ANTHROPOLOGY TODAY IN THE WAKE OF MALINOWSKI'S CENTENARY

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My lecture* commemorates the centenary of the birth of Bronislaw Malinowski who was born in April 1884. It is appropriate that I should give such an address because in 1937-1938 I was, for a period of about 15 months, a pupil of Malinowski and the number of surviving professional anthropologists who had that experience is now becoming very small.

My purpose is to consider the relationship between the social anthropology which Malinowski considered to be his personal invention and the kind of anthropology which is currently practised by those who see themselves as being in some degree his intellectual heirs. With that end in view I shall not say much about Malinowski as a person or about his professional career but I shall need to say something about his background.

Malinowski was born and educated in Cracow. When Poland was dismembered by the Congress of Vienna after the final defeat of Napoleon, the tiny republic of Cracow was the only piece of traditional Polish territory that retained its independence and even that was annexed by Austria in 1846. In consequence Malinowski's original legal nationality was Austrian though at heart he was very much a Pole.

Malinowski's anthropology, like that of his British contemporary and rival A.R.Radcliffe-Brown, owed a great deal to the sociology of Emile Durkheim but Radcliffe-Brown and Malinowski interpreted Durkheim in very different ways.

Both men accepted the idea that the individual is born into a society; which already exists and is constrained by the system of moral order which prevails in that society; but there the similarity ended. Radcliffe-Brown and his closest disciple Meyer Fortes thought of the social constraints in question as "jural rules", by which they meant specifiable regulations very similar to the legislative enactments of a modern state or the code of rules that is to be found in the book of Leviticus. But for Malinowski the moral order of society was more psychological, a set of internalised attitudes, something implicit in the culture overall, especially in its mythology, rather than in specifiable regulations.

This view of the relationship between the individual and his/her cultural environment influenced the American anthropologists Ruth Benedict and Margaret Mead and, through them, a whole host of American cultural anthropologists, though it has usually been rejected by Malinowski's intellectual descendants in Europe. But Malinowski's own attitude to these issues must surely have been related to his awareness that Polish nationalism had survived the 19th century precisely because individual Poles had consistently rejected the moral validity of the formal rules and regulations imposed upon them by external political authorities.

The circumstances of his upbringing led to Malinowski being a genuine polyglot and this too had an important bearing on his anthropology. By the time that he arrived in England in 1910 he already had a fluent command of Polish, German, French, Italian and English and his ready mastery of the languages he encountered during his field research became a legend among those who had seen him at work. He was not professionally trained as a linguist but his over dogmatic pronouncements about the dependence of lexical meanings upon social context have subsequently been treated with respect by a number of professionals in the field.

His own linguistic facility led him to expect a similar competence on the part of his pupils, which was not always fulfilled, but it also led him to back up his ethnographic descriptions with citations of vernacular texts which are often extremely valuable. In writing English he took as his model the rather florid manner of Sir James Frazer, the author of *The golden bough*, which was much more acceptable in the 1920s than it is today, but also, and more effectively, the style of his compatriot, Joseph Conrad, who is so fully accepted as a major contributor to

English literature, that many of the present generation do not realise that he was, by birth and upbringing, a Pole.

In reading Malinowski today it is important to remember that most of his significant work was published before 1935 and that much of is openly propagandist.

A good deal of Malinowski's success derived from the fact that his richly detailed ethnographic accounts of the Trobriand Islands in Melanesia were designed to appeal to a wide audience of general readers who had no professional interest in anthropological matters. But quite apart from the literary style which provides a striking contrast to what is now expected in a professional anthropological monograph, Malinowski also sought (and achieved) notoriety by going out of his way to shock the more conventional members of the English middle class society of which he had chosen to become an adopted member. His book *The sexual life of savages* is not the salacious work that might be supposed, but the title ensured that it was widely read, though not often publicly displayed.

It was first published in 1929. Only four years earlier J.B.S. Haldane, the Reader in Biochemistry at the University of Cambridge, had been dismissed from his post for committing adultery with his future wife. Times have changed! But those who read Malinowski's books today need to get some feel for the climate of opinion in which they were written, otherwise much of the combatant polemic will seem slightly absurd.

As often been remarked, and as I can confirm from personal experience, Malinowski was a great teacher. He had the charismatic qualities of a prophet; he attracted disciples. Even those who later turned against him, as many did, never managed to escape this personal spell. Malinowski's influence shows through quite clearly even in the work of Sir Edward Evans-Pritchard who was, in his public comments, passionately hostile to everything that Malinowski stood for.

Malinowski's senior pupil Sir Raymond Firth has described Malinowski's teaching methods in the following terms:

> «He (did not give) many ordinary lectures – he could use the rostrum brilliantly, on formal occasions, but what he really liked was the seminar, the informal discussion group, with someone else giving the paper. Bending over his sheaf of notes at the head of the table or sunk in his deep armchair, nothing would escape him – no loose phrase, no shoddy

thinking, no subtle point of emphasis. With a suave question, a caustic word, or a flash of wit, he would expose a fallacy, probe for further explanation, or throw new light on something said. At the end, after inviting opinion from all sides, he would draw together the threads in a masterly way, lifting the whole discussion to a higher theoretical level, and putting it in a perspective of still wider problems. He was always constructive. One of his gifts was so to transform what had been said as to bring out its value as a contribution to the discussion. He made each member of the seminar feel that, fumbling and inept as the words had been, Malinowski perceived the ideas and gave them all or more than they deserved».

I have to admit that my personal recollection of a Malinowski seminar is not quite like that but the passage I have quoted deserves close attention. Raymond Firth succeeded Malinowski as head of the Department of Anthropology at the London School of Economics. Firth's theoretical position has always remained close to that of Malinowski though it is quite untrue to say (as has sometimes been said) that during Firth's regime the LSE continued to be a stronghold of unmodified Malinowskian functionalism. But it was through Raymond Firth's performances as a seminar leader that the London School of Economics Department sustained its reputation during the 20 year period after the end of the last World War, and in this respect he did take over the mantle of his mentor. The passage I have quoted from Firth writing about Malinowski might well have been written by one of Firth's pupils writing about Firth! In this respect at least there has been continuity.

But outside his private stamping ground of the London School of Economics Malinowski's fame is generally associated with two keywords: first "fieldwork"; second "functionalism".

Malinowski's qualities as a fieldworker are universally recognised as quite outstanding and innovative, though his claims to have invented the whole procedure single handed was an exaggeration. In reality Malinowski's pupils profited from his mistakes; they were often able to improve on his techniques because he had warned them what not to do on the basis of his own experience.

On the other hand Malinowski's "functionalism" is usually treated with scorn. This is partly because what is commonly represented as Malinowski's functionalist theory is a thin

caricature of the original. But a great deal of what Malinowski himself taught under that label has now become assimilated into general anthropological theory and practice, and because it has ceased to be controversial it has ceased to be the hallmark of any academic school, functionalist, structuralist, symbolist or any other.

I will consider these two themes one by one.

First: fieldwork. The modern anthropological research worker goes into the field equipped with a variety of mechanical aids including a tape recorder, an instant camera of the polaroid type, film making equipment, and even (if the funds are lavish) a video tape camera with immediate play-back facilities. This plethora of gadgetry is mostly very recent and, to be frank, contemporary anthropologists have not as yet fully worked out how it might best be used. Malinowski and the generation of his immediate pupils had to make do with clumsy still cameras and simple notebooks. It is astonishing what they managed to achieve with these limited means.

There are serious weaknesses in the typical anthropological monograph of the Malinowski and post-Malinowski era, but the improvement on all earlier pre-Malinowski types of ethnographic description is immense.

What is the essence of this difference? Partly it stems from the fact that the pre-Malinowski and post-Malinowski researchers were tackling quite different kinds of problems.

19th century anthropology had developed as an adjunct to pre-history and zoology. The dark skinned "savages" who provided the subject matter of most anthropological investigations were considered to be barely human. They were objects of academic investigation because of what they were supposed to reveal about very long term human history. They were living fossils; survivals from the remote past rather than the contemporaries of their investigators.

Anthropologists went into the field to measure skull shapes in the belief that they could thereby map the distribution of the races of mankind. They collected material artifacts which were subsequently displayed in European and American museums alongside other similar looking objects from other localities. The shapes of such things and their geographical distribution were believed to provide information about the past "movements of peoples". The comparative study of languages was pursued in a similar way. It was not a matter of investigating how similar ideas

might be expressed in quite different ways but of mapping the distribution of languages for historical purposes. Likewise stories were collected as if they were material objects to be placed alongside other stories from other places. The special meaning of the story within its social context or the circumstances in which it might ordinarily be told were seldom investigated.

In all these activities the field anthropologist was a dissector of data. He took things to pieces. There was no suggestion that these sundry products of human activity might be fitted together into a totality which had social significance in itself. The human context was completely ignored.

Even the best of the pre-Malinowski ethnographic monographs are defective in this way. One such is W.H.R. Rivers' rightly celebrated account of the Todas of South India which was based on some 4½ months field research and published in 1906. Rivers emphasises that he worked throughout with interpreters and that he obtained his information from a large number of paid informants who were interviewed independently. In Rivers'view it was the lack of relationship between his informants which made their reports "objective" and therefore "scientific". Although Rivers attended some of the ceremonies that he describes he writes about them as individual items and not as part of a total system of interdependent institutions.

When Malinowski first went into the field in 1914 he certainly shared many of these conventional attitudes. The tribal peoples of Melanesia deserved study not because of what they were in themselves but because of what such study might reveal about the past history of the migrations of population or about the worldwide evolution of social institutions relating to the family, economics, politics, law and religion.

But in the light of his field experience Malinowski moved away from this traditional association of anthropology with history. Most of his writings about the Trobriand Islanders are presented as contributions to comparative sociology; the ethnographer and the people he is studying are all living in the 20th century. The reader's attention is constantly drawn to the fact that Trobrianders and Europeans have to deal with similar situations but that the Trobrianders have solved the economic problems of subsistence and the social problems of living together in a community in ways which are quite unfamiliar to contemporary Europeans.

During the pre-Malinowski era the procedures of field research served to emphasise the social distance between the ethnographer and those whom he was studying. Typically, field notes were derived from formal interviews with selected informants. The proceedings closely resembled a police court investigation with the harrassed informant making a statements under duress. The information that was collected was mainly of the question and answer type. The ethnographers considered that their first task was to collect verbal statements about exotic rather than day to day customary behaviour; they seldom attempted to observe behaviour at first hand; still less were they likely to become personal participants in the customs that they described.

As Malinowski gained experience he reversed this sequence of events. He taught his pupils to use their eyes and their ears to observe ordinary day to day life and only ask questions afterwards; he also encouraged his pupils to participate in the activities which they observed whenever that was at all possible.

This last feature, "participant observation", has come to be seen as the hallmark of all modern ethnographic research. The "participation" is never complete; a social gap between the observer and the observed will always persist but in the ideal case it is minimised. The modern anthropological fieldworker sees himself as a pupil rather than a police interrogator of his informants. This is a very radical change and, for the most part, it derives from Malinowski.

So what about "functionalism". The notion of "function", like that of "structure", has a long and diverse history in the biological, social and mathematical sciences and I shall not attempt to review this diversity. In radical contrast to his anthropological predecessors Malinowski repeatedly emphasised context. He insisted that the items and products of behaviour that the ethnographer records in his notebooks can only be understood if they are viewed in context. When removed to another environment and studied in a museum or in a folklore archive or as words in a dictionary the data are changed in their very nature. But what should we mean by context? Malinowski himself treated two units of observation as self evident, and both of them now seem very questionable.

First of all there is the human individual, the actor, considered as a biological organism. This individual has certain basic needs: food, warmth, the capacity to reproduce, the need for

defence against aggression, the need to communicate and associate with others, and so on.

From this may be deduced the obvious fact that Man is a social animal and that the human individual needs to live in a society in order to survive and reproduce.

Malinowski spoke of this larger entity, the society which envelops the individual, as if it were bounded and hard edged; a system that is complete in itself like the mechanism of a watch in which all the parts fit together into a single functioning whole with no loose ends.

Malinowski then treated this larger entity as if it were itself a directly observable organism. Just as the biological individual had physiological needs so also the socio-cultural organism of which the human individual was a member, had social needs. Malinowski then proceeded to argue that all the institutions that are to be observed in any particular cultural context (Malinowski studying the Trobriand Islanders; Firth studying the Tikopia and so on), can be fully "explained" by assuming that the social system as a whole is a smoothly operating, self regulating mechanism and that everything is as it is either because it serves to satisfy the immediate needs of the individual considered as a biological organism, or because it satisfies the psychological needs of the individual considered as a human being, or because it satisfies the larger social needs of the society as a whole considered as a bounded closed system. Malinowski never explained precisely how we were supposed to discover what these functional needs are, but if we judge by his practice in the matter it was a matter of pure hunch. The experienced anthropologist like himself had an intuitive understanding of what fits in with what.

The valuable part of this simplistic, self-fulfilling, thesis is the emphasis on context; it is a matter of quite overwhelming importance which many of Malinowski's successors including Claude Lévi-Strauss have tended to ignore.

Malinowski's emphasis on context was part of his insistence that social anthropology is an empirical science. It is the facts on the ground in their interrelations that the anthropologist has to understand. Malinowski was entirely scornful of idealist abstractions by which ideal models or taxonomic types of human society can be extracted from the empirical data and then used as the basis for a system of cross-cultural comparison.

During his lifetime (he died in 1942), this led Malinowski into

opposition both with his contemporary Radcliffe-Brown and with a number of his own former pupils, notably Evans-Pritchard and Fortes, who were strongly influenced by Radcliffe- Brown. Had he lived he would have found the structuralism of Lévi-Strauss with its idealist emphasis on "model in the mind" even less congenial to his way of thinking.

There is a genuine dilemma here. What is the anthropologist really trying to do?

For Malinowski the objective was to get insight into the way the individual "native" who is under observation perceived his own situation. Let me quote a passage in which Malinowski is explaining why he has presented a particular Trobriand story in context rather than simply as a piece of text:

> «Right through this account it has been our constant endeavour to realise the vision of the world, as it is reflected in the minds of the natives. The frequent references to the scenery have not been given only to enliven the narrative, or even to enable the reader to visualise the setting of the native customs. I have attempted to show how the scene of his actions appears actually to the native, to describe his impressions and feelings with regard to it, as I was able to read them in his folk-lore, in his conversations at home, and in this behaviour when passing through this scenery itself».

That was published in 1922. Many contemporary professional anthropologists would consider it an excellent summary of what they themselves are trying to do in 1985.

Most readers of Malinowski will concede that he achieves this particular objective triumphantly, but the problem then arises: what next?

If we accept Malinowski's way of dividing up the ethnographic map of the world then there are thousands of distinct cultural units each of them unique. Even if it were shown to be true that in one particular case everything fitted together like the gear wheels of a watch with no loose ends, we would not be able to assert anything in particular of any of the other cases. And even if Malinowski persuades us that he was able to understand the way his Trobrianders thought about their cultural situation, this still does not tell us anything about how human beings, in general, think about the world they live in.

Malinowski himself partly evaded this dilemma by persuading his audiences that the Trobrianders were, in some unspecified

way, prototypical of all "primitive" societies. But no one else has found this proposition in any way acceptable, so it is not surprising that after Malinowski's death less empirical attitudes to the data of ethnography came into fashion.

I have already mentioned the structural/functional approach developed in Britain by the followers of Radcliffe-Brown. In this case the underlying purpose was to develop a taxonomy of types of human society based on a comparative study of their social morphology, a concept derived from the sociology of Emile Durkheim.

The anthropological understanding of African societies was greatly advanced by applications of this type of theory but as a general thesis Radcliffe-Brown's claim that the anthropologists primary task is to classify human societies into species and types, had little in its favour. In the event, high fashion in academic anthropology quickly moved still further away from empiricism in the direction of idealist abstraction and universal generalisations concerning the operations of the human mind.

The structuralism of Claude Lévi-Strauss had its roots in the linguistic theories of Roman Jakobson which in turn derived from the linguistics of Ferdinand de Saussure. Saussure died in 1914; Jacobson, originally a Russian, was by 1950 a citizen of the United States. Structural linguistics in this style lies outside my brief. Let it suffice that it is concerned with language universals; the arguments are supposed to apply to all languages everywhere in all social situations. Lévi-Strauss' first exercises in the application of these linguistic theories to the data of ethnography date from 1945 and they became increasingly influential among anglophone anthropologists, both in Britain and in the United States, from around 1955 onwards.

In assessing the influence of Lévi-Strauss it is important to appreciate that while he had some limited experience of field research among Amazonian Indians during the period 1934-39 it was, by Malinowskian standards, fieldwork of an extremely superficial kind. In the course of his ethnographic travels he never stayed long enough in one place to gain a really intimate knowledge of the local Indian culture. In his autobiographical account of these experiences, *Tristes tropiques*, he follows Malinowski's precept of endeavouring «to realise the vision of the world, as it is reflected in the minds of the natives». But it is a confidence trick. Most experienced anthropologists would agree

that Lévi-Strauss' understanding of the native societies with which he had contact was extremely limited. If he is to be respected it is as a grand theorist rather than as fieldworker.

Lévi-Strauss' grand theories are supposed to apply to humanity in general and in this respect they resemble the fantasies made popular by Sir James Frazer. If the human mind is everywhere the same the workings of the human mind can be illustrated by any sort of ethnographic evidence drawn from any part of the map or from any period of history.

And indeed if Lévi-Srauss' work is viewed as a whole it can readily be seen that it has become increasingly Frazerian in its disregard for the contextual limitations of time and space. Lévi-Strauss'first structuralist essay, published in 1945, compared the descent and residence patterns of four primitive societies, all of them from the New Guinea region. The massive *Les structures élémentaires de la parenté* (1949), which related patterns of kinship terminology to systems of exchange, ranged north and south from Australia to the Arctic. Likewise when Lévi-Strauss applied his methodology to the analysis of myth, he at first confined himself to limited geographical contexts such as the Amazon basin or the North West Coast of America, but in later work he has abandoned all attempt to contextualise his arguments. Two stories are said to be variants of the same myth even when they come from localities many thousands of miles apart or even from quite different periods of history.

The influence of Lévi-Strauss' work on anglophone anthropology, both in Britain and in the United States, has been very great but it has been of diverse kinds. I cannot review the whole of that diversity but I want to say something about how it has interacted with the earlier influences that derived originally from Malinowski.

At first sight the two styles are diametrically opposed and, as Lévi-Strauss himself and his close associates practice their art, this is certainly the case. But in Britain and in many Universities in the United States it is still felt that no one can call himself/herself a professional social anthropologist who has not been through the traumatic initiation provided by a fairly prolonged spell of field research, working in the vernacular and studying the total way of life of a small scale community of very moderate size. This specification of the core of social anthropology is a legacy from Malinowski.

Contemporary anthropologists of this sort have abandoned

the Malinowskian dogma that cultural systems have clear-cut boundaries and that the integration of cultural systems is neat and tidy, leaving no loose ends. But they still consider that the total network of relationships among the members of a small community who are in regular face to face contact has a considerable degree of functional coherence. Social interaction between total strangers is always entirely different in quality from social interaction between neighbours no matter whether those neighbours are friends or enemies.

But "functional coherence", in the sense that I have just used the expression, need not have anything to do with the satisfaction of biological needs or the persistence of the organic totality of society in the manner postulated by Malinowski. It is simply that neighbours, unlike total strangers, bring to any situation of interaction a whole host of shared cultural values, shared symbols and metaphors, shared memories of past experiences. Harmonic echoes of such past experience then resonate into the present.

Lévi-Strauss' work, especially perhaps some of the very obscure arguments that are to be found in *La pensée sauvage* (1962), suggests how such resonances from past cultural experience may operate. That granted, it becomes feasible to draw inspiration from both Malinowski and Lévi-Strauss at the same time. In my view several of the very best field research monographs of recent years have been written by young graduate students who have been thoroughly trained in both styles of analysis. They remain functionalists to the extent that the argument is made to hang together in an integrated way and to apply a single clearly defined context of geographical space and historical time, but the integration is perceived as a transformation of models in the mind rather than as a utilitarian response to biological needs.

Although this must sound like a very Lévi-Straussian formula, the basic idea is already implicit in Malinowski's thesis that myths (which are certainly "models in the mind") are not just stories existing in isolation but charters of legitimacy for institutions that exist out there in the world within the context in which the myths are told.

But Lévi-Strauss' structuralist argument added an extra dimension to this thesis.

It is impossible to give you a satisfactory demonstration of this assertion because it would require a mass of detailed ethnographic evidence as well as some sidestepping into the

slippery world of semiotics of the sort that is so expertly expounded by Professor Umberto Eco in the University of Bologna. But I would like none the less to show you, in a simple minded way, that at least in certain respects the arguments of Malinowski and Lévi-Strauss are complementary rather than opposed.

Lévi-Srauss treats spoken language as the prototype for all types of cultural interaction. He argues that all forms of customary behaviour and all types of human artifact should be thought of as vehicles of communication. There are different levels of communication: 1) that which is manifest and conscious, 2) that which is implicit and unconscious. The methodology of structuralism is presented as a technique by which the messages which are implicit in custom can be decoded rather than simply interpreted by guesswork.

I must cut a lot of corners here, but in structuralist language any assemblage of cultural materials may be thought of as a text in an unknown language. In order to decipher the text we must compare it with other texts in the same language. In doing this we need to pay attention to differences rather than to similarities for it is only in that way that we can reach down into the grammar of what is being said.

I am afraid that that statement is so condensed that it will convey hardly anything to those who are not already familiar with the argument, but one very characteristic feature of Lévi-Strauss' technique of myth analysis is that he never attempts to interpret just one story by itself; he always takes a set of stories and treats each individual story as if it were a permutation of a common theme. The common theme is discovered by looking at the differences between the individual stories in the set. Most readers find Lévi-Strauss' performances in such matters rather like the work of a conjuror. The trick seems to work but you can't really see how it is done. The reader is not given enough ethnographic information to allow him to criticise.

In most respects Malinowski's technique of myth analysis is just the opposite. Although he often presents us with several myths from closely similar contexts and even with two or more stories which share the same characters, he always considers one story at a time. Each story is assumed to be complete in itself. When two stories are perceived as similar it is because details are alike; there is no discussion of contrasted polarities such as are

emphasised in a Lévi-Straussian analysis. The implicit meaning of the story, if deciphered at all, is determined by guesswork. Incidentally Malinowski's guesses usually assume that (in some very garbled fashion) even the most fanciful piece of mythology originated in an historical event, whereas Lévi-Strauss takes it for granted that the ultimate basis for mythology is dreamlike and derives from a mental complex of a Freudian kind.

On the other hand Malinowski provides an enormous amount of ethnographic detail and this allows the skeptical reader to form his own opinions. I personally much prefer Malinowski's style of presentation but I have to admit that the insights provided by Lévi-Strauss' conjuring tricks are more penetrating and more convincing than the simple guesswork offered by Malinowski.

I will give you just one example.

Malinowski's book *Argonauts of the Western Pacific* (1922) is concerned with the inter-island trading system that is known to ethnography as "the Kula". The text includes a large amount of material relating to the origins of the Kula, the origins of magic relating to the Kula, the origin of features of the landscape that are encountered by Kula voyagers, and so on.

Two stories which are by no means central to Malinowski's discussion concern a culture hero named Tokosikuna. The only features that these stories have in common (in Malinowski's view) is that Tokosikuna features in both, that in both stories he starts as a cripple, that in both stories he starts in the island of Digumenu, well to the East of the Trobriands and ends in Gumasila well to the South-West.

The text of the first story which Malinoswski describes as "obviously mutilated" takes up one paragraph. The text of the second extends over three pages. However the first part of this second story which is given in abbreviated form also takes up only one paragraph. From a Lévi-Straussian point of view the first story and the first part of the second story are obvious transformation of each other and pose interesting problems for analysis; Malinowski evidently saw no connection. The reason is obvious: Malinowski was looking for manifest correspondences in the two texts whereas the structural similarity is embedded in manifest inversions.

The gist of the short first story is as follows:
1) Tokosikuna is in Digumenu (evidently his birthplace). He lacks hands and feet and has to be carried into his canoe by his two daughters. The daughters and Tokosikuna go on a long range

Kula expedition, travelling to the West. They eventually reach Gumasila. The girls put Tokosikuna on a platform and go off to search for food. On their return they find him dead. They wail. Whereupon a male ogre appears who marries one girl and adopts the other. The ogre is very ugly. «The girls kill the ogre in an obscene manner» which Malinowski does not elaborate.

Here I insert some comments which were not made by Malinowski. In real life women do not go on Kula expeditions. The men are mobile, the women stay at home; here in the myth the man is in himself immobile and the women organise the expedition. A human body with no hands and feet would be a very phallic object. Although the story makes out that the dead Tokosikuna and the ogre are separate individuals, the ogre appears in the story as a substitute for Tokosikuna. The killing of the ogre by the girls is thus an act of parricide following on father/daughter incest. However according to the matrilineal ideology of the Trobrianders father/daughter incest is not a serious offence.

The gist of the second story is as follows:
2) Tokosikuna is again a native of Digumenu; he is again crippled but only partly so. He is so ugly that no woman will marry him. In circumstances which Malinowski does not elaborate, the chief of Digumenu sends Tokosikuna on a expedition to a mythical island in the Far North called Kokopowa to obtain a magic flute. He succeeds in his mission. He also obtains beauty magic, love magic, strength magic and Kula magic on his own behalf. Tokosikuna marries all the women in Digumenu.

The ex-husbands become Tokosikuna's Kula rivals. Because of his magical powers he frustrates their plots to kill him but his magic does not include the spells which make canoe lashings safe and at the end of the day he is stranded at Gumasila without passing on his magic to others.

Again I comment: In this case the women stay at home and the men go on the Kula expedition as in real life. In this case the cripple Tokosikuna is transformed into a beautiful young man near the beginning of the story when in Digumenu; in the first story he is transformed into an ugly ogre at the end of the story when in Gumasila. In this second story Tokosikuna's sexual exploits are legitimate; in the first story the ogre's exploits were illegitimate. In the second story Tokosikuna's male rivals try hard to kill him but do not succeed; in the first story Tokosikuna (and the ogre) are killed by his daughters. In Trobriand ideology a

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man's daughters are his affines. In real life the men on a Kula expedition tend to be related as brothers and as fathers and sons. Thus in the second story as in the first it is implied that (attempted) parricide is involved.

In real life, when Trobrianders make a Kula expedition towards the South-West in the direction of Dobu they always stop off at Gumasila to perform a special magic which is especially associated with that place and which is supposed to be part of the magic which was originally owned by Tokosikuna.

Now I am not going on there to attempt an "interpretation" of this mythology. My point is simply that when a structuralist methodology of a Lévi-Straussian sort is applied to Malinowski's own field materials new insights come to the surface which Malinowski had not perceived. The most important thing that anthropologists have learnt from Lévi-Strauss, who himself learned it from Saussure by way of Jakobson, is that when one cultural product is compared with another differences may be just as interesting as similarities.

And that is where I must stop. I have said on many occasions and I can say again that in my view Bronislaw Malinowski was the greatest of all anthropologists. This is not simply because of what he himself achieved but because he presented his materials in such a way that later generations of anthropologists can even now find more in his data than Malinowski found himself.

It is very proper that such a man should be commemorated.

Summary

The paper aims to consider the relationship between the social anthropology, which Malinowski considered to be his personal invention, and the kind of anthropology which is currently practised by those who see themselves as being in some degree his intellectual heirs. The main characteristics of his fieldwork methods are evaluated, as well as the relevant traits of his functionalist interpretation; a comparison between the Malinowskian approach and the Lévi-Straussian structuralism, exemplified on Melanesian examples, is also briefly sketched.

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Sommario

L'articolo prende in esame la relazione esistente tra l'antropologia sociale, che Malinowski considerava una sua personale invenzione, e il tipo di antropologia correntemente messo in pratica da coloro che si richiamano in qualche modo alla sua eredità intellettuale. Vengono dunque valutate le principali caratteristiche dei metodi applicati da Malinowski alla ricerca sul terreno, così come i tratti rilevanti della sua interpretazione funzionalista. Con l'aiuto di esempi melanesiani, viene infine brevemente tracciato un paragone tra l'approccio di Malinowski e lo strutturalismo lévistraussiano.