sorbed to the activated carbon and, in competition for the active sites, would have the ability to displace less strongly sorbed materials. Thus, when very large amounts of river water are brought in contact with activated carbon, only those materials most strongly adsorbed remain on the carbon. Hence, it is possible that a test carried out under the described conditions would fail to detect DDT or dieldrin when, in fact, large quantities were present.

In view of the foregoing considerations, little meaning can be attached to negative findings, nor can it be assumed in the case of positive findings that the concentration is below that "toxic to fish or hazardous to man."

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We appreciate the timely, well-phrased, and pertinent comments of Wolf. It was not, however, our intent to imply (i) that lack of detection indicated insecticide levels of less than 1 μg per liter or (ii) that detection indicated concentrations of 1 to 2 μg per liter.

We are well aware of the questionable quantitativeness of carbon filter sampling of raw water and have been closely associated over a 2-year period with studies concerned with the efficiency and applicability of this sampling method. The results reported are qualitative results which indicate only the presence of DDT and dieldrin. The numerical values given are based upon assumptions and are so qualified in the text. In the light of the present need for information of this kind, we felt that withholding these qualitative results was inadvisable.

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Sensory Deprivation of Rats

In a recent report (*1*) the authors say that "If the rat has been reared in visual sensory deprivation, even though he has not been subjected to sensory deprivation during his adult life, he does not prefer a response alternative leading to food. Instead, he tends to choose a response alternative leading to a more perceptually complex, stimulating situation."

The observation that is supposed to justify this statement is that rats reared in either uniformly white or uniformly black quart cans later fail to show typical learning to go to food placed in a half-white, half-black alley; they learn more quickly to go to food in an alley painted in a checkerboard pattern. The authors claim that "perceptual complexity" accounts for the difference.

I would claim that prolonged association of the simple stimuli with noxious conditions can more parsimoniously account for the difference. Eight years ago, Goodson and Brownstein (*2*) showed that rats would avoid stimuli which had previously been associated with electric shock. It does not surprise me to find that they will also avoid stimuli which have been associated with 45 days of close confinement. Had I been confined for a third of my life as these rats had, I would go rather hungry before entering a situation which was reminiscent of that confinement. The authors could hardly have designed a "simple" arm for their maze (half black, half white) better calculated to elicit any existing avoidance responses from rats reared under both of their "sensory deprivation" conditions.

The data themselves lend credence to the logical possibility that creating a secondarily motivating stimulus, rather than perceptual deprivation, is the critical step. As the authors themselves point out, a majority of the sensory-deprived subjects avoid the half-black, half-white arm of the maze on 21 of the 25 massed learning trials. The authors' Fig. 1, however, gives some indication that the degree of avoidance lessens, and hence that food *is* acting as a reinforcer. The tendency to go to the complex stimulus (away from the noxious stimulus?) is certainly not increasing, as it should if commerce with the complex stimulus is truly preferred to food.

The figure, then, presents evidence which is exactly contrary to the authors' statement that "Under the conditions of this experiment, food is not a uniformly reinforcing substance serving to increase the probability of responses associated with it." It is true that the probability of response to the food side in the critical group is not greater than .5, but I doubt if many drive theorists would make the blanket assertion that it should be, given some imbalance in the original probabilities of response, and given some arbitrary limit on the number of trials.

In summary, the authors have interpreted their results as supporting their preconceptions despite the lack of the controls necessary to justify the interpretation. I am not an exponent of a "drive theory," but if I were, I would not be concerned in the slightest with the results reported. Rather, I would suggest that the experiment be repeated with appropriate controls, including a group of animals which are restricted 45 days in the presence of a complex stimulus. I would predict that such animals would appear to be seeking commerce with *simple* stimuli, if these simple stimuli were less like the stimuli associated with restriction.

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References and Notes

1. G. P. Sackett, P. Keith-Lee, R. Treat, *Science*, **141**, 518 (1963).
2. F. E. Goodson and A. Brownstein, *J. Comp. Physiol. Psychol.* **48**, 381 (1955).

This reply to the criticism by Hillix might aptly be titled "To approach or to avoid: that is the question." A control group composed of animals reared in isolation in a visually complex environment certainly would have provided a valuable addition to our study. Unfortunately, such a group was not available at the time. However, data will soon be made available concerning rats reared under the same isolation conditions as in our report, but in a black-white checkerboard environment. When run in a three stimulus, free-choice, situation involving no food deprivation or food rewards, these animals prefer a *checkerboard* visual stimulus similar to that present during restricted rearing, rather than the "simple stimuli . . . less like the stimuli associated with restriction" referred to by Hillix in his prediction. Also, a study by Musselman (*1*) involving normally reared rats run without food deprivation or reward is pertinent. When adapted to a checkerboard stimulus and subsequently tested in a free-choice situation, these rats prefer the original complex stimulus to more "simple" stimuli (black, white, or black and white stripes). In this study, as in ours, animals adapted to more simple stimuli choose a more complex stimulus on the subsequent free-choice test trial.

Hillix's argument seems to stem from the anthropomorphic assumption that relative environmental confinement is a noxious stimulus for the hooded rat, perhaps even as noxious as electric shock. Some evidence on this point does not support his contention. Total confinement by binding or other means certainly has produced noxious physiological effects on rats. However, data by Welker (*2*) show that rats prefer a small, dark, confined area to a larger, well-lighted area in a novel exploratory situation. Data by Berlyne (*3*) reveal that close confinement (in a much smaller area than that employed in our experiment) immediately prior to testing had no effect on either the subsequent amount of exploration or on the particular stimulus objects explored, even though some of these objects were present during confinement. If confinement serves to produce "secondarily motivating stimuli," then certainly the negative effects of such stimuli should have operated in Berlyne's study. In fact, I do not know of any valid experimental basis for assuming that relative confinement produces negative motivational effects in the rat, especially effects comparable to those of electric shock. Further, and most important, it should be noted that in our study the degree of confinement in relation to the size of the quite young animals was not exceptional, particularly when compared with the degree of confinement present in "normal" laboratory rearing cages housing four or five rats. In fact, our purpose for concluding the confinement period at 45 days was to keep the average amount of cage space per rat approximately equal for both restricted and normally reared control subjects.

Although these comments do not resolve the criticism of our interpretation, it does not appear to us that Hillix's alternative conceptualization "can more parsimoniously" account for our data. Admittedly, the results might be interpreted by several alternative explanations. At issue seems to be the basic orientation of the behavioral scientist toward his subject matter. Hillix's explanation appears oriented toward the fairly common viewpoint that behavior proceeds as a function of avoiding stimuli and their consequences. Our explanation, like that of Montgomery in explaining exploratory behavior, Dember in explaining spontaneous alternation, and Fiske and Maddi in attempting a general theory of the effects of sensory stimulation (*4*), is oriented toward explaining at least certain behaviors as a function of approaching stimuli and their consequences. I hope that rather than leading to further argument, our study and the many others pointing in a similar direction will lead instead to sound research aimed at resolving such conflicts in theoretical orientation toward the study of what might be called sensory motivation.

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References and Notes

1. D. R. Musselman, "Free choice as a function of adaptation to stimulus complexity," paper read at the Western Psychological Association Convention, April, 1962. This study is also included in an unpublished doctoral dissertation by D. R. Musselman, Claremont Graduate School, 1963.
2. W. I. Welker, *J. Comp. Physiol. Psychol.* **52**, 106 (1959).
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4. K. C. Montgomery, *ibid.* **46**, 129 (1953); **48**, 254 (1955); W. N. Dember, *ibid.* **49**, 93 (1956); D. W. Fiske and S. R. Maddi, *Functions of Varied Experience* (Dorsey, Homewood, Ill., 1961).

Littorina littorea as an Indicator of Norse Settlements

Spjeldnaes and Henningsmoen [*Science* **141**, 275 (1963] have recently postulated (i) that the common marine gastropod *Littorina littorea* was probably introduced to North America by Norse settlers about A.D. 1000, and (ii) that the subfossil occurrence of the species may therefore be useful in the identification of early Norse settlements in North America. I am constrained to point out that these claims are not well founded.

The argument in favor of the first hypothesis centers on the apparent absence of *Littorina littorea* from Greenland during the "warm period" of the post-Pleistocene. At that time the species was common in Europe and in Iceland, and Spjeldnaes and Henningsmoen feel that since it was then absent from Greenland it was probably then also absent from North America. This is not necessarily so. Other common mollusks such as *Thais lapillus* and *Modiolus modiolus* occurred then in Europe, Iceland, and North America but not in Greenland. Absence of any particular pan-boreal North Atlantic species from Greenland is therefore not unique, and special significance should not be attributed to it.

Secondly, according to Spjeldnaes and Henningsmoen the only known early Norse archeological site in North America is at Lance aux Meadows, Newfoundland. Careful excavations of that site were made, however, and no *Littorina littorea* were found. This reduces the second hypothesis to mere speculation. Surely, since it is quite possible that *L. littorea* has existed alive in North America for many more than 1000 years, the subfossil occurrence of the species is not a trustworthy indicator of Norse settlements. It appears that shells of certain European origin would be much more useful in this respect.

In short, we are still faced with the task of establishing the time and mode of introduction of *Littorina littorea* to the Western Hemisphere and must yet reconcile its anomalous occurrence in pre-Columbian deposits in eastern Canada [for additional documentation see *Nautilus* **77**, 8 (1963)] with its recent and dramatic expansions of range along the east coast of North America.

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There are no records of *Littorina littorea* from interglacial and preglacial time in North America, and this is interpreted to indicate that it did not live there. If this species immigrated to North America without being carried by man, it must have done so during the warm postglacial period, when the species was living in Spitzbergen and when the climatic conditions would have allowed it to live in Greenland. Its absence from beds of this age in Greenland is interpreted to mean that it did not use this route of migration. *Modiola modiolus* and the other pan-boreal species already existed along the coast of North America before this time, and their presence or non-presence in Greenland are therefore not relevant in this case.

The hypothesis that *Littorina littorea* existed in North America before the advent of European culture is pure speculation. It is not founded on any material evidence and is not supported by our present knowledge of the zoogeography of the North Atlantic. All present evidence seems to indicate that this species was a European one, transferred to North America by man.

The fact that *Littorina littorea* has not been found in the site at Lance aux Meadows cannot be given much weight, because no shell material is found there, probably due to the conditions of preservation.

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