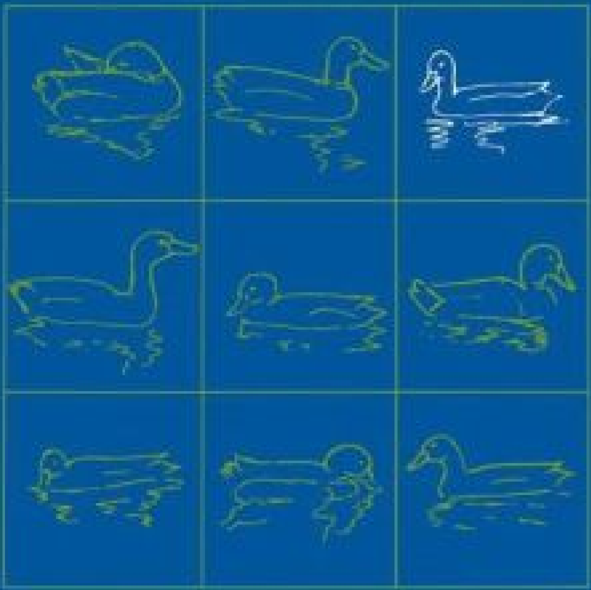
Konrad Z. Lorenz

The Foundations of Ethology



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Translated by Konrad Z. Lorenz
and Robert Warren Kickert

With 34 Figures

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To Nikolaas Tinbergen

Foreword

This book is a contribution to the history of ethology—not a definitive history, but the personal view of a major figure in that story. It is all the more welcome because such a grand theme as ethology calls for a range of perspectives. One reason is the overarching scope of the subject. Two great questions about life that constitute much of biology are "How does it work (structure and function)?" and "How did it get that way (evolution and ontogeny)?" Ethology addresses the antecedent of "it." Of what are we trying to explain the mechanism and development? Surely behavior, in all its wealth of detail, variation, causation, and control, is the main achievement of animal evolution, the essential consequence of animal structure and function, the raison d'etre of all the rest. Ethology thus spans between and overlaps with the ever-widening circles of ecology over the eons and the ever-narrowing focus of physiology of the neurons.

Another reason why the history of ethology needs perspectives is the recency of its acceptance. For such an obviously major aspect of animal biology, it is curious how short a time—less than three decades—has seen the excitement of an active field and a substantial fraternity of workers, the addition of professors and courses to departments and curricula in biology (still far from universal), and the normal complement of special journals, symposia, and sessions at congresses. Of course, animal behavior has been a subject of serious writings, usually called natural history, for centuries. However, for reasons that need historical perspectives to be evaluated, the dignity of a major discipline long escaped all but the facets embraced by psychologists.

Did the arrival and spread of acceptance of the modern study of free animal behavior await the impetus of a school, of advocates, of theories and models? Surely Konrad Lorenz was a major factor, through his thought-provoking approach, his eloquence, his inspiration to many students—and above all his intimate and wide familiarity with animals in nature.

When a figure so largely responsible for the emergence and development of a scientific approach chooses to summarize the field, as Konrad Lorenz does here for ethology, it is an historical document and worth attention. It is all the more important for a subject so bound up with concepts, conceptual methodology, and interpretation, and so dependent on accumulated experience in watching animals. It is Lorenz's perception that "fashion" and "ideological prejudice" have obscured our familiarity with some of the basic foundations of ethology, even on the part of knowledgeable authors, so that a main aim of this work is to remind us of the historical origins and of "how narrow that factual foundation is and hence how needful is thorough verification."

One cannot but wonder whether the brigades of workers who have followed Lorenz's lead have not provided either that verification or some modification. It is therefore an understandable hope of some readers to find Lorenz's evaluation of newer data and his position on the debates over concepts or terms. However, this would have meant virtually a full review of the accumulated literature and such is not Lorenz's aim within these covers. On the whole, apart from a few interesting admissions of changes in viewpoint and terminology, one has to look hard for changes in usage, definition, or concepts made as a result of the findings and reinterpretations of later workers.

It is only to be expected that such a document will be highly personal. Moreover, it is bound to sound familiar. But a close reader of Lorenz will note many new or refined positions. Of special interest to those who think of ethology as mainly concerned with unlearned, instinctive behavior of non-mammalian taxa will be two features. One is a series of chapters devoted to adaptive modification of behavior. In one of these Lorenz distinguishes between two forms of operant conditioning, a form which he believes is common in nature, and another that is quite uncommon. The other feature is an appendix devoted to *Homo sapiens.* Here he gives reason to claim that the science of human ethology has much to teach us, all the more so because the human species, while not outside the natural biological order, is very special indeed in cultural heritage and the elaboration of language.

Only a personal restatement of the foundations by one of the founders can have the poignancy this book has as the result of changes in the prevailing intellectual climate, such as the new respectability of discussions about animal consciousness and mental life. Not that such changes have an easily predictable effect, for example, in making Lorenz's viewpoint more persuasive or some ogres less like straw men. It is certainly one of the charms of this book to see for oneself what the arguments sound like in the light of all that has happened in science and society. Lorenz lets us reexamine not only the factual foundations but the methodological and even some philosophical bases. The reexamination may not always lead to greater sympathy for the Lorenzian alternative but should lead to better history and a greater motivation to broaden the base.

Appreciation of this book and the wealth of observation of nature it represents is enhanced if one adapts to Lorenz's methods of exposition. Each reader will no doubt develop his or her own approach. For example, I learned not to judge or even to try to absorb the sweeping induction that opens the paragraph, but to wait until the example unfolds that makes the key terms understandable. It is best not to be put off by expressions such as "explanatory monism," "atomism," "technomorphically," "teleonomy," or "relatively entirety-independent." But these are no doubt the limitations of a relatively example-dependent physiologist-reader! I could not expect every familiar term to be defined or strong language to be eschewed ("never," "any at all," "exploded"). And I learned that one road to understanding is to contrast the right view with a delineation of the bad guy's view.

There have been various images of scientists: bricklayers adding facts to erect an edifice of knowledge, revolutionaries putting together discrepancies to overturn established "paradigms," delvers uncovering nuggets of gold, and wise men avoiding the Baconian idols of the marketplace and idols of the tribe. This book conjures up the image of a slayer of dragons; the preferred form of exposition of scientific advance is to show how wrong was some previous view. Most authors cited are either protagonists or adversaries, and the latter are astonishingly bad. The list of bad guys is long: vitalists, monists, reflexologists, Pavlovists.

One reads on, captivated by the parade of striking examples. The enduring images are the vivid descriptions—of a shrike holding an object in its bill and searching for thorns, or performing impaling motions in vacuo; of a goose, satiated with corn but deprived of an opportunity to up-end and gather food from the pond bottom, so that when offered corn thrown into the pond at the right depth, began "feeding for the sake of up-ending instead of up-ending for the sake of feeding"; of the experienced male cichlid fish that is not fooled by the best fish-like dummy, in contrast to the male raised in isolation that gives a full male courtship response when the simplest dummy moves into view. While unusual for fish, this drives home both the nature of the evidence for innate releasing mechanisms and the possibility of refining both the sign stimuli and the recognition process by learning. These and scores of other graphic instances are mostly familiar to ethologists as classical examples; they form the solid empirical foundations of ethology. It is understandable how they invite categorization, concept formation, the induction of entities, and the attempt to interpret in some sort of crude mechanistic terms. It is too much to ask that the ethologist "stick to his last" and avoid wandering into physiology. We should expect and welcome such excursions, and judge their success not so much by the sophistication or modernity of the physiological model but by how heuristic it is in suggesting things to look for.

Rarely does a founder of a field give us his insider's view of it. Would that other participants in the founding of ethology were moved to emulate Dr. Lorenz in sharing their impressions of its development. In such a spectrum of memoirs, this book would have a key position.

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Preface

In some respects, the development of a science resembles that of a coral colony. The more it thrives and the faster it grows, the quicker its first beginnings—the vestiges of the founders and the contributions of the early discoverers—become overgrown and obscured by their own progeny. There is one drawback to the strategy of growth pursued by the coral tree. The polyps at the end of its branches have a much better chance of further development than those situated near the foundation. The ends go on growing faster and faster without considering the necessity for strengthening, in proportion, the base that must carry the weight of the whole structure. Unlike an oak tree, the coral colony does nothing to solidify its support. Consequently, there is a lot of coral rubble detached from points of departure, and this is either dead or, if still partly alive, growing in indeterminate directions and getting nowhere.

Having myself grown very near the point from which ethology, as a new branch of biological science, had its origin, it may seem presumptuous if I compare the present state of our science to a coral colony whose branches, by losing contact with their foundation, are producing quite a lot of rubble. However, there is no doubt that they do, and I *am* presumptuous enough to criticize this. My justification lies in the fact that really important discoveries, such as those made by Charles Otis Whitman, Oskar Heinroth, Erich von Holst, Kenneth Roeder, and others, are being forgotten, and for reasons, I contend, which are partly to be found in mere fashion, and partly in ideological prejudices.

So this book is not an up-to-date textbook on ethology. It does not presume to include all the most recent developments within this science, not even those which mean very real advances. Its aim is to remind scientists in general of the basic facts on which ethology is founded, and to remind ethologists in particular of how narrow the factual foundation of our science really is, how completely our science relies on these facts being correct, and how much, therefore, their thorough verification remains necessary.

Contents

Introductory History

Ethology, the comparative study of behavior, is easy to define: it is the discipline which applies to the behavior of animals and humans all those questions asked and those methodologies used as a matter of course in all the other branches of biology since Charles Darwin's time.

When one considers with what rapidity the ideas of evolution, and particularly the Darwinian concept of natural selection, caught on in almost all branches of biology, one searches for an explanation as to why these ideas were so tardily accepted by the disciplines of psychology and behavioral science. The main reason that biological thinking and especially comparative methods were prevented from penetrating the study of behavior was an ideological dispute between two prominent schools of psychology.

The bitterness with which this dispute was pursued was nourished, above all, by the diverse philosophies of the antagonists. The school of purposive psychology, represented primarily by William MacDougall and later by Edward Chase Tolman, postulated an extranatural factor: "instinct" was regarded as an *agens* or agency neither in need of nor accessible to a natural explanation. "We consider an instinct but we do not explain it," wrote Bierens de Haan as late as 1940. To this conception of instinct was always also appended a belief in its infallibility. MacDougall rejected all mechanistic explanations of behavior. For example, he considered it a consequence of instinct when insects pressed forward purposively toward light; he conceded the possibility of a mechanistic explanation, through tropism, only in those cases where these animals, most unpurposively, flew into a burning lamp. According to MacDougall and his school, everything animals do is in pursuit of a purpose and this purpose is set by their extranatural and infallible instinct.

Those of the behaviorist school of psychology justifiably criticized the assumption of extranatural factors as unscientific. They demanded causal explanations. Through their methodology they sought to place themselves as much apart as possible from the purposive psychologists. They regarded the controlled experiment as the only legitimate source of knowledge. Empirical methods were to take the place of philosophical speculation.

With the exception of a certain lack of appreciation for simple observation, this program incorporated no methodological error, and yet it brought about an unfortunate consequence: all research interests were concentrated on those aspects of animal and human behavior which readily lent themselves to experimentation—and this led to explanatory monism.

A combination of William Wundt's (1922) association theory with the reflex theory (reflexology) that was then dominating the fields of physiology and psychology, as well as with the findings of I. P. Pavlov (1927), facilitated the abstraction of a behavior mechanism—the so-called *conditioned reflex*—the qualities of which marked it as ideal for experimental research.

At that time the corrective criticism made by the behaviorists concerning the opinions held by the purposive psychologists was salutary in every way. But, unobserved, a ruinous logical error crept into behaviorist thinking: because only learning processes could be examined experimentally and since all behavior must be examined experimentally, then, concluded those of the behaviorist school, all behavior must be learned—which, naturally, is not only logically false but also, factually, complete nonsense.

Knowing the views of those in the opposition, and having made a justifiable critique of those views, the purposive psychologists as well as the behaviorists were pushed into extreme positions which neither of them would otherwise have taken. While those of one group were imbued with a mystical veneration of "THE instinct" and attributed excessive capacities, even infallibility, to the inborn, those of the other group denied its very existence. The purposive psychologists, who were quite aware of innate behavior patterns, regarded everything instinctive as inexplicable and, just as Bierens de Haan (1940), refused even to attempt a causal analysis. Those others who certainly would have been capable and ready to undertake such analytical research denied the existence of anything inborn, and dogmatically declared that all behavior was learned. The truly tragic aspect of this historical situation is that the purposive psychologists, particularly MacDougall himself, knew animals well and possessed a good, general knowledge of animal behavior, something which is still lacking among the behaviorists even today, because they regard simple observation as unnecessary, in fact, as "unscientific." In this context, the truth of a statement of Faust's comes to mind: "What one does not know is exactly what one needs, and what one does know one cannot use."

This ideological dispute between these two schools of psychology was still being actively pursued when, completely unnoticed by both and independent of their influence, the scientific study of innate behavior patterns came into being. At the turn of the century Charles Otis Whitman and, a few years later, independently of him, Oskar Heinroth discovered the existence of *patterns of movement*, the similarities and differences of which, from species to species, from genus to genus, even from one large taxonomic group to another are retained with just as much constancy and in exactly the same way as comparable physical characters. In other words, these patterns of movement are just as reliably characteristic of a particular group as are tooth and feather formation and such other proven distinguishing physical attributes used in comparative morphology. For this fact there can be no other explanation than that the similarities and dissimilarities of these coordinated movements are to be traced back to a common origin in an ancestral form which also already had, as its very own, these same movements in a primeval form. In short, the concept of *homology* can be applied to them.

These facts alone prove that these movements originate phylogenetically and are imbedded in the genome. It is just this that is overlooked by those students of behavior who would like to explain away every conceptual distinction between innate and acquired characteristics. When the African black duck *(Anas sparsa)* living on tropical rivers, the mallard living on our own lakes, the many species of wild ducks living on the ponds of zoos, and the domesticated ducks living in the barnyards of our farms, in spite of the differences of their environments and despite all the possible influences of captivity, display courtship movements that are unmistakably similar in a countless number of characteristics, then the program for these movements must be anchored in the genome in a manner exactly identical with that in which the program of morphological characters is coded in the genes. If, after this discovery, theories concerned with the problem of "nature versus nurture" continue to be published, this is explainable only through the assumption that these authors are unaware of the discoveries made at the turn of the century, or that they have chosen to ignore them. That they indeed do this was soon clear to me. American psychologists often visited Karl Bühler's institute. I asked each and every one of them whether they knew the name Charles Otis Whitman. Not one of them knew the name.

The discovery that movement patterns are homologous is the Archimedean point from which ethology or the comparative study of behavior marks its origin. Paradoxically, even the work of authors who deny the essential difference between innate and acquired behavior mechanisms is built upon the same factual base.

I discovered for myself, independently of Whitman and Heinroth, that patterns of movement are homologous. When studying at the university under the Viennese anatomist, Ferdinand Hochstetter, and after I had become thoroughly conversant with the methodology and procedure of phylogenetic comparison, it became immediately clear that the methods employed in comparative morphology were just as applicable to the behavior of the many species of fish and birds I knew so thoroughly, thanks to the early onset of my love for animals. Soon after this I met Oskar Heinroth, the discoverer of my discovery, and early in the 1930's both of us learned through communication with the American ornithologist, Margret Morse Nice, that Charles Otis Whitman had come to essentially the same conclusions as Heinroth about ten years earlier. At the same time all this was happening, we met the most distinguished of Whitman's students, Wallace Craig.

Neither Whitman nor Heinroth ever expressed any views concerning the physiological nature of the homologous movement patterns they had discovered. My own knowledge of the physiology of the central nervous system came from lectures and textbooks in which the Sherringtonian reflex theory (1906) was regarded as the last word and the incontestable truth. The expression "reflex" evokes, linguistic-logically, the vision of a simple, linear causal relationship between the incoming stimulus and the response given to it by the organism. In this simplicity lies the seductive effect of the concept: It is just as easy to understand as it is to teach.

Under Karl Bühler's tuition I gained enough knowledge of the two prominent schools of American psychology to feel myself qualified to criticize them on two fundamental points. The first was that the infallible, preternatural "instinct" in which the purposive psychologists believed simply did not exist; too often had I seen innate behavior patterns taking their course in completely blind and senseless sequences. The second criticism was that the point of view of the behaviorists, that all animal behavior is learned, was totally false.

I had published several short articles, based on my own observations, about the problem of the innate and homologous in motor patterns when my friend, Gustav Kramer, imposed himself on the course of these events by influencing the biologist, Max Hartmann, to invite me to give a lecture to the Kaiser Wilhelm Society for the Advancement of Science (now the Max Planck Society). Kramer was carrying out his intention of providing a setting for a discussion between Erich von Holst and me. He was von Holst's friend as well as mine, and he was well aware that the phenomena which I was observing in the motor patterns of intact animals were very closely related to those processes which von Holst was investigating experimentally at the neurophysiologic level. Gustav Kramer believed that the congruity between von Holst's research results and mine would be that much more startling and convincing the longer we worked completely independently of one another; that is why he perpetuated this remarkable feat of extended reticence.

So then, in 1935, I gave my lecture at Harnack House in Berlin. Its theme was based on my article, "The Concept of Instinct Then and Now." (1932) There I made it clear that any animal is perfectly capable, through goal oriented and variable behavior, of striving toward a purpose, but that this purpose may not, as the purposive psychologists supposed, be equated with the achievement of the teleonomic function of behavior. The purpose toward which the animal, as subject, is striving, is simply a run-through or discharge of that kind of innate behavior which Wallace Craig designated as "consummatory action" (1918) and which we now call the drive-reducing consummatory act. Up to this point what I said then is more or less what I believe today.

But what I had to say about the physiological nature of fixed action patterns was influenced by doctrinaire bias. Led by MacDougall, the purposive psychologists had continued their battle against the reflex theory of the behaviorists and, quite rightly, had emphasized the *spontaneity* of animal behavior. "The healthy animal is up and doing," MacDougall had written. I was already thoroughly familiar with the writings of Wallace Craig and, through my own research, I was well acquainted with the phenomena of appetitive behavior and of threshold lowering for releasing stimuli—and I should have borne in mind a particular sentence of a letter Craig had sent shortly before, in which he had argued against the reflex concept: "It is obviously nonsense to speak of a re-action to a stimulus not yet received."

At that juncture mere common sense ought to have prompted me to put the following question: Innate motor patterns have, apparently, nothing to do with higher intellectual capacities; they are governed by central nervous processes which occur quite independently of external stimuli and they tend to be repeated rhythmically. Do we know of any other physiological processes which function in a similar way? The obvious answer would have been: Such motor patterns are very well known, particularly those of the vertebrate heart for which stimulus producing organs are anatomically known and the physiology of which has been thoroughly studied.

I lacked the independence of mind and the self-assurance that would have been necessary to ask this question. My valid aversion toward the preternatural and inexplicable factors which the vitalists had summoned to interpret spontaneous behavior was so deep that I lapsed into the opposite error; I assumed that it would be a concession to the vitalistic purposive psychologists if I were to deviate from the conventional mechanistic concept of reflexes, and this concession I did not wish to make. During the course of that lecture I did cover completely, and with especial emphasis, all those characteristics and capacities of fixed action patterns which could *not* be accounted for by means of the chain reaction theory, yet, in my summary at the end, I still concluded that fixed action patterns depended on the linkage of unconditioned reflexes even if the cited phenomena of appetitive behavior, threshold lowering, and vacuum activity (I will return to this on page 127) would require a supplementary hypothesis for clarification.

Sitting next to my wife in the last row of the auditorium was a young man who followed the lecture intently and who, during the exposition on spontaneity, kept muttering, "*Menschenskind!* That's right, that's right!" However, when I came to the concluding remarks described above, he covered his head and groaned, "Idiot." This man was Erich von Holst. After the lecture we were introduced to one another in the Harnack House restaurant and there it took him all of ten minutes to convince me forever that the reflex theory was indeed idiotic.

The moment one assumed that the processes of endogenous production and central nervous coordination of impulses, discovered by von Holst, and not some linkage of reflexes, constitute the physiological bases of behavior patterns, all the phenomena that could not be fitted into the reflex theory, such as threshold lowering and vacuum activities, not only obtained an obvious explanation but became effects to be postulated on the basis of the new theory.

A consequence of this new physiological theory of the fixed motor pattern was the necessity to analyze further that particular behavioral system which Heinroth and I had called the *arteigene Triebhandlung* (literally, species-characteristic drive-action) and which we had regarded as an elementary unit of behavior. Obviously, the mechanism which selectively responded to a certain stimulus situation must be physiologically different from the fixed motor pattern released. As long as the whole system was regarded as a chain of reflexes, there was no reason for conceptually separating, from the rest of the chain, the first link that set it going. But once one had recognized that the movement patterns resulted from impulses endogenously produced and centrally coordinated and that, as long as they were not needed, they had to be held in check by some superordinated factor, the physiological apparatus which triggered their release emerged as a mechanism sui generis. These mechanisms that responded selectively to stimuli, in a certain sense served as "filters" of afference, were clearly fundamentally different from those which produced impulses and from the central coordination that was independent of all afference.

This dismantling of the concept of the *arteigene Triebhandlung* into its component parts signified a substantial step in the development of ethology. The step was taken in Leyden at a congress called together by Prof. van der Klaauw. During discussions that lasted through the nights, Niko Tinbergen and I conceived the concept of the *innate releasing mechanism* (IRM), although it is no longer possible to determine by which one of us it was born. Its further elaboration and refinement, and the exploration of its physiological characteristics, especially its functional limitations, are all due to Niko Tinbergen's experiments.

Concurrent with the conceptualization of the IRM, the concept of the fixed action pattern or instinctive motor pattern was also narrowed and made more precise, and in exactly the way Charlotte Kogon had proposed as early as 1941 in her book, *The Instinctive as a Philosophical Problem*, a book which regrettably remained unknown to me until just recently. Subsequently, and up to the present, the concepts of IRM and of the fixed motor pattern have proved their worth; their utility in the most diverse kinds of flow diagrams make it probable that they are, in fact, functionally if not also physiologically identical mechanisms. For the visualization and presentation of hierarchically organized instincts (Tinbergen, Baerends, Leyhausen) they have been especially useful.

During the years that followed ethology developed quickly, both in the results achieved and in the increasing number of researchers. A large store of data was laboriously assembled; many unique discoveries were made. If one chooses to criticize this period of felicitous research, it can be reproached for one-sidedness, even for a certain failure to think in terms of systems. This was inherent in an orientation that almost completely ignored *learning processes*; above all, the relationships and interrelationships that existed between the newly discovered inborn behavior mechanisms and the various forms of learning were barely touched. My modest contribution, which comprised a formulation of the "instinct-learning intercalation" concept, got no further than formulation; besides, the example on which the conceptualization—correct in itself—was based, was false. (See page 60.)

In 1953 a critical study appeared which had a behaviorist point of view but which did not come from a behaviorist. In "A Critique of Konrad Lorenz's Theory of Instinctive Behavior" Daniel S. Lehrmann dismissed, on principle, the existence of innate movement patterns and, in so doing, supported his argument substantially by using a thesis of D. O. Hebb (1940) who had maintained that innate behavior is defined only through the exclusion of what is learned and, thus, as a concept was "nonvalid," that is, unusable. Drawing on the findings of Z. Y. Kuo (1932), Lehrmann also asserted that one could never know whether or not particular behavior patterns had been learned within the egg or *in utero.* Kuo had already recommended abandoning the conceptual separation of the innate and the acquired. All behavior, in his opinion, consisted of reactions to stimuli and these reflected the interaction between the organism and its environment. The theory of a pre-extant relationship between the organism and the conditions of its environment is no less questionable, for Kuo, than the assumption of innate ideas.

My answer to Lehrmann's critique was short and forceful but, at first, missed the most essential mark. The assertion that the innate in comparative studies of behavior is defined only through the exclusion of learning processes is entirely false: like morphological traits, innate behavior patterns are recognizable through the same systematic distribution of attributes; the concepts of innate and acquired are as well defined as genotype and phenotype. The reply to the theory that the bird within the egg or the mammal embryo within the uterus could there have learned behavior patterns which then "fit" its intended environment was formulated by my wife and with a single phrase: "Indoor ski course." I myself wrote at the time that Lehrmann, in order to get around the concept of innate behavior patterns, was actually postulating the existence of an innate schoolmarm.

My formulation of the concept of the "innate schoolmarm" was clearly intended as a reductio ad absurdum. What neither I nor my critics saw was that in just this teaching mechanism the real problem was lurking. It took me nearly ten years to think through to where, actually, the error of the criticism and the counter-criticism was located. It was so very difficult to find because the error had been committed in exactly the same way by both the extreme behaviorists and by the older ethologists. It was, as a matter of fact, incorrect to formulate the concepts of the innate and the acquired as disjunctive opposites; however, the mutuality and intersection of their conceptual contents were not to be found, as the "instinct opponents" supposed, in everything apparently innate being, really, learned, but the very reverse, in that everything learned must have as its foundation a phylogenetically provided program if, as they actually are, appropriate species-preserving behavior patterns were to be produced. Not only Oskar Heinroth and I, too, but other older ethologists as well, had never given much concentrated thought to those phenomena which we quite summarily identified as learned or as determined through insight and then simply shoved them to the side. We regarded them—if one wishes to describe our research methods somewhat uncharitably—as the ragbag for everything that lay outside our analytical interests.

So it happened that neither one of the older ethologists nor one of the "instinct opponents" posed the pertinent question about how it was possible that, whenever the organism modified its behavior through learning processes, the *right* process was learned, in other words, an adaptive improvement of its behavioral mechanisms was achieved. This omission seemed particularly crass on the part of Z. Y. Kuo (1932) who had so expressly disassociated himself from every predetermined connection between organism and environment but who, at the same time, regarded it as axiomatic that all learning processes induced meaningful species-preserving modifications. As far as my knowledge goes, P. K. Anokhin (1961) was first among the theorists of learning to grasp the conditioned reflex as a *feedback circuit* in which it was not only the stimulus configuration arriving from the outside, but more especially the *return notifica-*