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Human Ethology: Claims and Limits of a New Discipline, edited by M. von Cranach, K. Foppa, W. Lepenies, and D. Ploog. Cambridge: University Press: 1979, pp xix + 764.

This two-kilogram volume, which was five years in the making, delivers the goods promised by the title. It is the written record of the Werner-Reimers-Stiftung Conference on Human Ethology, held in Bad Homburg, October 1977. It is meant as a state-of-the-art publication, sampling the thinking and findings of 36 researchers in human ethology and neighboring fields. The editors have encouraged contributors to bare their methodological breasts and to express their hopes or doubts. On the whole the invitation has been accepted, prompted perhaps by the conference format of challenge and response. The prose style is remarkably even, considering the disciplinary and national variety of the contributors. It is the compressed, dry prose of research papers and referees' reports. But, happily for the editors, the studied self-effacement of science is often penetrated by limbic pathos. There are territorial fights aplenty here; also threats and appeasement gestures, agonistic challenges and blandishments, not to mention occasional flashes of moralistic aggression.

Inspection of the contents reveals the powerful presence of Konrad Lorenz. Part I, Phylogenetic and Cultural Ritualization, contains the lead paper by I. Eibl-Eibesfeldt. Aware of what the occasion demands, Eibl-Eibesfeldt, and elsewhere in the volume, Paul Leyhausen, mince no words in responding to critics of the master. The Lorenzian heuristic construes its objects as behavior patterns whose physiology, phylogenetic origin, and adaptive function it is the job of the ethologist to discover and to integrate. Behavior is understood when classified and explained on these three planes. The keenness of Lorenzians on biological mechanism has generated a dazzling hardware of Innate Releasing Mechanisms, Fixed Action Patterns, and so on. This hardware and persistent mechanistic thinking lends Lorenz's school its special style. It also provokes the critics. Schneirla [1966] and others (not represented in this volume) object that the deployment of these concepts obliterates the ontogenetic interaction of organisms with their environment [Barnett, 1979; Hinde, 1974]. No behavior is properly said to be innate, they say, nor is any structure simply determined by the information in the zygote. The ethologist must con-

concentrate instead on the interplay between the organism and its entire environment (including conspecifics) if they are to understand the causes of behavior.

Eibl-Eibesfeldt dismisses these objections as an *ignoratio elenchus*, as Tinbergen did over a decade ago. The mechanist, he claims, does not deny interaction: "If our discussants argue that strictly speaking there are no innate behavior patterns, we would agree. The term is sloppy. Behavior patterns develop, but what must be recognized is that they do so with a range of variability determined by the genome" (p. 6). Having so established himself, Eibl-Eibesfeldt takes back what he apparently gave away: the range of variation may be very small — small enough to warrant the Lorenzian hardware. Clearly then the dispute concerns not interaction per se, but whether variability undermines the notion of behavioral mechanisms.

This debate is symptomatic of the current state of research. The critics persuasively maintain that Lorenzian ethologists, eager to get on with it, have run ahead of physiology and perhaps mistake imaginative description for science.

Although none of the conference participants expressly formulates this objection, it bounces through papers and discussion like a poltergeist. (See especially the contribution of William R. Charlesworth and Willem Levelt's discussion.) The objection goes to the core of the larger question whether human ethology is presently a viable research program. This book provides some good reasons for an affirmative answer.

The special genius of ethology is its power to isolate and observe behavior patterns and to compile behavioral repertoires ('ethograms') which integrate behavior with bodily structure and evolutionary adaptation. Even if human ethologists could appeal only to comparative anatomy, we should still be impressed by their ability to detect in *Homo* behavior that social scientists ignore. It is ignored, or goes undetected, because social scientists have no framework for assessing the significance of the eyebrow flash or curled upper lip, whereas the ethologist has. *Human Ethology* amply documents this observational finesse and its palpable significance.

Let me mention a few illustrations. In a terse footnote, Eibl-Eibesfeldt writes: "We are at present engaged in a program of cross-cultural documentation of unstaged social interactions and rituals. So far, 120 km of 16-mm films have been collected . . . every filmed event is accompanied by a protocol which gives the social context in which the pattern occurred . . . this alone allows an objective correlational analysis" (p. 20). The ethologist's film record of observations must soon transform anthropological method. Anthropological field studies labor under the liability that no matter how carefully done, they remain anecdotal accounts (usually by one observer) and therefore have no greater scientific value, as observation, than travelogues. Why have anthropologists not hit on the "obvious" remedy of systematic film use? For the same reason that they have not hit on the fast-action camera: the behavior ethologists observe is meaningless in the culturally-oriented anthropological explanatory framework [Shafton, 1976]. Summarizing results that

integrate his findings with comparable research on deaf and blind children, Eibl-Eibesfeldt rules out the possibility that some of the behavior he records could have been socially learned. He might also have mentioned the study by Richard Wills [1973] of the severely mentally handicapped who, though able to acquire only a few culturally transmitted forms of behavior, nevertheless retain a remarkably complete repertoire of primate behavior patterns and also speech.

Another illustration of the significance of ethological observation comes from Erving Goffman's paper, "Response Cries." Goffman's work is *sui generis*. As an ethnomethodologist, he eschews the standard methodology of the social sciences, and makes no attempt to connect his findings with conventional theoretical structures. His forte is minute descriptions of episodes of everyday life, loosely grouped around the theme of self-presentation in face-to-face encounters. His work reads more like fiction than science: one thinks of Jules Feiffer's *Little Murders* or Joseph Heller's *Something Happened!* It convinces as fiction does, by representing those habitual actions so revealing of emotions and self-conceit, which swim with little immoralities and repressed mortifications. In other words, Goffman has got our number, and we know it. Sociologists have not quite known what to make of this work. It has the unmistakable ring of truth, but where does it fit in? Jürgen Habermas, his respondent, fumbles with this problem as others have. No wonder. Goffman studies what he calls the "primitive, unsocialized" self (p. 238), whereas the ruling idea of the social sciences since Durkheim is the Social Man whose emotions and behavior are determined by "society." Since the visceral self is for the sociologist about as real as a unicorn, Goffman's characters cannot in principle exist; yet there they are in practice.

One can imagine strategies for linking Goffman's "intuitive ethology" of complex interactions with proved ethological techniques for studying simpler behavior. The appropriate level for this integration would be that of social psychology. The outcome of pairing social psychologists with ethologists in this volume suggests that competition in theory construction may pose a larger problem than breaking the code of group behavior. Ethologists Kummer and Reynolds cross swords with social psychologists Bandura, Tajfel, and von Cranach about questions of method and substance. Although the social psychologists declare their openness to ethology, they seem not to have digested the manifold implications of the premise that human sociability is evolved for tribal life [Lee and DeVore, 1968; Masters, 1976]. Bandura's elaborate models, with causal lines connecting "groups" to the complexity of contemporary societies, are unnecessary and misconceived if the ethologists are right.

In any case, it is hard to see how the models' complexity can be resolved into experimental studies. As Reynolds points out (p. 363), Bandura, for all his moves away from conditioning theory, still operates with the old Durkheimian belief that individuals are brain-washed (socialized) by some empirically inaccessible group mind (*conscience générale*) which imposes its own cognitive structures.

Ethologists have a thinking subject. The study is especially "element" of social. There will be profound understood on social psychology is not emotional. Flocking categories, and the Social scientists with a cognitive connection. They would rather rock concerts as social. That bonding and wealth of anecdotal identification is a to focus less on "

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Political institutions legitimating beliefs large numbers. "Structures or the consensus in the search process), it postulates challenges to dominant

Ethologists have no need to postulate group minds or the notion of "society" as a thinking subject generating "social cognitions" [von Cranach, p. 427]. Kummer's study is especially pertinent here, since he undertakes to show that the "unique element" of sociability is "the relationship between two individuals" (p. 382). There will be protests against this Spartan parsimony: how can crowd effects be understood on such a procedure? Probably they cannot. But then cognitive social psychology is not likely to be very useful either, for crowd effects are eminently emotional. Flocking, ritual and mood contagion are the appropriate ethological categories, and they lead back to tribal sociability and non-verbal communication. Social scientists who follow these hints would not attempt to find, for example, a cognitive connection between a political ideology and the success of party rallies. They would rather class evangelical revivals, party rallies, football matches, and rock concerts as settings contrived for mood contagion and solidarity feelings. That bonding and group identification occur in these situations is supported by a wealth of anecdotal evidence and naked-eye observation. But to understand why identification is classified (by participants) one way rather than another, we need to focus less on "cognitions" than on who dominates the group.

That brings us to politics. Roger Masters in this volume suggests that politics may be understood as an extension of agonistic and dominance behavior that evolved in the tribal bower. Cognition comes into play when civic life suppresses a number of activities common to tribal life: it calls into existence discipline and organizational complexity unknown in the tribal system, for example, class distinctions and division of labor anchored in distinct vocations. These factitious forms of association, which are the hallmark of political life, require practical intelligence to invent and to operate. "Cognitive structures" are therefore quite pertinent to the social psychology of political association. These structures do not, however, appear to emanate from the consensus of a group, or more vaguely, "society." If consensus sufficed to establish and maintain them, the use of organized force would be unnecessary. Yet we observe the regular use of organized force to compel compliance: judicial proceedings of accusation and defense are the "cognitive structures" through which the civil use of force is characteristically exercised. A consensual origin of "cognitive structures" seems also incompatible with the dominance and command relations built into political institutions, and with the fact that the institutions are usually manned by competing or cooperating individuals.

Political institutions are used by the persons who man them to indoctrinate legitimating beliefs, to morally coerce and coopt, and to direct the behavior of large numbers. "Society" is not a useful analytical concept to express these relationships or the cognitive structure involved. Apart from leading to an infinite regress in the search for causes ("society" as subject and object of every causal process), it postulates a homogeneity and consensus at odds with the ongoing challenges to dominant individuals and institutions.

The research position of social psychology may therefore be described as follows: the psychology of individuals in institutional settings may be expected to show a mixture of evolved tribal sociability, which largely governs face-to-face encounters, and the technical and legitimating cognitive structures that organize the functional activities of institutions. Goffman's intuitive ethology perhaps describes the psychological predicaments of individuals interacting in these mixed social spaces. It would be interesting to investigate whether the aggression, distress, and mortification of his actors are in some part the outcome of stress induced by the overlay of political association on tribal sociability.

Exactng as such a research program might be, contributions to *Human Ethology's* section on ontogeny indicate that the track is at least accessible by filling in more of the blank spaces in the ethogram. They also vindicate some of Tinbergen's and Lorenz's most important hunches about *Homo*. The papers in this section are on the whole perhaps the most innovative and exciting in the book. Papoušek and Papoušek, who pioneered new work on human infants during the sixties, contribute a synoptic, descriptive essay, "Early ontogeny of human social interaction: its biological roots and social dimensions." Colwyn Trevarthen contributes an ambitious paper, "Instincts for human understanding and for cultural cooperation: their development in infancy." This work, together with William Mason's report of studies of the ontogeny of rhesus monkey behavior, helps to operationalize a basic ethological hunch: that *Homo* is biologically endowed with behavior patterns and potencies thanks to which each individual actively negotiates his environment.

Trevarthen lays out his stunning claims in his opening sentence: "In this paper I claim evidence that human infants are *intentional, conscious* and *personal*; that above all they have a faculty of *intersubjectivity* which is in embryonic condition in the neonate and rapidly developing active control over experience after that, and which soon becomes the central motivator and regulator for human mental growth. I believe that each of the words italicized above may be defined clearly and defended scientifically by reference to how infants behave." Trevarthen's work is inspired by the psychobiology of Roger Sperry [1976] and the developmental psychology of Piaget [1962] and Bruner [1968]. His method is to contrive video-taped studio situations to facilitate observational study of the spontaneous behavior produced by the self-regulating psychobiological processes he describes (p. 582). The stimulus-response experimental approach is not suitable for this work, because it would "evade or destroy the kind of self-regulating process which we want to study." He is firm on this point, somewhat to the dismay of his commentator, Dalbir Bindra. Conditioning experiments assume that the experimental subject is a passive responding mechanism (p. 534); but if the subjects are autonomously active, it will not be easy to interpret what they do when stimulated if one has no provisional idea of what physiologically and behaviorally they are. By contriving his experiments to let infant and mother show their natural behavior to the analytical observer, Trevarthen aligns himself with ethological methodology (pp. 568 and following).

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Whoever eschews S-R psychology on these grounds is likely to be out of sympathy with sociological accounts of maturation. Trevarthen is. The infant and child are not so conditioned by the social milieu that it learns from others how to think, to assume a gender, to be a self, and so on. Instead, "what is learned and how it is learned are limited, regulated and organized by specific forms of [preadaptive] readiness of the infant for social interaction . . ." (p. 567). His most striking evidence against the passivity of socialization is his finding that all the functions required for self-awareness are present in ten-month-olds.

It seems to me that in method, observational finesse, and choice of subject matter, Trevarthen's contribution best exemplifies what human ethology might become in the next decade. His subject matter could scarcely be more human; but the behavior he studies is too plastic for the "rigid categories" of ethology (p. 567). The Lorenzian hardware is not supple enough for the human infant. Nevertheless, Trevarthen's approach, as he observes, is imbued with the spirit of ethology, including the capacity of his study to integrate phylogenetic comparisons.

A sign of this kinship is obtained by comparing Kummer's study of monkeys which attempts to locate the crux of socialability in the relation between two individuals, with Trevarthen's study of the mother-infant dyad. Furthermore, Trevarthen's objection to Lorenzian hardware is not an objection to nativism, but an appeal to the wide variation of brain physiology: "The life cycle of the brain looks like a cycle of mental growth with an enormous degree of prefunctional morphogenesis of main structures as well as great plasticity. It could well contain the specifications for a self-aware creature, with cooperative tendencies sufficient to generate cultures like those we know" (p. 571). This statement is an ethological way of making the transition from animal to human ethology.

The inevitable loose ends of such a collection do not detract from the solid contribution of the editors, contributors, and respondents. If not all points of view are represented, those that are sail in the mainstream of research. The editors have been fortunate in their groupings of contributors and respondents, which so often results in instructive disagreements. They have been thrice blessed in the splendid production by the Cambridge University Press.

Since it would be ungrateful to complain, I will not say that the volume suffers from a certain myopia, but rather commend it for remaining so steadfastly on the track of current research. Nevertheless, the contributors' persistent and trenchant critiques of the leading premises of current social science, together with the new methods and premises they illuminate, suggest that the estrangement of the social sciences from biological observation and experiment has been overcome, and that experimental social science has begun. In a statement characteristic of current thinking in the social sciences, Anthony Giddens [1976] has declared that sociologists who still await a Newton are not only "waiting for a train that will not come in": they are altogether "at the wrong station." Consistent with this despairing outlook, he asserts that sociology is basically an interpretative discipline, like literary studies, and that the social interactions studied by sociology are independent

of somatic properties. Now that the last of these dominoes have been removed, everything else must gradually fall into place. To be sure, the bureaucratization of academic life is a powerful bulwark against change, especially change so far-reaching. But there are signs that intellectually serious social scientists will draw appropriate conclusions from the fact that *Homo* is sociable by nature.

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