Konrad Lorenz 1960

Methods of Approach to the Problems of Behavior

In: The Harvey Lectures, 1958-1959. New York: Academic Press. pp. 60-103.

Methods of Approach to the Problems of Behavior*

It is my belief that the central importance of a Harvey lecture imposes upon me the duty to speak about problems of central importance, and the most important task I can set to myself on a trip to the United States is paving the way to a mutual understanding of American psychology and the branch of behavior research generally termed "ethology." The first stone which I hope to contribute to that pavement consists in correcting the widely spread error that ethology is characterized by a new and original method of approaching the problems of behavior. The whole aim of my present lecture will be to show that these methods are as old as natural science and that, if there is any merit in ethology at all, it is due to its strict observance of an epistemological discipline, long ago developed by natural science in general, and to the application of comparative phylogenetic methods developed by Charles Darwin (1859), in particular. To achieve this aim, I must begin at the beginning, that is to say with:

I. Epistemological Considerations

We are living in a time in which it has become fashionable to assess the "exactitude" and with it, the value, of any scientific result exclusively by the degree to which quantifying methods took part in obtaining it. This opinion is one of the dangerous half-truths which fashion is all too prone to accept. While it is entirely true that quantification invariably has the last word on *verifying* the correctness of any scientific statement, it is a fundamental error to assume that knowledge can progress on the basis of quantification alone. The current overrating of quantification as

* Lecture delivered November 12, 1958.

a source of knowledge has very serious epistemological consequences. The first and worst is that it leads to contempt of observation pure and simple which, as I am going to show, undeniably is the basis of all inductive science. The depreciation of observation has gone so far that the term "naturalistic," as applied to scientific work, has assumed, with some behavioristic psychologists, a definitely derogatory connotation. Another highly dangerous consequence of overrating the importance of quantification lies in forcing the several branches of inductive science into an entirely unnatural and unjust scale of values, in which physics, particularly atomic physics, not only range first, but are regarded as the paragon and only genuine representative of a "science" while, on the other hand, those branches of inductive research which are chiefly occupied with the description of structures, are entirely beneath contempt.

It is easy to demonstrate the fallacy of this attitude, which, incidentally, is by no means that of atomic physicists. What concerns us first is that the derogation of structure as such leads to despising all cognitive functions which convey to our knowledge the existence and the special configuration of structure. This again leads to *atomism,* which can be defined as the erroneous belief that natural laws prevailing in matter can be explained on the basis of more general laws of nature without taking into consideration the way in which the matter in question is structured. Not even the simple lawfulness of the functioning of a clock, for instance the "law" that the big hand moves twelve times faster than the little one, can ever be understood without a preceding morphological, in other words *descriptive,* study of the clock's structure in general and the relative numbers of cogs on certain cogwheels in particular.

The "atomistic" procedure of trying to explain the "one-to-twelve law" of the clock *without* previous investigation of its structure, on the basis of knowing the properties of the materials of which the clock is built and of general laws of physics, like those of the lever, the pendulum, etc., is not, on principle, entirely hopeless: only it equals the attempt to *invent* that clock. Anybody could do it, and much more complicated systems have been invented. There is even one extremely interesting case on record in the history of biological science in which a certain type of self-regulating system had not yet been fully analyzed by biologists when it was invented by technicians, by cyberneticists. So, I repeat, atomism is not wrong *on principle,* it only is *bad strategy* in most cases in which an investigator is confronted with a complicated system to analyze. No cyberneticist, on being given the task of repairing an electronic computer of unknown structure built by somebody else, would ever start "from scratch" by trying to reinvent that computer on the basis of the known elements of electronic tubes, switches, and so on. He indubitably would start with a morphological investigation of its structure. Anybody ever having constructed a system which really works will have sufficient respect for structure. Also, he would be aware of the fact that the same or analogous functions can be achieved by using different structures and that, therefore, the reinvention of a system performing a function identical with the one we want to explain, offers us a model of, rather than insight into, the system which we are trying to understand. But as all physiology has the all-important task of serving medicine, in other words the faculty of *repairing* systems that have got out of order, exact insight into those systems is imperative.

Another fallacy, infinitely more dangerous than atomism, also arises from contempt of structure. Let me again use a parable to explain my meaning. Assuming that scientists from Mars have just landed on our planet and are now trying to understand some sort of system found on earth, for the sake of simplicity let us suppose that the system they happen to find first is an automobile and let us, for the time being, pass by the question whether they could ever attain any real understanding of the machine before realizing what it is made *for,* in other words before ascertaining that it is a locomotor organ of *Homo sapiens* L. This, of course, they could find out only by observing the system in the frame of reference of an enveloping system, watching the car in its "natural habitat," driven along a road by a man sitting in it. Supposing all this to be already known to them, what would be the best way for them to proceed in order to "understand" that automobile? Obviously they would take it to pieces and put it together again. And just as obviously they would have to use *all* the pieces in order to achieve this resynthesis.

The same difficulty which our Martians would encounter in trying to understand the interaction of parts within a system, also confronts us, however completely we understand it ourselves, whenever we try to explain its working to somebody else who does not. We are forced to begin at *some* point, for instance by describing the crankshaft turning and pushing up and down the connecting rods and pistons, conveying at first a rough survey knowledge of anatomy and mechanical functions. Then we go on to say that, in going down, the piston sucks "mixture" out of the "carburetor," though, in using this term, we are fully aware that the recipient of our explanation cannot have, as yet, any idea of what a mixture or a carburetor is. What we hope is that our pupil will form a vague idea of what those things might be, and reserve, in the diagram of the engine we are trying to convey to his or her mind, some empty spaces corresponding to these conceptions, to be filled in later by detailed knowledge. At whatever end we begin, *no* single part can be understood, in the full significance of its form and function, before *all* others are. In other words, the parts interacting in a system can be understood only simultaneously and together, or not at all.

Thus the very nature of systems, built up of differently structured parts interacting with each other, imposes upon analysis a very definite strategy of procedure, beginning with making an inventory of all parts, investigating their several forms and structures *to the point of knowing these by heart,* though one does not, at that point, grasp their significance, pushing forward the knowledge of all parts and their interactions *simultaneously,* or at least never forgetting, while investigating one part, that there are innumerable others, nor where the object of the present concentration of interest is situated within the whole. R. Matthaei (1929), in his book on the Gestalt problem, has likened this procedure to that of a painter who begins his representation of reality by a very generalized provisional sketch and then advances by furthering all parts of the picture in equal proportion, until the whole presents itself, often more simple and easily intelligible than was expected.

This method is best termed that of the *analysis on a broad front*. It is imperatively imposed on research by the purely physicalistic recognition of a system consisting of different and mutually interacting parts.

To attempt the resynthesis of any such system on the basis of insufficient investigation of the question how many and what sort of different parts enter into its construction, is very dangerous, but what, for the reasons just given, must be regarded as entirely unpermissive, is to assert dogmatically, at the very beginning of analysis, that only a certain number of parts are sufficient, as explanatory principle, to explain the whole system. This fallacy is what I propose to call *explanatory monism,* though maybe the word "monopolism" would be better, as the concept under discussion has, of course, nothing to do with metaphysical monism, as opposed to dualism. If our Martians, after detaching a few nuts and bolts, should turn their backs on that automobile, retire to their laboratory, and there try to resynthesize the whole system on the basis of these two sorts of elements, their procedure would not be less logical than that of scientists who sincerely believe that all behavior can be explained on the basis of reflexes and conditioned responses. Not that nuts and bolts are not among the most important elements that must be understood in order to understand the function of the whole they take part in. Not that the same is not true of conditioned responses. Not that both do not have the all-important function of holding the other, mosaic-like, oddly shaped parts of the system together. They indubitably do. But to assert that *there are no other parts* or *explanatory principles,* is obviously nonsense.

Atomism and explanatory monism, though usually associated, must be conceptually distinguished. Also, the second is a much more serious obstacle to progress of knowledge than the first. The most confirmed atomist can, as we have seen, still invent a system, but explanatory monism blinkers the outlook because it channels investigation into a narrowed range of experimentation which precludes the discovery of other explanatory principles. What, in my parable of the Martians and the car, has been described as turning away from the object and taking the few detached parts back to the lab, is something which most deplorably happens in real life: further unbiased observation, which alone might lead to the discovery of further explanatory principles, is not only deemed unnecessary, but outright unscientific. A good example for this sort of deadlock is furnished by the role the reflex concept plays in the development of neurophysiology. The hypothesis that responding to stimulation was the one and only function of the central nervous system, led to a manner of experimentation which could not but confirm the preconceived theory — and that is absolutely the worst thing a working hypothesis can do. With few exceptions, the experiments of that time and school confined themselves to keeping the animal or the preparation under constant conditions, then letting a change of conditions, a "stimulus," impinge upon it, and then recording the response thus elicited. In that sort of set-up, the central nervous system, poor thing, never even got the opportunity to show that it could do much more than just react to stimulation!

Explanatory monism is pardonable only in the genius who has discovered a new explanatory principle. Nearly every genius and discoverer of such a principle has availed himself of that prerogative. I. P. Pavlow sincerely believed that all problems of behavior could be solved by the theory of conditioned reflexes. Jaques Loeb (1913) thought the same of tropisms, and so on. Perhaps it does little damage if the discoverer of a really new and important explanatory principle thus overrates the extent of its applicability, but when disciples tend to do the same, the discovery which originally was a great step forward toward our understanding of nature, may develop into a very serious obstacle to the next steps.

Atomism and explanatory monism are the more detrimental to research and the method of analysis on a broad front becomes the more imperative, the more the object investigated bears the character of a whole or system and the more complicated its structure is. In all the world there is no object exceeding, in these respects, the neurophysiological organization of the central nervous system which determines behavior. For this reason, atomistic approach and explanatory monism have less prospect of ultimate success in behavior study than in any other branch of natural science.

It is my conviction that the ardent endeavor to concentrate on quantification alone and to neglect all other cognitive functions, particularly those which convey knowledge of structure, is due to a misapprehension of the methods employed by physics. Physicists, more than most other scientists, are disciplined to think in terms of systems. I still remember, with gratitude, Dr. Vinzenz Blaha, my teacher of physics at the Schottengymnasium in Vienna, who unceasingly tried to impress on our juvenile minds the fundamental fact, that there is, in all the world, no action which is not interaction. *Actio* equals *Reactio* was the equation which he had printed in golden letters on a large board and hung in our classroom, and, in our first lesson of physics, he sat each of us in turn on a revolving table set on a roller bearing and told us to try to turn round. Obviously, he regarded the strategy of thinking in terms of systems as the first and most fundamental thing which the novice in physics ought to be taught, and this was his unforgettable way of doing it.

As physicists indubitably do think in terms of systems, they are also aware of the importance of studying structure and of the cognitive functions purveying knowledge of structure. No physicist ever believed that quantification was the only legitimate source of knowledge. If, in observing a physicist at work, we do see him, for most of the time, occupied with calculations, it is not because he neglects structures, but because he already *knows enough* about the structures which concern him. The basis of induction, on which the imposing building of modern physics is erected, consists just as much of observation and description as that of any other natural science. That the physicist is not ashamed of being "naturalistic" is beautifully borne out by a paper which Max Planck published in 1942. In this he demonstrates that the sequence of cognitive acts through which the physicist attains a partial understanding of the surrounding universe, does not differ, in principle, from the procedure by which an absolutely naive person or even a child achieves essentially the same understanding, if on a simpler level. All knowledge begins with perception aided by exploratory behavior; this results in what is termed latent learning in animals. In the chaos of stimulus data, we, animals, children, and scientists alike, learn to discern syndromes, that is recurring combinations of data. I use the word syndrome in a very wide and literal sense, meaning stimuli "running together" occurring together in space and time, just as the etymology both of the Greek and the Latin word implies. Young Adam masters the immense multiplicity of these syndromes with the help of giving names. Some of these names are generic: All dogs are called "bowwow" by our little Adam, and this all-important feat implies, in itself, that he has succeeded in arranging many syndromes in an order of similarities and dissimilarities. The next step is the discovery of a certain lawfulness underlying that order. This sequence of three cognitive functions, by which the child attains a tolerably adequate picture of the surrounding world, are absolutely identical with the three phases discerned by Windelband (1894) in the development of inductive natural science: the idiographic, the systematic, and the nomothetic phase.

Our ability to discern and recognize sets of regularly concurring stimuli as natural units is based on the function of a very wonderful, but by no means miraculous organization within our central nervous system, that of perception. Our conceptions of objects in space, as well as those of processes in time are based on its function. A set of extremely complicated nervous mechanisms performs the highly important task of keeping our perception of objects and processes constant, in spite of the varying accidental circumstances in which we perceive them. I perceive the paper before me as white, whether I am seeing it in yellow electric light, in the more bluish daylight of early morning, or in the reddish light of the setting sun. I perceive the head of an approaching person as having a constant size although the area on my retina covered by its picture increases tremendously with the decrease of distance. I perceive the form of my pipe as constant while turning it to and fro before my eyes, in spite of the fact that its retinal image assumes astonishingly different contours. All these "constancy phenomena" perform a function which is *objectivating* in the same sense in which this word is applied to rational scientific procedures: accidental sensory data, dependent on fortuitous circumstances are suppressed while lawfulnesses constantly inherent to the objects and/or processes are "abstracted."

True Gestalt perception indubitably is nothing else than one of the constancy functions, though, of course, it is by far the most complicated one and includes, or has for prerequisites, most other constancy mechanisms, such as those mentioned above. The constancy of form, as illustrated by the central interpretation of the retinal image of the pipe turned before my eyes, involves functions of stereometric computations attaining an immense complication — indeed, the phenomenon of form constancy with all the classic criteria of a Gestalt, such as transposability, independence of elements, etc. English-speaking psychologists are not usually aware that the German word *Gestalt,* in common parlance, means just external form. It is indubitably in the service of form constancy that the mechanisms of Gestalt perception have originally been evolved in higher organisms. But a central nervous organization, as well as a man-made computer, occasionally has an interesting way of being able to perform other functions besides the one for which it has been built. It is on record that an electronic device built to compute compound interest unexpectedly proved to be able to perform differential calculus. In an essentially similar way, just by performing their typical functions of detaching relevant configurations from a background of "white noise" that is to say, of inessential stimulus data, the mechanisms of Gestalt perception achieve the amazing feat of *abstracting generic conceptions*. It amounts to an erroneous rationalization to believe that this is done by abstract thinking. When little Adam invents his first noun, calling all dogs "bowwow," this is not owing to his having abstracted the diagnosis of *Canis familiaris* as given by Linnaeus, but to the functioning of his Gestalt perception, which enables him to disentangle the essential configuration common to all dogs from the background of inessential differences and which permits him to perceive, in the aunt's peke, the neighbor's dachshund, and the butcher's mastiff, *one* common Gestalt, that of *the* dog!

All these functions of the constancy mechanisms, including that of Gestalt perception, are performed entirely without any participation of reasoned thought; most of them are quite inaccessible to rational self-observation. Yet their procedure is so strictly analogous to that of ratiocination that the two have actually been confounded even by most profound thinkers. Heimholtz declared the processes of distance perception and size constancy to be the results of "unconscious conclusions." Egon Brunswick (1957), in order to stress the analogy, as well as the physiological differences, existing between the two kinds of computing processes, coined the term "ratiomorph" for those here under discussion.

Useful though this distinction doubtless is, I doubt whether it does not imply the danger of suggesting too strict a dichotomy between two types of functions which, in reality, merge into each other by all possible gradations. The functions of perception are cognitive functions, notwithstanding the fact that their mechanism is inaccessible to self-observation and incorrigible by ratiocination. They are *aprioristic* in the strictest sense of Kantian theory of knowledge, being in existence previous to all personal experience and being indispensable in order to make experience possible at all. It is my epistemologically heretical, but firm, conviction that the Kantian "forms of ideation," space and time, are by no means two independent entities, but mere abstractions of two aspects of *one* functional system, that of the mechanisms which enable us to move and to perceive movement in space *and* time.

The philosophy of inductive science does not concede absolute validity to *any* of the "aprioristic" forms of ideation or of the "aprioristic" categories of thought. The scientist must necessarily attribute, at least as a working hypothesis, the character of reality to the world he is endeavoring to explore. For the physiologist or psychologist who claims to be a scientist at all, it would mean the highest degree of inconsistency, not to attribute the same character of reality and, on principle, the same degree of relative cognizability both to outer reality and to the physiological mechanisms to whose function we owe its cognizance. The biologist thinking in terms of evolution necessarily must regard all cognitive functions as having evolved in interaction with, and in adaptation to, the things and the laws of outer reality. He does not expect them to convey, to us, absolute truths about outer reality, but just that kind of working knowledge which is necessary for the survival of the species.

The biologist, therefore, is not in the least surprised when the physicist tells him that the picture of the world conveyed by our "aprioristic" forms of ideation and categories of thought, though adequate for the practical purposes of survival, do not, on close investigation, fit reality in a very exact way. Space, as we are wont to visualize it, stirringly infinite, beautifully homogeneous, and intelligibly three dimensional, is shown by the physicist — and we must believe him unless we want to cast overboard all the laws of mathematics — to be not only finite, but to possess a fourth dimension in which it is surprisingly crooked in the most unexpected manner. Time gets off no better than space; the statement that two things took place simultaneously does not have any precise meaning for the physicist and even the category of causality itself, logical and inescapable as it would appear to be, is shown to correspond to facts only in rather haphazard, roughly statistical manner.

What the physicists have done in order to achieve this pitiless critique of the world picture conveyed by the cognitive functions we are naturally endowed with, was a most revolutionary and, from the viewpoint of idealistic theory of knowledge, a strictly illegitimate procedure. Physicists have had the temerity to doubt the absolute necessity and validity of all the rational forms of ideation and of thought and to treat them exactly as if they were nothing else than working hypotheses. And that, of course, is exactly what they really are!

The old contention waged by idealists and empirists, whether some forms of ideation or thought are given to us *a priori* or *a posteriori,* loses most of its epistemological importance the very moment one regards Man as a product of evolution. The empiristic assertion that there is nothing in the human intellect of which Man had not previously been informed by his senses, would, of course, be the merest nonsense if we should interpret it to mean that the central nervous system is, in the young organism as yet devoid of experience, a completely unstructured mass and attains structures only through experience as conveyed by the sense organs. But if, on the other hand, we consider the mechanisms by which evolution achieves the creation of adapted structure, then their functioning appears analogous to experience in so many points that we cease to be astonished by the similarity of the results attained by evolution on one, and by learning on the other, side. The genome, the set of chromosomes, contains an unbelievable store of information which would fill innumerable textbooks of anatomy, physiology, ecology, behavioral sciences, and what not, were we able at all to express its contents in written words. All this treasure has been collected by a procedure closely akin to trial-and-error learning. The arrangement of the genes within the chromosomes, the limited dosage of their mutability, the possibility of their recombination through the processes of sexual reproduction, all taken together represent an apparatus which performs an altogether successful series of experiments, progressively gathering information concerning the powers that be, forever daringly staking part of the progeny and yet prudently never jeopardizing the survival of the species — in other words, the amount of information already collected. The overwhelming success of this method is demonstrated by the fact that all higher organisms, plants as well as animals, are descended from the creatures that have "invented" it, from the flagellates. Campbell (1958) has shown very convincingly that the evolutionary procedure under discussion is essentially identical with that of pure, e.g., deductionless induction. The same, of course, is true of the process composed of pure trial-and-error plus conditioning.

As far as is known at present, these two mechanisms are the only ones by which an organism can attain information concerning the universe surrounding it. All structures and/or functions, including those of behavior, which possess a definite survival value, owe it either to one or to the other. The question, to which of the two? is of obvious importance from the viewpoint of causal analysis of behavior, but from that of theory of knowledge, its answer is irrelevant.

Thus, all cognitive functions with which we are endowed, indubitably are, like all other adaptive life processes, the function of organic systems evolved in age-long interaction between the organism and its environment. The recognition of this fact has repercussions in many directions. One of them, directly important to the subject of my lecture, concerns the assessment of the relative values attributed, in the day's work of inductive research, to the several cognitive functions ranging from the highest, rational ones implying the categories of causality and quantity, down to those of perception and the functions of receptor organs. The most important conclusion to be drawn from all the above-mentioned evolutional and epistemological considerations is that *all* these cognitive functions go to form a functional unit in which no single part can be dispensed with without doing damage to the whole. Therefore, no single function can be regarded as being "more important" than any other.

As a rule, overrating the highest thought processes and underrating sensory and perceptual functions is a failing characteristic of philosophers rather than of scientists, and it was at the former that Wolfgang Metzger (1936) directed his witty satire when he said that there were some people who, by epistemological considerations, were incurably prevented from using their senses as a source of scientific cognizance. Yet much the same is true of many behavioral scientists who, under the misapprehension that quantification is the only truly objectivating cognitive function, endeavor to use it exclusively to the extent of despising all others. Of course, the epistemological inconsistency of this attitude is much easier to show up than that of the idealistic philosophers at whom Metzger is poking fun. It simply consists in denying to perception the character of cognitive function when it is used in direct observation, devoid of hypothesis, yet endowing it with that selfsame character whenever it is used to read a measuring instrument.

Yet a worse inconsistency lies in forgetting that, whereas the objectivating function of perception is independent of that of quantification, the basic function of the latter, simple counting, is absolutely dependent on perception in general and on Gestalt perception in particular: The latter irrefutably is the indispensable purveyor of the equal, or at least comparable, entities which are to be counted. I never can help a shrewd suspicion that the worshipper of quantification and despiser of perception may occasionally be misled into thinking that two goats plus four oxen are equal to six horses. Counting pecks of pigeons in Skinner boxes without observing what the birds inside really do, might occasionally add up to just this. It is not at all a rare experience that, in studying central nervous processes, one finds this sort of one-sided dependence of the higher, more derived and complicated function on the phylogenetically older and more primitive one. I confess that I absolutely fail to understand why this universal and unconditional, one-sided dependence of counting on perception is not in itself sufficient to authenticate, in the eyes of every scientist alive, perception as a legitimate and independent source of scientific knowledge.

As it is, however, I have recently written a whole paper on this subject (Lorenz, 1959), addressed exclusively to a comparatively small yet important group of scientists who are underrating the importance of structure and that of the cognitive functions which convey to us knowledge about structure. These scientists are American psychologists strictly adhering to behavioristic concepts and methods. They are the only atomists I know of. Atomic physicists are no atomists by any means; quite the contrary, they are all thinking in terms of structured systems and they would not have succeeded in splitting the atom if they had not.

Also, atomic physicists are in the most complete agreement with the evolutionist's theory of knowledge. This I endeavored to formulate in a little paper (Lorenz, 1941b) which appeared just prior to Max Planck's aforementioned paper (1942) "Die exakten Naturwissenschaften," in which he stated very much the same opinions. The temporal sequence was lucky, not because the priority of my paper is of any importance, but because the obvious independence of the conclusions is: Nobody will ever think that Planck was influenced by me, whereas the opposite suspicion would indeed be pardonable. I feel free to state that it was the proudest moment in my life when I read, in a letter from Max Planck: 'Es erfüllt mich mit grösster Befriedigung dass man, von so völlig verschiedenen Induktionsbasen ausgehend zu so völlig übereinstimmenden Ansichten über das Verhältnis zwischen realer und phänomenaler Welt kommen kann" [It gives me great satisfaction that, starting from such entirely different bases of induction, one can reach so entirely consonant views concerning the relationship between the real and the phenomenal world].

There are other interesting consequences of our fundamental conviction that all our "aprioristic" forms of ideation as well as all our categories of thought must be regarded as being nothing but working hypotheses developed in phylogenetic interaction with reality, sufficient for our survival, but by no means conveying absolute truth. The most important of these consequences is the recognition of the interdependence of all branches of science, as well as of science and theory of knowledge. It is, to any scientist, a matter of course that every branch of science is, for the success of its analysis, dependent on the existence of another, more general, more basic neighboring branch. What we call *explaining* a process or a lawfulness found in nature means making it intelligible on the basis of *more general* laws of nature *and* the *special structure* the function of which they govern. Describing in detail the special structure of the object investigated is the very own task of the investigator, the task from which he cannot be excused. But as regards the more general laws, he *has* to rely on the findings of the "man downstairs" that is to say, of the scientist investigating the adjoining level of more simple and more basic natural laws. Otherwise he would have to undertake the impossible task of pushing analysis down to the level of atomic processes all by himself. Thus, it is a necessary arrangement that each story of the collective building of inductive science is erected on the supporting structure of the next-lower one. The accent here is on the word "next," as obviously it is impossible to skip a level. When, for example, Sutton and Boveri sought an explanation of the Mendelian laws it would not have helped them, if they had had even the profoundest insight into the biochemistry of chromosomes, or any such more basic laws and processes. What they needed to know was the general gross structure of chromosomes and the processes of their division and reunion during reduction and fertilization. The detailed knowledge of biochemistry may, as a further step, prove necessary to explain the lawfulness of the latter processes, in their turn. Jumping levels means neglecting structure and is, therefore, again equivalent to the methodological fault of atomism.

One prerequisite for the successful collaboration of all the innumerable particulate levels on which investigation is being carried forward is, of course, that of a certain temporal sequence proceeding from the lower levels upward. The upper story remains, for the time being, literally hanging in the air, unless the next one downstairs is far enough advanced to offer, ready-made, those natural laws which the man upstairs needs for his explanatory endeavor. This type of deadlock does not seem to have occurred all too often in the history of science. In most cases, the man downstairs was just ready with the necessary support in the form of more general laws where the man boring downward from the next higher story had got deep enough to need them. This certainly was the case with atomic physics when chemistry had advanced far enough to need its support. The fusion of physics and chemistry is indeed the classic example of analysis achieving ultimate success. The word analysis actually means dissolution and what it dissolves is not, as many think, the independent existence of a natural law: The latter remains a law as much as ever, even when we attain full understanding of why things work that way. What analysis really disintegrates, if only in the case of ideal success, is the seemingly strict boundary line separating natural processes which take place in different structures and on different levels of complication and/or integration.

The procedure of "giving an explanation" as I have just defined it, determines, by its very nature, the direction in which inductive analysis invariably has to progress, that is the direction *from* the more complicated more highly integrated process *toward* the simpler and more basic one. As an important consequence of this universal directional trend of all inductive research we find a queerly asymmetrical one-sided relationship of dependence between the several branches of science. On whatever level of complication an investigator is working, he must know practically all about the results which the men on the next-lower level have brought to light, but he need not, on principle, know anything about what is going on in the investigation of processes more highly complicated and integrated than those which he is himself trying to analyze. The organic chemist must know all about inorganic chemistry, but need not know anything about the physiology of metabolism; but the physiologist investigating metabolism certainly won't get far unless he is thoroughly conversant with organic chemistry. Thus, all the branches of science form a functional unit, fitting into each other like a system of Chinese boxes. The beauty of this imposing structure lies not only in the fact that *it is humanity's one and only really* collective endeavor, but also in another fact: It is not man-made, but is imposed on all our striving for knowledge by the real structure of the real universe.

This grandiose frame which holds all Man's cognitive efforts has, for a basis, two assumptions: the aforementioned assumption that the outer world is real, and the second one, that is is governed by *one* set of ubiquitous and uncontradictory laws which never suffer the least exception, however complicated and integrated the structural systems whose function they govern. This assumption is absolutely compatible with our conviction that, far from containing absolute truth, all our *formulations* of these laws are but rough approximations, and this applies equally to empirically gained, scientific laws and to the "aprioristic" axioms imposed on our ideation and our thinking by the organization which our brain has acquired in phylogenetic interaction with reality. The most fundamental among these laws are, on the empirical side, those of physics and, on the aprioristic side, those of mathematics of logic. The ultimate goal of our explanatory endeavor must be to trace back, to the basis of these laws, all the structures and functions we encounter in the universe, proceeding from one level of complication to the next lower one by the ancient means of examining structure and reducing its function to more general laws derived from the next-lower level. Hence all natural science necessarily is "mechanistic” or, since physicists themselves have lost their absolute trust in the laws of classic mechanics, *physicalistic*.

Yet atomic physics is not the ultimate, absolute fundament, on which the pyramid of all other sciences is built and which represents rock bottom to all human cognitive endeavor. Some scientists who still are naive realists, and some philosophers who are transcendental idealists, tend to think so. Atomic physicists do not. They are fully aware that our *a priori* forms of ideation and of thought, inevitable and logical though they seem, are nothing more than working hypotheses which fit the facts of reality no better than man-made ones do and which are accessible to constructive criticism in exactly the same way: The great physicists have abundantly shown that forms of ideation and of thought can be confronted with experimental results and the limits of their explanatory validity can be thereby determined, using the same procedure which is applied in examining man-made working hypotheses. Doing just this has been the revolutionizing deed of men like Planck and Einstein. Though their views deviate fundamentally from those held by Kant, it is nevertheless doubtful whether the great physicists, without the basis of Kantian philosophy would ever have been able to formulate their own criticism of human cognitive functions as clearly as they have done. In any case, it was no coincidence that Max Planck was a great admirer of Kant and a highly learned expert in Kantian theory of knowledge.

At this point we discover an unexpected interdependence of science and philosophy — if we concede that theory of knowledge belongs to the realm of the latter. We find that, on the one hand, it is impossible to push our knowledge of outer reality any further without critically examining the cognitive functions through which this knowledge is conveyed to us, and also that, on the other hand, it is quite impossible to effectuate this constructive criticism without pushing forward, in its turn, our knowledge of outer reality. Any attempt at a theory of knowledge which does not take these facts into account is comparable to the procedure of thoroughly examining a photographic camera, say a Leica, without taking into consideration that it is an apparatus made to take pictures of the outer world and that its makers have developed it, from simpler models, by a procedure in which the relative quality of the pictures taken has had an influence on the development. This kind of "pure Leicology" would afford a very poor idea of the camera's essential nature. For analogous reasons, theory of knowledge is dependent on science.

The reasons for which science is dependent on theory of knowledge are just as obvious, but are different ones. They are the same for which a man using a microscope must have a thorough knowledge of all the optical properties of the instrument he is using, in order to escape the danger of mistaking, for real properties of the things perceived, those peculiar characters which are imposed on all pictures by the limitations and shortcomings of even the best optical instrument (Lorenz, 1941b).

The views stated in the preceding paragraph were published, as conclusions drawn from purely biological, evolutionistic considerations, in my little paper of 1941 when I was living in Koenigsberg, in the shadow of Immanuel Kant himself. So it was not surprising when they were most emphatically rejected by philosophers.

But it was a surprise, and a most pleasant one, when great physicists emphatically agreed. I could not summarize my own views more succinctly than P. W. Bridgman did in his remarks (1958) on a talk given on the subject under discussion by Niels Bohr (1958): 'The object of knowledge and the instrument of knowledge cannot legitimately be separated."

The epistemological attitude of which I have attempted to give an outline is identical with the one that Donald T. Campbell (1958), in his brilliant paper on the subject, has termed *hypothetical realism*. It is the attitude which, consciously or unconsciously, *all* scientists take in respect to theory of knowledge, provided they have outgrown naive realism at all. I may add that it does not do much damage to most branches of natural science if they have not. It is no coincidence that, among all scientists, atomic physicists and physiologists of behavior are the ones who are most intrigued by the problems of theory of knowledge: Quite obviously these problems are the responsibility of the science concerned with the ultimate, basic objects of knowledge and the one concerned with the physical instrument of knowledge. If the goal of understanding the inner mechanisms of our brain to the extent of explaining the limitations of our cognition is a Utopian one, it is nevertheless one that is inescapably set to us by the structure of the universe which dictates the strategy of our possible advance.

All scientists working on the levels of complications and/or integration which lie *between* the levels of physiology of cognition and atomic physics are, on principle, free to act, in their day's work, as if they were naive realists. No damage can be done by this attitude as long as they remain really naive, and trust their sense organs, their perception, and their power of reasoning to an equal degree. Man's cognitive organs and functions are *made* so that they work tolerably well within the "middle ranges" of their application and naive realism does not interfere with the rules which, in the game of research, command certain things and forbid others. To collect one's basis of induction without as yet having formed a theory biasing the act of collecting, to investigate structure conscientously, to think in terms of systems consisting of universally interacting parts, never to believe that any one single explanatory principle can achieve, by itself, the understanding of such a system, never to skip a level of integration, on the long journey analysis has to go through on its way downward to atomic physics, these are some of the commandments which are practically never broken by natural scientists.

With psychologists, this is an entirely different matter. I have already said that, in the historical development of natural science, either by design or by sheer luck, the research on the more basic levels had, in the majority of cases, advanced far enough when the analysis in progress on the adjoining higher level began to reach downward far enough to need the "man downstairs" for further support. Physics had progressed just that far when chemistry began to need it, organic chemistry when physiology of metabolism did so, and so on; in short, the more basic processes were, on the whole, investigated earlier than the more complicated ones. With psychology, this was not so. Psychology is the daughter of philosophy and is, for this reason, much, much older than any natural science. It was very old indeed when, at the dawn of this century, it discovered that Man was, after all, part of nature and that even the highest functions of his mind ought, on principle, to be treated and investigated as natural phenomena. It was admirable and consistent when psychologists of that time, with Wilhelm Wundt for a leader, took the first steps to make psychology an empirical natural science. The fact that these first steps led in the direction of a deplorable atomism is easily explained — and excused. Psychology started its research on the level of the very highest, most complicated process in the universe. Very little was known about the structure and the functional organization of the central nervous system; the "man downstairs" of my parable simply did not exist. In the overwhelming chaos of inexplicable facts two tangible processes offered a foothold to research: the reflex and the conditioned responses. As a matter of course, psychologists and behavioral scientists grasped at what, after all, were by no means straws, but very real explanatory principles, and it is easily understood that they modeled their "stratagem of research" on that of classic physics rather than on that of biology, though the latter would have been their legitimate neighboring and more basic branch of science. All this conspired to lead the most serious and scientifically respectable among psychologists to adopt the most absolutely atomistic attitude ever encountered in the history of natural science. They became confirmed in it by two factors. The first was the tremendous initial success which reflex and conditioned response had to show as explanatory principles. Indeed, in a rat's behavior, so much really *is* explicable on the basis of these two things that it is nearly — if not quite — pardonable to believe that *all* its behavior is! The second factor was the spirit of contradiction aroused by the criticism — justified in that one point — which was uttered by the more or less vitalistic opponents of behaviorism. This historical development explains why behaviorists so consistently forget the immensely complicated structure of the central nervous system and why they despise all cognitive functions which, like Gestalt perception, pure observation, description, comparison, and systematization, convey, to the human mind, the knowledge of structure, and which by *all* natural sciences are regarded as the indispensable steps that must have been gone through *before* the first attempt at the abstraction of natural laws, of nomothesis in Windelband's classic terms, is undertaken.

I have had to go into epistemological details rather extensively in the attempt to convince you of what I said in the first paragraphs of my lecture, namely, that the methods of approach which many American psychologists believe to be characteristic of ethology, are, in reality, fundamentally common to all natural science *except* behavioristic psychology, a fact of which some of the more outspoken critics of ethology quite obviously are not conscious. I can summarize what I have tried to explain hitherto in one sentence: *The method of approach of all inductive natural sciences is* "*naturalistic*"*!*

I now come to the discussion of a more special set of problems concerning not the theory of knowledge, but the practical strategy of research imposed on all biological sciences by the fact of evolution. The ethological approach has been criticized as being "finalistic" by Kennedy (1954) and Lehrmann (1953) and as "pre-formationistic" by the latter.

II. Treatment of Phyletic Adaptation

It devolves upon me to show that the treatment of phyletic adaptation by which ethology has incurred these reproaches, is absolutely the same in *all* branches of evolutionistic biology. Finalism, in the correct connotation of this word, may be defined as the belief that a metaphysically determined end or goal enters, as another causal factor, into certain causally connected chains of events directing them to that particular end, which is invariably one that has a definite survival value for the animal or its species. Of course this opinion is entirely untenable from the physicalistic viewpoint common to all science, as even the slightest "directing" factor implies a force: Every change of direction has the dimension of an acceleration and is, therefore, unthinkable without a force. Thus, the finalistic view implies a breach of the second law of thermodynamics.

No scientist is farther from assuming the kind of directing factor assumed by finalism than is the biologist thinking in terms of evolution. The indubitable fact that the organic world, as we know it today, owes its existence to a historical process which, at least on principle, is explicable on the basis and without any infraction of the ubiquitous laws of physics has, for a consequence, a very specific conception of structures and functions which "serve the end" of survival. This conception of what one might call, in English, "expediency" has nothing in common with the finalistic conception of an end or goal, still less with the vitalistic idea of a prestabilized harmony existing between organism and environment, as implied by J. von Uexküll's conception of a pre-existing *Bauplan*. If the German word *Zweckmässigkeit* contains, etymologically, the implication that the structure or function in question is constructed in such a way as to attain a certain result, why, so it is, not by a miracle and still less on a pre-existing plan, but by mutation and selection performing something that is very close to trial-and-error experimentation.

To an observer whose eye is not sharpened by phylogenetic comparison, any higher organism gives the impression of such a supreme and absolute harmony that it would appear outright sinful to search for imperfections and that the assumption of a plan, preconceived and preordained by a supernatural agency, seems inevitable. But a closer, comparative look at *many* organisms discloses an entirely unmiraculous, if still more wonderful fact: Organisms are *never* built on the principle which must be postulated on the basis of preformationistic concepts, that is to say, like buildings whose functions a far-seeing architect foresaw, whose smallest details were agreed upon before the first stone was laid, and whose actual erection then took place all in one sitting. Organisms are always constructed on the principle on which a house is built by a settler, who first erects a simple provisional shack in which to live, and who later on, as his affluence and his family increase, adds one piece after the other to the building. In such a house, many parts will, in the course of time, change their functions completely: What first was the living room, may become the stable or even the lumber room; compromises and temporary shifts will become necessary, many among them of very doubtful expediency. The efficiency of the building as a whole will necessarily remain inferior to that which could be achieved by the construction of a planning architect, but the latter is unattainable just *because* the house has to be lived in all the time and can never be torn down altogether and wholly reconstructed.

All structures of all organisms came into being in a strictly analogous manner. Had they not done so, we should know still less about the history of their phyletic development than we do. The essentials of this development are, as Goethe formulated it, differentiation and subordination of the parts. This means that, with higher development, all parts become more and more different from each other and, at the same time, more dependent on each other and on the whole of the organic system. This is clearly the consequence of a progressive division of labor, each part becoming more and more specialized for its own function. Here, for the first time, the word "for" is encountered in connection with function, so here is the point at which we must discuss what it means.

For the biologist conscious of evolution the question "what for?" makes sense only when applied to the structures and/or functions which have undergone a development, in the above-defined sense of differentiation and subordination and which thereby have become specially adapted to the service of one particular survival value. In other words, the biologist asking: "What has the cat sharp, crooked, retractile claws *for?*" and answering correctly: "To catch mice with," does by no means profess finalism.

His question and answer are just "shorthand," meaning: Which is the function whose survival value exerted the selection pressure which caused, in the herpestoid ancestors of cats, the evolution of this particular form of claws? So this question is definitely directed not at a metaphysical goal, but at a natural cause. The biologist investigating phylogeny is the last man to forget that so-called finality is a direction arrow which we attached to events *post festum*. Every single living organism is a product of the factors causing evolution and is a construction which is "expedient" enough for survival; were it not, it would not exist. Because we have, before our eyes, only the successful ones among the innumerable experiments which evolution is constantly performing by its patient trial-and-error method, it is all too easy to forget the extinct results of the unsuccessful ones, which are certain to outnumber the survivors by billions.

Knowing where to put and where not to put the question "what for?" in the above-defined sense, is one of the most important assets to the biologist's strategy of research. Not to put the question where it legitimately can be asked, means neglecting a most important line of causal research. Applying it to a process devoid of survival value means hard work leading to nothing. In medical science, the decision whether to put or not to put the question is equivalent to deciding whether a process or structure is to be regarded as "normal" or as "pathological," which is exactly why medical men are particularly good at deciding it. Also, medical men usually possess a commendable awareness of the fact that the conception of the "normal" cannot be strictly defined and has only the very loose meaning of "just good enough for survival."

For the biologist investigating the structure and behavior of healthy wild animals, it is easier to decide when to ask the question "what for?." Whenever an organic system attains a degree of complication and regularity which makes it impossible to assume that it arose accidentally, the question is not only legitimate, but obligatory. If some cows are spotted brown and some black, no biologist will trouble to investigate the survival value of one or the other. But supposing we were the first biologist to come to South America and to shoot a giant anteater. Investigating the bizarre structure of its jaws, its hyoid bone, and its tongue we should know, with what amounts to absolute certainty, that these characters which diverge so strongly from the corresponding ones of other mammals, could have attained their present properties only under the urgent selection pressure of a most definite function. So the question 'what for" is indispensable to the endeavor to attain causal understanding. I may add in parentheses that all these considerations not only hold true, but are of quite particular importance in those cases in which a structure or an innate behavior pattern has lost, as a secondary step in phyletic history, the survival value whose selection pressure had caused it to originate The "adaptation of yesterday," the so-called *vestigial* character, is one of the chief sources of phylogenetic knowledge and of particular importance to the student of innate behavior. It is an empirical fact that fixed motor patterns are apt to be even more "conservative" than morphological characters. Probably a small vestigial hump on the body surface or a blind appendix to an intestine is more liable to impede some function, and thus set off selecting processes leading to its disappearance, than is the performance of some senseless motor pattern which therefore, to the joy of the phylogeneticist, often is carried over almost indefinitely. Having now said what I considered it absolutely necessary to say about fundamentals, in respect to which ethological approach is identical with that of science in general and that of the vast unit of biological sciences in particular, there still remains to me the task of comparison between the ethological approach and that of other schools of behavior study.

III. Comparing the Ethological Approach to that of Other Schools of Behavior Study

A. Behaviorism

In my endeavor to set forth, as clearly as possible, the methods of approach characteristic of ethological behavior study, I have been forced to begin with epistemological fundamentals and then to go, in some detail, into the consideration of the part which the problems of survival value take in determining our method of approach. I hope you will pardon it if, in addressing an American audience, I have felt it necessary to *defend* the ethological strategy of research against some misunderstandings contained in the criticism which ethology has aroused on the behavioristic side. I have tried to do this by showing that the epistemological attitudes and the ways of treating the question of survival value, in which ethology differs from behaviorism, are the very same ones in which most or all inductive natural sciences, particularly the biological ones, differ from it in exactly the same respects.

This attempt has inevitably entailed some serious criticism of the behavioristic approach, and if this seemed too aggressive I hasten to redeem this impression by emphasizing the tremendous debt owed to behaviorists by science in general and quite particularly by ethology. I am, at the moment, not speaking of the vast success which behavioristic thought and experimentation have had in unraveling the problems of learning, and which is absolutely undisputed.

The values which the world owes to behaviorism are by no means restricted to the field of practical research only. Its importance is even greater concerning the theoretical approach to the body-mind problem. By its radical self-restriction on what can be objectively observed in the behavior of an organism, it has put an end, once for all, to the general confusion of psychological and physiological causes and effects.

The student of animal behavior is in a highly conflicting situation regarding the problem of the relationship between objective, physiological, and subjective, psychological processes. While fully aware that it is only the former that are accessible to the methods of inductive research, he cannot help believing in the reality of the latter. No normal man is able to avoid that type of "empathy," and it is simply not honest not to confess to it. After all, we profess it by refraining from inflicting torment on animals. But, at the same time one must keep strictly aware that empathy does not convey to us even the slightest degree of knowledge. Though I know" that the tame graylag goose accompanying me is frightened" when it stretches its neck upward, flattens its feathers against its body, and utters a certain note, it is not any empathic flow of communication flowing from the bird's soul to mine which gives me information about its behavior. It is the exact opposite of this process that has taken place. From experience, repeated literally thousands of times, I have learned that in a goose performing just those expression movements all thresholds of escape responses are extremely low and that, in all probability the goose will fly off the next moment. If I admit, as I don't hesitate to do, my own belief that the goose *does* experience, at that moment and in correlation with that behavior, something like fright, this belief has nothing to do with scientific knowledge. But it is based on scientific knowledge if I predict that the goose will fly off, for science is, in F. Fremont-Smith's aphoristic definition, "Everything that makes things predictable."

In human beings of course, psychology proper, that is to say the investigation of Man's subjective phenomena, can be pursued by the method of true induction. But it is a fundamental epistemological error to believe, that these subjective phenomena can ever, even in case of a Utopian final success of research, be "explained" on the basis of "underlying" physiological processes. These physiological processes do not "underly" their psychological correlates, but they go parallel to them in a most mysterious way which, on principle, is not accessible to the operations of our rational thought. The relation between the correlated processes is, as Max Hartmann has termed it, a-logical; even the most cautious parable expressing it by speaking of a certain "parallelism" is somewhat misleading, because the "parallel" is very one-sided: All subjective experiences are indubitably correlated with physiological processes, but, on the other hand, by no means all physiological, not even all neurophysiological, processes are experienced as subjective phenomena.

For this basic epistemological reason, psychology proper, as the science dealing with subjective phenomena, does *not* have a branch of natural science for a neighbor, in the way in which any more special branch of research has a more general one to support it. Though psychology proper applies the same methods of observational induction, deduction, and experimentation, it is not "founded on" neurophysiology, but runs parallel to it in the same fundamentally inexplicable manner in which subjective phenomena run parallel to physiological ones. The eternal impenetrable diaphragm partitioning the field of our knowledge into two incommensurable realms is something we indubitably have to accept — as philosophers. As men, we find it difficult to do so, and most investigators of behavior would have to confess, if they were quite candid, that they really are gnawing away at the hard bone of the body-mind problem, hoping against hope to crack it, while they are patiently investigating the physiological as well as the subjective side of life processes. At least, even the strictest psychophysiological "parallelists" ought to confess to their belief, that subjective and physiological phenomena are only two sides of the same reality. I do not hold with any philosophy to which I cannot remain consistently faithful in the realm of commonplace commonsense, and when I say that my friend Frank is sitting there in the second row, I mean neither the objectively observable physiological phenomena of his body, nor the vaguely correlated soul, the existence of which I do *not* infer by certain analogies between his behavior and my own, but the reality of which is inescapably forced on my conviction by what Karl Bühler (1922) has termed "Du-Evidenz." What I mean is precisely the unity of his body *and* his soul, a unity which the cognitive functions I am endowed with make it quite impossible for me to doubt. And this is exactly why, of the three attitudes it is, in theory, possible to take toward the body-soul problem, one assuming identity, the second interaction, and the third parallelism between both, I find myself compelled to profess the one first mentioned.

For the practical purposes of psychophysiological research it is irrelevant which of these three positions one assumes, as long as one keeps conscious that none of them release us from the obligation to keep constantly in mind the existence and the nature of the impenetrable partition between our two fields of knowledge.

One of the most frequent mistakes is to regard this partition as being correlated to levels of integration or complication of different life processes. This error is already essentially contained in the current use of the terms "physiological level" and "mental level," as if the great partition were, in a manner of speaking, a *horizontal* one. In Hempelmann's otherwise admirable "Lehrbuch der Tierpsychologie" (1926) one finds this entirely misleading assumption practically on every page. The evolution of life processes, and particularly neural ones, is treated as if, up to a certain level of complication, they were "still" physiological and as if, above that level, they neither were standing in need of, nor accessible to, a physiological explanation. In reality, of course, the great partition runs *longitudinally* through all levels of complication of all life processes. There are quite simple ones, even on the "vegetative level," which percolate through the great diaphragm and appear, subjectively, as the most dramatic experiences of pleasure or displeasure, ranging from the ecstasies of voluptuousness to the throes of seasickness. There are, on the other hand, neurophysiological processes which, with respect to the complication of their function, equal the highest mathematical operations of our rational thought, and which are nevertheless entirely devoid of any subjective phenomena running parallel to them, as is the case with the amazing "computations" performed by many mechanisms of our perception, such as those of the constancy phenomena and those of Gestalt perception.

Another mistake, and probably the one most frequently found in literature dealing with psychophysiological problems, lies in connecting subjective phenomena as causes with physiological processes as effects and vice versa. Let me illustrate the fallacy of this procedure. Supposing a man has been punched on the jaw by another, we can describe what happens to him from the subjective as well as from the objective, physiological side. He experiences shock and pain, both of which frighten him, his fright causes a deep if momentary depression of his self-assurance which subsequently elicits anger and an urgent need for self-assertion which, in turn, finds its satisfaction in retaliating by another punch and enjoying the sweetness of revenge as well as the feeling of self-esteem restored. The objective, physiological account of the identical chain of events would be something like this: A slight concussion of the central nervous system and a strong stimulation of pain receptors causes a quickly passing paralysis of central functions; the man not only remains motionless for a moment "as if paralyzed," he really is, for the time being. His jaw sags, his head droops, his knees give, his skin becomes pale, all in consequence of a severe drop in the tonus of his sympathicus. In the next second the well-known process of rebound sets in, his adrenals spout, his sympathicotonus shoots upward, his eyes pop, his skin turns deep red, his striated muscles get taut, the temporary paralysis in his central nervous system gives way to excitation, and complicated, partly instinctive, fighting responses are released.

Obviously, it would be incorrect to regard any one of these events either on the subjective or on the objective side of the chain as being the *cause* of the phenomenon correlated to it on the other. One cannot be the cause of the other because it is, in fact, identical with it and only viewed from another, incommensurable side. Also, it would be incorrect to regard event number one, on one side, as the cause of number two on the other. The temptation to commit this breach of epistemological discipline is particularly great if the first event is familiar on the subjective, and completely unanalyzed on the physiological, side, while the opposite holds true of the second. Thus it is quite usual, in common parlance as in psychosomatic medicine, to say that, for instance, a severe disappointment causes a heart neurosis, etc., etc., connecting cause and effect right across the great partition, ignoring it completely.

These infractions of epistemological discipline would not be very dangerous, were it not for the fact that conceding the possibility of a psychological cause for a physiological effect offers a most welcome loophole for vitalistic "factors" to creep in. If we have today achieved a more or less general acceptance, in all behavioral sciences, of a disciplined epistemological treatment of the problems of psychophysiological correlation, this commendable state of affairs indubitably is, to a great extent, a merit of the behavioristic school which, by its rigorous self-restriction to the objective side, once for all severed the Gordian knot of the body-mind problem which seriously threatened to enmesh all behavior study. Perhaps the fact that the schools of behaviorism and ethology see eye to eye regarding these questions is an affinity that ought to do more than just compensate the discrepancies concerning other methodological points.

B. Purposive Psychology

This school, represented by William McDougall, E. C. Tolman, E. S. Russel (1934, 1945), Bierens de Haan, and others, makes a point of studying the purposiveness of animal and human behavior. E. C. Tolman (1932) has given a perfectly good definition of a purpose in exclusively objective terms: A purpose is given whenever an organism keeps on changing between varying types of behavior until a *constant* effect is achieved. If, for instance, a dog first attempts to jump over a fence, then, finding himself unable to do so, tries to tear a lath away with his teeth, then, this also being of no avail, digs a hole under the obstacle, and, having got in, eats the rabbit enclosed by that fence, it makes perfectly good sense for the objective student of behavior to say that the eating of the rabbit is a purpose to the accomplishment of which the other behavior patterns serve as means.

It is superfluous to emphasize the ubiquity and the importance of this organization of behavior and, therewith, the theoretical merit of pointing out that it *is* an organization of most definite survival value — which is just what the behavioristic opponents of McDougall (1933) and his school have failed to do. It is not a vitalistic assumption, but plain fact, that life processes very often keep running in one definite direction and are able to resume it again when put off course by extrinsic agencies. Any regulative process of this kind, compensating the effects of outer disturbances, must, of course, unconditionally be regarded as the function of a "built-in" regulating system, developed by the species under the selection pressure of just this function. It is of tremendous survival value of any species of higher animal if it has at its disposal a number of mutually exchangeable types of behavior accomplishing the same end, and, additionally, the faculty to learn which is the most suitable under a given set of circumstances. From the viewpoint of the biologist aiming at a causal analysis of behavior, showing up the existence of a purpose only means having established the fact that some kind of regulating system is at work; in other words, establishing the existence of a purpose means the discovery only, and not the solution, of a problem.

Some among the purposive psychologists cannot be spared the reproach that they did regard it as an answer to the question *why* behavior was running along certain lines when they succeeded in discovering *to what purpose* it did so. Contrary to behaviorists who tended to disregard adaptednesss and survival value of behavior, purposivists were keenly aware of their importance, but did not consider them as problems to be approached by natural science. "Wir betrachten den Instinkt, aber wir erklären ihn nicht" [We meditate on instinct, but we do not explain it] Bierens de Haan wrote in 1940.

But in criticizing William McDougall himself, even the most pedantically physicalistic student of behavior must concede that his insight into the nature of self-regulating systems was surprisingly little impaired by his professed belief that regulation as such neither stood in need of, nor was accessible to a natural explanation. Indeed, his representation of "instinctive" behavior and particularly the regulative interaction of "instincts" contains innumerable factual insights the scientific truth and value of which is in no way diminished by their discoverer's not believing that they were, on principle, explicable on the basis of physical laws. Although this disbelief prevents McDougall from seriously attempting physiological explanations of complicated and regulative processes in animal behavior, it does not hamper him in *describing* them in a detailed and absolutely correct way, emphasizing just those points which are, amazingly, the ones on which later physiological analysis found a foothold.

And absolutely the same holds true of William McDougall's treatment of the *spontaneity* of animal behavior. His slogan "The healthy animal is up and doing" which he continued to throw into the teeth of reflex theory, contains a core of indubitable truth. Exactly as in the case of purposive regulation of behavior, he did not search for a physical explanation of this truth. It is even somewhat doubtful whether he believed such an explanation possible on principle, but it is *not* open to any doubt that he *did* see the existence as well as the theoretical importance of facts which remained completely inexplicable by the theory of reflex and conditioned response and which, therefore, were simply negated by behaviorists. It is less damaging to the progress of scientific knowledge to refrain from explaining certain indubitable facts than to refrain from noticing *and describing* them — only because they fail to fit in with a preconceived theory. In other words, the impediment of causal analysis inherent to purposive psychology did not prevent it from giving a detailed and correct description of those fundamental phenomena in animal and human behavior which *stand in need* of causal analysis. In any case, purposivists know what some behaviorists obviously still refuse to believe, that observation and description have to precede analysis in any study of natural processes.

A comparison of observational and descriptive behavior studies performed by purposive psychologists on the one hand, and by ethologists on the other hand, shows a striking affinity of both: Both would be termed "naturalistic" in the parlance of behaviorism. In reading McDougall the ethologist cannot help feeling that facts are being brought to his notice which are highly relevant to the understanding of animal behavior — shortly and naively expressed, that McDougall "knows more about animals" than anybody believing their behavior to be explicable in terms of reflexes and conditioned responses alone. His descriptions display a most acute vision for natural units in the structure of behavior, in other words for *organization*. He is quite explicit about the fact that "appetite," or what ethologists would objectively describe as appetitive behavior, owes its existence to an entirely different "organization" than instinctive behavior patterns. He has formed, in all precision, the concept of patterned organization on the receptor side of behavior, which he called the innate "perceptual pattern." He knows all about the interaction of independently variable instinctive motivations and about the phenomena arising in case of their conflict, down to a quite correct description of what ethologists, much later, were to call displacement activities. Also his basic assumption concerning the different organization of unlearned behavior and perceptual patterns on one side, and of learning and insight on the other, and quite particularly his views on the manner in which the two last-named functions serve to integrate, into a functional whole, the several particulate elementary unlearned patterns, are nearly identical with the views held by ethologists on the same subjects.

On the other hand, even the greatest admirer of McDougall has to concede that he very often assumes an attitude that is not only indifferent but positively hostile to causal explanation in general and to explanation on the basis of conditioned response in particular. It is characteristic of a great genius with an almost visionary insight into the great coherent contexts of wholes that he tends to despise details and also people insisting that the whole is explicable on the basis of details. If I say that McDougall behaves, in this respect, exactly like Goethe, this surely expresses the rank I am ready to accord to him. What he refuses to see, is that causal explanation is altogether compatible with the existence of wholes or systems, that the possibility of a physiological explanation of behavior does not preclude subjective consciousness nor the existence of purpose, and that the conditioned response is a particulate element of behavior which, so far from being incompatible with the conception of wholes or systems, plays the most important part in integrating other behavior elements into a whole or a system.

Wherever, in his writing, he describes examples of animal behavior which quite indubitably consist in conditioned responses, he hastens to repudiate this assumption by describing, most correctly, the subjective phenomena, particularly those of perception and of pursuing a purpose, which, certainly in men and possibly in animals, are the subjective correlates of this type of behavior. It never seems to occur to him that *both* ways of describing the facts are *equally* correct.

Similarly, he treats instinctive behavior as if the fact that it indubitably serves survival were incompatible with its being naturally caused. As instinct, to him, is an infallible directive factor, he is prevented from taking into consideration what, to ethology, has been one of the most important sources of information: the illuminating cases in which the organization of instinctive behavior miscarries and fails to achieve its normal survival value. To him, miscarriage is proof that, by definition, the behavior in question is not caused or directed by instinct. Thus, the only case known to me, in which McDougall actually proposes a causal explanation of behavior, concerns the case of insects killing themselves by flying into a light. Discussing this in his book "Outline of Psychology" (1933, p. 64), he suggests that a photo-tropism in the sense of Jaques Loeb (1913) might be the cause.

C. Jakob von Uexküll

If there is, in the history of science, one really convincing illustration of Hegel's doctrine that any thesis is true only when taken in conjunction with its antithesis, it is furnished by antagonistic attitudes assumed, by purposive psychologists on one side and by behaviorists on the other, in relation to the epistemological problems already discussed. I have tried to show, in another paper (1950) that both parties tended to extreme positions which neither of them ever would have taken, had it not been in contradiction to the opposite opinion. This was particularly deplorable in respect to the methodological treatment of self-regulating systems: Because directed regulation, as I tried to define it (page 90) was regarded as the effect of an entelechial "factor" by purposivists, self-regulating systems were strictly ignored by behaviorists who, on principle should have been ready and able to undertake a physiological, causal explanation of these processes.

I have attempted to make clear, at the very outset of this presentation, that thinking in terms of complicated, structured systems is not only compatible with, but fundamental to, the endeavor to "understand" life processes in general, and behavior in particular, in a "natural," that is to say in a physicalistic, manner. I have tried, in my parable of the motorcar found by the Martian explorers, to sketch the method forced on inductive research by the nature of systems.

Curiously enough, it was a self-professed, dyed-in-the-wool vitalist, who first consciously and consistently applied that method of "analysis on a broad front": Jakob von Uexküll, whom, in spite of fundamental philosophical dissension, I regard as one of my most important teachers. The point of departure, in all his investigations, is a complex of observational, empirical facts representing a *system* in which the organism and its environment are found to stand in a relationship of multiple, mutual interactions. Analysis invariably has to begin with the question: Which, among the many data of environment, are the ones that, on one hand, have a releasing effect on certain behavior patterns of the animal and are, on the other hand, influenced and changed, by the activity thus released, in such a manner that their change has, in its turn, a repercussion on the organism's responses? All data, or complex of data which, in this way, are sending releasing stimuli to the animal's receptors and, at the same time, are offering points of attack to the animal's effectors, represent the "things" constituting the animal's "world." The system which von Uexküll (1921), regards as the elementary unit of behavior, is the *junctional cycle (Funktionskreis)*. It consists in the circular chain of causes and effects which — running through the organism and its environment, from the stimuli impinging on the animal's receptor organs, on through its nervous system to its effector organs, then back into environment — by the functions of its receptors and its effectors, divide its world into two sectors, the perceptual field *(Merkwelt)* comprising all the receptor cues *(Merkmale)* to which the animal responds, and the effector field *(Wirkwelt)* consisting of all the points of attack which environment offers to the animal's effectors (Fig. 1).

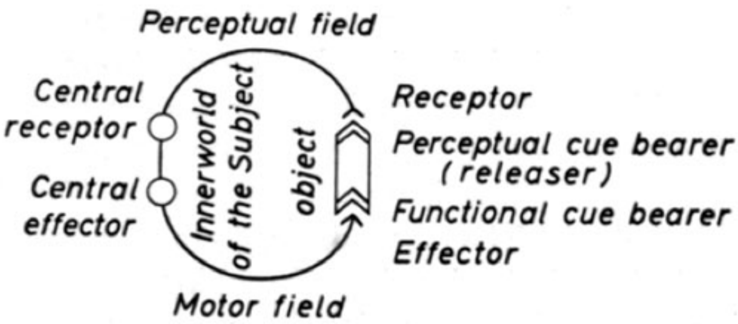

Fig. 1. See text for explanation. From von Uexküll (1921).

In this simple diagram, a vast program of research is implied. The investigation of a species of animals and of its behavior must necessarily begin with the endeavor to ascertain the number and the properties of its functional cycles, to draw up an inventory of the receptor and effector cues taking part in the cycle, and to analyze their causal interaction with the structure of the organism, on one side, and that of environment on the other. The organism's "inner world" *(Innenwelt)*, comprising the whole of its bodily structures and/or functions, is causally influenced from the side of the "perceptual field" *(Merkwelt)*, that is to say, those parts of environment which affect its receptor organs, in other words those which are selected, out of innumerable others, by the specific organization of the receptors. The conception of the latter does not by any means include the sense organs only, but also the whole organization which, within the central nervous system conveys specifically releasing stimulation in the direction toward effector organizations. The function of receptors is not only to receive releasing stimuli, but also to exclude all others from becoming effective. All the stimulus data originating from receptor cues converge centerward into a network of interactions *(Merknetz)* which constitute the central representation and the unity of the receptor cue. On the effector side, a corresponding network *(Wirknetz)* serves to integrate and coordinate the single muscle contractions into an activity which represents an adaptive response to the cues received. The task of investigating the biology of any species of animals can be regarded as fulfilled only when all functional cycles are fully analyzed and when we have gained full knowledge of what "strings" keep the organism suspended, in a steady state, within its environment. These strings of course, are, as we all know, causal chains interlocking by hook and eye. It is mechanical problems that are confronting us on every side.

These statements quoted, for the greatest part literally, from von Uexküll's book "Umwelt und Innenwelt der Tiere" (1921), hardly sound vitalistic! The research program mapped out in them is pretty nearly identical with that of ethology, very many of the provisional, more or less operational concepts of part functions are the same, even if semantics are different, von Uexküll has by no means confined himself to stating a program but has, with very many species of invertebrates, driven causal analysis of behavior to a point where he "knew the strings by which an animal is suspended in its environment" to a degree hardly ever surpassed by an ethologist.

Although the starting point and frame of reference for all his considerations always was the animal *as subject,* the method of his research always was objectivistic to the extreme. Together with Beer and Bethe he devised a nomenclature avoiding all subjective terms to describe internal states and processes within an animal's central nervous system. Even his conception of the animal's subjective world is determined by what he found objectively, represented in its responses. Even a purist of objectivism could find no fault with von Uexküll's method in that respect.

In some respects, however, ethologists and, for that matter, all nonvitalistic natural scientists must file a protest against von

Uexküll's views. One of these is that he altogether rejects the fact of evolution. As a consequence, he is prevented from regarding adaptation, or, to be more precise, adaptedness of structure and/or behavior as the result of a natural process of historical, causally determined development, of phylogeny. The *Bauplan,* a pre-existing, pre-established harmony of organic and environmental structure accounts, in his opinion, for the amazing fact that all organisms are equally well adapted to their respective environments, and, more particularly for the adaptedness of behavior which hinges on the other surprising fact that, in the animal's world, it is always the same thing in which the receptor cues, releasing certain activities coincide with the properties offering points of attack to these activities, in such a way that behavior achieves survival value.

If it is only the biologist conscious of evolution who takes exception at these decidedly preformationistic views, I doubt whether any natural scientist can follow von Uexküll's philosophy of knowledge which is somewhat reminiscent of Leibnitz's doctrine of "monads." Von Uexküll repudiates the belief in the existence of any sort of extra-subjective real universe, common to all organisms living in it. What, in his opinion, is real, are only the innumerable particulate worlds of all the innumerable animal and human subjects.

It was lucky for the progress of behavior study that von Uexküll, in his day's work, did not remain consistently faithful to his own philosophical doctrines. Even his classic diagram of the functional cycle which proved so fruitful in his research, is fundamentally incompatible with the doctrine of the nonexistence of extra-subjective reality. What is marked "object" *(Gegengefüge)* in Fig. 1 obviously represents the structure of outer reality which is influenced by the animal's activities and, by being changed, exerts an influence on the organism in return, indubitably is assumed to be real, though it does *not* appear in the animal's "world"!

Another example of how von Uexküll accomplished grand work by not adhering too strictly to his professed philosophy concerns his experimental treatment of receptor cues, of what ethologists would call key stimuli. According to his doctrine of the

Bauplan, it is due to pre-established harmony that any "thing" in the animal's world unites, in itself, those properties which, by sending out receptor cues release the animal's response, with those others that offer the right points of attack to the activities released. The tick, *Ixodes rhitinus* L., whose normal hosts are mammals, responds to two main receptor cues: The releasing object must have a temperature of approximately 37°C. and must smell of butyric acid. In nature, admirably, such an object practically always will offer, to the tick, the "effector cues" of being soft, stingable, and possessing blood vessels for the tick to suck. Receptor cues and effector cues always coincide, or at least they *did* until von Uexküll himself made them discontinue doing so, at least in the laboratory, by presenting the tick with suitable dummy objects and showing that the tick will blindly sting anything offering the two key stimuli mentioned. This experiment opened the path to the investigation of what physiologists of behavior today call the innate releasing mechanism, that is to say the organization of sense organs and the afferent structures within the central nervous system which cause the organism to mechanism.

Though von Uexküll repudiates just those problems and just those phylogenetic methods which are most characteristic of ethology, this young science certainly owes more to his teaching than to any other school of behavior study. He was the first to show clearly that (1) causation and survival function of behavior are two points of view which not only can be, but have to be, considered simultaneously; (2) subjective interpretation and physiological analysis of behavior are compatible, though the two aspects must never be confounded or mixed; (3) the realization that organisms and their behavior are forming, together with their environment, a "whole" or *system* is not an obstacle at all to the attempt to explain that system on the basis of natural laws.

D. Herbert S. Jennings

I have attempted to give a very short sketch of the different, independent schools of behavior study, just sufficient to show their agreement and disagreement with the etiological approach. Even this necessarily crude abstract would be incomplete without paying tribute to the man who, independently of and synchronously with Jakob von Uexküll, took the all-important step of applying to animal behavior, those methods which are imperatively dictated to research by the nature of its object, namely a structured regulating system. It was H. S. Jennings who, in his classic work "The Behaviour of Lower Organisms" (1904), clearly defined the conception of the *system of actions* characteristic of a species.

Although by virtue of his background and his schooling he probably ought to be regarded as a behaviorist, his methods as well as his results agree and converge with those of von Uexküll in a surprising number of details. It is highly gratifying and, indeed, reassuring that absolutely identical results can be attained even on the basis of entirely incompatible philosophies, if only the good old method of inductive approach is applied, the method that begins with a thorough and unprejudiced observation and description of facts and proceeds to analytical and abstract nomothesis only after having accumulated and systematized a sufficient basis of fact to build upon. This method is not only dictated by the epistemological considerations on which I enlarged in the first part of this presentation, but by the simplest common sense as well. Moreover, this method comes automatically to those whose eyes are held to the object of their observation by that spell which the beauty of organic nature casts on some of us. Because this aesthetic appreciation is closely allied with the faculty of perceiving wholes or systems, in other words of Gestalt perception, no single investigator of nature who is gifted that way will ever be an atomist. On the other hand, this very gift may turn into a curse for the progress of our knowledge for a very simple reason. Remnants of idealistic philosophy, with which our whole Western culture is imbued more than most of us realize, make it impossible to some otherwise astute thinkers to *attribute value to anything explicable in a natural way.* In such cases, the very reverence engendered in the student of organic nature by its beauty and harmony, makes it impossible to him to *want* to analyze and understand it in a natural way. He cannot help feeling that any explication is a devaluation, or even a desecration. This attitude is so characteristic that it could be used as a personality test for students of behavior, it indubitably has caused innumerable excellent men to join the ranks of the vitalists, and it also explains why so many among the antagonists of natural explanations are, surprisingly, such very nice people.

The uniqueness of H. S. Jennings simply lies in the fact that his gift to perceive harmonious systems and his obvious appreciation of their beauty did *not* in any way conflict with his quest for their causal explanation. It is with a strong sense of indebtedness that I remember how, on Karl Bühler's advice, I read the "Behaviour of Lower Animals" and how it suddenly flashed upon me that acknowledging the existence of wholes or systems did not imply a confession to vitalism. To a young student this truism bore the character of a revelation.

The importance of H. S. Jennings can be expressed in one sentence. He was the first student of behavior who was not a vitalist, and yet approached its problems by a method adequate to its character of a whole or system.

IV. Methods Peculiar to Ethology Proper

There are no methods that can justly be said to be exclusively characteristic of ethology, unless one regards it only as a school of behavior study, and not as the branch of biology which it really is. As a branch of biology, ethology originated, as a new branch of science legitimately should, with a discovery that opened a new line of investigation. Charles Otis Whitman (1899), and Oskar Heinroth (1911) discovered independently of each other that there are certain motor coordinations which are just as reliable and widely spread taxonomic characters, as any morphologic properties one can think of. In other words, these movements are just as constant characters of a species, a genus, an order, or even larger taxonomic category as any of the bodily structures used in their definition and identification. This discovery had important consequences, mainly because it upset most of the then accepted theories of "instinct." In that sense it was something new.

But of the method that had led to it, the very opposite is true: It was the orthodox method of comparative morphology, the method of arranging the properties of species systematically in order of similarity and dissimilarity, the method that had already taken an important part in leading Charles Darwin (1859) to *his* discoveries, that now, when applied to the new field of behavior study once again achieved unexpected results.

Of course, the very peculiar physiological nature of the fixed motor patterns thus discovered necessitated special methods of investigation. In spite of their great constancy of form, these motor patterns are definitely not chains of reflex-like processes, as was formerly assumed. Rather, their physiological nature is the same as that of certain movements based on endogenous, automatic production of motor stimuli within the central nervous system, a type discovered and investigated by Adrian (1950), von Holst (1935, 1936), Paul Weiss (194la,b), K. D. Roeder (1955), and others. The fixed motor patterns are coordinated in a highly peculiar way by processes in which afferent processes in general and proprioceptors in particular take no part and which have been investigated and analyzed by E. von Holst. The physiology of these movements, the way in which they are organized to form an integrated regulative system, the physiological mechanisms inhibiting them during quiescence and setting them off at the biologically adequate moment, and so on, all necessitate methods of research specially adapted to the process investigated.

But again it would be entirely misleading to call these methods exclusively characteristic of ethology. They are also characteristic of the branch of physiology represented by the investigators mentioned above and of others. As a matter of fact we are applying, in my department, such methods, for instance that of electric stimulation in the hypothalamic region, which was first used by W. R. Hess (1943) on cats and later modified and adapted to use on birds by E. von Holst. Our opinion of the importance of « these physiological methods is borne out by the fact that I have moved into one institute with E. von Holst.

V. Summary

I hope that I have succeeded in convincingly supporting my anticipatory assertion that ethology does not, in its fundamental philosophy, its epistemological attitudes, its methods of approach, and its "strategy of research," differ from any other natural sciences, least of all from biology. Where it does differ from certain schools of psychology, orthodox evolutionistic biology does the same. Ethology can be briefly defined as the application of orthodox biological methods to the problems of behavior. If, in the course of my presentation, I have criticized other schools of behavior study, accusing behaviorism of atomism, of explanatory monism, and of a neglect of structure, accusing purposive psychologists of finalism and vitalism, and von Uexküll of preformationism, if I have even slightly reproached H. S. Jennings for not introducing, into his considerations, the phylogenetic viewpoint, I must very sincerely beg you to believe that I offer these criticisms without the slightest feeling of superiority. Ethology certainly has no right to claim any merit for adhering to the old methods of biology, which is its mother. All other schools of psychology and behavior study are descended from philosophy sired by great philosophers — behaviorism by Rene Descartes, purposive psychology by Aristotle and Plato, and von Uexküll's *Umweltforschung* by Leibnitz — whereas ethology has, for a father, a very plain zoologist: Charles Darwin.

REFERENCES

Adrian, E. D. (1950). *Symposia Soc. Exptl. Biol.* 4, 85.

Bierens de Haan, J. A. (1940). "Die tierischen Instinkte und ihr Umbau durch Erfahrung. Eine Einführung in die allgemeine Tierpsychologie." Brill, Leiden.

Bohr, N. (1958). *Daedalus.*

Bridgman, P. W. P. (1958). *Daedalus.*

Brunswik, E. (1957). *In* "Contemporary Approaches to Cognition" (J. S. Bruner *et al.,* eds.). Harvard Univ. Press, Cambridge, Massachusetts.

Bühler, K. (1922). "Handbuch der Psychology." 1. Teil, die Struktur der Wahrnehmungen. Fischer, Jena.

Campbell, D. T. (1958). "Methodological Suggestions from a Comparative Psychology of Knowledge Processes." Oslo Univ. Press, Inquiry.

Darwin, C. (1859). "Origin of Species." London.

Darwin, C. (1868). "Variations of Animals and Plants under Domestication." London.

Descartes, R. (1924). "Philosophische Schriften." Berlin and Vienna.

Heinroth, O. (1911). *Verhandl. V Intern. Ornithol. Kongr., Berlin.*

Heinroth, O. (1928). "Die Vögel Mitteleuropas." Borntraeger, Berlin.

Hempelmann, F. (1926). "Lehrbuch der Tierpsychologie." Leipzig.

Hess, W. R. (1943). *Helv. Physiol. et Pharmacol. Acta* 1.

Jennings, H. (1904). "A Contribution to the Study of the Behaviour of Lower Organisms." Washington, D.C.

Kennedy, J. S. (1954). *Brit. J. Animal Behavior* 2.

Lehrman, D. S. (1953). *Quart. Rev. Biol.* 28.

Loeb, J. (1913). Tropismen. Hdb. d. vgl. Physiol. Bd. 4, Jena.

Lorenz, K. (1937). *Naturwissenschaften* 25.

Lorenz, K. (194la). J. *Ornithol*. 89.

Lorenz, K. (I94lb). *Bl. deut. Philosophie* 15.

Lorenz. K. (194lc). J. *Ornithol*. 89.

Lorenz, K. (1943). *Z. Tierpsychol*. 5.

Lorenz, K. (1950). *Symposia Soc. Exptl. Biol*. 4, 221.

Lorenz, K. (1954). In "Die Evolution der Organismen" (G. Heberer, ed.), pp. 131-172. Fischer, Jena.

Lorenz, K. (1959) *Z. exptl. u. angew. Psychol*. 6(1).

McDougall, W. (1933). "Outline of Psychology." 6th ed. Scribner, New York.

Matthaei, R. (1929). "Das Gestaltproblem." München.

Metzger, W. (1936). "Gesetze des Sehens." Frankfurt.

Planck, M. (1942). *Naturwissenschaften*.

Roeder, K. D. (1955). *Sci. Monthly* 80, 362-370.

Russel. E. S. (1934). "The Behavior of Animals." London.

Russel, E. S. (1945). "The Directiveness of Organic Activities." Cambridge.

Tolman, E. C. (1932). "Purposive Behavior in Animals and Men." Appleton-Century, New York.

von Holst, E. (1935). *Pfluger's Arch. ges. Physiol*. 236(4-6).

von Holst, E. (1936). *Pfluger's Arch. ges. Physiol*. 237(1); 237(3).

von Uexküll, J. (1921). "Umwelt und Innenwelt der Tiere." Berlin.

Watson, J. B., and MacDougall, W. (1929). "The Battle of Behaviorism." New York.

Weiss, P. (1941a). *Proc. Am. Phil. Soc*. 84.

Weiss, P. (1941b). *Comp. Psychol. Monogr.* 17.

Whitman, C. O. (1899). Animal Behavior. Biol. Lectures Marine Biol. Lab. Wood's Hole Boston.

Windelband, W. (1894). "Geschichte der Naturwissenschaft." Strassburg.