systems in general, and that by the "six Newtonian coordinates for each element" he means what control theorists call "state variables" (*2*, p. 14; *3*).

If this is correct, historical causality can be described in the following conventional notation. A dynamic system described by the causality relationship

$$\frac{dx_i}{dt} = f_i\,[\mathbf{x}\,(t), t] \qquad i = 1, 2, \ldots, n$$

(where **x** is the state vector and *t* is time) requires no "action across a time lapse." However, some systems can be described only by a relationship that includes hereditary influences

$$\frac{dx_i}{dt} = f_i\,[\mathbf{x}\,(t), t, \mathbf{x}\,(t-\tau)] \quad \begin{array}{l} i = 1, 2, \ldots, n \\ \tau > 0 \end{array}$$

where dx_i/dt is a function of the past history of **x**. This latter type of system is mentioned in almost every text on modern control theory. For example, Bellman points out (*2*, p. 5) that every feedback control process falls into this latter category.

Therefore, Householder's professed belief that the principle of historical causality "has not been seriously considered in science" is incorrect. His further statement that "a vehicle for action across a time lapse seems hard to imagine" is difficult to understand because the well-known vehicle, memory, is pointed out by Householder himself later on in the review. In both the digital computer and the human being, action across a time lapse is the normal mode of operation, and memory is the mechanism by which this is accomplished.

Even the epiphenomenalism that apparently bothers Householder is familiar to control theorists. This arises from the arbitrariness of the state variables; in a complex system, it is seldom obvious what the optimum set of state variables is, and interpretation of experimental data or even simulations is often difficult.

It seems likely that Householder is intimately familiar with control theory and that an explanation exists for the apparent confusion in the review. However, if the review is to be useful, Householder must clarify his position.

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References and Notes

1. A. S. Householder, *Science* **141**, 895 (1963).
2. R. E. Bellman, *Adaptive Control Processes* (Princeton Univ. Press, Princeton, N.J., 1961).
3. Bellman defines the elements of the state vector as being that set of functions $[x_i\,(t)]$ containing all the information we will ever wish to have concerning the dynamic system.

I can assure Blakemore that I have some slight acquaintance with the literature on control theory and on the formal properties of differential-difference equations. In my review of Culbertson's book I was not concerned with the formal description of action across time. In ordinary applications of control theory the vehicle for this action is clearly present and well enough understood.

Culbertson himself seemed to feel that there was some novelty in the principle of historical causality, and the novelty lay just in the fact that no vehicle was provided. It explains nothing to talk about memory unless there is something there to store that which is remembered. The storage device in a digital computer is a perfectly definite part of the machine. The storage "device" in human beings is not too well understood, but presumably the storage is accomplished by physicochemical changes in the nerve cells. Culbertson is explicitly postulating some carry-over without use of the corresponding nerve cells, and the question at issue is how this comes about. Naturally we can, if we wish, just postulate that it takes place, and in accordance with such-and-such stated principles. We can do this, but I suspect that very few of us would find the procedure philosophically satisfying.

I regret that my language should have permitted such a basic misunderstanding of the main point I was trying to make.

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Detecting Insecticides in River Water

The recent report by Breidenbach and Lichtenberg [*Science* **141**, 900 (1963)] confirms the presence of DDT or dieldrin in river water at 14 of 101 locations. The method of extracting the insecticides from the water is carbon adsorption. Although the procedure used is adequate as a qualitative method, the inference is made that the lack of detection indicates an insecticide level of less than 1 μg per liter of water and that the amount observed in the case of positive tests indicates a level of 1 to 2 μg per liter of water. These allegations are based on a quantitative interpretation of the data which is not substantiated by either the experimental procedure or the references cited. The carbon adsorbate was prepared by passing the river water through a bed containing about 2 liters of activated carbon at a flow rate of 1.9 liters per minute. Up to 19,000 liters, or about 10,000 bed volumes, were used. Under these conditions of fast flow rate, granular carbon would be needed.

It is known that the efficiency of adsorption by activated carbon in a column decreases with decreasing bed depth, with increasing particle size, and with increasing flow rate. For high recovery of large organic molecules from solution with granular carbon, it has been my experience that flow rates of 1 bed volume each 20 minutes in columns containing a carbon bed 1 to 1½ m in depth are usually required. Less complete removal is obtained with faster flow rates and shorter columns. The adsorption condition of 1 bed volume per minute in a shallow (½ m) bed would undoubtedly give far from complete recovery of insecticides.

The low concentration also contributes to poor recovery of the insecticides, since under similar conditions of adsorption the percentage recovery decreases with decreasing concentration in the feed solution. This decrease, reflecting both adsorption and elution steps, is usually exponential. Although recovery experiments with the actual experimental conditions employed would be needed to determine the precise effect of concentration on the yield, the authors point out that DDT adsorption is 98 percent complete, and that 80 percent is eluted when an emulsion containing 5 mg per liter is used. This suggests that the recovery would be very poor indeed at concentrations of 1 to 2 μg per liter in the feed stream, since the residual concentration in the spent solution would be 100 μg per liter and the amount remaining on the carbon would be equivalent to 1000 μg per liter. Thus, rather than substantiating the sensitivity of the method, these data indicate the probability of low recovery and point out the need for further study.

Moreover, there is another serious objection to recovery estimates, and that is the effect of other substances which may be present in the water. Trace organic materials are present in any body of water, in contact with microorganisms, nutrients (both soluble and insoluble), and air. Some of these substances would be strongly adsorbed 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