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Letters

Psychosocial Synthesis

If the allegations contained in Margaret Mead's letter in a recent issue of *Science* [129, 1514 (1959)] are true, then I and all those with whom I worked and who reviewed my contribution are very deluded people.

There was no technique for any psychosocial synthesis in 1938. There were only exhortations to find one. There were Lamarckians like Freud and Roheim who were seeking universals and survivals, like castration and Oedipus complexes. Others mentioned by Mead were deriving the character of modern man from anal erotism (Fromm).

What I described in *The Individual and His Society* was a technique and not an ideology. It could only have been conceived by one who knew enough about psychodynamics to identify what personality traits were derived from what specific cultural directives. This belongs to a school of psychodynamics known as *adaptational*, which in 1938 was just arising. It does not follow Freud's libido theory, which bypassed the cultural variable and could not include it in the frame of reference.

The anthropologists with whom I worked would not have permitted their names to be used in an endeavor based on a technique plagiarized from others. Linton and Du Bois kept me well informed about what was being done by other anthropologists. Sapir and Benedict participated in these early seminars. There were most interesting ideas, but there was no psychodynamic technique.

I do not know what is meant by "historical parallelism." The most charitable interpretation is that at the time such a technique was "in the air." It was indeed; but it has been there since Herodotus. If my work was just a coincidence, why did Mead abandon the culture pattern for the genetic study of character in Bali, and Benedict adopt it whole for the study of Japanese character? The basic principles for this technique were worked out on Marquesan and Tanalan cultures. The technique was reinforced by Du Bois and me in 1939-1940, in the study of Alor. Mead and Bateson discussed this latter work at my invitation publicly in 1942. I then heard no untoward insinuations about this technique.

In a recent survey (1955) of *Psychoanalytic Schools of Thought*, a disinterested writer, Ruth L. Munroe, spends much time on my contribution, but she fails to pick up either the plagiarism or the historical parallelism. Munroe

shows a thorough acquaintance with those whom Mead alleges to have been my original sources. The only one who invented the tools that I used was Freud, even though he did not use them as I did.

In any case, I request that Mead expose me, not by decree or innuendo, but by documentation. This will require finding a technique (meaning a procedure, based on demonstrable principles, not an ideology, or intention of finding a procedure) that was in existence before 1938 and that could perform the following: derive the personality formation specific to each culture without the aid of the libido theory; demonstrate the relation of this personality to the adaptational problems of the community as a whole; demonstrate the relation of the products of fantasy to this personality; demonstrate the relation of child-rearing practices to the development of affectivity; demonstrate the relation of this latter development to the devices the society has for maintaining intrasocial balance and demonstrate criteria for intrasocial imbalance; and, finally, demonstrate the use of psychoanalytically oriented biographies as a source of information about the impact of social directives on personality formation and have these results check with projective tests like the Rorschach.

A. KARDINER

Department of Psychiatry,
Emory University, Emory University,
Georgia

Kardiner's letter is in response to my comment [*Science* 129, 1514 (1959)] made in reply to the review [*Science* 129, 322 (1959)] by Julian H. Steward of my volume on Ruth Benedict, that "at the time that Abram Kardiner independently began to apply psychoanalytic theory to the study of culture, the major theoretical lines for the study of personality and culture . . . had already been worked out. . . ." His response illustrates a dilemma that is widespread in those areas in which clinicians and research scientists attempt cooperation. The clinician, tied to his consulting room, with limited time for reading and usually none for field work, is often compelled to reinvent the known in his search for the unknown. This inevitably leads to a sense of having initiated *de novo* a whole field which may have existed for years. When I added to my comment the statement that "I have always regarded Kardiner's work as an example of historical parallelism," I was reasserting my belief that he did develop his approach independently. Such "parallelism" is the exact opposite of plagiarism.

In 1941, at Kardiner's express re-

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Letters

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quest I wrote a considered evaluation of his book, *The Individual and His Society*, for the *American Journal of Orthopsychiatry* [11, 603 (1949)]. At that time I characterized his method as unique in that "Dr. Kardiner has exposed himself to auditory experience [that is, listening to the accounts presented verbally by anthropologists] of the integrated set of social institutions which make up a culture." In the same review I commented also that Kardiner had undertaken "the ambitious task of developing a social science *de novo* with no original data of his own and without an explicit reliance upon other workers—an ambition which is very much tempered in his own field by an explicit utilization of the work of Freud himself, and Fromm, Horney, Rado and Reik." When I undertook to write this review, I specified that a parallel evaluation of the book must be requested from a psychoanalyst and Sandor Lorand's review appeared in the same issue [*Am. J. Orthopsychiat.* 11, 605 (1949)]. After commenting on the psychoanalytic theory, Lorand expressed the hope that "the anthropological data . . . may stimulate anthropologists to recognize the importance of cooperation between psychoanalysis and anthropology." This it did, and anthropologists should be grateful to the very substantial stimulation and backing which Kardiner gave to several important pieces of field work, particularly the work of Cora Du Bois.

I do not now—as I did not in 1941—make any attempt to evaluate Kardiner's contribution to psychoanalytic theory (it was for this reason that I stipulated that a review of *The Individual and His Society* by a psychoanalyst should be included). I speak only of that body of theory known as "culture and personality," which Lawrence K. Frank must be credited with having initiated in the 1920's. The first published use of the phrase that I know of was by the anthropologist Leslie White, in 1925, in his article, "Personality and culture" [*The Open Court* 39, 145 (1925)].

In the folder which contains my manuscript of the review published in the *Journal of Orthopsychiatry*, I find a page originally designed as a footnote but not published with the review. I reproduce it here, as it was written in 1941—so much closer to the events in question.

This review will be confined to a consideration of the first part of Dr. Kardiner's book, that which concerns itself specifically with cross-cultural problems as the book falls into almost distinct sections, with the latter of which only a prac-

ticing analyst is fully competent to deal.

This is the most ambitious attempt to rear a purely theoretical structure of the inter-relationships between the socialization of the child and the overt cultural forms, within the framework of psychoanalytic theory based upon experience in analyzing adults. During the last fifteen years there has been increasing attention devoted to this problem and it has been attacked from several different angles, by an adult analyst with anthropological training who went into the field and applied anthropological and ethnological methods in the field (Roheim), by child analysts working in cooperation with analytically trained anthropologists (Erikson, working among the Sioux in cooperation with Scudder Mekeel, and among the Yurok in cooperation with Kroeber; Levy, working within a cultural context which was being studied by J. Mirsky), by psychoanalysts working out of the field upon material collected in the field by a psychologically oriented field worker (Spitz analyzing Mead), by a psychoanalyst analyzing ethnological material which contained formal material on children (E. Jones on Malinowski), and by field workers who have taken into the field a psychological orientation and [have] applied it specifically to the study of children (Mead, Gorer, Henry, Bunzel, the Beagleholes, Whiting). In all of the instances cited the concrete material has involved direct observations upon children collected with direct reference to the problem of character formation. During the same period there has been an increasing amount of theoretical writing upon the problem of personality and culture which made certain very definite assumptions about the importance of the socialization process, although it did not use data based on the study of children (notably Benedict, Sapir, Frank, Sullivan, Lasswell, Dollard, Bateson, Horney, Fromm, Hallowsell). . . .

Kardiner's second claim—that in response to his work, initiated in 1938, I abandoned the "culture pattern" approach in my work on Bali (which, incidentally, was planned in 1933–1934) for "the genetic study of character"—shows the same profound lack of knowledge of my published work which makes me feel quite safe in acclaiming his independence of the published work of his predecessors and contemporaries. I began using the "genetic study of character" in my first field work in 1925, and before the Balinese work was planned this approach had been progressively systematized in cooperation with other workers in the field. Notable among these were Lawrence K. Frank, who in the early 1930's inaugurated a cross-cultural study of this type at Yale under Edward Sapir and John Dollard; Erik Erikson, whose systematic specifications of developmental stages were already available; and Erich Fromm, who was relating economic behavior and character structure.

Ruth Benedict's inclusion of some child-rearing material in *The Chrysan-*

themum and the Sword (1946) dates back to her slowly growing recognition of the importance of this approach through her participation, in 1936, in the National Research Council Committee on Psychological Leads in Field Work (which was chaired by Edward Sapir, and for which Geoffrey Gorer worked on the background material) and also to the work on Japanese character done at the beginning of World War II by Gorer, Gregory Bateson, Clyde Kluckhohn, Alexander Leighton, myself, and others.

In his letter, Kardiner invokes Ruth Munroe, the author of *Schools of Psychoanalytic Thought*. In this book (page 141), Munroe writes: "She [Ruth Benedict] taught Kardiner, Mead and others in their earlier approaches to this problem." Ruth Benedict and Ruth Bunzel both participated in the seminar organized by Kardiner in 1936, to which Cora Du Bois and Ralph Linton later contributed.

The techniques which Kardiner claims and which he illustrates through reference to Du Bois' work—that is, the use of life histories, projective tests, children's drawings, myths, and observations on child-rearing practices—had all been developed before 1938.

MARGARET MEAD
*American Museum of Natural History,
New York, New York*

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