

our use of nonreinforced test trials rather than of a learning paradigm. It is surprising that such a "general facilitation of learning" criticism is still voiced, in view of the existence of evidence to the contrary (3). Even if we had tested our recipients twice, first with the original stimuli and then with the new transposition stimuli, Yaremko and Hillix could still have argued that our recipients simply preferred large circles, and this time twice instead of only once. One could in fact argue that the subjects of any "larger-than" transposition experiment simply prefer larger stimuli, but isn't that what "larger-than" transposition experiments are all about?

In their second criticism, Yaremko and Hillix argue that our transfer effect may have been due to factors, other than learning itself, to which the donors were exposed. This is the "sensitization" argument, which they support by quoting a single study of planaria. Yaremko and Hillix do not mention that many reliable transfer effects have been demonstrated since that study in 1964, some involving considerable specificity (4), in spite of excellent con-

trols of confounding variables such as differential handling, activity, sensory experience, and so forth. In response to data from recent well-controlled experiments, proponents of "mere sensitization" are being forced to posit "differential sensitization" or "specific sensitization"—other words for learning or acquired information, which appears to be transferred after all.

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Food Habits of Early Man:

Balance between Hunting and Gathering

Leopold and Ardrey (1) have argued that (i) there is a wide range of toxic or poisonous materials present in plants, (ii) the major means by which man eliminates these materials is by cooking, (iii) the regular, controlled use of fire is relatively late in human evolution (about 40,000 to 50,000 years old), and (iv) anthropologists have overestimated the importance of vegetable foods in the diets of early human societies.

There are many objections to both the substance and the logic of this argument. For example, although the authors present a long list of the variety of toxic effects animals experience when eating certain plants, they give no sense of (i) how statistically widespread these actions are either in any complete range of plants which people are known to eat, or in the array of plants present in any particular environment; (ii) how much the plant kingdom of 40,000 years ago may have resembled that of today; (iii) the relative importance of toxicity according to the parts of plants which people ordinarily consume (ber-

ries, nuts, seeds, and fleshy roots are all usually more important as food than are leaves); (iv) the extent to which "emergency foods" could be regularly eaten (we know that people are generally particular about what they eat, and that a much larger inventory of edible, but uneaten foods is available to any human group); (v) the variety of methods other than cooking that exist for removing toxicity, such as drying, soaking, pressing, and leaching; and, most important, (vi) the relative importance of cooked compared to uncooked vegetable foods in the diets of present-day primitive people and whether, in any case, cooking is necessary or only desirable for some other reason (such as "palatability," or the ease with which skin can be peeled from a tuber).

I do not know to what extent the food habits of modern "hunter-gatherers" can be used to help reconstruct the subsistence pattern of preagricultural societies. Leopold and Ardrey make only one reference to this subject. R. Lee informs me that the !Kung Bushmen of the Kalahari desert, the one

group to which the authors refer, eat more than 50 percent of their vegetable foods in the uncooked state (2). The Gadio people of New Guinea, with whom I have worked, depend to a significant extent on the wild plant foods of the sort Leopold and Ardrey consider. About 8 percent (by weight) of the vegetable food portion of their diet is from wild plants (excluding processed sago flour, which accounts for 22 percent); garden food constitutes 65 percent and hunted animal food 4 percent. While the majority of these wild plant foods are cooked, this has nothing to do with removing "toxic properties," for, with one exception, they possess none (most of the leafy greens, barks, fern fronds, and fruits are also occasionally eaten in the raw state).

Although the question of the relative dependence of primitive people on wild plant foods has been only marginally investigated by anthropologists, there are several studies of this subject which the authors have not considered (3). In general, however, there are excellent nutritional, ecological, and energetic reasons for contending that plant foods have almost invariably been of much greater quantitative importance in primitive dietaries than have the useful, but in many ways supplementary "fruits of the kill."

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30 May 1972

While Dornstreich says that he finds "many objections to both the substance and the logic" of our assertions regarding the limitations of vegetable foods by toxic substances, his list of objec-

tions is principally composed of statements about the shortage of information.

The suggested objection based on the possibility that the extent of plant toxic substances might have changed in the 40,000 years since the widespread utilization of fire for cooking deserves special comment. On the basis of pollen analyses, it is known that the rate of evolutionary change in angiospermous plants since the start of Pleistocene time has been very slow, some tenfold slower than the rate of evolutionary change in mammals (1).

It would not be a sound archeological argument to presume that plants have changed extensively in respect to their chemical constituents since the time man learned to cook. As we have pointed out (2), one has to choose between the possibilities that man has lost a resistance to toxic substances by a backward evolution in the last 40,000 years, or that his susceptibility to toxic materials was similar to that which we know today. We cannot see a reasonable basis for assuming a loss of resistance, since there would have been no apparent evolutionary basis for selection toward such a loss.

We are pleased that Dornstreich agrees that the food habits of modern hunter-gatherers may not be reliable indexes of the possible extent of vege-

table consumption by preagricultural societies. His assertion that about 8 percent of the food plants eaten by the Gadio people in New Guinea are wild does not seem to strengthen the argument against our statement that the importance of vegetable foods to evolving man may have been overestimated by anthropologists. The fact that 50 percent of the vegetable foods of the !Kung are eaten uncooked has been published (3), and this vegetable food is known to be magongo nuts, which are available year-round in the range they occupy in Bechuanaland. We have pointed out the general acceptance among anthropologists that the scene of hominid evolution was the savannah, where tropical fruits would not be abundant throughout the year.

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Newborn Walking: Additional Data

Some additional data are pertinent to the controversy between Gotts (1) and Zelazo *et al.* concerning the work of the latter (2) on newborn walking. From birth, infants in an African foraging people (3) had extensive experience in vertical postures in a sling at the mother's side or held sitting or standing in the mother's lap. Although the newborn walking reflex was not systematically exercised, parents expressed the belief that motor milestones would not appear unless trained and attempted to train them in advance of maturation. These infants' neurological status at birth conformed closely to the European pattern, but they were advanced in sitting, standing up, and in the mature phase of walking compared to the American standardization sample (4) assessed by different investigators.

The simplest explanation for this change is that parental treatment accelerates development. This is the ex-

planation offered by Ainsworth (5) and Géber (6) for the precocity of the infants in their Ugandan sample. While genetic factors cannot be ruled out, one isolated finding (7) of a difference between European and African infants in neurological status at birth has been difficult to repeat, and the method used has been challenged (8). However, precocity of milestones is well established (9), and Géber and others have noted that African infants raised in a European manner do not show the same degree of precocity (8).

The study by Zelazo *et al.* is thus not an isolated or arbitrary sort of intervention. Parental behavior which has the effect of accelerating motor development is widespread. The facts that among Bushman foragers, as among other hunting and gathering peoples (10), infanticide was practiced to effect adequate birth spacing and that, because of their mobile subsistence pat-

tern, a new child was unwanted until the last child could walk well, combine to suggest that the practice of deliberately accelerating infant motor development may have considerable antiquity. Observations of Bushman newborns (3) also confirm the view of Zelazo *et al.* that in many infant care contexts the newborn reflex repertoire in general has functions (other than to be elicited by examiners) and that these functions may have survival value.

There are also cultures in which motor development is slower than (11) or simply different from (12) our own. Speedy motor development has no a priori claim to desirability. Its desirability is a matter for research. But what Zelazo *et al.* have suggested is well within the bounds of what at least some people have done with their babies, probably for many centuries. Only ethnocentrism makes it seem artificial to us. More important, they have used a research paradigm, that of intervention in a normal infant care context, quite different from the interventions in "deprived" and institutional settings with which we have become familiar. More research like theirs needs to be done, not only in motor maturation but in other areas of infant care and development. It makes sense to be guided in these interventions, at least to some degree, by the infant care practices of other cultures.

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