equipment and to limit distraction by other laboratory sounds.

We found that the rate at which the events were presented dramatically affected the performance of the subjects. The 12 subjects who received 5 events per minute detected 210 out of 240 signals. At the higher event rate of 30 per minute, the other 12 subjects detected only 87 out of 239 signals (the information on one signal for one subject in this group was lost). These results are presented in more detail in Fig. 1 which shows that performance was relatively steady at the low event rate of 5 events per minute.

The familiar decrement in vigilance appeared with the higher event rate of 30 per minute. This decrement, the drop in signal detection from about 60 percent during the first 20 minutes to about 30 percent in the later parts of the vigil, is statistically significant at the .02 level as determined by the Friedman test (*4*). The difference in performance between the two groups was, of course, also statistically significant ($p < .001$).

It is important to keep in mind that the two groups of subjects, differentiated in Fig. 1 by the two rates at which the events were presented, were not systematically differentiated in any other way. The difference, then, had nothing to do with the rarity of signals; signals were equally rare for both groups at least as events embedded in a matrix of time. Nor could the difference be attributed to the length of time that these students had to keep up the dull watch, since the watch was equally long for both groups. Factors such as memory for the signal were controlled by having all of the information that an observer needed to make a "paired-comparison" judgment available within the event. For the sensory task, the observer needed only to compare the two successive deflections of the bar that constituted an event, and the temporal structure of an event was the same for both of the groups.

The large difference between the number of signals detected in the two groups could be due only to the different rates at which the events were presented. We, therefore, have the apparently strange situation in which the "detectability" of a signal is determined by what is going on at times when no signal is being presented (*5*).

In a sense, this experiment was a test of the relative importance of the passive waning of attention in the face of boredom as opposed to the active, though not necessarily conscious, decision to be inattentive. The passive waning of attention might be described as a reduced arousal level (*6*) related to the amount of incoming stimulation. In these terms, low event rates should produce a lower arousal level than high event rates, and in fact, subjectively, there was no question that the situation in which events were presented at the low rate was more dull and monotonous. Therefore, the predictions from an arousal point of view would be that high event rates should produce higher degrees of alertness and more detections than low event rates.

Our results were completely contrary to the arousal position, and fit in very well with the decision-theory approach to vigilance (*7*). We consider that the observer's "decision" about whether or not to observe or be attentive is associated with the average payoff or "expected value" of attending to the regularly recurring events. The observer, then, behaves most economically by being less likely to attend to a given event when few of the events are signals, and more likely to attend to a given event when more of the events are signals. When, as in this experiment, signals are presented at certain fixed times, a change in the event rate produces an inverse change in the probability that an event will be a signal. The "expected value" of observing an event is therefore greater for the lower event-rate.

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**References and Notes**

1. D. N. Buckner and J. J. McGrath, Eds., *Vigilance: A Symposium* (McGraw-Hill, New York, 1963).
2. A role for reinforcement of observing was demonstrated by J. G. Holland, *Science* **128**, 61 (1958), although the emphasis in that report was on the schedule of detections as governing the rate of free emission of observing responses. In this report, the observing response is treated as a hypothetical construct with an emission rate proportional to the rate of appearance of attention-demanding events that might be signals.
3. H. J. Jerison and R. M. Pickett, *Human Factors* **5**, 211 (1963).
4. S. Siegel, *Nonparametric Statistics* (McGraw-Hill, New York, 1956), pp. 166–172.
5. This result recalls some of the effects found in "signal detectability" studies in which changes in detectability are also associated with nonsensory factors; for example, J. A. Swets, *Science* **134**, 168 (1961). The main difference between such studies and vigilance studies is in the demands made on observing behavior. "Signal detectability" studies are concerned with detection performance when observing occurs, whereas vigilance studies are concerned with factors affecting observing or attending.
6. D. E. Broadbent in (*1*), pp. 184–198, gives a detailed critique of the arousal concept as it has been applied to vigilance studies.
7. Our results can also be described empirically as favoring signal probability as opposed to signal frequency (signals per unit time) as a determinant of vigilance. W. P. Colquhoun, in *Ergonomics* **4**, 41 (1961), used these terms to describe the outcome of an experiment in which the event rate was constant at 30 per minute, and signal probability was associated with the relative frequencies of two types of randomly occurring occasional signals. The results as well as the procedures of Colquhoun's experiment were more complex than ours. For example, signal probability was favored over signal frequency for only certain spatial configurations of Colquhoun's signal. We believe his results involve much more than the making of decisions about whether or not to observe a display. We cite his work now in anticipation of possible confusion due to the applicability to both his work and ours of the distinction between signal frequency and signal probability. Colquhoun's "probability" was the conditional probability of a signal of one type given a signal of either type, with event rate ignored. Our "probability" is the conditional probability of a signal given an event.
8. This work was supported by the U.S. Air Force under contract AF 33(657)-7362, monitored by the Aerospace Medical Laboratories, Aeronautical Systems Division. John F. Wing and Austin W. Kibler gave us helpful criticisms of this manuscript, James M. Campbell served as instrumentation engineer, and Philip H. King was an invaluable research assistant.

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## Behavior: Persistence of Shock-Induced Aggression

Abstract. *Previous research has shown that aversive stimulation causes aggression in several lower species of mammals prior to any specific conditioning. Our results show that fighting in response to shock tends to persist in spite of negative reinforcement for other behavior. The frequency of shock-induced fights decreased significantly only when the reinforcement of shock termination was made contingent upon a specific nonaggressive response.*

Electric shocks (*1*), loud sounds (*2*), and strong lights (*3*) all decrease the frequency of the responses which they follow. Conversely, a number of responses have been conditioned entirely on the basis of escape from or reduction of these same aversive stimulus conditions (*2, 4, 5*). Typically, in these experiments, the aversive stimulus has been presented to a single organism rather than to a pair or a group of subjects. One of several exceptions to this single-subject type of experiment is Miller's study (*6*), in which paired

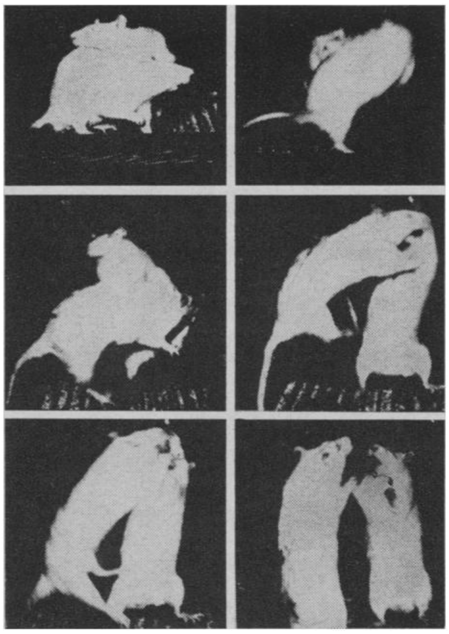

Fig. 1. Example of a pair of rats assuming the stereotyped fighting posture immediately after the onset of shock.

rats were placed in a chamber and shocked. When the subjects happened to approach each other in a sparring position similar to that used by rats in fighting, the shock was abruptly turned off. After a minute without shock the current was again turned on, and the animals were given another trial. The act of sparring was thus said to be rewarded by escape from shock. More recent studies (*7, 8*), however, have shown that simply presenting an aversive unconditioned stimulus will produce attacks between paired animals prior to any specific conditioning (Fig. 1). Such findings suggest that the conditioned fighting mentioned above (*6*) would have eventually occurred even if no reinforcement through shock termination had been given. We have attempted to determine whether rats could be conditioned not to fight when given an electric shock. Nonaggressive responses were reinforced with shock removal, and all aggressive responses were followed by further shock.

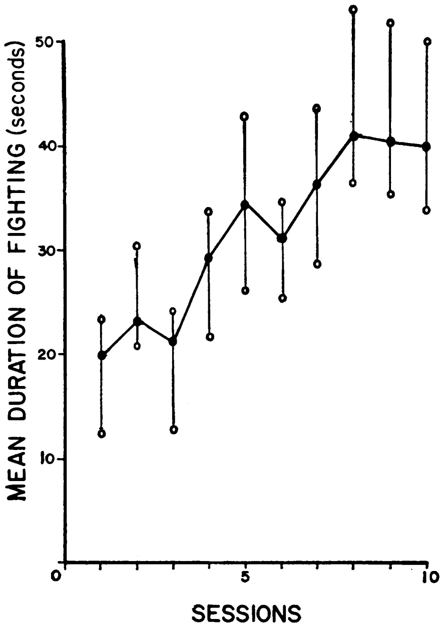

Fig. 2. Mean number of seconds (dark circles) spent fighting by four paired rats during ten sessions. Open circles indicate the range.

Thirteen Sprague-Dawley rats were subjects. The experimental space measured 0.3 meter square and was housed inside a large soundproof chest. A small one-way window at the side of the chest allowed unrestricted observation. The floor of the inner chamber was constructed of parallel grids through which shocks of changing polarity could be delivered. The criteria for defining an aggressive response were the same as those used in a previous experiment (*7*). An aggressive response was recorded by an observer who depressed a microswitch for any striking or biting movement by either animal toward the other while in the stereotyped fighting posture. When shock was delivered the animals would typically assume and maintain this posture (Fig. 1, bottom right) for periods during which several striking movements might be made. Ninety-five percent agreement between observers was obtained regarding the occurrence or nonoccurrence of an aggressive response.

Paired animals were placed in the experimental chamber. After 60 seconds had elapsed shock was presented continuously until a nonaggressive response occurred, whereupon the shock was immediately terminated for 60 seconds. Following this 60-second period the shocks were again presented and the procedure was repeated. A record was kept of the amount of time the subjects spent in fighting. The session ended after the first nonaggressive response after the 15th presentation of shock.

The results, shown in Fig. 2, give the mean number of seconds each pair of subjects spent fighting during each of ten trials. The amount of time spent fighting during the later sessions was actually greater than that observed at the beginning. This increase in fighting occurred in spite of the fact that shock was terminated after nonaggressive responses and continued after aggressive responses.

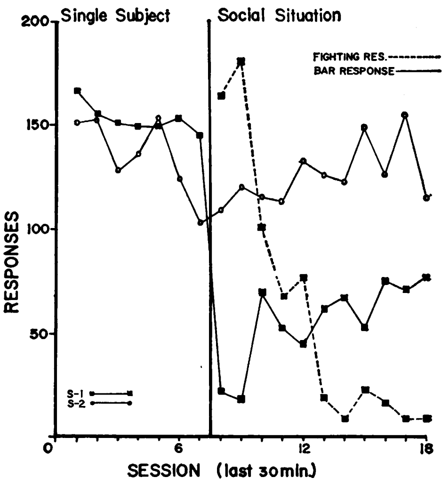

Fig. 3. Fighting and bar press responses in a social situation as compared to a situation where only one animal was present. S-1 (black squares) was paired with a naive rat, S-2 (open circle) was paired with a small rat-sized rubber dummy. Fighting responses are depicted by the dotted line and occurred only toward a live, second subject.

One possible explanation for this failure to suppress fighting is that, while the continuation of shock was solely contingent upon fighting, its termination was related to a number of different responses. The animals, for example, could move away from each other in several directions. One animal might simply lower to the floor as the other jumped back toward an opposite wall, or both might fall away from each other onto their backs. In short, the escape response was not specific.

Since previous findings have shown that reinforcement is most effective when it is contingent upon a specific response, we decided to incorporate such specificity in our investigation of the reinforcement of nonaggressive behavior. Two rats with a long history of stable performance in a discriminated avoidance situation were used with one naive rat and one small rat-sized dummy. The trained animals had been conditioned to press a bar which produced a period of 20 seconds of no shock. If the bar was not pressed, brief (0.5-second) shocks (1 ma) occurred every 5 seconds. During this experiment each animal was run alone for 3½ hours of a typical 4-hour session. A naive rat or the small dummy was put in the chamber during the final 30 minutes.

No major change in avoidance behavior took place when the dummy was put in the chamber (Fig. 3). Furthermore, no attack movements were made toward the dummy. In contrast, when a naive animal was placed with the trained animal, there occurred a sharp drop-off in the number of responses (145 to 24). Observation indicated that in the social situation with the naive animal, bar pressing had been replaced by aggressive behavior. In fact, during the first two sessions the subjects fought after almost every shock. In the subsequent sessions, however, the actual fighting fell off rapidly to an average of ten fighting responses per session. On the other hand, during the same period, the escape-avoidance responding did not return to the single-subject rate. It appeared that the reason for this lack of recovery in bar pressing was related again to the fighting phenomenon. In the single-subject situation the trained animals tended to remain near the bar and only occasionally after a response did they move away. However, after a bar press in the presence of another live subject, the trained animal would frequently return to the naive animal and reassume the stereotyped fighting posture. This posture was maintained until the preshock stimulus appeared, whereupon the animal would return to the bar, press it, and return again to the naive animal, thus apparently lowering the probability of bar pressing. This preoccupation with the naive animal appeared to be one of the factors responsible for the low frequency.

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### References and Notes

1. N. H. Azrin, W. C. Holz, D. F. Hake, *J. Exptl. Anal. Behav.* **6**, 141 (1963); W. C. Holz, N. H. Azrin, R. E. Ulrich, *ibid.* **6**, 115 (1963).
2. N. H. Azrin, *ibid.* **1**, 183 (1958).
3. W. N. Schoenfeld, unpublished study (1947).
4. O. H. Mowrer, *J. Abnormal (Soc.) Psychol.* **35**, 56 (1940).
5. F. S. Keller, *Psychol. Rec.* **4**, 235 (1941).
6. N. E. Miller, *J. Abnormal (Soc.) Psychol.* **43**, 155 (1948).
7. R. E. Ulrich and N. H. Azrin, *J. Exptl. Anal. Behav.* **5**, 511 (1962).
8. R. E. Ulrich, P. C. Wolff, N. H. Azrin, *Animal Behav.*, in press.
9. Supported by grants from the National Institute of Mental Health and the Research Fund of the Illinois Department of Mental Health. The assistance of T. J. Stachnik, P. C. Wolff, and Miss M. Mack is gratefully acknowledged.

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## Hormonal Control of Egg Development in Calliphora

In "Tanning in the adult fly: A new function of neurosecretion in the brain" G. Fraenkel and Catherine Hsiao mention a "gonadotropic" hormone from the brain of the fly *Calliphora erythrocephala*, and state that the term "gonadotropic" hormone is used in the sense given it in a paper by A. O. Lea and E. Thomsen (*2*), "according to which ovarian development in the fly is activated by the corpus allatum by means of a secretion from the median neurosecretory cells, and not, as was formerly assumed [here referring to E. Thomsen (*3*)] by the cells through the corpus allatum."

I want to point out that we have not used the word "gonadotropic" hormone for the hormone produced by the medial neurosecretory cells, (m.n.c.), and do not regard it as such. The m.n.c. have a profound influence on the development of the ovaries (*3*), but they also regulate the production of proteolytic enzymes by the midgut cells, which must be regarded as the protein synthesis of these cells (*4*). That the m.n.c. might exert their effect on the growth of the ovaries through an influence on the protein metabolism and not solely through the corpus allatum was already suggested in the paper by E. Thomsen in 1952 (*3*).

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### References

1. G. Fraenkel and C. Hsiao, *Science* **141**, 1057 (1963).
2. A. O. Lea and E. Thomsen, *Mem. Soc. Endocrinol. No. 12* (1962), pp. 345-347.
3. E. Thomsen, *J. Exptl. Biol.* **29**, 137 (1952).
4. ——— and I. Møller, *Nature* **183**, 1401 (1959); "The Ontogeny of Insects," *Acta Symposii de Evolutione Insectarum* (Prague, 1960), pp. 121-126 (English-language edition, I. Hrdy, Ed., Academic Press, New York, 1959); *J. Exptl. Biol.* **40**, 301 (1963).

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## Retrograde Amnesia from Electroconvulsive Shock

Lewis and Adams contend in their report (*1*) that the retrograde amnesia which occurs if electroconvulsive shock is given immediately after a learning trial results from the convulsive response itself, which competes with and replaces the previously learned response. This, they suggest, gives "the appearance of amnesia" but is actually a conditioned prepotent response.

Thus this report emphasizes, as does much of the critical research on perseveration theory, the effects of the interference technique rather than the effects of interference upon learning. This is understandable, since most studies on perseveration theory use electroconvulsive shock as the interference technique. However, ether anesthetization can also be used and is as effective as shock in producing retrograde amnesia. (*2*). It is difficult to conceive of the complete loss of response which occurs with anesthetization as conditioned competing responses. A better explanation would seem to be that both the shock and anesthetization somehow interfere with the consolidation of perseverative neural activity and thereby produce amnesia.

In addition, it is difficult to fit the competition-of-response explanation to the data of studies indicating a time relationship in retention. These studies have shown that as length of time between end of learning trials and administration of interference increases, amount retained increases also. Again, it seems that there must be a neurological mechanism involved.

Further, evidence for the competition-of-response theory is apparently based on the assumption that, in previous studies on perseveration theory, animals were given the electroconvulsive shock in the stimulus situation. This is not the case. Examination of at least several studies indicates that animals were removed from the stimulus situation before shock was administered (*3*).

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### References

1. D. J. Lewis and H. E. Adams, *Science* **141**, 516 (1963).
2. C. J. Dye, thesis, Washington Univ., St. Louis, Mo., unpublished; W. B. Essman and M. E. Jarvik, *Psychopharmacologia* **2**, 172 (1960).
3. C. P. Duncan, *J. Comp. Physiol. Psychol.* **42**, 32 (1949); K. J. Hayes, *ibid.* **46**, 216 (1953); R. Thompson, *ibid.* **50**, 644 (1958).

19 November 1963