

Some logical fallacies in the classical ethological point of view

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Article:

E-E has written a concise exposition of the classical ethological view of human behavior. The ethology of Lorenz and his followers has been incisively criticized by Lehrman and others, but it appears from E-E's essay that these criticisms have brought about no major modifications of the thinking of classical ethology.

I would like to discuss certain logical fallacies that are evident in E-E's interpretations of key experiments and studies, specifically the deprivation experiment and the study of cross-culturally universal behaviors.

According to both E-E and Lorenz (*op. cit.*, 1965), if an individual is deprived of an opportunity to learn a species-typical behavior, be it a song or a smile, by observing conspecifics performing that behavior, and if that individual nonetheless performs the species-typical behavior, then the behavior must be "innate," "encoded in the genome" or the genetic "blueprint" for a nervous system. I maintain that it is fallacious to draw any conclusion about the genetic inheritance of a behavior pattern from an environmental deprivation experiment. The improper inference made by classical ethologists follows from a dogmatic restriction of the causes of species-typical behavior to two classes of phenomena: observation learning, or imitation, and genetic coding, presumably in DNA molecules.

If all possible causes of a behavior pattern can legitimately be dichotomized, then of course an experiment which yields results that exclude one cause. therefore compels the researcher to affirm the complementary cause, and it does so with all the authority of simple logical reasoning. If the possible causes cannot be neatly dichotomized, then no conclusion about the cause of a behavior can be drawn from an experiment which allows the exclusion of only one cause.

Suppose there is an urn known to contain only black and white marbles, If one marble is drawn and it is found to be not black, is therefore white. No further investigation is necessary to assert this conclusion, because prior investigation has already established that the entire stock of marbles from which the sample was drawn consists of only two kinds. However, if the urn contains black, white, red, green, yellow, and blue marbles, and if it is established only that a marble drawn from the urn is not black, then there is no way of determining its actual color without further investigation. It could quite plausibly be either white, red, green, yellow, or blue.

If we look at the life of an animal, it is obvious that a vast array of mechanisms *are* active in its development from conception to the time it first displays some species-typical behavior. Some well-known mechanisms include the following: (1) the set of chromosomes, or the "genome," determined at fertilization; (2) host of organelles and macromolecules in the cytoplasm of the zygote; (3) spatial configuration of the cellular components of the zygote; (4) the external environment of the zygote with its characteristic temperature, pH, salinity, osmolarity, and so forth; (5) environmental factors such as nutrients and viruses which are absorbed and in some cases assimilated by the organism; (6) the web of interactions among components within each cell during development; (7) the numerous interactions among the diverse cells comprising the developing

organism; (8) in birds and mammals, maternal or parental care; (9) exercise by the organism; and so on. A further mechanism may also be observation of conspecifics performing a species-typical behavior.

If a deprivation experiment is carefully conducted so that two random samples of animals from a single population are reared in two environments which differ in only one respect - opportunity to observe a conspecific performing a particular behavior - and if the animals in the deprived environment nonetheless perform the species-typical behavior, then one and only one thing can be concluded: observation of performance by a conspecific is *not necessary* for normal performance of the behavior. Such an experimental result does *not* prove that the behavior is encoded in the genes (mechanism 1 above). Neither does it prove it to be encoded in the Cytoplasmic organelles (mechanism 2) or any other of mechanisms 3 through 9 cited above.

This basic point has been made by Jensen (1961), Kuo (1967), Whalen (1971) and, most lucidly and eloquently, by Lehrman (*op. cit.*, 1953, 1970); yet the classical ethologists maintain their dogmatic view of the deprivation experiment.

When Lorenz and E-E conclude from this kind of result that the behavior is "encoded in the genome," they are in effect uncritically accepting the null hypothesis, which may constitute what is termed a Type II error in statistical inference. They in effect hypothesize *a priori* that the behavior is "innate", and from this null hypothesis they predict that the animals reared in normal and deprived environments will behave the same way. If there is no significant difference between the two groups, then they accept the null hypothesis as true. This is a serious error of logical reasoning.

The only way to draw a valid conclusion about the role of genes in species-typical behavior is to vary the genes themselves, perhaps through a mutation or selective breeding, and demonstrate that the behavior varies as a consequence. Because heredity consists of more than Mendelian genes in chromosomes special crossing experiments must be done to dissect the contributions of Mendelian inheritance from numerous other mechanisms of inheritance (see Wahlsten, 1979) and establish that the behavior is indeed "encoded in the genome."

Investigations of the songs of isolated birds, the babbling of human neonates, the smiles of the blind, or the greeting gestures of primitive peoples are relevant and informative for students of behavior, but none of these studies can provide proof that behavior is "encoded in the genome" or "innate." The deprivation experiment is only one of many techniques for the analysis of behavior, and the knowledge which can be gleaned from its results is narrowly circumscribed.

Consider further the case where experiential deprivation does indeed disrupt species-typical behavior. This certainly demonstrates the plasticity of the behavior in question, but it does not in any way prove that the "genome" is *irrelevant* for performance of the behavior. The chromosomes may play an important role in the development of a nervous system capable of rapidly acquiring information through observation.

It seems to me that the classical ethologists have responded to previous criticisms of their doctrine by adopting an eclectic approach in order to blunt the effects of further criticism. On the one hand, they stubbornly cling to their original ideas, while on the other, they incorporate criticisms into their writings in the form of disclaimers that they really mean what their words appear to mean. This is apparent in the essay by E-E.

For example, in his abstract he presents a very clear dichotomy of causes of behavior by asserting that "innate and culturally evolved patterns of behavior can often substitute as functional equivalents for one another." Then at the end of section 3 he contradicts himself: "I wish to emphasize that the whole nature/nurture issue should not be considered as a matter of either/or, nor can the contribution of each be measured quantitatively, in terms of percentages." Now, what is *substitution*, if not a matter of "either/or?"

In the section on cross-cultural comparison, E-E begins with this disclaimer: "The fact of their universal appearance alone does not allow us to deduce that these patterns are innate in man." There seems to be a two-step process of reasoning implicit in this statement. First is the inductive inference that a behavioral pattern which appears in every one of a sample of cultures is universal, that is, occurs with probability 1.0. Second is the deduction drawn from the classical ethological theory that universality means the pattern is innate.

An inductive inference may be made on the basis of "circumstantial" evidence, and it always has a certain probability of being incorrect because of sampling error. A deduction from theory, on the other hand, should be made with mathematical certainty. It is a very inadequate theory that yields deductions which are only "probable," such that they may or may not follow from the theory.

Consequently, E-E becomes mired when he further states that patterns of expressive behavior which are virtually the same in all respects in many cultures provide "strongly suggestive circumstantial evidence for the hypothesis that they constitute phylogenetic adaptations," and when he speaks of "universals which can be said, with a high degree of probability, to constitute phylogenetic adaptations." These phrases clearly show that E-E has no doubts that the patterns are universal. Rather, he vacillates over the statement: Universal, therefore innate.

Evidence presented in this essay and other publications by E-E is not sufficient to convince me that the behavioral patterns in question are in fact universal, but in any event, the universality of a behavior does not prove its genetic encoding. The method of cross-cultural comparison has the same drawback as the deprivation experiment; it provides no direct evidence for genetic determination of the specific pattern of behavior. At best it provides "circumstantial evidence" in support of the hypothesis of "innate" behavior. Nevertheless, circumstantial evidence, unlike direct evidence obtained by manipulating genes themselves, cannot constitute proof that the hypothesis is true. It is logically fallacious to assert otherwise.