CHAPTER 3

From Watsonian Behaviorism to Behavior Epigenetics

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If we accept mechanistic and non-Darwinian concepts of behavior as basic to behaviorism, we must consider both Jacques Loeb (Loeb, 1889, 1918) and John B. Watson as the pioneer behaviorists. The word "behaviorism" was coined by Watson (1913, 1914), and the movement was, at first, a methodological revolt rather than an attempt to build a new system of psychology. At that time, psychology was still the handmaiden of philosophy, and Watson himself had been trained by philosophically-oriented psychologists, although his own researches were mainly on animal behavior. (In this respect, Loeb, as an experimental biologist, was freer from the bondage of philosophy.) Soon such philosophers as Bertrand Russell and Ralph Barton Perry became interested in behaviorism, and psychologists like Tolman, Weiss, Lashley, and even Watson himself attempted to give behavioristic interpretations of the mental entities – thinking, memory, and the like – losing sight of the original aim of the behavioristic movement. To call thought processes subvocal speech (Watson, 1914, 1919); to give behavioristic interpretations of consciousness, ideas, memory, and the like (e.g., Tolman, 1951); to substitute for mental faculties various new names of "hypothetical constructs"; to devise ingenious and elaborate schema of cerebral structures, applying the principles of electronic computers or information processing; to explain reasoning, intelligence, memory, etc., or to rename "volition" or "will" as drive or motivation (with physiological implications): all these are philosophical speculations at best. They hardly help us solve any basic problems in behavior. Instead, the behaviorist should concentrate his efforts on behavioral problems, leaving all the problems of mental concepts alone; these, as a matter of fact, should be imperceptibly and ultimately absorbed, never to reappear, as our knowledge of behavior advances and broadens and as our experimental techniques, especially the biophysical and biochemical ones, are improved.

The fundamental error of the behaviorists, therefore, was that they paid too much attention to the claims of philosophers and philosophically-oriented psychologists and tried to use objective or physiological constructs as replacements for the names of the old mental entities. Watson's greatest contribution to the behavioristic movement was not his hypothetical constructs, but his repudiation of introspection as a scientific method, his pioneer work in the study of the behavior of newborn infants, and this statement in the forceful and courageous preface to his 1919 book *Psychology from the Standpoint of a Behaviorist:*

From Zing-Yang Kuo, *The Dynamics of Behavior Development* (New York: Plenum Press, 1967), pp. 3–26.

> The key which will unlock the door of any other scientific structure will unlock the door of psychology. The differences among the various sciences now are only those necessitated by the division of labor. Until psychology recognizes this and discards everything which cannot be stated in the universal terms of science, she does not deserve her place in the sun . . . nor does the author claim behavior psychology as a creation of his own. It has had rapid development and is a direct outgrowth of the work on animal behavior. It is purely an American production. . . .

On the other hand, the concepts of tropism (taxis) reflex and of instinct and learning and the stimulus and response (S-R) formula, which may appear to us now as extreme oversimplifications or as obsolete, refer to real behavioral problems and, as such, must be carefully examined and investigated by the behaviorists before they can be discarded and replaced by newer and broader concepts of behavior.

The Descendants of Watsonian Behaviorism

Watson's S-R formula and his almost unquestioning acceptance of the concept of conditioned reflex as the key to the study of behavior were rather unfortunate because of their oversimplification of the phenomena of behavior. However, with some minor variations such as Tolman's "purposive behaviorism," the S-R formula and conditioned reflex concept have become the standard-bearer of behaviorism and the keynote of American animal psychology for the last fifty years. Its main thesis is animal learning. Its basic principle is conditioning – Pavlovian or Skinnerian. And the animal used in learning experiments has been mainly, if not exclusively, the albino rat. Theories of learning may vary, but none has been diverted from Watson's original S-R formula.

Whether or not one agrees with the fundamental views of the psychologists of learning, one must admit that the descendants of Watsonian behaviorism have evolved what has now become an American tradition in psychology. There are two aspects of this tradition: (1) The strictly experimental attitude which has resulted in the development of laboratory techniques with rigid standards and elaborate procedures, and (2) mathematical-mindedness. The statistical requirements for any animal experiment have become so rigid and so sophisticated that no editor of an American technical periodical will accept any paper on animal behavior without adequate statistical data. These research techniques constitute a very commendable contribution, and no coming student of behavior, whatever his scientific outlook, can afford not to master them before he begins his research on animals. And many younger European workers on animal behavior have just begun to appreciate this scientific achievement of the American animal psychologists.

However, it must be admitted that much American experimental work, especially the learning research on rats, has been carried out in a very narrowly confined environment so that conclusions from the results of such experiments may not be accepted without a great many reservations. Moreover, the stress on mathematics has gone beyond the bounds of scientific proportion. Although recent progress in mathematical physics and computer engineering has been rapid, the phenomena of behavior are far more complex than physical events, and the science of behavior is still in its infancy in comparison with the very advanced stage of the physical sciences. For these reasons, I feel strongly that it is premature to devise "mathematical models" for the prediction of behavior. If many learning theorists have apparently succeeded in their mathematical predictions, that can be explained by the fact that they have ignored the great complexity and variability of behavior and have reduced behavioral phenomena to an extremely few simple parameters. For example, the behavior of a fly would be a very complicated affair if we took into account the complexity, variability, and interplay of such determining factors as morphology, biophysics, biochemistry, developmental history, environmental context and the various characteristics of the stimulating objects. However, if we simply put a fly in a very small blackened box with two small openings both penetrated by outside light and send a stream of cold air through one opening and a stream of warm air through the other, we could predict with almost mathematical accuracy by which of the two openings the fly would escape from the box and in how many trials. But, in so doing, we must ignore other details of the fly's activities inside the black box. In other words, the success of such an experimental prediction is due not to the accuracy of mathematical theories or models, but rather to the oversimplification of the experimental situation in animal learning.

The Unsolved Problem of Behaviorism

Because the behaviorists have confined themselves to methodological approaches without broadening their outlook on behavior and raising their scientific horizon beyond the simple S-R formula and the concept of conditioned reflex, they have been forced to fall back on the problem of the "psyche." If even learning cannot be explained without postulating motivation, learning set, or modification of cerebral structures, no behaviorist can avoid the problem of intervening variables. To call them physiological counterparts of behavior neither explains behavior nor satisfies the demands of the mentalists. Or to admit the existence of neural processes, while denying the relevance of the neural connection between stimulus and response, as does the operant behaviorist, is a scientific evasion at best. The protestation against behaviorism from philosophers, mentalistic psychologists, or even social scientists (cf. the symposium on "The Limits of Behaviorism in Political Science," American Academy of Political and Social Science, Philadelphia, 1962) may be ignored. But when a physiologist like Adrian complains that he cannot understand "thought" without reference to the mind or when the psychologist of animal learning insists that he, too, needs some such concepts as motivation or learning set in order to explain the learning process, it seems that the time has come for us to re-examine the current scientific outlook of physiology and reassess the basic scientific value of the behaviorists' S-R formula. Perhaps the merger of physiology with the science of behavior, with a much more broadened prospectus, may lead to the eventual dissolution of the problem of intervening variables.

Gestalt Psychology as a Protest Against Behaviorism

In reviewing the history of behaviorism it may be fitting to say a few words about Gestalt psychology as a countermovement against it before World War II. The Gestalt psychologists' criticism of the oversimplification of the S-R formula and the concepts of reflex and chain reflexes was valid. Nevertheless, I feel compelled to part company with them in practically every phase of their treatment of animal behavior. This, even though my concepts of behavioral gradients and environmental context may appear to some as another holistic approach. As a matter of fact, both concepts are not only experimentally analyzable, but can be analyzed only by the atomistic methods that have been anathematized by the Gestalt psychologists.

It should also be noted that, besides being predeterministic concerning development, the Gestalt concept of behavior is not free from vitalistic implications.

The Protest of Ethology

Although C. O. Whitman and O. Heinroth and various others have been credited as pioneers in the field, it was Konrad Z. Lorenz and Niko Tinbergen who systematically conceptualized ethology. Ethology was unknown in America before World War II, yet its impact on the thinking and research of American behavioral scientists was no less dramatic than Gestalt psychology had been in the late 1920s and the 1930s. Like the Gestalt psychologists, the ethologists' chief target was American behaviorism in the current sense. The main principles of ethology consist of revival of the concept of instinct and an emphasis on (1) naturalistic observation, (2) social aspects of behavior, (3) species-specificity, (4) the adaptive value of behavior for the survival of the species, and (5) the need for a diversification of animal species in behavior studies. These are all aimed at American learning psychologists and perhaps rightly so, especially at those who had confined themselves to running rats in mazes.

Behaviorism Reconsidered

In reviewing the history of the behavioristic movement one most important aspect has been long overlooked: the ontogenetic implications. These were clear to me in the basic tenets of behaviorism, in Watson's own later interest in the behavior of human neonates, in the vigorous anti-instinct campaign in the 1920s, and in Watson's effort (1924) to catch up with the anti-instinct trend. An objective and unbiased historian of science should have been able in the late 1920s to forecast the future trend of development of the original Watsonian behaviorism, namely, the ontogenetic approach to the study of animal behavior. Despite theoretical differences that were an inevitable historical outcome, the great proliferation of studies of prenatal behavior of various vertebrates and of newborn infants in the 1930s was not merely a coincidence. The studies of various aspects of the ontogeny of postnatal behavior after World War II must be regarded partly as a continuation of the 1930s trend and partly as silent (in most cases) protest against the rat-learning psychologists from Tolman to Hull, Skinner, and their followers, who seem to have deviated from the original tenets of Watsonian behaviorism.

"The basic aim of this book is to present the epigenetic point of view as a logical crystallization of the continuous and truly behavioristic trends initiated by Loeb and Watson; it is an attempt to replace the versions of Tolman, Hull, Skinner, and the learning theorists on the one hand and the main concepts of behavior advocated by the ethologists on the other." In what follows we shall discuss the various aspects of this viewpoint, illustrated, whenever possible, with experimental or observational evidence; at the same time, we shall suggest a new orientation for future behavioral studies.

The Meaning of Epigenesis in Behavior

We shall define behavioral epigenesis as a continuous developmental process from fertilization through birth to death, involving proliferation, diversification, and modification of behavior patterns both in space and in time, as a result of the continuous dynamic exchange of energy between the developing organism and its environment, endogenous and exogenous. The ontogenesis of behavior is a continuous stream of activities whose patterns vary or are modified in response to changes in the effective stimulation by the environment. In these epigenetic processes, at every point of energy exchange, a new relationship between the organism and the environment is established; the organism is no longer the same organism and the environment no longer the same environment as they were at the previous moment. Thus, in ontogenesis, both patterns of behavior and patterns of the environment affect each other and are therefore in a constant state of flux; that is, changes in the environmental patterns produce changes in behavior patterns which in turn modify the patterns of environment. The epigenetic view of behavior is bidirectionalistic (Gottlieb, 1970) rather than environmentalistic, as it considers every behavior pattern as a *functional product* of the dynamic relationship between the organism and its environment, rather than as a passive result of environmental stimulation. In other words, the epigenesis of behavior is a continuum of the dynamic process of interlocking reactions between the organism and the environment, resulting in the reorganization or modification of the existing patterns of both the behavioral gradients and the environmental context. At the same time, it does not violate the current views of heredity held by physiological geneticists and experimental morphologists, according to whom ". . . the phenotype is more and more considered not as a mosaic of individual gene-controlled characters but as the joint product of a complex interacting system, the total epigenotype" (Mayr, 1963, p. 6). The epigenetic behaviorist accepts the morphological structures and their functional capabilities at a given stage as the end result of development in the preceding stages, but considers them merely as one of the five groups of determiners of behavior. The higher the phyletic level (especially, when there is a complex social life) and the more advanced the morphogenesis, the more important are the other determining factors in shaping or modifying behavior patterns. In vertebrates, and in birds and mammals in particular, as ontogenesis progresses and as the developing animal widens its environmental contacts, especially through sensory inputs from the distance receptors, more new patterns of behavior are required from time to time to meet the new demands of the continuing changes of the environment. Such new patterns are determined mainly by the developmental history or historical antecedents of the animal, the existing pattern of behavioral gradients, and the environmental context; morphological structures and their functional capabilities act as determining factors of behavior only in a negative way, that is, they merely set a limit to certain body movements (for example, a dog can only snarl at or bite its enemy but cannot throw a stone at him).

Some Salient Points on Behavioral Epigenesis

The following comments should help to clarify the epigenetic view of behavior and serve as a guide for future investigations of behavior:

1. The behavior of animal and man is a continuous stream of activity from fertilization to death. In such a continuous process of energy exchange between the organism and the environment, behavior never comes to an end. If there is an end in behavior, that end must be death for death is the end of all ends. However, in this continuum, behavior patterns are in a process of perpetual change – from moment to moment, from sleep to wakening, from eating and drinking to defecation and micturition, from combat to copulation, from laying eggs to brooding and feeding the young, and so on. But for the sake of scientific convenience, we often take only a very small fraction of such ceaselessly changing events for investigation. We are justified in doing so only if we are fully aware that we are not reading any teleological meaning into such a small fraction of ever-changing behavioral events. Neither courtship, threat, fighting, hoarding, nor learning a T-maze to discriminate visual patterns is an "intended," "driven," or "motivated" behavior, for no behavior pattern or series of patterns is an entity, nor does it have any "goal" to reach. When we say that the animal has reached its "goal" (e.g., the food box or copulation), it is an anthropomorphic statement of the fact that the sequence of behavioral events under observation has undergone a shift. In this situation, the behaving organism itself has no purpose, but its stream of activities is arbitrarily sectionalized or fractionalized to serve the purpose or the observer or experimenter. Furthermore, in ontogenesis, no movement is a preparatory act to serve a future purpose of the animal. Thus the beak movements of the chick embryo are no more a preparatory action for postnatal food getting than for crying in fright or pain, crowing, grooming, or fighting, etc. The epigeneticist does not entertain any concept of finalism in behavior. In short, behavior begins with heartbeats and ends when all cardiac movement stops; this is the true and only finalism of behavior.

2. Both the American animal psychologists and the European ethologists have made two basic assumptions: the uniformity of nature (environment) and the uniformity of behavior. Both assumptions are based on inadequate observation.

As far as environment is concerned, light, sound, temperature, humidity, wind force, and the chemical composition of the air are in a state of constant change, except in an elaborately and mechanically controlled laboratory room. When social environment is added, the variation is far greater. Even the effectiveness of the stimulating object varies from moment to moment. If we include intraorganic or endogenous stimulation as part of the animal's environment, the environmental variability becomes far more complex. And when we come to the animal's environment in nature or in the field, environmental changes are far beyond the manipulation of the observer. I have been a bird-watcher since childhood, yet I have not seen two birds in the same nest grow up under the same natural conditions; as a result, no two birds react to the same stimulating object in an identical manner. But biased by an a priori assumption of uniformity, students of behavior are often apt to overlook or ignore the variability of environment and its effects on behavior.

To emphasize the significance of the variability of behavior let us illustrate it somewhat in detail with a single case from our unpublished records on fighting behavior in the dog.

The dog in question was a smooth-haired Shan Chow named Bobby who was selected for training as a fighter. He had all the anatomical qualities of a good fighter. After training, Bobby would always attack any dog in sight, regardless of sex. However, his fighting behavior manifested the following variations:

- If the dog attacked offered no resistance, Bobby would grab him by the back of the neck, shake him for a minute or less, and then let him loose.
- If the victim happened to fall to the ground on his back, Bobby would grab his throat and shake him until the dog was motionless or dead.
- If the other dog offered resistance, fighting would be very fierce. Bobby would never stop until the other dog was either killed or so injured

that he became almost motionless, or until the fighting was interrupted by the trainer.

If the other dog's resistance was not easily overcome, fighting became furious and continued until the victim was killed or both dogs were seriously wounded and exhausted, or until they were stopped by the trainer.

If the first fight did not end decisively, Bobby and his opponent would fight so ferociously on the second encounter that the trainer and his assistant would be unable to separate them except by ducking both dogs into a pond. Climbing up from the pond, both would rush to fight again until Bobby finally succeeded in killing or subduing his enemy.

When Bobby was attacking another dog and some other dogs who were his friendly companions came to his assistance, he would give up his victim and walk away.

When Bobby was a small pup, he had been attacked by another fierce dog. Later, after he had grown up to be the strongest dog in the laboratory, Bobby would always walk away from his old attacker whenever he saw him in the distance. However, on one occasion, he was too close to avoid him; the other dog then attacked Bobby but proved to be no match for him. Fighting was ended by the trainer's intervention. From this time on, Bobby would rush after his old enemy whenever he was in sight.

Bobby was trained to attack any female on sight. But, on one occasion, he rushed to attack a female in heat, and he was changed into a totally different dog. After several faulty attempts, Bobby succeeded in his first copulation. From that time on, Bobby never attacked any bitch whether in heat or not.

When feeding was two to three hours overdue, Bobby was very reluctant to engage in any fighting.

After two or three copulations in a day, Bobby would not initiate any fighting unless he was attacked.

His tendency to attack other dogs was reduced by about a third during extremely cold or hot days.

If Bobby was five or more pounds heavier than his usual weight, he was much less aggressive, his fighting was much less fierce, and it did not last as long as usual.

- When Bobby was between ten and twenty days old, the trainer used the sound of a Chinese rattle to frighten him. After he grew up to be a fighter, the sound of the rattle would always stop him when he was rushing after another dog. However, once his attack was already in progress, the rattle became ineffective.
- Soon after a heavy engagement in fighting, Bobby was reluctant to get involved in another battle.

This account of the variation in the fighting patterns of this dog has been confined to the level of gross activities. We have not touched upon variations in the biophysical and biochemical components of the patterns. These would make our description of the fighting behavior even more complex.

In fact, this description of Bobby's fighting patterns is an extreme oversimplification of the variability of behavior. Bobby never attacked the same dog twice in the same manner. He might make a frontal attack, leap on the other's back, grab its neck, or ear, or push it to the ground and grab and seize any part of the victim's body, etc. Furthermore, his attack varied with the reaction of his victim. A really stereotyped response pattern, especially in the higher vertebrates, hardly exists.

The seemingly uniform responses we may sometimes observe are more apparent than real. Not only does the same organism not make two identical responses to an identical stimulating object, but even in such a seemingly simple movement as the lifting of the head of a four or five day old chick embryo, its amplitude and extent are never twice the same. The number of muscle fibers and their neural connections involved in the head lifting, the extent of the muscular contractions, the energy consumption, and other physical and chemical changes vary from moment to moment. We must take all these variations into consideration if the science of behavior is not to remain on the crude descriptive level of gross bodily movement.

3. The developmental history of the animal is of utmost importance.

4. In view of the variability of behavior and of environment and the diversification of historical antecedents among individuals of the same species,

we believe that no two individuals of the same species acquire in their life history the same behavioral repertoire, except those motor activities which are the direct results of morphological structures and their functional capabilities. In animals of lower phyletic levels and in embryos or fetuses, in which environmental variations within the shell or uterus are more or less standardized and anatomical and chemical factors play a comparatively more important role in determining behavior, there are greater similarities in behavior among the growing animals of the same species. In such instances there may be some justification in calling such prenatal behavior or behaviors of lower organisms, *species-typical* or *species-characteristic* (both are better terms than "species-specific," as used by many ethologists and American animal psychologists with the implication of predeterminism).

On the other hand, when we come to postnatal development, except for certain movements, such as modes of locomotion or eating or drinking which are directly determined by anatomical structures or their functional differences, the organization of behavior patterns is so varied and so diversified that we can hardly group them into categories and call them species-specific unless we project our subjective, purpose-directed aims into the end results of the patterns, ignoring the variables of such patterns. In other words, unless we project human ends into the classification of animal behavior and disregard all the variables in environment and developmental history, it does not seem possible for students of behavior to arrive at a behavioral inventory or repertoire common to all vertebrate animals of the same species. Our objective should be to discover the behavioral repertoire of the individual animal and its causal factors rather than that of the species. On the same grounds we shall raise questions with regard to behavioral genetics.

5. In methodology, the behavioral epigeneticist insists on strict laboratory procedure and, at the same time, makes use of field or naturalistic observations as a means of discovering problems of behavior to be solved in the laboratory. However, we must point out emphatically that it is a dangerous procedure to arrive at any scientific generalizations from naturalistic observations, especially when such observations are of short duration, because the observer may not have seen enough of the variability of behavior and environment in the field.

Lorenz's observations on the fighting behavior of wolves and chow dogs is a case in point. He reports that fighting within either of these species almost always ends harmlessly for both combatants. Whenever two animals are engaged in combat, the weaker one soon lies on the ground and exposes its throat while the stronger one climbs over it, and both bark. The victor then walks away with an appearance of dignity and makes no further attack on the vanquished. Lorenz concludes that these behavior patterns result from an evolutionary process and are of great value for the preservation of the species. I have never observed wolves fighting. However, since my childhood I have been observing the fighting behavior of chows, and since 1928 I have conducted a large number of controlled experiments on the fighting behavior of these animals. Apart from such experimental fights, I have kept no record, but I have undoubtedly seen over one thousand of these fights. Therefore, on the basis of observations and experiments on these animals as well as on other species, I have concluded that animal fighting is so complex and variable, and involves the interwoven reactions of so many factors, that it would be most unscientific to follow Lorenz and single out one pattern of fighting and chivalry and explain it in Darwinian terms while ignoring many other fighting patterns. The case of Bobby previously described is evidence enough. Suffice it to state here that there are indeed certain situations in which the weaker dog saves itself from injury by lying on its back, a truly successful application of Gandhi's philosophy of nonresistance or Russell's pacifism through submission: "Better Red than dead." However, there are also many circumstances in which such a response is an invitation to certain death, as it gives the top dog an opportunity to grab the underdog's throat and shake it until the underdog is dead or at least shows no sign of bodily movement. I have seen many cases in the field and in my laboratory of truly pacifist and nonresistant chow dogs dying after they had behaved in the fashion Lorenz described. A remarkable product of adaptive behavior through evolution and natural selection, indeed!

As far as my own observations are concerned, when dogs fight in the manner Lorenz describes, both combatants belong to the class of what I used to call the Gandhi variety. Both the top dog and the underdog lack the anatomical elements to be good fighters. As a matter of fact, the top dog of this type has never engaged in a real fight in its life and is disqualified for training in fighting. Whenever it meets a stronger and more aggressive dog, it immediately becomes submissive and readily accepts the role of the underdog, although it will reverse its role when confronted with a still weaker and more submissive one. Chow dogs of this type in the Chinese villages are mostly underfed, look unhealthy, and have a great variety of skin diseases and verminous infestation. All such dogs are true to the type observed and reported by Lorenz. The chow dogs he observed are probably family pets; unlike the Chinese peasants, no Western dog lover will allow his pup to fight with his neighbors' dogs to the finish. One who was familiar with the developmental history and the anatomical make-up of the dogs Lorenz observed would probably have arrived at a different conclusion. Personally, I have witnessed scores of dog fights which, unless forcibly interrupted by men, would always end in either death or severe injury to one or both combatants. These cases do not include the dogs with special training for fighting in my laboratory.

Let me illustrate my point with another naturalistic observation. For some decades I have been observing the behavioral relationship between the common magpie and common eagle in South China. The magpie is much less than one-half the size of the eagle, and its wing span is hardly more than one-third of the eagle's. Moreover, its beak and claws lack the sharpness of the eagles' hook-like beak and talons. Nonetheless, every time I hear an eagle cry in pain, I am sure, even without looking up in the sky, that the eagle must be flying low and that a magpie must be making diving attacks on the back of its head or neck. The eagle is absolutely defenseless until it climbs up in the air beyond the reach of the magpie. On the other hand, when an eagle is resting on a roof top or the top of a tree, a magpie might come along, hopping back and forth on its long legs in front of the eagle, making typical magpie noises, and often coming within one foot of the eagle. However, neither bird appears to be concerned about the presence of the other. There is no sign of territorial dispute, neither threat nor hostility, nor friendliness. Each simply "ignores" the presence of the other. In both cases, the behavior of the two birds appears stereotyped and species-specific as far as my observation is concerned; the ethologists would, therefore, say that this behavior must be taken as genetically determined through natural selection. However, the type of behavior pattern on the roof or tree top and the type in the air seen incompatible. One favors the notion of interspecies coexistence, the other interspecies struggle. But why have both types been preserved? Have they survival value at different moments? To me this is not a question of natural selection, nor does it have anything to do with the question of survival value for either species. It is a question of the ontogeny of behavior during which different environmental complexes have brought about both types of rather incompatible patterns. This is an experimental problem to be tested in the laboratory so as to determine the actual environmental–ontogenetic factors responsible for the development of such incompatible patterns. If contradictory patterns of behavior exist in two species even in nature as is the case between the magpie and the eagle, it at least indicates that animal life in nature is far more complex and variable than the naturalists have thought.

In sum, naturalistic observations are valuable only insofar as they help the student of behavior broaden his outlook so that in devising experimental programs he will look beyond the narrow confines of his laboratory animal.

Five Groups of Determining Factors

Despite the great variability and complexity of behavior, for the convenience of investigation and description we may classify the determining factors of behavior into the following five categories:

- Morphological factors
- Biophysical and biochemical factors
- Stimulating objects
- Developmental history
- Environmental context

Every response of the animal is the *functional product* of the combined effects of the interwoven reactions of these five factoral groups through inputs and feedbacks and their organic trace effects (Schneirla, 1965). Our present knowledge concerning any of these factors and their interrelations is not adequate to devise any useful mathematical formula for the prediction of behavior. The behavioral epigeneticist hopes for the day when he will be able, like the theoretical physicist, to arrive at such a formula even though he may have to wait for some decades. His hope is based on the fact that, notwithstanding such great variability and complexity, there are to be found some common factors in behavior such as those due to some common morphological characteristics of the species. For example, morphological structures of the limbs determine the modes of locomotion; the oral structure determines the modes of eating and drinking, the vocal apparatus determines the characteristics of voice and singing. Moreover, there are certain common characteristics both in the developmental history (e.g., in prenatal life most animals develop under a more or less standardized environment) and in the environmental context. We can use such common characteristics as the base upon which we should be able to build our correlations with the biochemical and biophysical factors as well as with the varying characteristics of the stimulating objects (all of which also have certain constant characteristics) in order to arrive at a tentative formula. This would probably be an appropriate and relatively fast approach toward the goal of prediction and control of behavior and ultimately toward the creation of behavioral neo-phenotypes, that is, novel behavior patterns, unknown or unobserved before.

I have italicized the words *functional product*, to stress the fact that behavior is not merely an accumulated or arithmetical sum of these five categories of determinants. Thus, if we were to designate Beh to represent behavior; A to represent biophysical and biochemical factors; B, morphological factors; C, developmental history; D, stimulating objects; and E, environmental context, this would be the formula for behavior: $\text{Beh} = (A + B + C + D + E)$. But this formula would be much too simple to make any scientific sense. We must bear in mind that each category of the determining factors comprises an enormous number of variables. The interwoven reactions among these variables themselves are complex enough. But when those reactions of the variables of all five categories are brought together, it may be far beyond the capability of ordinary statistical methods to arrive at an adequate formula. Unless we deal with the behavior of unicellular animals or animals without social life, it may require many years, even decades – especially when we deal with the species known as *Homo sapiens* with its complicated language capacities and social cultures – before any attempt at a mathematical formula for the prediction of behavior or even aspects of behavior would be justified.

While it is fully justifiable for the behavioral scientist to limit his investigations to one level, or even to certain aspects of one level, of behavior (for example, various aspects of learning or conditioning; effects of early experiences on later life; "imprinting"; effects of cortical lesions on learning; so-called critical periods, etc.), we must not forget the fact that our knowledge of behavior based on the results of such investigations is negligible. Any attempt to draw conclusions from such studies and to use them as principles of behavior in general or as the basis for theories of behavior is bound to be hampered immediately by a large number of problems that may be insoluble, unless we broaden and reorient the outlook of behavioral studies.

Conclusion

Thus, from the standpoint of the epigenetic behaviorist, the relationship between the behaving organism and its environment is an extremely complex and variable dynamic process. It goes deeper and beyond the molar level. As we shall see more clearly in the following chapters, *behavior* is far more than the visible muscular movements. Besides such movements, the morphological aspect, the physiological (biophysical and biochemical) changes, the developmental history of the animal, and the everchanging environmental context are interwoven events which are essential and integral parts of behavior. In our study of behavior all such events must be investigated in a coordinated way. The behavioral process is not just a stimulus–response relationship, a conditioning process, nor just a revelation of innate actions in the form of "courtship," "threat," food-begging, egg-rolling in the nest, and the like. In other words, *the study of behavior is a synthetic science*. It includes comparative anatomy, comparative embryology, comparative physiology (in the biophysical and biochemical sense), experimental morphology, and the qualitative and quantitative analysis of the dynamic relationship between the organism and the external physical and social environment.

The chief objective of the epigenetic behaviorist is to seek order out of such complex behavioral phenomena in order to formulate laws of behavior without resorting to vitalism, either explicitly or implicitly. He or she has two main tasks: to obtain a comprehensive picture of the behavioral repertoire of the individual and its causal factors from stage to stage during development; and to explore the potentials and limitations of new behavior patterns ("behavioral neo-phenotypes") that are not commonly observed or do not exist in "nature" so as to predict or control the evolution of behavior in the future.

This is the main theme of radical behaviorism from the epigenetic standpoint. In the light of this theme, facts derived from experiments and observations are presented and interpreted, and certain more or less unconventional concepts of behavior are developed and discussed in the following chapters.

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