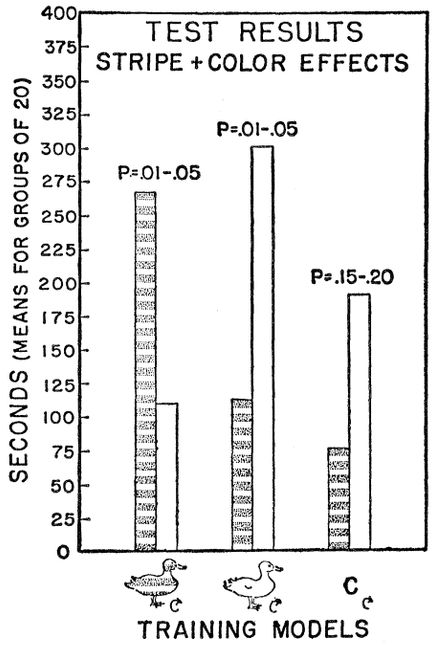

Fig. 4. Interaction of color and stripedness in imprinted preferences.

inal training model, that is, they showed "imprinting." This occurred with the crosshatched and plain, the red and yellow, and again with the red striped and plain white models. And in the third category the ducklings preferred one particular model irrespective of their training model, though the training experience was a necessary prerequisite to this preference. This was similar to our original result, here replicated, with the varicolored and white model.

We do not yet know whether a pair of nondiscriminated models would also be equivalent in a more conventional associative-learning task. It is nonetheless clear that the discriminability or nondiscriminability of the training model is unrelated to its "conspicuousness" in the sense of Bateson (*2*), that is, in the number or disposition of stripes. Nor is it simply related to color: red was not distinguished from white, though red and yellow were distinguished. According to Schaeffer and Hess (*3*), Vantress chicks greatly prefer red over yellow, and white least of all. An interaction is discernible in that the red-striped model was distinguished from a white model, while this was not the case for either an entirely red or black-striped model. This would also seem to preclude flicker effects as the relevant cues.

Let us first assume that models treated as equivalent (the first category) are merely not perceived as different, or only so perceived with difficulty; that is, in a trained discrimination task the ducklings either would never achieve criterion or require substantially more trials to do so than when given two models to which imprinting does occur. If the ducklings do not easily detect black stripes on a white field and only barely distinguish red from white, the results of categories one and two require no separate explanation. However, the results of the third category remain as great an enigma as ever.

We can also assume that the ducklings are, in fact, capable of making learned discriminations as easily between any one pair of models as another. The results of categories one and two then suggest that imprinting is a selective process; that is, it can occur only to certain kinds of models. These models presumably can be ordered along a continuum from those to which imprinting simply cannot occur (category one) through those to which imprinting can occur with equal ease (category two), to those for which an immanent preference exists the expression of which is tied to the imprinting experience (category three). The interesting results of Gottlieb (*5*), who examined preferences for and imprinting to acoustic stimuli, also lend themselves to such a scheme.

These results require that students of imprinting devote at least some of their energies to a more thorough analysis of the characteristics of their imprinting surrogates, as perceived by their subjects, as well as studies of their discriminability under other conditions. The phenomenon of imprinting cannot be elicited with any set of stimuli. Our present results suggest imprinting is far more complex a process than originally suggested by the *tabula rasa* model.

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References and Notes

1. P. H. Klopfer and J. P. Hailman, *Z. Tierpsychol.* **21**, 755 (1964); ———, *Science* **145**, 1333 (1964); P. H. Klopfer, *ibid.* **147**, 302 (1965); ———, *Behav. Sci.* **12**, 122 (1967).
2. P. P. G. Bateson, *Biol. Rev.* **41**, 177 (1966).
3. H. H. Schaefer and E. H. Hess, *Z. Tierpsychol.* **16**, 161 (1959).
4. M. W. Tate and R. C. Clelland. *Nonparametric and Shortcut Statistics* (Interstate, Danville, Ill., 1957).
5. G. Gottlieb, *J. Comp. Physiol. Psych.* **59**, 345 (1965).
6. I thank J. Potter and C. Dewey for assistance. Supported by NIH grant No. MH04453 and a career development award to P.H.K.

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Permanence of Retrograde Amnesia Produced by Electroconvulsive Shock

Reappearance of an avoidance response previously abolished or attenuated by electroconvulsive shock (ECS) has been demonstrated by Zinkin and Miller (*1*) with repeated testing of convulsed animals. The authors therefore question the permanence of ECS-produced retrograde amnesia.

Although few have explicitly tested the assumption of the permanence of amnesia, Zinkin and Miller appear to have examined only the effect of repeated exposures to the experimental situation on retrograde amnesia, and therefore they have not directly tested the permanence of the phenomenon. To directly examine the stability of ECS-produced amnesia, repeatedly tested, convulsed animals must be compared with subjects that are tested for the first time at an interval (after ECS) equal to that at which the repeatedly tested animals receive their last test (in Zinkin and Miller's study this would be at 72 hours, when the repeatedly tested subjects evidenced apparent recovery from amnesia). In this way the permanence of interference with memory can be separated from the performance effects of repeated exposure to the situation in which the original learning took place.

We administered ECS to groups of mice within 75 seconds of a single appetitive or aversive learning trial in the same apparatus, a chamber with a culde-sac into which a mouse could poke its head to receive either water or shock from a drinking cup (*2*). Half the animals were tested repeatedly; the amnesia which they evidenced on the first test day, 24 hours after reinforcement and ECS, had largely disappeared on two subsequent retention tests (at 48 and 72 hours) for both appetitive and aversive groups. However, true recovery may not have occurred since the changes in behavior suggestive of recovery from amnesia were similar to those observed in reinforced and nonreinforced control groups, reflecting, in part, gradual adaptation to the experimental situation after repeated exposures. The remainder of the animals, tested for the first time at 72 hours, however, showed degrees of retrograde amnesia equal to or greater than that observed in the subjects tested for the first time at 24 hours. This observation appears to indicate that reexposure to the experimental situation

is a necessary condition for recovery.

The results of both studies in which the animals are tested repeatedly can be interpreted in terms of Zinkin and Miller's alternative explanation, that is, that learning takes place during reexposure which "would depend on there being some minimum retention of the . . . properties of the situation on the first day of testing." The recovery obtained in our study may also be attributed to adaption-habituation phenomena. However, the amnesia observed in animals tested for the first time at intervals longer than 24 hours indicates that retrograde amnesia is apparently permanent.

Note added in proof: The recent report of Luttges and McGaugh (*3*) in which no recovery was observed with repeated tests over periods as long as 1 month suggests that recovery, when it occurs, may be a function of procedural or task variables.

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References

1. S. Zinkin and A. J. Miller, *Science* **155**, 102 (1967).
2. H. V. S. Peeke and M. J. Herz, *Proc. Annu. Conv. Amer. Psychol. Ass. 75th* **2**, 85 (1967).
3. M. W. Luttges and J. L. McGaugh, *Science* **156**, 408 (1967).

2 March 1967

The point we wished to make in our article was that, with avoidance latency as the criterion for retention, individual ECS-treated animals were showing more evidence of retention on the second or third day of testing, or both, than on the first day. The interpretation of this effect was intentionally left open, since it was not clear whether the retention was a function of the repeated testing or of the passage of time—but in either case a partial or subthreshold trace must have been present at a time when no sign of retention was elicited by behavioral testing.

While Herz and Peeke's results favor the first explanation, a "shrinkage of amnesia" interpretation may still apply in cases where the interval between learning and ECS is longer than we (and presumably Herz and Peeke) were using. With a longer ECS delay, amnesic effects may reflect a disturbance of retrieval as much as of storage, and such a disturbance could dissipate with time. We are currently investigating this possibility.

On the other hand, ECS administered immediately after learning almost certainly interferes with the actual setting up of the memory trace. Whether or not behavioral retention will appear (in *any* subsequent test) is likely to depend on how much of the original trace manages to survive the ECS, and this in turn may depend on a variety of factors, including the strength and duration of stimulation, the path taken by the current (that is, by means of ear or corneal electrodes), and the nature of the pre-ECS learning trial. Differences in variables of this sort might, for example, explain Luttges and McGaugh's recent failure (*1*) to obtain the recovery effect found by both Herz and Peeke and ourselves.

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Reference

1. M. W. Luttges and J. L. McGaugh, *Science* **156**, 408 (1967).

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Type of Sodium Bond in Mammalian Hair

In a recent report [*Science* **155**, 588 (1967)] G. S. Kennington has argued that sodium is probably present in mammalian hair in two states—a relatively free and a relatively strongly bonded form. His conclusions are based on washing experiments carried out with antelope hair. Sodium-22 incorporated by impregnation could be washed out completely, leaving a sample that still contained sodium, as shown by activation analysis.

Interaction between human hair and solutions of different salts has been under investigation in our institute. The information obtained is important for evaluating the possibility of distinguishing persons by concentrations of trace elements in their hair.

During this work we have observed anomalies in the diffusion of certain elements, but not with sodium. However, our experiments have shown that human hair can concentrate sodium from very dilute aqueous solutions. For instance, a solution of 10 parts of NaCl per million leads to a concentration in hair of 60 ppm. It would, therefore, be extremely difficult to wash hair completely free of sodium because water could easily take up some sodium from the glass vessel. Unfortunately, the end concentration of sodium in Kennington's washed sample is not given. No conclusions can be drawn from the gamma spectrum of the irradiated sample because the irradiation and waiting times were not specified. Therefore, it seems advisable to postpone conclusions concerning the type of sodium bonding until more quantitative data are available.

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Professor Houtman's comments call attention to one of the problems of activation analysis of tissue—that of the pervasive presence of sodium. Sodium is not only normally present in tissue but also may be inadvertently introduced even during careful handling when, for instance, clean forceps touch table space where ungloved hands have previously rested, or, as he suggests, minute amounts of sodium washed from the glass vessel remain in the hair sample that is to be activated. In all my tests I included a blank of filter paper to check for the presence of contaminated water and vessels and to check technical procedures. The spectra of these blanks showed no sodium (or other) residues. Of course it is possible that sodium from these sources is concentrated or held by the hair fiber in a manner different from the way it is held by filter paper fibers, but the sodium-22 experiment would argue against that.

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