Each behavior is a product of heredity and experience

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Article:

In "Phylogeny" Skinner makes two points quite effectively. First, he argues that it is not possible to know either the phylogenetic or the ontogenetic origin of a behavior from knowledge of its apparent purposiveness or adaptiveness, Second, he maintains that ontogenetic contingencies can be well investigated and understood without analyzing the phylogenetic contingencies that may have contributed to the evolution of the behavioral processes. By this he does not mean that species or strain differences should be ignored or that exhaustive knowledge of one mammalian species can substitute for in-depth study of other mammals. Although his earlier writings did imply these erroneous views, this 1966 article does not.

When Skinner ventures an opinion on the relation between heredity, experience, and behavior, however, he seems to concur with the views of the "classical" ethologists. Lorenz (1981) and Eibl-Eibesfeldt (1979), for example, stubbornly maintain that a behavior that is resistant to modification by experience is therefore innate and genetically encoded. This view is false. There is no necessary relation between the modifiability of a behavior during ontogeny and its degree of heritability in a population. In some instances the symptoms of a genetic disease may be altered dramatically by changing the diet, as in certain forms of diabetes (Lee & Bressler 1981), whereas a morphological characteristic that is established by environmental conditions during a critical period of development may later be almost impossible to change, such as the determination of the sex of certain reptiles by nest temperature during incubation (Bull & Vogt 1979; Ferguson & Joanen 1982). For this reason, as well as others discussed previously (Wahlsten 19791, the only way to demonstrate a genetic influence on behavior or morphology is to do a genetic experiment whereby the genetic material is modified.

Skinner states that "genetic variables may be assessed . . by studying organisms upon which the environment has had little opportunity to act (because they are newborn or have been reared in a controlled environment)" He is wrong for the same reasons the ethologists are wrong. He should also note that the organism's environment begins to act when the organism begins to exist, at conception. A vast number of environmental influences on the development of vertebrate animals prior to birth or hatching have been documented.

There are several statements in "Phylogeny" that indicate that Skinner agrees with the view that there are two mutually exclusive and exhaustive categories of behavior, inherited and acquired. This view is also false. First of all, no behavior is inherited, Skinner recognizes this problem and qualifies his statements about "inherited behavior" by suggesting that "bodily features" are actually inherited or genetically specified. In fact, no characteristic of the adult organism is inherited as such. Substances comprising the egg and, in most cases, the sperm are inherited, and these develop epigenetieally through interaction with the environment. The notion that specific phenotypes are inherited is a vestige of the preformationist view of development. Today it is valid only for single-celled organisms wherein body parts such as cilia are replicated and transmitted directly to "offspring" and persist in essentially the same form during the life of the animal (Nanney 1977). In vertebrates, the chromosomal genes are part of a complex chemical and physiological system which is inherited and develops. Mutation of a specific gene may modify the course of development and consequently modify later

behavior, but this does not mean that the normal form of the gene codes specifically for that behavior or for a brain structure that organizes the behavior (Stent 1981; Webster & Goodwin 1981).

Furthermore, a specific behavior is often found to be influenced by both heredity and experience. A behavior that appears to be instinctive, in that it is performed relatively competently on the first attempt, may be modified by experience. The work of Hailman (1969) on the pecking behavior of newly hatched gull chicks is a good example of this. The species-typical song of the cowbird occurs despite rearing by another species of songbird, but controlled experiments reveal that the song pattern can be changed by auditory experience (West, King & Eastzer 1981). The literature in behavioral genetics provides abundant evidence of behaviors modified by both heredity and learning (Wahlsten 1978).

Physical structures or "bodily features" are also modified by both heredity and experience. The living organism is not analogous to an electronic computer wherein there is a clear distinction between prewired "hardware" and acquired "software." Heredity does not code for brain structure in the sense that a wiring diagram or blueprint specifies how to arrange wires and transistors to make a computer. Memories in the adult brain can be stored without the growth of new axons or synapses, but experience can also change the organizational structure or wiring of the brain, especially during the formative period when the nervous system is becoming organized. Even the structure of bones can be altered dramatically by the early experience of the organism's own movement (Drachman & Sokoloff 1966). Two bones do not fit together and articulate well purely because genes code precisely for the shape of each one separately; rather, they are custom fit to each other during ontogeny through mutual contact and motion. Genes undoubtedly play an important role in the development of structures, but they are certainly not the sole source of information directing construction of a brain or a bone.

It seems to me that Skinner gave too much weight to the dogmatic opinions of Lorenz and adopted certain of them as his own without first exploring the extensive literature of behavioral genetics or developmental biology. Eighteen years ago the paper was thought provoking and timely, but its discussion of the problem of heredity, evolution, and behavior was not sufficiently precise or authoritative to make it a real classic in this area of study. It may be relevant that in the commentaries on the BBS article by Eibl-Eibesfeldt (1979) on human ethology, no one referred to Skinner's 1966 paper or any other work by him. Perhaps it is a little unfair to judge something written long ago in the light of extensive evidence and detailed discussions published subsequent to it. Perhaps it would be better to inquire whether Skinner still adheres to everything he wrote in the 1966 paper. The real measure of a scientist is the extent to which he responds adaptively to ontogenetic contingencies occasioned by new evidence and argumentation.