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My family and other animals

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Konrad Lorenz watching well-fed, large *Zanclus canescens,* a species difficult to keep in the middle of Europe.

When, as a scientist, one tries to get insight into the deeper roots of one's own life interest, it is quite instructive to delve into earliest childhood memories. As a very little boy, I loved owls and I was quite determined to *become* an owl. In this choice of profession I was swayed by the consideration that an owl was not put to bed as early as I was, but was allowed to roam freely throughout the night.

I learned to swim very early and when I realized that owls could not swim, they lost my esteem. My yearning for universality drove me to want to become an animal that could fly and swim and sit on trees. A photograph of a Hawaiian goose sitting on a branch induced me to choose a Sandwich goose as my life's ideal. I was not yet quite six years old when I was hit by the impact of Selma Lagerlöf's immortal book *The Journey of Little Nils Holgerson with the Wild Geese*. Consequently I wanted to become the sort of wild goose idealized by the Swedish poet.

Very slowly it dawned upon me that I could not *become* a goose, and from then on I desperately wanted at least to *have* one and when my mother obstructed this, because geese are too damaging in a garden, I settled for a duck. When a farmer in the neighborhood had a mother hen guiding a brood of day-old ducklings, I wheedled my mother into buying one for me. She did, in spite of the remonstrance of my father, who said that it was cruelty to animals to trust a six-year-old with a live duckling. He prognosticated its early demise. He was wrong. This duck lived to a ripe old age rarely attained by domestic ducks.

One interesting detail that, I confess, fills me with a certain pride, is that I chose the only wild-colored duckling in the brood. My future wife, who got another one for herself the next day, had to content herself with one that had a white wing. I don't know why we did not like white ducks, but admiring the wild and despising the domestic fowl seemed natural to me then, as it does now.

Our getting those two ducklings had, for its consequence, the discovery of imprinting. The process of imprinting is, in domestic ducks, not as clearly marked as in the wild mallard; in particular its time limits are not as strictly defined. Although they had already followed the mother hen, our two ducklings became tolerably well imprinted on both of us; mine, however, which I obtained nearly twenty-four hours earlier than my wife got hers, was clearly much more affectionate and much more closely attached to me than my wife's was to her — though she keeps denying this to this day. What we did not notice and what became apparent only many years later was the fact that I had, at that period, become imprinted on ducks. My undying love for ducks is a good illustration of the fundamental irreversability of the imprinting process. My wife is slightly older than I am: perhaps that is the reason she was not similarly affected.

Tribute is due, at this point of my life history, to Resi Führinger, who was my nanny. Possessed of the true peasant woman's deep wisdom, she taught me many things, among others how to keep a duckling well fed and, which is infinitely more important, well warmed. By helping those ducklings to survive, she probably exerted a more profound and lasting influence on my scientific career than any other of my most revered teachers.

After the acquisition of these ducklings, my future wife and I took to "pretending" again: we pretended to be mother ducks. We waded along the banks of suitable backwaters of the Danube, choosing pools with much insect life in them, watching gleefully how our ducklings enjoyed this natural food. They did, indeed, thrive most wonderfully and at least one reason for this was, besides Resi's teachings, that we had come to understand to perfection our duckling's utterances and expressive movements. We responded correctly when our charges gave the distress signal and we usually guessed correctly whether it was hunger, cold, or loneliness that made the bird cry out. We understood the "happy-feeding-note" and preferred puddles in which the birds uttered it, thus indicating that they had found palatable worms or Chironomid larvae in the ooze in which these prey remained invisible to us. When we heard their enchanting "going-to-sleep" trill, common to all Anatidae as well as to gallinaceous birds and they nuzzled us in the way they do when they want to be brooded, we instantly conformed and made a nice warm pocket in our sweaters for them to sleep in. We lived with them a complete duck's life and within a few months we were most thoroughly familiar with the whole repertoire of all the things a duck can do or say. We did not know that this was to be called an "ethogram" many years later. I remember in vivid detail everything we did with the ducklings, and what the ducklings did with us, in that happy summer of 1908. One evidently does not forget scientifically important memories, even if they are acquired seventy-five years ago.

The amount of important knowledge that two intelligent children can, without any special tuition, gain from two domestic ducklings bears witness to the fact that these birds are particularly instructive objects of study. Of course, my appetite for possessing — and knowing — more ducks was whetted. I wanted more ducks, my next aim being American wood ducks *(Aix sponsa),* which A. E. Brehm's famous book said were easy to breed. My parents, to whom I owe an immense debt of gratitude, were singularly permissive toward my duck infatuation. The first pond was built, a second came along and finally a third. My passion for collecting was awakened, if, for the duration of my early childhood, unrequited. However, I could watch at the zoo those ducks that I could not own myself.

Even in that early childhood I lived a sort of double life, my interests being divided between my ducks and my aquaria. My first aquarium was brought into being by a female spotted salamander giving birth to forty-four larvae. Shortly prior to my going to elementary school, my parents brought back, from a Sunday walk in the Vienna woods, a spotted salamander. My father gave it to me with the injunction that we should liberate it again next Sunday at the place where it had been taken from. Reluctantly I agreed to this contract and was amply rewarded for my filial obedience. The salamander gave birth to forty-four larvae, all of which were swimming one fine morning in the small water container of the terrarium.

This event obviously necessitated an aquarium and I had my first lesson about the metabolism of water-breathing animals. I wanted to keep the aquarium well filled up, but Resi Führinger convinced me that the larvae wanted very shallow water because soon after birth they would develop lungs, their external gills would be slowly reduced, and they would become dependent on air respiration. Owing to Resi's "green thumb" in keeping animals, we reared twelve of these forty-four larvae to metamorphosis.

A newly metamorphosed spotted salamander is among the most enchanting animal babies, being an exact miniature of the adult, showing the same bright black and yellow coloration and having very nearly identical body proportions. I remember as if it had been yesterday, how the young salamanders began to breathe air by moving the floor of their mouths, inhaling air, and pumping it into their lungs. At first these movements were irregular but soon they became as rhythmical as they are in most adult amphibia. I fully realized that the young salamanders will drown at that age if they are not able to crawl out of the water. None of mine did.

The salamanders turning into land animals were the cause of my building a beautiful large outdoor terrarium, which was occupied by these salamanders, and, additionally, by newts of different species and small frogs. I had become a collector and amateur of amphibia. I remember successes and failures, for example, the tragic failure in keeping small frogs alive through the winter.

When the last of my twelve salamanders had completed metamorphosis, the aquarium was vacant, so, of course, I began to keep freshwater fish, beginning with common native species. Even at that time I had a vague feeling that all spiny-rayed fish are much "nobler" than minnows. Later I specialized in North-American sunfish (Centrarchidae) and later still on cichlids. Breeding cichlids and observing their parental behavior brought me some insight into the workings of instinctive activities. The most important discovery, however, was made by my friend Bernhard Hellmann. He was trying to breed the very aggressive South-American cichlid *Geophagus brasiliensis*. His male had killed the female and, after having been isolated for some time, incontinently proceeded to kill any new conspecific put into the tank, irrespective of sex. At this point Bernhard definitely had a stroke of genius. He bought a new female but before putting it with the male, confronted the latter with a mirror and let the fish fight his own mirror image to complete exhaustion. After this, he put the female into the tank and the male immediately proceeded to court it. In other words, Bernhard Hellman, at age seventeen, must have had some inkling that what we now call "action specific energy," is "dammed up" during inactivity and is consumed by acting out a specific action pattern.

On finishing high school, I proceeded to study medicine at the Vienna University; it was my father's wish that I should do so. I really wanted to study zoology in general and paleontology in particular. I was obsessed by a burning interest in evolution. Again I very distinctly remember the awakening of this interest. We were sitting under the Caria tree in our garden having tea and a wasp alighted on a slice of bread and honey held in my father's hand. My mother wanted to chase it away but my father prevented her and explained that the wasp would never sting without being held fast. Then he went on to explain to me what an insect was and that it was so called because of the partitions of its body, head, thorax, and abdomen and he, furthermore, called my attention to the metameres of the wasp's abdomen sliding into each other like the parts of a telescope. Some days later I observed an earthworm creeping and saw all the metameres of the worm slide together and again away from each other as a certain part of the worm's body was contracted or extended. I was reminded of the body structure of the wasp and subsequently asked my father whether the earthworm was an insect. My belief in my father's omniscience suffered severely, because he obviously was out of his depth when I asked this question.

A few days later I discovered the picture of *Archaeopteryx* in a book by Wilhelm Bölsche called *Schöpfungstage (The Days of Creation)*. The text stated clearly that *Archaeopteryx,* with teeth in its jaws and the long tail with two feathers to each vertebra, represented a transition from reptile to bird. I immediately realized the explanatory value of evolution and at once generalized that, of course, annelid worms were the ancestors of insects!

Childhood memories are of the character of still pictures and not of movies. I know for sure where, on a walk in the woods near Altenberg, I explained my discovery of evolution to my father. Usually he was not too willing to listen to my prattle — my wife assures me that I was prattling rather excessively at that age. This time, however, my father listened very attentively and began to smile most benevolently. Then, quite suddenly, I realized that he *knew* all about what I was trying to tell him. I remember experiencing a deep resentment against my father for knowing something of such tremendous importance and not deeming it necessary to tell me all about it. I do not know when this conversation with my father took place though I know where it did. It must have been rather early in my life because I was not yet above the infantile play of pretending. In my enthusiasm about paleontology and that Bölsche book, my wife and I were inspired to play at pretending to be Iguanodons. I know for certain that we attached pieces of old garden hose to our backs for tails and walked around solemnly, keeping our hands in front of us and the thumbs extended upwards, as is shown in all pictures of *Iguanodon bernissartensis*. Infantile play persisted until very late in my life; nonetheless I don't think that I can have been much above six years of age when all this happened, because I distinctly remember having that Bölsche book read aloud to me by Resi.

My yearning to become a paleontologist was diverted by my study of medicine. One of the first lectures that I attended was that of the systematic anatomist Professor Ferdinand Hochstetter. He was not only a brilliant comparative anatomist with an extensive knowledge of vertebrate zoology, but, what meant much more, his main interest lay in comparative embryology. He was a dedicated teacher of the comparative method and very intent on convincing the students of its value.

At that time I had reared not only ducks but a number of birds, fish, and amphibia and had studied their ontogenetic development of behavior, and I was also familiar with the behavior patterns of courtship, which, by converging evolution, are so strikingly similar in newts, fish, and birds. I would have had to be considerably more stupid than I actually was, not to realize at once that exactly the same methods of comparison could and should be applied to behavior. After all, Bernhard and I had already done this when we compared the artemia larvae's movement of its antennae with that of cladocera. Obviously, all I knew about waterfowl behavior should be evaluated in the same way.

This is actually the discovery on which all ethology is based. I certainly did not, at the time, realize the importance nor did I do so when, a few years later, I met the man who had long ago made the same discovery: Oskar Heinroth. Not only had he discovered that behavior patterns constituted just as reliable characteristics of species, genera, and higher taxonomic categories, but he had done so by studying the same group, Anatidae! Much later, after Heinroth and I had become close friends, we discovered the real pioneer in this field of comparative behavior study, Charles Otis Whitman who, ten years earlier, had fully recognized exactly the same facts — only with pigeons for a subject.

Neither Heinroth nor Whitman ever fully realized the far-reaching scientific consequences of their discovery. The fact that innate behavior patterns are clearly homologous in many species explodes certain doctrines to which quite a few psychologists adhere even now. Whitman and Heinroth remained unknown to psychologists; Whitman himself was never mentioned in American psychology. When I was studying in Vienna under Karl Bühler, who had many American psychologists for guests, I asked every one whether he or she knew of Charles Otis Whitman. None did. Not so many years ago I happened to meet his son, a very successful businessman. He had no idea of his father's importance. The only thing he knew about him was that he was "crazy about pigeons" and kept many aviaries full of these birds.

Let me put in, at this place, a word about the "amateur" or "dilettante," Amateur is derived from the Latin *amare,* "to love," dilettante from the Italian *dilettarsi,* "to delight in something." It is fashionable in science nowadays to experiment rather than to observe, to quantify rather than to describe. Yet descriptive science, based on plain, unbiased observation is the very fundament of human knowledge. As regards our knowledge of animal behavior, I contend that not even a person endowed with the almost superhuman patience of a yogi could look at animals long enough to perceive the laws underlying their behavior patterns. Only a person who looks with a gaze spellbound by that inexplicable pleasure we amateurs, we dilettanti enjoy, is in a position to discover that, for instance, the gruntwhistle is very much the same in many members of the genus *Anas,* but not in the Garganey-Shoveler group, or that the precopulation display is the same in swans and geese.

It was quite impossible that the homology of motor patterns should be discovered by anybody but "dilettanti." The same is true for the discovery of many other important laws of animal behavior. Karl von Frisch, one of the greatest biologists, made his most stirring discoveries on the honey bee, and he made them not in his famous laboratory but at his home in the old hamlet of Brunnwinkel on the Wolfgangsee, which has been in the possession of the Frisch family for many generations. I contend that this great experimenter would not have made his discovery if he had not, for many years, been taking a delight in bees.

We, the amateurs and dilettanti, can certainly claim a number of great scientists as members of our community and this is particularly true of those delighting in waterfowl. Not that an amateur need necessarily be a scientist; he is under no obligation to be one. However, if you are a true amateur, a true lover of some kind offish, bird, or mammal, you cannot help becoming an expert. Again, the expert need not be a scientist, but the scientist is undoubtedly under an obligation to be an expert. If the expert so often tends to become a scientist this is, in some cases, brought about by the following sequence of events. The expert, who still does not consider himself a scientist, reads and hears what scientists of great renown have said and written about the subject with which he, the expert himself is thoroughly familiar. To his great surprise he finds that these great men have no idea what they are talking about and that they consequently talk nonsense.

This was my disillusioning experience when my teacher Karl Bühler made me read the works of the great vitalistic and purposivistic psychologists on the one hand, and of the great behaviorists on the other. None of them knew the phenomena that I was strenuously attempting to understand. One of the most encouraging facts in the study of animal behavior is that experts always agree on fundamentals. Whether I am talking to the nestor of waterfowl experts, Jean Delacour, or to one of the youngest, of which, luckily, there are several, there is never any discussion of those fundamentals that still are so controversial in psychology and even in ethology. All experts simply know what an innate behavior pattern is, often without ever having been told.

In 1922 Bernhard Hellman gave me as a birthday present Oskar Heinroth's classical work *Die Vögel Mitteleuropas,* which is neither more nor less than a book on the "comparative embryology of bird behavior." I realized that comparing the similarities and dissimilarities of living animals was a much better way to reconstruct their genealogy and to gain insight into the path evolution had taken, than the study of its fossil documents, which were too few and too far between. I suddenly realized that the sheer observation of what birds do, the occupation I had hitherto regarded as a mere hobby of an amateur, was indeed good legitimate science. The aims of my life as a scientist had become very clear.

During the last two years at high school — that is, from 1920 to 1922 — Bernhard Hellman and I got intensely interested in an odd group of small Crustacea, in phylopods. This interest stemmed from our collecting live food for our aquarium fish. An older friend, to whom I owe an undying gratitude, presented me with a small microscope and mere curiosity made me look at all the little creatures that I later fed to my fish. Confronted with the magnificent multitude of forms, I was caught by the devil of collecting. Maybe every zoologist in the making has to repeat the history of his science and pass through a phase of collecting.

The group that caught our fancy was the Cladocera, of which we collected preparations as well as photographs, which we made by taking the lens out of a very old large camera and mounting it directly over the microscope. We knew that

Cladocera belonged to the larger subclass of Phylopoda and we longed to study some representatives of this group.

I vaguely remember that in 1909 the water covering the fields during an inundation of the Danube had been teeming with free-swimming Crustacea much larger than Daphnia and we concluded retrospectively that these must have been Conchostraca and particularly Estheriides. The only Euphylopods that we could get hold of were *Anemia salinat* the eggs of which were obtainable in pet shops. When we observed the early nauplia stage of these creatures, and saw that they were scullying along by movements of their second antennae, we had the bright idea that Cladoscera were derived from Euphylopods by way of neoteny, which accounted for the reduction of their body segments.

We also formed the hypothesis that Cladoscera had developed di-phyletically, the bivalve forms from *Conchostraca,* while *Onychopoda* like *Bythotrephes* and *Polyphemus* had descended from forms similar to *Branchinecta* or *Branchipus*. It was not before 1937 that a severe and long persisting inundation of the Danube caused the meadows near Altenberg to be covered by water that evoked an eruption of thousands and thousands of Euphylopods of seven species, including the great *Triops cancriformis*. Bernhard Hellman had by then emigrated to Holland; this did not save him from being gassed by the Nazis during the war.

Even before I obtained my doctorate I became at first instructor, and later assistant in Hochstetter's department. Also I began to study zoology at the zoological institute of Professor Versluys and to participate in the psychological seminars of Professor Karl Bühler, who took a lively interest in my attempt to apply comparative methods to the study of behavior. It was Bühler who drew my attention to the fact that my findings contradicted, with equal violence, the opinions held by the vitalistic or "instinctivistic" school of purposive psychology on one hand, and those of the mechanistic or behavioristic school, represented by Watson and Yerkes, on the other hand.

Bühler made me discuss at his main seminar the most important books of the purposivistic school, W. McDougal's *An Outline of Psychology* and Edward Chase Tolman's *Purposive Behavior in Animals and Men,* and in a subsequent lecture, a book by Watson. By doing this, Bühler forced me to read these books thoroughly and in doing so I suffered a really shattering disillusion: none of these people really *knew* animals. None was familiar with them as Heinroth was or as even I was at the age of just over twenty years. I felt crushed by the amount of work that was still to be done and that obviously devolved on a new branch of science that, I felt, was more or less my own responsibility.

During the years from about the middle twenties to the middle thirties I seem to have done an amazing number of things at the same time. I again lived a double life, the less reputable side of which consisted in a rather violent passion for motorcycle riding. I rode a large, two-cylinder Brough Superior. Bernhard Hellmann and my wife each rode a 500 cc overhead-valve Triumph, and, together with our friend Willy Reif, we toured all over Europe during our summer vacations, visiting the Bretagne, Switzerland, and Italy. I was a pretty good rider and I accepted an invitation from the British Leyland Company to enter some road racing on a factory-owned machine. That was very good fun, but after I had one crash, fortunately, without injury to myself, my wife forbade further participation; she said that there were more rewarding forms of committing suicide.

Together with my friend Gustav Schmeidl, I bought a whaleboat, originally from the Austrian dreadnought *Viribus Unitis,* which had sunk and stayed on the bottom of the harbor of Pola since 1918. Into this well-preserved hull we built an ancient Mercedes motor of 13,000 cc's and, on this romantic vessel, successfully navigated the Danube. In order to do this I had to pass an examination that would permit me to pilot small steamers because a license to drive motorboats had not been provided for by the Austrian legislature. This was by no means the easiest exam I had to pass during my long life. On our farthest voyage, with my wife's brother and his wife for crew, we took our ship to Budapest and — which took far more time — back again.

All these not-so-commendable activities did not prevent me from being scientifically diligent. I published nine papers, one of which, "A Contribution to the Comparative Sociology of Colonial Nesting Birds," I read at the 8th International Ornithological Congress in Oxford. I kept birds in Altenberg, concentrating on social species, settling a colony of free-flying jackdaws in the roof of our house and a colony of night herons on the high old trees in our garden. At the same time, I did duty as assistant in the Anatomical Institute, I studied zoology and got my doctorate, I married, and I studied psychology at Karl Bühler's institute — at least enough to realize the tremendous importance that Bühler's theories on perception would have for a future epistemology.

I was deeply influenced by Egon Brunswick, at that time assistant to Bühler, who had just published his book *Psychologie vom Gegenstand her,* which may be translated as *Psychology from the Point of View of the Perceived Objects*. What I learned from Bühler and Brunswick regarding the functions of perception was fundamental to the later development of my views on the epistemology of my late colleague Immanuel Kant. (It was one of the virtually incredible acts of fate that wafted me, in the year 1940, to Königsberg to actually occupy the very chair of professor of philosophy held long ago by the greatest philosopher of all times. But I am getting ahead of my story.)

All my life I seem to have been persecuted by the goddess of good luck. I had had a sequence of good teachers beginning with Resi Führinger, proceeding to an excellent biology teacher, P. Philip Heberdey, a benedictine monk who taught us all about Darwin and natural selection, to Ferdinand Hochstetter, who taught me how to reconstruct the path phylogeny had taken by studying similarities and dissimilarities of living creatures, and to, last not least, Karl Bühler and Egon Brunswick, who kindled my interest in epistemology.

I decided that it was incumbent on me to read Immanuel Kant, which, in fact, is some undertaking indeed. With beginner's luck I struck on the *Prolegomena zur Kritik der reinen Vernuft* and had just finished reading it, when the goddess of fortune interfered. Two things happened in quick succession. I had applied for a lectureship in animal psychology at Vienna University and one professor raised objections on the ground that animals don't have a soul, another one raised difficulties about my becoming a lecturer at one faculty, while being assistant at another; psychology belonging to the philosophical and anatomy to the medical faculty. So I decided to leave my position at the anatomical institute, resigning myself to losing my salary and relying on that of my wife, who was at that time already responsible for a department in an obstetrical hospital in Vienna. I owe her great thanks for encouraging me to leave the anatomical institute, which was then under the directorship of another professor. People often asked her how she could stand the many birds and animals, including lemurs, capuchin monkeys, ravens, great crested cockatoos, to name only some of them, running and flying free in and around our house in Altenberg. Her habitual answer was that she did not mind because she hardly ever was at home, being on night duty in the hospital and earning the money necessary to feed the Altenberg menagery.

The climax to these fruitful and definitely formative years came when I gave a lecture on the concept of instinct at the Harnack House in Berlin and was invited to repeat it at a congress, entitled "Instinctuus," convoked by Professor Van der Claauw in Leyden. At both occasions I talked in detail about the fact, discovered before me by C. O. Whitman and O. Heinroth, that there are motor patterns, the similarities and differences of which, from species to species, from genus to genus, even from one larger taxonomic group to another, are retained with exactly the same constancy and/or variability as are morphological characteristics. In other words, these patterns of movement are just as reliably characteristic of a particular group as are the formation of teeth or feathers and such other proven distinguishing attributes used in comparative morphology. For this fact there can be no other explanation than that the similarities and dissimilarities of these coordinated motor patterns are to be traced back to a common origin in some ancestral form that also already possessed, as its very own, these same movements in a primeval form. In short, the concept *of homology* can be applied to them.

Neither Whitman nor Heinroth ever expressed any views concerning the physiological nature of the homologous motor patterns they had discovered. My own knowledge of the physiology of the central nervous system came from textbooks and lectures in which the Sherringtonian reflex theory ruled supreme and was regarded as the last word and the incontestable truth.

In this lecture I refuted the purposivistic opinion held by McDougall and E. C. Tolman that "instinct" as a supernatural factor directed animal behavior to its goal, as well as the behaviorists' doctrine that all animal behavior was formed by environment. I made it perfectly clear that any animal is perfectly capable of striving toward a purpose by goal-oriented and variable behavior, but that this purpose must not, as the purposive psychologists supposed, be equated with the achievement of the teleonomic function of the behavior pattern in question. The purpose toward which the animal, as a subject, is striving is neither more nor less than the run-through or discharge of that kind of innate behavior that Wallace Craig designated as "consummatory action" (1918) and that 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 McDougall, 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," McDougall 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 that occur quite independently of external stimulation. Do we know of any other physiological processes that function in a similar way? The obvious answer would have been: Such motor patterns are very well known, for instance 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 that 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 special emphasis, all those characteristics and capacities of fixed action patterns that could *not* be accounted for by 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 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 assumes that the processes of endogenous production and central nervous coordination of impulses, discovered by Erich von Holst, constitute the physiological basis of behavioral patterns and not some linkage of reflexes, all the phenomena that could not be fitted into the reflex theory, such as threshold lowering and vacuum activities, not only were easily explained, but became effects to be postulated on the basis of the new theory.

One important consequence of this new physiological theory of the fixed motor pattern was the necessity to analyze further that particular behavioral system that Heinroth and I had called the *arteigene Triebhandlung* (literally, "species-characteristic drive action") and that we had regarded as an elementary unit of behavior. Obviously, the mechanism that 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 that triggered their release emerged as a mechanism *sui generis*. These mechanisms that responded selectively to stimuli and in a certain sense served as "filters" of afference, were clearly fundamentally different from those that 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 Professor Van der Claauw. 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 which one of us actually gave birth to it. Its further elaboration and refinement and the exploration of its physiological characteristics, especially its functional limitations, are all due to Niko Tinbergen's experiments.

The following summer, Niko Tinbergen, with his family, came to Altenberg to continue our discussions about instinctive motor patterns and innate releasing mechanisms and to cement what proved to be a lifelong friendship. At this period I was delivering a systematic lecture in Vienna twice weekly and Niko used to accompany me and to listen. When we were not discussing, we were digging a pond in the lowest parts of our garden in Altenberg — I was studying my first greylag geese at that time. One fine day, when the pond was progressing beautifully Tinbergen refused to come to my lecture; he preferred to complete a particularly intriguing part of the water conduit and he knew what I was talking about anyhow. Just that day a rather arrogant and slightly older colleague approached me and said in a somewhat supercilious manner, "1 hear that Tinbergen is working at your station; what is he working on?" When I answered, quite truthfully, that, at the moment, Tinbergen was digging a pond, he thought I was pulling his leg and walked away offended.

Retrospectively, this summer with Niko Tinbergen was the most beautiful of my life. What we did scientifically had the character of play and, as Friedrich Schiller says, "Man is only then completely human when he is at play." Niko and I were the perfect team. I am, as described before, an amateur and prefer observing to experimenting. It comes hard to me to endanger the happy life of a brood of birds or fish in order to perform an experiment on them. Niko Tinbergen is the past master of the unobtrusive experiment, of asking a question of the organism without unnecessarily disturbing it. We published, in joint authorship, a paper that has become a classic.

I missed Niko Tinbergen most dreadfully when he left Altenberg in the autumn of 1937. Work went on, however, and the same situation, my wife earning the money and myself doing research, continued until after the beginning of the last war.

Then, in 1939, I received a call to the second chair of philosophy at the University of Königsberg. This surprising invitation was triggered by the fact that Erich von Holst was playing the viola da braccia in a chamber quartet in which Eduard Baumgarten played the first violin. Baumgarten was then professor of philosophy at Madison University and had just received a call to the first chair of philosophy at the University of Königsberg. Baumgarten, being a direct pupil of John Dewey and a dyed-in-the-wool pragmatist, was somewhat doubtful about settling in the cast shadow of Immanuel Kant. At one of the quartets, he casually asked von Holst, whether he perchance knew a psychologist with some biological, particularly evolutionary, background who might be interested in epistemological problems, particularly in the nature of what Immanuel Kant was calling the "a priori." Von Holst told him that by a rare coincidence he could furnish just such a rare bird, meaning myself. Von Holst and Baumgarten approached the zoologist Otto Koehler and the botanist Kurt Mothes, on whose authority the philosophical faculty of Königsberg invited me to the chair of psychology.

I don't think I am superstitious, but the incredible improbability of the coincidence of all these contriving factors impressed me as something like the "finger of God" and I accepted at once. As a professor of psychology, my biological background caused some controversy among my new colleagues on the several chairs of humanities. Interestingly enough, Professor Héraucourt, Anglicist and comparative linguist, at once claimed brotherhood of method and called attention to the fact that the methodology that I had learned from F. Hochstetter was identical with that used in linguistics in investigating the historical development of language. Other philosophers repudiated everything I said and did and asserted that by my occupying what had been administratively Immanuel Kant's chair, philosophy had not only gone to the dogs, but actually to the fish. This unkind cut referred to the many aquaria that I had installed at the rooms of my new department. I automatically became a member of the Kant Society, which convened every Monday evening, and I was seduced at an early date to air my rather immature views on epistemology.

In my opinion the recognition of new and important facts begins with a subconscious growth, not only in the mind of one single man but simultaneously in that of very many thinkers. Some ideas, already widely spread at a certain time in an unreflected subconscious manner, suddenly erupt in the form of a relevation in the mind of one who consequently considers himself and is considered by others as having wrought a great breakthrough. But, and this is the trouble, when an idea of this kind is mature enough to emerge, it is not only one man to whom it suddenly becomes clear, but very often to several at once, Wallace and Darwin being the classical example.

To any thinker thoroughly familiar with the facts of evolution, it is a matter of course that the organization which enables us to perceive the external world, the sensory organization as well as that of our central nervous system, is something real and has evolved in interaction with and in adaptation to the outer reality that surrounds us. I contend that a vast number of biologically informed scientists are holding this very same epistemological attitude even if they are completely uninterested in epistemology and never have given a thought to the problems of the Kantian *a priori*. Kant's question, how it is to be explained that our aprioristic categories of thought and forms of ideation fit adequately to the external world, receives an easy and even banal answer on an evolutionary basis.

For Immanuel Kant even these categories of thought and forms of visualization are *a priori* in the sense that they are there before any experience and must be there in order to make experience possible at all. According to Kant the *Ding-an-sich* is unknowable on principle. I never could quite understand how this can agree with his statement that our categories of thought and forms of ideation are "adequate" to make experience possible. I believe that my own understanding of this question was furthered by the paradigm of my old inadequate microscope, which adorned all objects seen through it with rainbow-colored edges. This made me realize that objectivity depends on a thorough knowledge of the apparatus through which we perceive the world. Otherwise it is not possible to avoid mistaking for a characteristic inherent to the object observed something that in fact results from the shortcomings of the instrument through which we perceive it.

I never believed that microscopic animals really had rainbow-colored edges but the great poet-philosopher Johann Wolfgang von Goethe committed just such an analogous error in regarding color qualities not as a product of our perceiving apparatus, but as physical properties of light itself. What I had learned from Egon Brunswick regarding the function of color constancy helped me considerably in understanding all this.

At an early age, thanks to the tuition of Karl Bühler, I had thoroughly understood that knowing an object results from an interaction between an organization within the observer and an object in the outer world, both of which are equally real. I sincerely believe that this fundamental truth is the solid base of all striving for objectivity in research. P. W. Bridgeman said as early as 1958, "The object of knowledge and the instrument of knowledge cannot legitimately be separated but must be taken together as a whole." Objectivity cannot be reached, as behaviorists think, by ignoring subjective experience, but by thoroughly studying what Bridgeman calls the instrument of knowledge, what Karl Popper called the perceiving apparatus, and what I have called the *Weltbildapparatur.*

Karl Popper writes with a remarkable casualness in *The Logic of Scientific Discovery,* "The thing-in-itself is unknowable: we can only know its appearances, which are to be understood (as pointed out by Kant) as resulting from the thing-in-itself and from our perceiving apparatus. Thus appearances result from a kind of interaction between the things-in-themselves and ourselves." Popper does not quite seem to realize that Kant himself, and some neo-Kantians even more so, would violently object to these statements. According to Kant's transcendental idealism, there is no correspondence between the *Ding-an-sich* and the way our a-prioristic forms of ideation make it appear in our experience. What we experience is, for Kant, in no way an *image* of reality, not even a crude or distorted image. He saw clearly that our forms of apprehension are determined by preexisting structures in ourselves and not by those of the object apprehended. What he obviously did not see is that these structures of our perceiving apparatus have something to do with reality. In paragraph 11 of the prolegomenon to the *Kritik der Reinen Vernunft,* Kant wrote:

If one were to entertain the slightest doubt that space and time did not relate to the Ding-an-sich, but merely to its relationship to sensuous reality, I cannot see how one can possibly affect to know *a priori* and in advance of any empirical knowledge of things, i.e., before they are set before us, how we shall have to visualize them as we do in the case of space and time.

Kant was obviously convinced that an answer to this question in terms of natural science was impossible in principle. He was right in contending that our forms of ideation and our categories of thought are not, as Hume and other empiricists believed, the product of individual experience. He found clear evidence that they are not individually acquired by learning.

A highly pertinent question is what Kant would have thought of the *a priori,* if he had been familiar with the facts of evolution. I assert that he would, without any objections, have taken the view held by what we call evolutionary epistemology. Karl Popper's statements quoted above put our views in a nutshell and Donald D. Campbell in his essay "Evolutionary Epistemology," has convincingly demonstrated why and how it is necessary for an understanding of our cognitive apparatus to know how it has phylogenetically evolved. It is an approach that has also received the express approval of no less a man than Max Planck, who wrote to me that it gave him "deep satisfaction that, starting from such different premises, I should have arrived at the same view on the relationship between the phenomenal world and the real world as he had done himself. "

I regard evolutionary epistemology as superlatively important to our views of man and his relationship to the rest of creation. But I do not flatter myself to have made very important contributions. The time for the recognition of these epistemological revelations was simply ripe in our day and I do not doubt that besides Max Planck, Karl Popper, Donald Campbell, Rupert Riedl, and myself, many other thinkers have independently arrived at the same results. If I myself was the first to put it into words, it was because in my controversial position in Immanuel Kant's chair, I was exposed to a critique that simply forced me into counterattack.

As my knowledge of Kant's work was, at that time, infinitesimal, this counterattack was an insecure undertaking. I relied, in regard to Kant's teaching, mainly on the utterances and particularly on the letters of my opponents. I owe great gratitude to the physiologist, H. H. Weber and to Annemarie Koehler, the first wife of my teacher and friend Otto Koehler, the zoologist. I often used the argument *hic dicat quispiam* — here the Kantian would say — and quoted literally what Weber or Frau Koehler had said originally. This is how my paper "Kant's Lehre vom Apriori im Lichte moderner Biologie" was written. This daring and even foolhardy attack on transcendental idealism was not published until I had paid off my indebtedness to my teacher Heinroth by publishing an elaborate paper "Comparative Studies of the Behavior of Anatidae." Both papers were published after I had been recruited into the German army.

I was called up as a motorcycle dispatch rider in a motorized unit. My first days as a recruit, usually so disagreeable, were made easy by a piece of stunt riding. The sergeant in charge showed me a 600 cc Norton-licensed NSU with a huge sidecar, and asked me whether I was afraid of that machine. I said no, mounted it, and rode round the court on two wheels, balancing the sidecar very high in the air, which was difficult because it was on the outside of the circle. I let it come down with a thump in front of the sergeant and sat at attention. He at once deputized me as a motorcycle-riding instructor and left the locality. I was exempt from some very tiresome drill. Instructing, whatever it may be, is an amusing job.

It was not long, however, until it was discovered by the military authorities that I had been a professor of psychology. However, after a few weeks as a psychologist in Posen, which consisted mainly in administering routine psychological tests to aspiring officers, I lost my job because Göring abolished the institution of military psychology altogether. He had found out that according to previous psychological testing, Mölders, one of the greatest among fighter pilots, had been declared hopelessly unfit to ever become an airman.

During my occupation as a psychologist I had become acquainted with Dr. Herbert Weigel, who was in charge of the Department of Neurology and Psychiatry at the Reservelazarett 1 in Posen. He wanted me to come to his department, which, however, seemed impossible because Posen belonged to Wehrkreis 1 and I was recruited at the Wehrkreis of Königsberg, the number of which I have forgotten. Weigel applied for my transfer to the psychological department in Posen which, owing to Göring's intervention, did not exist any more. The higher levels responsible for the transfer were, as Weigel had forseen, quite unaware of this fact and I was duly transferred to Wehrkreis 1.

Immediately following my transfer, the medical section claimed me, as it was unlawful for a medical man to do duty of any other sort. I was included in Weigel's department, which again was one of my strokes of luck. Nothing is more humiliating than having to pretend to be expert on a subject of which one knows too little, and I definitely knew too little of medicine to fulfill the duties of a doctor. With the special subjects of neurology and psychology, on the other hand, I felt myself quite able to cope, as I knew enough of anatomy and of the physiology of the central nervous system, and also had some inklings of psychiatry acquired in mixed seminars held in Vienna by Professor Pötzl, the psychiatrist, and Professor Bühler, the psychologist.

Weigel was a good teacher and one of the few German psychiatrists who dared to confess that they took Freudian psychoanalysis seriously. During the two years of my activity in Posen, my chief occupation was the treatment of neuroses, mainly of hysteria and of compulsive neuroses. Much later this schooling proved important to me when I realized the degree to which neuroses have become epidemic and threaten humanity as a whole.

In 1944 I was sent to the front to a hospital in Witebsk, which was enclosed and beleaguered by the Russian army from the day on which I arrived. In the complete dissolution and disintegration in which the siege ended, I quite unwillingly found myself commanding a small group consisting almost exclusively of sergeants who had refused to panic. After waging two days of private war we were all taken prisoner. I was examined by a Russian major who spoke German tolerably well. When he found out that I was a university professor, he went into an elaborate propaganda speech, telling me that in Russia science was taken much too seriously to take university professors away from their posts and send them to the front. "But," he concluded, "*inter arma silent muse,* do you understand?" I did, and he seemed very impressed by it while I, on my side, received an entirely erroneous opinion of the erudition of the Russian army. In fact, I never again encountered a Russian, even among doctors, who knew as much Latin as that major.

My first appointment as a prisoner of war doctor was satisfying insofar as I really was able to save some lives. I was put in charge — of course under the supervision of Russian doctors, mainly women — of a neurological hospital in Chalturin full of more than six hundred patients, all of whom suffered from what by German army doctors was called *Feldpolyneuritis,* which is best translated by "front soldier's neuritis." It consists in an inflammation of the anterior roots, including the ganglia to which they belong, and is caused by the additive effect of cold, overexertion, and lack of vitamin C. Its symptoms are the disappearance of all tendon reflexes and, at later stages, in paralysis of striated musculature. At its worst the illness kills the patient by paralysis of the breathing musculature.

The Russians did not know this illness and the doctors in charge of the hospital all had thought that the patients were suffering from an epidemic diphtheria, which also produces areflexia. The treatment is extremely simple; it consists in keeping the patient quiet and warm and making him swallow great doses of ascorbic acid. The latter was available and so were some additional blankets and by this simple treatment I achieved what the Russians regarded as a miracle cure on all the patients except two, who, in the first days after my arrival died a horrible death by asphyxiation. An iron lung could have saved them, but none was available.

Another interesting story that happened in Chalturin is worth telling. The hospital often received patients emaciated and in the last stages of starvation. They always came from prisoner of war camps that were in an isolated position that made official supervision difficult. We received a young Austrian in this stage who, from pure weakness, had developed gangrene of the toes and the adjoining part of the right foot. I assisted my friend Hans Theiss, who was in charge of the surgical department, in amputating the forefoot in the Lisfrank's articulation. Unluckily, the gangrene crept upwards and a few days later I was asked to assist the Russian surgeon in amputating the leg at the knee joint. The Russian cut merrily into the bend of the knee and, against discipline, I could not help saying, "ostaroshno [look out] arteria poplitea." The Russian surgeon did not take offense but asked "shto takoi [What's that] Arteria poplitea?" — and cut it through in the next second. The artery squirted blood across the room, but only twice. At the next heartbeat the Russian had got the artery in his forceps and a second later had efficiently ligatured it. Obviously, manual dexterity can substitute for anatomical knowledge.

After this operation, the patient, who was a very neurotic personality, went on a hunger strike. The Russian prisoner of war hospitals took the most extreme pains to save their patient's lives. Russian nurses acted as donors for blood transfusions, and the man whose story I am telling, was offered the most incredible choice of food. However, no tidbit could tempt him and I was called in, as the case obviously came within the competence of a psychiatrist. Though the man came from Vienna as I did myself and thus spoke the same dialect, I found myself quite incapable of convincing him that the loss of his leg was only an advantage, as amputated prisoners regularly were sent home at once. He simply had had enough of it all and went into a hysterical state like a small child in a tantrum. He was in extreme danger and in desperation I resorted to the last means of treating hysterical reactions: I simulated extreme rage, roaring like a gorilla at the poor little remnant of a man, actually beating my breast and threatening to make mincemeat (in Austrian *Krenfleisch)* of him, if he did not eat this nice soup at once. Miraculously he did, and I felt tremendously relieved because I had had my doubts about the humaneness of my treatment.

It is characteristic that a person cured, or more accurately, "snapped out," of a hysterical reaction feels extremely grateful to the "snapper outer." From then on the patient would eat greedily, but only under one condition: I had to be present. The small starveling, who had looked about eighty years of age plumped out with amazing speed and turned out to be a quite good-looking young man in his early twenties. I had to look after my department consisting of over six hundred beds and I became tired of walking over, breakfast, lunch, and dinner, to the surgical department for his feeding. So at last I lost patience and told him that, unless he would eat his meals even in my absence, I would administer the beating I had threatened a few weeks ago. I concluded, "Do you think that I have the time to sit on your bed and feed you spoon by spoon?"

Weeks later I met the man again in Orithchi, where I was interned on a "vacation" after the hospital in Chalturin had been dissolved. He was due to be repatriated the next day and he offered with real heroism to take a message to my wife. He carried, in his cheek, the first message telling my wife that I was still alive — I had simply been reporting missing. The point of this story is the tale this man told to my wife. He said that he was lying in a corner uncared for, on the point of starvation and ready to die. Then he said that I had found him and had sat on his couch and saved him by feeding him spoon by spoon. When my wife told me the story later I did not, of course, remember at once who the man was until the story of my feeding him "spoon by spoon" rang a bell. The interesting thing is that the man knew, at heart, that I had saved his life but, for obvious psychological reasons, had displaced the disagreeable method by which I had done so.

Altogether I went through thirteen Russian prisoner of war camps: the Russians shifted us about quite a lot. I was always working as a camp doctor. I strongly dislike the term *psychosomatic,* because there is hardly an illness that does not affect mind and body alike. Therefore, the keeping up of morale in a prisoner of war camp is at least as important as medical supervision and so I acted as a mixture of medical man, father confessor, and buffoon, the latter activity not being the least important.

I had some time on my hands, particularly when staying in smaller camps, so I started to write a book. I could get some ink but no paper. However, by bribing the camp's tailor with a few pieces of bread, I got him to iron out cement sacking, which I cut into suitable pieces as paper to write upon. It was quite a package when I had finished what ultimately became my book on evolutionary epistemology entitled *Behind the Mirror.*

Some of my prisoner friends thought this writing extremely dangerous and took a very glum view when, from the camp in Erewan, which sat at the foot of Mount Ararat, I, quite alone and convoyed only by one officer, was very suddenly transferred to Krasnogorsk in the precincts of Moscow. On the railway, my guard got a severe attack of malaria. When we arrived in the oil town of Baku he was quite unable to leave the railway car, so he gave me money and I had to walk quite illegitimately in a German uniform through the Russian town with nothing to prove that I was not an escapee. After having bought what I had been told by the convoy officer, I found the opportunity to wash myself at a fountain in the main square of Baku. While I did so, I suddenly saw approaching the intimidating figure of a one-legged Russian soldier with Mongolian features, waving in his hands a huge razor. I was reassured when he said in very bad and therefore for me very understandable Russian: "You have soap, I have razor, you give me some soap, I shall shave you." It is a very consoling memory to visualize a German soldier being shaved by a Russian invalid in the middle of a Russian city.

On my arrival in Krasnogorsk, my presence was made known to the *natschlanik lagera,* the commandant of the prisoner of war camp, by the spreading of the alarming news that one of the prisoners had gone crazy and went around catching flies and putting them into match boxes. The commander, being a highly intelligent man, at once realized that the professor had arrived. He had the amazing kindness to arrange that bird food be provided for me. In Armenia I had reared a starling that I could let fly free for most of the time, as it was securely imprinted on myself. Once it had flown away with a huge swarm of other starlings but had come back to me on my whistling when, later in the day, the swarm had happened to pass above the camp. This earned me the fame of being a magician. It was this fame that had preceded me to Krasnogorsk and the Russian lieutenant-colonel in charge of the camp had correctly associated it with the news of a madman catching flies.

In the camp of Kransnogorsk, I was asked to procure a typewritten copy of my manuscript and was promised that, after it had passed the official censor, I should be allowed to take a copy home with me. The date for the next repatriation transport for Austrians was approaching and my manuscript had not yet come back. The gloomy prognostications of my friends in Erewan threatened to come true.

One day before the transport was to start, I was suddenly summoned to the commander. This in itself was highly alarming for a prisoner of war in Russia but what followed was the most astonishing and, in fact, the most beautiful experience I had during the war.

When I entered the *natchalnik's* office, he amazingly rose from his seat and bade me to sit down. Then, with a very serious face he said, "Professor, you are not a prisoner any more, nor am I your superior officer. Now, from man to man, I want to ask you a question: Can you give me your word of honor that your original manuscript which you have kept, contains nothing which is not identical with what you wrote in the copy submitted to censure?"

I did not understand in the least what he had in mind and I answered at once: "No, I have eliminated one chapter, enlarged another and generally improved on the style of the whole thing."

He laughed outright and said, "No, professor, you don't get my meaning. I am asking you whether your manuscript contains nothing besides your scientific work, any secret notes you took in some camp or other which are not contained in the manuscript submitted to censure."

It was then my turn to laugh and I answered that regarding this question I could indeed give him my word of honor *[chestnyi slowo]*.

Thereupon he wrote a *propusk* that I was allowed to take with me, on the repatriation convoy starting the next day, "one manuscript, one bird cage and one wooden sculpture," the latter being a little wigeon duck that I had carved of hornbeam wood on my wife's birthday. Furthermore, he instructed the convoy officer that I was not to be searched, and that he should pass on this word to the convoy officer succeeding him and the latter should tell it to the next one, and so on. Of course, this exceeded by far the competence of a camp commander and he would have been in very serious trouble indeed if I should have been searched after all and should have been found in possession of any politically relevant notes. I do not think that I know of another example of one man utterly trusting another's word of honor. I am deeply moved whenever I tell the story and I find my eyes moist now, while writing it.

I came home in February 1949.[1] My father had died that year at the age of ninety-one; the rest of my family was in good health. My wife had turned part of the tree nursery that she had inherited into a farm and herself from a gynecologist into a most successful farmer. So we had enough to eat but absolutely no money.

At the right time we received unexpected help from the English writer J. B. Priestley, who donated to the Austrian Academy of Sciences a considerable sum due to him for plays enacted in Austrian theaters, with instructions that it be used to support research in Altenberg. The first coworker to arrive at the new Station for Comparative Ethology under the Protectorate of the Austrian Academy of Science was Wolfgang Schleidt, now professor at the University of Maryland. Next followed Heinz Prechtl, now professor in Groningen, with his wife Use, a doctor of zoology in her own right, and, last, Irenaus Eibl-Eibesfeldt, who is now in charge of a department for Human Ethology of the Max Planck Society. The high quality of my early coworkers caused my wife to exclaim, many years later: "Funny, now all the boys are professors!" Of which I am admittedly proud.

The first non-Austrian ethologist who came to visit our station was William H. Thorpe of Cambridge, who had taken the immense trouble of getting a permit to visit us in the Russian-occupied zone. He remained a friend for life.

In 1950 I participated at a symposium of the Society for Experimental Biology in Cambridge and there I met Niko Tinbergen again. Though he had spent years in a German concentration camp and I even longer in a Soviet prisoner of war camp, we found that this had made no difference whatsoever, which Niko put in a nutshell by saying: "We have won."

We were living quite happily and very modestly at our station in Altenberg when the University of Graz proposed me unanimously to the chair of zoology as a successor to Karl von Frisch; Frisch was returning to his former chair in Munich. My friends in England, Thorpe and Tinbergen, had predicted that I would never get a chair in Austria, and indeed, I never did. The then minister of education refused to confirm my nomination, not because I had previously been at

[1] Lorenz came back February 1948. His father had died 1946 at the age of 92. [klha.at]

a Nazi university, but because, and this was explicitly stated, as a Darwinist and Evolutionist I was unwelcome.

I wrote this bad news to Niko Tinbergen and Bill Thorpe and they obviously worked a miracle because only a few weeks later I received a call for a lectureship at the University of Bristol. As an injunction it was added that I should work as an ethologist on the great collection of waterfowl at Slimbridge. This indicated that my friend Peter Scott also had a hand in that miracle.

I had already consented to accept the lectureship in Bristol when the Max Planck Society intervened. Erich von Holst had spoken to the president, Otto Hahn, and this great man acted at once. Unhesitatingly he exceeded his competence by asking me whether I would consent to stay in Altenberg, if the Max Planck Society were to pay me a salary of 1000 Austrian shillings monthly. Breaking my committment to Bristol University I at once accepted Otto Hahn's proposal.

However, these plans were soon superceded by other, more extensive ones. Late in 1950 the Max Planck Gesellschaft decided to install a station for ethological research at the castle of Buldern in Westphalia. In this location Baron Gisbert von Romberg had offered accomodation for scientists as well as some beautiful ponds for our waterfowl. The tremendous advantage of this offer was that I could give jobs to my coworkers from Altenberg; Eibl, Prechtl, and Schleidt became my assistants.

We worked in Buldern happily and satisfactorily; I myself was nominated professor at Münster University. My station was nominally under the jurisdiction of the Max-Planck-Institut für Meeresbiologie in Wilhelmshaven, at which Erich von Holst was working. In 1955 the Max Planck Society founded, for the two of us, the Max-Planck-Institut für Verhaltensphysiologie in Seewiesen. There followed a few — all too few — years of very fertile collaboration ended by Erich von Holst's tragically early demise. I myself continued working at the Max-Planck-Institut until my retirement in 1973.

During these years, ethology developed apace both in regard to results achieved and the number of research workers collaborating. 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.

In 1953 a critical study that had a behaviorist point of view but that did not come from a behaviorist appeared. In "A Critique of Konrad Lorenz's Theory of Instinctive Behavior," Daniel S. Lehrman dismissed, in principle, the existence of innate movement patterns and, in so doing, supported his argument substantially by using a thesis of D. O. Hebb, 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), Lehrman 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 an organism and its environment. The theory of a pre-existent 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 Lehrman'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 that then "fit" its intended environment was formulated by my wife with a single phrase: "Indoor ski course." I myself wrote at the time that Lehrman, 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 fact, everything learned must have as its foundation a phylogenetically provided program if, as they actually are, appropriate species-preserving behavior patterns are to be produced.

Not only Oskar Heinroth and I, too, but other older ethologists as well, had never given much concentrated thought to these phenomena that we quite summarily identified as learned or as determined through insight and then simply shoved 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 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 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 notification* reporting on the completion and the consequences of the conditioned behavior that provided an audit of its adaptiveness.

As in many other cases of erroneous reasoning, the behaviorists' absence of questions about the adaptive value of learned behavior may be traced to their emphatic antagonism to the school of purposive psychology. The latter's uninhibited commitment to behavior's extranatural purpose created in the behaviorists such antipathy to all concepts of purpose that, along with purposive teleology, they also resolutely refused to consider any species-preserving purposefulness, including teleonomy as defined by C. Pittendrigh (1958). This attitude, unfortunately, made them blind to all those things that could be understood only through a comprehension of evolutionary processes.

The "innate schoolmarm," which tells the organism whether its behavior is useful for or detrimental to species continuation, and, in the first instance reinforces and in the second extinguishes that behavior, must be located in a feedback apparatus that reports success or failure to the mechanisms of the first phases of antecedent behavior. This realization came to me only slowly and independently of P. K. Anokhin.

I published my theories on this subject in 1961 in my monograph "Phylogenetische Anpassung und adaptive Modifikation des Verhaltens," which I later extended and enlarged for a book in English, *Evolution and Modification of Behavior.* As I emphasized in that work, whenever a modification of an organ, or of a behavior pattern, proves to be adaptive to a particular environmental circumstance, this also proves incontrovertibly that *information about this circumstance* must have been "fed into" the organism. There are only two ways this can happen. The first is in the course of phylogenesis, through mutation and/or new combinations of genetic factors and through natural selection. The second is through individual acquisition of information by the organism in the course of its ontogeny.

Innate and *learned* are not each defined through an exclusion of the other but through the method of *entrance taken by the pertinent information* that is a prerequisite for every adaptive change.

The bipartition, the "dichotomy" of behavior into the innate and the learned is misleading in two ways, but not in the sense maintained in the behaviorist argument. Neither through observation nor through experimentation has it been found to be even in the least probable, still less a logical necessity, that every phylogenetically programmed behavior mechanism must be adaptively modifiable through learning. Quite the contrary, it is as much a fact of experience as it is logical to postulate that certain behavior elements, and exactly those that serve as the built-in "schoolmarm" and conduct the learning processes along the correct route, are *never* modifiable through learning.

But, on the other hand, every "learned behavior" does contain phylogenetically acquired information to the extent that the basis of the teaching function of every "schoolmarm" is a physiological apparatus that evolved under the pressure of selection. Whoever denies this must assume a prestabilized harmony between the environment and the organism to explain the fact that learning — apart from some instructive failures — always reinforces teleonomic behavior and extinguishes unsuitable behavior. Whoever makes himself blind to the facts of evolution arrives inevitably at this assumption of a prestabilized harmony, as have the cited behaviorists and that great vitalist, Jakob von Uexküll.

The search for the source of information that underlies both innate and acquired adaptation has, since those earlier years, yielded significant results. I will mention only the research done by Jürgen Nicolai with whydah birds (Viduinae) in which the information can be "coded" in such an intricate way: essential parts of the adult bird's song have been learned by monitoring the begging tones and other tonal expressions of whichever species of host bird by which the whydah happened to be hatched and reared.

Inquiry into the phylogenetic programming of the acquisition processes has proved to be important in many respects. Like imprinting, some acquisition processes are impressionable only during specific sensitive periods of ontogeny; a failure to perceive and meet their needs during those crucial periods in animals and humans can result in irremediable damage. Within cultural contexts the distinction between innate and the acquired is also significant. Man, too, and his behavior are not unlimitedly modifiable through learning and, thus, many inborn programs constitute human rights.

As early as 1916, Oskar Heinroth wrote in the conclusion of his classic paper on waterfowl:

> I have, in this paper, drawn attention to the behaviour used in social intercourse and this, especially in birds living in social communities, turns out to be quite amazingly similar to that of human beings, particularly in species in which the family — father, mother, and children — remain together living in a close union as long as, for instance, geese do. The taxon of Suropsidae has here evolved emotions, habits and motivations very similar to those which we are wont to regard, in ourselves, as morally commendable as well as controlled by reason. The study of the ethology of higher animals (still a regrettably neglected field) will force us more and more to acknowledge that our behaviour towards our families and towards strangers, in our courtship and the like, represents purely innate and much more primitive processes than we commonly tend to assume.

This early admonishment notwithstanding, ethology was curiously tardy in approaching humanity as a subject.

In the investigation of humans it is not easy to fulfill the primary task of ethology, which is the analytical distinction of fixed motor patterns. No less a man than Charles Darwin in his monograph *The Expressions of Emotion in Man and Animals* (1872), pointed out the homology of some human and animal motor patterns. The homology was convincing, but solid proof remained necessary.

Irenaus Eibl-Eibesfeldt was the first to afford this proof. He chose the same movements that Darwin had studied — those expressing emotions. For obvious reasons, the experiments involving social isolation that are generally used to prove a motor pattern to be independent of learning could not be used with humans, so Eibl fell back on the study of those unfortunates with whom an illness had already initiated this experiment in an equally cruel and effective manner: he studied children born deaf and blind. As he was able to demonstrate by means of film analyses, these children possessed a practically unchanged repertoire of facial expressions although, living in permanent and absolute darkness and silence, they had never seen or heard these expressed by any other human.

As a second route to approach, Eibl-Eibesfeldt used the cross-cultural method to study the expressions of emotions in humans. He observed and filmed representatives of as many cultures as he could, in standardized situations such as greeting or taking leave, quarreling, experiencing grief and enjoyment, courting, and so on. The essential patterns of expressing emotions proved to be identical in all the cultures he was able to study, even when the patterns were subjected to minute analysis by means of slow motion films. What varied was only the control exerted by tradition: this affected a purely quantitative differentiation of expression.

The most important result of Eibl-Eibesfeldfs extensive and patient research can be stated in a single sentence. The motor patterns shown undiminished by deaf-and-blind children are identical to those that, through cross-cultural investigation, have been shown to be inaccessible to cultural change. In view of these incontrovertible results, it is a true scientific scandal that many authors still maintain that all human expression is culturally determined.

A strong support for human ethology has come from the unexpected area of linguistic studies; Noam Chomsky and his school have demonstrated that the structure of logical thought — which is identical to that of syntactic language — is anchored in a genetic program. The child does not learn to talk; the child learns only the vocabulary of the particular language of the cultural tradition into which it happens to be born.

A surprising and important extension of ethological research was the application of the comparative method to the phenomena of human culture. In his 1970 book *Kultur und Verhaltensforschung,* Otto Koenig demonstrated that historically induced, traditional similarities on the one hand, and, on the other hand, resemblances caused by parallel adaptation — in other words, the reciprocal action between homology and analogy — are interacting in the development of human cultures in much the same manner as in the evolution of species. For an understanding of cultural history, the analysis of homology and analogy is obviously of the greatest importance.

Rather late in life this interest in human culture awakened my *medical* interest in my own species. New ideas arose from the association between the results of Eibl-Eibesfeldt and Otto Koenig and all that I had learned during my two years of activity as a psychiatrist. I do not think that without this schooling, as unwillingly as I had received it, I would have realized *how crazy* the collective behavior of humanity had become in our time. When, about twenty years ago, I heard a lecture by William Vogt, who was one of the first who had seen the immense dangers approaching, I was not impressed at all — as I must shamefacedly confess. It was Rachel Carson, whom I knew personally and respected highly as a marine biologist, who actually recruited me into the army of conservationists.

While still busily studying greylag geese at the Max-Planck-Institut für Verhaltensphysiologie in Seewiesen, I was writing the book *Civilized Mans Eight Deadly Sins,* which I dedicated to Eduard Baumgarten on the occasion about three years earlier, of his seventieth birthday. The book on greylags already exists in the form of an immense accumulation of data, but I am only just writing it.

Almost simultaneously with my retirement from the Max Planck Institut I received the Nobel Prize, sharing it with Karl von Frisch and Niko Tinbergen. Von Frisch ought to have got it much earlier, but the fact that Niko and I got it together is a matter of deep satisfaction. If ever two research workers depended on each other and helped each other, it is the two of us. I am a good observer, but a miserable experimenter and Niko Tinbergen is, as I have already said, the past master of putting very simple questions to nature, forcing her to give equally simple and unambiguous answers.

When I heard the news of the prize by telephone, my first thought was an objectionable one: That's one in the eye of behaviorism." The one excuse for this is that I was thinking not of myself but of the gain in respectability achieved by ethology as a science. My second thought, more commendable, was of my father, that is, what a pity it was that he did not live to hear this news. I can almost hear what he would have said: "It is incredible! That boy gets the Nobel Prize for fooling around with birds and fish." My father had been nominated for the Nobel Prize several times and had failed to get it by a very narrow margin. It is remarkable how strong the wish for a father's approval can still be in a man seventy years old.

An autobiography ought to end with the retirement of its author. I am afraid mine does not. In 1973 I felt that my work on the greylag goose, so far from being completed, left a surprising number of loose ends. The Max Planck Gesellschaft very generously agreed to finance further research, provided that I could find a place in Austria where I could continue it. By the mediation of my friend Otto Koenig, of the Austrian Academy of Sciences, of the Ministry of Science, and of the Duke of Cumberland it became possible to continue the work on greylag geese.

The Cumberland Foundation offered a gloriously suitable location in the Almtal in Oberösterreich and K. Hüthmayer, the head of the Foundation, worked miracles in the speedy construction of aviaries, ponds, and accommodation for scientists. In the year 1973 one hundred and forty-four greylag geese were moved to the Almtal. This uprooting and replanting of a colony of wild birds was a highly interesting experiment in itself. Among other interesting things, it turned out that the strongest factor keeping a free-flying wild goose from leaving the new location was its personal bonding to the human foster parent who had reared it, even if this had been two years ago.

The station in Grünau, first founded as a department of Otto Koenigs Institute for Ethology, has lately been turned into a separate institution by the Austrian Academy. The new Research Station for Ethology, Konrad Lorenz Institute of the Austrian Academy of Sciences, comprises at the moment research stations at three independent localities. The youngest one of these, the Biologische Arbeitsgemeinschaft Steiermark in Bruck/Mur in Styria, is occupied with research on the ethology and ecology of owls and raptors, with the aim of saving endangered species by breeding them in captivity.

The station in Grünau/Almtal continues its research on the ecology and sociology of greylag geese. The population of greylags in Grünau can claim to have been studied intensely and continuously for more than twenty years. There are, in the world, only two other populations of free-living undomesticated animals that can compete with this, the colony *of Macaca fuscata* in Japan, and the chimpanzees in the Gombe River Station studied by Jane Goodall and her coworkers.

The station in Altenberg consists mainly of an aquarium in which aggressive behavior of fish is being studied. Our most surprising result is that personal aquaintance constitutes the strongest factor inhibiting aggressivity. In *Zanclus canescens,* the formation of nonanonymous collective territory was proven and the same social structure was also found in a not too closely related species, *Zebrasoma veliferum*.

Concurrently a Konrad-Lorenz-Gesellschaft was founded, headed by my pupils Professor Antal Festetics and Professor I. Eibl-Eibesfeldt. These two men are a very comforting guarantee that my kind of work shall be continued even when I shall not be able to do so any more. So there is some hope that the research groups just mentioned will continue their work beyond the time at which I finally retire. Before I do so, I shall certainly have published the book at which I am presently at work, "Der Abbau des Menschlichen, subtitled "und was man dagegen tun könnte (The Waning of Humaneness and what we should do about it)."

After having finished this book, which will be soon, I intend to "evaluate" the immense amount of data on the social life of greylag geese that we have collected through the years. I intensely hope that I shall be able to complete that book too. In case I should still be able to work after having done so, I intend to collect all I know about perch-like fish (Percomorpha) in a rather "pre-scientific" description, in which I plan to give a title culled from Heinroth's famous paper "Beiträge zur Biologie, insbesondere Psychologie und Ethologie der Anatiden," substituting fish for waterfowl.

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