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Dilemmas in the Constitution of and Exportation of Ethological Facts

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Dilemmas in the Constitution of and Exportation of Ethological Facts[1]

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Abstract

Early etho about behaviour? How reliably
viour be exhibited (and in how many specimens) before
aid to be species-typical? And how similar do the
of two species need to be before it is reasonable to say
aviour is true of both? They sought to convince others of
for interspecific behavioural commonalities through a
nee species boundaries problem of establishing that a fact controversial. For if the
-behaviour is also a fact about duck-behaviour was of
est, it was of considerably more significance when one of
es was human. With the publication of works such as
n Aggression and E. O. Wilson's *Sociobiology*, what had
ginal issue for zoology was now of considerable political
and the original claims for inter-specific behavioural
ell under renewed and intense scrutiny – leading to the
on of the original facts on which ethology was

fledgling ethologist Robert Hinde observed a happy
en Konrad Lorenz and Niko Tinbergen, the founders of
rst days together after the Second World War. The
oridge, England. The occasion for the ethologists being
a special symposium on "Physiological Mechanisms in

[1] Sections of this paper are based on my book,

Animal Behaviour," hosted by the Society for Experimental Biology. The interchange in question happened outside of the official proceedings. As Hinde recalled:

> We were walking down Jesus Lane in Cambridge, and Tinbergen and Lorenz were discussing how often you had to see an animal do something before you could say that the species did it. Konrad said he had never made such a claim unless he had seen the behaviour at least five times. Niko laughed and clapped him on

this was the defining feature of his whole enterprise. Indeed, instead of the word “ethology,” he preferred to call his field “comparative behavior study” (*Vergleichende Verhaltensforschung).*

That said, we also need to consider how the ethologists positioned themselves with respecwma6s1r4(d)9.2(D-4.1cip d)9.linthes6s1Prioher451if th(wa)9(r,if w

Probably everyone in Lorenz's audience recognized this as hyperbole. If not, they should have. Whitman was indeed a lover of pigeons, but he was also thoroughly engaged with the broadest questions of biology. Issues of evolution, heredity, and development constituted the *raison d'être* of Whitman's pigeon studies. The portrait of a happy empiricist does not suit him in the least. Heinroth, on the other hand, fits the picture better. He and his wife Magdalena, in their classic study on the birds of central Europe, operated on the assumption that what was innate and what was learned in different bird species could only be determined by means of experiments conducted on a species-by-species basis. Their painstaking multi-year project involved rearing individuals of every different central European bird species by hand, from the egg, and watching how each bird behaved from the time it hatched all the way to its adulthood.[8] Even Heinroth, though, was capable of looking up from his facts to see a broader vision. In 1910 he expressed what might be called the "sooner or later" motif of animal behavior studies, that is to say, the belief that such studies would ultimately have something of value to offer for understanding human behavior. At the international ornithological congress of 1910 he closed his paper on the ethology of ducks and geese with the prediction: "The study of the ethology of the higher animals—unfortunately a still very untilled field—will bring us ever closer to the realization that in our conduct with family and strangers, in courtship and the like, it is more a matter of purely inborn, more primitive processes than we commonly believe."[9]

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[8] Heinroth, Oskar and Magdalena Heinroth, *Die Vögel Mitteleuropas in allen Lebens- und Entwicklungsstufen photographisch aufgenommen und in ihrem Seelenleben bei der Aufzucht vom Ei ab beobachtet*, 4 vols. (Berlin: H. Bermühler, 1924-1934).

[9] Oskar Heinroth, Beiträge zur Biologie: namentlich Ethologie und Psychologie der Anatiden," in *Verhandlungen des 5. Internationalen Ornithologen-Kongresses in Berlin, 30 Mai bis 4. Juni 1910*, ed. Herman Schalow, pp. 589-702 (Berlin: Deutsche Ornithologische Gesellschaft), p. 702. All translations from the German

Lorenz would embrace this goal a generation later. In 1931, not long after becoming acquainted with Heinroth and Heinroth's work, Lorenz wrote ecstatically to the older man saying: "Who knows what will become of today's human psychology if one can only know what is instinctive behavior and what is rational behavior in humans? Who knows how human morals with their drives and inhibitions would look if one could analyze them like the social drives and inhibitions of a jackdaw."[10] From the 1930s onward, Lorenz was keen to proclaim that the study of animal social instincts would shed light on human social instincts. This appears indeed to have been one of the reasons he welcomed the German takeover of Austria in the spring of 1938. In the years immediately preceding that, he had begun to believe that his career as a scientist in Austria was being thwarted by the Catholic educational establishment, which wanted no part of his ideas about the animal roots of human behavior. He imagined that the Third Reich would provide a more receptive *Weltanschauung* for his ideas.[11]

We will come back to the topic of extrapolating from animal behavior to human behavior. For now, let us shift attention to the relations between early ethology and American comparative psychology, with special attention to facts crossing borders.

In 1899, Charles Otis Whitman threw a gauntlet down to modern animal psychologists. In a paper entitled "Myths in animal psychology," he skewered a handful of writers who had misinterpreted various facts of animal behavior. One of the authors was the Englishman George John Romanes. Romanes had repeated an account provided to him by an English lady, who had described the way a male pigeon performed courtship displays to a ginger beer bottle whenever the bottle was put in

[10] Heinroth, Oskar and Konrad Lorenz, *Wozu aber hat das Vieh diesen Schnabel? Briefe aus der frühen Verhaltensforschung, 1930-1940*, edited by Otto Koenig (Munich: Piper, 1988), p. 42.
[11] See Burkhardt, *Patterns of Behavior*, chapter 5.

the bird's vicinity. Romanes offered the bird's behavior as an instance of avian insanity. Whitman, who knew pigeon behavior better than anyone else, offered Romanes's analysis instead as an example of how far one could go astray if one had not first gained a thorough knowledge of the normal behavior of the species in question. In Whitman's words: "The qualification absolutely indispensable to reliable diagnosis of an animal's conduct is an intimate acquaintance with the creature's normal life, its habits and instincts. Little can be expected in this most important field of comparative psychology until investigators realize that such qualification is not furnished by parlor psychology." What was required, he continued, was nothing less than years of close study…." Later in his paper Whitman complained again of "students ambitious to reach the heights of comparative psychology through a few hours of parlor diversion with caged animals, or by a few experiments on domestic animals."[12]

These themes would be repeated half a century later. Ethologists would insist that the first thing a student of animal behavior needed to do was to learn the full behavioral repertoires of the particular species in which he or she was interested. Ethologists would furthermore complain about the psychologists' use of a limited number of highly domesticated animal races, especially the white rat. By the 1940s and 1950s, however, they could no longer claim that the psychologists had spent just a few hours in their studies. American comparative psychology had by this time put hundreds of researchers to work for their entire careers doing experiments on learning in the white rat.

We will return to the comparative psychologists, but first let us consider an interesting experiment conducted by Tinbergen and Lorenz in the spring of 1937 when Tinbergen spent three months at Lorenz's home in Altenberg, Austria. The two zoologists never wrote up the experiment

[12] C. O. Whitman, "Myths in animal psychology," *Monist* 9 (1899), 524-537.

fully, but Lorenz described it briefly in a paper of 1939, and Tinbergen did the same, with illustrations, in a paper of 1948 and then again in his book, *The Study of Instinct*, in 1951.[13]

Figure 1: Tinbergen's illustration of some of the different shapes he and Lorenz used for dummies when testing the innate fear responses of juvenile birds. From Tinbergen (1948).

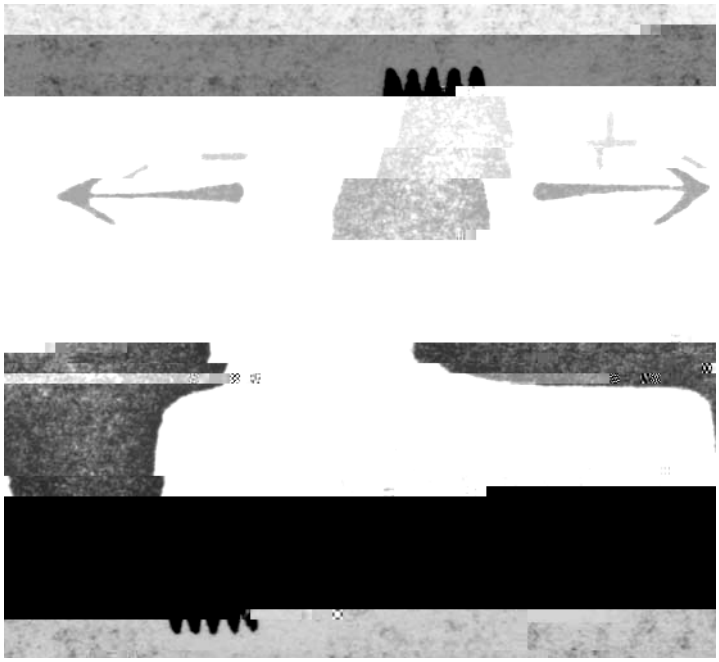

Figure 2: Tinbergen's illustration of "a card board dummy that elicits escape reactions [in young turkeys] when sailed to the right ('hawk') but is ineffective when sailed to the left ('goose'). From Tinbergen (1948).

The two naturalists concluded that the difference corresponded to the basic shapes of avian predators versus avian nonpredators. Predators like hawks have short necks relative to the rest of their bodies. Nonpredators like geese have long necks relative to the rest of their bodies. Tinbergen and Lorenz concluded that the young turkeys' response to the gestalt of the slowly moving, short-end-forward shape was an innate

response, forged by natural selection, to an environmental cue signaling “predator.”

Here, certainly, was an experiment involving specific facts. Here too was an explanation to go with them

to refute this by saying that his own studies on white, laboratory rats showed the hamster results to be untenable.[15]

The ethologists' response was entirely valid. It does not negate it to note that in 1961, Wolfgang Schleidt, one of Lorenz's students, redid the Tinbergen-Lorenz turkey experiment and found that young turkeys do not respond to specific shapes but rather to the speed at which the shapes moved. [16] Nor is the ethologists' response negated by the fact that Lorenz himself often leapt from one species to another in ways that left other scientists uncomfortable.

that wild birds do possess an IRM [Innate Releasing Mechanism] which enables them to respond to birds of prey on the first occasion that they see them. This IRM probably has different properties in different species but short neck and relative speed of movement are among them."[18] Less nuanced was the response of the distinguished ornithologist and secretary of the Smithsonian Institution, S. Dillon Ripley, an admirer of the work of Lorenz & Tinbergen. Ripley became a major proponent of the idea that the silhouette of a raptor could deter songbirds from flying into windows. Under his direction the Smithsonian Museum shop began marketing raptor stickers to put on windowpanes. Such stickers continue to be used today, without any evidence that they actually work.[19]

Let us return now to the fact of significant interspecific differences in behavior. It would be worth attempting to track how this fact made its way into American comparative psychology. That this appreciation was needed was signaled not only by the Continental ethologists but by a few American comparative psychologists as well. Prominent among them was Frank Beach. In his article of 1950 entitled "The Snark was a Boojum," Beach

When Beach went on to discuss the potential benefits of a genuinely comparative approach, the first two authors he cited, even though he was talking about learning rather than instinct, were Tinbergen and Lorenz. He cited Tinbergen for his studies on learning in the hunting wasp. He cited Lorenz for his observations on imprinting in precocial birds.[21]

Beach's case would be worth a more extended examination than can be provided here. Lorenz, in a letter to W. H. Thorpe in 1955, described how he had made a convert out of Beach by showing him films. In Lorenz's words: "The best means to convince people that there is2a"ree

insights that ethologists could gain from American comparative psychology.

The present story would be simple enough if the ethologists' fact of significant interspecific differences in behavior made steady inroads into American comparative psychology after 1950. It did make inroads, but only haltingly, and with qualifications. There were several reasons for this. In the first place, American comparative psychologists as a group were simply not interested in details about animal behavior that occurred in naturalistic rather than laboratory settings. In the second place, it was one thing to acknowledge the existence of species-specific differences in behavior but quite another to accept the ethologists' assumption that the behavior in question deserved to be called innate. In the third place, in acknowledging the importance of interspecific behavioral differences, the comparative psychologists could turn the tables on the ethologists and criticize them for having too readily supposed that behavior displayed at one level of organic complexity was comparable to behavior displayed at another level of complexity. These last two points were among those presented by the American psychologist Daniel Lehrman in his famous critique of Lorenzian ethology in the *Quarterly Review of Biology* in 1953. Lehrman's paper also took note of the political dimensions of Lorenz's writings. Criticizing Lorenz for "[equating] the effects of civilization in human beings with the effects of domestication in animals," Lehrman observed that Lorenz had promoted this idea in 1940 in support of German race purity laws.[24]

Associations such as these are not incidental to how facts travel. The question of Lorenz's wartime affiliations would continue to lurk in the background as Lorenz offered new pronouncements about the biological

[24] Daniel S. "A Critique of Konrad Lorenz;s theory of instinctive behavior," *Quarterly Review of Biology*, 298 (1953). 354.

bases of human behavior and how these related to the human predicament. Of these new pronouncements, two in particular were especially eye-catching. Lorenz's first claim was that the human species is unique among higher animals in that it lacks innate inhibitions against killing its own kind. The second claim was that aggression is an instinct, and that, as such, it builds up internally, like a fluid in a reservoir, eventually requiring release. Lorenz presented both of these claims essentially as facts, though neither is credited with that status today, nor were they universally regarded as such when Lorenz first pronounced them.

Lorenz highlighted the first of these claims with a striking contrast between wolves and doves. Wolves, he allowed, have been equipped by evolution not only with fearsome weapons—their strong jaws and their sharp teeth —but also powerful, instinctive inhibitions against using these weapons against other wolves. When two wolves fight, and one gets the better of the other, Lorenz explained, if the loser submissively exposes its neck to its adversary, the victor cannot finish the loser off. Instinctive inhibitions prevent it from doing so.

Doves, in comparison, have no powerful natural weapons. Because of this, they have not had to develop inhibitions against hurting their own kind. In nature, by Lorenz's account, when two doves fight, the bird that loses can simply fly away. If the birds are confined to a cage, however, fleeing is impossible, and the weaker bird is in danger of being killed, because the winner has no innate inhibitions against continuing the fight to the end. Lorenz described how he placed a male turtledove and a female African blond ringdove together in the same cage, hoping they would mate. When he returned, he found that the ringdove had nearly pecked the turtledove to death.

Were there implications here for the human species? Lorenz believed there were. The human species, he argued, is more like the dove than the wolf when it comes to dealing with its own kind. Humans do not have powerful natural weapons, like wolves do, and thus until relatively recently, evolutionarily speaking, humans have had no need to develop strong instinctive inhibitions against killing one another. Unfortunately, in the latest stages of our histor-23srla(the lar 0 TD-0.00anctn9st)-7phh.0073 , and thus

The present writer has not attempted to track how far this first claim by Lorenz traveled.[26] It seems, in any case, that Lorenz's ideas on aggression traveled farther, at least in the sense that they were more widely and recurrently debated. What Lorenz claimed to offer in his best-selling book of the 1960s, *On Aggression*, was an analysis of the natural history of aggression. His basic message was that the human race had to come to understand its instinctive aggressive drives in order to learn how to deal with them, and that essential to this understanding was a recognition of the positive as well as the negative aspects of aggression. Although man was faced with a predicament of the most urgent sort —"in his hand the atom bomb, the product of his intelligence, in his heart the aggression drive inherited from his anthropoid ancestors"—Lorenz was prepared to offer an "avowal of optimism." He believed the biologist could rescue humankind from its precarious state by teaching humans to change for the better.[27]

Lorenz's portrayal of aggression attracted a great amount of attention. The critics included ethologists as well as representatives of other disciplines. They rejected in particular his claim that aggression is an instinct that builds up internally and requires release.[28] Even as an idea that has been rejected, this one

He is remembered in two contexts: One as the author of an interesting but generally discredited theory of aggression; the other as the scientist who called attention to the phenomenon of imprinting.

Most of the “traveling facts” offered thus far in this paper have straddled the line between facts and theories. Let us consider yet another case, in this instance one where where some of Tinbergen’s facts passed beyond ethology to the realm of American social science education. This occurred in the federally-funded social science curriculum entitled “Man: A Course of Study” – or MACOS. The prime mover of the curriculum was Jerome Bruner, the cognitive psychologist who was co-founder and Director as of 1960 of the Center for Cognitive Studies at Harvard. Bruner’s desire was to “form the intellectual powers” of the students the curriculum was supposed to serve, namely elementary school students in the fifth and sixth grades. He wanted students to become self-conscious about their strategies of thought. The content of MACOS was identified in 1965 as “man: his nature as a species [and] the forces that shaped and

coast salmon and the herring gull. Baby salmon must do without parental protection in their struggle to survive. Their story was used to highlight the significance in humans of the length and the quality of the *human* infant's dependence on its parents. Herring gull chicks, unlike baby salmon, are taken care of by their parents. The gull story, based on the work of Tinbergen, was used to examine more closely the *causes* of animal behavior. Observations of how the gull chicks must peck at the red spot on their parent's beak if they are to be fed provided an entry to the discussion of innate versus learned behavior. The herring gull section also helped introduce the idea that behavior patterns, like physical structures, should be understood in terms of their adaptiveness or survival value.[31] Beyond this, the herring gull study was intended to give children the opportunity to study territoriality, fighting, and communication. The authors of MACOS suggested that children are intrigued by the idea of an aggressive instinct, and the gull study would allow them to "consider the ways a human handles his aggressive feelings without really fighting." They recommended that children be given a chance to act out scenes of adult male fighting in herring gulls, where the use of particular bodily gestures enables the antagonists to escape serious harm. Ta Tw[(r of adult366ped)]TJ-15.0404 .37

As he put it, “in different environments, different characteristic are adaptive.” Expressing a theme that would recur at different levels through the course, Trivers wrote: “There are no traits in this scheme that have an absolute value, an absolute value irrespective of the environment.”[33]

The notion of no traits having an absolute value irrespective of the environment was what ultimately caused trouble for the MACOS curriculum. Perhaps no one would have objected if the story had stopped with herring gulls or even baboons, but when it was applied to human behavior, as exemplified by the lives of Netsilik Eskimos, this was too much for people who believed that human values are God-given.

In her book, *Science Textbook Controversies and the Politics of Equal Time*, Dorothy Nelkin describes what transpired. United States Congressman John Conlan of Arizona in 1974 described MACOS as “a Godawful course,” “almost always at variance with the beliefs and values of parents and local communities.” He urged that National Science Foundation appropriations for MACOS be terminated because of its “abhorrent, repugnant, vulgar and morally sick content.” Federal funds were withdrawn, and textbook sales dropped sharply between 1974 and 1975.[34]

While MACOS stirred up one angry group, E. O. Wilson’s book, *Sociobiology: the New Synthesis*, published in 1975, stirred up another. The most vocal protesters in the second case were not conservative, fundamentalist Christians but instead the radical scientists who constituted themselves as the Sociobiology Study Group of Science for the People. In

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[33] Robert Trivers, “Natural Selection,” in *Man: A Course of Study. Talks to Teachers* (Curriculum Development Associates: Washington D.C., 1970 [1983 edition]), pp. 35-41, quotation on p. 41. This volume, *Talks to Teachers*, also included a section by Tinbergen entitled “The Study of Animals,” extracted from his book *Animal Behavior* (New York: Time-Life Books, 1965), and a section by Irven Devore, with the assistance of R. Trivers and I. Rothman, entitled “Innate and Learned Behavior.”

[34] Dorothy Nelkin, *Science Textbook Controversies and the Politics of Equal Time* (Cambridge, Massachusetts: MIT Press, 1977), p. 112.

their attack on Wilson, they lumped him together with Lorenz as a biological determinist and noted that Lorenz had been associated with the Nazis. But there were other critics as well. One of these was the American psychologist Frank Beach. Prominent among Beach's complaints was "[the sociobiologists'] apparent omission or disregard of facts concerning

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