we are considering a population that is now doubling every 40 years. It is one thing to double the 1910 population of 60 million, quite another to double the 1960 population of 180 million.

My remarks on the American standard of living were, to a certain extent, made tongue-in-cheek. The rich flavor of chlorine in my drinking water, an open sewer named Clearwater Creek, the green scum and aroma of dead fish coming from my old swimming hole all tell me that something obscene has long since affected the *quality* of my "water standard of living." More important, perhaps, is the fact that, in the last 10 to 15 years, *quantity* of water has become an expanding problem in more and larger areas of the United States. There are very few major areas left where no water problem exists. Some places are in deep trouble. It therefore really takes no sophisticated mathematical insight to see that the limits of water supply in the nation as a whole, for the ways in which we are now using it, are practically at hand. In other words, I don't argue too much with Burton and Kates's manipulation of my figures since to me they merely suggest that we already have *passed* the peak in our water standard of living.

Regarding Gertel's comments, is he saying that increased yield of crops (forest and pasture included) per acre will not require a linear increase in *transpired* water, or does he perhaps mean that by more thorough plant cover and management a larger proportion of the rainfall can be shifted from evaporation to transpiration? If he means the former the statement should be documented. If he means the latter I agree. In fact I hinted in my article that herein lies our biggest opportunity to effect water conservation.

Gertel suggests that the "people versus water" picture is not as bleak as I have painted it. Would he care to apply his own figures toward answering the question posed in my article: How many more years can we sustain our present water standard of living with the projected population curve?

I realize that the timetable for Malthusian limits to be imposed on the population of the United States is not really foreseeable. I merely indicated that present population trends and present rate of rainfall would, in 200 years, bring us to the point of using all our rainfall to raise our food; I based the calculation, of course, on the transpiration ratios and the assumed diet of 2 pounds of bread and 1 pound of meat per day per person. I understand that the latest census studies indicate a slight leveling in the population growth curve, and this curve, of course, is the key to any calculation of timetables. Furthermore, I am certain that long before we begin to approach Malthusian limits we will not be insisting on a daily ration of steak. Any wholesale dietary shift from bread and meat to, say, marine plankton would make the transpiration ratios meaningless although few Americans today would construe such a shift as a gain in our standard of living.

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"Critical Periods" in the Development of Behavior

Scott's interesting article (*1*) on "critical periods" in behavioral development merits a thoroughgoing critical review bearing on the validity of his general conceptions of behavioral ontogeny basic to the idea which he has extrapolated from embryology to the study of behavior. Here, however, we comment specifically on certain inferences that might be drawn from his allusion to our recent article on behavioral development in cats (*2*).

In discussing his concept of critical periods, Scott reports us as having "suggested that there are critical stages of learning—that what has been learned at a particular time in development may be critical for whatever follows."

Although we are not disposed to dispute this broad statement, it is not ours. In our view, any such sentence should have a more comprehensive context, to the effect that what the young animal may attain in behavior at any phase of ontogeny depends upon the outcome of earlier development in its every aspect. The point we wish to emphasize here, however, is that our position might be seriously misunderstood in at least two important respects from Scott's allusion to our article. (i) Although, as our study of social behavior in newborn kittens (*2, 3*) indicated strongly, learning is involved at all phases in behavior development, our findings have broader and very different implications for social ontogeny than might be gathered from Scott's mention of the work. (ii) The

context indicates that Scott has misunderstood the criticisms we have offered (*2*, *3*) of his "critical periods" concept.

The gist of our objection is not that we favor describing three critical periods of social development, corresponding to the three main stages in the ontogeny of social-feeding behavior in kittens for which we found evidence, or whatever number of critical periods might be advanced as an alternative. We were interpreting our results from a viewpoint definitely at odds with Scott's notion of "critical periods" when we wrote (*2*): "These considerations favor a very different view of the concept of 'critical periods' from the one now held by many writers. In the social development of the cat, we are led to the idea that striking changes in the essential progression are grounded not only in the growth-dependent processes of maturation but also, at the same time, in opportunities for experience and learning arising in the standard female-litter situation. This conception of social ontogeny encourages stressing not just one or a few chronologically marked changes in the behavior pattern, but rather indicates that normally each age period is crucial for the development of particular aspects in a complex progressive pattern of adjustment." We consider the implications of this theoretical viewpoint for developmental research very different from those of Scott's concept.

Evidence supporting our view demonstrated that in kittens, at all age periods, social approaches preliminary to feeding behavior undergo a course of development in the litter situation significantly different from the behavior of kittens reared under conditions of isolation and fed from an "artificial mother" (*2*, *3*). No evidence was found for any time interval in which the different conditions of rearing failed to produce a pattern of feeding approaches and suckling in kittens reared in isolation that was significantly different from that in normally reared kittens of corresponding ages. This result had been predicted from the theory of social ontogeny (*4*) which guided our work.

Those who examine this theory (*4*, *5*) and related considerations (*2*, *6*, *7*) will find an emphasis upon the fusion of maturation (growth-contributed) and experience (stimulation-contributed) processes at different stages in

behavioral ontogeny, together with the contention that the contributions both of maturation and of experience (the latter including, but not confined to, conditioning and learning), as well as the interrelations of these contributions, may differ greatly according to stage in any animal. This theory thus differs sharply from Scott's, with its emphasis (see *8*) upon factors of maturation presumably specific for "critical periods" and its apparent assumption that "learning" is a distinct and probably a delayed contributor.

From our theory of behavioral development (*4–6*), we conclude that factors of maturation may differ significantly in their influence upon ontogeny, both in the nature and in the timing of their effects, according to what relations to the effects of experience are possible under the existing conditions. We found, in support of this view, that gains in suckling made by kittens reared in isolation differed greatly from gains made by litter mates reared by the mother, and that the kittens reared in isolation were, at best, only partially adapted to the demands of social feeding and suckling of the mother at the time of their return to the litter. The differences between the experimental animals and the control litter mates were striking. In no single phase of development during the first 2 months of life did these two very different conditions of rearing—with mother and litter or isolated, with an "artificial mother"—fail to have very different effects on the development of suckling responses, despite the presumable equivalence of potential factors of maturation for kittens in the two groups. The results indicate that the actual effects of maturation differed considerably in the two cases. We do not find such evidence compatible with the meaning of *maturation* that would seem to follow from the critical-period hypothesis.

What is social behavior? Scott states (*1*) that in puppies the period of socialization begins at approximately 3 weeks of age. We submit, however, that much of the evidence he cites bears only tangentially on the question of when socialization really begins. Although his article is mainly concerned with social behavior, it does not deal primarily with *intra*species behavioral relationships but deals, instead, with the responses of puppies to human handlers, of young birds to artifacts, and the like. Under the heading, "Process of

primary socialization" we find, for example, citations of tests in which machine-fed and hand-fed puppies "became equally attached to people . . .," hand-fed puppies "yelped more when they saw the experimenter . . .," "hungry puppies became more rapidly attached to the handlers . . . ," and "separating young puppies overnight from their mother and litter mates . . . speeded up the process of socialization to human handlers."

The degree of equivalence between such results and the ontogeny of interactions among species mates may prove to be appreciable, but it has not been determined. We suggest that Scott, in basing his principal treatment of social development upon experimental interventions featuring responses to conditions other than association with species mates has been led astray. In our studies of the social-feeding behavior of kittens we were able to distinguish early forms of intraspecies social responses not evident in reactions to human handlers or to artifacts such as the "artificial mother." A dependence upon the latter two sources of evidence might have led us to neglect aspects of intraspecies behavior which we found crucial for the understanding of social ontogeny.

With due emphasis upon intraspecies behavioral relations, we maintain that processes of socialization and formation of the social bond begin at birth, if not earlier (*2–6*). Kittens make consistent progress from an hour or two after birth in becoming oriented to their environment and to species mates, in becoming adjusted to the litter situation and the "home area," and in making individual, distinctive responses to particular nipples. Such aspects of behavior concern reciprocal relations of dependency with the mother and with litter mates, hence are social.

Because Scott was concerned with giving an account of the critical-period concept and illustrating it, evidence centered on the ontogeny of intraspecies responses may not have seemed relevant to him. Yet, because his reference to our article might imply that it supports his own concepts of behavioral development, we assert that there is an important difference between his view and our own.

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Statements of contrasting theoretical positions are useful if they stimulate the collection of additional data, leading in turn to the modification and evolution of theory. I am sure that the comments of Schneirla and Rosenblatt were offered in this spirit, as is my reply.

The points raised in their letter go far beyond the scope of my original article. I believe that most of them are complementary rather than contradictory.

1) Failure to find a critical period for primary socialization in the cat. In order to thoroughly understand the development of behavior in a given species, one should have timed data on the development of basic behavioral capacities, including sensory, motor, and learning capacities, as well as on the development of patterns of social behavior. In addition, one should have a description of adult social organization, to which behavioral development seems to be strongly related. In the absence of such general background information for the cat, I have been unable to evaluate Schneirla and Rosenblatt's data in terms of evidence for the existence or nonexistence of a critical period for primary socialization. Although cats are not highly social animals, I would be surprised if there were no such period in this animal, in view of the wide occurrence of the phenomenon in social birds and mammals, and even in insects.

2) Role of learning in behavioral development. With respect to the importance of learning in behavioral development, our points of view are supplementary rather than antithetic. Where Schneirla and Rosenblatt emphasize the fusion of maturation and learning, I would add that the learning process also changes with time and can itself be thought of as a developmental process. Much evidence indi-

cates that the rate of development of the capacity for learning varies widely from species to species, but additional information is badly needed along these lines. To my knowledge, in no species, not even the human, has a study of any simple learning capacity been carried completely through the stages of early development.

3) Nature of the process of socialization. Schneirla and Rosenblatt have concerned themselves with the point at which the process begins; my article dealt with the period during which the process proceeds most rapidly. Both kinds of information are necessary for understanding the process. As I said in the article, processes are not necessarily limited to the periods in which they are most prominent.

4) Experimental methodology. Two principal methods of studying the development of social behavior have been used: cross-fostering between species (or upon models) and social isolation. The two methods produce quite different results. In general, cross-fostering transfers social relationships but often has little effect on the development of social behavior patterns. Social isolation prevents the development of social relationships and may distort or inhibit the development of certain social behavior patterns. Both methods contribute to our understanding of the process of socialization. Schneirla and Rosenblatt observe that the cross-fostering method is more likely to reveal the existence of critical periods; this method has not yet been applied in the cat.

5) The nature of behavioral development. Two testable hypotheses can be stated (in stating them I do not ascribe either extreme of the two viewpoints to Schneirla and Rosenblatt). (i) Behavioral development is a unitary process, and what takes place in one period of development is dependent on everything that went before. (ii) Behavioral development may be composed of many different processes having a considerable degree of independence, both within the individual and between species. The actual facts probably lie somewhere between these two viewpoints.

Behavioral development is an extremely complex phenomenon. In interpreting any given set of data it is important that alternative hypotheses be considered and eventually tested.

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