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A CRITIQUE OF CULTURE-PERSONALITY WRITINGS

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THIS paper is concerned with an analysis and criticism of what have come to be known as "culture and personality" writings, including among others the work of Benedict, Mead, Gorer, Kluckhohn, DuBois, Linton, La Barre, Erikson, and Kardiner. The scholars who have contributed to this movement have a common general orientation although some differences of opinion and emphasis exist.¹ One wing of the movement includes psychoanalytically trained persons like Fromm, Erikson, and Kardiner. Another wing, represented by a

writer like Benedict, places the main emphasis upon descriptions of cultural configurations and personality types, but puts relatively little emphasis upon genetic explanations or on psychoanalytic concepts. Most of the writers fall between the extremes, using a sprinkling of psychoanalytic terminology, sometimes in combination with ideas derived from other areas.

The interdisciplinary nature of this approach is often stressed but it is, in actual fact, sharply limited. For example, the theory and research of most psychologists, social psychologists, and sociologists who are concerned with personality and psychological processes, are virtually unaffected by the culture-personality writings. Conversely, in the latter there is rarely any reference to the research of social psychologists or psychologists other than clinicians and psychiatrists of Freudian persuasion, and almost no references to the writings of foreign psychologists.

The major preoccupations of the culture-personality writers are: (a) the description and psychological characterization of cultural configurations and the delineation of personality types associated with them, and (b) the explanation of given personality types as products of cultural influences and especially of interpersonal relations in early childhood. We shall discuss each of these major interests in turn.

¹ E.g., Linton questions the homogeneity assumption as applied to non-literate cultures, wonders if status roles may not have a basic influence on personality, and stresses the overlapping of personality types between cultures; Hsu repudiates Kardiner's kind of psycho-analytic interpretation; Kardiner himself has some second thoughts about his own scheme; Fromm sharply criticizes Kardiner and the whole infant discipline ideology; Beaglehole attacks Fromm's interpretation of Western man; Kluckhohn attacks Mead's view of American character and also raises a number of critical questions concerning the culture-personality approach in general. See: F. Hsu, *Under the Ancestor's Shadow*, 1948, pp. 12-15; R. Linton in *Culture and Personality* (Viking Fund Publication), 1949, pp. 163-173; A. Kardiner, *ibid.*, pp. 59-73; E. Fromm, *ibid.*, pp. 3-4; Kluckhohn, *ibid.*, pp. 75-92, and his review of Mead's *Keep Your Powder Dry*, *American Anthropologist*, XLV (1943), 622-624; E. Beaglehole, "Character Structure," *Psychiatry*, VII (1944), 158-159.

CULTURAL CONFIGURATION AND MODAL PERSONALITY POINT OF VIEW

The traditional method of ethnology emphasized the exhaustive description of primitive societies with relatively little emphasis upon psychological characterization as such or upon the total configuration or gestalt. The emphasis was rather upon specific modes of behavior in definitely delineated situations and upon the "psychological" features mainly as exhibited in the overt behavior and verbalization of the natives. The change in viewpoint initiated by the culture-personality school is well indicated by Kroeber's² comment: "As late as 1915 the very word 'personality' still carried overtones chiefly of piquancy, unpredictability, intellectual daring. . . ." Influenced by conceptions borrowed from Gestalt psychology and psychoanalysis, and by Sapir's early stress on the need to study the individuals in a society, some ethnologists have attempted to characterize societies in psychological terms as functioning wholes or configurations. The observer seeks to characterize what may be called the "essence" of the culture in psychological terms, i. e., the people's view of the world and of human relations. Such characterization of peoples and nations is not a totally new enterprise. Long before the rise of modern anthropology, writers and scholars attempted the same sort of description of what was called the "genius" or "ethos" of a people. As Kroeber³ notes: "More than eighteen hundred years ago Tacitus gave to posterity one of the masterpieces of this genre in his analysis of German custom and character."

Following logically from this emphasis on cultural configurations is the idea that given cultural configurations have their counterparts in the individuals of each society. Given cultures produce one or more types of personality designated by such terms as "modal personality," "basic personality structure," "character structure," and so on.

In arriving at their characterization of cultures and personality types the investiga-

tors rely upon conventional ethnological techniques and data, but seek to go beyond them by utilizing them in combination with studies of individuals. Much attention is paid to interpersonal relations, childhood training, projective and objective tests, and sometimes even to photographing people in specified situations.

The investigator immerses himself in a given society as far as the barriers of language, time, available informants, and his own personality permit. From the welter of data he arrives at his characterizations through acts of abstraction, selection, and synthesis. Some characterizations are made vicariously, the writer utilizing materials collected by others, supplemented usually by interviews with emigrants.

The investigators do not describe very clearly or in detail how given characterizations are arrived at.⁴ Stress is placed upon offering the reader a mass of data concerning those aspects of behavior which are the focus of the characterizations.

It should be noted that anthropologists often view the culture-personality approach as something in the nature of a fad, although it is generally conceded that it offers interesting and potentially significant knowledge. In terms of total output, culture-personality writings constitute only a small portion of anthropological writings. Current popularity of the point of view is attested by Kroeber⁵ who remarks: "Personality is the slogan of the moment . . . the prospect may look dire to those who are interested in culture as such. But with experience one learns that these waves go much as they come."

The works of the culture-personality

⁴ Cf. Fromm's vagueness on this point when pressed by Bateson (*Culture and Personality*, Viking, pp. 10-11, 1949). This vagueness is characteristic of the whole literature. Linton recognizes this when he says "The Modal Personality" for any society can be established directly and objectively by studying the frequencies of various personality configurations among a society's members. The fact that, to the best of my knowledge, it never has been so established does not invalidate the concept." *Ibid.*, p. 163.

⁵ A. L. Kroeber, "White's View of Culture," *American Anthropologist*, L (1948), 413-414.

² A. L. Kroeber, *Anthropology*, 1948, p. 414.

³ *Ibid.*, p. 317.

writers, widely read outside of academic circles, offer a valuable antidote to provincialism and ethnocentrism. The implications of the cultural relativity principle have not by any means been fully taken into account either by social scientists or by the general public. The point, no doubt, needs to be hammered home as these writers are doing. Their works amply demonstrate the enormous range of variation in the organization of societies and human responses. The criticisms which follow are not intended in any way to detract from this substantial accomplishment.

CRITICISM

Oversimplification and the homogeneity postulate. The attempt to make psychological characterizations of cultures "may be regarded as attempted short-hand translation of the more general patterns of a culture."⁶ This procedure raises questions having to do with selectivity, neglect of inconsistent data, proof of assertions, and the possibility of corroboration by other investigators. No one, of course, questions the existence of gross differences between cultures. The question is rather that of the scientific precision of specific characterizations and the methods of obtaining them.

Anthropologists have questioned the accuracy of the boiling-down process when carried too far. Benedict, for example, was criticized for describing Zuni, Kwakiutl, and Dobu peoples too simply. In short, one notes that the number of questions that are raised concerning any characterization tends to increase with the number of investigators familiar with the society.⁷ The question was

⁶ Kroeber, *op. cit.*, p. 586.

⁷ For examples of criticisms by specialists see: P. Nash, review of Gorer's *Himalayan Village*, *American Anthropologist*, XLIII (1941), 242; R. Thurnwald, review of Mead's *Sex and Temperament in Three Primitive Societies*, *American Anthropologist*, XXXVIII (1936), 666; M. Titiev, review of Joseph and Thompson, *The Hopi Way*, *American Anthropologist*, XLVIII (1946), 430-432; J. Whiting, review of Mead's *The Mountain Arapesh*, *American Anthropologist*, XLII (1940), 161-162. See also Benedict who stresses "shame" in Japanese character and La Barre who does not mention it: R. Benedict, *Chrysanthemum and the Sword*, 1946;

raised whether many non-literate societies might not be characterized more profitably in terms of multiple patterns or "themes." A similar point has been made with respect to the numbers of personality types within given societies. The earlier culture-personality writings often understressed or ignored individuals who did not conform to the personality type assumed as typical of the culture. This explaining-away or ignoring of negative evidence has given way to

. . . the study of the *range* of personalities in a society. . . . Characteristic personality subtypes may develop from the differing situations of the life of persons who play different roles in a given group.⁸

This trend toward studying the "range of personalities" and of multiple themes within a culture, if carried out to its logical limits, implies a radical revision of the original ideas, as we shall show later. It represents a healthy tendency to move toward more limited and specific problems which can be handled by the established techniques of analysis and proof, rather than dealing with the impossible task of handling entire cultures in one fell swoop, as "wholes." A good many of the questions now being raised will no longer be pertinent when the tendency described by Herskovits is carried further. When this is done, however, stricter standards of proof will have to be met, and many other theories besides the neo-Freudian will have to be taken into account. The dangers inherent in gestalt descriptions of societies are graphically brought out by culture-personality efforts to describe complex modern societies. Any social scientist who seeks to characterize a modern nation, even in a whole volume, to say nothing of a few pages, has to handle a host of detailed problems and meet a number of exacting requirements. These are so numerous and so complex that to one not imbued with the culture-personality fervor the task looks impossible. These

W. La Barre, "Some Observations on Character Structure in the Orient: the Japanese," *Psychiatry*, VIII (1945), 319-345.

⁸ M. J. Herskovits, *Man and His Works*, 1948, p. 56.

problems and requirements have to do with such matters as sampling, statistical distributions, regional differences, migration, ethnic differences, social classes, diverse group affiliations, standards, social change, culture conflict, and enormous bodies of literature and historical materials.⁹ One may admire the boldness of the attempts to make broad general characterizations of such peoples as the Americans, Japanese, and Germans, but one must view the results and methods of proof with a generous measure of skepticism. The same strictures apply with even greater force to attempts to characterize Western character and culture in general.

The applications of culture-personality methods to modern societies—especially the American, with which we are reasonably well acquainted—have fared so badly at the hands of competent critics that one wonders along with Bierstedt¹⁰ whether the effect has not been “to stimulate the growth of skepticism concerning the information which anthropologists have given us about non-literate peoples.”

Undoubtedly the heterogeneity of modern nations, as many of the writers themselves have pointed out, offers a considerable obstacle to the application of present configurational methods. It is hoped, however, that after the techniques have been perfected in the study of simpler, more “homogeneous” societies they may be extended successfully to more complex groups. A more fundamental question must, however, be raised concerning the general validity of the homogeneity assumption itself, even as applied to the larger

groupings of non-literate peoples. One suspects, as Bernard¹¹ has said, that too much attention is being paid to “the blond Swede.”

Psychic Entities vs. Behavior. The homogeneity-configuration postulates savor strongly of Aristotelian conceptions of “essence” and “accident.” The “essences” (configuration, basic personality structures) are given high status in the realm of “being,” whereas the behaviors which “express” these essences are of an inferior status. Even though the behavior may vary from one individual to the next, and from one generation to the next, it is thought of as an emanation or manifestation of the same essence. Current recognition of a range of personality types and of multiple configurations within a single society is an effort to deal with negative evidence and deviations often ignored by earlier writers, but the accident-essence framework is still retained since the number of essences is merely increased. The range idea also has the effect of making it doubly difficult, if not impossible, to prove that the generalizations reached are either true or false.

There is a tendency in these investigations to deduce psychic entities from overt behavior in specific situations, and then to explain the overt behavior in terms of these reifications. There is a search for something like the “real inner personality” or “authentic individual” conceived as something apart from behavior. The inner reality thus becomes a force which manifests itself in the behavior from which it is inferred. Linton¹² explicitly states this position:

The nature and even the presence of psychic needs are only to be deduced from the behavior to which they give rise. . . .

Personality will be taken to mean ‘the organized aggregate of psychological processes and states pertaining to the individual.’ [This defini-

⁹ Cf., Kroeber’s criticism of Mead’s a-historical bias, as when she discusses American educational practices of 1930 without reference to an American and European past: A. Kroeber, review of Mead’s *Growing Up in New Guinea*, *American Anthropologist*, XXX (1931), 248-250. Bierstedt recently has pointed out the shortcomings of a non-historical approach in many anthropological writings when applied to the study of complex societies: R. Bierstedt, “The Limitations of Anthropological Methods in Sociology,” *American Journal of Sociology*, LIV (1948), 22-30. One might point out that non-literate societies also have important histories.

¹⁰ Bierstedt, *ibid.*, p. 29.

¹¹ J. Bernard, “Sociological Mirrors for Cultural Anthropologists,” *American Anthropologist*, LI (1949), 675. See also R. Linton, *op. cit.*, p. 172: “But I must say my own experience has made me feel terribly doubtful about pictures of fairly consistent cultures.”

¹² *The Cultural Background of Personality*, 1945, pp. 6, 84, and 26-27.

tion] rules out the overt behavior resulting from the operation of these processes and states, although it is only from such behavior that their nature and even existence can be deduced. . . .

In general, all the individuals who occupy a given position in the structure of a particular society will respond to many situations in very much the same way. . . . Until the psychologist knows what the norms of behavior imposed by a particular society are, and can discount them as indicators of personality, he will be unable to penetrate behind the facade of social conformity and cultural conformity to reach the authentic individual.

What is meant by "authentic individual"? Do not cultural roles and internalized norms connected with them (e. g., sex roles) influence the "authentic individual"?

The search for the "real motives," the "deep inner core," the "authentic individual," conceived as something separate from behavior leads to circularity of proof and immunity to negative evidence.¹³ Thus, if there is no available evidence that ascribed reactions actually take place, it can always be assumed that they are "unconscious" reactions. If the persons seem to have no knowledge of them, or deny the imputed motives, or give other interpretations of their behavior, these objections are easily disposed of by calling them "rationalizations" or by pointing out that, after all, the people are not usually aware of the premises of their culture which as motivations underlie their daily conduct.

A gross example of this procedure is provided by G. Roheim,¹⁴ who argues with regard to knowledge of procreation among primitives, that:

If we see, on the one hand, that the Arunta deny knowing anything of the matter, and on the other that they have beliefs and rites that are only explicable on the assumption that such knowledge exists somewhere and makes itself felt in their psychic system, we shall say that

¹³ The postulation of entities may be legitimate under certain conditions: (a) when existing knowledge and evidence make it necessary and when empirical means of determining the existence of the entity are suggested, or (b) when the postulation of the entity leads to verifiable inferences which cannot be made otherwise.

¹⁴ *Social Anthropology*, 1936, p. 144.

they are unconscious of their own instinctive knowledge of procreation and that the concepts that enter consciousness are symbolic substitutes of a physiological account of the process of procreation.

The above may be dismissed as an extreme psychoanalytic fantasy, but, with some differences, the same technique of calling on unconscious ideas when the evidence fails, or is disputable, is widespread. Thus Benedict¹⁵ in her book on the Japanese says that: "In this task of analysis the court of authority is not necessarily Tanaka San, the Japanese 'anybody.' For Tanaka San does not make his assumptions explicit, and interpretations written for Americans will undoubtedly seem to him unduly labored." Such a procedure allows the interpretive framework of the investigator to persist undisturbed in the face of negative evidence and criticisms, even from intelligent and trained members of the group being characterized.

Trait psychology lends itself very readily to the use of reified psychic elements to explain behavior of which these traits are, in reality, merely names. Thus, when aggressive behavior is explained in terms of a "fund of aggression,"¹⁶ or of a "trait of aggressiveness," this amounts to saying that behavior is aggressive because it is aggressive. These traits are often not self-evident, and at the beginning of his research the investigator often is uncertain of the "meaning" of specific acts. The "meanings" that are finally found are thus the investigator's inferences from behavioral data. The final psychological characterizations often leave this behavioral or situational basis of the inferred psychic elements or traits out of consideration.

Confusion of Fact and Interpretation. The terms that are used in these characterizations are inevitably taken from Western psychological vocabularies, and inevitably lead the reader to think of the people according to the Western models with which he is familiar. A description of the psychological responses of people within the behavioral

¹⁵ *Op. cit.*, p. 17.

¹⁶ Cf. W. La Barre, *op. cit.*

context of the society does not run into the same dangers of unchecked inference. In this regard a remark of Titiev's,¹⁷ a South-west specialist, is pertinent:

Dr. Thompson . . . exhibits an unfortunate tendency to distort various items taken from literature. A girlish pursuit game somewhat comparable to follow-the-leader, is magnified into a faithful portrayal of "the guidance role of the mother and the difficult and centripetal life course of the Hopi girl."

Titiev's criticism may be extended to many culture personality inferences. Thus, whenever it is postulated that a given people have a given trait such as "aggressiveness," "passivity," "withdrawnness," "impulsiveness," as part of their "basic personality structure," it is easy to take the unwarranted step of regarding specific behavior as a manifestation or effect of the given trait. Conclusions of this type are buttressed not so much by evidential proof as by the piling up of illustrations which are unlikely to convince anyone who is not already sold on the underlying ideology.

No one is likely to quarrel seriously with characterizations of a people when these descriptions are couched in objective behavioral terms, as in conventional ethnological accounts. But when ethnologists interpret the "meanings" of behavior in psychological terms, it becomes exceedingly difficult for the reader to separate facts from interpretations. An interesting comment bearing on this point was made by the Murphys¹⁸ in a review of Mead-Bateson's *Balinese Character*. They conclude that "in spite of the photographic record, the study still shows some lack of systematic framework, the lack of sharp distinction between hypotheses and fact."

The extensive use of photographs in the Mead-Bateson book made it possible for the reviewers to question some of the authors' interpretations. The reviewers go on to say that the photographs allow the reader to observe incidents in the backgrounds of the

pictures which raise questions about matters in the foregrounds. They add that "as a device for cultural study this has very important advantages over one which presents data and interpretations so intertwined that they are impossible to handle independently."

The necessity for presenting "data" and "interpretations" separately becomes greater the more remote and inaccessible the culture.¹⁹ The closer a society is to us and the more that is known about it, the easier it becomes to dispute interpretations of it. One wonders what would happen to the various characterizations of psychologically remote societies if the natives, as well as the investigator's own colleagues who happen to have some knowledge of the society, were able to answer back! We know what happened when the "natives" read the Mead and Gorer material on the United States.

Two interesting incidents that bear upon this point may be cited. Herskovits²⁰ writes that "Li, a Chinese anthropologist, whose own physical traits made him inconspicuous among the Indians [Zuni], found them, as people, to be quite different from the picture of themselves they had presented to white students." Li spent a mere two and one-half months of moderately intimate participation in Zuni life—the Zuni being among the most studied and most characterized non-literate peoples in the world. Another relevant case is that of the anthropologist Peter Buck, of Maori descent, who called into question some of the fundamental interpretations of Maori character and culture made by the Beagleholes.²¹

The recent tendencies to present more documentation of conclusions is certainly a step in the right direction since it allows the reader to form some opinions of his own. This documentation usually consists of autobiographies and test results. The utilization

¹⁹ J. Bernard has also noted the ready confusion of fact and interpretation in anthropological writings: "Observation and Generalization in Cultural Anthropology," *American Journal of Sociology*, L (1945), p. 284-291.

²⁰ *Op. cit.*, p. 51.

²¹ See his "Foreword" in E. and P. Beaglehole, *Some Modern Maoris*, 1946.

¹⁷ *Op. cit.*, p. 431.

¹⁸ L. and G. Murphy, *American Anthropologist*, XLV (1943), 615-619.

of these materials has, however, raised additional questions. For example, there is the question of sampling that arises when autobiographies are gathered. Du Bois' study of the Alorese employs this method and illustrates the problem very well. H. Powdermaker²² suggests that the autobiographies do not represent Alorese modal character because Du Bois was apparently able to interview only relatively unsuccessful Alorese, "those who did not approximate the goals of their culture." She also raises the question of the influence of the investigator upon the interview situation. "We know of no society where people will talk about their private inner feelings upon request [and for pay], and in response to questions from a relative stranger at regular periods each day." The use of autobiographical documents is of course desirable but does not in itself prove anything. The critical reader is not convinced that the persons used in obtaining the documents constitute a representative sample, or that the documents cannot be interpreted in a variety of ways.

The claim that projectives and other tests may be used to validate analyses made by other ethnological methods must be qualified by noting that test results are not self-explanatory, but must themselves be interpreted like other data. The tests are certainly useful, but they are not an open-sesame to the truth. All of them were devised and validated by Western investigators operating within the confines of Western culture, and even within that culture their significance is a matter of controversy. This is especially true of the projectives.²³ The discrepancies between Kardiner's²⁴ interpretations of Alorese character and Overholzer's inferences from Rorschach results raise some doubts about the use of projectives in culture-personality research.

²² Review of Du Bois' *The People of Alor*, *American Anthropologist*, XLVII (1945), 155-161. For similar criticism of informants in general see C. Kluckhohn in *Culture and Personality* (Viking), p. 91, and O. Klineberg, *ibid.*, p. 136.

²³ Cf. Murphy, *Personality*, 1949, pp. 663-700.

²⁴ A. Kardiner, *The Psychological Frontiers of Society*, 1945, pp. 240-247.

The use of tests may prove to be misleading by suggesting an illusory precision and definitiveness. This is especially true when the usual statistical precautions are not followed. Thus, in a review of the *Children of the People*, M. Kuhn²⁵ remarks:

. . . a defect is the failure of the researchers, after espousing the use of quantitative methods, to apply even the minimum sampling standards, such as tests of representativeness, adequacy, and statistical significance of difference which are required by these methods.

An idea of the inadequacy of some of the interpretations of the tests may be obtained from the fact that in *The Hopi Way* conclusions about Hopi animism are based on the answers to a single question!²⁶ And this is done in spite of the extensive controversial literature on methods of testing animism in children. As other examples, Powdermaker²⁷ notes that the thirty-seven Alorese who took Du Bois' Rorschach test were unidentified and probably unrepresentative, and Titiev²⁸ questions how the Hopi way, "which is a subtle, complex, and mature outlook on life, can be properly interpreted or clarified on the basis of tests administered to 190 school children, of whom no less than 45 per cent were 10 years of age or younger."

The Operation of Western Biases. The use of projective tests points up one of the fundamental and pervasive weaknesses of many of the interpretations of non-Western peoples; namely, that Western biases must inevitably find expression in the inferences made about the psychological characteristics of given peoples. As R. Benedict²⁹ has said:

No man ever looks at the world with pristine eyes. He sees it edited by a definite set of customs and institutions and ways of thinking. Even in his philosophical probings he cannot go behind these stereotypes; his very concept of the true and the false will still have reference to his particular traditional customs.

²⁵ *American Sociological Review*, XIII (1948), 118. These remarks apply equally well to such a study as *The Hopi Way*.

²⁶ L. Thompson and A. Joseph, *The Hopi Way*, 1944, p. 92.

²⁷ *Op. cit.*

²⁸ *Op. cit.*

²⁹ *Patterns of Culture*, 1934, p. 2.

Herskovits³⁰ makes a similar point. "*Judgments are based on experience, and experience is interpreted by each individual in terms of his own enculturation.*" (Italics his.)

Anthropologists constantly warn their readers against Western biases, and quite rightly. They are generally aware that these biases can, and perhaps must, unwittingly influence their own research. This warning has not been taken into account in anything like its full implications by culture-personality writers. Admittedly the problem of describing non-Western peoples without including one's own biases in the account is a difficult undertaking. One cannot help but feel that many conclusions reached about non-Western character structures and their genesis should have been couched in much more tentative and cautious terms. This is especially relevant to characterizations which seek to get at "inner psychic realities" and their origins.

A comment from Li,³¹ whose short participant-observer residence among the Zuni we have previously mentioned, portrays vividly the culture-personality writer's difficulties:

We find another one-sided statement on . . . the problem of interpretation of Zuni life. Avoidance of leadership in social life is a corollary of the lack of personal feelings in religion. If one is not interested in vision quest . . . what is more natural than the supposition that leadership among men is not desired. But here is just a case in which the premise is correct enough while the conclusion does not necessarily follow. Dr. Benedict reports that a Zuni is afraid of becoming "a leader of his people" lest he should "likely be persecuted for sorcery," and that he would be "only interested in a game that a number can play with even chances" for "an outstanding runner spoils the game." The basic fallacy seems to lie in *the tendency to reason with the logical implications of one's own culture.* [Our italics.] In the competitive Western world where one is brought up to assume that the world is made for his exploitation, and where if one does not push ahead, one is surely pushed

³⁰ *Op. cit.*, p. 63.

³¹ Li, An-che, "Zuni: Some Observations and Queries," *American Anthropologist*, XXXIX (1937), 67-68.

behind, it is certainly logical that lack of personal acquisitiveness implies the denial of leadership.

One of the aspects of anthropological thinking which tends to neutralize the wholesome emphasis on cultural variability and the dangers of ethnocentric bias, is the out-of-hand dismissal of the hypothesis that intellectual processes may vary in different societies and even within different groups within the same society. This is part of the reaction against the writings of some scholars like Levy-Bruhl, who have attempted to give brief, simple characterizations of primitive thought in general. Linton³² perhaps summarizes a fairly usual position when he asserts categorically:

As far as we can ascertain, the intellectual processes themselves are the same for all normal human beings in all times and places. At least individuals who begin with the same premises always seem to arrive at the same conclusions.

Linton³³ has inconsistently assailed his own view by elsewhere describing language as "a tool for thinking" (note the characteristic dualism which separates language behavior from thinking behavior by animistically designating the former as a tool of the latter); and asserting that "concepts which are an integral part of all linguistic forms have a subtle influence upon individuals' ways of thinking. The concepts are even more compulsive because they are totally unconscious." His primary criticism of linguistics appears to be that it has ignored this problem

³² *Cultural Background of Personality*, 1945, pp. 101-102. This statement appears flatly contradictory to the earlier one by Benedict. Mead also confesses that ". . . one serious difficulty confronts the anthropologist. When writing about some strange South Sea culture, there is the persistent difficulty of translating strange native ideas into English, until one wishes passionately that it were possible to describe Samoa in Samoan and Arapesh in the Arapesh language": *Keep Your Powder Dry*, p. 10. For a convincing recent treatment of the great significance of different languages for different modes of reasoning, see D. D. Lee, "Being and Value in a Primitive Culture," *Journal of Philosophy*, XLVI (1949), 401-415.

³³ *The Science of Man in the World Crisis*, 1945, pp. 7-8.

of how linguistic forms condition different ways of thinking.

Since virtually all readers of characterizations of non-literate peoples are themselves Westerners, unacquainted with the peoples in question, there are few competent critics to point out any but the most flagrant instances of the influence of Western "projective systems" on the ethnologists' accounts. It is, for example, relatively easy to detect La Barre's³⁴ wartime pro-democratic feeling in his unsympathetic account of Japanese "compulsive" character, and it is easy to agree with J. Honigmann³⁵ that Kardiner has placed a rather gross evaluation upon Alorese "narrowness" and "unfitness for cooperation"; and one may readily agree with Kroeber's³⁶ statement that Du Bois' characterization of the Alorese:

. . . seems one-sidedly repellent. . . . The appraising observer comes from a culture that values internalization, conscience, reliance, scruple, courage, consistency of feeling and relations, dignity, and achievement, qualities that are under-developed in Alor. Hence the picture is black.

The detection of more subtle biases awaits the scrutiny of other trained observers—especially natives and cultural hybrids—and the development of more objective techniques of evaluation. Thomas and Znaniecki's *Polish Peasant* might be taken as a suggested model in that one of the authors was a native Pole.

Kroeber³⁷, having the Western bias in mind, has suggested that although some of the characterizations of non-Western peoples are undoubtedly partially correct, there is not at present any way of distinguishing what is valid from what reflects merely "personalized reactions." He even suggests that the basic assumptions of culture-personality studies may be unwarranted since "the categories of psychological characterization developed among Occidentals for Occidentals break down, tend to lose their meaning when

applied to Asiatics." He suggests that comparative studies of Western societies may be a necessary preliminary to valid configurational and personality studies of non-Western peoples. D. Haring's³⁸ caution on drawing conclusions about Japanese character might well be extended to all works in this field:

. . . those who do such research should spend years, not months, in Japan. The writer "learned all the answers" in his first year in Japan. The next six years taught him that practically all of those answers were misleading or false. Perhaps another seven years would have indicated the wisdom of saying nothing at all.

DEVELOPMENT OF MODAL OR BASIC PERSONALITY POINT OF VIEW

In culture-personality writings, personality is conceived largely as the product of interpersonal relationships in childhood. Various degrees of emphasis are placed upon different types of experience. The more psychoanalytically-oriented writers, such as Gorer, Roheim, Kardiner, La Barre, and Erikson, stress the earliest years as the most crucial; whereas others, like Thompson, Kluckhohn, Goldfrank, Mead, and Benedict place considerable emphasis upon later experiences. Some of the genetic explanations employ a straight neo-Freudian terminology, and most of them use at least a few psychoanalytic concepts. Virtually the only hypotheses which are generally regarded as worthy of checking are the modified Freudian ones. A. I. Hallowell³⁹ gives the rationale for this tendency:

This problem [personality] could not be appreciated by either anthropologists or students of human psychology until a working hypothesis about the nature of human personality as a structural whole had been developed. Neither academic psychologists nor psychiatrists of a generation ago had much to offer. It is here that psychoanalysis enters the picture.

There has been some recent attention paid

³⁴ *Op. cit.*

³⁵ Review of Kardiner's *Psychological Frontiers of Society, Psychiatry*, VIII (1945), 499.

³⁶ *Anthropology*, pp. 588-589.

³⁷ *Ibid.*, p. 591.

³⁸ D. Haring, *Personal Character and Cultural Milieu*, rev. ed., 1948, p. 406.

³⁹ A. I. Hallowell, "The Rorschach Technique in the Study of Personality and Culture," *American Anthropologist*, XLVII (1945), 196-197.

to the possible applicability of learning theories in this field, but in general the work of social psychologists and the mass of critical material on Freudian concepts are ignored.

CRITICISM

Effects of Infant Experience Are Undemonstrated. The lack of attention to alternative hypotheses and the neglect of criticism and negative evidence concerning various aspects of psychoanalytic theory give the culture-personality writings the characteristics of illustration and documentation of a point of view already assumed to be true. The principal problem merely seems to be to show how the view may be extended to other cultures and perhaps modified in minor ways in the process.

A point of view that looms very large in these writings is the one that emphasizes the predominant character-forming efficacy of the infant disciplines: bowel and bladder training, nursing, weaning, mothering, restraint of motion, punishment, amount and kinds of frustration, and so on. Thus, La Barre⁴⁰ virtually ascribes the main features of Japanese personality to the rigid bowel training of infants; C. Kluckhohn and O. Mowrer⁴¹ state that too precipitous training of the child in weaning, cleanliness, sex taboos, and aggression control lays the groundwork for "obsessive ambition" and "severe competitive behavior" in adults. E. Erickson⁴² carries this type of explanation to an absurd limit:

The Yurok child . . . is weaned early and abruptly, before the full development of the biting stage, and after having been discouraged from feeling too comfortable with his mother. This expulsion may well contribute to the

⁴⁰ *Op. cit.*, especially pp. 328-329. See also for a similar view, G. Gorer, "Themes in Japanese Culture," *Transactions of the New York Academy of Sciences*, II (1943), 106-124. For suggestive negative evidence on this point see M. Sikkema, "Observations on Japanese Early Child Training," *Psychiatry*, X (1947), 423-432.

⁴¹ In J. McV. Hunt (ed.), *Personality and the Behavior Disorders*, vol. I, 1944, p. 93.

⁴² In C. Kluckhohn and H. A. Murray (eds.) *Personality, In Nature, Society, and Culture*, 1948, pp. 188-189.

Yurok character a residue of potential nostalgia which consequently finds its institutionalized form in the Yurok's ability to cry while he prays in order to gain influence over the food-sending powers behind the visible world. . . . The Yurok, in order to be sure of his food supply, feels it necessary to appear hallucinatory, helpless, and nostalgic, and . . . to deny that he has teeth or that his teeth can hurt anybody.

The general unproved assumption lying behind this type of interpretation is expressed as follows by Erickson:⁴³

We hold that a child absorbs through his needy senses the cultural modalities of what happens in, to, and around him long before he is provided with a vocabulary. . . . Adults . . . selectively accelerate and inhibit the sensual maturation of body orifices and surfaces, and they encourage and restrict the gradual expansion of sensory, muscular, and intellectual mastery. In doing so, they systematically though unconsciously establish in the infant's nervous system the basic grammar of their culture's patterns.

H. Orlansky,⁴⁴ in an excellent recent paper, has critically evaluated the data and assertions bearing on the question of the influence of infant care on personality development. He has shown that there is no body of evidence to support assertions like those given above. Some of his main points may be summarized as follows: (a) various writers attribute different and contradictory effects to the same or similar childhood experiences; (b) the alleged influences of given infant disciplines or types of experience on personality have not been proven within our own society, to say nothing of others; (c) the method of "proving" that early infancy is of primary importance is shot through with anthropomorphism and unsupported assumptions; and (d) post-infantile childhood experiences are probably of more vital importance in shaping personality than the pre-lingual ones.

Most psychologists and social scientists agree that there is a special significance attached to first or early learning. There is

⁴³ *Ibid.*, p. 180.

⁴⁴ "Infant Care and Personality," *Psychological Bulletin*, XL (1949), 1-48.

good evidence for this assumption. What we do not know, and are unable to discover from the culture-personality writings, is what precisely it is that is learned in early infancy and what its exact significance may be for later training. As D. O. Hebb⁴⁵ tersely remarks: "In such matters, our ignorance is virtually complete."

Ineffectual Attempts to Salvage Infantile Determination. In an attempt to bring post-infantile experiences into the picture and to salvage remnants of the original doctrine it is commonly asserted (a) if post-infantile experiences tend to reinforce the personality trends established in infancy, then the resulting adult traits will conform to the infantile pattern; however, (b) if later experiences run counter to earlier ones the resulting adult character may be something not predictable from infantile experiences alone.

Thus E. Beaglehole⁴⁶ distinguishes between the "primary character structure" formed in infancy and "secondary character structure" formed later if later experiences do not reinforce the earlier ones. Similarly, Kluckhohn and Mowrer⁴⁷ assert that:

It should be emphasized that, like biological heredity, infant experiences, while placing certain constraints upon personality, give mainly potentialities. . . . Whether these potentialities become actualized or not, or the extent to which they become actualized, depends upon later social and other conditions which structure the individual's experience.

Kardiner⁴⁸ makes the same point when he notes concerning the effects of infantile experiences: "The . . . question that arises is whether these attitudes need remain permanent. They need not, if other factors are introduced into the child's life which would tend to counteract them. However, if they are not counteracted, they tend to continue."

These statements raise serious methodological problems that are not dealt with adequately, if at all, in this literature. A verifi-

⁴⁵ *Organization of Behavior, A Neuropsychological Theory*, 1949, p. 265.

⁴⁶ "Character Structure," *Psychiatry*, VII (1944), 145-162.

⁴⁷ *Op. cit.*, p. 95.

⁴⁸ *Psychological Frontiers of Society*, p. 28.

able theory is one which can be proved to be right, and this implies that conceivably it might be proved wrong by exceptional cases. The latter possibility is not allowed for in the doctrine since, as Kluckhohn and Mowrer⁴⁹ state:

Substantially the same personality trait may be caused by different patterns of childhood experience. . . . The same basic discipline or event in early life may result in quite different personality trends, depending upon the juxtaposition of various other disciplines, the problems which individuals in each particular society have to meet, and, always, the differing biological equipment of different individuals.

Thus, whatever happens, the theory is confirmed in a heads-I-win-tails-you-lose procedure. Orlansky⁵⁰ has made a similar point in speaking of infantile disciplines:

. . . the same childhood experience is arbitrarily read as having one significance for personality formation in one society and the opposite significance in another. . . .

The concept of causation which we are criticizing might be called "proof by juxtaposition." Using this method, culture-personality writers describe two sets of phenomena widely separated in time, and assert a causal relation. The *post hoc* nature of this reasoning is clearly exemplified by Kardiner's⁵¹ own account:

It is well nigh impossible to tell in advance what particular elaborations will take place in a given culture of such a basic pattern. However, once we are told by the Rorschach that certain end results can be identified, it is a relatively easy matter to reconcile them with the more basic traits.

This *post hoc* method apparently does some-

⁴⁹ *Op. cit.*, p. 96.

⁵⁰ *Op. cit.*, p. 27. See also W. D. Wallis, review of Kardiner's *Psychological Frontiers of Society*, *Annals of the American Academy of Political and Social Science*, CCXLII (1945), 200-201.

⁵¹ *Psychological Frontiers of Society*, pp. 245, 250. See also G. Gorer in his *Himalayan Village*, 1948, who though he espouses the doctrine of infantile determination of personality says: "Owing to the very late psychological maturing of the Lepchas it is difficult to speak with confidence of the character of most of the men under thirty and most of the women under twenty" (p. 367).

times have its difficulties, for, as Kardiner⁵² tells us: "I feel somewhat ashamed to confess that some of the main points in Alorese personality did not become clear to me until four years after I originally got to know the material."

Some writers stress not only that culture shapes personality, but also that personality affects culture. Though the latter assertion is not of concern in this paper, it may be noted that the same sort of *post hoc* reasoning is used. Thus Du Bois⁵³ suggests that institutions and child training techniques should be regarded as interdependent variables, and advances the thesis that institutions should be altered indirectly through changes in child-rearing practices.

In an excess of enthusiasm, Gorer⁵⁴ carries the *post hoc* method to an all-time high when he offers twentieth-century urban middle-class fads in child training as the basis for the American form of government established in the eighteenth century.

Anthropomorphism. Culture-personality explanations of the development and fixation of personality in early infancy and childhood are pervaded by anthropomorphism, as Or-lansky has amply shown. The main reasons for this appear to be (a) that little direct study of infants or children is undertaken to determine whether the reactions attributed to them actually occur, and (b) it is assumed that the reaction of infants to a given type of experience "must be" of a certain character without any effort to prove that such is the case, and (c) the dualistic procedure, which postulates psychic "processes and states" as forces or "first causes"⁵⁵ that produce behavior, invites the investigator to attribute motives and reactions which appear reasonable or plausible to him. The following quotation⁵⁶ nicely illustrates the last two of these points:

⁵² *Psychological Frontiers of Society*, p. xvii.

⁵³ Quoted by Kluckhohn and Mowrer, *op. cit.*, p. 281.

⁵⁴ *The American People*, 1948.

⁵⁵ Linton, *Cultural Background of Personality*, p. 10.

⁵⁶ D. Leighton and C. Kluckhohn, *Children of the People*, 1947, pp. 30-31.

To the white child, whose feedings and other routines are rigidly scheduled, the mother or nurse *must appear incalculable*. He finds that there are rules of behavior which are above and beyond his needs or wishes. No matter how hard he cries, he does not get his bottle until the clock says he should. *He must develop a feeling that each individual is alone in life.*

To the Navaho baby, on the other hand, other persons *must appear warmer and more dependable*, for every time he cries, something is done for him. . . . [Our italics.]

What is "Basic"? Everyone will agree that persons in adult life change occupations, learn new skills, change their status, and so on. It will be admitted that such changes involve personality alterations of some kind. What objective grounds are there for stating that such changes are or are not "basic"?

The idea that basic personality patterns are established in the first couple of years of life or in pre-adolescent childhood involves the assumption that personality does not change, or changes only in minor ways, in response to later experiences and cultural influences. This view of the matter involves a considerable commitment on an issue that must still be regarded as unsettled, and requires that some kind of objective statement about the so-called "basic" elements of the personality be made. It may be pointed out that if personality is conceived as a system of responses arising in a cultural matrix, the individual lives his entire life within such a matrix and is never independent of it. Why, then, unless one assumes that learning and the organization of responses takes place only in childhood, should later experiences be largely ruled out? Most of the culture-personality studies by their very emphases are only partially situationally oriented—that is, with respect to childhood—and take the relative insignificance of later experience for granted. Though this assumption appears to be generally plausible to most social scientists, it is nevertheless necessary to show empirically which response systems change readily and which do not, and under what conditions.

Indirect vs. Direct Learning. The belief that personality patterns are fixed un-

consciously and early involves a corollary assumption that these patterns cannot be directly taught, or that they can be taught later only if the childhood training has been favorable. The latter argument is another heads-I-win-tails-you-lose proposition; the former argument rests upon an invidious comparison of different types of behavior, some being judged as more basic than others without specification of the grounds for these conclusions. At times the argument assumes a purely circular form: those patterns which come first are most important because they are the earliest ones.

In reports of research on non-literate peoples considerable data are of course given on direct teaching, but in the interpretation of the deeper meaning of the data and in offering genetic explanations of personality there is a clear tendency to stress the major influence of indirect and unconscious learning. For example, the Beagleholes⁵⁷ explain the free spending habits of the Maori in terms of childhood frustrations. The fundamental motive operating here is said to be the "buying of love" which the individual is afraid of losing because of the impact of certain childhood experiences. Peter Buck⁵⁸ denies this interpretation, suggesting that patterns of handling money are directly taught—a point that is also made by B. Mishkin.⁵⁹

The Beaglehole interpretation is rendered untenable anyhow by the fact that, regardless of types of childhood training, most non-literate peoples were resistant to the introduction of European economic practices and ideas.

Though this particular interpretation by the Beagleholes is more obviously vulnerable than others of like character, it is, nevertheless, a good example of the emphasis on cumbersome and unverifiable theories of indirect learning where much simpler explanations are available. Admittedly the hypothesis of direct learning is not always applicable,

but whenever it is, it is attractive by contrast in its simplicity and verifiability. The predilection for indirect explanations no doubt stems from stresses placed upon "unconscious" processes, upon emotional aspects of interpersonal relations, and upon the deep, hidden, inner reality called "personality." We agree with Linton⁶⁰ who says:

. . . how far is the personality formed by these factors which operate on the child without the child really understanding what is happening, and how far is it formed by actual instruction? I think this is a question we have not solved at all at the present time.

SUMMARY AND SUGGESTIONS

The bulk of this paper has been concerned with negative criticisms, raised by us and others, concerning the conclusions, evidence, methods, and general conceptual framework offered and used by culture-personality writers. These criticisms seem to us to indicate quite clearly that available evidence offered by the writers in support of their conclusions is inadequate and does not justify their conclusions. Positive generalizations made in this area are generally based upon unwarranted confidence in rather loose unscientific methods of interpreting data, and upon a relatively uncritical acceptance of a particular conceptual scheme.

Research on the psychological responses of non-Western people needs to be made more specific and concrete. Culture-personality writers have, on the whole, tended to avoid this kind of limited investigation for a number of reasons having to do with the danger of viewing a given segment of behavior out of its cultural context. The emphasis upon cultural configurations was in part a reaction against such segmental interpretations.⁶¹ Moreover, ethnologists have not been concerned with specific psychological problems because they have been urgently concerned with gathering descriptive materials about

⁵⁷ In *Culture and Personality*, p. 172.

⁵⁸ E. and P. Beaglehole, *op. cit.*

⁵⁹ Buck, *ibid.*, "Foreword."

⁶⁰ "The Maori of New Zealand," in M. Mead (ed.) *Cooperation and Competition Among Primitive Peoples*, 1937, pp. 452, 454-455.

⁶¹ Cf., M. Mead in L. Carmichael (ed.), *Manual of Child Psychology*, 1946, p. 674, who writes that "Emphasis is laid first upon collecting data upon the total [!] socialization process, and then focal points within that process may be studied."

non-literate societies before they vanished or were distorted by Western influences.

In his role as a psychologist the anthropologist needs to integrate his work as a careful ethnologist with a large body of psychological theory and research, including the non-clinical.⁶² The study of limited, specific, and verifiable propositions does not necessarily run counter to the ethnologist's insistence that a culture must be understood as a whole before specific psychological studies are undertaken. The cultures best suited for these purposes should be those concerning which a considerable amount of ethnological material is available. Such in-

⁶² As early as 1934, R. Lowie, who has remained aloof from the culture-personality trend, objected to Sapir's studies of personality, saying that these were contributions to philosophy and did not apply any psychological principles known to psychologists: review of Goldenweiser's *History, Psychology and Culture, American Anthropologist*, XXXVI (1934), 115.

vestigations would be valuable, not only as correctives of certain ethnocentric tendencies in psychological theorizing, but should also make constructive theoretical contributions on specific issues. Aside from the obvious benefits accruing to anthropology from this "gearing-in," another advantageous effect might be to arouse much more interest in anthropological work on the part of the great majority of psychologists and social psychologists.

A concern with more concretely limited and traditionally emphasized psychological problems would broaden the culture-personality ethnologist's range of choice of conceptual schemes and hypotheses. As it is now, the substantial choice is between no psychology at all and a brand of neo-Freudianism. The emphasis should not be on committing oneself to one school of thought or another, but of checking all rival hypotheses on specific problems by accepted scientific procedure.

A SOCIOLOGICAL ANALYSIS OF A SCHIZOPHRENIC TYPE*

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THE PURPOSE of this inquiry¹ is to analyze a transient schizophrenic type who is characterized by a relatively normal childhood and adolescent adjustment, a conflictful, explosive breakdown and a favorable chance for improvement or recovery.² This type is in definite contrast to

the chronic schizophrenic who has withdrawing or perverse tendencies from childhood, a slow, insidious breakdown and an unfavorable chance for improvement or recovery—at least under present conditions of therapy in state mental hospitals.³

This dynamic and developmental classification of schizophrenics differs in the following ways from the static taxonomy of Kraepelin, whose criteria were based upon symptomatic end-reactions:⁴

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¹ This report, which is a phase of a larger inquiry, has been facilitated by a grant from the Social Science Research Council.

² Many dichotomous terms are used in referring to the distinction between the chronic schizophrenic and the acute schizophreniform, as: endogenous vs. exogenous, constitutional or somatogenic vs. psychogenic, true vs. pseudo, predisposed vs. situational, classical vs. atypical, malignant vs. benign, process vs. episodic. The term "schizophreniform" was first used by Langfeldt, then by Wittman and Steinberg. See G. Langfeldt, *The Schizophreniform States* (Copenhagen, 1938).

³ Rosen has treated 37 "deteriorated" schizophrenics so that they either improved or recovered. Were these patients in state hospitals many would not have improved, and some would very likely have spent the rest of their lives there. See J. N. Rosen, "The Treatment of Schizophrenic Psychosis by Direct Analytic Therapy," *Psychiatric Quarterly*, 21 (1947), 1-37.

⁴ See E. Kraepelin, *Dementia Praecox and Paraphrenia*. Edinburgh: E. and S. Livingston, 1919 tr. R. M. Barclay. (Some dynamic psychiatrists claim