2. Students wishing to pursue this subject further may wish to consult Rowell (1972), Jolly (1972), van Lawick-Goodall (1971). The last presents an interesting first-person account of the author's studies of chimpanzees in the Gombe Stream Reserve.

3. K. R. L. Hall, (1963) makes an extensive review of these materials.

4. There is some indication of displacement in the dance of the bees, but it involves an entirely different evolutionary sequence from that found in the development of communication systems among primates.

5. Oh's Profit, a novel by John Goulet (1975) tells the story of Oh, a gorilla with a complex personality, who has been taught sign language, and of the quarrels this successful experiment produces among linguists and psychologists with competing theories. For a nontechnical review of a large number of studies, see Emily Hahn's book, Look Who's Talking (1978).

6. In a brilliant book, the paleontologist S. J. Gould has reviewed the long history of the idea of a relationship between the development of the individual organism and that of life forms. He demonstrates the significance for modern evolutionary biology of the "changes in developmental timing that produce parallels between the stages of ontogeny and phylogeny" (Gould 1977:2). These changes allow him to reconcile continuity and this continuity in evolution and would account, for example, for both the similarities and the differences between human beings and chimpanzees.

7. Sets of the Files are located at member universities, and microcard versions of the Files are available at subscribing institutions. The topical headings were set out in An Outline of Cultural Materials (Murdock et al. 1965) and cultures are listed in Outline of World Cultures (Murdock 1963). Coding of data on 863 societies is to be found in the Ethnographic Atlas (Murdock 1967), and these data have been analyzed by Bourguignon and Greenbaum (1973). Discussions of the cross-cultural statistical method are to be found in the Handbook of Methods in Cultural Anthropology, edited by R. Naroll and R. Cohen (1970). In 1978, HRAF published a Guide to Social Theory: World-Wide Cross-Cultural Tests. This massive work presents a cross-indexed methodological analysis of some 1375 propositions tested in 300 holocultural studies.

8. G. P. Murdock was greatly influenced by his teacher, the sociologist Albert Keller, and through him, by W. G. Sumner. Tylor, at a time when statistics was in its infancy, had attempted the first correlational study. (See F. W. Moore 1961 for some of the precursors of holocultural research).

INTRODUCTION: WHEN SOUTH MEETS MIDWEST

After spending two years in Georgia, my colleague Professor Donald has recently returned to the Midwest. He was glad to be back, he said, because life in the South had made him uncomfortable. People there deal with business contracts as if they were carrying on personal relationships. Even a trip to the bank involves exchanges of pleasantries and personal inquiries instead of being limited to straightforward business transactions. "They are very insincere," he concluded.

Another thing they taught was that nobody was ridiculous or bad or disgusting. Shortly before my father died, he said to me, "You know—you never wrote a story with a villain in it."

I told him that was one of the things I learned in college after the war (Kurt Vonnegut, Jr., in Slaughterhouse-Five).
The disagreement between Professor Donald and Mary Ellen could not have been more striking. What one approved and cherished, the other abhorred. They agreed on one thing only; that there are indeed differences in interpersonal behavior between South and Midwest. They disagreed, however, energetically on their interpretations and evaluations of the behaviors, and they did not use the same criteria in making their judgments. Whereas she looked for "warmth" and "friendliness" in human interactions, he spoke of "sincerity" and "businesslike." What she expected seemed inappropriate to him, and he therefore saw it as "insincere." What he perceived as "businesslike" she felt to be "cold."

The experiences of Professor Donald and Mary Ellen did not fulfill their expectations. As a result, each was frustrated and disoriented. For us to play our own roles effectively, others must live up to the expectations we have of them. When they do not, the pattern is broken. We are apt to interpret and evaluate others in terms of our own local traditions; we may then respond inappropriately and risk applying criteria that are foreign to the situation.

Behavior, expectations, attitudes, and values are not merely situational and temporary but form part of the image we have of ourselves and that we present to others. Therefore they belong in a discussion of personality. To the extent that they are not unique to an individual but are part of the norms of a given population, we can speak of shared personality features. Our examples suggest that Midwestern and Southern personalities differ in significant respects. According to one set of norms, as we have seen, behavior should be "warm" and "friendly," and according to the other it should be "sincere" and "businesslike."

To what extent do individuals actually live up to these norms? We find that there are variations in the degree to which persons comply. Midwesterners might then say that "Mr. Smith is unusually businesslike, for a Southerner," or Southerners might consider Mrs. James is "an unusually friendly person, for a Midwesterner."

Before we examine scientific approaches to culture and personality, we must consider a basic issue: the contrasting views of those who are primarily interested in cultural diversity and those who seek to demonstrate the existence of a universal human nature. These two perspectives have often been in fundamental opposition, yet both are basic to anthropology.

THE RELATIVITY OF CULTURE AND THE UNIVERSALITY OF HUMAN NATURE
Is it possible to reconcile a recognition of the existence of major cultural differences with a view that there is yet an underlying, single, universal human nature? We have already touched on this point in our earlier discussion of cultural evolution. There Leslie White, a foremost evolutionist, was also vehemently opposed to culture-and-personality studies. Even among those who have con-
tributed greatly to the development of psychological anthropology, opposition between a relativist and a universalist view exists.

It is, of course, an easily observed fact that cultures differ widely with regard to a great many matters: they differ concerning standards of behavior in all aspects of life, and they differ, as we have seen, in their interpretations, and apparently in their experience, of the behavioral environment. From this observation, relativists draw important practical implications for child training, mental health, sex roles, and various other matters. On the other hand, universalists stress certain universal constants in "human nature" shared by all members of the species. The various positions we shall examine stress one of these two aspects of the problem, often at the apparent expense of the other.

The classical evolutionists, and the psychologists who were influenced by them, such as Freud and Piaget, were universalists. They dealt with cultural differences by ordering their data within an evolutionary framework, seeing cultural features most at variance with nineteenth century life as "earliest" and most "primitive." Anthropologists of the twentieth century who rejected evolutionary theories attempted to understand cultures as unique, or as specific, historically derived ways of solving common problems faced by all societies. The position formulated by students of Franz Boas, such as Ruth Benedict, Margaret Mead, and Melville J. Herskovits, transformed the observation of variety and relativity into a philosophy of cultural relativism. Relativism denies the possibility of evaluating the practices of other cultures, and makes comparison difficult on any but strictly formal grounds. In psychological anthropology, relativism has had its most important impact on the development of criteria of mental health.

The discovery of the relativity of cultural practices, beliefs, and attitudes has had a profound influence not only on anthropology but on all the social sciences. It is one of the major intellectual developments of the twentieth century. One of its results has been to call into question the concept of a universal human nature. For example, we have already mentioned Margaret Mead's study of the adolescence of Samoan girls, which cast doubt on the universal existence of a period of stress and conflict as a concomitant of the biological transformations of puberty. If the stresses were due to cultural factors, and these factors were variable, what other aspects of the human nature we believed in were, in fact, merely aspects of our own culture? What did it mean when people said, to explain someone's behavior, that "it's only human nature" to take advantage of others, to wish to better oneself, or to be competitive, or, when they said that wars could not be abolished because "it's human nature" to fight?

Introductory college courses in the social sciences began to teach that "human nature" is a meaningless term, because behavior, attitudes, and values are culturally variable. But is this conclusion valid? Is human personality infinitely malleable? Are there no limits to cultural variation? Are there not perhaps certain "built-in" requirements, basic needs and drives that must be satisfied at the peril of psychological as well as physical disaster? If so, can we not attempt to evaluate human cultures with regard to the degree to which they achieve the satisfaction of human wants, and conversely, and perhaps more easily, with regard to the degree of stress to which they submit human psychological and biological functioning? We have become increasingly aware of the impossibility of separating the biological and the psychological aspects of human existence; psychological stresses are reflected in physical ailments. The basic questions about human nature to be answered, open a perspective that must be confronted in a world that gave rise to German concentration camps and the Russian prison camps of the Gulag Archipelago.

Knowledge of cultures outside Western civilization has long been used to question the certainties of that civilization. In the eighteenth century, travel literature reporting true or imaginary adventures among foreign peoples was used to challenge familiar institutions and values. Swift did so with Gulliver's Travels, Montesquieu with his Persian Letters, and Diderot with his spurious Supplement to the Voyages of Bougainville to the South Seas. These reports raised doubts about the institutionalized constants of individual and social life.

To question the immutability of society is a revolutionary act; it implies that observations of alien ways of life may shed some light on our own. The differences between human groups are not so radical that we cannot recognize ourselves as we are, or as we might be, in others. Unless we draw this conclusion, we will find ourselves arguing that others are less than human, like the proponents of slavery who argued that Africans had no souls.

The development of psychology raised further questions. Psychology proceeded from the assumption that human psychological functioning was amenable to scientific study, as was all else in nature. Scientific study meant the search for regularities, for laws of nature that were valid universally. We find this assumption of the existence and discoverability of universal regularities in behavior equally in the laboratory work of Wundt and in the clinical work of Freud.

It is psychoanalysis, the personality theory of Freud, that gave a major impetus to culture-and-personality research in anthropology and, in a number of modified forms, gave it its major theoretical orientation. Even those who have taken other theoretical positions have often done so in order to disprove some aspect of psychoanalytic theory. The history of culture and personality may thus be seen as a dialogue of sorts between cultural anthropology and psychoanalytic personality theory.¹

As we saw in Chapter 2, Freud was interested in anthropology in the early years of the century, and anthropologists have criticized him in large measure for the anthropological theories he incorporated into his work. A psychology of "primitive" societies remained to be constructed on the basis of better comparative data and of what, at each period, has been perceived to be a more adequate anthropology. It could then also utilize Freud's theoretical constructs, without much concern about his specific views on primitive society.

Psychoanalysis grew out of Freud's work with neurotic patients. It is both a therapeutic method and a theory of personality, which changed and evolved in the course of his lifetime and which has been further developed by his successors.
It is primarily as a dynamic theory of personality that it has been of relevance to culture and personality. As such, it is first of all rooted in human biology. Therefore, insofar as human biology is constant without reference to culture, the laws of human psychology may be assumed to be constant also. In other words, psychoanalysis denies the dichotomy of mind and body that has so profoundly affected Western thought for such a long time. Secondly, a personality is held to be the result of the individual's life history.

Freud delineated a series of maturational stages of psychosexual development. These stages correspond both to the physical and emotional development of the child and to the disciplines imposed on it. An individual who encounters distortions and trauma at one stage of development will develop corresponding personality distortions. It is part of the therapeutic process to uncover the infantile sources of adult disturbances, be they character disorders, neuroses, or psychoses.

It is largely under the influence of Freud's formulation of the stages of infantile development and their significance for personality development that anthropologists have paid considerable attention to child-care practices in their studies of culture and personality. There has been interest in discovering whether the stages are universal; specifically, it has been asked whether the Oedipus complex occurs in all societies or whether it is a function of a particular social organization. The universality of a latency period, prior to puberty, also has been questioned. Another aspect of psychoanalysis taken up by anthropologists deals with the consequences of child care for adult personality. This issue has involved both direct studies of individuals and the study of cultural products, such as myths, religion, and other institutions. In part, this approach was already initiated by Freud and some of his students, when they saw an analogy between dreams and myths (for example, Abraham 1913), and when Freud likened the institutions of primitive societies, the rituals and taboos of the Australian aborigines, to the expressions of neurosis in European patients. With regard to field research, psychoanalytic practice has suggested the significance of acquiring psychological data by collecting dreams together with the dreamer's associations, life history, and so forth. As we shall see, some problems have been encountered in operationalizing psychoanalytic theory, that is, in setting up clear-cut tests of the theoretical propositions.

Whereas anthropology has been heavily influenced by psychoanalysis, anthropological in its turn has influenced psychoanalysis in this country, particularly in the 1930s and 1940s. Certain Neo-Freudians, such as Karen Horney, Erik H. Erikson, and Erich Fromm, have been interested in reducing the emphasis Freud placed on the physiological basis of psychological development and in allowing room for social and cultural factors.

A number of anthropologists have attempted to construct a perspective that brings together both an awareness of cultural diversity and a conception of a universal human nature. Hallowell, as we have seen, viewed a human psychological level of adaptation as an emergent of human evolution. He stressed the universal existence of extrinsic symbolic processes, of cumulative learning, and of the ability to transcend learning. He pointed to the fact that every human social order is a moral order with a normative orientation (although specific norms are culturally variable), with reflexivity (the individual's self-awareness and self-judgment) and with the ego defense mechanisms posited by Freud, which permit compromise between individual drives and social demands. He posited the construction of a culturally constituted behavioral environment, so that the "reality" to which individuals and societies adapt is, in fact, a variable reality.

Melford Spiro (1954, 1978) also has sought to define a universal human nature while recognizing cultural variability. In his second analysis of the problem, he concludes that there is much greater constancy than he had envisaged earlier, and he describes how he arrived at his conclusion on the basis of fieldwork in four drastically different societies: among Chippewa Indians, on Ifaluk atoll in Micronesia, in a kibbutz in Israel, and in Burma. Spiro defines personality as "a system of cognitive, perceptual, motivational and affective dispositions underlying behavior" (Spiro 1978:351). This definition leads him to question the simple identification of cultural and behavioral variability with personality variability. Instead he points out:

however much societies may differ, they all must cope with man's common biological features, especially his prolonged infantile dependency; the adaptively viable means for coping with the latter condition exhibit common social and cultural features across a narrow range of social and cultural variability; these common biological, social, and cultural features are a set of constants which, in their interaction, produce a universal human nature (Spiro 1978:355; italics added).

This position does not mean that culture and cultural diversity may be neglected in our investigation of human nature. Rather, Spiro's approach requires that personality, as a system, be investigated separately from culture and behavior. Group differences in behavior tell us about cultural differences, but not about differences in personality, as he defines it, nor about the relativity of human nature.

We shall see how this dialogue between the constant and the variable, the universal and the relative, appears again and again in the context of specific investigations of numerous cultural groups. Note that Spiro's 1978 statement comes much later than his own earlier, more relativistic views, and that our review of the work of culture-and-personality anthropologists will now turn back to an earlier era.

THREE APPROACHES TO THE INTERACTION BETWEEN CULTURE AND PERSONALITY

The existence of differences among cultural groups seems to be self-evident to all who have ever come into contact with individuals outside their native community. It is precisely the "otherness" of outsiders that makes them "aliens." Yet, al-
The Configurational Approach

Ruth Benedict's epoch-making book, Patterns of Culture, was first published in 1934 and has been reprinted many times since. Benedict selected three culture areas for her analysis: the Pueblos of New Mexico, as illustrated principally by the Zuni; the Dobuans of Melanesia; and the Northwest Coast of North America, centering on the Kwakiutl. In addition, the culture of the Pueblos is contrasted with that of the Plains Indians. The analysis of Zuni culture is based primarily on Benedict's own fieldwork among these people, the Dobuan data were furnished by Reo Fortune, and those on the Kwakiutl by the work of Franz Boas. Each culture is identified by a dominant configuration, and for this purpose two key terms—Dionysian and Apollonian—are taken from Nietzsche's analysis of Greek tragedy. The third term, Paranoid, is taken from psychiatry. The Dionysian seeks ecstasy, and the "illuminations of frenzy." The Apollonian, to the contrary, distrusts all extremes and "does not meddle with disruptive psychological states" (Benedict 1961 [orig. 1934]:79). The Pueblos are Apollonian; the Kwakiutl and their neighbors are Dionysian, as are the Plains Indians. The Dobuans are Paranoid, fearing and hating each other. The Kwakiutl, in addition to their Dionysian propensities, show traits of megalomania, an aspect of paranoia.

Benedict has been much criticized for these labels. Mead, in her preface to later editions of the book, notes:

She was building not typology; she held no belief that Nietzschean or psychiatric labels were suitable for all societies. Nor did she believe that any closed system could be constructed into which all human societies, past, present, and future, would fit. Rather, she was committed to a picture of developing human cultures for which no limit could be set because the possible combinations were so many and so varied as to be inexhaustible (Mead, in Benedict 1961:viii).

Patterns of Culture was Benedict's attempt to bring order into the vast "diversity of custom." Something more than a catalog of that diversity was required: the significance of cultural behavior is not exhausted when we have clearly understood that it is local and man-made and hugely variable. It tends to be integrated. A culture, like an individual, is a more or less consistent pattern of thought and action. Within each culture there come into being characteristic purposes not necessarily shared by other types of society. In obedience to these purposes, each people further and further consolidates its experience and in proportion to the urgency of these drives the heterogeneous items of behavior take more and more congruous shape . . . We can understand this only by understanding first the emotional and intellectual mainsprings of that society (Benedict 1961 [orig. 1934]:46; italics added).

Notice, here, the likening of culture to "an individual." Like individuals, cultures are said to have "purposes"; the question of how these characteristic and distinctive purposes come into being is not investigated. The need for congruity and order is seen as a "drive," and the purposes a society has are said to be its "emotional and intellectual mainsprings." They are characterized in the terms we have already cited: Apollonian, Dionysian, and Paranoid or following Spengler for Western Civilization, "Faustian."

Culture then, is isomorphic with individual personality, and it can be discussed by means of studying the personality of its bearers. Conversely, the personality pattern that typifies individuals is also the one that typifies a culture. The analysis therefore can shift back and forth between these two concepts with ease, particularly because culture is said to "mold" personality:

If we are interested in human behavior we need first of all to understand the institutions that are provided in any society. For human behavior will take the forms those institutions suggest, even to extremes of which the observer, deep-dyed in the culture of which he is a part, can have no intimation (Benedict 1961 [orig. 1934]:236).

In a sense, then, culture is built into human individuals, and it is discovered, or some parts of it are, by studying individual behavior. Yet individuals do not appear in this book at all. Cultural forms are illustrated through reports of individual acts, but there is no assessment or presentation of individual personalities. The end product is necessarily a picture of fairly homogeneous communities, although the existence of deviants is recognized. Deviation and adaptation are related to the concept of "temperament," which, however, is never made clear. Because culture and personality are in fact one and the same thing in this presentation, separate methods of studying each are not necessary, nor is the evaluation of a "fit" between them.

Benedict holds that the particular patterns that characterize a given culture are the result of "selection": each culture actualizes only some part of the human potential. However, she presents no theory of why some aspects of this human potential are selected for actualization by a given culture, rather than others. Although there is emphasis, as we have seen in Benedict's statement, on the integration of culture, she also recognizes that some cultures are better integrated, such as the Zuni, or less well integrated, such as the Kwakiutl. The possibility
that selection might in some way be related to ecological and technological factors, or that the degree of integration might be related to culture change, are not considered. Such ideas have received greater emphasis only more recently.

Essentially, Benedict's approach, as represented here in its first major expression and as later developed both in her own work and in that of Margaret Mead, stresses cultural—and therefore psychological—variability. Cultures are "understood" by revealing their patterning—the way various parts of the total picture fit together. "Understanding" does not involve a search for causes or a discovery of correlations, a testing of hypotheses or even an unravelling of historical sequences. The principal problem to be solved in this approach, in addition to a discovery of the pattern, is the way in which culture (and thus typical personality) is transmitted from one generation to the next. The pattern is seen in all aspects of culture, and in the culture of childhood as well as in that of adults. Cross-cultural comparisons are used primarily to show incomparability.

Benedict's approach has been criticized on the one hand by those who reject a psychological approach altogether, such as Leslie White and Marvin Harris. It has also been criticized by those who search for a fit between personality on the individual level and the cultural demands resulting, for example, from the adaptation of a society to its particular ecological niche, and by those who search for causes or test hypotheses concerning cross-cultural regularities. As Robert Le-Vine remarks with regard to the work of both Benedict and Mead: "We are asked to forfeit the aim of explaining cross-cultural differences in personality in favor of simply appreciating them" (LeVine 1973:55).

On another level, Benedict's choice of labels also has been questioned. In the remarks quoted earlier, Mead argues that this criticism is based on a misunderstanding, and that Benedict was not seeking to establish a finite typology. Two other points deserve attention in this connection.

First, both the Plains Indians and the Kwakiutl are identified as Dionysian. Here is some of the evidence Benedict offers to show that in both areas there was a seeking after ecstasy and transcendence. In the Plains, men sought visions with "hideous tortures":

They cut strips from the skin of their arms, they struck off fingers, they swung themselves from tall poles by straps inserted under the muscles of their shoulders. They went without food and water for extreme periods. They sought in every way to achieve an order of experience set apart from daily living (Benedict 1961:81).

The purpose of the Plains Indian vision quest was to receive power from the spirits in a private, personal encounter. It was a "cultural mechanism which gives a theoretically unlimited freedom to the individual . . . It gave individual initiative a scope which is not easily equalled" (Benedict 1961:83).

The context in which the Kwakiutl on the Northwest Coast sought ecstasy was different. Here the altered states of consciousness were part of the rituals and ceremonies of secret societies, into which persons were initiated, presumably, by the spirit patrons of the societies. In the Cannibal Society, the period of initiation involved several months of seclusion and "a demonstration of frenzy" during the ritual of the initiate's return:

The whole Winter Ceremonial, the great Kwakiutl series of religious rites, was given to "tame" the initiate who returned full of "the power that destroys man's reason" and whom it was necessary to bring back to the level of secular existence (Benedict 1961:177).

There was, then, a striving for ecstasy, but the context of the Kwakiutl experience and its personal and social significance were quite different. Among the Plains Indians it was individual power that was sought and a supernatural support for personal initiative and independence. Among the Kwakiutl, the initiate was received into membership in a secret society, the Cannibal Society having highest prestige and rank. Indeed, whereas the Plains Indians lived in equilibrarian societies of buffalo hunters and raiders, the Kwakiutl lived in a hierarchically organized society of settled villages. The people were divided into nobles, commoners, and slaves, and there were great differences in wealth, prestige, and power. The Plains Indians, to use the terms employed by Barry and his associates (1959), lived primarily by hunting and had a low accumulation subsistence economy. The Kwakiutl, who were sedentary salmon fishermen, by contrast, had a high accumulation economy.

Both types of societies may be considered to be Dionysian, in Benedict's classification, yet with regard to just about any other aspect one might wish to choose, these two cultural areas represent drastic contrasts. As a result, we see that even the Dionysian aspects play different roles in the total cultural fabric and have different personal meanings.

A second problem in labeling is that by using the terms Apollonian and Dionysian as a contrasting pair of labels for different cultural types, Benedict appears to suggest that a single cultural emphasis or personality type could be used to characterize a total culture. She implies a high degree of cultural and psychological homogeneity in a society. Nietzsche, who coined the terms, had actually used both of them in his analysis of Greek tragedy; he found both patterns to be present in the single culture of classical Greece.

Whether a single configuration, or pattern, can be said to characterize all the elements that make up a given culture, or whether at least some cultures might not be more complex, is ultimately an empirical question. Benedict herself presented a more complex picture of a culture a dozen years later in The Chrysanthemum and the Sword (1946), her wartime study of Japanese culture. The problem, here, is not primarily the matter of labels, but rather the basis on which a typology is established, what is assumed to begin with, and what is discovered in the process of investigation.

With regard to labels, a third, more serious problem was raised by the use of terms, such as "paranoid" and "megalomania," taken from the language of
psychiatry. Benedict's concern was to show that behavior that might resemble that of pathological individuals in Western society might be institutionalized in other cultures, and therefore be "normal." Thus, like everything else about human behavior, the "normal" and the "abnormal" were to be understood as culturally relative. Unfortunately, the language of psychopathology came to "taint" the culture-and-personality enterprise. In fact, it was, in part, used in the psychological warfare of World War II. In the contemporary period, when the people studied by anthropologists read the books written about them, when anthropologists have been tarred with the brush of "colonialism," and when much scientific work has been politicized, the overtones carried by the psychiatric terminology have, at times, been taken to be insults and political exploitation.

All of this criticism was far in the future when Benedict published Patterns of Culture in 1934. Indeed, by stressing relativism, she intended to strike a blow for tolerance and to express a hope for harmony among human societies. Curiously, the extent of criticism to which this book has been subjected is itself one measure of its great impact within the anthropological profession, as well as among a wider readership.

Criticism of quite a different sort has turned on the issue of Benedict's data and of her interpretation of them. John W. Bennett reviewed a large body of writings on the Pueblo Indian cultures of the Southwestern United States. He documents two points of view concerning them in the anthropological literature: On the one hand, he finds, writers like Benedict, see Pueblo culture as highly integrated, with a consistent and harmonious set of values and with an ideal personality "which features the values of gentleness, nonaggression, cooperation, modesty, tranquility, and so on" (Bennett 1956:204). On the other hand, some writers claim that "Pueblo society and culture are marked by considerable covert tension, suspicion, anxiety, hostility, fear and ambition ... the individual is suppressed and repressed. Witchcraft is covert, but highly developed" (Bennett 1956:204; italics in original). Bennett suggests that these two views reflect differences in the writers' values. The first group appears to approve of Pueblo ways of life, not merely to describe them. This group stresses the positive character of some features of Pueblo culture that one might consider to be clearly at variance with modern Western civilization. The second group stresses aspects the first seems to give little attention to: the cost to the individual at which the "harmony" is achieved. The two views appear to be complementary rather than contradictory as far as the information itself is concerned. They do, however, lead to a rather different picture of the Pueblo way of life and of the Pueblo individual. The second group of writers, in particular, looks to ecology as a framework within which the development of Pueblo social organization and child training have taken place.

This disagreement raises several matters of considerable importance. Regarding cultures where several anthropologists have carried out fieldwork, there may be a high degree of agreement on certain aspects of the materials and less on others; there may also be a great deal of difference in the interpretations the data are given. The interpretations, although perhaps not the data themselves, may reflect unstated, and even unconscious, premises and value orientations of the fieldworker. It follows that where we have the report of only one fieldworker, no such check is available to us, and questions concerning the validity and reliability of the picture we are offered must be kept in mind.

In the Pueblo case, not only do we have reports by a series of fieldworkers, but these reports extend over a decade or more—even longer if we include those prior to Benedict’s formulation and the psychologically oriented studies of the thirties. The changes in the field reports therefore reflect not only individual views and values, but also changes in theoretical perspectives within anthropology. For example, researchers like Esther Goldfrank (1945), who see Pueblo culture as repressive of individual development and expression, distinguish conceptually between culture and personality in a way that the configurationists do not, and it is the latter who emphasize the picture of harmony and integration. The concern with ecology, mentioned earlier, also appears to be an example of such a shift in theoretical interest, and this shift is clearer now than at the time of Bennett's writing. Incidentally, most of the studies cited by Bennett appeared after Patterns of Culture, which gave a strong impetus both to the configurationist view and to contrasting views.

In summing up the configurationist approach initiated by Benedict and further developed by Mead and her coworkers, LeVine argues that it focuses on the concept of culture and reduces "personality to a mere individual reflection of culture, and personality development to the intergenerational transmission of culture" (LeVine 1973:55). Margaret Mead has written extensively on a broad variety of subjects, yet throughout one of her principal concerns has indeed been the subject of cultural transmission.

In summary, several difficulties were encountered in the process of trying to demonstrate and delineate group differences in personality. These difficulties have to do with the conceptualization of the problem on the one hand and with the acquisition and analysis of the data on the other. The debate between the protagonists of the configurationist approach to Pueblo culture and their opponents suggests another difficulty, if we are to believe Bennett: anthropologists have often argued that studying alien, "primitive" societies made it possible to be a good deal more "objective" than studying one's own society. Nonetheless, this debate suggests that unadmitted and often unanalyzed biases may creep into the study of a foreign society as well as into the analysis of one's own way of life. The debate also shows the importance of restudy, or of the study of a single society by more than one anthropologist. Likewise the selection of conceptual tools, of hypotheses to be tested, or of concepts to be applied tends to structure our findings. Observers who use different concepts and different methods of analysis and of data acquisition will necessarily produce results that are hardly comparable.

We now turn to students of personality and culture who have treated the field as involving the interaction of two systems, or portions of two systems, rather than as a single system.
Psychoanalytic Reductionism

At about the time Benedict was formulating the configurational approach, another view of the interrelationship between culture and personality was being developed by Géza Róheim, a Hungarian anthropologist trained as a psychoanalyst. In 1928, Róheim was given the opportunity to carry out psychoanalytically oriented fieldwork by Marie Bonaparte, Princess George of Greece, herself a student of Freud. He chose to go to Australia, among other societies, because he regarded the Central Australians as the most primitive human group living, and because information on the Australian aborigines had been basic to Freud's argument in *Totem and Taboo*. Róheim sought to check, in the field, Freud's hypotheses concerning the relation between primitives and neurotics, cultural evolution, and the universality of the developmental stages and psychic mechanisms identified by Freud. He attempted to close the gap that "had made itself felt in psycho-analytic ethnology, since psychoanalysts have never been in a position to gather ethnological observations in the field" (Róheim 1932:2). Among the Aranda, he collected and analyzed folklore and mythology, as well as dreams and the dreamers' associations to their dreams, which he compared to the mythological materials. He studied ceremonies, interviewed individuals, and observed children at play. He also utilized a form of play analysis. His book, *Children of the Desert* (Róheim 1976) was published only long after his death. In it, he described a way of life that has now, for the most part, disappeared.

On the basis of his anthropological and psychoanalytic fieldwork, Róheim developed his "ontogenetic theory of culture." This theory views culture as the result of the human "delayed infancy." It considers the "group character" of a given society as the outcome or response to the infantile trauma typical of that particular society. That is, he holds that in each society there is one particular aspect of the interaction of infants and adults that is especially painful. Róheim recognizes the existence of individual variations in this situation in every society. However, he suggests that variations are smaller in primitive societies than in modern ones.

By "delayed infancy" Róheim means the great length of time during which the human infant, compared to the young of other primate species, is dependent on adult caretakers. During this extended period, the infant develops strong attachments, particularly to the mother or mother substitute. Freud had drawn attention to the existence of infantile sexuality, which had been denied by nineteenth century ideology. Róheim points out that the child's sexual development is premature in relation to both its physical and its emotional development. He argues that the disproportionate rates of development are responsible for the constellation of feelings Freud had discovered and had named the Oedipus complex. Róheim points out that, since the biological facts that he holds responsible for that complex are a human universal, the complex itself must be generically human. He therefore rejects Freud's explanation of the Oedipus complex on the basis of a collective unconscious and on the inheritance of acquired characteris-

tics, for in *Totem and Taboo* Freud had traced the origins of the Oedipus complex to events in the earliest stages of humanity.

Moreover, Róheim notes, Freud had claimed that neuroses were due to various kinds of trauma experienced in early childhood. Since parents in different cultures vary in the ways in which they treat their children—a finding he confirmed through his fieldwork—it follows that each culture is characterized by its own typical "ontogenetic trauma." That is, each culture is based on a particular, specific, childhood trauma, which also accounts for the specific character type to be found in each society. According to Róheim, then, the origin of culture in the evolution of humanity is to be found in infantile trauma, and each specific culture results from a particular trauma. He goes so far as to say that such infantile experiences are more important than the "real conditions" of life:

The Australian native has never undergone the trauma of being weaned. The children will go on sucking as long as they like and will easily get another woman to let them do it if their own mother has no milk... The Australian native lives in an environment that can hardly be called favorable... And yet nobody has ever heard that an Australian native feels anxious about tomorrow's meal. Facts might well justify such an attitude, but there is no basis for it in the libidinal development. With such yielding mothers, we are all heroes (Róheim 1932:78).

Róheim is saying, in effect, that because there is no trauma of weaning for the Australians, even harsh economic conditions are not able to break their faith in being always provided for. In more general terms, then, since the pattern of life is established by infantile experiences, all adult activities are seen as derivative, involving sublimation or acting out on the level of reality (as in economic activities) or on the level of fantasy (as in rituals).

Róheim has been criticized for, in effect, denying adulthood—the existence of rational adaptive strategies—in favor of seeing adult behavior and institutions as based on childhood experience. However, the opposite position, which would deny the irrational component in adult personal and institutional behavior, is certainly also quite off the mark. As we saw in our discussion of the culturally constituted behavioral environment (Chapter 2), "realistic" behavior must be seen in relativistic terms.

Since Róheim wished to test Freud's theories in the field, he was open to the possibility that cultural variability might affect Freud's stages of psychosexual development. In fact, he claims that among the Aranda, the latency period does not exist (Róheim 1932), an observation which had been made earlier by Malinowski with regard to the Trobriand Islanders. However, while Malinowski had claimed that the Oedipus complex as described by Freud did not exist among the matrilineal Trobriand Islanders (Malinowski 1927), Róheim reported the existence of this complex among the Normanby Islanders, who are also a matrilineal group, in many respects culturally similar to the Trobriand Islanders. (We shall return to the Oedipus complex in Chapter 4).
Röheim collected a substantial amount of observational material, in part by innovations in field research. He collected dreams and the dreamers’ associations to them; he observed children at spontaneous play and also set up play situations for them. Although he emphasized the biological basis of psychological development and of personality structure, stressing the universal aspects of human nature, he was willing to admit the possibility of cultural variations. In fact, the whole basis of his ontogenetic theory of culture traces differences in culture to differences in child care.

Röheim’s theory has found little acceptance among anthropologists. For example, F. L. K. Hsu states emphatically that “psychological anthropology is not simply the psychology of the individual, and it must shun psychoanalysis of whole cultures in the manner of some of Freud’s disciples” (Hsu 1972a:10). Röheim has also been criticized for not allowing room in his theory for cultural change, although he argues for cultural evolution. Because adults are, in effect, condemned to act on the basis of their own childhood trauma when dealing with their children, there appears to be a vicious circle: personality results from childhood trauma, and the same trauma is perpetuated in the next generation by the parents as a result of their own personalities.

This brief discussion has emphasized some of the difficulties Röheim’s contribution has encountered. However, in spite of these strictures, it contains many valuable insights and observations, and represents a significant element in the development of psychological anthropology.

The Personality Mediation Approach

The next important step in the development of theories about the relationship between culture and personality came from the collaboration between a psychoanalyst, Abram Kardiner, and several anthropologists, in particular Ralph Linton and Cora Du Bois. LeVine (1973) called it the “personality mediation view.”

In their first book, The Individual and His Society (Kardiner 1939), the culture of the Marquesas and that of the Tanala of Madagascar are described by Linton and analyzed by Kardiner. A second volume, The Psychological Frontiers of Society (Kardiner 1963), includes three cultural descriptions: Linton describes the Comanche; “James West” (Carl Withers) describes a midwestern American community dubbed Plainville; and Cora Du Bois describes the Alorese. The cultural descriptions in these two volumes were based on research carried on under a variety of circumstances and for diverse purposes. Only Du Bois’ work was specifically designed to provide the cultural and psychological data required for Kardiner’s analysis as it had been developed in his earlier trial formulations. She also presented her materials at much greater length in her book, The People of Alor (Du Bois 1960). This book should be read together with a shorter description and Kardiner’s analysis, both of which are included in the Psychological Frontiers of Society. In addition to obtaining a good deal of general ethnographic materials on which her description of the culture is based, Du Bois also collected eight rather full life histories from both men and women and obtained personality test protocols and children’s drawings. The life history materials and the tests could then be used as a basis for personality analysis, which could be related to the cultural materials, including information on child training and parental attitudes.

Kardiner uses specific information on individuals to derive what he calls a society’s basic personality structure. (Du Bois prefers the term modal personality). This term refers to those characteristics shared by the majority of a group’s members as a result of their common formative experiences. These formative experiences are the result of the childhood disciplines of a society, family organization, and so on, referred to as primary institutions. Certain other aspects of culture are termed secondary institutions: religion, folklore, mythology, art, and so on. Secondary institutions are products of the “projective systems” or “projective screens”. According to this model, a society’s primary institutions create its basic personality structure, which in turn projects or expresses certain psychological content—conflicts, fantasies, and so on—in the form of the society’s secondary institutions. Thus, personality “mediates” between the two types of institutions.

To understand this model, it is necessary to know what Kardiner and Linton mean by “institutions.” As Kardiner puts it (1963:25), “Institutions should be defined to mean what people do, think, believe, or feel. Their locus is within the
human personality; and they have an accommodative or adaptive function.” The primary institutions are taken as givens. Their origin or source is not investigated, nor is a hypothesis advanced to account for their existence: “The origin of an institution has nothing to do with the effect it creates on the growing individual” (Kardiner 1963 [orig. 1945a]:25). Furthermore, the model does not claim to account for the entire culture or social system. Kardiner thinks that practices of “purely rational origin” are likely not to be involved with the basic personality structure, either as cause or effect.

This approach has been criticized on a variety of grounds. Let us consider the matter of the data, first of all. As already mentioned, of all the cultural descriptions included in the two Kardiner volumes, only that of the Alorese by Cora Du Bois draws on fieldwork conducted specifically to provide data for the purpose of this analysis. Sometimes sketchy information is used to construct formidable theoretical edifices. For example, after Linton’s description of the Tanala of Madagascar Kardiner summarizes as follows:

The old Tanala society had as its basis the cultivation of dry rice. This technique permitted a certain type of social organization, based on communal ownership of land ... under the extreme authoritarian rule of the father. The basic needs of the individual ... were completely satisfied notwithstanding ... submission to despotic rule ... When the wet method of rice cultivation was introduced, communal ownership of land had to be abandoned. The individual suddenly became important, and his rights were threatened by the competitive needs of other individuals for the same means of subsistence. This led to the disruption of the whole family organization. This resulted in a great increase in crime, homosexuality, magic, and hysterical illnesses. These social phenomena indicate quite clearly that when the personality, as shaped by the customs suited to the old method of economy, encountered, in the new economy, psychological tasks it was in no way prepared to meet, the result was an enormous outbreak of anxiety (Kardiner 1945b:113).

This analysis is an oversimplification of Linton’s description. In fact, Linton speaks of two neighboring societies, Tanala and Betsileo, of whom he says: “basically we can regard Betsileo as Tanala culture, after all the changes consequent upon wet rice have become consolidated, organized and institutionalized” (Linton, in Kardiner 1939:283).

Linton’s work has been widely quoted as evidence for a relationship among economy, family structure, and personality structure. It is also a hypothesis about the relationship among ecology, economy, and social and political organization. As such it has recently been the subject of a restudy by Aidan Southall, who states that, having worked among both the Betsileo and the Tanala, he undertook this exploration “in a spirit of piety to the ancestors” but that he has “become increasingly uncomfortable as the sense of piety dissipated” (Southall 1975:604). He summarizes Linton’s hypothesis as follows: “He [Linton] assumed, perhaps plausibly but without evidence, that the Betsileo had once been a stateless people occupying a forested plateau, but that by clearing the forest and developing terraced, irrigated rice cultivation, they had also developed kingdoms and a caste structured society. He argued that the Tanala were developing in exactly the same direction in the late 19th century until the process was altered by the French occupation of 1895” (Southall 1975:604).

Because Linton published his ethnography of the Tanala in 1933, evolved the hypothesis in question in 1937, and developed it further in the Kardiner volume in 1939, Southall is able to compare three versions of the materials. He finds that “Linton himself contradicts all the crucial elements of his celebrated hypothesis” (Southall 1975:606; italics in original). He goes on to say: “It almost seems as if, in these later writings, he became so enamoured with an exciting idea that he proceeded to elaborate it without any regard for the facts which he himself had previously recorded” (Southall 1975:607). Finally he concludes:

The moral of the story would seem to be that ecological interpretations of social change should not be applied mechanically without due regard for the facts of history as well as social structure ... Linton’s worst theoretical mistake was to pose two similar sequences of socioeconomic development and political change for two quite dissimilar ecological niches (Southall 1975:608). As far as the psychological implications of the supposed developments are concerned, Southall makes no reference to them at all.

What, then, is left of the Linton-Kardiner analysis of the Tanala-Betsileo? It appears that we can no longer say with Kardiner that Linton has offered us “a remarkable text for the study of the dynamics of social change” (Kardiner 1939:329). We can no longer consider that the Betsileo show us what the Tanala are in the process of becoming. Could we argue, on the other hand, that the Tanala show us what the Betsileo once were? Southall says this idea is “plausible,” but “without evidence.” Perhaps, then, we can use the separate descriptions of the two societies for purposes of comparison, seeking to identify patterns of concomitant variations in both culture and personality. However, Linton provides only cultural data for the two, and it is Kardiner who infers differences in basic personality structure from these descriptions. Kardiner tells us that among the Betsileo, a society with irrigation, permanent villages, and despotic rulership, there is greater anxiety than among the Tanala, where there is dry rice farming, shifting cultivation, and the joint family. He infers this anxiety from Linton’s claim that among the Betsileo there are more forms of spirit possession trance and there is more sorcery than among the Tanala. This last is a curious claim, because we have learned to expect from numerous studies (Hallowell 1940, Kluckhohn 1944, B. B. Whiting 1959, Swanson 1960) that sorcery is more likely in societies where there is no superordinate authority. The Betsileo-Tanala comparison appears to suggest the opposite. As to spirit possession, one would need a good deal more information to be able to draw Kardiner’s and Linton’s rather audacious conclusions.

In sum, Kardiner concludes that the changes or differences in basic personality structure that he infers from the cultural data must be the result of changes or differences in the subsistence economy, because, he says, there are no differences
in the family organization. Yet Linton tells us that while there is a joint family organization among the dry rice Tanala, this organization does not exist or “has broken down” among the wet rice Betsileo! Kardiner also says that the “basic disciplines” (the child-training practices) are the same in the two societies, but the information on which this statement is based is not provided. Given then, that the difference in the subsistence economy is the only one among the primary institutions that he can locate, he suggests that the greater anxiety he finds among the Betsileo, as compared to the Tanala, is due to scarcity. But would one not be justified in expecting the opposite—that irrigation agriculture produces more of a surplus than shifting cultivation?

Although comparison between cultures is a useful technique to shed light on concomitant variations, the approach does not work here because the information we are given is too slight to support such a major edifice of inferences. The description of a basic personality structure for the two societies is drawn from cultural materials; primary and secondary institutions are outlined and a basic personality structure mediating the two is inferred from them. No independent personality data or measures of personality are available.

The People of Alor. Cora Du Bois’ work on the Alorese represents a great step forward. Her fieldwork was specifically designed to provide, for Kardiner’s analysis, a body of detailed psychological materials, together with cultural data providing information on both the “primary” and the “secondary” institutions. The People of Alor (Du Bois 1944) offers a rather full ethnography of childhood, as well as an overall picture of the culture. The book also presents the full text of autobiographies of eight individuals, four men and four women, together with Kardiner’s analyses. Kardiner also provides an overall analytic chapter. In addition, there are summaries of findings based on several psychological tests: the Proteus Maze Test, word associations, children’s free drawings, and the Rorschach test. This last was submitted to a “blind” analysis by the Swiss psychiatrist Emil Oberholzer; he was given none of the cultural information. The children’s drawings were interpreted by Trude Schmidl-Waehner in the same “blind” manner. Both of these experts utilized the formal properties of the materials rather than the content for their analysis. The resulting agreements between Kardiner’s interpretations and those of the other two specialists are remarkable, although, of course, imperfect.

The detailed information Du Bois was able to provide made it possible for Kardiner to formulate models of Alorese basic personality structure at several developmental levels, through adolescence and early adulthood. Kardiner draws psychological conclusions from cultural data, but he also uses the autobiographies to confirm his picture of the basic personality structure.

To understand the great significance and impact of this study, it is important to place it in its historical context. Cora Du Bois herself has done so in her introduction to the paperback edition of The People of Alor (1960). She notes that “In the 1930s anthropology was preoccupied with functionalism, patterns and configurations. Cultural relativism was rampant, to the detriment of careful comparative studies” (Du Bois 1960:xvii). This relativism had a number of implications. One of these was that each society was studied, and presented, as a unique whole, and no parallels were drawn with other cultures. This limitation was serious for theory building, but in their training anthropologists had been exposed to information on many different cultures, so at least implicitly a comparative framework existed for them. However, when they published for a broad public, as Mead and Benedict did with such books as Coming of Age in Samoa and Patterns of Culture, such a framework was not made explicit. The situation was even worse when anthropologists began to collaborate with professionals trained in other disciplines.

This lack of comparative references is evident in Kardiner, in spite of his exposure to anthropology. One need only consider a few passages to become aware of this lack, as well as of a number of inconsistencies. For example, Du Bois tells us: “The Alorese mother has economic responsibilities that make her welcome the birth of a child less cheerfully and care for it less solicitously than she might in another culture” (Du Bois 1960:38). A great deal of emphasis is placed by both Du Bois and Kardiner on the mother’s work and the neglect of the infant, although Du Bois herself notes the lack of quantitative data and urges caution in drawing conclusions. She also notes differences between the care of infants born in the early dry season, when mothers are freer, and infants born at busier times (Du Bois 1960:34). Here, on the other hand, is one of Kardiner’s summaries of this situation:

The influences to which the child was subjected in this society were of a unique character. Owing to the peculiar division of function between males and females, the woman bore the brunt of the vegetable food economy. She worked in the fields all day and could take care of her children only before she went out to the fields and after she returned. Maternal neglect was therefore the rule, and by this is meant that the supportive influences of the mother in establishing the structure of the ego were in default (Kardiner 1945b:115).

What makes Kardiner think that this situation is unique? There are a great many societies in which women carry the principal burden of the subsistence economy, whether horticultural societies (where farming does not involve the plow) or hunting and gathering societies. Besides, Margaret Mead had already published her work on the Mundugumor of New Guinea, among whom women provided most of the food, and who disliked having children, while the men spent most of their time on head-hunting and other “nonproductive” activities (Mead 1935). Furthermore, is there a necessary relationship between the women’s agricultural work and child neglect? Kardiner seems to think so, for here is another version of this situation, as it appears in The Psychological Frontiers of Society:

Poor maternal care in Alor was an accident resulting from the mother’s having to work all day in her fields. The basic institution is that the mother works in the fields all day; the neglect of the children is not institutionalized, although almost universal (Kardiner 1963 [orig. 1945a]:24).
By “not institutionalized” Kardiner appears to mean that there are no sanctions against the good care of children, nor is there a social ideology that requires child neglect. One of the sources of criticism against the Alorese study was based precisely on this point, since a number of anthropologists were able to point to societies where the mothers took their children along to the fields. Du Bois herself says explicitly: “It is not customary for the mother to work with the child on her back, or even near her, as it is in some societies.” (Du Bois 1960:34; italics added)

Kardiner also makes references to the women’s unconscious hatred of the mother and of the maternal role. The male’s frustrations are expressed in an unconscious denial of their dependence on women, and this denial is at least a partial explanation of their elaborate prestige economy, which involves complex financial transactions and other “important” but unproductive activities. It would appear, then, that it is inaccurate to say that women’s economic responsibility causes child neglect; rather, its primary source is the women’s attitudes toward their children. Rohner (1975), incidentally, uses the Alorese as his type case of a society in which parents reject their children.

Kardiner draws a number of conclusions from the Alorese study. For instance, he comments:

A culture which sacrifices adequate maternal care for infants to other interests starts a cycle which never ends. Adequate maternal care, as judged by Alor, means assisting the ego in the early years to the formation of adequate executive capacities to deal both with the outer world and with other members of the community (Kardiner 1963 [orig. 1945a]:253).

He finds the Alorese case distressing and full of implications for Western society:

The whole situation appears all the more absurd in view of the fact that the women do not need to take over the subsistence economy and tend to the fields instead of aiding the child in the formative years. The men do not contribute, save very sporadically, to the subsistence economy and do nothing with their time except run for debts in what seems to us a meaningless although complicated financial system (Kardiner 1963 [orig. 1945a]:254).

This comment is rather disconcerting, for it shows that the anthropologists with whom he had worked over a period of several years had not succeeded in indoctrinating their psychoanalytic collaborator with the then “rampant” relativism of which Du Bois speaks. The fact that the financial system of the Alorese appears “unproductive” and “meaningless” to us, does not, of course, mean that it is not of the highest significance to the Alorese themselves, nor that it does not require analysis in terms of its relation with, and impact on, Alorese social structure, political organization, and ecology. Kardiner perceives it only in a psychological framework, which is interesting, to be sure, but from the point of view of the 1970s, this approach offers us an incomplete understanding of the situation.

Alorese men actually do raise chickens and goats for food; the pigs they raise play an important role in their financial transactions, and although they are ostensibly not raised for food, they are eaten at feasts. The men also hunt for wild pigs and wild rats. Moreover, until the recent past they were involved in warfare and head-hunting.

With reference to a system in which women work in the fields and neglect their children as a result, while men do nothing “useful,” Kardiner adds the following interesting comment:

It can be safely said of the Alorese that they are totally devoid of social insight and have no idea of the effect of the institutions by which they live. However, in that regard, neither are we concerning our culture (Kardiner 1963 [orig. 1945a]:254; italics added).

Moreover, Kardiner is quite aware of his occasionally ethnocentric position, and he notes explicitly that:

all value judgments used in the appraisal of Alor were drawn from our culture . . . There is no need . . . to maintain the pretense of disinterestedness, since there are no absolute values which we can use as a yardstick. However, the value judgments . . . were not based on moral issues but on purely psychological grounds. We can call an adaptation effective or ineffective without lapsing into moralization (Kardiner 1963 [orig. 1945a]:252-253; italics in original).

In the Alor case, for the first time, actual personal data on individuals were available. It was therefore possible to compare the character of individuals as it appears in the life histories and the psychological tests with the basic personality structure derived from the cultural materials provided by the anthropologist.

On this very score, Kardiner has been criticized heavily. Wallace (1970:124-125) for example, is scathing in his juxtaposition of Kardiner’s occasionally inconsistent comments on specific individuals and on the “typical” Alorese personality. However, Kardiner himself appears to be not unaware of some of these discrepancies. Furthermore, he himself raises the question of “whether the concept of basic personality structure is validated by these studies,” that is, by the autobiographies and their analysis. He answers his own question by remarking:

The concept is essentially a check on institutions and not on character. What we see in each of the four men is a highly individual character. Each has some features of the basic personality structure, but each is molded in turn by the specific factors in his individual fate (Kardiner in Du Bois 1960 [orig. 1944]:548; italics in original).

On the whole, there was considerable agreement among the analysis of the Rorschach test data, the children’s drawings, the picture of the Alorese that emerged from the cultural data, and that which Kardiner drew from the autobiographies. There were, however, also some discrepancies. With regard to the
Rorschach, Kardiner makes a most illuminating remark: "The psychologist who operates only within the knowledge of the psychopathological entities found in our society," he says, "has an insurmountable handicap—he is capable of identifying only those entities found in our society." That is to say, as a practicing psychoanalyst, Kardiner was limited in his understanding of the personality structure of people in other societies, such as the Alorese, by his training and experience. The Rorschach test, by contrast, "demonstrates emotional combinations which are not identifiable in the psychopathological entities common in our society.... [It] is an instrument not only for checking conclusions already reached but for discovering new entities inaccessible to other techniques" (Kardiner 1945b:117).

As we have seen in several of these comments, it is evident that Kardiner has in mind, for the most part, a standard of "normal" personality development based on "adequate" care in infancy and childhood. Deviations from this pattern are viewed in terms of pathology. Benedict's use of clinical terminology, as we saw, was coupled with a relativistic view of psychopathology; this is not true of Kardiner, who maintains a constant, although not fully articulated, standard of "normal" and "healthy."

In spite of the interesting developments in theory and the collection of important new data, the collaboration between Kardiner and the anthropologists has been criticized further on various grounds. Some critics have attacked the research methods. One important element in the research design used by Du Bois was the collection of autobiographies. As Du Bois herself points out, the individuals who were willing to collaborate with her in this time-consuming task were not necessarily the most typical or representative of the society. After all, the anthropologist has only limited control over available sources of information. The method whereby the material was collected has also been questioned. The anthropologist inquired about dreams and tried to get free associations to them; the interviews were not structured, and on the whole an attempt was made to make these autobiographical accounts as comparable as possible to the kinds of materials a psychoanalyst might obtain from patients. As several commentators have pointed out (for example, Powdermaker 1945, Ortigues and Ortigues 1966) an unwarranted assumption is involved here: the patient, who seeks a cure, is motivated to cooperate with the therapist, to tell the truth, and not to withhold anything. The informant interviewed by the anthropologist has no such motivations, even if he or she understands the instructions concerning "free associations." We cannot tell, therefore, how much is withheld or presented with the intent of giving a particular kind of picture.

Kardiner's approach has been criticized as "too psychoanalytic" by some social scientists, and as "culturalist" by some psychoanalysts. He himself was critical of the orthodox Freudian views of psychosexual development, and although he makes casual references to the Oedipus complex in the analysis of the Alorese and other data, nowhere does he treat the subject systematically. He uses the language of psychoanalysis without defining his own theoretical position vis-à-vis the concepts involved in his terminology. On the other hand, he himself has expressed criticism of both Freudians such as Géza Róheim and NeoFreudians such as Karen Horney and Erich Fromm.

Most of the criticism of Kardiner's work appeared at the time of the publication of the several studies that we have reviewed, when they stimulated a good deal of discussion and debate. However, specialists working in the same general area have continued to find it necessary to define their own position on both theory and research method by taking Kardiner's work as a significant point of departure.

The comments of Ortigues and Ortigues (1966) are an example. (M. C. Ortigues is a psychoanalyst who works in Dakar, Senegal, with disturbed children from several African ethnic groups.) These authors characterize Kardiner's view as "a very optimistic conception of the effects of education." Commenting on Kardiner's distinction between primary and secondary institutions, they note that in traditional societies the primary institutions (child care) belong to the domain of women and the secondary institutions (religion, mythology) to the domain of men. They suggest that by seeking to explain the public life of a society (the man's world) through the private life of that society (the women's world) one loses the means of understanding the difference between the position of men and of women in the society as a whole. For Ortigues and Ortigues, the essential problem the child must face is to grow up. This task of growing up they hold to be the essence of the Oedipus complex as conceptualized by Freud. We shall return to a discussion of the Oedipus complex and its curious history in Chapters 4, when we deal more systematically with the subject of socialization.

Ortigues and Ortigues bring up two additional points: First, they, like numerous other critics, find Kardiner's model circular. Second, they criticize his concept of the "secondary" institutions as involving "projection." The fact is that Kardiner has transformed Freud's concept of projection, without making that transformation explicit. To Freud, projection is a defense mechanism of the ego in which an individual assigns to others ("projects" onto them) some unconsciously held emotions. A well organized system of projections might be found in patients suffering from paranoia. Kardiner instead implicitly uses the term to describe a social ideology. That is, not unlike Benedict, he treats the society as a whole as having a personality; the secondary institutions may then be considered to be its fantasies. While aspects of the religious system may find support to some extent in the private fantasies of given individuals, they are not over and over created and reinvented by individuals, whose characters actually show a considerable amount of variation. In fact, Oberholzer, in his analysis of Alorese Rorschachs remarked on the great range of variations presented by the test protocols, an observation which tends to weaken somewhat Kardiner's stress on a common basic personality structure.

The View from Psychohistory. The criticism that Kardiner's model is circular has been made repeatedly. Lloyd de Mause (1974) raises this objection not only
against Kardiner, but also against Röheim and against Freud himself. As a psychoanalytically oriented historian, he comments: "That child-rearing practices are the basis for adult personality was proven again and again. Where they originated stumped every psychoanalyst who ever raised the question" (de Mause 1974:2).

De Mause himself presents a "psychogenic theory of history," which argues that "the central force for change in history is neither technology nor economics, but the 'psychogenic' changes in personality occurring because of successive generations of parent-child interactions." Thus, rather than viewing the parents' treatment of their children as a repetition of how they themselves had been treated, as other psychoanalytically inclined writers had done, de Mause hypothesizes "the ability of successive generations of parents to regress to the psychic age of their children and work through the anxieties of that age in a better manner the second time they encounter them than they did during their own childhood" (de Mause 1974:3). The source of the child-training practices of each age, he suggests, is the reduction of the adult anxiety that results from the closing of the psychic distance between adult and child, for "the history of childhood is a series of closer approaches between adult and child, with each closing of psychic distance producing new anxiety." Furthermore, "a society's child-rearing practices... are the very condition for the transmission and development of all other cultural elements" (de Mause 1974:3). Based on these hypotheses, de Mause proposes an "evolution of childhood," from antiquity (infanticide mode) to the present (helping mode).

Kardiner was not unmindful of the charge of circularity in his view of society. Speculating on the matter, he comments: "our researches up to now indicate that institutions change first and that personality changes follow. While not inaccurate, this formulation does not establish the continuity and interrelatedness of institutions and personality; both change each other in a continuous cyclic process" (Kardiner 1963:256). Although this statement suggests a way out it does not fully resolve the problem, particularly since by "institutions" he appears to be referring to child-training practices. In brief, Kardiner appears to be aware of a larger number of problems than he can handle, and his approach, although highly provocative, is not sufficiently systematic.

The Alorese in Comparative Perspective. In spite of its limitations and handicaps, the study of Alorese culture and personality was a landmark event. Its analytic framework produced much fruitful discussion and debate and represented a necessary step forward. Cora Du Bois' rich data remain valuable today for comparative purposes. We have already referred to their use by Rohner (1975) in his important work on the acceptance and rejection of children.

To Kardiner, the Alorese appeared to be "unique" and "peculiar" in both their institutions and their basic personality structure. In the intervening thirty years, a good deal of additional research has been carried out on child training, personality, and social institutions. Attempts have been made at ordering materials for cross-cultural comparison, so that we need no longer look at each society as totally novel. How do the Alorese look to us when we place them in a comparative perspective?

First, let us consider the division of labor. In Alor, women provide the major portion of the subsistence economy, which is based on crops that they plant and harvest. Kardiner says that the women do not need to work, because, after all, the men do not do anything useful. How widespread is subsistence dependence on women's work?

Murdock's Ethnographic Atlas (1967) gives us information on the division of labor by sex for 639 societies in all parts of the world where some form of agriculture is practiced. In 36 percent of these societies, women do the agricultural work alone or do appreciably more than men, and in another 32 percent there is equal participation in agricultural work by both men and women (Bourguignon and Greenbaum 1973, Table 14). That is, women are heavily involved in agricultural work in two-thirds of the societies, and in only one-third do they not participate in it to any significant extent. (Where there is substantial dependence on gathering, women predominate in this work in 78 percent of the societies). On the other hand, women are much less involved in working with animals in animal husbandry, hunting, or even fishing. Consequently, with regard to women's work, the Alorese no longer seem quite so strange.

Second, let us consider the treatment of Alorese children. Kardiner claims that because mothers work in the fields of Alor, the children are neglected. This neglect does not necessarily follow. As we have noted earlier, in many societies women take their children along, whether working in the fields or gathering. Indeed, among gatherers, Rohner tells us specifically that children are accepted, not neglected. Nor does it follow that children are neglected when their mothers do not take them along. In fact, Rohner (1975:114) cites some evidence to suggest that in societies where there is more than one adult caretaker, children are accepted rather than rejected. Among the Alorese the neglect, particularly the inconsistency and unreliability of caretakers, applies not only to mothers but to substitute caretakers as well, be they grandmothers, fathers, or older siblings. On the other hand, neglect, and rejection of children as the typical pattern of a society is not rare. Rohner (1975:187) shows for a sample of forty-two societies that sixteen (or 38 percent) reject their children. For this sample, he finds a high correlation between rejection and emotional instability, a finding that applies well to the Alorese.

Third, let us take a cross-cultural look at adult behavior. Alorese child rearing quite predictably produces adults who are hostile and aggressive and who believe in hostile spirits. Their emotions are shallow and unstable. The men engage in litigations and continuous quarreling. Could it be that that kind of personality is adaptive to both the natural environment and the behavioral environment in which they find themselves? To answer this question, it would be desirable to have a modern ecological study of the area. A number of points might be explored. The area appears to have been under some population pressure at the time of
Du Bois' fieldwork, although the evidence is not clear. The group she studied had been relocated some twenty years before by the Dutch colonial government from a mountain site to one on the valley floor. The social and economic consequences of this relocation, as well as of the cessation of warfare and head-hunting, were not explored. The population of the island is divided into a number of linguistic groups, a situation that tends to reflect social distance between them and may have also been related to earlier intergroup warfare. The hostile character of the Alorese, together with their belief in hostile spirits might suit such a situation quite well.

As we have seen, Kardiner takes the division of labor and the child-rearing practices as given. He sees the secondary institutions as resulting from the basic personality structure, which, in turn, results from the primary institutions, the child-training practices of the society. When we seek to understand the relationship between culture and personality we may, however, formulate our question in somewhat different terms, and ask instead: Given the fact that the Alorese child is born into an ongoing social system, what does it take to be a well adapted, successful individual by Alorese standards? We have seen that, from the point of view of subsistence, the women carry a heavy burden. However, they also have relatively high status, since all agricultural produce is owned by women, and women as well as men inherit fields. Also, at ceremonial feasts food is distributed to the women who take it home for redistribution within their families. From the point of view of the needs of the society, as the Alorese perceive them, the work of the men is even more important: they control the political structure of the society, which involves access to power and prestige. This is a society of self-made men, in which status is not inherited but must be achieved. The financial system of credit, debts, and litigation is directly related to male status-seeking. Thus power, money, marriage alliances, and dealings with supernaturals, as well as head-hunting and warfare in former times, all fall into the male domain. It may well be that in order to be successful in such a system, it is necessary to be aggressive, hostile, deceitful, and without strong emotional attachments.

It is interesting to note that the emphasis on pigs, finances, debts, and achievement—as well as the presence of intergroup conflict and warfare—is widespread in Melanesia and New Guinea. Alor is on the margin of this area, and presents the complex only partially. However, a more thorough comparison between Alor and similar societies in the region might well provide us with a basis for understanding how such a system could have grown up.

The adjectives used to describe Alorese personality tend to be consistently negative: hostile, aggressive, deceitful, shallow, unstable, uncreative, and so on. At least some of them could be phrased more positively: aggressive, enterprising, clever, free of emotional entanglements, and so on. One wonders whether the hostile reaction the Alorese stimulated in analysts and readers in the United States might not be due to the fact that in many respects they appear to represent a caricature of our own prestige structure.

The People of Alor in Retrospect. It is, of course, easy to be critical of Kardiner's contribution from the hindsight provided by more than thirty years of work in the field of culture and personality, much of which would have been difficult without his contribution and that of the anthropologists with whom he collaborated. A different perspective is provided by the psychohistorian R. J. Lifton, who comments: "Kardiner's analyses of anthropological data taught a generation of psychiatrists important empirical lessons" (Lifton, 1971:7).

When her book was republished in paperback in 1960, Cora Du Bois looked back on the whole venture. Her comments are of importance, for they help us put this study into proper perspective. In contrast to the comments by Kardiner, who was impressed by the high degree of agreement between his own analysis and the findings that resulted from several other psychological approaches, she writes:

It is not surprising that the undertaking failed to reveal any very high degree of consistency between the aspects investigated. It is doubtful that any culture is fully integrated in this sense . . . nevertheless, given the theoretical climate of the times, the subsequent analysis of the data stressed, and may have oversimplified, the congruities that did emerge, and largely ignored the incongruities and discrepancies (Du Bois 1960:xviii).

She concludes:

The People of Alor was a partial, preliminary and provocative sally into a broad, significant and inchoate field. Today, the common sense observation that groups of people in different socio-cultural environments differ in the configuration and distribution of personality traits and processes has been given some degree of scientific confirmation. The explanatory dynamics still offer a rich field of research. It is quite probable that man's intrapsychic constitution is less complex and diverse than his sociocultural constructs in terms of institutions, roles, values, and beliefs . . . Human personality variables are in all probability less numerous than the sociocultural variables in which they are expressed (Du Bois 1960:xxvii).

By the time Du Bois was writing these lines, a number of other issues were being raised and some different approaches were coming to the fore. We shall turn to one of these important issues next.

TWO VIEWS OF CULTURE: THE "REPLICATION OF UNIFORMITY" VERSUS THE "ORGANIZATION OF DIVERSITY"

We mentioned earlier A. F. C. Wallace's criticism of Kardiner's attempt to establish a fit between the characters of Alorese individuals, as revealed by their autobiographies, and the basic personality structure of the Alorese deduced from analysis of their primary and secondary institutions. This criticism is placed in
the context of a more general argument. To Wallace, Kardiner's approach is only one of many taken by those who have sought to identify a single personality type for each society. In these studies, which constitute the bulk of the culture-and-personality literature, the investigator's interest is focused on "the extent to which members of a social group, by virtue of their common group identification, behave in the same way, under the same circumstances," notes Wallace. He goes on to say: "Under such circumstances, the society may be regarded as culturally homogeneous and the individuals will be expected to share a uniform nuclear character" (Wallace 1970:22).

The primary interest of these researchers concerns the "mechanisms of socialization by which each generation becomes, culturally and characterologically, a replica of its predecessors" (Wallace 1970:23). Using such an approach, one assumes that the individual's personality will reflect faithfully the culture of the society in which it develops. One also assumes an essentially static situation; the mental model used does not allow for culture change, for it is basically circular. All of the approaches we have discussed so far belong to this category, which Wallace calls the "replication-of-uniformity" approach.

By contrast, Wallace presents another view, which focuses not on the shared aspects of culture and of personality but rather on their diversity. Instead of stressing stability, this view emphasizes the modifications and variations in culture and social organization over time. "Culture," Wallace comments, "shifts in policy from generation to generation with kaleidoscopic variety and is characterized internally not by uniformity, but by diversity of both individuals and groups, many of whom are in continuous and overt conflict in one subsystem and in active cooperation in another" (Wallace 1970:24).

These two contrasting conceptions of culture imply differences in research goals and research strategies. They also involve different concerns with personality. The first group looks at socialization, to find out how personality is molded by culture, how child training creates both personality and certain other aspects of culture, or even how personality causes patterns of child training and all other aspects of culture. Certain assumptions are made, usually more or less Freudian, about what the structure of personality is and how it develops over time. It is assumed that human motives can be known, and behavior is explained in terms of these motives. The second group, on the other hand, which stresses diversity, also emphasizes the predictability of behavior. It is possible, argues Wallace, that the same behavior may flow from different motives, and from different perceptions of a given situation. Personality, in this view, is essentially a "black box"—we do not know what is in it, nor need we know. The only thing that concerns us is to know what it does, how it behaves.

In support of his view, Wallace points to the fact that total homogeneity would render the operation of society impossible. On the contrary, every society consists of individuals occupying a number of different status positions. These statuses, for example, may involve differences of age and sex: children and adults, young adults and elders, men and women, and so on. The positions in a kinship system furnish another instance: father and son, mother and daughter, older brother and younger brother, brother and sister, husband and wife, uncle and nephew, and so on. These statuses involve complementary roles, and by virtue of their positions, people have not only different rights and obligations, but also different perceptions of a given situation. Relationships among individuals, in this view, are based not on shared expectations, but rather on complementary expectations. To the extent that these expectations are realistic, they constitute in effect a social contract. It is this social contract, which we may call culture, that is shared. The motivation of the individuals involved in these transactions is basically irrelevant to the operation of the contract, that is, to the social system.

Wallace is drawn to this position as a result of several research experiences. The first involves his 1952 study, The Modal Personality Structure of the Tuscarora Indians. Here Wallace utilized the Rorschach test on a sample of seventy adult Indians, both men and women, ranging in age from sixteen to seventy-one. At that time, early in his career, Wallace was looking for shared personality characteristics within the population under investigation. However, his work was innovative in two respects: First, he used the Rorschach to discover personality characteristics of the Tuscarora, not to confirm a picture derived from a cultural description and analysis. Second, instead of assuming the existence of shared features, he sought to discover how widespread features actually were.

In previous studies using the Rorschach test, it had been the practice to construct a single test profile for the population sample by averaging the Rorschach scores in each of the several categories into which the test is broken down for purposes of analysis. Instead, Wallace proceeded differently: he counted the frequency distributions of responses in each of the scoring categories and used these distributions to identify a modal type. Just over one-third (or 37.2 percent) of the total number of cases fell within that modal group. Wallace tells us of this group that it consists of

Rorschach records . . . [whose] quantitative aspects cannot be distinguished from one another, at conventional levels of statistical significance, insofar as their relationship to the modal value of the total is concerned (Wallace 1952:78).

It is well known to social psychologists analyzing test results that the larger the number of items one deals with, the smaller the number of responses will be in which there is coincidence of all the items. Also, as G. Spindler (1978b) has noted, the Tuscarora sample is likely to have been made up of individuals at different levels of acculturation. Yet in reacting to the expectations derived from the writings of Kardiner, Wallace notes:

The variability of personality data in general, within the confines of any given society, and of the Rorschach data in particular presented here, cannot be easily discussed as long as one assumes every individual to have been "subjected to exactly the same cultural influences." One can, indeed, describe Tuscarora culture. But, simply because a culture can be described, one need not assume that every individual's forma-
Thus, the question Wallace must deal with is this: if only a limited percentage of a population shares a modal personality, how then can we account for the operation of a social system, the sharing of some values, a certain cultural heritage, and so on?

The second important element in Wallace's thinking relates to another aspect of his work with the Iroquois, of whom the Tuscarora are one group. It has to do with his research into culture change, and more particularly, into a type of radical cultural transformation for which he has coined the term "revitalization movement." It will be remembered that the replication of uniformity model is also an essentially static model. A concept of diversity makes it somewhat easier to deal not only with gradual change over time, but also with dramatic and sudden cultural innovations. We shall return to this question at greater length in Chapter 9.

It should be noted that Wallace's position appears to be sharpened for polemical purposes. No one has really argued for total cultural and psychological homogeneity. The differences between Wallace and the other authors we have discussed are, at least to some extent, differences in emphasis. Likewise, Wallace argues strongly against the reification of culture. This danger, too, is recognized by other investigators. Its appearance may be attributed in large measure to a casual use of language, or if one prefers, to a certain lack of methodological rigor, more than to any real confusion on the issues.

Another point of difference between Wallace and those he criticizes lies in the perceived aims of culture-and-personality research. Wallace has expressed his own views in the following terms:

Culture-and-personality is ... significant in the field of cultural anthropology because it is concerned with certain aspects of the theory of cultural processes ... culture-and-personality is least significant in the monitoring of specific cultures, since a good ethnography permits far more accurate prediction of specific behavior than any national character study. Its raison d'être resides in the fact that it systematically takes account of noncultural data in explaining and predicting cultural phenomena (Wallace 1970:4).

For Wallace, then, culture and personality is a field of anthropology, not of psychology or psychiatry. Personality is not to be explained through an understanding of culture, nor does he wish to study the operation of cultural processes on personality. Rather, it is cultural processes and phenomena that are to be explained and predicted through the use of personality data. The same point is phrased in somewhat different terms in the following lines:

the true function of the culture-and-personality approach in anthropology lies not in its ability to provide ... descriptions of the psychological correlates of culture ... Culture-and-Personality takes the documented facts of cultural evolution, cultural change and cultural diversity as the phenomena to be explained (Wallace 1970:243).

Here Wallace is in clear agreement with Spiro (1961), who also argued in favor of using personality concepts in seeking to understand social structures.

The concept of the "organization of diversity" comes into its own in the study of complex societies, to which we next turn our attention.

STUDIES OF "NATIONAL CHARACTER"

Although the term national character is sometimes treated as synonymous with basic or modal personality, it is customarily used in a particular kind of research. National character studies typically deal with the psychological patterns observed in the populations of complex, large-scale, diversified, modern nation states. Such research has not been limited to anthropologists, but usually has been carried out by interdisciplinary teams, including sociologists, social psychologists, historians, political scientists, and others. The term culture and personality, on the other hand, is specifically identified with the work of anthropologists studying small-scale, traditional societies. These societies may be tribes, so-called "primitive" people, or a single traditional peasant village.

This difference in the scale of the societies studied is important because it implies differences both in research methods and in the theoretical approaches of the investigators. For example, whatever psychological tests, techniques, and concepts may be utilized by students of culture and personality, the basic research involved is rather standard, primary ethnographic fieldwork. It is generally carried out by one or two individuals living in a community for several months or longer, interacting with people face to face. Often these communities have not been studied previously, and as a rule the people are nonliterate, or have been nonliterate until recently. Usually, few written records are available, and those that are may be of an ethnohistorical kind, provided by outsiders like explorers, missionaried, or colonial administrators. Because the group is small, it will be relatively homogeneous, and because little, if any, historical information is available, changes in culture and society over time are not easily discovered. The anthropologist's fieldwork is carried out within a limited time, so the result may well be a study that has the static appearance of a snapshot. And because individuals cannot be observed throughout their lives, it will be assumed that the child-rearing practices observed today are the same as those that gave rise to the adult personalities manifested by the population at the present time. The picture of homogeneity that Wallace complains of, and the circularity in the theoretical models that we have discussed, relate in part to such conditions of research.
Typically, then, culture-and-personality studies are investigations of small-scale, traditional groups, based on a form of ethnographic field research and limited in time depth by the scarcity of written documents.

On the other hand, national character studies deal with large-scale, modern societies that include millions of people. These societies are known to have considerable time depth, and much written documentation of changes over time is available. The people are literate and have written about themselves for a long period of time. For the recent past, there will be elaborate statistical data concerning demography, economics, and other aspects of social life. Ethnographic methods of investigation—a single researcher or a small team studying a single community for several months—under such circumstances could produce only a fragmentary, local picture, which could be generalized to the total society only with difficulty. In particular, such a small community is likely to include only a limited range of status positions and occupations, and it is therefore not fully representative of the society as a whole. James West’s Plainville as analyzed by Kardiner (1963) is an example of such an attempt. For a total study, the methods of history, sociology, demography, political science, social psychology, and even literary analysis are required. Hence the emphasis on interdisciplinary teams. Because such societies are heterogeneous far beyond any single band or village community, field research should involve sampling procedures. Diversity of responses will necessarily be found when interviews are conducted or tests are administered.

One of the major problems to be confronted by researchers into the national character of a large-scale complex society involves the problem of generalizations, which can be easily distorted beyond their justifiable limits into stereotypes. This likelihood increases when researchers see culture as consisting of traits that are shared by all its “carriers,” or of an all-pervading, all-integrating pattern revealed in diverse institutions as well as in the behavior of specific individuals.

National character studies were popular in the United States in the years centering about World War II and the cold war that followed it. It must be remembered that these studies grew up, developed, and were supported in a particular historical, political, and international context. Most of the studies dealt with societies with which the United States was in conflict. Here, then, the methodological problems referred to earlier were compounded by at least two additional difficulties: First, the studies were carried out, implicitly or explicitly, with political motives. The war, and later the cold war, necessarily colored the view that the United States and its allies were able to have of enemy countries and their societies. The very formulation of the problems to be researched was structured by political facts. Part of the problem, from the point of view of the Western democracies, was to explain the psychology of totalitarian societies: How can we understand the mentality that accepts and fosters the development of totalitarianism? During the six-year period before the war, most Germans, by all appearance, were enthusiastic supporters of Hitler. How could that be?
A second difficulty lay in gaining access to information—first about Germany and Japan, later about the Soviet Union and other East European countries. This problem included finding not only written sources but also potential informants for anthropological research. The only informants in the West accessible to the researchers were former members of these societies. This difficulty applied to the World War II studies of Japan, for example, of which the most brilliant and successful was undoubtedly Ruth Benedict's *The Chrysanthemum and the Sword* (1946). For this large-scale, collective research it was necessary to interview Japanese and Japanese Americans living in the United States. It will be remembered that these people were then in a hostile environment; in many cases they had been removed from their homes and placed in relocation camps. Also, many had been away from Japan for a number of years and were incompletely informed about changes that had taken place in that country.

One interesting additional difference between national character studies and traditional anthropological studies lies in the fact that members of modern societies want to read what has been written about them. Benedict's book was soon translated and published in Japan in 1948. In 1953, J. W. Bennett and M. Nagai reviewed the Japanese reaction to the book. By 1952, they reported, it had gone through eight editions and had been reviewed and commented on widely. Bennett and Nagai summarized several scholarly symposia and professional articles devoted to it. They note that "much that Benedict had said was well known in Japanese social science and scholarship, but the organization of the material was unique, the insights unusual, and the fact that the book had been written by an American, a woman, and one who had never visited Japan made it an object of vast and intriguing importance" (Bennett and Nagai 1953:404; italics in original).

Although many critics in the United States attacked the patternist approach exemplified by Benedict for inadequate quantity and interpretation of data, the Japanese commented favorably on this aspect of the study. Also, they liked the use of an intuitive approach in the construction of patterns or configurations. Specifically, the Japanese critics approved of the attempt to draw together data from many different sources to construct a single total configuration.

On the other hand, a series of negative criticisms leveled at this work are of considerable importance. Questions were raised about just who "the Japanese" are that Benedict speaks of: whether many of her statements do not apply only to some specific segment of the society and are unjustifiably generalized to cover the society as a whole. The Japanese focus on the heterogeneity of Japan, whereas Benedict sees it as essentially homogeneous. Furthermore, the critics noted that, since many of Benedict's informants had been reared in Japan at the beginning of the century, the picture refers more accurately to an earlier period than to the 1940s. The Japanese scholars were also critical of Benedict's "reliance on data concerning child rearing for making pattern generalizations at the cultural level," and they argued that "the purely logical or plausible matching of the two cannot yield anything other than imaginative hypotheses" (Bennett and Nagai 1953:409). In other words, they argued for empirical substantiation, rather than imaginative or intuitive interpretations, to confirm the claims concerning the relationship between child rearing and the overall pattern of Japanese culture that Benedict constructs. In conclusion, Bennett and Nagai quote one of the Japanese scholars who offers the following harsh assessment of Benedict's effort:

This book is instructive in the sense that it tells us that the social anthropologist is unable to analyze, single-handedly, the social behavior of a modern society (Minami, cited in Bennett and Nagai 1953:410).

Since World War II, a great many studies of a variety of aspects of Japanese culture and character have been carried out by scholars from the United States and Japan, and a much more complex and diverse picture has emerged. With regard to child rearing, notably the rigid toilet training emphasized by U.S. wartime investigators, there has been only limited confirmation of the picture constructed by Benedict and other anthropologists such as Geoffrey Gorer (1943) and Weston LaBarre (1945). With regard to the investigation of psychocultural factors Japan is now probably the best-studied country in the world, for intensive research work has been carried out there over a period of more than thirty years by both Japanese and foreign scholars. Many of the wartime conclusions have had to be modified as oversimplifications. At the same time, it has been possible to observe the considerable changes that Japanese society and culture have undergone during that period. Moreover, research during this period has been more highly focused, and attempts at constructing an overall configurational synthesis have not been pursued.¹⁶

With regard to the Soviet Union and other East European countries analyzed in the immediate postwar period it has on the whole not been possible to carry out on-the-spot verification of the various hypotheses. This research was conducted by a team at Columbia University, and much of its information is published in a volume edited by Margaret Mead and Rhoda Métraux and aptly entitled *Studies of Culture at a Distance* (1953). The emphasis on selected specific childhood disciplines, such as Geoffrey Gorer's "swaddling hypothesis," created a vigorous debate (Gorer and Rickman 1949). Margaret Mead, who strongly supported Gorer, argued that his position had been grossly misrepresented and caricatured by his critics. In a spirited defense of the national character approach she restated Gorer's hypothesis in the following terms:

The prolonged and very tight swaddling to which infants are subjected in Russian child-rearing practice is one of the means by which Russians communicate to their infants a feeling that strong authority is necessary (Mead 1953:644; italics in original).

She goes on to note that, by taking culture into account as a "mediating variable," this statement

¹⁶Do Group Differences Exist?
insists that when it is Russians (who themselves embody their whole culture) handle their own children (who are in the process of becoming Russians) in a particular way that this way of handling becomes a form of communication between parents and children in which the child learns something the adult has already learned... (Mead 1953:644–645).

In spite of Mead’s vigorous defense of the national character studies of this type, by 1953, when this article was published, they were already on the wane. Both psychological anthropology and the anthropological study of complex societies were moving in other directions. One direction involved cooperative research with scholars in the countries under investigation. As we saw in the case of Japan, research became generally more narrow in scope, and specific subjects were investigated in greater detail.

An important and highly innovative approach in the study of complex civilizations is that of F. L. K. Hsu. Chinese in origin, trained in Great Britain under Malinowski, and working in the United States, Hsu has been impressed with the importance of a comparative perspective. In contrast to most anthropologists, he believes it important that one study not only alien cultures, but also, systematically, one’s home culture. The results of this orientation have been a series of important studies by Hsu on the Chinese (1948, rev. 1972), on Americans in comparison with the Chinese (1953, rev. 1970), on Chinese, Indians, and Americans (1963) and on the Japanese (1975).

Holding that “man’s most important environment is the social environment,” Hsu (1978b:156–157) divides social relations into two aspects: role (or usefulness) and affect (or feeling). While role differentiation increases as society grows more complex, affects are limited in number, including “love, hate, rage, despair, hope, anxiety, forbearance, loyalty, betrayal” (Hsu 1978b:157). Hsu suggests that patterns of affects vary among cultures, and that the basic pattern of affect of each society is likely to persist over thousands of years. Hsu sees differences between various European societies and the United States and between the modern West and ancient Greece as local variations, when contrasted with the great differences between the West and China. Anthropologists should give greater attention to these differences in the ways in which people express feelings and in the factors that produce the various feeling states.

A second point of importance in Hsu’s thinking is the hypothesis of the “dominant kinship dyad.” He suggests that in China the principal relationship is between father and son, and all other social patterns derive from this relationship, whereas in the West the prime relationship is that between husband and wife. The traditional U.S. emphasis on independence and self-reliance is associated with this kinship pattern, whereas the Chinese emphasis on the group and on continuity through time has its root in the father-son relationship.

Hsu speaks of “affective needs” that are developed in the context of the kinship system, and as the individual grows up, in contact with the wider institutions of the society. Human beings, says Hsu, must maintain a Psychosocial Homeostasis (Hsu 1971a); that is, they must maintain the pattern of affective needs acquired in the course of socialization. As a result, major changes in the patterns of social roles, in technology, economy, and political systems, even when these changes are brought about by culture contact or by revolutions, may take place without modifying the basic pattern of affects.

The study of complex societies will increasingly attract the attention of anthropologists, including psychological anthropologists. In this connection it is important to remember that many of the peoples of small societies and village communities who have been the targets of anthropological research in the past are now becoming members of nation states. Many are moving to the cities, while others find that the cities are reaching out to them. The line between simple and complex, traditional and modern, nonliterate and literate societies is less and less evident as the peoples of the world undergo massive transformations in all aspects of their lives. Not only must we study the ongoing changes and their psychocultural implications, but also in our conceptualizations of the problems we must take these new realities into account. Concepts such as Hsu’s “psychosocial homeostasis” and Wallace’s “organization of diversity” may help us to deal with some aspects of our contemporary situation.

SUMMARY

We started this chapter by asking: do group differences exist? We found the answer to be a complex matter. One underlying issue we examined concerns the shifting emphasis on cultural relativity and its relationship to a universal human nature. Another debate we reviewed deals with ways in which culture, personalities, and their interactions have been conceptualized by different students of the subject. In our survey we distinguished three major approaches.

First we examined the configurationist approach of Ruth Benedict and Margaret Mead. Here primary stress is placed on the transmission of culture, which is said to “mold” personality. Applied to complex societies, this approach was utilized in the early national character studies.

Second, we looked at the approach of some psychoanalysts. This approach is most clearly illustrated in the work of Géza Róheim. It sees childhood experience as responsible for the development both of adult personality and of culture. Because psychological factors are interpreted as primary, the approach has been called reductionist.

Third, we discussed the personality mediation approach. Here certain aspects of culture (the childhood disciplines) are said to be “primary” and to give rise to personality, which in turn gives rise to “secondary” aspects of culture such as religion. Personality, in other words, “mediates” between two segments of culture. This model was originally formulated by Abram Kardiner. Although it has been exposed to various criticisms, in modified form it is still influential, affecting both studies of childhood and holocultural studies of various expressive systems.
In much of the research based on these approaches stress was placed on the shared elements of culture and psychology. This uniformity was obviously an oversimplification and distortion. To correct it, an analytic model was required that could take individual variation into account. Wallace’s concept of the organization of diversity does just that.

In the early studies much stress was placed also on the cultural influences on personality formation. In reaction, Wallace and Spiro, among others, began to urge a reorientation of the field. They emphasized the role of psychological factors in maintaining social systems, an understanding of which was seen as the primary goal of cultural anthropology. Hsu’s concern with patterns of affects also emphasizes the psychological dimension, starting, however, with a typical kinship structure derived from the cultural tradition.

For the most part then, we have learned that, over a period of years, there has been much discussion of methods and concepts among cultural anthropologists; that research has consisted of a series of more or less successful attempts and approximations, which have led to transformations of many of the original questions, but not to truly definitive answers. Anthropology, like other social sciences, is continuously exposed to the impact of social and political currents, and some of the theoretical formulations and some of the choices of problems to be studied can only be understood in such a context. Furthermore, in an interdisciplinary field such as psychological anthropology, the growth of related disciplines, in this case the various specialized fields of psychology, has made itself felt significantly. This interdisciplinary effect is, as we shall see, a two-way street.

As for the tangible results of research on group differences in psychological features, we may echo Robert LeVine’s assessment of the situation:

Humans exhibit more behavioral variation from one population to another than any other species, and it seems likely that some of it is stabilized in individual dispositions that are more than immediate situational responses. A growing body of cross-cultural evidence supports this view... But problems of measurement in the psychology of personality... have so far prevented the generation of conclusive evidence (LeVine 1973:11).

NOTES

1. Beginning in the early 1960s, there appeared to be a decline in interest in the possible applications of psychoanalytic theory to anthropological data. More recently, there is evidence of a lively revival of a psychoanalytic orientation.

2. This important subject will be discussed in Chapter 8.

3. Spiro also refers to the lengthy period of infant dependency. This concept is related to S. J. Gould’s discussion of the evolutionary significance of “changes in developmental timing” (Gould 1977). See note 6, Chapter 2.

4. Although neither Linton nor Southall mentions it, there is an intriguing similarity between Linton’s hypothesis, as it took shape over time, and the Marx-Wittfogel model of “oriental despotism,” positing a relationship between irrigation agriculture and the development of the centralized state dominated by an autocratic ruler (Wittfogel 1938, 1955, 1957).

5. A number of anthropologists have attempted to explain the widespread pattern of warfare in New Guinea as due to population pressures resulting in a shortage of land. Divale and Harris (1976) go further and argue that in band and village societies in general warfare is best understood as an attempt to keep population stationary. However, these arguments are not universally accepted among anthropologists, or even among New Guinea specialists. Paul Sillitoe (1977), for example, points out that the essential demographic data on New Guinea are lacking, and he concludes on the basis of a review of available data from that area that only a small number of wars can be accounted for in ecological terms.

6. The concern of this section is with the development of Wallace’s views. Others at about the same time also discovered diversity on American Indian reservations, particularly in acculturation. An important demonstration of this diversity was made by G. D. Spindler (1955) in his study of the Menominee of Wisconsin. The Menominee studies will be taken up in some detail in Chapter 9.

7. There is also an extensive literature of national character studies authored primarily by sociologists and political scientists. For reviews of this literature, see Inkeles (1972) and Inkeles and Levinson (1968). The sociologist Daniel Bell has suggested a number of ways in which national character studies, having fallen on hard times, might be revived and “rehabilitated.” See Bell’s article in Norbeck, Price-Williams, and McCord (1968).

8. Books such as G. DeVos, Socialization for Achievement: Essays on the Cultural Psychology of the Japanese (1973) and T. S. Lebra and W. P. Lebra, eds., Japanese Culture and Behavior (1974) are excellent sources for a review of these newer studies. DeVos has reviewed the twenty-five-year history of his own, generally cooperative, research on Japanese cultural psychology (DeVos 1978).

9. In a paper entitled “Passage to Understanding” (1978b), Hsu has traced the evolution of his thinking in autobiographical terms. Kinship and Culture, edited by Hsu (1971b), brings together articles by a number of authors, describing a series of diverse cultures within the framework of Hsu’s dominant kinship dyad hypothesis. We shall return to the hypothesis of the dominant kinship dyad in Chapter 4.