

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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13. I am grateful to Irven De Vore and Jerome Kagan for discussions of an earlier draft of this comment.

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