ing period. Analysis of variance yielded significant main effects of treatment conditions ($F = 7.58$, d.f. 2/18, $P < .01$) and blocks of days ($F = 5.06$, d.f. 3/27, $P < .01$). As the interaction was insignificant ($F < 1.0$ overall treatment means were compared by the Newman-Keuls procedure (*3*). The E trials were significantly slower ($P < .05$) than N or R trials. Figure 2 shows the running speeds were slower on extinction as were again slower than trials on new paper after the first three test days. Trials on reward paper displayed a more aberrant trend measured by running speed than by starting speed. The R trials were slower than E and N trials over the first half of testing. However, from day 7 until the end of the testing period, performance on reward paper was coincident with trials on new paper; both were faster than trials on extinction paper. Analysis of variance yielded only a significant main effect of treatment conditions ($F = 3.66$, d.f. 2/18, $P < .05$). Again the interaction was insignificant ($F = 1.74$, d.f. 6/54, $P > .10$) and comparison of the overall treatment means was made by the Newman-Keuls procedure. The E trials were significantly slower ($P < .05$ than N, but not R trials.

These results indicate that the odor trace of a rat undergoing experimental extinction can significantly disrupt the performance of a subsequently run animal that was continuously reinforced. This disruption has previously been termed the "pseudo-extinction" effect and was evidenced as slower starting speeds on E as compared to N and R trials and slower running speeds on E as compared to N trials. This suggests that the mere traversal of another subject is not sufficient to disrupt the succeeding animal's performance. Rather, the state of the animal laying the trace seems to be critical in the elicitation of competing behaviors within the experimental animals. The pattern of results evidenced by the two dependent variables was different. There is the possibility that the repeated testing procedure had differential effects on running than on starting times, this influencing the time course of the observed effects.

Our experiment does not discriminate between qualitative and quantitative odor effects since experimentally extinguished animals were on the paper floor longer than rewarded animals. Nor does it identify the olfactory stimuli involved, particularly whether these olfactory stimuli are isolable from those of the excretory products deposited by the ET animals. The experiment does, nonetheless, demonstrate the importance of olfactory stimuli to the "pseudo-extinction" effect.

Rats can discriminate odors from animals of the same species put under stress by electric shock (*4*). Experimental extinction is apparently a situation capable of producing the emission of some olfactory stimulus which, when present on the paper floor of a subsequently run animal, elicits some behavior which interferes with running for food reward. Such odor effects appear to be an important, potential confounding variable in studies where learning rather than the transmission of information between conspecifics by chemical means is investigated (*5*). Results from situations involving noxious stimulation, such as electric shock or nonreward, which seem likely to increase the probability of odor emission, should be reevaluated because of such confounding. Control for odor effects would seem desirable if interpretation of experimental outcomes is to be unambiguous.

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References and Notes

1. J. H. McHose and H. W. Ludvigson, *Psychonom. Sci.* **6**, 485 (1966); J. H. McHose, *ibid.* **9**, 281 (1967); H. W. Ludvigson and D. Sytsma, *ibid.*, p. 283; J. H. McHose, L. L. Jacoby, P. A. Meyer, *ibid.*, p. 401.
2. D. W. Lauer and T. S. Carterette, *J. Comp. Physiol. Psychol.* **50**, 334 (1957).
3. B. J. Winer, *Statistical Principles in Experimental Design* (McGraw-Hill, New York, 1962), pp. 80–85.
4. J. G. Valenta and M. K. Rigby, *Science* **161**, 599 (1968).
5. Although technical difficulties have precluded the use of and reference to the term "pheromone," the general similarity of the odor effects in the present experiment to pheromones should be noted. For a discussion of pheromone effects in vertebrates, see K. K. Gleason and J. H. Reynierse [*Psychol. Bull.* **71**, 58 (1969)].

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Occurrences of $CaCO_3 \cdot H_2O$ and Its Naming

In the report by Marschner (*1*) of the formation of the compound $CaCO_3 \cdot H_2O$ ("hydrocalcite") in scales deposited from cold waters, the statement that "it has hitherto not been observed in nature" is incorrect; the compound was first observed in 1959 in bottom sediments from Lake Issyk-Kul, Kirgizia, by Sapozhnikov and Tsvetkov (*2*), whose analysis gave $CaCO_3 \cdot 0.65H_2O$. In 1964, Semenov (*3*) showed that the optical and x-ray data for the material corresponded to those for the well-known synthetic compound, hexagonal $CaCO_3 \cdot H_2O$. The x-ray powder diffraction data differ slightly in spacings and intensities from those of Marschner but undoubtedly refer to the same compound.

A second occurrence of $CaCO_3 \cdot H_2O$ was reported in 1963 by Carlström (*4*), who found it in trigonal crystals ($a = 6.100$ Å, $c = 7.553$ Å) among the statoconia of the tiger shark *Galeocerdo cuvier*.

Semenov (*3*) named the material monohydrocalcite, and this name has priority over Marschner's "hydrocalcite." The latter name is doubly unacceptable, because it had already been used by Kosman in 1892 to designate material that was perhaps $CaCO_3 \cdot 2H_2O$ or $CaCO_3 \cdot 3H_2O$ (*5*). This is an excellent example of unnecessary confusion in the mineralogical nomenclature that could easily have been avoided if the proposed new name had been referred to the Commission on New Minerals and Mineral Names, International Mineralogical Association.

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References and Notes

1. H. Marschner, *Science* **165**, 1119 (1969).
2. D. G. Sapozhnikov and A. I. Tsvetkov, *Dokl. Akad. Nauk SSSR* **124**, 402 (1959).
3. E. I. Semenov, *Kristallografiya* **9**, 109 (1964); abstract in *Amer. Mineral.* **49**, 1151 (1964).
4. D. Carlström, *Biol. Bull.* **125**, 441 (1963).
5. See E. S. Dana, *System of Mineralogy* (Wiley, New York, ed. 7, 1951), vol. 2, p. 227.
6. Publication authorized by the director, U.S. Geological Survey.

6 October 1969

Hard Clam Pumping Rates: Energy Requirement

The paper by Hamwi and Haskin (*1*) on oxygen consumption and pumping rates in *Mercenaria mercenaria* seems to draw a conclusion not warranted by the data they presented. I have reproduced their Fig. 2, from which they conclude that pumping rate may be regulated by oxygen require-

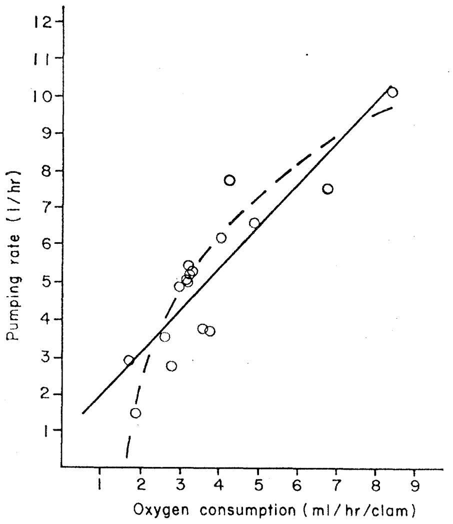

Fig. 1. Pumping rate in the hard clam *Mercenaria mercenaria* (liter/hour) graphed against oxygen consumption rate (milliliter/hour/clam). Modified from Fig. 2 of Hamwi and Haskin (*1*). Their regression line is unrealistic because it crosses the ordinate at a pumping rate of 0.84 liter/hr. The relation is obviously nonlinear as indicated by the dashed line, drawn in by inspection.

ment (Fig. 1). Oxygen consumption is directly proportional to pumping rate, but not linearly. I have drawn in a curve (dashed line) to represent the shape indicated by the data. However, two straight lines with a slope change at a pumping rate of about 5 liters per hour would also fit these data. But the unrealistic nature of the linear regression equation computed by Hamwi and Haskin is demonstrated by the fact that it predicts zero oxygen consumption when pumping rate is 0.84 liter per hour! Obviously the clam has a measurable oxygen consumption rate as pumping approaches zero, and the data indicate that this nonpumping rate of respiration is about 1.5 ml/hr per clam.

The slope of the oxygen consumption curve at minimum pumping rates indicates that the work of pumping consumes about 0.3 ml of oxygen per liter of water pumped. The slope at maximum pumping rates indicates that the work of pumping consumes about 1.5 ml of oxygen per liter of water pumped. It is not surprising that high respiration rates are correlated with high pumping rates. Pumping is work, and it is accomplished by energy supplied by respiratory processes. There is nothing in these data that suggests that pumping rate is regulated by oxygen requirement; rather, the oxygen consumption required to power the pump at maximum rate is about five times greater than it is at the minimum rate.

The data allow computation of the efficiency of the pump. In aerobic respiration the caloric value of the fuel consumed is about 0.112 calorie per micromole of oxygen consumed. Hence the pumping of a liter of water at the maximum rate burns about 7.5 calories worth of respiratory substrate. At minimum pumping rates the pump is more efficient, requiring only about 1.5 calories worth of fuel per liter pumped. Intermediate efficiencies would, of course, be obtained between these extremes.

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Reference and Note

1. A. Hamwi and H. H. Haskin, *Science* **163**, 823 (1969).
2. Supported in part by NSF grant GB-4041.

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Blockage of Olfactory Discrimination

Blockage of olfactory discrimination in salmon by puromycin, cycloheximide, and actinomycin D (*1*) does not necessarily mean that "expression of long-term olfactory memory . . . requires continuous protein or RNA synthesis, or both." While this is one possibility, numerous experiments have shown that these antimetabolites—especially puromycin—have side effects that impair neural function nonspecifically and in some cases by mechanisms unrelated to inhibition of macromolecular synthesis (*2*). The partial inhibition of protein and RNA synthesis observed by Oshima *et al.* may therefore be coincidental rather than causal. Since the effect was at least partly reversible, the only definite conclusion should be that the antimetabolites interfered with retrieval of information.

The authors' contention that their data, if sustained, "would indicate that long-term memory is a continuous metabolic process . . ." must also be questioned. While the expression of memory may have been blocked, the memory itself, or "*capacity* to behave in a way which is modified by experience" (*3*), was not destroyed. The problem may be semantic only, but in this field a clear distinction between retrieval and the storage form of information is crucial.

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References and Notes

1. K. Oshima, A. Gorbman, H. Shimada, *Science* **165**, 86 (1969).
2. S. H. Barondes and M. E. Jarvik, *J. Neurochem.* **11**, 187 (1964); H. Cohen and S. H. Barondes, *Science* **157**, 333 (1967); N. Dahl, *J. Neurobiol.*, in press; J. B. Flexner and L. B. Flexner, *Proc. Nat. Acad. Sci. U.S.* **57**, 1651 (1967); R. B. Roberts and L. B. Flexner, *Quart. Rev. Biophys.* **2**, 135 (1969).
3. D. Richter, in *Aspects of Learning and Memory*, D. Richter, Ed. (Heinemann, London, 1966), pp. 1–4.

24 July 1969

We have no argument with Irwin's statements, and, as he says, our possible differences may be only semantic. In fact it is pleasing to see that he feels "the only definite conclusion should be that the antimetabolites interfered with retrieval of information." We feel that this action of the antimetabolites may in part, at least, characterize the nature of the retrieval process. If you equate our term "expression of memory" to Irwin's "retrieval" there is no real disagreement. We agree also that the question of the storage form of nervous information is a separate one, but here again, our experiments may at least indicate something about what this storage form is *not*.

Finally, I should point out, as was done in our report, that we present these data primarily as an alternative to the current hypothesis that memory is based on residual protein or RNA templates. As an alternate hypothesis we propose that if a template exists it is probably neither of these and that expression of memory (retrieval) requires a continuous metabolic process. As Irwin points out, and as we suggested, this hypothesis is not the only one derivable from our data, but it is an important enough characterization of the memory process to deserve to be considered and tested further.

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