated equally; in both situations this interval averaged 13 sec. In any case, the range of difference, given the first definition, would not be sufficient to account for such dramatic differences in performance. At most it would result in different rates of avoidance learning, not in different asymptotes of the learning curve.

If a rat is placed in a shuttle box and exposed to a discriminative-avoidance procedure, it will readily learn to avoid the onset of the noxious stimulus. But when the rat is placed in a lever-pressing apparatus under the same experimental contingencies as seem to prevail in the shuttle box, it will rarely avoid but only escape. These facts are not in dispute, but the problem of reconciling differences remains. Intertrial interval and shock duration are weak candidates, and response contingency, as hinted at in my procedural note, is not an exhaustive explanation either. The explanation would seem to lie in the fact that lever pressing (together with some other response classes) differs in that the response has a temporal dimension, whereas crossing from one grid to the other is defined in terms of a change in location and for practical purposes has no temporal properties. The rat in the lever-pressing apparatus during the escape-avoidance training procedure soon learns both to press the lever and to hold it. If the lever is held for too long, so that it coincides with the onset of the signal and the subsequent presentation of the shock, as often happens, lever holding is punished. The animal now learns not to handle the lever when the signal is on, but to confine its lever-pressing responses to shock onset where a press is reinforced by shock termination: only escape learning takes place. These suggested response-contingency factors have the advantage of being testable.

A careful analysis of the rat's reactions during the early trials under a discriminative escape-avoidance procedure, particularly its lever-holding behavior and the rate at which it learns to perform escape responses, may yield fruitful results. Alternatively, there are a number of ways of programming the signal-shock and the response contingencies so as to produce increased escape or avoidance responding in both the lever-pressing and the shuttle-box apparatus. This would throw light on the feasibility of the explanation somewhat sketchily offered here.

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