

## MALINOWSKI: EPISTEMOLOGY AND OEDIPUS

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Levi-Strauss (1963) has suggested that the nature of totemism can be understood as an expression of one of the dimensions of thought: opposition and integration. In these terms, the disciplines concerned with human behavior are not a little totemistic in arbitrarily carving their respective fields out of the continuous human reality. Hence, a persistent concern has dealt with the problem of the legitimate domain of anthropology. It was the boldness with which Malinowski saw the inclusiveness of anthropology that deserves attention.

Malinowski's work is still relevant because of its emphasis on continuity and integration in the explanation of human behavior. But conversely, the shortcomings of his thinking can be attributed to a lack of significant conceptual differentiation and opposition. Nevertheless, Malinowski can serve as a starting point in a critique of the over-weighted use of discontinuity as an explanatory principle.

For example, we can consider the extreme emphasis Leslie White (1945) puts on discontinuity. He attempts to argue for the empirical autonomy of culture by making pseudo-philosophical arguments which involve an arbitrary segregation of various aspects of reality. He attempts to demonstrate an empirical reality by showing the possibility of an a priori logical reality. The danger in this is that there is a temptation to place a higher value on the a priori category distinctions than on actual explanation.

An example of the above can be found in Geertz's (1957) study of a boy's funeral in a Javanese village. He attempts to show that the "logical-meaningful" cultural aspects of the ritual must be seen as discontinuous with the "causal-functional" social aspects of the ritual in order to explain a conflict that arose between two factions as to the proper burial. He assumes that it is only the social aspects of the ritual that illuminate the problem. But actually the reverse is true. As the conflict arose, both factions were operating in accord with their respective beliefs. There was a continuity between the two levels rather than the contrary. The a priori discontinuity was so embedded in Geertz's mind that he failed to see that his ethnographic case supported the contrary of his own argument.

The assumption of discontinuity in order to delineate areas of inquiry is not only justifiable, but is necessary. Different systems must be seen in their own terms before they can be integrated. This is partly why Levi-Strauss is so adamant in his insistence on the autonomy of structural analysis.

However, it is rather defeating to deny the existence of mechanisms of continuity if they have explanatory power. At this juncture it is instructive to see what Levi-Strauss has to say about Malinowski's theory of totemism:

Psychoanalytic theory, which Malinowski implicitly makes use of [Levi-Strauss is incorrect about Malinowski's use of psychoanalysis; see below], sets itself the task of teaching us that the behavior of

disturbed persons is symbolic, and that its interpretation calls for a grammar, i.e., a code which, like all codes, is by its very nature extra-individual. This behavior may be accompanied by anxiety, but it is not anxiety that produces it. The fundamental error in Malinowski's thesis is that it takes for a cause what, in the most favorable circumstances, is only a consequence or a concomitant (1963:69).

Actually, impulses and emotions explain nothing: they are always results, either of the power of the body or of the impotence of the mind. In both cases they are consequences, never causes (1963:71).

Actually, Levi-Strauss cannot hold that there is an absence of continuity between naturalistic factors and the structure of thought. He merely turns his conception of psychoanalysis upside-down: thoughts cause emotions. Yet, he does not explicate the principles of this continuity in psychological terms nor does he explain why the relationship should be non-reversible. But the fact is, that there is a great quantity of evidence that demonstrates that drive-states do have a determining effect on the structure of thought. Whatever his sophistication, Malinowski was not entirely wrong on this point.

The important thing is not whether these distinctions are logically justifiable, but simply whether they are useful as explanatory devices. Spiro (1951) clarified only half the problem in arguing that the distinction between personality and culture is a false dichotomy; the real question is whether the dichotomy is useful in terms of particular questions. At certain points differentiation may be necessary.

Sol Tax (1956) has suggested that anthropology is characterized by the intercommunication of scholars with different, but related, interests. But this puts the emphasis in the wrong place. So many types of inquiry under one rubric reflect, not only the intercommunication of scholars, but an underlying continuity in human behavior. Although there are particular problems with particular solutions, in the long run the viability of anthropology as a discipline will depend directly on comprehending this continuity.

Malinowski clearly recognized the need for integration; he saw that there must be continuity between man's animal nature and cultural behavior. He says:

We need a theory of culture, of its processes and products, of its specific determinism, of its relation to basic facts of human psychology and the organic happenings within the human body . . . (1944).

As Kardiner and Preble have observed, it is this side of Malinowski's anthropology that is both controversial and important:

Malinowski recognised no boundaries. This trait infuriated professional anthropologists who wanted to establish an independent scientific discipline. . . . He ignored the academic partitioning of the field of human behavior into cubicles of anthropology, sociology, and psychology, and moved freely from one medium to another according to the requirements of the problem (1961:161).

Throughout his work, Malinowski was concerned about the characteristics that differentiated man's behavior from that of pre-hominid animals; for him the locus of this difference was culture. His theory building can be understood as an attempt to synthesize knowledge about man's animal and cultural aspects.

Stripped of elaboration, Malinowski's theory of culture is rather simple. It involves the operation of two principles: function and organization. Function is always the satisfaction of basic or derived needs. The basic unit of organization is the institution. Culture is instrumental; it satisfies needs. Responses to these needs are organized into various "instrumental imperatives of culture" which lead to economic institutions, education, social control through morality and law, and political organization. The instrumental act, through reinforcement, also becomes satisfying. The former, physiological satisfaction, is related to "Drive 1" while the latter is related to the derived "Drive 2." It is obvious that Malinowski's theory of culture involves a simple stimulus-response psychology.

Consequently, Malinowski argued that form was always determined by function (1944:149); that is, organization is reducible to function (1944:144, 151). His explanation of how function determines form, inasmuch as he has an explanation, is ultimately evolutionary and evades the problem with an apparent tautology: ". . . the moment such devices [culture] have been adopted, in order to enhance human adaptability to the environment, they also became necessary conditions for survival" (1944:121). Malinowski called these "necessary conditions" derived needs and cultural imperatives. It follows that an evaluation of Malinowski's theory of culture resolves itself into a consideration of biological needs and the mechanisms of their permutations and transformations.

Piddington has suggested that: "The specific contribution of the theory of needs is that it emphasizes, at all levels, the biological determinants of cultural activities and so provides a principle of analysis and comparison of universal validity" (1957:38). This, without doubt, is one of the "wide open doors" that Lowie (1937:234) has accused Malinowski of "battering down." What is objectionable in this kind of biological determinism is not that it is inaccurate, but simply that Malinowski never presented a systematic theory for explaining specific behavioral facts in terms of generalized needs. Piddington has attempted to answer this criticism for Malinowski in the following way:

Why, it is sometimes argued, if all cultures are to be regarded as responses to the same needs, should there be any variation between them . . .? The answer is to be found in the variety of ways in which the less specific needs may be satisfied. And the anthropologist is not called upon to account for this variety, any more than the palaeontologist is always expected to say why from a common ancestral type, different species have evolved in different directions (1957:39-40). This, of course, is absurd; cross-cultural variation is one of the things the anthropologist is called upon to explain. Malinowski was also aware of the same problem:

The cogency of the functional approach consists in the fact that it does not pretend to forecast exactly how a problem posed for a culture will be solved. It states, however, that the problem, since it is derived from biological necessity, environmental conditions, and the nature of cultural response, is both universal and categorical (1944:115).

But not only does Malinowski decline to explain cultural variation, he denies the possibility:

When an anti-functionalism remonstrates that, after all, there are cultures where neither spoons nor forks nor knives are used, and that,

therefore, function explains nothing, we simply have to point out that that explanation to the scientific thinker is nothing else but the most adequate description of a complex fact. The type of criticism levelled against functionalism, to the effect that it never can prove why a specific form of drum or trumpet, or table implement or theological concept, is prevalent in a culture, derives from the pre-scientific craving for first causes of "true causes" (1944:117).

Without disagreeing with Malinowski's assertion that there is no more to explanation than description, it is still possible to criticize him on the ground that what he considers description is not adequate. He failed to see that abstract theorizing can lead to better description, i.e., prediction.

Piddington also notes that another problem which is closely related to the one above concerns ". . . the precise definition of different needs and minimal conditions necessary to their satisfaction" (1957:43). But it is difficult to see how any significant advance in this direction will enhance Malinowski's theory. It is apparent that the classification of needs can be multiplied indefinitely; it is possible to invent additional needs to explain each new item of behavior. Consider the following: "The functional explanation of art, recreation, and public ceremonials might have to refer to directly physical reactions of the organism to rhythm, sound, color, line and form, and to their combinations. It would also relate, in decorative arts, to manual skills and perfection in technology, and magical mysticism (1944:174). But Piddington notwithstanding, this kind of ad hoc verbalizing is as sterile as it is non-theoretical.

What is required, and what Malinowski's theory lacks, is a psychology that can explain the psychodynamics of needs. Kluckhohn has framed this kind of problem in general terms: "A classification is useful to the degree that it sheds light on the relation of one set of facts to another" (1960:134). In other words, what is missing in Malinowski's theory is not a classification of needs, but a systematic statement about how they are dynamically related to thought.

We have returned to the same problem that Levi-Strauss detected in Malinowski's functionalism; it is Malinowski's failure to derive cultural complexity from biological needs that appears to vitiate his entire theory. But the deficiency is not what Levi-Strauss thinks it to be. Rather, it is Malinowski's failure to integrate sophisticated psychological thinking into his theorizing. Parsons has summarized this point well:

. . . even on the basis of learning psychology alone, Malinowski takes up only the one idea of instrumental learning and altogether ignores the possible significance of contiguity learning and classical conditioning. Even more serious, he seems to be guilty of a basic confusion, namely between the necessary conditions for a process of learning to take place, and the motivational structure of psychological process after the learning has occurred. There is no reason to doubt that the motivation of all secondary drives or derived needs goes back to the genetic history of the individual to the satisfaction of primary drives. But that in the mature individual the "ultimate" motive for any specific act of learned behaviour must be the continuing satisfaction of a specific drive is certainly not an established psychological doctrine.

Above all, perhaps, the most serious source of the difficulty of Malinowski's position lies in his failure to consider the problems of the organization of human personality as a motivational system. He clearly leaves it as a bundle of biologically inherited basic needs, about each of which there then develops a cluster of learned instrumental patterns of behaviour. . . .

Perhaps this aspect of the matter can be summed up by saying that Malinowski failed to establish a theoretically adequate link between the observed facts of cultural behaviour and the psychological sources of motivation to such behaviour (1957:66-67).

What is remarkable about Parsons' criticism is that it is contrary to general opinion. Murdock (1943:444), Lowie (1937:234) and Herskovits (1948:48) have credited Malinowski with making positive contributions to the integration of psychology and the study of culture. The fact is, however, that these anthropologists are mistaken; Malinowski had little, if any, psychological sophistication. LaBarre (1958), Kluckhohn (1943), and Roheim (1950:167) are in agreement with Parsons' criticism. As Kluckhohn says: "As for psychology, Malinowski remained rooted in an outmoded behaviorism. His publications show no mastery of contemporary learning theory. Psychoanalytic theory he influenced importantly, but he was never analysed, and psychoanalysis failed to become part of his systematic thinking (1943:216).

Malinowski's relation to psychoanalytic theory requires further examination, but before returning to this aspect of his work, it is necessary to look at the epistemological foundations of his theory. His lack of psychological sophistication is merely one expression of his general lack of theoretical inclination. Parsons (1957:70) concluded that Malinowski's contribution to theory was mainly at the "clinical" level, rather than on that of general theory. Kluckhohn (1943:209) makes a similar, but more severe, criticism.

The question that arises, is why Malinowski was such a poor theoretician. Was he incapable or does it reflect something more general about his scientific outlook?

The answer is, that there was always an implicit preference in Malinowski's thinking for seeing human behavior as paradoxical rather than determined. There is certainly no lack of the word "determinism" in Malinowski's writing, but in actuality he was never a thoroughgoing determinist. In this respect Leach's paper on "The Epistemological Background to Malinowski's Empiricism" is perhaps the most insightful of the critiques of Malinowski's work. Leach's criticism is consistent with that of Kluckhohn and Parsons; he says that Malinowski had a ". . . bias against abstract theory which kept his imagination firmly earthbound" (1957:120). But Leach proceeds to offer an explanation.

He argues that Malinowski was grounded in the epistemology of William James. Like Malinowski, James did not restrict the bases of scientific belief to rationality (logicality) or plausibility (predictability) but also maintained that: ". . . we are entitled to believe whatever can be shown to be biologically satisfying even though the belief in question may be metaphysical and incapable of verification either by experiment or rational argument" (1957:123). Both James and Malinowski take a proposition

to be valid if it is "sensible" and what is sensible to Malinowski is usually what is obvious. Leach notes how this aspect of Malinowski's thinking is related to the kind of naïve biological reductionism that was noticed above:

. . . for Malinowski social phenomena exist in order to satisfy needs of the biological organism. Functions are thus both purposive and positive and to detect them requires intuitive judgment. Functionalism, in Malinowski's hands, became something very like a religious creed; it is presented to us as sensible (practically useful) rather than reasonable (logical or plausible). The "truth" of Functionalism is itself simply a matter of functional utility (1957:123-124).

Malinowski's functionalism was part of a general tendency to limit himself to obvious answers, but when the test of validity is sensibility, even the obvious is sometimes incorrect. More serious, is the fact that Malinowski's functionalism side-steps a serious quest for the determinants of human behavior. Leach suggests an explanation for this:

Malinowski, like William James, was a rebel against the mechanistic implications of late nineteenth-century thought and . . . his "functionalism," like James's "Pragmatism," was an aspect of this revolt. . . .

Malinowski's biggest guns are always directed against notions that might be held to imply that, in the last analysis, the individual is not a personality on his own possessing the capacity for free choice based on reason (Leach 1957:126).

Leach goes on to show how his emphasis on rationality is reflected in various aspects of Malinowski's work including his theories of magic, kinship, and technology (1957:127-135). Leach makes the point only implicitly, but the insight to be gained from his paper is that the reason Malinowski never developed a sophisticated theoretical system is because such a program would have been short-circuited by his belief in the essential rational and individualistic nature of man. For Malinowski the function of cultural behavior is obvious because man is obviously rational.

Malinowski's examples of biological functionalism were usually very simple and transparent; he carefully avoided applying his functional explanations to subtle and complex cultural phenomena. This explains why Malinowski attacked any interest in the strange or exotic as being unscientific. He decries the fact ". . . that non-functional as well as anti-functional tendencies exist in anthropology. The field-worker with his eye on the exotic or picturesque is one example" (1944:149). "The less directly organic the need to which human behavior refers, the more likely it will breed those phenomena which have provided the greatest amount of food for anthropological speculation" (1944:73).

What Malinowski did not like about the exotic was that he could not explain it in terms of his model of the rational man.

Theory for Malinowski was indeed at the "clinical" (= obvious) level as Parsons has said; his behavioristic empiricism militated against the development of a significant theoretical system. And on the other side of this was the result that Malinowski's view of man was saturated with paradox and ambiguity. It is fair to say that this state of affairs satisfied Malinowski for he was left with his cherished belief that individualism and freedom were central in human life. The dialectic Malinowski saw between determinism and freedom is nowhere better revealed than in Freedom and Civilization (1960a) and Crime and Custom in Savage Society. It is clear

that he opts for freedom: "The fact is that we, one and all, do feel such a craving for freedom, and that we demand it with all the emotional insistence of our being" (1960a:70).

Related to this is the fact that Malinowski was not even a consistent biological reductionist, as the following shows: "This new artificial environment obeys a determinism of its own. There exist laws of cultural process, of the constitution of culture, and of the efficiency of concerted activities. Hence culture inevitably becomes a source of new restraints imposed upon man" (1960a:34). This appears to be in conflict with his biological determinism, but Malinowski slips between the twin horns of culture and biology and remains faithful to freedom: "Culture thus provides man with the wider and larger instrumentality for the satisfaction of all his primary, that is, biological needs. It also makes him independent of certain environmental trammels and dangers. In this there enters that increase in range of choice and purpose as well as in the efficiency of behavior which we define as the cultural increment in freedom" (1960a:104). Malinowski is indecisive; he is at once a free agent, culturologist, pseudo-psychologist, biological reductionist and a master of self-contradiction.

Another aspect of Malinowski's escape from determinism was his preoccupation with double standards and inconsistencies in human behavior: "In a community where laws are not only occasionally broken, but systematically circumvented by well-established methods, there can be no question of a 'spontaneous' obedience to law, of slavish adherence to tradition" (1926:81). Note the open contradiction in the above and Malinowski's inability to recognize it. Or consider the following: "This, like everything else in human cultural reality is not a consistent logical scheme, but rather a seething mixture of conflicting principles" (1926:121).

Many of Malinowski's detractors have criticized him for presenting Trobriand life in such manifold complexity. But this kind of criticism is unconvincing; it was Malinowski's genius to be able to see and record the complexity of Trobriand life. If the complexity has remained inexplicable, the answer is not to discard the facts. That he did not simplify his observations to fit his theory is to Malinowski's credit. But what he did do was to simplify his criteria for explanation so that his theory could fit the data. The deficiency is not that Malinowski detected the apparent inconsistency and complexity of human behavior, but that he did not really attempt to explain this complexity.

It is not difficult to criticize Malinowski for not being more theoretical, but it is only fair to Malinowski to realize that his conception of himself and of science in general corresponds with Parsons' evaluation of him. Malinowski repeatedly emphasized that his theories were merely a means for facilitating observation (1944:65, 67): "We see, thus, that although at first sight our definitions may appear 'vague, insipid, and useless,' in reality they are condensed formulae which contain extensive recipes for the organization of perspective in field-work. And this really is the hallmark of scientific definition. It must principally be a call to a scientifically schematized and oriented observation of empirical fact" (1944:115).

Malinowski's pragmatic empiricism comes clearly to the fore; science is a matter of "recipes" and not of theory. His answer to the apparent paradoxes in human behavior was to continue documenting their existence, using

traditional, common sense categories. Freedom and paradox were more real than systematic determinism.

It is now possible to see the relationship of psychoanalytic theory to Malinowski's thinking. The point that needs to be made is one that is contrary to general opinion; namely, Malinowski had no understanding of psychoanalysis. As noted above, LaBarre, Kluckhohn, Parsons, and Roheim share this opinion.

It is worth pressing this matter because it has important implications in terms of Malinowski's general theoretical thinking and also because many students have the false impression that Malinowski made significant use of psychoanalytic theory (Lowie 1937:234; Murdock 1943:444; Fortes 1957:161, 165). Others, with equal error, have felt that Malinowski brought significant negative evidence to bear on psychoanalysis (see, Herskovits 1948:48; Whiting and Child 1953:13; Linton 1956:99; Kardiner and Preble 1961:221; Singer 1961:61; Carstairs 1961:537; Kennedy 1961:413).

First, it is important to consider Malinowski's own opinion of psychoanalysis. More often than not he was highly critical. His criticism was always highhanded, and never came to grips with psychoanalytic theory itself. His comprehension did not go beyond the popular stereotypes. For example: Freud and his followers extended the drive which we modestly listed as sex appetite into a somewhat metaphysical concept of the libido, and attempted to account for most phases of social organization, ideology, or even economic interests by infantile fixations of libidinous drives. In this process they also included the activities of the colon and bladder, and thus reduced the prime movers of humanity to the regions and processes occurring just below the human waist (1944:82).

It would appear that the only difference between Malinowski's conception of psychoanalysis and his own theory of culture is that in his system some of the prime movers are just above the waist. But this does give us an insight into Malinowski's thinking, for it shows that he cannot conceive, even while being critical, of a theory that is any more complex than his own simple theory of needs.

On the other side of the issue is the popular belief that Malinowski made a major criticism of psychoanalytic theory in arguing that the oedipus complex has a sociological aspect. Linton's acceptance of Malinowski's "findings" is typical:

. . . Malinowski was of the opinion that it [the oedipus complex] did not exist in the Trobriand Islands, where . . . the father is not the person in authority over the child. It is my opinion, shared by a good many of my psychoanalytically sophisticated anthropological colleagues, that in such situations the oedipal attitudes are not directed at the real father, but at the mother's brother, child's closest male relative--assumes the disciplinary and rewarding functions which, among ourselves, are associated with biological fatherhood (1956:99).

But in point of fact, Malinowski was not telling the psychoanalysts something they did not already know. Malinowski's insistence on the sociological origin of the oedipus complex merely shows that he was not acquainted with the psychoanalytic literature. From the earliest formulations of the oedipus complex and after, Freud and his students traced its variations to



variations in familial dynamics and configurations. That the psychoanalytic theory of the oedipus complex was always essentially an inter-personal theory is clear from the fact that later psychoanalysts have found it possible to discard the notion of its phylogenetic origin without having to revise the general theory (Fenichel 1945:97; Hartmann, Kris and Loewenstein 1951:16).

Malinowski's thesis is well known: in a matrilineal society the family complex consists of incestuous feelings between brother and sister and hostility and ambivalence directed at the maternal uncle. But why did Malinowski think this necessitated a critique of Freudian theory?

The answer is somewhat involved and relates to the general epistemology of Malinowski's thought. But the following passage gives an indication:

. . . if, as has been proved, there are no traces of it [the oedipus complex] either in Trobriand folklore or dreams or visions, or in any other symptoms; if in all these manifestations we find instead the other complex--where is then the repressed oedipus complex to be found? Is there a sub-conscious below the actual unconscious and what does the concept of a repressed repression mean? Surely all this goes beyond the ordinary psycho-analytic doctrine and leads us into some unknown fields of metaphysics (1960b:130).

Malinowski's glibness is but an unwitting confession of ignorance. He appears to think that Freud argued that the unconscious is found undistorted in dreams, jokes and the like, but in fact this is precisely what Freud found not to be true. Malinowski never grasped the distinction between latent and manifest content because his behaviorism only allowed him to consider meaningful that which he could observe. For example: "All that is said is clearly written on the surface of the myth, and I have hardly attempted any complicated or symbolic interpretation. . . . Then it is clear that we need not rely so much on roundabout or symbolic reinterpretations of facts, but can confidently let the facts speak for themselves" (1960b: 106-107). But only a pragmatic empiricist like Malinowski can hear facts speaking (ideas of reference?). The truth is either manifest or it is not truth. This explains why Malinowski repeatedly misused psychoanalytic terminology; he habitually used words like "ambivalence" and "repression" to refer to overt behavior rather than to endopsychic processes as the psychoanalysts intended them to be used.

Hartmann, Kris and Loewenstein have raised a general problem with respect to the controversy over the presence of the oedipus complex in non-western cultures:

If we are informed that anthropological data collected in any given society do not reveal the existence of an oedipal conflict, we are inclined to raise the question what such statements mean. They assume obviously that the oedipus complex can be seen by outside observers. If this was true why did it need psychoanalysis to advance the view that there was in Western civilization a stage in child development in which the conflict was typical? Or is it that once this has been stated we can now assume that, with open eyes, we won't fail to see what was previously missed (1951:15-16).

Although Hartmann, *et al.*, did not make this statement in direct reference to Malinowski, it clearly applies. It is now possible to answer

the question as to why Malinowski thought the Trobriand case implied a critique of psychoanalysis. As with everything else, he thought the complex, if it were there, would be manifest at the behavioral level. He took the social structure of the Trobrianders to be an expression of the complex.

There is some hostility towards the maternal uncle and the fear of incest with the sister is highly ritualized, but there is nothing repressed about them. As the mythology shows, these themes are quite overt and constitute part of the daily living of every Trobriander. Oedipal feelings are perhaps ontogenetically prior to these social structural sentiments as Jones (1925) argued in his criticism of Malinowski; but Jones was undoubtedly wrong in explaining social structure as a result of psychological factors alone. Malinowski was equally mistaken in reversing Jones' argument.

But there is yet another aspect of this problem. Malinowski has stressed that the uncle is the primary disciplinarian (1960b:24). But students have been in such a hurry to accept Malinowski's thesis that they have ignored the basic facts of Trobriand ethnography. In the face of this, Roheim (1950:175) has been alone in pointing out that because of descent and residence systems the boy does not even live in the same village with his uncle. But Roheim's observation can be extended: a second point is that the boy would not, in any case, consider the uncle as his sexual rival for the mother as this would imply brother-sister incest. In fact the uncle is excluded from all sexual matters relating to a boy's mother:

By the brother's inability to control or to approach, even as a distant spectator, the principal theme in a woman's life--her sex--a wide breach [sic!] is left in the system of matriliney. Through this breach the husband enters into the closed circle of family and household, and once there makes himself thoroughly at home. To his children he becomes bound by the strongest ties of personal attachment, over his wife he assumes exclusive sexual rights, and shares with her the greater part of domestic and economic concerns (1929:203).

Further, Malinowski indicates that the maternal uncle does not take an active part in disciplining the boy until well after the oedipal stage (1960a:49). As Roheim puts it: "So what is he doing in the meanwhile? Just waiting to develop an avuncular complex?" (1950:175). Malinowski also indicates that the father actually does the disciplining before the boy goes to his maternal uncle (1929:7).

In Freudian theory the father is seen as a rival by his son because the father has access to his wife's sexuality, not because he acts like a tyrant vis-à-vis his little boy. Hence, there is no reason to suspect on theoretical grounds that the oedipus complex would be absent or substantially different in the Trobriands. In fact, in the similar culture of the nearby Normanby islands, Roheim found obvious oedipal themes in dreams (1932) and children's doll play (1941).

Although Malinowski has maintained that there is no trace of the oedipus complex in Trobriand folklore, this assertion is open to serious question. It is remarkable that only Roheim has noticed it, but great insight is not required to see that there are oedipal themes in Trobriand folk narratives. For example:

A woman named Karawata gave birth to a white cockatoo, who flew away into the bush. One day Karawata went to the garden, telling her kasesa (clitoris) to look after the kumkumuri (earth baking oven). The kasesa

replies confidently: Kekekeke. But the white cockatoo has seen everything from the bush; he swoops down and strikes the clitoris, who cries out plaintively: Kikikiki, and topples over, while the cockatoo eats the contents of the oven (1929:408).

The symbolism is transparent: woman gives birth to son = white cockatoo; earth baking oven = vulva; and eats contents of the oven = to have intercourse.

In the story about the "stingaree" there is a woman with five clitorises who has five sons. The stingaree utters a "ribald and cruel" ditty and "The stingaree then proceeds to business, copulates with the old woman and cuts off one of her multiple appendages. My native informants, in their commentary affirmed that the va'i [stingaree] had a penis . . ." (Malinowski 1929:406). The eldest son then attempts to protect the mother from the stingaree's next attack but is driven off by the latter's ditty, as are the other sons on consecutive days until the mother has only one clitoris left. Then the youngest son protects the mother by spearing the stingaree to death (Malinowski 1929:405-408). Roheim (1950: 175-176) cites both of these myths as oedipal and interprets the stingaree, of course, as a symbol of the father. The following ethnographic evidence can be cited in support of Roheim's interpretation: the stingaree is considered to be very defiling (Malinowski 1929:31); excrement is considered to be disgusting and defiling (1929:444); and one of the most central aspects of the father-son relationship is the fact that the son, as an infant, defiles his father with his body-wastes (1929:21, 444).

But Roheim (1950) fails to mention the most oedipal of all Trobriand myths, that of Dokonikan, the terrible ogre. As the monster approached, . . . the family decided to fly. The sister, however, at that moment wounded her foot and was unable to move. She was therefore abandoned by her brothers, who left her with her little son in a grotto on the beach of Laba'i, and sailed away in a canoe to the south-west. The boy was brought up by his mother, who taught him first the choice of proper wood for a strong spear [!], then instructed him in the Kwoygapani magic which steals away a man's understanding. The hero sallied forth, and after having bewitched Dokonikan with the Kwoygapani magic, killed him and cut off his head (1960b:103-104).

Afterwards, the boy puts the ogre's head in a pudding and gives it to the uncle, who, overcome with remorse, offers his nephew a number of gifts. The boy is unsatisfied until he takes the uncle's daughter in marriage. This marriage is highly improper, although not quite incestuous. Malinowski has to admit that Dokonikan symbolizes the father, but says that this part of the myth comes, in some unspecified way, from an unspecified patriarchal culture! (1960b:104-105). But the myth is complete: parricide and incest.

It is submitted that Malinowski did not like psychoanalytic theory for the same reason he disliked theory in general. On the one hand, it would have implied that human behavior is systematically determined and on the other, he could not accept notions about the unconscious, the libido, and so forth because they had no obvious behavioral counterparts; his pragmatic empiricism did not allow him to see patterned relationships and generalizations beyond those immediately given by his intuition and senses. It was Malinowski's love for the obvious that led him to praise the simplistic psychologies of Shand and McDougale (1960b:155-157) and to make the preposterous statement that Havelock Ellis (1931:77) had anticipated most

of Freud's discoveries. Malinowski gave credit to psychoanalysis because it views man as biological as well as experiential (1944:22), but because of his distrust of theory he failed to see that psychoanalysis could have helped him repair the weakest part of his own theory: that is, the problem of the transformation of biological drives into cultural behavior.

I have tried to look at Malinowski's work in terms of the ramifications of a particular theme: continuity. We can expect this much: a rational, unified theory of human behavior may be quite unlike anything we are now capable of imagining. Malinowski's failing was that he had too little imagination.

#### BIBLIOGRAPHY

- Carstairs, M.  
1961 Cross-cultural psychiatric interviewing, in B. Kaplan (ed.), Studying personality cross culturally. Evanston, Row-Peterson.
- Fenichel, O.  
1945 The psychoanalytic theory of neurosis. New York, W. W. Norton and Co.
- Fortes, M.  
1957 Malinowski and the study of kinship, in R. Firth (ed.), Man and culture: an evaluation of the work of Bronislaw Malinowski. London, Routledge and Kegan Paul.
- Geertz, C.  
1957 Ritual and social change: a Javanese example, *American Anthropologist* 59:32-54.
- Hartmann, H., E. Kris, and R. M. Loewenstein  
1951 Some psychoanalytic comments on "Culture and Personality," in G. B. Wilbur and W. Muensterberger (eds.), Psychoanalysis and culture: essays in honor of Geza Roheim. New York, International Universities Press.
- Herskovits, M. J.  
1948 Man and his works. New York, Alfred A. Knopf.
- Jones, E.  
1925 Mother-right and the sexual ignorance of savages, *International Journal of Psycho-Analysis* 6:109-130.
- Kardiner, A. and E. Preble  
1961 They studied man. New York, The World Publishing Co.
- Kennedy, D.  
1961 Key issues in the cross-cultural study of mental disorders, in B. Kaplan (ed.), Studying personality cross culturally. Evanston, Row-Peterson.
- Kluckhohn, C.  
1943 Bronislaw Malinowski 1884-1942, *Journal of American Folklore* 56: 208-219.  
1960 The use of typology in anthropological theory, in Selected Papers of the Fifth International Congress of Anthropological and Ethnological Sciences. Philadelphia.

- LaBarre, W.  
1958 The influence of Freud on anthropology, *American Imago* 15: 275-328.
- Leach, E. R.  
1957 The epistemological background to Malinowski's empiricism, in R. Firth (ed.), *Man and culture: an evaluation of the work of Bronislaw Malinowski*. London, Routledge and Kegan Paul, pp. 119-137.
- Levi-Strauss, C.  
1963 *Totemism*. Boston, Beacon Press.
- Linton, R.  
1956 *Culture and mental disorders*. Springfield, Thomas.
- Lowie, R.  
1937 *The history of ethnological theory*. New York, Farrar Rinehart.
- Malinowski, B.  
1926 *Crime and custom in savage society*. New York, Harcourt, Brace and World, Inc.  
1929 *The sexual life of savages*. New York, Harcourt, Brace and World, Inc.  
1931 Havelock Ellis, *Birth Control Review*, March, p. 77.  
1944 *A scientific theory of culture*. Chapel Hill, University of North Carolina Press.  
1960a *Freedom and civilization*. Bloomington, Indiana University Press.  
1960b *Sex and repression in savage society*. New York, Meridian Books.
- Murdock, G. P.  
1943 Bronislaw Malinowski, *American Anthropologist* 45:44-51.
- Parsons, T.  
1957 Malinowski and the theory of social systems, in R. Firth (ed.), *Man and culture: an evaluation of the work of Bronislaw Malinowski*. London, Routledge and Kegan Paul, pp. 53-70.
- Piddington, R.  
1957 Malinowski's theory of needs, in R. Firth (ed.), *Man and culture: an evaluation of the work of Bronislaw Malinowski*. London, Routledge and Kegan Paul, pp. 33-51.
- Roheim, G.  
1932 The psychodynamics of primitive cultural types, *International Journal of Psycho-Analysis* 8:1-224.  
1941 Play analysis with Normanby Island children, *American Journal of Orthopsychiatry* 11:524-529.  
1950 The Oedipus complex, magic and culture, in G. Roheim (ed.), *Psychoanalysis and the social sciences, II*. New York, International Universities Press, pp. 173-228.
- Singer, M.  
1961 A survey of culture and personality theory and research, in B. Kaplan (ed.), *Studying personality cross culturally*. Evanston, Row-Peterson.

- Spiro, M.  
1951 Culture and personality: the natural history of a false dichotomy, *Psychiatry* 14:19-46.
- Tax, S.  
1956 The integration of anthropology, in W. Thomas (ed.), *Current Anthropology*. Chicago, University of Chicago Press, pp. 313-328.
- White, L.  
1945 History, evolution, and functionalism: three types of interpretation of culture, *Southwestern Journal of Anthropology* 1: 221-248.
- Whiting, J. W. M., and I. L. Child  
1953 *Child training and personality: a cross-cultural study*. New Haven, Yale University Press.