to treat human social behavior as a special case.

Note that it is not human biology in general which is separated by recent evolutionary and historical events. Human physiology is much like that of other mammals, and most of medical research is based on animal models (Bourne 1973). Much of what is known about the human brain is based on research on laboratory animals, especially rats, cats, and monkeys. There is nothing special about the mechanics of human genetics or human evolution. However, the final product of these very general processes has the ability to learn and communicate at a level that makes it useful to separate social science from biology.

The analytical separation is useful. It avoids the kind of misunderstandings that make no clear distinction between genetics and learning, nature and nurture. The social sciences depend on this distinction. Sociobiology depends on returning to the nineteenth-century confusions.

There are three classes of problems that need to be kept clearly in mind: the understandings that come from human biology (heavily dependent on experimental studies of other animals), the understandings that come from the study of human social behaviors, and the interrelations of the two. Psychiatry is the best example of useful relations between the two.

The fundamental issues are old and were clearly stated many years ago (long before I was college!), but modern biology is complex, and evolutionary theory cannot be stated usefully in a few pages. There are major problems in the theory itself and in its application to any particular case. Sociobiology may be described as genetic theory without genes, a theory that reduces biology even more than it does social science.[1]

Crisis In Anthropology,
Garland Publishing, Inc.
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Hoebel, E.A.; Currier, R.;
Kaiser, S. editors.

Chapter 23

HUMAN BEHAVIORAL BIOLOGY: PREPARATIONS FOR THE BIRTH OF A PARADIGM IN ANTHROPOLOGY

Melvin J. Konner

I

I am grateful for this opportunity to write a few words describing more generally my vision of the future of biological anthropology in the context of anthropology. Being young, I will leave history to teachers and colleagues who have lived it; to their account I can add little besides easy hindsight. Being inexperienced, I will, a bit brazenly, set out my sense of the emerging present, blithely oblivioua of how silly it

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This essay is dedicated to the memory of Gerald Henderson, something similar in outline form, and was teaching it, in 1965. The author thanks Irven DeVore, Beatrice Whiting, John Whiting, Lionel Tiger, Alex Morin, Nicholas G. Blurton Jones, Jane Lancaster, and especially Marjorie Shostak, for invaluable discussion. During most of the period leading to this paper, the Foundations Fund for Research in Psychiatry and the Harry Frank Guggenhein Foundation supported the author's work.

may seem a few years hence. I hope that forthrightness will not be mistaken for arrogance. I feel very keenly the privilege of being heard by people whose thinking formed my own. I believe that misplaced reticence would serve the privilege poorly.

To begin, I will step back a bit from my dot on the discipline's map and attempt some brief remarks on the look of the whole. Then, refocusing on the title's promise, I will describe an emerging paradigm that, I believe, can reunify and further invigorate biological anthropology, give it a new platform from which to communicate with ethnology, archaeology, and linguistics, and fill an important gap in current liberal arts and sciences education.

I believe there *is* a crisis, but not the one some see. It is not the one caused by the modernization of tribespeople, nor the one caused by the recalcitrance of Third World governments, nor the call for "application" and "action," nor the economic recession affecting universities. Though these are certainly crises, the first three seem to me to have been foreseeable and foreseen, and to be with some adjustment, surmountable. The last seems beyond the scope of this Conference.

The crisis I see is a deeper, intellectual one. It has been characterized inadequately in various ways, currently as the "fragmenting of the discipline." This phrase gives us a poor view of the problem. The problem is not new nor caused by recent advances. Coming as they did from law, business, physics, classics, medicine, philosophy, with their stunningly disparate notions about people and how to study them, the "fathers" of the discipline brought it into the world in pieces--or perhaps as a multiple birth. The businessman who embraced the Iroquois and then spawned a brood of "laws" that became a cornerstone of Marxist thought; the young physicist who turned from seawater to Eskimos and, thereafter, turned his back on "laws" forever; the classicist who helped us marry psychology; the South African physician with his own handaxe to grind--considering these, and others, it is not "the fragmenting of the discipline" but the fact that their disciples managed to rally under one banner, that should make us wonder.

Since this mythic and heroic birth, the discipline, far from fragmenting further, like the Plains Indian Trickster, through a series of questionable antics, has been healing and growing whole. Before skeptics I place in evidence Exhibit A, the Spring Hill Conference, as odd a collection *éminences grise* and raw young thugs as ever assembled under one roof. All call themselves anthropologists; none shout "Let's put a stop to this madness!" and all seem to think it should go on.

The crisis, then, is a crisis of healing, like the distressing itch of a wound as it knits together. There is one crucial cleavage line that I believe will endure, and to which I will return. This aside, we have grudgingly acknowledged each other's existence, shrugged, and gotten on with it. Having embarked on this little boat, anthropology, perhaps as a life raft shoved away from a sinking social science, we were none too pleased to be thrown together; but gradually we stopped trying to cast each other overboard and settled down to the business of bailing water.

The healing crisis is twofold, as I discern it, corresponding to two revolutions--or perhaps revitalizations--one nearly completed, one just beginning. The first is the *scientization* of the nonbiological sections of the discipline.[1] By this I mean not quantification alone--although this has certainly happened--but the whole panoply of scientists' shop tools, including classification, induction, hypothesis testing and rejection, theory building, and above all, the turn of mind which actively seeks laws of nature--laws of structure and function, as well as of cause and effect. We need not belabor the point. Some of the people who helped bring into anthropology psychological measurement for culture and personality studies, the method of controlled comparison for the study of sociocultural evolution, econometrics for the study of development, and cross-cultural method and ecological theory for the study of world ethnographic samples, are contributors to this volume. The laws, such as they are, appear rather small and inelegant compared to the grandiose plans of the last century. This is as it should be. People being biological phenomena, laws explaining them are likely to resemble biological laws; except for natural selection, these *are* pretty small and inelegant when compared with physical laws. (Even the physicists, to be fair, are learning to brave complexity, as the number of subatomic particles approaches the number of cultural units in the World Cultural Sample.)

The second process--the new, and consequently controversial one--I will call the *biologization* of the already "scientized" sociocultural subfields. This second revitalization has been four-pronged, as biological troops have marched into sociocultural anthropology (and archaeology) under the separate banners of evolution, ecology, population biology, and ethology. Montagu's pioneering collection, *Culture and the Evolution of Man* (1962), Spuhler's *Evolution of Man's Capacity for Culture* (1959), Count's *Being and Becoming Human* (1973), and Washburn's *Social Life of Early Man* (1961), among others, sounded the call to battle, and rallying under their banner were subsequent books and papers such as Lee and DeVore's *Man the Hunter* (1976), Rappaport's *Pigs for the Ancestors* (1967), Harrison and Boyce's *The Structure of Human Populations* (1972), and Beatrice and John Whiting's *Children of Six Cultures* (1975). Meanwhile, Harris's *The Rise of Anthropological Theory* (1968) and Tiger and Fox's *The Imperial Animal* (1971) continued to sound the battle cry, as the battle raged.

I realize that these are strange bedfellows. Since all have tended to bring evolution, ecology, population biology and/or ethology into the theory and/or method of anthropology, I view them all as parts of the general *biologizing* process.

Again, two processes: the first, affecting most of non-biological anthropology, resulted in a sort of post-Malinowskian functionalist empiricism. Indeed, whoever merely accepts Malinowski's notion of the social order as a "vast instrumental reality" existing to satisfy human needs for nutrition, reproduction, bodily comforts, safety, relaxation, movement and growth is already well disposed toward the outlook presented in this chapter (Malinowski 1939; see also Harris 1968:549-551). (I am not making the emic/etic distinction; emic things can be empirically and functionally studied--as in much of *Coral Gardens and Their Magic*. Thus, I include cognitive anthropology under the rubric of my first "revitalization.") The second process, following on the heels of, and superimposed on, the first, is an imperial, often imperious attack on the nonbiological sections of the discipline by biologists, sometimes through a fifth column. The juxtaposition of the first and second processes is not a coincidence. Since people are biological phenomena, it was inevitable that "scientizing" the study of them should finally lead to biology. The biologists, for their part, have waited patiently, if eagerly, for the ground to be prepared for them to strike. In the last couple of decades, it was prepared--and they have struck.

This is how it seems to a not quite metamorphosed biological anthropologist, snug in his cocoon, peeking out. If it is utterly useless as an etic grasp of the discipline--and, recall, there is still that "crucial cleavage" to deal with--let it at least serve as an account of something emic: the folkview offered by a bemused, well-meaning informant, wrong perhaps, but data in its own right.

II

Let me now turn to the subject of the title. In 1950 or so, the ancestor of biological anthropology--called physical anthropology--was not the most appealing of all undergraduate fields. Its subject matter was the somatology of the living and the dead, especially long dead. Its data were measurements of length and, sometimes, thickness. Its dimensions were geological time, geographic distance, and years since an individual's birth. Its communication with social anthropology consisted of a grunt in the faculty lounge and an occasional joint expedition. Its "theory" is best not spoken of. A conception of what it was like, in the context of related fields, is shown in Figure 3.

I am unfair for the sake of the joke, but not far off base. To its credit, it had stirred excitement about fossil man for almost a century, and Hooton had recognized the existence of nonhuman primates. The study of the secular trend and other aspects of physical growth verged, occasionally, on the physiological. To its debit, it at various times allied itself with racism, although not always deliberately.

In any case, we have left it far behind. What we have in its place--biological anthropology--is different enough to have earned its new name. Figure 4 shows its corresponding disciplinary structure and context. I will not dwell on it further; the diagram is largely self-explanatory. To be really adequate, both diagrams would have to be drawn in a multidimensional hyperspace; two-dimensional representation obscures many relationships. But you will get the idea. The remainder of my remarks will explore some relationships in the left half of Figure 4.[2]

What I discern in the subdiscipline at present is a great intensification of activity in the left half of the figure. I call this portion Human Behavioral Biology but we might more aptly call it Biobehavioral Anthropology, Physical Anthropology of Behavior, or any one of various other names. Whatever we call it, if I am right, it will constitute a forthcoming major step in the history of the subdiscipline. Its gestation and development will occupy the energy of a substantial number of biological anthropologists for many years. For my "harder-nosed" colleagues, who see in the primate behavior and sociobiology "revolutions" a "softening" of the subfield, I propose a return to anatomy and physiology--but to varieties which will strengthen, rather than ignore, behavioral work. For those who see biological anthropology growing apart from the rest of the discipline, I propose a return to the central concern of anthropology--human behavior--but with an emphasis on universals, growth processes, sex differences, and biological correlates, which are simultaneously consequences *and* underpinnings of behavioral events. These subjects form a natural bridge to recent work in psychological anthropology, primate behavior, human growth, and human evolution. For those who see the study of human behavior as getting away from anthropology and into the hands of psychiatrists, psychologists, ethologists and others with little sense of the empirical range of human behavior, the role of culture, or the facts of human evolution--people with little of that special sensibility, that *sympathy*, anthropologists have for the people they study, I propose a paradigm shift which will bring this ball squarely back into

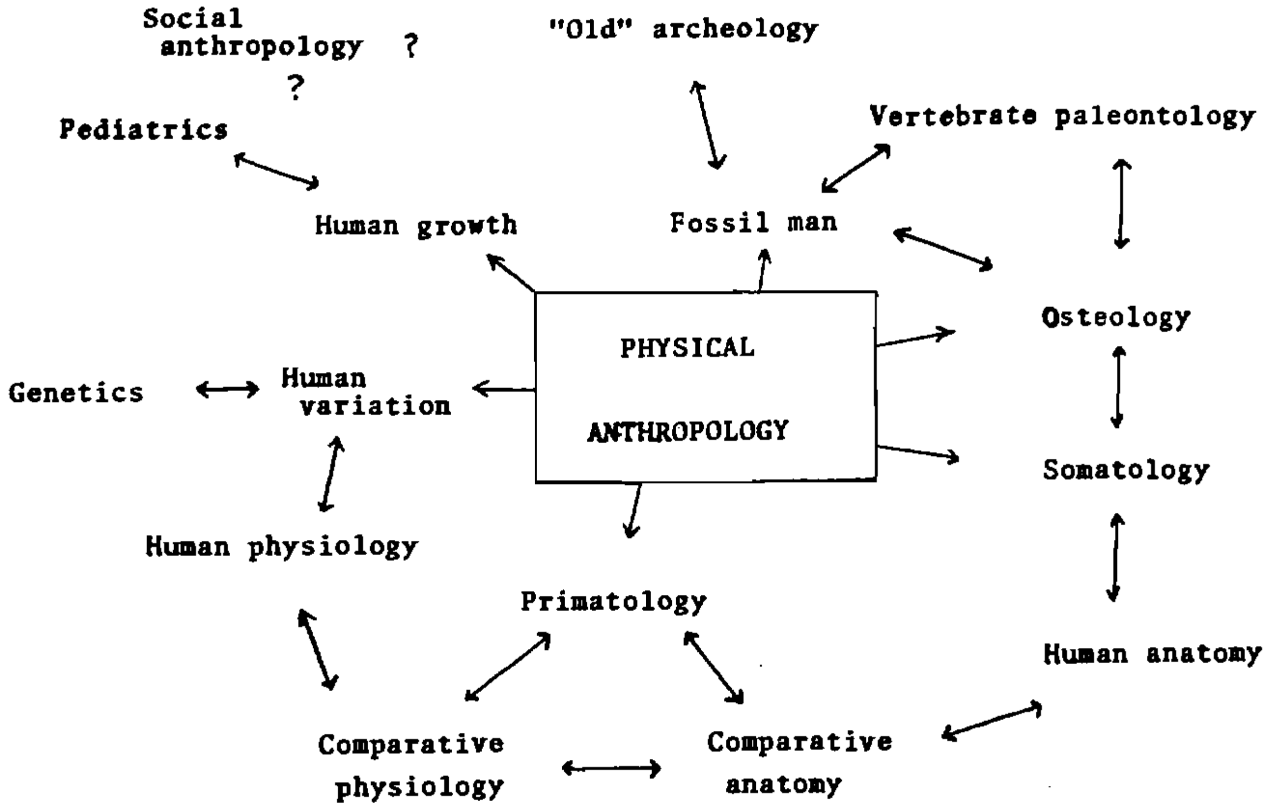

Figure 3. Physical Anthropology: Its Subfields and Related Areas, ca. 1950.

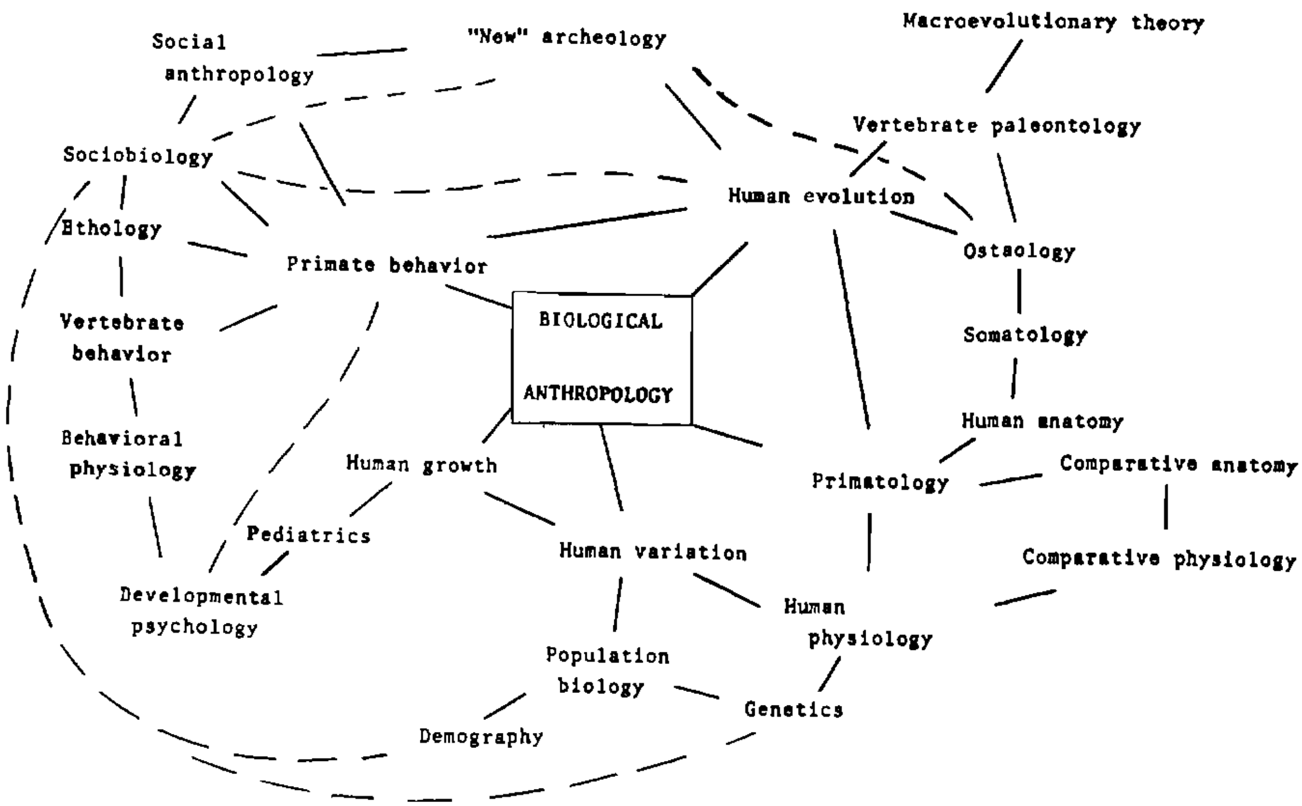

Figure 4. Biological Anthropology: Its Subfields and Related Areas, ca. 1980.

our discipline's court, where, I believe, it belongs. But we shall see. If I am right, I am not *proposing* at all, but merely describing or predicting. If I am wrong, then, of course, no amount of persuasion can make it happen.

But enough of soft-edged abstractions. A few details and examples of this so-called paradigm shift will evoke it better than such abstractions can. The question is, What kind of teaching and research will this entail?

I have now (January 1980) taught three times at Harvard a course entitled Human Behavioral Biology, as part of the upper-level undergraduate program in biological anthropology. Students from other fields of anthropology, and from psychology, biology, and human development--a stimulating mix--have taken it. Many are premedical students, for whom the course seems especially valuable, since they soon go on to a course of study with distressingly poor behavioral and evolutionary dimensions. They need more than a course that will make them "well-rounded;" they need a bridge from the anatomy and physiology they will be burdened with memorizing to the behavior they will experience every day. The students from behavioral disciplines need a course in behavioral biology that is sympathetic to their concerns and backgrounds--not an assault, but an invitation.

The course requires one year of college biology as a prerequisite. Students with previous courses in behavioral biology, especially behavioral physiology, are at an advantage, but we make every effort to prevent this from becoming a disadvantage to the others. Graduate students in various fields have taken the course, and teaching assistants have come from biological anthropology and, occasionally, from physiological psychology. The course has included a human brain prosection--watching a dissection by the instructor--each time it has been given; the last time it included, in addition, a sheep brain dissection by each student.

One may sense the character of the course from the following schedule of lectures:

I. Introduction
   1. Explaining behavior

II. Evolution of behavior
   2. Evolutionary process I: Genes in evolution
   3. Evolutionary process II: Natural selection theory
   4. Behavior-genetic analysis
   5. Non-Mendelian processes affecting behavior
   6. Macroevolutionary process and primate phylogeny
   7. Hominid phylogeny and probable behavior
   8. Principles of comparative ethology
   9. Species-specific behavior of *Homo sapiens*

III. Basis of social behavior in the neural and endocrine systems
   10. Central nervous system: Overview
   11. The nerve cell: Structure and function
   12. The nerve cell: Neurotransmission
   13. The limbic system
   14. The autonomic nervous system
   15. The hypothalamus
   16. The pituitary and some hormones affecting behavior
   17. The steroid hormones and behavior

IV. Development of behavior
   18. Evolution of the nervous system
   19. The early environment and the neural and endocrine systems
   20. Earliest reflexes and perceptuomotor development
   21. Maturation and learning in infancy: The epigenetic view
   22. The biological basis of earliest social behavior
   23. Adolescence
   24. Causation of sex differences in social behavior

V. Specific behaviors: Evolution, physiology, development
   27. Fear and the flight response
   28. Attachment and pair-bonding
   29. Parental behavior and varieties of altruism
   30. Courtship and sexual behavior
   31. Aggression and hierarchy
   32. Language

VI. Behavioral pathophysiology
   33. Schizophrenia
   34. Depression
   35. Behavior control

The teaching approach in Human Behavioral Biology begins with traditional subject matter of biological anthropology: the details and process of evolution, especially as applied to brain and behavior. It introduces the elementary anatomy and physiology of the nervous system (to the tune of many assurances that it *can* be learned), but with a strong emphasis on structural features believed to underlie human social behavior, including language. Following a biologically based account of developmental processes in behavior (which will probably be shortened because of a new course on development described below), we come to what I call the *payoff*: specific categories of human social behavior that are treated from the multidimensional viewpoint stressed and prepared for throughout the course. While others in the university devote much energy to arguments over whether evolution, or culture, or instinct, or early experience, or learning, or physiological effects, or cognition, or genes are the real and true causes of human behavior, we proceed on the assumption that we must distribute explanatory power liberally among all these causes and, more important, among their myriad complex interactions. For example, we treat language, a basic feature of human behavior, as follows:

I. The motor output
  1. Animal communication and human language
  2. Oral/aural communication, etc.
  3. Features of human language: Semanticity, productivity, displacement, arbitrariness (Hockett, Brown)
  4. Phrase structure grammar (Chomsky)

II. Neurology of language (Geschwind)
  5. Broca's area and aphasia
  6. Wernicke's area and aphasia
  7. Conduction aphasia
  8. Limbic aspects of language; Giles de la Tourette's disease; schizophrenic language
  9. Other aspects of hemisphere dominance

III. Language development
  10. Language specialization in neonate brain? (Witelson)
  11. Language receptivity; neonatal movement synchrony with speech (Sander)
  12. Receptivity in later infancy; motor precursors of language? (Kagan, Bruner)
  13. Language Acquisition Device; myelinization sequences (Lenneberg, Lecours)
  14. Child grammar (Brown)

IV. Phylogeny
  15. Primate communication and arousal
  16. Language learning in chimpanzees
    a. Sarah (Premack)
    b. Washo (Gardners)
    c. Lana (Rumbaugh *et al.*)
  17. The phylogeny of hemispheric dominance

V. Natural selection: Adaptive functions of language
  18. Teaching of young; mutual teaching of adults
  19. Planning of hunts
  20. Relationship to toolmaking and other skills
  21. Modulation of arousal
  22. Deception in courtship and dominance

Examination essay questions often require that language, say, or aggression be explained without neglect of any of the above-mentioned categories of causes.

Obviously, such treatment must be cursory, and the course itself must, in some sense, be superficial. But it does not mean to train students in a dozen bodies of scientific knowledge. Its purpose is to get them used to the idea of paying attention to all the categories while thinking about behaviors; to "inject" them with a model of human behavior which maps all these causes.

In fact, I actually *do* put them on a map, on the blackboard, repeatedly, during this part of the course. The map, in its most general form, appears in Figure 5. In specific lectures on, say, language or aggression, I fill in some of the known facts about the categories of causes, and students often use the model to structure their examination answers. The overall goal, again, is not to turn the student into an encyclopedia, but to constrain him or her to keep multiple causation seriously and firmly in mind while thinking about human action; to develop the habit of resisting the facile drift into one or another narrow realm of causation that we so often see, among not only students but colleagues, who usually have more to lose by granting any real weight to causes outside their province.

The course appears to succeed in this. If anyone accuses it of being a mile wide and an inch deep, one might point out that anthropologists have been accused of this before and that they have countered that our unique role in the disciplinary interstices provides an essential service to the academy. Students view the course as demanding, but that is almost beside the point. Accused of dilettantism, I recall Emerson's 1836 address at Harvard, "The American Scholar," in which he says we have had enough of fingers and elbows, what we need is a whole person. If true then, how much more so now!

At the first meeting of the course, I distribute the lecture outline with a cover sheet on which appears nothing but this passage from William Blake's *The Marriage of Heaven and Hell*:

> All Bibles or sacred codes have been the causes of the following Errors:
> 1. That Man has two real existing principles: Viz: a Body & Soul.
> 2. That Energy, call'd Evil, is alone from the Body; & that Reason, call'd Good, is alone from the Soul.
> 3. That God will torment Man in Eternity for following his Energies.
>
> But the following Contraries to these are True:
> 1. Man has no Body distinct from his Soul; for that call'd Body is a portion of Soul discern'd by the five Senses, the chief inlets of Soul in this age.
> 2. Energy is the only life, and is from the Body;

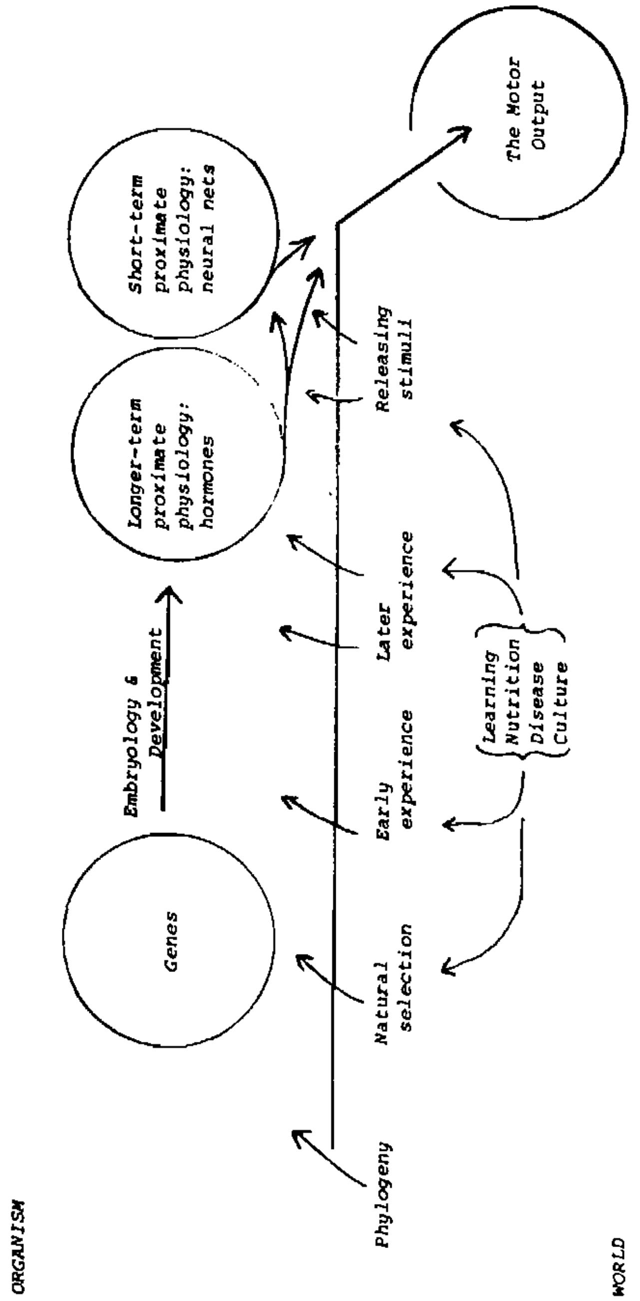

Figure 5. A Multicausal Model of Behavior, Shown As Used in an Undergraduate Upper Level Course, *Human Behavioral Biology*. The upper part shows events internal to the organism; the lower part, external events impinging on it. The model should be read from left to right, with *The Motor Output* as the explanatory goal.

and Reason is the bound or outward circumference
of Energy.
3. Energy is Eternal Delight.

It was a startlingly prescient passage for its time and is an apposite preview of the course today. But I often find my stduents puzzled by this, and other such things, that appear on most of my lecture outlines. "Why waste time on this," they ask, "when there's brain chemistry to learn?" To me, it is of the essence. It is precisely because their biology and chemistry professors do not do such things that I believe in the importance of this course. It is a course in anthropology and, as such, weaves the subjective and humanistic thread of experience into the fabric beside the chemistry, anatomy, learning theory, and laws of natural selection.

Another lecture course, begun in 1976, is entitled Development Through the Life Cycle. It is of interest here because it forms a natural bridge forward from two still vigorous anthropological traditions: physical growth (in biological anthropology) and comparative child development (in social anthropology). The course's physical growth thread focuses heavily on neural and endocrine growth, that is, on aspects of physical growth most intimately related to behavior. Students are asked to remember *amygdala* and *dopamine* rather than *nasion* and *acromion process*.

The topical subject sequence of the course on development follows:

1. Physical growth: General features
2. Neural, neuroendocrine, and neurochemical growth: An introduction
3. Behavioral growth: modification or metamorphosis?
4. Embryo into fetus: Neurobehavioral growth
5. Sex differentiation of the brain
6. Birth, I: The onset of physiological regulations
7. Sudden Infant Death: An extant question
8. Neurobehavioral status of the newly born
9. Transnatal environment effects
10. Early deprivation and the visual system
11. Neurobehavioral epigenesis: The first two years
12. Nutrition and neural growth: An extant question
13. Neurolinguistic epigenesis
14. Social behavior before the age of schooling
15. The five-to-seven shift
16. The control of the onset of puberty
17. The secular trend: An extant question
18. Adolescent metamorphosis
19. Rhythms: A natural history

20. Birth, II: The parent's experience and the onset of parent-infant relations
21. Aspects of adult behavioral growth
22. Decline and fall
23. Coda: Continuity in the development of behavior

The behavior-development thread of the course considers fixed and flexible features of the process; it uses the comparative and experimental literature on child training and early experience to explore the flexible dimension, as do courses in psychological anthropology. Psychological anthropologists who follow the literature of developmental psychology are aware that the fixed or maturational dimension has been gaining prominence in research and thinking for several decades under the rubric of *cognitive development*. This makes that literature all the more compatible with the study of neural growth. However, in neurobiology itself during the same period, several rich lines of experimentation have led to the firm conclusion that experience (including stimulation, stress, training, and social milieu) alters the anatomy and chemistry of the neuroendocrine systems in enduring and substantial ways (Fleeter and Greenough 1979, Hubel, Wiesel, and LeVay 1977, Stolk *et al.* 1974). This work gives vivid biological substance to the time-honored conviction of psychological anthropologists that experience accounts for much of the known variation in human behavior and that the effects of experience can be systematically characterized. From the viewpoint of the course on development, it provides a basis for a biological outlook that is not biologically determinist, but on the contrary one that brings new power and sophistication to environmentalism.

Figure 6 shows the model of human society which I believe reflects the outlook I am describing. It is extensively modified from the latest version presented by Beatrice Whiting and John Whiting in *Children of Six Cultures*. My additions are represented in dotted arrows; I do not suppose that Whiting and Whiting would approve of them. The model, however, provides a possible first approximation of the synthesis of biological and nonbiological processes which will be required for a true grasp of the organization of human social life. Anatomy and physiology do not appear on the model but are assumed; they are the substance of the arrows leading from "ecology" to "maintenance structure," from "child training" to "adult behavioral tendency," and so on. Only the arrows leading from phylogeny are different. They are not merely anatomical or physiological, but genetic. The genes provide the anatomical and physiological substrate of the causal relations; or more properly, part of this substrate. It is because the genes endow the system with certain features, that ecology may be said to

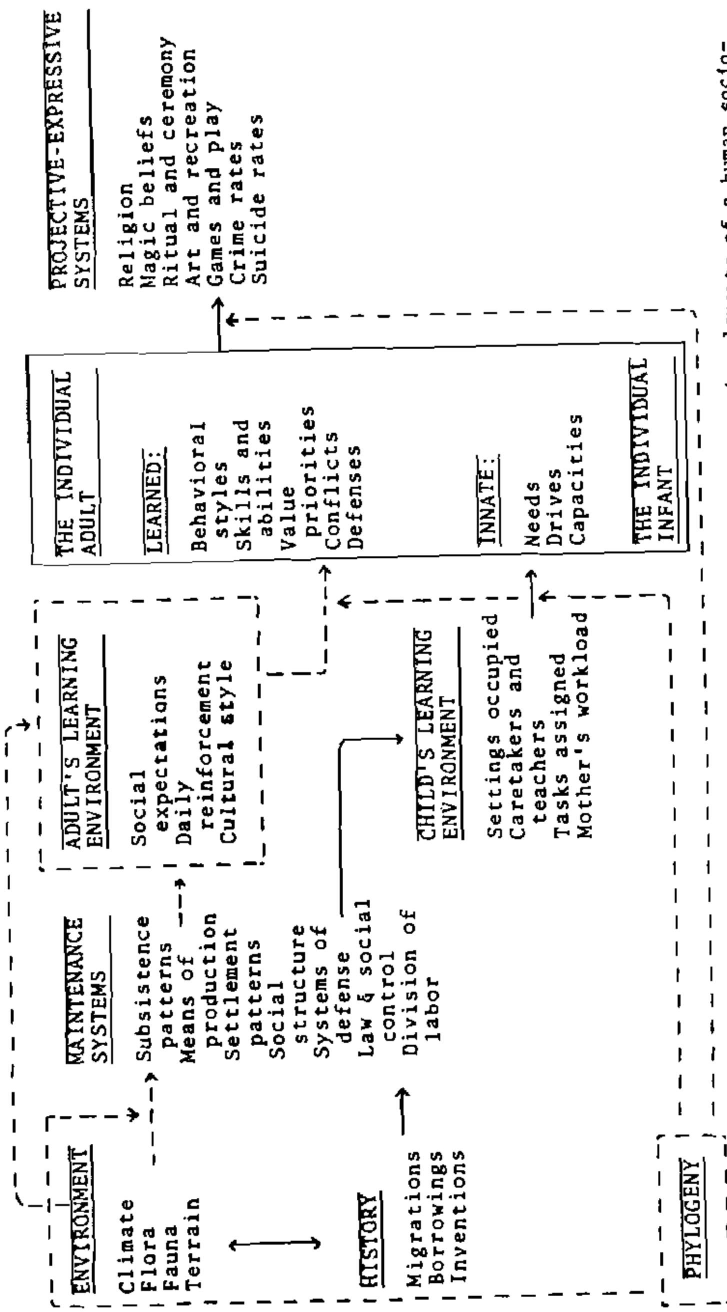

Figure 6. Modified Whiting and Whiting model of the relationships among the elements of a human socio-cultural organization, focusing on the relationship between the individual and society. The arrows indicate the general flow of causation as hypothesized by the model. Dashed arrows and dashed boxes show the additions made to the model. Feedback arrows are omitted, but feedback is presumed to occur. (Adapted from Whiting and Whiting, 1975.)

affect social structure and that training may change personality. In the felicitous phrase of Peter Ellison, the genes provide an equation for each of the causal transformations on the map, a plan for the response of the social or individual organism to the environment, and to changes in it. They are largely responsible for the phenomenological substance of the arrows.

Feedback, both positive and negative, must occur at many points in the model. I have followed the practice of Whiting and Whiting in omitting the feedback arrows for two reasons: first, I believe the primary direction of flow of causation to be as shown; second, I believe the model to be more testable in this form.

It seems to me that much of the anthropology of the future will concern itself with fleshing out this map. Human behavioral biology, dealing as it does with the nature of the arrows, will be an important, though small, part of this process. The rest will consist of improved versions of traditional and current psychological anthropology, ecological anthropology, archaeology, comparative child development, culture change, and cross-cultural statistical and nonstatistical comparisons, among other efforts. But a new kind of biological anthropology, centered in human behavioral biology, will have an unprecedented basis for communication with workers in these nonbiological fields.

Progress in a discipline may often be followed in the stages by which it incorporates or defines new units. So in recent years we have seen physical anthropology transformed by the gene and the fertility or mortality rate, ecological anthropology by the kilocalorie, psychological anthropology and primate behavior by the observable, countable behavioral event. I propose now that out of the "new physical anthropology" we make a *new* new physical anthropology by incorporating into our basic tool kit the neuron and what I call *the behavioral molecule*--chemicals acting as neurotransmitters or hormones affecting behavior.

From the viewpoint of psychological anthropology, enough is now known about events in the Black Box, including those which accompany complex behavior, so that this knowledge may not be ignored if the field is to continue to be a force in behavioral science. To be sure, we will still find out much using the Black Box assumption, but we will also learn by transcending it. The Black Box era is over in psychology and ethology, and it should end in anthropology. The neuron and the behavioral molecule enrich our understanding of every behavior we study, even if we look at it in isolation; but if we are to be interested in the effects of diet, genes, drugs, brain surgery, light, temperature, or stress on behavior, then the invocation of these units is indispensable.

Let me say that while I have modified the Whitings' model, I believe I have not changed its essential meaning. Their model was drawn to help incorporate psychoanalytic theories--and units--into psychological anthropology, in addition to subsuming some aspects of learning theory and the soft determinism of the ecologically oriented social anthropologists and archaeologists. Freud, too, believed that the arrows were physiological and produced by a long phylogeny, but he did not know how to study these things in any substantive way. Now we know how, or are coming to know. When I hear behavioral and social scientists talking in the psychoanalytic or learning theory language of the 1930s, I am reminded of something I once heard Erik Erikson say in a lecture (1967): "The trouble with followers is that they repeat what the leader said fifty years ago and they think they are following him, but they are not following him any more." In his essay, "On Narcissism," Freud said that "we must recollect that all our provisional ideas in psychology will presumably some day be based on an organic substructure." He was a neurologist first, and he started from that substructure (or from what was known of it then). One of his first forays into behavioral science was the Project for a Scientific Psychology of 1895, a neurological theory of behavior. He later gave up this effort, wisely estimating that in his lifetime not enough would be known to make it prosper. But we are now addressing a new generation of students, born a century after Freud's birth, who will write *their* "projects" in 1995. I believe that if Freud, or, for that matter, Clark Hull (1943) were alive today, they would think in units of neurons and behavioral molecules, not cathexes and S-R connections.

A substantive instance of research will illustrate the paradigm. I choose an example from my own research. Lorna Marshall (1960, 1976) had described !Kung San (Bushman) infant and child care, in a general way, as highly indulgent. Richard Lee (1979), Irvven DeVore (Lee and DeVore 1976), and Patricia Draper (1976) confirmed this, so I decided to study !Kung infancy in greater detail. (Lee and DeVore, among others, made this possible.) In the course of a two-year study of infant growth and development among the !Kung, which I conducted in 1969-1971 (Blurton Jones and Konner 1973, Konner 1972a, 1972b, 1973, 1975, 1976, 1977, 1979, 1980, Konner and Worthman 1980, West and Konner 1976), I observed a pattern of nursing that was striking in several respects. First, age at weaning was typically later than three years. All infants under one year of age were nursing, as were 90 percent of those in the second year and 75 percent of those in the third year (Konner 1976). Second, virtually all mothers of children younger than three years of age reported that their children awoke to nurse one or (usually) more times during the night (of the overall sample of nursing mother-child pairs). Third, and most striking to me, was the frequency of daytime nursing. Nursing sessions were brief--a few seconds to a few minutes--and frequent. At all ages under two years (older children were not studied by this method), fewer than 25 percent of fifteen-minute observations of the mother-infant pair elapsed without a nursing session.

From ecological studies by Richard Lee (1979) and demographic studies by Nancy Howell (1979), it was also known that the !Kung population had unusually long birth-spacing--as high as forty-four months in traditional bands. For a noncontracepting, nonabstinent population, this seemed a noteworthy interval between live births, and it resulted in an overall low natural fertility of 4.7 live births per woman. The nutritional hypothesis of infertility seemed one possible explanation of this long interbirth interval, but not a sufficient one; studies of !Kung diet and nutritional status by Lee, Trusswell, Hansen and others (see Lee and DeVore 1976); see also Gaulin and Konner 1977, Howell 1979, Wilmsen 1978) suggested that mild seasonal caloric undernutrition was the only form of malnutrition endemic in this population.

It seemed ressonable to hypothesize that the unusual temporal pattern of nursing throughout early childhood might help to account for the long interbirth interval. An extensive experimental and clinical literature shows that prolactin is promptly secreted in response to nipple stimulation in human females, increasing twofold to twentyfold in plasma during five to fifteen minutes of mechanical stimulation, with a half-life in plasma of ten to thirty minutes. Prolactin suppresses gonadal function, either directly at the ovary or indirectly through gonadotropin antagonism at the anterior pituitary. There has even been an in vitro demonstration of prolactin suppression of progesterone secretion from cultured ovarian granulosa cells. (See Konner and Worthman 1980, for references and for further details on the study described below.)

This, and other evidence, suggested the possibility that temporal patterning of nursing was a key variable mediating the influence of nursing on fertility. Briefly, in populations such as our own, where the interval between nursing sessions is an order of magnitude higher than the half-life of prolactin in plasma, there is little reason to expect effective suppression of ovarian secretion; but in a population such as the !Kung, with intervals between nursing sessions shorter than the half-life of prolactin in plasma, effective suppression might occur.

We tested this hypothesis among the !Kung in a return field study in which nursing behavior and the gonadal hormones

progesterone and estradiol-17β were measured in seventeen mother-infant pairs, with infants ranging in age from 12 to 139 weeks (mean: 64 weeks.). For each mother-infant pair, six hours of nursing observations (at standard times on three separate days) and two maternal blood samples (at 10 A.M. on different days) were collected. In a related study, the monthly ovarian cycle was followed in eight women with normal cycles. Both estradio-17β and progesterone were significantly lower in the twelve noncycling nursing women than in the eight cycling women during follicular phase, when both hormones were at their usual low ebb. For the former group, the mean hormone values (E_2, 24.7 pg/ml; P, 186 pg/ml) were comparable to those found in the hyperprolactinemic subgroup of amenorrheic Western women.

More interesting, a product-moment correlation matrix for the nursing sample showed that levels of the two hormones related significantly to infant's age, and more highly to the mean interval between nursing sessions (for E_2, $r = .67$, $p < .01$; for P, $r = .71$, $p < .01$; two-tail), but not to total nursing time or mean length of nursing session. That is, even within this frequently nursing sample, interval between nursing sessions predicts levels of ovarian hormones in plasma, giving confirmation to the hypothesis beyond that provided by the overall profound suppression seen in the sample. Of many variables of nursing pattern that might be involved in suppression, this study points to interval between nursing sessions as a crucial one.

Although causal inferences cannot be finally made from correlations, our present working model holds that the key change as the infant grows is the lengthening of the interval between nursing sessions. Late in the second year, the child's play occasions longer separations from the mother. When the child is between two and three years of age, the level of prolactin, which presumably has been tonically high previously, is allowed to fall low enough for a long enough time so that its antigonadal or antigonadotrophic effects are impaired, and ovarian cycling is reinstated. Subsequent pregnancy could be further postponed by other effects of suckling, such as erratic or anovulatory cycles, with short luteal phases or otherwise impaired luteal competence and, conceivably, interference with implantation, either by prolactin or by suckling-induced oxytocin release. After the end of lactation amenorrhea, such effects, together with some nutritional infertility and some fetal wastage (estimated to be quite low in this population) could lengthen the birth interval to more than three years. We believe that this solves the puzzle of !Kung birth-spacing.

However, there are other possible implications of this nursing pattern apart from those evident for fertility. For example, Marjorie Shostak (personal communication) interviewed !Kung women extensively; almost all asserted that they experience this suckling pattern as physically pleasant, in spite of its obvious freedom-limiting effects. It is not beyond the realm of possibility that the women's pleasant subjective experience is linked to altered levels of hormones in plasma such as those described above--they may, in some sense, be "drugged" by the nursing pattern--in addition to the reinforcing effects of pain reduction produced by breast emptying.

There are, of course, many potential implications for the infant as well. For example, Nicholas Blurton Jones (1972) has noted that all higher primates have frequent nursing, and that human milk has a similar distribution of chemical constituents to that of these, and other, frequent feeders, while the makeup of milk of spaced-feeders is different. Judith Wurtman (personal communication) has shown that spacing of feeding influences the composition of laboratory rats' milk on a short-term basis. She has also analyzed !Kung milk and found it high in fat and low in the amino acid tryptophan, although higher in tryptophan than is the milk of Guatemalan women on a corn diet. Richard Wurtman and John Fernstrom (1974a, b; 1975) showed that amount of dietary tryptophan alters the level of a neurotransmitter, serotonin, which the brain makes from tryptophan. Loy Lytle (Lytle *et al.* 1975) subsequently showed that dietary tryptophan affects pain sensitivity by changing brain serotonin--the first demonstration of an immediate effect of diet on behavior mediated by known dimensions of brain function. It was also shown that any large carbohydrate-containing meal elevates brain serotonin by an indirect metabolic effect (Fernstrom and Wurtman 1972). Spaced feedings of long duration in American infants might fall into this latter category.

Brain serotonin level has known relations not only to pain sensitivity, but to sleep and waking, depression, and other behavioral dimensions. Because of the different pattern of meal size and tryptophan content for !Kung infants, I (who had gone to the field armed only with the Whiting and Whiting model of culture, child training, and behavior, plus a few vague ideas about adaptation) now had to prepare myself for the possibility that some features of !Kung infant behavior and development, which I observed, differed from the American pattern because of this and other causes. It did not seem likely, but it seemed possible.

Meanwhile, the infant is, of course, growing. This does not mean merely "getting larger" or even "changing shape," but also changing and increasing in behavioral competence. It now seems undeniable that the transformation of competence in early infancy is largely the result of brain growth--again, not mere growth in size but differential maturation of the functional characteristics of specialized neural systems. Much about mother-infant relations may now be understood by reference to such maturation. To take a trivial example, it is only because of the complex state of organization of the oral reflexes at the time of birth--rooting, sucking, stripping with the tongue, and swallowing, all while continuing to breathe--that the !Kung can choose to make nursing such a central feature of mother-infant relations. In the middle of the first year, the regulation of the timing of nursing will gradually shift from the mother to the infant, due to the maturational emergence of the visually directed grasp. Finally, maturation of the infant's social, cognitive, and motor capacity in the second and third years will draw the infant away from the mother often enough and long enough so that the infant's control of the mother's neuroendocrine balance will pass away, and she will become fertile again. The latter process will eventually produce an end to the first child's infancy in the form of a younger sibling. It is now possible to account for much of the causation of these developing capacities by reference to specific and universal maturational changes in the human infant's brain, given no more than present-day knowledge of neural structure and function.

There is space only to mention other physiological ramifications: for the infant, colic and other feeding difficulties, oral gratification from sucking, and level and dynamics of blood glucose, with their possible implications for Sudden Infant Death (see Konner and Super in press); for the mother, success of milk flow and chance of breast infection and injury, as well as attitude toward nursing and likelihood of ovulation and implantation--all these and more may be affected. We come full circle back to a human custom, a cultural choice--frequent suckling of infants--by way of the frontiers of neuroendocrinology, neurochemistry, and neuroanatomy, which we would scarcely have thought had anything to do with it.

III

Let me anticipate a few questions.

What does this have to do with anthropology? Under the leadership of Sherwood Washburn (Washburn and Dolhinow 1972), Ralph Holloway (1979), and Jane Lancaster (1968), neuroanatomy, particularly the neocortical basis of language and other higher functions, has become part of the New Physical Anthropology. Holloway (1966) also studied environmental enrichment effects on brain anatomy. Younger physical anthropologists are studying annual and monthly rhythms of hormone levels and effects of early nutrition on neurocognitive growth in free and captive monkeys. They are also giving attention to the effects of nutritionally induced hormone changes on human reproductive capacity. In psychological anthropology, John Whiting has long been interested in the physiological effects of early stress and early handling as studied in the laboratory, and has used such effects in explaining some findings in his cross-cultural analyses (Gunders and Whiting 1968, Landauer and Whiting 1964, Whiting 1974). Arthur Wolf and William Lambert (1976) have studied the effects of child training in a Taiwanese village on children's epinephrine and norepinephrine levels, as well as the relation of these to aggressive acts by the children. Anthony Wallace (1961:275) has discussed a hypocalcemia theory of the Arctic psychosis *pibloktoq* and has made numerous other explorations on the interface between psychological anthropology and behavioral physiology (Wallace 1959, 1969, 1970). David Hamburg, a distinguished psychiatrist with a long-standing interest in biobehavioral anthropology, has made many contributions on the same interface (e.g., Hamburg 1963, Hamburg and McCown 1979). More recently, work has been done on blood glucose and aggression and on stress and gastrointestinal physiology in various cultural contexts. The youngest generation of physical anthropologists includes a number who are doing excellent work that incorporates brain science into evolutionary and developmental studies (Gibson 1977, Steklis and Raleigh 1979). These several neurobiological threads of physical and psychological anthropology may now be drawn together to great advantage.

How does this differ from physiological psychology, ethology, and biological psychiatry? None of these is doing, or is likely to do, the task proposed for the new paradigm. The classical and still major concerns of physiological psychology are sensation, perception, learning, memory, and such well-studied motivational systems as feeding and drinking. The new paradigm will focus on the classical concerns of several branches of anthropology: language, aggression, sexual behavior, parental behavior, kinship, early experience effects, and sex and population differences. Ethology, at present, is only weakly concerned with either physiology or human beings. Biological psychiatry has perhaps the greatest affinity with the new paradigm, and in some senses, the new paradigm may be said to serve the same function in undergraduate education that is served by biological psychiatry in medical school. The latter, however, has poor communication with cross-cultural psychiatry, with research on training and environmental effects, and with notions like adaptation and evolution. Above all, it is mainly concerned with abnormal behavior and with clinical problems, rather than with normal human behavior in its full, natural range.

How can anthropologists learn all this, in addition to everything else they have to learn? They cannot, of course; not in addition. As always, it is a matter of choices. I send my students to neuroanatomy and neurochemistry courses rather than to osteology and genetics; the intrinsic level of difficulty is no different. As for the sacred classical concerns of physical anthropology, we have seen them so transformed in the last generation that I do not see what harm it can do to transform them further. Flexibility, not tradition, seems to be the hallmark of modern science. Courses such as the ones I have described can serve to start biological anthropology students on the arduous road to neurobiological competence and at the same time give social anthropologists some of what they need to know to talk to neurobiologists.

Does this not encourage racism, sexism, and other forms of biological determinism? Every advance in science is a two-edged sword, an evil power if wielded in evil hands. Know-nothingism and antiscientism in the ranks of the good is just what the evil hope to see. Since it can do no more than delay the advance of science, it leaves the forces of good in total disarray, once the advance is made; and evil can use it to cut a wide swath. Biological determinism is a grim specter, but, as the psychiatrist Merton Kahne (1976) has said, "There is one encouraging thing about it. It's wrong." If anthropologists learn some biology, it can be fought on its own ground. It cannot be effectively fought on any other.

IV

Now to that "enduring crucial cleavage" within anthropology and its interests.

When I was in college, Franz Boas was sometimes ridiculed for his lack of theory. Without theory, it was said, the ethnographer was a little boat without sail or rudder tossed on a stormy sea. Witness Boas's poor grasp of Northwest coast kinship, while he recorded five traditional recipes for blueberry pie, in the original Kwakiutl (but, see Hoebel and Eggan, Chapters 1 and 3, respectively, this volume).

I believe he knew very well what he was about, for he had both sail and rudder in a turn of mind, which I call "the ethnological sensibility." Personally, I look forward to the opening, one day, of a Native American restaurant in New York, and I intend to try all five kinds of blueberry pie. That is what I mean by the ethnological sensibility. It is the taste of the culture of nonindustrial civilizations. Theory cannot replace, guide, or even aid it. It has nothing to do with theory, or, for that matter, with measurement, system, cause, effect or process. It has a delicate life of its own, which all these endanger.

To me, the ethnological sensibility is the single most important contribution of anthropology to human intellectual life. I believe we must take strong steps to protect it, like a beautiful but weak and declining species in the face of technological advance. Modern social anthropology, ethnoscience, and "The New Archaeology" are as much a threat to it as is biology. The task required is that of bringing students and others into human confrontation with the creative life of nonindustrial peoples. Structurally, it is the same task as is done in Departments of Classics, English and Art History. The ethnological sensibility is impeded, not aided, by science and scientists, who--except as amateurs--know nothing of it.

I have only an amateur's interest in it. I have no solution, but I feel the problem keenly. I do not see most social and cultural anthropologists addressing it. Perhaps there should be a separate Department of Nontechnical Civilizations, in the Humanities Division, in which students confront, through film and the written word, the art, literature, language, music, dance and ritual of nonindustrial peoples, not to dissect them as scientists but to contemplate them as scholars--or better still, as people. I do not think such things as Lévi-Strauss's *Mythologiques*, admirable as they are, serve this function. One is either discovering universal principles of the human mind, or being deeply moved and lifted by a beautiful mythic story which has moved a group of people living, say, in the Orinoco basin, for a thousand generations. I do not see how one can do both at once.

V

I must write some reluctant words about sociobiology. Reluctant, because it is exhausting to defend something peripheral to one's main interest before a group of sensitive people who probably detest it. Nevertheless, discussions of the role of biology in behavior seem inevitably nowadays to result in misunderstandings caused by strong feelings about sociobiology. As a nonsociobiologist who has taken an interest in it, I feel a certain responsibility to state my position. I hope that this will help to avert the usual misunderstandings.

First, let me say that sociobiology is very narrow, compared with behavioral biology, and very wide, compared with *Sociobiology*--a fat textbook with wonderful pictures, written by E.O. Wilson. The glossary of that book defines behavioral biology as "the scientific study of all aspects of behavior, including neurophysiology, ethology, comparative psychology, sociobiology, and behavioral ecology." The field which I have described and called Human Behavioral Biology has a similar range, but with a tight focus on humankind. In consequence, it has a regard for culture, unknown in the study of termites. Sociobiology (small *s*) is a part--a small part--of the behavioral biology paradigm. It concerns the study of animal social behavior from the vantage of ecological, population, and natural selection theory. As for the distinction of sociobiology from *Sociobiology*, many sociobiologists of note, including G.C. Williams, Irven DeVore and Robert Trivers, find whole large sections of Wilson's book (for example, the group selection chapter) completely unacceptable. I admire the book and find it useful and beautiful, but it is beginning to feel like an albatross with 700 double-columned wings.

Second, neither sociobiology, nor *Sociobiology*, constitute the scientific triumph their advocates make of them, nor the political threat their critics see in them, from the viewpoint of the study of human behavior. In 1971, I returned innocently from a two-year study of !Kung infancy to find Harvard in a state of turmoil over sociobiology, then called simply *natural selection theory*. I reacted in three stages over the course of about two years: (1) "This is the most obnoxious pack of nonsense I ever heard." (2) "I think these characters may have something." (3) "All right, I believe it. Enough already, can't we move on to something else?" What I moved on to was a postdoctoral fellowship in neurobiology, which resulted in the research and teaching program I have described. I felt far from the turmoil, until Wilson's book was published; then the turmoil widened until it engulfed me again.

I have given, I think, careful attention to many critiques of the book and the larger approach. I have felt the sympathy that comes of having spent a year or two criticizing it vigorously myself. I listened to Marshall Sahlins talk about it for six hours--in effect a dramatic reading of his book, *The Use and Abuse of Biology* (1976). His grasp of the basic scientific issues, however, is so poor that discussion of his critique is really impossible. I write this sadly because I admire Sahlins' other work and wish he would get on with it instead of dwelling on this issue. Marvin Harris (1979) and Sherwood Washburn (Chapter 22, this volume), who do understand some of the substantive issues, seem to be to be pointing to specific errors. They are right to do so, but their corrections will only result in a more mature and precise sociobiology. Indeed, one might say that these two scientific leaders have been doing sociobiology within anthropology for many years.

Similar things can be said of other critiques. On the whole, they are trenchant but minor, or else sweeping but either irrelevant or inept, and they are destined to be incorporated into sociobiology, or to pass away.

However, the survival of sociobiology does not mean that it will swallow up anthropology or any other neighboring academic fields, as predicted by Wilson in his remarkably silly first chapter (for a mellower statement, see his *On Human Nature*, which also takes a more flexible stance on the subject of human sociobiology than did the last chapter of the 1975 volume). We have had astronomy at least since Stonehenge, and people are still watching the sky for a living (although with better instruments than big rocks) in spite of theoretical revolutions in the physics of movement and light which have transformed astrophysical thinking from generation to generation. Wilson himself, in his mellower moments, points to the revolution in cellular chemistry of a hundred years ago, which chemists thought would swallow up histology. He notes that a century of biochemical progress has not made a dent in the problems that faced eighteenth-century cell morphologists, and the latter still earn a good living by looking at cells through microscopes and describing what they see.

Similarly, anthropologists will be carrying cameras and notebooks into the field and describing what people do and say as long as there are people. Theoretical advances, like transformations of behavior and customs of the subjects themselves, far from obviating this effort of description, will render it ever more crucial and more satisfying.

VI

Among many missteps in his presentation, Sahlins makes one I would not have expected of him: a misinterpretation of Lévi-Strauss. Attempting to wield the master's authority in combat, he hurts himself with it, as follows.

He correctly cites the early chapters of *Les structures élémentaires de la parenté* as a decisive exposition of the nature culture distinction, and believes he has destroyed Wilson's theorizing by doing so. He seems unaware of Lévi-Strauss's argument with Sartre, in the last chapter of *La pensée sauvage*:

> And I count as an aesthete since Sartre applies this term to anyone purporting to study men as if they were ants. But apart from the fact that this seems to me just the attitude of any scientist who is an agnostic, there is nothing very compromising about it, for ants with their artificial tunnels, their social life and their chemical messages, already present a sufficiently tough resistance to the enterprises of analytical reason.... So I accept

> the characterization of aesthete in so far as I believe the ultimate goal of the human sciences to be not to constitute, but to dissolve man. The pre-eminent value of anthropology is that it represents the first step in a procedure which involves others. Ethnographic analysis tries to arrive at invariants beyond the empirical diversity of human societies; and, as the present work shows, these are sometimes to be found at the most unforseen points. Rousseau foresaw this with his usual acumen: "One needs to look near at hand in order to study men; but to study man must learn to look from afar...." However, it would not be enough to reabsorb particular humanities into a general one. This first enterprise opens the way for others which Rousseau would not have been so ready to accept and which are incumbent on the exact natural sciences: the reintegration of culture in nature and finally of life within the whole of its physico-chemical conditions (Lévi-Strauss 1966:247).

I could hardly make a better summary of my own point of view in this paper, or of Wilson's. What a pity for Professor Sahlins that Sartre and Lévi-Strauss should have chosen as their example the very ants Wilson has spent his life studying and which gave rise in the first place to his sweeping, incisive theory. As if to put salt on an open wound, Lévi-Strauss footnotes this passage, saying,

> The opposition between nature and culture to which I attached much importance at one time (Elementary structures, ch. 1 and 2) now seems to be of primarily methodological importance.

Maurice Godelier, Lévi-Strauss's heir apparent in French social anthropology, seems to agree. He has written a paper called "Anthropology and Biology: Toward a New Form of Cooperation," which serves as the perfect complement to this one (Godelier 1975). It proposes the kind of social anthropology which I, as a biological anthropologist, would like to see us reaching for.

In natural science, yesterday's revolution is either today's amusement or today's ruling paradigm, and tomorrow's dull history or textbook fact. In social science, yesterday's revolution is today's armed camp--and tomorrow's, and tomorrow's--whose soldiers remain entrenched and unresponsive to challenges, except for those summoning them to indecisive skirmishes. This sort of stalemate can end, if we are willing to have the same open-mindedness about human beings that other scientists have about other natural phenomena.