FRAZER AND MALINOWSKI: A CA* DISCUSSION

After agreement with authors and the publishers of Encounter for permission to reprint the discussion between Edmund Leach and I. C. Jarvie which appears below, 27 Associates were asked for CA* comment. The following responded with written comments: Edwin Ardener, J. H. M. Beattie, Ernest Gellner, and K. S. Mathur. The comments written for publication and printed in full below, were sent to Leach and to Jarvie for reply. Leach responded with the reply for publication which follows the comments.—EDITOR.

On the „Founding Fathers”*

by Edmund Leach

Between the basic absurdity Frazer attributed to primitive practices and beliefs and the spurious validation of them in terms of the supposed common-sense invoked by Malinowskis, there is scope for a whole science and a whole philosophy.

The study of man must be central for everyone but Anthropology is just another -ology. Opinion may be about evenly divided as to whether it is the study of apes or the name of an ob-scure religious sect. Even so, every now and then, a professional anthropologist becomes an international “celebrity,” and one wonders why. Of the living, only Margaret Mead has quite achieved this, but among the recent dead there are at least two others, Sir James Frazer, the author of The Golden Bough, and Bronislaw Malinowski, “who wrote something or other about sex.”

Public renown need not imply professional esteem. Contemporary anthropologists for the most part consider Malinowski to be a major figure; they decry Frazer as a mere miser of facts. Anyone who doubts this need only take a look at the two latest general textbooks of the subject. Both authors (Bohannon 1963, Beattie 1964) take for granted a whole set of Malinovskian, and Bronislaw Malinowski, “who wrote something or other about sex.”

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of his volumes. His inability to convince seems to contradict his power to convert and to inspire.

Dr. Jarvie also disapproves of Malinowski because of the way he emphasized the value of original fieldwork. Dr. Jarvie appears to be an anthropologist mangel; philosophy was his second love. He now justifies his infidelity by saying that the first lady would have been most uncomfortable. ‘Frazer the Evolutionist’ and ‘Malinowski the Functionalist’ represent the contrast between a concern with how things have come to be as they are and a concern with how things, as they are, are interrelated with one another. For Dr. Jarvie, functionalist investigations are pointless because they cannot give causal explanations of historical sequences; in contrast “the evolutionists were answering different questions from those Malinowski was interested in, but theirs were satisfactory answers to the questions they had posed themselves.” This is a surprising opinion for a follower of Professor Popper.1 What are we to make of the case?

The continuing celebrity of Sir James Frazer (Dr. Jarvie apart) is an astonishing phenomenon. There are now two quite separate one-volume abridgments of the huge thirteen-volume Golden Bough, and both apparently have a steady sale. Who are the buyers? What do they get from their reading?

From one point of view (the evolution of his world fame), the most important single fact in the career of the historical Frazer is that in 1896, at the age of 42, he married Lily Grove, a French widow, who thereafter made the enlargement of her husband’s public image her sole preoccupation. It was an outstandingly successful public relations operation, and it has contributed to the distortions of the legend. Worldly success in the form of a Knighthood, an Order of Merit, and strings of Honorary Degrees only started coming in around 1914, and it is this perhaps which has led Dr. Jarvie to imagine that in the early 1920’s, when Malinowski was in the ascendancy, Frazer was the securely established leader of his profession. That was not the case; Frazer’s personal influence was by that time insignificant. His strictly academic reputation had begun to fade before 1900. In later years he had grown renowned; he maintained a voluminous correspondence; and his books were always widely reviewed. But it does not appear that his views were highly regarded. Sometimes the style of his critics suggests that they might have been his close disciples, but this too is deceptive. The leading anthropologists of his time (including Frazer himself) were all close imitators of two much more brilliant men: E. B. Tylor and W. Robertson Smith. Frazer was an outstanding representative of the anthropology of his day, but that day had ended by 1910. For the next 15 years British historical anthropology was completely dominated by the diffusionists of Elliot Smith and W. J. Perry; as for the sociologists, they were taking all their cues from the school of Emile Durkheim in Paris. Frazer had ceased to matter.2

Frazer’s original competence was in the classics and here his skill was very great indeed. Classical erudition is common enough, but even so Frazer’s carefully edited translations of Pausanias’ Description of Greece and of Ovid’s Fasti are outstanding of their kind. The source of Frazer’s fame lay elsewhere; his colleagues were convinced that the novel use to which he applied the “comparative method” which he had taken over from Tylor.3 The first (two-volume) edition of The Golden Bough appeared in 1890. This was acclaimed on the quite specious ground that it revealed comparative anthropology [as] a serious study actually capable of elucidating a Greek or Latin text.

Classical scholars have always been frustrated by lacunae in the records, and perhaps the “comparative method” could be used to make good this deficiency.

The avowed purpose of The Golden Bough, as expressed in the first chapter, was to investigate certain classical legends and to account for them in connection with the worship of Diana at Nemi in southern Italy. The accounts are very incomplete, and Frazer agreed that there is not enough direct evidence to justify any particular interpretation. He proposed, however, to fill in the gaps by resorting to analogy. First he postulated that the Priest of Nemi was deemed to be the spouse of Diana; then, having cited examples of ritual theology from

This, I may say, is a typical example of the style which admirers find so exhilarating. In fact, of course, the “evidence” is totally irrelevant to the “conjuncture,” and it was not very long before this irrelevance came to be fairly generally appreciated. Thereafter the interest of the professional classicists waned rather rapidly.

In the much narrower field of professional anthropology Frazer’s standing was nil. British historical anthropology had ended by 1910. For the next 15 years British historical anthropology was completely dominated by the diffusionists of Elliot Smith and W. J. Perry; as for the sociologists, they were taking all their cues from the school of Emile Durkheim in Paris. Frazer had ceased to matter.

Frazer’s career as an author extended from 1884-1938. His output, excluding multiple editions of the same work, fills at least two yards of shelf space; yet in all this vast mass of print the total amount of material which represents a genuinely original contribution by Frazer himself probably adds up to only a few hundred pages. The rest consists of excerpts from the writings of others, sometimes quoted verbatim, but more often rephrased to suit the sentimental lilt which Frazer considered to be the essential quality of fine writing. Quite explicitly he thought of himself as making a contribution to literature rather than to science, and it does not seem to have occurred to him that in “improving” his sources he might also be distorting them. He was perfectly frank about his procedures. Commenting on the difference between the original quotations recorded in his notebooks and the passages which appear in his own published works, Frazer wrote (1938):

[The notebook extracts] are written for the most part in a careful hand, and I doubt whether the authors contented themselves with describing in simple language the things which they have seen or have heard reported by competent and his phenomenal Few, if any, possess that magic charm of style which, by firing the imagination or touching the heart, can alone confer what we fondly call immortality upon a work of literature.
Frazer knew better, and how right he was! Clearly, there had always been monarchy, like Dr. Jarvie, "find Frazer glorious and thrilling reading."

All the same the diligence is quite extraordinary. A doubtful "conjecture" does not become less doubtful by stating it 20 times over; but even the most sceptical critic finds himself yielding in fascinated incredulity as to the skillful scissor-work by Lady Frazer. A work of art, the opus 4 Malinowski claimed to be able to discern in the earlier of these two works the germ of his own functionalist theory.

So far as the anthropological portion of this corpus is concerned the formative period was 1883-1890. The dates are significant. 1883 was the year in which, after a notorious cause célèbre, W. Robertson Smith (who had been dismissed for heresy from his Chair of Old Testament Exegesis at the Free Church College in Aberdeen) moved to Cambridge as Professor of Arabic; 1890 was the year in which Robertson Smith was first struck down by the fatal illness which caused his death in 1894. The close association between Frazer and Smith was mutually acknowledged on numerous occasions. Smith's Religion of the Semites appeared in November, 1889, with credits to Frazer; Frazer's Golden Bough appeared in June, 1890, with dedication to Smith. The collaboration was widely recognised at the time and was remarked upon by reviewers both in The Athenæum and in Folklore. The latter considered both books old-fashioned in their appeal to primitive religion, and wondered if their author was an evolutionist bias! Even at this early date diffusionism was becoming an academic orthodoxy.

It is quite evident that in the Smith-Frazer teamwork all the inspiration and originality came from Smith. As soon as Smith's support was withdrawn Frazer's capacities were reduced to those of a voraciously diligent library mole. For the next 50 years he simply went on repeating himself over and over again on an ever larger scale, adding nothing of significance in the process. Serious social anthropologists can still read Religion of the Semites with great advantage. Frazer's works may be examined for their bibliographies; otherwise they accumulate dust.

Frazer held a life fellowship in Trinity College, but he played no part in University affairs either in Cambridge or elsewhere. His title of Professor derived from a Chair at Liverpool University which he occupied only for one year (1908). Anthropology began to receive formal recognition in Cambridge around 1898 and achieved the status of a tripos subject in 1919. But Frazer had no part in this development which stemmed from the enthusiasms of A. C. Haddon and W. H. R. Rivers. One of Rivers' first pupils was "a Mr. Brown" who in 1908 became a Fellow of Trinity College where Frazer had already been installed for nearly 30 years. This was A. R. Radcliffe-Brown who was later to be Professor of Social Anthropology at Oxford; it is impossible to discern in his work the slightest trace of Frazerian influence.

Nor was this simply a case of a prophet without honour in his own country. The platforms for anthropological debate at this period were the meetings and publications of the Anthropological Institute, the Folklore Society, and Section H of the British Association; Frazer's name seldom appears in any of these places. In 1911, the year after the appearance of Frazer's four-volume Totemism and Exogamy, the British Association held a major international symposium on Totemism under the chairmanship of A. C. Haddon; Frazer did not attend; his views were not represented; in the published report his name is never mentioned.

Frazer's strictly academic reputation seems, as I have said, to have passed its peak before 1900. That year saw the publication of the second (three-volume) edition of The Golden Bough which was widely reviewed. The anthropologists were not only appalled. Andrew Lang was positively insulting; Hartland and Haddon praised Frazer's zeal but were cautious about his theories. Ten years later Frazer had become a bore; at the tail of a long review of Totemism and Exogamy Hartland (in Man) drops into mock Frazerian phraseology and hints that the great man has become prematurely old.

Frazer could well afford this patronising disrespect by his professional colleagues, for he had other publics which were more rewarding and more influential. One of these came from the ranks of liberal-minded 'modern churchmen who felt a strong common interest in discovering the true historical origins of Christianity. For them the passages in The Golden Bough which draw attention to parallels between Christianity and other Middle Eastern cults were both disturbing and fascinating. This material had originally occupied less than 100 pages, but in response to special demand it was blown up into a separate volume (Adonis, Attis, Osiris). By 1914 this book alone took up two volumes.

Frazer's upbringing had been rigorously Presbyterian; although in later life his attitude towards established religion became increasingly cynical, his direct references to Christianity are always carefully ambiguous. As a result, The Golden Bough was treated as an
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lar of the sacrifice of the Divine King (with its uncom-
able association with Christianity) and the entanglement of this theme with vegetation gods and the magical powers which they possessed. Fertility persists throughout; but the author's main general concern is with the world-wide irrationality of custom. Huge chunks of highly elaborate and highly valued religious practices then develop out of magical techniques. When magical attempts to control the course of nature fail, the primitive mind conjures up devices of magic, should make religion un-
all clumsy means are used in the hope that the rituals will bring about a desired end. Primitive Man, being childish and ignorant, has much magic but little science. The modern European, being more adult and wiser, has less magic and more science. Re-
ligion, which is Frazer's third major category of action, is less precisely conceived. The notion of deity arises through an intellectual confusion. Primitive Man is groping after the definition of such abstract ideas as "power," "life," "fertility," "soul"; but he gets these ideas mixed up, and he fails to distinguish clearly between attributes of Man (e.g., authority, human sexuality) and attributes of Nature (e.g., vegetable fertility). Religious practices then develop out of magical techniques. When magical attempts to control the course of nature fails, the primitive mind conjures up devices of magic, should make religion unnecessary. But even in the abridged edition it takes Frazer four closely printed pages to say this and even then the argument is ambiguous. He does commit himself to the proposition that: "In short, religion, regarded as an explanation of nature, is displaced by science." Did he really suppose that religion is nothing more than "an ex-
ploration of nature"? The reader of The Golden Bough is left to guess.

But this much is clear enough: for Frazer, all ritual is based in fallacy, either an erroneous belief in the magical powers of men or an equally erroneous belief in the magical powers of imaginary deities. The overall ef-
fractional. Europeans, too, have their childish moments but, in general, the dichotomy is clear: the White man is wise; Black, Brown, and Yellow men are foolish. Frazer was writing pre-
ically at the point when European colonial expansion had reached its peak; it must have been consoling for many liberal-minded imperialists to find that the "White Man's Burden" could be justified by such detached scholarly procedures! And this may well be an important factor in the enduring popularity of the book.8

Perhaps, too, there are some who can still find pleasure in the sadoma-

6 The widely held view that The Golden Bough "explicitly sets out to discredit present-day religion" (Jarvie) derives from hostile reviews of the second edition. Volume 3, pp. 138-200 of this edition is a new section which discusses the Gospel story of the Crucifixion under the heading "the Saturmalia and kindred festivals, with special reference to which is orthogonal Chris-
tians. Frazer suggests that the gospel story of the Crucifixion is a folk record of a hypothetical Jewish festival at which a living malefactor was annually hanged to receive a ritual sacrifice.8

On the other hand, Frazer's more general thesis that the Gospel should not be inter-

7 Many who would never openly say so still sincerely believe that White superiority is a fact of Nature which depends upon a basic and intrinsic maturity of outlook. How often in the past two decades have we heard it argued that the African is not ready for self-government, that he is too inex-
perienced, too irresponsible, too ignorant...? For those who really believe this (and Dr. Jarvie gives hints that he may be one of them), Frazer must make congruent reading.

8 The knighthood came in 1914, but he gets these ideas mixed up, and he fails to distinguish clearly between attributes of Man (e.g., authority, human sexuality) and attributes of Nature (e.g., vegetable fertility). Religious practices then develop out of magical techniques. When magical attempts to control the course of nature fails, the primitive mind conjures up devices of magic, should make religion unnecessary. But even in the abridged edition it takes Frazer four closely printed pages to say this and even then the argument is ambiguous. He does commit himself to the proposition that: "In short, religion, regarded as an explanation of nature, is displaced by science." Did he really suppose that religion is nothing more than "an ex-
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them, and where customary gifts are offered to them, and whence, after the full moon is over, they are ceremonially but unceremoniously driven away.

The driving out of the spirits is a children’s ralk which Malinowski likens to Guy Fawkes day.

Frazer’s account antedates Malinowski’s. His source is a missionary, the Rev. Dr. George Brown, whose brief account (1910:144) is quite consistent with Malinowski’s longer study:

The dances and feasts lasted many days. When these were finished all the people gathered together, shouted, beat the posts of the houses, overturned everything where a spirit might be hiding, drove away the spirits and the feasts were over. The explanation given is that the spirits were thus made wealthy for another year. They had shared the feasts, had seen the dances, and heard the songs. The spirits of the yams were theirs, the spirits of the property displayed were also theirs, and they were now made wealthy. They were provided for and so they were driven out.

Frazer’s citation of this material comes in a section entitled “The periodic expulsion of evils” (The Golden Bough, 3rd edition, vol. 9, p. 134). Note carefully the modifications of Dr. Brown’s text (italics added):

When the festivities were over, all the people gathered together and expelled the spirits from the village by shouting, beating the posts of the houses, and overturning everything where a wily spirit might be supposed to lurk. The explanation which the people gave to the missionary was that they had entertained and feasted the spirits and provided them with riches, and it was now time for them to take their departure. Had they not seen the dances and heard and gorged themselves on the souls of the yams, and appropriated the souls of the money and all the other fine things set out on the platform? What more could the spirits want? So out they must go. Among the Hos of Togoland in West Africa the expulsion of evils is performed annually before people eat the new yams.

By intruding emotive words like “wily” and “gorged,” substituting “soul” for “spirit,” and juxtaposing the “expulsion of evils” by the Hos, the kindly Trobriand ancestors are adroitly converted into evil demons! So they were driven out.

Frazer’s new edition of The Dying God (this book rates as Vol. 4 of the twelve-volume edition of The Golden Bough). Likewise, Malinowski at first maintained that his studies of Trobriand garden magic fully confirmed the brilliant intuitive insights of the Master (1923). Such retrospective confirmations of hypotheses were felt to be clear demonstrations of Frazer’s genius.

But the disciples were mistaken and bemused by faith. We now feel certain that the Shilluk did not murder their Divine Kings (Evan-Pritchard 1948), and we see quite plainly that Malinowski’s view of magic is directly antithetical to that of his predecessor; for while a living people interpreted magic as an evocation of the mysterious, a procedure closely allied to religion, Frazer saw no more than a childishly mistaken attempt to achieve the technically impossible.

The trouble with Frazer is that he leaves no room for the imagination. A myth must always be a direct transcription of a rite and vice versa. If myth tells of the killing of a god-king, then the only possible origin of such a story is that an actual god-king was actually killed. The modern anthropologist, with his more immediate experience of how myth and ritual are interconnected, is much more cautious. For example, animal sacrifice is a very widespread human institution which, being irrational, must always be justified by myth. Observed in situ, two features of such sacrifice are easily recognised: firstly, the effect of the sacrifice is to improve the “ritual condition” (the state of purity) of the donor; secondly, there is a direct symbolic association between the donor and the animal that is killed. In a mythological context it serves to prove the state of his own divinity by destroying a mundane part of himself. This, of course, is a thoroughly non-rational procedure, but it is fully in accord with mythological stories which tell how “in the beginning” there was a god-king who was killed (as a human being) in order that he should become an immortal god. Some of Frazer’s “dying god stories” are accountable in this way but others may have quite a different, though equally rational, explanation. We have detailed information about a set of sacrificial rituals and the mythology that goes with them; we are certainly likely to find a structural consistency between the ritual and the mythology; but we cannot take short cuts and infer rite from myth or myth from rite in the way that Frazer tried to do. In this respect he was quite fundamentally in error.

I suppose that Dr. Jarvie might argue that it is precisely because Frazerian hypotheses have been refuted that they were worth making in the first place. No one can deny that when The Golden Bough first appeared in 1890 it caused a stir. It didn’t actually say anything which had not been said before, but perceptions of hypotheses were felt to be clear demonstrations of Frazer’s genius.
Malinowski was never seriously opposed to evolutionism though he was always willing to have a dig at the more preposterous form of conjunctural history postulated by Lewis Morgan and Robert Briffault. Dr. Jarvie seems to imagine that Malinowski's formal adoption of a "functionalist" creed meant that "he" thereafter he evaded all attempt to grasp with the logical analysis of historical change. The facts are entirely otherwise. Not only is his posthumous book Freedom and Civilization (1947) thoroughly evolutionist in tone but nearly all the writings of the last five years of his life (see Malinowski 1945) are concerned with problems of developmental process ("culture change").

Finally, we may note that whereas Frazer's "ideas" ("conjectures"), which Dr. Jarvie so admires, were only produced so as to force the ethnographic records into Frazer's determinist mould, Malinowski's theory of fieldwork, which Dr. Jarvie so despises, corresponds very closely to that of the Austrian philosopher Popper, an ideal scientist. When Dr. Jarvie says "you cannot collect facts without a theory," he is quoting Malinowski verbatim. Frazer thought exactly the opposite.

This does not mean that what Malinowski said corresponds exactly to what Malinowski did; nor does it mean that either Malinowski or Professor Popper is correct about the way that scientists actually achieve their results. It is simply that by his own criteria Dr. Jarvie ought to judge Frazer's methodology deplorable. But then I am writing about Frazer and not "Frazer," and there's the rub.

Bronislaw Kaspar Malinowski was of Polish aristocratic origin. His father was a professor of philology at the University of Cracow, where Malinowski himself obtained a Ph.D. in Mathematics and Physics in 1908. The reasons which then led him to abandon an assured scientific career for the uncertain favours of "sociology" are obscure, but after spending nearly two years at Leipzig working with Karl Bücher and Wilhelm Wundt, Malinowski moved to the London School of Economics where he came under the influence of Westermarck, L. T. Hobhouse, and C. G. Seligman. It was the last named who enabled him to find the financial support which, in 1914, took him to Australasia. He remained there for the next six years.

Of this period two full years were spent on the Trobriand Islands in Eastern New Guinea, a further six months being devoted to shorter spells of fieldwork among other primitive groups. Malinowski returned to London in 1920, and over the next two decades built up an outstanding reputation as a polemical writer and speaker.

His literary output was substantial but not vast; by far the greater part of it is devoted to the description and analysis of various aspects of life in the Trobriand Islands. Malinowski's style is vivid and full of colour but he was often careless, so that his writings provide many easy targets for the hostile and pedantic critic. It is this written work which provides the grist for Dr. Jarvie's attack, but it needs to be stressed that during his lifetime Malinowski's main academic influence was through his teaching. Frazer's contribution to learning is to be discovered from his books; he passed on nothing by his social contacts; he had no pupils. With Malinowski, it was the other way around. He was a dynamically powerful personality, a "charismatic leader" who aroused intense emotional feelings of love and hostility among all those with whom he became closely associated. What he taught to his students was partly partly recoverable from what he wrote in his books, and it is no doubt on this account that Dr. Jarvie's "Malinowski" is so completely unrecognisable.

Like Frazer, Malinowski had several different publics. The reputation which made him a celebrity was quite different from that which gave him fame and notoriety among his professional colleagues.

The professional reputation was directly tied in with the unique quality of his field research which had been of a quite unprecedented intensity. No professional anthropologist had ever before spent two full years studying a single tribal group, actually living in a native village and sharing the native way of life. Malinowski's Trobrianders are living in a village with a formal set of customs; they are living human beings; they are villagers engaged in all the intricacies of village and domestic life. In his ethnographic monographs Malinowski was concerned to demonstrate two things: (1) that the Trobrianders' social life, at the ordinary domestic level, is based on entirely different assumptions from our own (e.g., Trobrianders deny that a child is genetically related to its mother's husband); and (2) that the patterns of customary behaviour which correspond to this different set of assumptions form a viable set. The quaint customs of these people, which Frazer would have judged to be palatable evidence of their childish ignorance, are shown to make logical, adult sense. They "make sense" because they are mutually consistent with each other, and also with the framework of cognitive ideas through which the Trobrianders view their environment and their social world. These ideas, Malinowski thought, are in themselves no better or worse than those which we employ ourselves. Some of them, of course, are scientifically false. It is untrue that a child is genetically unrelated to its father; but our own social assumption that men are in all respects the physical and intellectual superiors of women is no better.

Hunting, the practice of each society makes sense in its own setting. It is neither true nor false, neither good nor bad, neither wise nor ignorant. Humanity is everywhere the same:

When you enter a new cultural setting, the behaviour, individual or collective, of any new type of human beings seems strange, unmotivated, irrational, in short incomprehensible. You learn the language, you gradually adopt the strange habits and the new points of view—and imperfectly what was alien becomes familiar and you feel at home in what recently had been an exotic milieu. The universally human running through all the variations is a common measure of comprehension and adaptation.

...Even in such cases as eating of human flesh, underdone beef, or plum pudding, playing golf, running amok, and the practice of the hound and sickle, the attempt to survey the psychological raw material of the pursuit, can assume a certain diversity of taste in human beings, and define the pursuit in terms of the universally human.

Malinowski's unqualified acceptance of the doctrine of "cultural relativism" has a vinegary taste for those who retain a lingering belief that one can make humane value-judgments and that the course of man's history suggests to us what they are; but his passionate insistence that technological sophistication implies neither moral superiority nor higher intelligence is still embarrassingly relevant.

In 1965, when most of us are prepared to recognise "primitive people" as qualified human beings, Malinowski's programme, thus stated, does not seem very daring, but in 1920 it was unorthodox in the extreme.

Frazer, as I have suggested, had supposed that the savages, whom he had never seen, were simply grown-up children, and Malinowski himself started with Frazerian assumptions which he had never fully abandoned. In 1922, he asserted that "natives commonly as well as individually never act except on traditional and conventional lines," and 20 years later he sentimentalised over 'the lowest primitives, the living representatives of archaic man" among whom "war does not occur" and "a somewhat higher level...the world order of savagery" where "cannibalism, head-hunting, human sacrifice or scalping" is only a ritual game. But this kind of argument which stresses the "otherness" of the primitive is completely at variance with his own major premise which insisted that the society of any primitive tribe of the present day is a
normal society of fully adult rational human beings, who simply happen to run their affairs in a different way from ourselves:

The most important thing to realise is that primitive man makes full use of his knowledge wherever he can. You must discard the notion that the savage is a child or a fool, a mystic or a nincuppoop. I have seen the savage hunter at work: he knows his animals and their habits; he is familiar with the properties of his weapons, the strength of his spear and the flight of his boomerang. I have trusted myself to savage sailors in their frail craft over the dangerous seas and under trying conditions. They understand wind and weather, stability and tides, in a truly reliable, that is, in a scientific, way. It is only because he is able to observe correctly and to think clearly that, with his simple tools and limited cooperation, primitive man can master nature as well as effectively as he actually does....

Malinowski had difficulty in coming to terms with his own evolutionist assumptions, but his more immediate problem was to deal with the current orthodoxy of academic anthropology. The current vogue was not evolutionism, but so-called “diffusionism” in the exaggerated variant propounded by Sir Grafton Elliot Smith. In the pre-Malinowski era, all anthropologists had thought of themselves as engaged in the reconstruction of prehistory. If you assume that savages are stupid automatons, bound in the chains of unmemorial tradition, you can also assume that “ Customs” are imperishable artifacts, as hard and enduring as flint tools and sherds of pottery. You can then set about reconstructing history from the data of anthropology by exactly the same procedures as are adopted by an archaeologist in reconstructing history from the data of an excavation.

Such an assumption may seem naive, but to a certain kind of academic mind it is essentially “ sound,” factual, scientific. It was precisely on this basis that anthropology was eventually accepted as an academic discipline of university status. Indeed it is because scholars of the 1900-1920 period thought it sensible to treat customs as though they were potsherds and old bones that modern social anthropologists must still often share an uncomfortable ménage à trois with the prehistoric archaeologists and physical anthropologists! In the older universities, the psephologists are as much at the same situation. It is always quite respectable to study the behaviour of rats under “laboratory conditions”—that’s science. To study human beings leading ordinary lives is mere frivolity.

In the early years of this century, a number of leading British anthropologists had a deep interest in psychology of the human sort. Rivers, who was, with Haddon, the founder of Cambridge academic anthropology, and Seligman, who had much the same role at the L.S.E., were both professionally qualified medical psychologists; this seems to have had little effect on academic sentiment. Rivers and Seligman themselves kept their “psychological” and “ethnological” interests sharply distinguished. The former were part of experimental science, the latter a part of prehistory. Consequently, Malinowski’s first problem was how to get his subject recognised at all. He wanted to discuss the sociology of a primitive society. Who would listen? How could he persuade the academic world that it might be scholarly to treat “ savages” as adult human beings rather than fossilised survivals from a bygone age?

The convention of the 1920’s was to think of “ customs” as free-floating entities which move about from place to place independently of the human groups to which they belong. On this basis anthropology had become a study of the “distribution of customs” and the human beings were left out of account. Malinowski made a direct frontal assault on this mode of thought. In order to reaffirm that anthropology is the study of Man and not the Study of Custom (in isolation from Man and his group) he devoted the whole weight of his analysis upon the relation between culture and biology. Where his predecessors had been satisfied to write learned monographs on the distribution of string figures or the design of pipe stems, Malinowski disgusted everybody by discussing the pleasure of lice-hunting and varying styles in copulation.

It was this last form of shock tactic which finally established his reputation as a “celebrity,” though there was a preliminary more reputable and more academic phase.

Malinowski’s first major ethnographic monograph was Argonauts of the Western Pacific, published in 1922 (Frazerian title; preface by Frazer; dedication to Seligman). Its central theme is the Trobriand system of ceremonial exchange. It was a truly revolutionary work which is still standard reading for undergraduate anthropologists. In France it was treated as a work of sociology and became the basis on which Marcel Mauss constructed his celebrated Essai sur le don, a study of the way in which reciprocal and obligatory gift-giving comes to reflect the structures of social relations.

Malinowski’s “functionalism,” which was built up around the thesis that we can only understand social institutions if we take account of the fact that they satisfy the needs of living human beings, was really a development out of Mauss’s theme, though the biological twist which Malinowski gave to his “principle of reciprocity” was quite alien to Mauss’s more structural (even mathematical) train of thought.19

Argonauts was favourably and intelligently reviewed in The American Anthropologist (by Gifford), but in England the going was not so good. More ignorant of Mauss, or more in Love, Nature was complimentary but treated it as simply another ethnography; Man passed the book to an American (F. R. Barton) who headed his notice “Sociology” but largely missed the point:

The book not only gives in picturesque detail the visible aspect of the various scenes and ceremonies pertaining to the Kula, but sheds also much light on the psychological mechanism on which the institution is based.

Malinowski must have felt a desperate need for a wider and less conventional audience. In the following year his essay “The Problem of Meaning in Primitive Languages” appeared as an appendix to The Meaning of Meaning by C. K. Ogden and I. A. Richards, a book which has had a lasting influence in many fields of thought, including philosophy, linguistics, psychology, and literary criticism. This work gave Malinowski the sort of general intellectual public he was looking for.

In the same year (1923) he contributed two pieces to Nature. The first was a long review of the one-volume edition of The Golden Bough (Dr. Jarvis should study this item; Malinowski’s praise of Frazer is exuberant and unqualified). The second was a “Letter to the Editor” entitled “Psycho-analysis and Anthropology,” which was the opening broadside in a long series of publications on the psychology of sex in “savage” society. The Trobriand Islanders happen to have a matrilineal organisation, and Malinowski claimed that in these special conditions the mother’s brother and the mother’s husband share between them the social role played by the father among Europeans. In such a situation the psycho-analytic concept of the Oedipus complex needs modification. Malinowski’s presentation of this thesis was intentionally provocative, and Ernest Jones’ hostile reaction (1925) was both predictable

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19 Mauss (1923-24); Lévi-Strauss (1950). Malinowski’s view that the “functional method” was something specif and peculiar to his own brand of anthropology developed about 1928. He recognised the German ethnologist, Richard Thurnwald, and A. R. Radcliffe-Brown, as fellow “functionalists”; but his considerable debt to the French sociologists was never stressed.
and welcome.\(^{11}\) Psychoanalysis was at that time very much in vogue, a fashionable novelty among “advanced” radicals. Malinowski’s controversial challenge to the orthodox Freudian position put him right at the centre of London intellectual life.

In these very unanthropological circles it was immediately assumed that the Trobriand’s freedom from sexual restraint could provide moral lessons for ourselves. Before long, Malinowski’s views became slogans of progressive education and the Trobrianders’ sex life was being accepted as a model of virtue by such disparate propagandists as Havelock Ellis (1929) and Bertrand Russell (1929).

Malinowski delighted in the ensuing furor and went out of his way to create a sense of outrage. The Sexual Life of Savages is simply an account of Trobriand domestic organisation and is much less libidinous than the average modern novel. But its title assured it a place in Old Compton Street. Lawfully bound, it is still to be found alongside sealed-up versions of Fanny Hill and the Marquis de Sade. The contemporary notices of this work are fascinating. Man, the official journal of the anthropological profession, ignored it altogether. In the highbrow weeklies the reviewers consistently missed all the anthropological point. They were interested only with astonished disbelief that Trobriand girls could fornicate without getting pregnant and that Trobrianders are ignorant of the biology of procreation. The reviewers’ scepticism was justified; but the book itself remains a work of major scientific importance; it was the first (and is still the best) detailed study of family life in a matrilineal society.

The renown which flowed from this kind of publicity led to countless requests for public speeches, broadcasts, and journalistic articles. Malinowski willingly acted as his own populariser. At each repetition, the story became more simplified and more distorted. In the end the Trobriander merged with Rousseau’s Noble Savage. Sexual laxity became a virtue in itself. This was all nicely in tune with the ethic of D. H. Lawrence, but it could hardly be claimed as a contribution to family science.

And there, perhaps, we can stop. For though Malinowski’s rating as a professional anthropologist does not rest on his contribution to sexology, this was and is the context of his public celebrity.

Crudely summarised in this way neither Frazer nor Malinowski appears particularly laudable. Both men seem to have been more concerned with the plaudits of the gallery than with the pursuit of truth. Both made a cult of the outrageous. Frazer by cynical comments on religion, Malinowski by challenging English sexual morality. But in all other respects they at first seem notably different. Judged by what they did, what they wrote, and the way they set about propagating their views, they appear as polar types, and in the mythology of modern undergraduate anthropology (as well as in the pages of Dr. Jarvie’s book) they are just that: Frazer who deals in items of custom drawn out of context from here, there, and everywhere, and takes no account at all of individuals; Malinowski who constantly emphasises the importance of the total social context, and never for a moment forgets the essential unities of time and place and dramatic personae. Yet in an odd way the interests of the two men were very much the same, and at their grandest, they spoke in much the same language.

For both, the field of greatest professional renown was that of magic and religion and primitive psychology. There were important technical points of disagreement. Malinowski understood the expressive nature of ritual behaviour in a way that Frazer did not, and he is inclined to merge magic with religion rather than magic with science. He did not consider it a sign of intrinsic inferiority that a man should believe in mire as. Malinowski and Frazer both accepted Robertson Smith’s thesis that belief (dogma, myth) is to be understood as a manifestation of ritual; but they use this insight very differently. Frazer writes as if myth and ritual were interchangeable—if he finds the record of a myth, he “conjectures” as to the nature of the corresponding ritual, and vice versa. Malinowski sticks firmly to the observable evidence; the myth is a “charter for social action,” but only if demonstrably so: no guessing. Yet so far as the Common Reader is concerned the similarities are more striking than the differences.

Frazer was eager to forensic the psychological Primeval Man, thought of as a unity. In all Frazer’s writings the immense diversity of human culture is treated as a manifestation of just a single element—the simple-minded childishness of the savage, his ignorance, his lack of understanding of cause and effect. And why not? If the psychologist, says Frazer, “the power of mankind.” Frazer was surely justified in developing a synthetic picture out of multiple parts. The parts come differently from all corners of the globe and have no chronological unity, but if Primitive Man is a unity then the diversity of source material cannot matter. We should not understand the Priest of Nemi by looking at what goes on in the Maldive Islands.

Malinowski went about things the other way round. He concentrated exclusively on one small group of “savages” and looked at them under a sociological microscope. But he too, like Frazer, postulated a psychological unity of mankind and gradually step by step found himself talking, not about the Trobrianders in their uniqueness, but about Primitive Man in his generality. And why not? Why should a Trobriander Islander be deemed any more, or less, typical of the human race than the Priest of Nemi? And here, perhaps, we begin to see the roots of their popularity. “Typical Man” may not be a very satisfactory kind of concept from the scientific point of view, but it is surely of interest to all of us.

For the professional anthropologist, Malinowski has other virtues (and other vices) than those which I have considered here; and I need not pursue further my disagreements (and occasional disagreements) with Jarvie. Malinowski made contributions to many fields which Frazer never touched—language, kinship, primitive law, and economic relations in particular. Anyone with close knowledge of the subject must concede that Malinowski has left his personal mark on contemporary anthropology in a way that Frazer has not. He was a much less trivial scholar than my cursory and biased comments might suggest. But that is not the point.

My problem at the outset was to consider why, every now and then, an eminent anthropologist should rate as a “Celebrity.” What is there about a Frazer and a Malinowski (or a Bally and a Mead) which gives public notoriety? What is there about a tourist and a Maldive Islands which gives public fame as well as professional distinction? My account suggests an answer.

Frazer and Malinowski in their different ways were both prepared to make sweeping generalisations about human nature itself. Frazer could never have seriously expected that his general reader would be terribly interested in what did or did not go on at Nemi in 200 B.C., and the reader of Malinowski can get along very well without worrying as to whether the Trobriand Islands lie North or South of the Equator or East or West of longitude 180\(^\circ\). Both authors are really talking about Mankind, i.e., about you and me. It is because each of us can recognise with Dick Smith his generality. And why not? Why should a Trobriander Islander be deemed any more, or less, typical of the human race than the Priest of Nemi? And here, perhaps, we begin to see the roots of their popularity. “Typical Man” may not be a very satisfactory kind of concept from the scientific point of view, but it is surely of interest to all of us.

There are many lesser, more pedantic men who in some ways can be considered much better anthropologists. But the public which has given these two a special accolade is not at fault.
In Defence of Frazer*  

by I. C. Jarvie

Dr. Edmund Leach’s article on Frazer and Malinowski (Encounter, November 1965) is readable, packed with interesting and not easily accessible material on the history of anthropological studies, and I’m sure I’m not alone among Encounter readers in having learnt a lot from it. However, it is difficult to accept Leach’s rich matter just as he presents it. In fact there are flagrant contradictions in several of the main points of the article which would have to be smoothed out before it would be possible to tackle his view of Frazer and Malinowski as a possible account of “what actually happened.”

Leach’s view is that Frazer was “a voracious library mole,” “a mere miser of facts” who took what few ideas he had from Tylor and Robert-son Smith; Malinowski was a vivid and colourful writer and an inspiring teacher, but “was often careless” and neither Frazer nor Malinowski appears particularly laudable. Both men seem to have been more concerned with the plaudits of the gallery than with the pursuit of truth.

Why they received the “plaudits of the gallery” is no doubt a serious sociological problem, but it is not so important as their status as seekers of the truth. This especially with respect to Frazer, who gets grossly mistreated in Leach’s article.

Leach and I agree that Malinowski was intellectually important and academically influential. Also, that Frazer was once academically influential and is no longer so among anthropologists. Yet we disagree sharply, it seems, in that I think Frazer was and still is intellectually important, and that he is underrated at present, especially among anthropologists; while Leach thinks he was not intellectually important and his academic influence rightly disappeared after 1910. Clearly we disagree less about the sociological facts of Frazer’s reputation than about values—that is, about the true evaluation of Frazer’s contribution to anthropological knowledge. And though it seems clear that this is our disagreement, I do not think it is clear to Leach, or that it becomes clear from his article.

Against my view that Malinowski ousted Frazer from the leadership of the anthropological world in a father-killing revolution, Leach holds that Frazer ceased to be the anthropological leader by 1910 at the latest, and moreover, Malinowski thought the world of Frazer. Thus Malinowski neither needed to overthrow Frazer nor was inclined to do so.

It is true that Malinowski thought the world of Frazer: “In... (1923) he contributed two pieces to Nature. The first was a long review of... The Golden Bough. (Dr. Jarvie should study this item; Malinowski’s praise of Frazer’s work is exuberant and unqualified.)” Such praise in no way tells against Malinowski trying to oust Frazer. The point of calling it “father-killing,” of course, was that Malinowski’s attitude to Frazer was ambivalent: one loves one’s father. But all this would be unnecessary if Leach could show Frazer was not the leader of the anthropological world and so did not need ousting. In trying to argue this Leach merges the questions of the real or scientific status of a man and that of his academic influence or reputation. Indeed his arguments seem directed at both points at once. Leach discusses Frazer’s ideas, but purely in terms of their reception or lack of it by the academic gallery. Perhaps Leach identifies the plaudits of the academics with the truth, but that is not clear. I shudder to think what it could do to some reputations. At any rate, it would make all discussion superfluous which aimed at showing that a man is academically underrated. What is clear is that nowhere in Leach’s article are there any criticisms of any of Frazer’s theories, or of Malinowski’s theories, or of any theories whatsoever. All that is to be found is talk about Frazer inspiring awe, evoking no respect, failing to inspire enthusiasm, and becoming a bore. Now Frazer may be regarded as the victim of “what actually happened,” the “plaudits of the gallery,” which is another interesting sociological or, if you like, anthropological problem—not necessarily connected with popular fashion—but I am sure Dr. Leach would agree that Frazer’s standing as a research worker, a seeker after truth, should not be mixed up with his academic reputation. Unfortunately, Leach does not carry out such a separation in his article:

It does not appear that his views were highly regarded... Frazer propounded in all three quite different theories of the “origin” of totemism. These he prints side by side in the 1910 volumes, but no fellow anthropologist has ever expressed any marked enthusiasm for any of them... [1900] saw the publication of the second... edition of The Golden Bough... the anthropologists were notably cool... and Haddon praised Frazer’s zeal but were cautious about his theories. Ten years later... Frazer had become a bore... [All my italics.]

Leach is in effect asking us to join him in decrying Frazer as a scholar and seeker after truth because his colleagues did so. But that they did so is less important than why they did so. Had they any good criticisms of his theories which showed them to be not true? A man who presents three theories side by side deserves critical discussion, not dismissal as a miser of facts, etc.

Leach frequently announces that Frazerian theory was an anthropological error. Again, so what? A man can be most important in the development of a subject even if all his theories were wrong, provided they stimulated others, or that he opened up new problems which were taken up by others.

Leach’s suggestion that the interest of classicists “waned” after The Golden Bough seems on the face of it an error. Frazer certainly heavily influenced Jane Harrison and, through her, Gilbert Murray. And certainly the work ofCornford, Burnet, and Finley, not to mention Arnold Toynbee and George Sarton, owes considerable debts to the approach pioneered by Frazer. Their problems, the way they set them, and their general comparative approach remain Malinowskian. In fact Frazer is so much part of classics now that he is no more mentioned than Boyle is at meetings of the Chemical Society.

There is also, I am afraid, a serious contradiction in Leach’s account of Frazer’s academic reputation among anthropologists. Leach tells us:

Frazer’s personal influence by that time [the early 20’s] was insignificant. His strictly academic reputation had begun to fade before 1900... Frazer had ceased to matter. Frazer played no part in university affairs either in Cambridge or elsewhere... In 1911... the British Association held a major international symposium on “Totemism”... Frazer did not attend; his views were not represented; in the published report his name is never mentioned.

All this “evidence” is very weak, especially when set against the con-tradictory testimony (1926) of the leading young anthropologist of the time: B. Malinowski. Far from thinking Frazer was no good, of no importance, and a bore, Malinowski is quoted by Leach as saying about Frazer:

No sooner had I begun to read this great work than I became immersed in and enslaved by it... and became bound to the heritage of Frazerian anthropology.

And later Leach cites Malinowski (1942) as speaking of Frazer’s “enormous creative influence” and “power to convert and inspire.” How can we reconcile Leach’s view that Frazer was without influence after 1910 with Malinowski’s words in 1923, 1926, and 1942?

Leach has several ways of getting out of this glaring inconsistency. Malinowski could be
lying. Or, Leach could claim that Malinowski did not appeal for the classicists and anthropologists; or that he was "mislaid and bemused by faith." Perhaps Malinowski was taken in by Lady Frazer's "outstandingly successful public relations operation"? Whatever we conclude, Leach's account of "what actually happened" in the history of anthropology must be read critically and not taken at face value. It is internally inconsistent and Leach's own evidence is inconsistent with it. A minimum requirement of an historical theory is that it is self-consistent and consistent with the known facts. And this does not by any means exhaust the contradictions and doubtful assertions in Leach's article. Malinowski is said to hold that "you cannot collect facts without a theory" and later is said to stick "firmly to the observable evidence; the myth is a 'charter for social action,' but only if demonstrably so: no guessing." Either Leach or Malinowski (or both) doesn't know that theories are guesses.1 At another place it is this theory, as unsupported by any substantial evidence ("quite erroneously") that Frazer was an unqualified atheist; yet later Leach says Frazer's "direct references to Christianity are always carefully ambiguous." Was the "careful" ambiguity intended perhaps to conceal that Frazer was a theist?

After all this discussion of contradictions and errors, I should like to end by saying something about Leach's solution to his main problem. The problem was the popularity of Frazer and Malinowski. It might be explained by publicity, as Leach mentions; or by sex, as Leach also mentions; but his main solution is the universal human appeal of their generalisations about human nature: here, perhaps, we begin to see the roots of their popularity. "Typical Man" may not be a very satisfactory kind of concept from the scientific point of view, but it is surely of interest to all of us. Now if we look at this we discover an anthropological conjecture about what interests "all of us," i.e., "Typical Man"—unfortunately "not a very satisfactory kind of concept from the scientific point of view." In his main solution to his main problem Leach condemns himself out of his own mouth.2

Reply*

by Edmund Leach

To reply to Dr. Jarvie's reply: obviously we could keep this up for ever. My opponent has changed his front. He now wants to draw a subtle distinction between Frazer's academic reputation and the "true evaluation" of his contribution to anthropological knowledge. Only Dr. Jarvie, of course, can make that evaluation. This is not where we started. Dr. Jarvie's book begins by representing Frazer as the head of an academic lineage which included Haddon, Rivers, Seligman, Spencer, Gillen, Malinowski, and Radcliffe-Brown, and by affirming that the two last named, at a date unspecified, "tried to overthrow the influence of clever dons like Frazer."

In contradiction I affirm that this is a completely ludicrous misrepresentation of the facts of anthropological history. Malinowski himself, who had read The Golden Bough while convalescing in a German hospital around 1908, always claimed that he found Frazer's writings inspiring; Spencer and Gillen, whose principal researches were conducted in 1896, were Frazer's disciples but not his pupils; the influence of Frazer on any of the other persons named seems quite minimal. Dr. Jarvie complains that I fail to show that Frazer "was not the leader of the anthropological world" around 1923. Surely the onus is on Dr. Jarvie to show that he was. Faced with such a task an historian would presumably consult the anthropological journals of the period. This I have in fact done, and Dr. Jarvie has not. I have been astonished to find how infrequently Frazer's name is mentioned at all. The Golden Bough is cited now and again as a convenient source book but my resolve cracked over Leach's hint (footnote 1) that I hint that I am a white supremacist.

In fact I agree with, and everything in my book is consistent with, Leach's assertion that Malinowski's "passionate insistence that technological sophistication implies neither moral superiority nor higher intelligence is still embarrassingly relevant." I don't find it embarrassing, but I do find Leach's conclusion that relativism is the only alternative to white supremacist quite illogical. Man has made a little progress and some societies are better than others, e.g., a society without slavery is ceterus paribus better than one with it.

Leach's other travesties of my views are less important and I would only mention that my main criticism of Malinowski was not that he was "assailing the work of his predecessors" or that he was "encouraging a non-starter"; it is internally inconsistent and Leach's own evidence is inconsistent with it. A minimum requirement of an historical theory is that it is self-consistent and consistent with the known facts.

1 There are even such theoretical guesses to be found in Leach's article, especially about Lady Frazer who "no doubt" reissued Frazer's books as part of a campaign to keep in well with the Establishment and not to defend my book. But Frazer's theories hardly get a mention.

2 In this reply I have tried to confine myself to discussion of Frazer, and of Leach on Frazer, and not to defend my book. But Frazer's conclusion that relativism is the only alternative to white supremacist is still embarrassingly relevant. I cannot believe that Dr. Jarvie is serious in suggesting that I should offer to readers of Encounter "critical discussion" of views ascribed to Frazer; his third theory never received so much serious attention. Dr. Jarvie is quite free to resurrect it, but the main reason for this neglect was that already in 1910 Goldenweiser had published his massive Totemism, an Analytical Study, which persuaded almost everyone that the whole basis of Frazer's work on the subject was illusory. "When approached analytically, totemism, which in its elements presents nothing unique or specific, tends to disappear, partly or wholly." It is precisely because professional anthropologists already understood this in 1910 that Frazer's "influence" thereafter was so slight.

As to the classicists, if Dr. Jarvie read the stuff instead of talking about it, he would readily see that while Jane Harrison's Prolegomena (1903) is heavily influenced by Frazer, Themis (1912) takes all its anthropological cues from Durkheim. Gilbert Murray and Corfond in this respect followed Jane Harrison, and their successors rejected the liaison with anthropology.

Vol. 7 · No. 5 · December 1966
altogether. If M. I. Finley is to be rated a Frazerian I give up.

The fact that Malinowski read The Golden Bough in hospital in 1908 and was thereafter grateful for the experience is no more evidence of Frazer's enduring influence than is the assertion of a colleague of mine that it was the accident of his coming across a copy of the abridged edition of The Golden Bough in a prisoner-of-war camp in 1943 that led him to become an anthropologist!

“Guessing.” Yes, theories are guesses but there are profitable guesses and useless ones. It is useless to guess about historical events which could in no circumstances ever be demonstrated, which is what Frazer habitually did. Malinowski made guesses about how the observable facts might be expected to fit together; he never made guesses about the facts themselves. Dr. Jarvie can guess that Frazer was either an unqualified atheist or a qualified theist: that is his affair. Apparently he is prepared to let us all. The first proposition is demonstrably false, the second is meaningless and untestable.

Comments

by Edwin Ardener*

Oxford, England. 27 IV 66

Dr. Leach's stimulating account of Frazer and Malinowski is both a comment on Dr. Jarvie's view of the relationships between them and an attempt to explain the non-academic vogue of these two figures. Leach demonstrates clearly that (1) Frazer was too isolated to have represented any kind of “establishment” that Malinowski overthrew, and (2) Malinowski admired Frazer. These points are quite enough to demolish Dr. Jarvie's version of the past (1964:43, 170-76), especially as his presentation of it as a Freudian primal situation was not a very good fantasy to start with (Ardener 1965:57). But in the course of documenting conclusion (1), Leach lays so much weight on Frazer's lack of influence that conclusion (2) begins to appear inexplicable. It is a little surprising to find Leach quite so preoccupied with the opinion held of Frazer by his later contemporaries; especially by stressing Frazer's absence from the conference circuits of his day. Having conclusively demonstrated that there was no Frazerian academic establishment, Leach seems very close to using this evidence in another way: to suggest that its absence was in itself evidence of Frazer's lack of any ambition at all.

In the Victorian-Edwardian period, as Leach himself hints, eminent scholars could present their points of view either to fellow-specialists or, over their heads, straight to the educated public. The latter approach has been used into our own days by many in history, philosophy, psychology, and sociology, as well as in our own subject. The procedure may now appear to some to be sub-professional, but even at least in 1964 Leach's day this was not a judgment, had it been made, to strike chill to the heart. Perhaps Frazer's very isolation played some role in the attitude of his contemporaries to him. In any event, their academic fates have hardly been kinder than his.

This raises the more general question of the influence of predecessors upon successors in subjects like our own. Quite often the “influence” appears to be in the reverse direction. The role of students is mentioned by Leach: certainly in the short term the student-less scholar is vulnerable to oblivion. Conversely, students may preserve the memory of some longer than they deserve. A succession of students may not, however, offer the same advantages. Among academics, the desire to reflect their ideas off some earlier figure seems to be deep-seated. The disillusionment with one outmoded master leads regularly to the selection of another, previously neglected: Bloomfield gives way to Sapir, or ancients like von Humboldt are rediscovered. The academic tradition is reconstructed with the zeal of termites building new tunnels in a damaged termitarium. This may shape the whole convention of attribution and reach the point where, we are told, authors cite people they might have been influenced by, but were not (Malinowski 1965:80). History (1964:24) ends an account of his views on ethnographic method with the note: “Since this paper was written in the field, the citations to the literature that would ordinarily have been given are regrettably but necessarily lacking.” It is almost as if these reflectors may be added like ornaments to a Christmas tree. This is all nice, fit enough, no doubt, and performs a supra-bibliographical function. Dr. Jarvie is in part reflecting his ideas off Frazer; so, probably, was Malinowski. Malinowski's extreme homage to Frazer seems a little out of place if the climate of opinion was as Leach describes it. Malinowski probably had his “Frazer, too,” which was certainly not Leach's, nor entirely Leach's. Perhaps Malinowski was speaking to Frazer over the heads of the professionals, thereby laying claim to a place in that “westerly sun.”

Leach’s remarks concerning the appearance of “celebrities” are of great interest. In which case the public publicity is required, be it through Lady Frazer or through Old Compton Street: but we have, of course, no guarantee that those whose names are thus propagated have made any permanent contribution beyond the publicity itself. There is clearly a real difference between Frazer and Malinowski in the quality of their contributions—on almost all counts Frazer is and must be the lesser—but as a public figure, in the time of Malinowski, it was barely Frazer's equal. It would be enlightening to enquire into the points in the history of an academic discipline at which successful publicists arise. There is probably a tendency for the epigonoi of publicists to be sober men, and perhaps for those of the sober to be flamboyant. On such immanent generational cycles, however, it may be that there are times when the “appeal to the public,” if successful, is of great service in itself; perhaps to re-establish the intellectual claims of a discipline, or to break a tendency to inanition within it. Frazer was evidently successful in the first of these aims, Malinowski in both. If the lay view of anthropology is only slightly less archaic than Leach suggests, and if even Jarvie's polemic account appears quite old-fashioned, it is no surprise that a new appeal to the public—from all directions—seems to be under way. When the dust finally settles and the names the public remembers are examined by a later generation, there are no grounds for dissatisfaction if it turns out that (in Wellington's approving words on the Order of the Garter) “there's no damned merit in it!”

by J. H. M. Beattie*

Oxford, England. 7 III 66

It is difficult for me to comment on Leach's article, since I entirely agree with almost everything he says. I wonder, though, if he doesn't push a little too far his concluding theme that makes Frazer and Malinowski (and Margaret Mead) “celebrities” is their willingness “to make sweeping generalisations about human nature itself” (p. 567). Others, less celebrated, have not hesitated to do this. Is it not rather, or at least also, that they have written what they have written with style, and so have made it intelligible and interesting? If the work of modern social anthropologists with something to say were more readable, it would be more read.

On a point of ethnography, do we now feel so certain that the Shilluk did not “murder” their divine kings (p. 564)? Evans-Pritchard (1962:76) wrote of probability, not certainty, 1

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1 They may have some demographic basis, related to the structure of professional reward systems and career cruft (Ardener and Ardener 1966:309).
and if Lienhardt's informants (1961: Chap. 8) are to be believed, it seems likely that the neighbouring Dinka sometimes buried their "masters of the fishing spear" alive, at the latter's request. Symbolism is no less symbolic (and no more pseudo- or proto- "scientific") when, as often it is, it is translated into action.

by Ernest Gellner*

London, England. 19 iv 66

Leach's article is readable, witty, elegant, and stimulating. But by no means all of what he says is true. There is surely a profound self-directed irony, perhaps conscious and intended, in Leach's complaint that both Frazer and Malinowski "made a cult of the outrageous."

The curious thing is that in this attack Dr. Leach is not so much outrageous as outraged against. He is evidently outraged by Dr. Jarvie. In his attempt to destroy Jarvie's account, he makes a number of categorical assertions, some of which are untrue; some of these can be shown to be untrue on internal evidence. This is of some importance, as Leach claims that he wants to set the record straight. His tone of authority is well buttressed by the remarkable apparatus of scholarship—no fewer than 11 footnotes in what appeared, after all, in a literary and political rather than an academic journal. This will show the journalists how we in the universities check our assertions. But not everything supported by 11 footnotes is necessarily true.

Leach attacks Jarvie first of all for allegedly over-rating the standing of Frazer within professional anthropology. "Frazer's works may be examined for their bibliographies; otherwise they accumulate dust" (p. 562). This does not seem to be the view of Leach's colleague Meyer Fortes, who asserts (1959:8) that "... sooner or later, every serious anthropologist returns to the great Frazerian corpus"; nor does Elie Evans-Pritchard, who writes (1965:27): "Frazer is... the best known name in anthropology and we owe much to him and to Spencer and Tylor." References to Frazer on 22 separate pages of the latter book suggest that Evans-Pritchard's copies of his works are not gathering dust.

But worse than this is the blatant contradiction within Leach's own argument. Having first challenged Jarvie's account of Frazer's influence, Leach puts forward a second charge: Malinowski was not, contrary to Jarvie's view, reacting against Frazer. In his eagerness to press the second charge, Leach blatantly contradicts his first and establishes that Frazer did matter:

... Malinowski at first maintained that his studies of Trobriand garden magic fully confirmed the brilliant intuitive insights of the Master. Such retrospective confirmations of hypothesis were felt to be clear demonstrations of Frazer's genius (pp. 563 and 564).

Malinowski's first major ethnographic monograph was Argonauts of the Western Pacific, published in 1922 (Frazerian title; preface by Jarvie: J.J. (p. 566). In the same year (1923) he [Malinowski] contributed two pieces to Nature. First was a long review of the... The Golden Bough (Dr. Jarvie should study this item: Malinowski's praise of Frazer is exuberant and unqualified) (p. 566).

These quotations do not establish what Leach wishes to establish, namely that Malinowski was not reacting against Frazer: for the greater the hero slain, the greater hero the slayer. This was what the French call a boomerang compliment. It is strange to find an anthropologist making a comparison, which, though latent, is so blatantly latent. The quotations do clearly establish, however, that Leach's first charge against Jarvie cannot be true: Frazer manifestly was not considered insignificant within the anthropological profession, least of all by Malinowski.

In a footnote (no. 9) Leach does admit that Malinowski later "said flatly" that Frazer's "theory of magic... is untenable." Leach criticises Frazer's use of evidence; but his own use of evidence from Malinowski is amazingly selective, for he does not quote from the same work a few pages further on, under the significant heading "Whither Anthropology?" (1960:211):

In this critical assessment of Frazer's work we find that in many ways he embodies a past epoch in anthropology.... With many of its defects and with all its qualities... Frazer's... Frazer's... explanation... and his explanations by survival are at times not acceptable.

All this does not sound either as if Leach had not mattered—he is said to embody the anthropological past—or as if Malinowski was not reacting against him—for he is said to embody the past. But the passage does not leave much doubt about who embodied the anthropological future.

Malinowski's reference to Frazer's evolutionism, his "explanations by survival," bring me to Leach's third and most interesting charge against Jarvie: the claim that it was not evolutionism, but diffusionism, which was the academic orthodoxy against which Malinowski set himself.

Once again, Leach himself underscores his own error. Speaking of Malinowski (p. 566) he says, "How could he persuade the academic world that it might be scholarly to treat 'savages' as adult human beings rather than fossilized survivals from a bygone age?" (Italics mine.) Had he been reeling against diffusionism, his concern would have been with rejecting interpretations in terms of borrowings rather than in terms of survivals. No doubt, that was also part of his concern: but as Leach unwittingly indicates in this sentence, which is meant to characterise Malinowski's principal intellectual posture, it would have been so, it was not his primary concern. Leach also says (p. 561): "For the next 15 years [after 1910] British historical anthropology was completely dominated by the diffusionist views of Elliot Smith and W. J. Perry; as for the sociologists, they were taking all their cues from the school of Emile Durkheim in Paris." Leach not merely fails to give any evidence for his amazingly strong claim of "complete domination," but even fails to make clear what it could conceivably mean. Does it mean that everyone alive at that time and concerned with the question of social development at a respectable academic level was a diffusionist and not an evolutionist? This is demonstrably untrue. Does it exclude the possibility that whilst there was a tendency to accept diffusionist corrections, anthropology retained the underlying and far more important evolutionist Problemstellung? Leach does not tell us. The reliability of his sweeping generalisation, looking at what he says about sociologists (without even bothering to go into the crucial question of what the distinction between sociologists and anthropologists could mean in pre-Malinowski days): They were taking all their cues from the school of Emile Durkheim.

"Did Hobhouse take all his 'facts' from Emile Durkheim? A recent authoritative essay on Hobhouse (Ginsberg 1966:xiv) suggests the very opposite: Hobhouse has not commented on Durkheim's ethical or sociological theory. But though, as we have seen, they were largely interested in social development and their method was that of sociology, they differ seriously on other matters.... Durkheim and Hobhouse further differ in the degree of their interest in the study of social development.... [Hobhouse] was concerned... to trace the..." (Within diffusionism, the notion of "borrowing"

Vol. 7 . No. 5 . December 1966

571
of course plays a role similar to that of "survival" in evolutionism: it is the contrast between as it were horizontal or vertical intrusions in time.)

Another point where Leach himself makes it plain that Malinowski must have been reacting primarily to evolutionism is the one at which he observes (p. 565): "In the pre-Malinowski era, all anthropologists had thought of themselves as engaged in the reconstruction of pre-history." Quite so. But what is the connection between defining one's subject in terms of pre-history, and diffusionism? Diffusion is something which can also be studied in the present: indeed it can be studied in the present better than anywhere else. If Malinowski taught anthropologists not to conceive their subject as "the reconstruction of pre-history," then it must have been because he taught them to disregard the evolutionist question, and not just the diffusionist correction of its answer. There is an essential connection between evolutionism and pre-history, but there is no such connection between diffusionism and pre-history. The refutation of diffusionism, in as far as it was refuted, was a corollary of the attack on evolutionism, and not something independent. It was only relevant to the kind of diffusionism that is parasitic on evolutionism. (In any case, neither doctrine was refuted; the question was reformulated so as to make both of them of marginal interest.)

No doubt that, within the wider class of those who agreed in rejecting evolutionism, Malinowski also fought a minor internal battle against diffusionists: but there can also be no doubt about which conflict he considered to be the more important one. Of course, it is possible that at times the internal, subsidiary struggle may have preoccupied Malinowski more than the more general and important struggle, precisely because he took the latter for granted, just as at times an internal conflict between two clans of one tribe may obsess the participants more than a wider struggle between the tribe as a whole and outsiders. Leach does not establish that even this was the case, but it is possible. Even if it were, it would in no way undermine the point that within the general logical structure of Malinowski's thought, the rejection of evolutionism was far more important than that of diffusionism.

The situation is somewhat complex: I suspect that the logical segmentation did not correspond to the social segmentation. Logically, evolutionists and diffusionists are sub-clans of the same grouping, and Malinowski rejected this grouping as a whole. Within the grouping, however, diffusionists may have been members of the same wider grouping as Malinowski, in as far as they were both labelled "anthropologists" and as such opposed to another wider grouping, "sociologists," and, with time, evolutionists simply came to be classed as sociologists. In as far as this may have been the case, and in as far as rejection of close cousins may be a more passionate matter than the rejection of distant ones, Malinowski's preoccupations may at times have been as Leach claims. But this fact, if such it is, is not one of very great importance.

Hence, it is possible that there is an element of tautology in the claim that Malinowski was, within anthropology, opposed primarily to diffusionists. The tautology comes in as a consequence of a new demarcation of subject boundaries, a demarcation to which Malinowski's work and position had itself contributed a great deal. Malinowski had done much to help establish an autonomous subject of "social anthropology." Social anthropologists in his tradition conceived of themselves as studying the material or less immediately physical aspects of activities and institutions in primitive societies investigated by fieldwork. In consequence, anyone interested in a wider sweep and documentary evidence tended to be classified as a sociologist. An anthropologist who generalises is said to be "doing sociology," but this notion is in contrast: the major group of social anthropologists also had to have another frontier, vis-a-vis those still interested in tools, skulls, or customs in isolation.

This frontier was necessary in as far as social anthropologists had ceased to be interested in tools as such, as opposed to the light they might throw on a subject of vital importance. But the people who were still interested in tools, etc., needed some name. They weren't sociologists, and they weren't "social"; so they were just "anthropologists" or "physical" or "cultural" anthropologists. At the same time many of them could hardly avoid being tainted with diffusionism, for it is hardly possible to take an interest in, say, a tool without being interested in who used it, and hence in its distribution and the sequence of the distribution. For these reasons, it is arguable that "within anthropology" diffusionism became the enemy, simply because other (and more important) kinds of enemy were reclassified as being outside anthropology. But by itself tells us nothing about which enemy, of the enemies available, was the most significant, logically, socially, or emotionally. All the evidence seems to me to point to the fact that evolutionism occupied this position. (I am not saying that the new rules of demarcation sketched in above were applied rigidly, neatly, or consistent-ly. In as far as there was a tendency towards their application, however, the conclusion which Leach wishes to establish creeps in as a corollary of a new and half-established terminological convention; but, as such, it fails to support the point which he wishes to make.)

Leach had already made his point about the alleged importance of diffusionism in his review (1964) of Jarvie's book. He offers no evidence in support of it, other than the authority of his own assertion. Ironically, he once again offers conclusive evidence against it. Speaking of Malinowski's anthropological training, he observes: "[Malinowski] moved to the London School of Economics where he came under the influence of Westermarck, L. T. Hobhouse, and C. G. Seligman." The three male recruitment of these three to the diffusionist, non-evolutionist school will come as a surprise to the historian of ideas, not to mention the surviving followers of Hobhouse. Thus evolutionism was not absent from Malinowski's intellectual cradle. Nor did it disappear at any subsequent time in the course of Malinowski's life. On the contrary, Hobsonian evolutionism continued, without interruption, to be taught at the London School of Economics by Morris Ginsberg till long after Malinowski's death, as Leach knows full well.

Leach offers a far more convincing picture of the background to which Malinowski was reacting in one of his earlier works (1957:120): Malinowski entered the British academic field in 1910. At this period . . . mechanism, materialism, linked with . . . doctrines of . . . progressive evolution, still held the field, but was under serious attack. . . . in social studies, the evolutionist comparative method had achieved a kind of massive fertility in the vast tomes of Frazer and Westermarck and the only real stimulus was coming from the writings of Durkheim and his school, whose empirical content was often extremely low. Diffusionism with a superficial emphasis on material facts seemed likely to become the dominant voice in the near future.

What facts have come to Leach's notice since 1957 which have converted him from the belief, implied here, that diffusionism would have become the dominant voice had Malinowski not entered the academic field in 1910 to the belief he now stresses that it was indeed the dominant voice which Malinowski had to fight against? In any case the climate as he found it, the ideas and assumptions he destroyed, are more significant for understanding him than some residual enemies whom he only mopped up later, after his principal ideas were already formed. What is true is that, after Malinowski had made his impact, evolutionism...
Malinowski was reacting to diffusionism and not to evolutionism because the former had replaced the latter, this wouldn’t be a point of very great importance: what Malinowski had destroyed was not so much the answer which evolutionists had offered, but the question. (It is for this very reason that he was prepared to allow that the ‘evolutionary principle’ would never become completely rejected. It survives as an answer to a subsidiary question. We are not returning to Genesis or Aristotle!) This rejected question, evolutionism shares with diffusionism. Faced with human institutions and customs, both evolutionism and diffusionism ask, “Where did they come from?” One seeks the answer in evolution through time, the other in diffusion through space. Malinowski’s importance hinges on the fact that he showed that this was a relatively unimportant, secondary question. The superiority of anthropology over sociology during the period of his greatest influence was due in part just to this. In fact, both diffusion and evolution occur: but the explanatory concepts of evolutionism and diffusionism are too weak to explain either of them.

Leach also himself provides further evidence against his own doctrine that Malinowski was an evolutionist after all. He can cite ‘Malinowski’s unqualified acceptance of the doctrine of “cultural relativism”’. (p. 565). If you accept cultural relativism in an unqualified manner, you cannot also be an evolutionist. Evolutionism means the ordering of societies or institutions in some kind of evolutionary order. I am not suggesting that Malinowski was never interested in such a point, but is legitimate for Leach to appeal selectively to various aspects of Malinowski’s thought according to his need?

Of course, Leach is admirably candid about his own principles of exegesis. What Malinowski actually wrote is apparently not adequate evidence:

Malinowski’s style... was often careless, so that his writings provide many easy targets for a hostile and pedantic critic. It is this written work which provides the grist for Dr. Jarvis’ attack... Frazer’s contribution to the “problem of diffusion” was only one example of an attack on diffusionism by Malinowski, whilst it abounds, ironically, in unintended evidence of Malinowski’s rejection of evolutionism. Apart from this concrete failure to illustrate, let alone document, his case, Leach also fails to perceive that the principles of segmentary organisation also apply to doctrines. “Opposition” is not an absolute notion. X may be opposed to Y in a number of different ways: the opposition may be within a wider agreement, and may be submergeable, so to speak, by a wider and more general opposition in which X and Y are on the same side of the fusionism-diffusionism dichotomy. This is the theory of privileged access is not only a bit improper, but also unreliable.

In brief, Leach’s attack on Jarvis depends on three claims: (1) the academic unimportance of Frazer at the relevant time, (2) the acceptance by Malinowski of Frazer and of evolutionism, and (3) the replacement of evolutionism by diffusionism prior to Malinowski. Leach does not make out his case on any of these points; rather, it is contradicted by conclusive evidence available both inside and outside his article. It is curious that an article which is meant to demonstrate that it was diffusionism, and not evolutionism, that Malinowski was reacting against, does not contain a single example of an attack on diffusionism by Malinowski, whilst it abounds, ironically, in unintended evidence of Malinowski’s rejection of evolutionism. Apart from this concrete failure to illustrate, let alone document, his case, Leach also fails to perceive that the principles of segmentary organisation also apply to doctrines. “Opposition” is not an absolute notion. X may be opposed to Y in a number of different ways: the opposition may be within a wider agreement, and may be submergeable, so to speak, by a wider and more general opposition in which X and Y are on the same side of the fusionism-diffusionism dichotomy. This is the theory of privileged access is not only a bit improper, but also unreliable.
and not just a kind of fellow-member, with evolutionism, of a wider class of views which he was really concerned to reject (and of which class evolutionism was in fact by far the more important member). To make out his case, Leach would also have to exclude this possibility; this I do not believe can be done, because this possibility corresponds to the facts of the case.

Apart from his attack on Jarvie, Leach is puzzled by the popular success of Frazer. He considers various explanations of the impact both of Frazer and Malinowski, but the one he finally offers is both circular and vacuous. Nothing is explained by saying that an anthropologist who makes a great impact does so because we see "Mankind" through his work. Naturally, to say that an anthropologist makes an impact on non-specialists means that his work illuminates humanity. What other impact would you expect an anthropologist to have? Leach goes on to say: "It is because each of us could recognize in their pages the savage within us that we feel the excitement of insight, the unverifiable validity of a statement of genius." Perhaps it is unkind to take this statement as more than a mere decorative purple passage, though Leach himself is very harsh on poor Frazer's stylistic efforts in this line. Does Leach really believe that "statements of genius" are generally unverifiable and valid? What can all this mean?

Curiously, he does not comment on one obvious but interesting and ironic factor in Frazer's later popularity. Frazer himself was a rationalist; he believed the various myths which he collected to be false, and he offered explanations of how these false beliefs came to be held. His official explanation was of a rather intellectual kind, in terms of false associations or the misapplication of proper reasoning. But other explanations, of which he was possibly unaware, are implicit in his arrangement of his material. One thing of which he was unaware and of which he would have disapproved is that the analogies which he assembled lend themselves better to a Jungian interpretation than they do to his own official explanation. He was no proto-Jungian, but he can be posthumously or retrospectively enlisted on the side of Jungian and similar interpretations.

Why should myth come in such suggestively similar patterns, if error, presumably random, is responsible? Frazer is, for this reason, a godsend to those many who seek a new, depth-psychological foundation for religious belief. Those who find support for religious belief (or indeed, who find the very meaning of religious belief) in the fact that its objects are somehow allegedly the archetype furniture of the human mind, find their illustrations, if not their evidence, in Frazer. Does not Frazer now live on through the eyes of T. S. Eliot?

 Reply

by K. S. Mathur*

Lucknow, India. 25 Iv 66

I admire Dr. Leach for his very frank "evaluation" of Frazer and Malinowski. Leach's pen is like a surgeon's knife—sharp, incisive, and to the point. His thesis is that "scientists become "celebrities" because they make sweeping generalizations about human nature itself; that is, they, provide man with a mirror in which he can look at himself. Grudgingly, Dr. Leach admits this, though he does not make a secret of his feeling that "celebrities" are not necessarily good scientists. Now, this may be so for other sciences. For anthropology, the situation is different. The science of man, to be the science of man, must be able to generalize for humanity as a whole.

Apart from this very competent defence of his thesis, Dr. Leach's paper is devoted to a brilliant and scathing criticism of Frazer and a surprisingly vigorous defence of Malinowski apparently in rejoinder to Dr. Jarvie. Two-thirds of this 12-page paper is devoted to the dethronement of the "divine king" that Frazer has become in the eyes of the English-reading public. Methodically, Frazer is pulled down, till at last we see him as a weak man who lacked imagination, scientific precision, conviction of thought, and scholarly dedication, and who in fact depended for some of his brilliant ideas on "accepted" masters like Robertson Smith.

Malinowski was all that Dr. Leach makes him out, and perhaps more. What I wonder is if Frazer was as weak and poor as Dr. Leach has made him out to be! Frazer's greatness lies not in his theoretical sophistication but in the richness of his material, his command over factual data from a vast area of space and time, and his masterly ability to classify and interpret them. Should we not be more generous in our evaluation of our pioneers—view them in the context of their times and the knowledge available to them? (After all, anthropology is warmly human rather than coldly mechanical.) This would probably save considerable useless controversy and promote better understanding among the scientists of man.

by Edmund Leach*

This is a typical piece of British academic in-fighting. May I remind readers of CURRENT ANTHROPOLOGY that the topic is the anthropological merits of Frazer and Malinowski, not the scholarly merits of Jarvie and Leach. Since I always find it hard to stand in exactly one posture for any great length of time, it may be relevant that the original Encounter article was in proof in 1964 and that the Jarvie-Leach correspondence reproduced above was in proof in Summer 1965: it is now May 1966.

Gellner accuses me of (a) being too scholarly and (b) being un scholarly. It is hard to be virtuous. His remarks about my senior colleagues are ingenious and hardly support his own posture of scholarly detachment. Evans-Pritchard (1965) is a collection of essays about the history of anthropology. Frazer's views are treated with explicit contempt (pp. 27-29) and though his name crops up quite frequently it is always as a minor member of an evolutionist crowd—e.g., on p. 56, where the reference is to "McClenan, Robertson-Smith, Wundt, Frazer in his earlier writings, Jovons and Freud." What is significant here is that Evans-Pritchard should have been so contemptuous of Frazer when he first wrote these essays in the early 1930's.

The principal theme in Gellner's remarks seems to be that Malinowski must have felt bitterly hostile to the evolutionists and that therefore my assertion that Malinowski was himself an evolutionist of a sort must be wrong. He writes learnedly about how the category formations of anthropologists and sociologists ought to have evolved, but he does not consider how in fact they did evolve. Gellner can denounce and gesticulate as much as he likes, but the evidence remains there in print. British anthropologists at the beginning of the century thought of diffusionism and evolutionism as radically opposed creeds: new recruits to that subject had to join one side or the other, and Malinowski, a pupil of Wundt joining the L.S.E., naturally became an evolutionist. He never really thought of himself as anything else, though he came to think of evolutionist interests as old-fashioned and rather uninteresting. But his hostility to diffusionism, as he understood it, was bitter and relentless;
evidence for this is to be found throughout his writings, for example in Malinowski (1926), Elliot Smith et al. (1928), Calverton and Schmalhausen (1930:311n), and Malinowski (1945:19). Gellner and Jarvie both accuse me of inconsistency, arguing that when I show that Malinowski wrote in praise of Frazer, I demonstrate that Frazer was still influential among serious anthropologists in 1923 and later. But my critics have missed my point. Malinowski always admitted to having been enthralled by _The Golden Bough_ when he first met up with it in 1908. He wrote a laudatory review of the abridged edition in 1923 at the point in his career when it was important for him to attract a non-anthropological audience. The fact that he had persuaded Frazer to write a preface to _Argonauts_, rather than choosing the academically much more appropriate Seligman, also indicates Malinowski’s eagerness, at this stage, for public rather than anthropological acclaim.

Gellner cites a passage from Malinowski (1944) as evidence that Malinowski saw Frazer as the “embodiment of the past,” i.e., the slain Father. Since the passage in question comes from a commissioned obituary, it is hardly surprising that Frazer should here have been singled out as “the last survivor of British classical anthropology”! But I cannot see what bearing this has on Jarvie’s thesis.

What do I mean when I say that diffusionism was dominant in British anthropology between 1910 and 1925? I mean what any ordinary reader of “a literary and political rather than an academic journal” would think I mean. Gellner’s pretence that he doesn’t know is a philosopher’s quibble.

Gellner’s discussion of Hobhouse is a red herring but must be answered. Hobhouse was a classical evolutionist whose anthropological attitude never varied from that specified in Hobhouse (1906). His later philosophical works appear to show accommodation to Durkheim. Hobhouse, Wheeler, and Ginsberg (1915) is a work of technical innovation in anthropology, the ancestor of the modern H.R.A.F. analyses, but in contemporary British anthropology its influence was nil. That Hobhouse post-1910 carried little anthropological weight in England is shown by two details: (1) Malinowski (1913), written while the author was a student in a department of which Hobhouse was joint head, never mentions Hobhouse; and (2) after 1915 Hobhouse himself lost interest in the British anthropologists; his substantial essay, intended to be a supplement to Hobhouse (1906), was published in German (Hobhouse 1928-29) and did not appear in English until long after his death.

Gellner insists that Malinowski was preoccupied with a struggle against evolutionism and historical reconstruction generally; this just isn’t true. At no point did Malinowski see his functionalism as radically opposed to evolutionism. The posthumous Malinowski (1947) is entirely evolutionist in tone, and in another posthumous work (1944:16-17) he writes:

Nevertheless the general principle of evolutionary reconstruction is the special foreword of the third edition of _The Sexual Life of Savages_ (1932), where in a section entitled “An evolutionist’s recantation” he claims that between 1927 and 1929 he abandoned his previous evolutionist assumptions. But even here (1932:xxii) he merely says that “I have ceased to be a fundamentalist of evolutionary method. . . . I have grown more and more indifferent to the problems of origins.”

The fact is that Gellner is simply misinformed. Whatever he may like to think is the logic of the case, diffusionism did become the dominant viewpoint in British anthropology very soon after 1910, and it was regarded as something radically opposed to evolutionism. The critical event was W. H. R. Rivers’ “conversion” in 1911, confessed by Rivers himself in his Presidential Address to Section H of the British Association of that year (Rivers 1911). If Jarvie is still searching for slain Fathers, he should consider Rivers as a possible victim. His death in 1922 while at the peak of his academic career and President of the Royal Anthropological Institute was certainly opportune from Malinowski’s point of view, though it may have inhibited the subsequent vitriol. Malinowski’s later hostility towards Radcliffe-Brown (who was Rivers’ first anthropological pupil) had multiple causes, but was influenced by Malinowski’s extreme distaste for Rivers’ style of kinship analysis, of which some of Radcliffe-Brown’s work seemed to him a continuation.

After that, it is nice to be able to say “thank you” to Beattie. I fully concede his point about Shilluk king-killing, but it is important that the verdict remain “not proven.” Young (1966) goes too far in the opposite direction, seeming to imply that we aren’t interested at all in whether divine kings were really killed or not.

I like Ardenner’s point about Frazer’s personal isolation having some relevance for the creation of the Frazerian mystique. His point about the role of pupils links up with some of Gellner’s remarks. The recollection of pupils is not likely to be an accurate record of “what really happened,” but nor is the written word. If we are interested in history we must take what evidence there is at hand, written, verbal or any other. But if we draw a sharp distinction between public fame and influence on academic successors, as in Frazer’s case I think we should, then his lack of pupils is most relevant.

To Mathur I would merely reply that it is not a question of generosity of evaluation. The greatness of Frazer “in the context of his time” is not in question. I put that time around 1890; Jarvie puts it about 1925.

Some parts of the Jarvie-Leach correspondence, which appeared in _Encounter_ have been reprinted here and since Jarvie is willing to leave it at that, so am I.

References Cited


DISCUSSION AND CRITICISM

On “Homo habilis”

by Tadeusz Bielicki

Warsaw, Poland. 21 xii 65

With reference to P. V. Tobias’ article, “New Discoveries in Tanganyika, Their Bearing on Hominid Evolution” (CA 6:391-411), I should like to make the following remarks on the taxonomical validity of “Homo habilis”:

1) A statistician confronted with the question whether two samples differing in a certain parameter came from a single population or from two different ones always starts with the first of these two alternatives as his working hypothesis; and it is only if calculations show that, odds are heavily against it that he accepts the observed disparity as non-accidental.

An analogous principle should, I think, underlie the palaeontologist’s reasoning. Whenever one deals with fossil fragments which (a) evidently belonged to animals closely related anatomically and ecologically and (b) come from the same site or region and the same horizon and thus probably represent sympatric forms, the assumption of co-specificity is the null hypothesis from which the palaeontologist should begin his taxonomic analysis. This hypothesis can be rejected only if it is shown that the differences are definitely too large to be attributed to any of the 4 categories of intra-specific variation, i.e., to sexual dimorphism, age differences, polymorphism, and polytypism. I think that this has not been shown so far for the Bed I hominids from Olduvai (nor, incidentally, for the Swartkrans and the Djetis hominids).

2) Since all the post-cranial bones from Bed I are valueless for taxonomic assessment, and since the cranium of Hominid 13 (by far the most complete find of those on which Leakey, Tobias, and Napier originally based the definition of the new taxon!) has now been removed from the “habilis” to the erectus grade, the whole case for H. habilis rests upon: one very fragmentary and crushed calotte, one juvenile mandible, one very mandibular fragment with a single root, and a few loose teeth—of which, judging from the descriptions, no more than three are analyzable. Thus, the mere quantity of this material indicates the need for extreme caution in arriving at far-reaching conclusions. If populations rather than individuals are to constitute the elementary units of classification—and this principle is the cornerstone of modern systematics—then the setting up of new species on the basis of material so scanty and incomplete can hardly be justified, particularly when the differences between the newly proposed taxon and other contemporary forms are, as in this case, either minute or doubtful.

3) According to Tobias, one of the strongest arguments for the generic distinctiveness of “H. habilis” from the australopithecines is the large cranial capacity of Hominid 7, estimated at 680 cm. But the method of estimating had to be very indirect and involved several possible sources of error (e.g., errors in reconstructing each of the two parietal fragments from the many smaller pieces; errors in reconstructing the complete biparietal wall; errors in estimating the B/A ratios in other fossil crania; measuring errors). Under these circumstances, can the resultant figure be regarded as anything more than a rough guess? One should not forget that even in crania much more com-

576 CURRENT ANTHROPOLOGY


