ANTHROPOLOGY'S MYTHOLOGY

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I DEEPLY appreciate the honour you have paid me in inviting me to address you on this occasion. In speculating on your reasons for extending me this courtesy I find it improbable that you wish me to expound on any of the views which I have expressed in print. Your familiarity with them is evidenced by the near unanimity with which anthropologists in this country have seen fit to disagree with them. I assume, therefore, that what you would like to hear from me are the mature reflections of a senior colleague on the present status and future prospects of anthropological science.

There are two aspects of our subject on which I may speak with a certain measure, if not of authority, at least of experience. The first is descriptive ethnography; the second is theory. I estimate that I have read the descriptive literature on at least 2,000 societies in all parts of the world and at all levels of cultural complexity. I have not only read these sources but have specifically analysed their content on a variety of subjects for comparative purposes, and in this way have become familiar with the work of scholars of many countries and many persuasions. I have taught courses in sociological and anthropological theory for nearly fifty years, and have devoted most of my intellectual energies over this period to an attempt to arrive at a personally satisfying theoretical formulation for the study of human social and cultural behaviour.

While I have been innovative to a very modest degree, I have mainly sought for guidelines in the work of others. I have been exposed to, have adopted or adapted from, and have actively participated in every major theoretical movement in anthropology during my lifetime. I was early indoctrinated strongly in a late version of nineteenth-century evolutionism, and was subsequently heavily influenced by Rivers, Kroeber, Malinowski, Radcliffe-Brown, and several of the senior members of this audience. I have been, at different times and in different combinations, a cultural anthropologist, a functionalist, a structuralist, a comparativist, and even a historical anthropologist. I speak, therefore, as an older informant on the culture of anthropology who is still actively practising his craft and still vitally concerned about its future.

During a lifetime of personal involvement in anthropology I have also attempted to keep a watchful eye on the writings of our colleagues in the other disciplines of the social sciences. Many of these disciplines are more methodologically sophisticated than we are, but they still depend far too heavily on information gathered by others, such as census data or the records of commercial and financial transactions. Or they conduct rapid and relatively superficial surveys, or poll for ephemeral attitudes. They construct sophisticated theoretical models, put them to the test with elaborate schedules or questionnaires, and publish the statistical results. But rarely, if ever, do they present the actual behavioural data they have collected.

Only anthropologists, in general, seek consistently to penetrate social relationships in depth and to view behaviour empathically from the point of view of the participants themselves. For this purpose we have developed ever more adequate field techniques, beginning with the admirable 'genealogical method' of Rivers and culminating in a series of methods derived from linguistics and cognitive psychology, among them those of so-called 'ethno-science'. Above all, anthropologists have recognised the obligation to publish their descriptive findings for the benefit of their colleagues in the future. As a result, the ethnographic record accumulates in steadily increasing quality as well as quantity, at a rate which is the despair of the beginning graduate student.

Of the several thousand peoples on the face of the earth there are few that remain completely undescribed, and for hundreds of them the ethnographic descriptions are admirably rich and comprehensive, representing a vital legacy to be transmitted to our students and our students' students. This cumulative body of factual data provides a firm foundation for the social science of the future. From this perspective I have no hesitation in characterising the corpus of descriptive ethnography which we have produced as by far the greatest achievement in anthropology—the crowning glory of our discipline.

If ethnography is the glory of our science today, it is worth remembering that it was not ever thus. Many of the towering figures of early anthropology—men of the stature of Durkheim, Frazer, Graebner, Mauss, Marett, Schmidt, Spencer, Sumner, and Tylor—did little if any fieldwork of their own, leaving this task largely to explorers, missionaries, and colonial administrators, whose findings they merely collated and analysed in the effort to make scientific sense of the life-ways of other and simpler peoples. This original lofty disdain for achieving a first-hand understanding of other peoples on their own terms was not dispelled until the present century—actually within

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my own lifetime. It was Franz Boas and his students who first made field research the *sine qua non* of professional standing in anthropology, and it was Bronislaw Malinowski and his students who took the next step and insisted on the necessity of prolonged participant observation, including the acquisition of the indigenous language. It behoves us today to be generous in according credit to the men who created and established our standards of field investigation. What does it matter, after all, if we now find some of their theoretical constructs less than satisfactory? Viewed in perspective, they were dedicated scholars and great teachers, and we have learned as much from their errors as from their wisdom.

The late Bronislaw Malinowski used to maintain that good fieldwork depends upon sound theory—by which he implied, of course, his own institutional approach. I would take issue with him in this respect. The quality of ethnographic description naturally improves with innovations in field techniques, especially when these involve prolonged immersion in the life of the people studied and the acquisition of their language. But it seems to me, on the basis of my exposure to the literature, to depend remarkably little on the specific theoretical orientation of the observer. Ethnographic accounts of outstanding quality have been produced by anthropologists with the most diverse theoretical preoccupations, as well as by missionaries such as Junod and Sahagún, by colonial administrators like Rattray and F. E. Williams, and even by political exiles like Bogoras and housewives like Lorna Marshall. What counts is not so much the theoretical orientation of the fieldworker, for this can probably produce as many blind spots as genuine insights, but rather such qualities as intellectual curiosity, a real interest in the people studied, sensitivity, industry, and objectivity. When two or more ethnographers with such qualities have studied the same people, their independent accounts usually corroborate each other to a highly gratifying extent, or at least provide a basis for a reinterpretation of their findings. In short, my respect for the ethnographic record and for those who have produced it is unqualifiedly high, and grows apace with each year.

I feel far less happy about the body of theory we have produced to account for the diversity of social behaviour which we have observed and recorded. Indeed, the more I have sought to expand my own intellectual perceptions by assessing and attempting to incorporate the views of others, the more frustrated and dissatisfied I have become. Very slowly, and very reluctantly, I have come to the conclusion that most of the principles we have advanced to order our data bear little resemblance in kind to the systems of theory that have been developed in the older physical and biological sciences. They have far more in common with the equally complex, but unverified and often unverifiable, systems outside the realm of science which we know as mythology, or perhaps as philosophy or even theology. It is for this reason that I have chosen to designate them in this paper as 'anthropology's mythology'.

I realise that Evans-Pritchard has anticipated me in this pessimistic conclusion, and I fully share his sense of disillusionment. I nevertheless do not agree with him that anthropology can never become a genuine science and must reconcile itself in perpetuity to the status of a purely humanistic discipline. I merely feel that our predecessors, for all their merits, made a grievous and fateful error in formulating their approach to the explanation of man's collective behaviour, and thereby committed us to a mass of derivative errors from which we have never as yet been able to extricate ourselves. My purpose in this paper is to indicate how we were led astray and also hopefully, how we can correct our errors and redirect our course into channels which give promise of immensely more fruitful results in the future.

I wish at this point to acknowledge a deep sense of indebtedness to Meyer Fortes. I value him as a long-standing personal friend, as a magnificent ethnographer, and as a seminal theorist whose suggestive interpretations of social organisation have immensely stimulated my own thinking, even though, as we shall see, I have not found them convincing in the long run. It is his recent book, *Kinship and the social order* (1969), which gave me the final impetus for a thorough rethinking of the fundamental issues of anthropological theory, the results of which I shall attempt to convey in this paper.

Fortes distinguishes two main streams in the historical development of anthropological theory. The first, stemming from Morgan, was continued through Maine, Spencer, Durkheim, Rivers, Lowie, Radcliffe-Brown, the later British structuralists, and Lévi-Strauss. It laid primary emphasis on the concept of a social system, and its adherents are designated collectively as 'social anthropologists.' The second main stream, stemming from Tylor, was continued through Frazer, Haddon, Boas, Kroeber, Malinowski, and, I would add, the German-Austrian diffusionists and most contemporary American anthropologists. It stresses the concept of culture, and its adherents are collectively characterised as 'cultural anthropologists'. I have no serious criticism of this classification, though the placement of Morgan seems unduly simplified. At any rate it will serve quite adequately as a basis for discussion.

Fortes aligns himself unequivocally with the first of these streams of thought and is uniformly critical of cultural anthropology. His strictures would have aroused my strenuous objections some years ago, but I have gradually arrived by a different path, and for quite different reasons, at a comparable scepticism regarding the validity and utility of the cultural approach to human behaviour. I am reminded here of the meeting of the Fifth International Congress of Anthropological and Ethnological Sciences at Philadelphia in 1956, when Heine-Geldern, supported by Haekel and Koppers, formally renounced the basic tenets of the Vienna culture-historical school to which they had previously adhered. Adopting this admirable example of intellectual integrity as my model, I would like to take this occasion to renounce in similar fashion my own former adherence to the cultural position of most of my American colleagues.

I cannot, however, accept the social anthropological position held by Fortes and his British colleagues. A careful reading and rereading of *Kinship and the social order* has illuminated many aspects of the theoretical orientation of Fortes and Radcliffe-Brown that had previously seemed unclear to me, but instead of conveying conviction it has accentuated my earlier mild scepticism to the point where I now find the position as unacceptable as I do that of cultural anthropology. My rejection of it is equally total.

A number of serious criticisms can be levelled at the theoretical systems of both cultural and social anthropology, and I will shortly allude to some of them. But the most basic objection applies equally to both of them and goes back to a common fundamental error perpetrated at the time when anthropology and sociology were first emerging as an independent discipline (or, if you wish, disciplines). Herbert Spencer, for example, faced squarely the problem of defining the appropriate unit of scientific investigation for the new science. Was it the human individual or some supra-individual phenomenon? While he recognised that the individual is unmistakably the isolable unit of human perception, thought, feeling, and action, he nevertheless—for reasons which need not concern us here—opted for the social aggregate as the preferable unit for study and was thus led to his conception of the superorganic. Durkheim and most subsequent sociologists and social anthropologists have followed in his footsteps without ever seriously questioning whether he had made the correct choice. Tylor likewise rejected the individual as the appropriate unit of investigation in favour of another supra-individual concept, that of culture, and has been unquestioningly followed by subsequent cultural anthropologists. Kroeber even adopted Spencer's term 'superorganic' to designate the alleged cultural level of behaviour.

It now seems to me distressingly obvious that culture, social system, and all comparable supra-individual concepts, such as collective representations, group mind, and social organism, are illusory conceptual abstractions inferred from observations of the very real phenomena of individuals interacting with one another and with their natural environments. The circumstances of their interaction often lead to similarities in the behaviour of different individuals which we tend to reify under the name of culture, and they cause individuals to relate themselves to others in repetitive ways which we tend to reify as structures or systems. But culture and social structure are actually mere epiphenomena—derivative products of the social interaction of pluralities of individuals. More precisely, they resemble the illusory constructs so prevalent in the early days of the natural sciences, such as those of phlogiston and the luminiferous ether in physics, and systems of theory based upon them have no greater validity or utility.

When I characterise the concepts of culture and social system as 'myths', I do not imply that they bear no relation to reality, for they are obviously derived from observations in the real world. I mean merely that, as reified abstractions, they cannot legitimately be used to explain human behaviour. Culture and social aggregates are explainable as derivates of behaviour, but not *vice versa*. All systems of theory which are based on the alleged or inferred characteristics of aggregates are consequently inherently fallacious. They are, in short, mythology, not science, and are to be rejected in their entirety—not revised or modified.

This conclusion is supported by a variety of evidence. In any established science, for example, there is substantial agreement among its leading practitioners on the essential core of its body of theory, whereas in anthropology there is virtually no such consensus. In analysing the recent volume by Fortes I discovered—to my astonishment in view of my great respect for his work—that it contained scarcely a single theoretical assumption, postulate, generalisation, or conclusion which I could accept as valid without serious qualification. I had had a similar reaction once before—in reading the theoretical work of Leslie White. And I have since experienced it a third time when, stimulated by Fortes, I reviewed the theoretical writings of Alfred Kroeber. Having known all three men fairly intimately, I am aware that none of them has found my own views any more acceptable than I have found theirs, and that each of them has felt an equally profound scepticism regarding the views of the others. It is inconceivable that four men of comparable standing in any established field of science, such as astronomy, nuclear physics, or genetics, could differ so radically from one another on basic theoretical issues. One can only conclude from this that what Fortes, White, Kroeber, and I have been producing is not scientific theory in any real sense but something much closer to the unverifiable dogmas of differing religious sects.

In some instances this is almost embarrassingly apparent. Thus Kroeber entitles his most succinct expression of his theoretical views 'Eighteen professions' (1915: 283-8), thereby almost explicitly likening his own profession of faith to that of Martin Luther. White is notoriously accepted as a prophet by many of his students—and acts the part. Fortes freely acknowledges his discipleship to Radcliffe-Brown, expounding and explicating the dogmas of the latter but seldom deviating from them. This hidebound adherence to 'revealed truth' contrasts strikingly with the fluidity and rapid advances in theory that have characterised such unquestioned sciences as genetics.

Moreover, what passes as theory in anthropology includes remarkably few propositions which meet the basic requirements of science, that is to say, which explicitly state relationships between phenomena, specify precisely how these change as relevant variables are altered, and support such statements with adequate validating evidence. It consists in the main of what George Homans (1967: 10-19) calls 'nonoperating definitions' and 'orienting statements'. Prominent among the former are the concepts of culture and social system.

It is noteworthy that biographers, novelists, and playwrights invariably account for the behaviour of their characters in terms of the influences exerted upon them by other individuals in social situations. Rarely if ever do they resort to custom, or roles, or comparable supra-individual abstractions as explanatory principles. Here, as so often, the intuitive perceptions of humanists prove superior to the ponderous postulates of social scientists.

The inadequacies of both cultural mythology and social mythology—to call them for once by their appropriate names—are especially clearly revealed by the manner in which they have dealt with change. Cultural anthropologists have always been preoccupied with change. In the nineteenth century they concerned themselves primarily with the evolutionary sequence of changes. Was agriculture invariably preceded by a pastoral type of economy? Was matrilineal descent universally antecedent to the tracing of kinship ties through males? In the twentieth century the emphasis shifted to the provenience of change. Where and when did the plough or circumcision originate? From what sources were the elements of any particular culture derived? After 1925, especially in the United States, attention turned increasingly to the processes or dynamics of change rather than its substance. Always, however, the focus was on the illusory thing — culture — which was supposedly undergoing change rather than on the actual behaviour of real people.

In Great Britain the social anthropologists who followed Radcliffe-Brown have commonly ignored change, or at least have allowed no place for it in their body of theory. They focus attention exclusively on another myth, that of a social system, clearly an organismic concept as is evidenced by the analogies they draw with physiology. Like a living organism, such a system is conceived of as characterised by a sort of homeostasis; in the words of Fortes (1969: 44): 'the usages, rules, and forms of grouping have functional value and work together consistently to maintain the social system'. The parts are in equilibrium with the whole, and only synchronic analysis can bring an understanding of their interrelationships. Nothing depends on what has gone before—on the previous state of the system. Change can occur, if at all, only in terms of some sort of shifting equilibrium, which is always assumed rather than demonstrated. A sharp dichotomy is thus drawn between the diachronic and the synchronic, and this polarity is generalised to the extreme conclusion that all of anthropology itself is divided into two opposing wings. One of them, called 'social anthropology', utilises only synchronic analysis and is alone capable of reaching valid scientific conclusions. The other, dubbed 'ethnology', takes diachronic relationships into consideration and relates what is observed in the present to what has occurred in the past. It is considered totally unreliable, yielding only false generalisations which are stigmatised as 'pseudo-history' or 'conjectural history'.

I submit that all this is arrant nonsense—completely mythical and even mystical. Rather than argue the point, I shall merely cite two instances that reveal its falsity. Fred Eggan, in many respects the soundest of Radcliffe-Brown's students, has demonstrated, in his *Social organization of the western pueblos* (1950), what extraordinarily convincing conclusions can be reached when the two modes of analysis, the diachronic and the synchronic, are used in conjunction with one another. The master should have listened to his pupil!

The second instance concerns what is probably the most universally accepted body of theory in all of anthropological science, namely, the genetic classification of the languages of the world developed by historical linguistics. Except for certain written records on the Indo-European and Hamito-Semitic languages, this is based exclusively on inferences as to their antecedents from the lexical and grammatical forms exhibited by thousands of languages at the periods when they were recorded. In view of this solid achievement, it is absurd to deny the possibility of deriving valid conclusions from the use of diachronic or historical methods in ethnology, even though most of the results to date have admittedly been disappointing.

I therefore feel no hesitation in rejecting the validity and utility of the entire body of anthropological theory, including the bulk of my own work, which derives from the reified concepts of either culture or social system, and in consigning it to the realm of mythology rather than science. Some of the fragments of existing theory which escape such stigmatisation will engage our attention toward the end of this paper.

We may now inquire what led our founding fathers to impose upon us this hydra-headed incubus of false science. The answer seems clear: it was a misconception of the role of the emerging sister discipline of psychology. Spencer, Tylor, and Durkheim understood psychology as the science which had undertaken the study of the behaviour of the human individual. With the individual pre-empted as an object of investigation, they felt compelled to search elsewhere for an appropriate subject matter for the new discipline. Sensing that this must be different, but not quite realising in what respects, they not unreasonably assumed that it must be some supra-individual realm of phenomena. Culture and society presented themselves as the two most logical candidates, and practically everyone plumped for one or the other, or sometimes both, as the primary basis for explaining social behaviour. Thus sociology and anthropology alike committed themselves from the outset to an egregious error from which neither has yet recovered.

The distinction between psychology and the social sciences is not one of subject matter. Psychologists study social as well as individual behaviour, as the very existence of the subdiscipline of social psychology demonstrates. And anthropologists and sociologists often focus on individual behaviour as when they investigate such subjects as invention, leadership, and deviation. Years of close collaborative association with a number of eminent psychologists at Yale University brought me to a realisation that the actual relationship between the psychological and social sciences is both more complex and more intimate than is usually thought, and much of what follows reflects my intellectual indebtedness to them.

It was the consensus of these colleagues, mainly experimental and behavioural psychologists but also including some psychoanalytical and social psychologists, that all human behaviour which is not biologically determined in the narrowest sense depends upon the interaction of two sets of factors. The first is a series of basically innate mechanisms, such as those of perception, cognition, and learning, through which all behaviour is mediated. The second consists of the specific conditions under which any behaviour occurs. The mechanisms, being fixed by heredity, are fundamentally identical for all mankind, and even to a remarkable extent throughout the mammalian order. The conditions, on the other hand, are almost infinitely variable over time and space. It is through the complex interaction of the constant mechanisms with the varying conditions that man's behaviour becomes adapted to his environmental circumstances (cf. Miller and Dollard 1941). Neither factor is more important than the other; both are equally crucial.

Psychologists have focused their attention primarily on the constant factor, and their experimental activities have laid bare in admirable fashion a large proportion of the basic mechanisms involved in behaviour. In so doing they have kept the conditions as uncomplicated and carefully controlled as possible, for example, by using animal rather than human subjects or, if the latter, those with a minimum of past experience in comparable situations. The more cautious among them have refrained from attempting to isolate the underlying mechanisms where the conditions are highly complex, as they invariably are when the behaviour is termed 'social' or 'cultural', and have assumed that such situations fall more appropriately within the province of the social sciences. The less cautious—and there have been some in even the most rigorous schools of psychology—have chosen to ignore the conditions of behaviour or to assume their constancy and have sought to explain complex behaviour in terms of the mechanisms alone. This procedure, which may be regarded as a naïve form of 'reductionism', is totally incapable of producing dependable results, yielding only what may be regarded as a peculiarly psychological version of scientific 'mythology'.

On the other hand, psychologists have been notably successful in avoiding the pitfalls of anthropology's 'mythology'. With near unanimity they find the locus of the mechanisms of behaviour in the individual human being rather than in such reified supra-individual abstractions as culture or a social system. The same mechanisms are operative in even the most complex forms of behaviour. The sociologist Homans (1967: 43-55), in surveying the scientific propositions advanced by economists, historians, sociologists, and anthropologists, has discovered no valid ones which are not clearly identical with or derivable from the basic principles of behavioural psychology. It seems to me obvious that the psychologists, with their sophisticated experimental techniques, enjoy an enormous advantage in the effort to discover and understand the mechanisms of human behaviour, and that the social scientists would be well advised to pay far closer heed to their findings than has been the case in the past.

The situation is quite different with respect to the conditions of behaviour. Here the knowledge of the psychologist scarcely extends beyond the highly simplified conditions which he manipulates in his experimental laboratory. He has no understanding of the special conditions that have helped shape the complex forms of behaviour in which the social scientist is interested. He is totally incapable of accounting for differences in economy, in technology, in social and political organisation, in ideology, or in value systems. If he attempts to do so on the basis of his knowledge of behaviour mechanisms alone, he achieves only his own peculiar kind of mythology. Analysis of the conditions which underlie complex forms of behaviour requires detailed information on such matters as the climate and geographic setting, the man-made environment of settlement pattern, physical structures, and artefacts, the distribution and density of population, the transmitted and partially shared habitual response tendencies that are commonly designated as culture, and the patterned network of interpersonal relationships which is often termed the social structure. Such information is rarely available except in the records left by acute first-hand observers. These are usually social scientists and, more often than not, anthropologists.

I have already paid tribute to ethnography as the finest achievement of anthropology. At this point I would like to assert that it is also the outstanding resource in the social sciences for the analysis of the conditions under which the more complex forms of human behaviour arise and take shape. Noteworthy contributions have also come from geography, history, and sociology in particular, but even in their totality these cannot compare even remotely with the ethnographic record in quantity, quality, range, or depth. Without ethnography the social sciences would completely lack evidence on the conditions under which such phenomena as shamanism, totemism, cannibalism, and matrilineal institutions occur, and would therefore be helpless in attempting to account for them scientifically.

It is therefore my personal conviction that the future development of a valid and productive science of man will depend largely on an increasingly intimate collaboration between the disciplines of psychology and anthropology. From the former will come an ever fuller understanding of the underlying mechanisms of behaviour, from the latter an ever-expanding comprehension of the varying conditions under which the mechanisms operate to produce differing forms of behaviour. Neither by itself offers much of intrinsic scientific value. Psychology, generalising from mechanisms alone without a precise understanding of conditions, can generate only a sort of reductionistic mythology. Anthropology, despite its potential command of the conditions of behaviour, produces only cultural and social mythology when it ignores the findings of psychology on the mechanisms of behaviour in favour of its own illusory substitutes. Only if the two disciplines collaborate to the full, with anthropology trusting psychology to reveal the basic mechanisms of behaviour and with psychology trusting anthropology to ascertain the relevant configurations of conditions, will a genuine and full-fledged science of man emerge.

If anthropology is to play the vital role in such a development for which it seems so peculiarly well fitted, it must subject its existing corpus of theory to radical surgery. It must eliminate all assumptions and postulates which are clearly inconsistent with the findings of psychology. In particular, it must relegate to the limbo of false science the two major segments of existing theory which depend on the mythological concepts of culture and social system respectively. There is no objection, of course, to employing such concepts in a metaphorical sense, or even sometimes as organising principles, as archaeologists, for example, commonly use the concept of culture. Under no circumstances, however, is it safe or proper to employ them as operating or explanatory principles. Culture and social structure at best are the results of the interaction of individual human beings; as reified abstractions they can never be causes or operant factors in behaviour. It is fortunate that the leaders of both cultural and social anthropology have left behind such a rich heritage of descriptive ethnography, for their legacy of theory, however admirable for its ingenuity and provocative quality, includes virtually nothing of solid value for the future science of man.

It is not quite true, however, that we must make a completely fresh start, for, embedded in the chaff of our mythology, there are scattered kernels of valid or promising theory which provide a nucleus around which we may conceivably construct a sound and viable science. I will call attention to a few of them here; there are undoubtedly many more.

Our prevailing theories of invention or innovation, for example, seem completely unvitiated by supra-individual concepts. They deal explicitly with the individual innovator and with the conditions which make possible the new synthesis of pre-existing elements which is the essence of most innovations. They identify in a reasonably satisfactory manner the particular configurations of conditions under which an invention is likely to be made by two or more individuals independently, as well as those under which any specific invention is virtually impossible. A similar realism is not uncommon in those who deal with other aspects of social or cultural change. Here we need simply note the contrast between those who speak of cultural diffusion, where an illusory thing—culture—is conceived of as spreading, and those who speak of borrowing, where the focus is on particular individuals imitating the behaviour of others.

A second area of valid and important anthropological theory is seen in Malinowski's treatment of reciprocity in his *Crime and custom in savage society* (1926). I still recall my excitement as a young man in discovering this demonstration of the falsity of the Boasian assumption of an inherent inertia in culture as compared with the alternative hypothesis that the persistence of an element of behaviour in an interpersonal relationship depends upon the reciprocally rewarding character of the activities of the interacting individuals.

I may perhaps be excused for selecting a third example from my own work in view of my admission that most of what I have written is clearly vitiated by dubious cultural and structural assumptions. I refer to my exposition of the determinants of kinship terminology in my book *Social structure* (1949). Here I refrained from postulating any supra-individual entities, either of culture or of social system; I made use of mechanisms exclusively derived from behavioural psychology and of conditions carefully defined by anthropologists and fully attested by ethnography; and I validated their combined effects by what was then a highly sophisticated scientific methodology. Though this piece of work has had few admirers among anthropologists, and no imitators, it has been regarded by other social scientists as a substantial contribution to science, and I see no compelling reasons for rejecting their judgement.

Finally, I would like to call attention to the decision-making approach to the study of social phenomena, which is advocated by Fredrik Barth and followed by an increasing number of younger anthropologists in the United States and elsewhere. This focuses on the events of social life rather than on its morphological or statistical features and views social behaviour from the point of view of the decisions made by individuals in 'the allocation of time and resources' from among the alternatives available to them. In the words of Barth:

> People make allocations in terms of the pay-offs that they hope to obtain, and their most adequate bases for predicting these pay-offs are found in their previous experience or in that of others in their community . . . If the pay-offs are great, one can expect the behaviour to be emulated by others . . . [otherwise] they revert to older allocations (1967: 668).

Such decisions are likely to have 'systematic effects' over time, leading to gradual changes in traditional patterns of behaviour and in the structure of social relationships. The emphasis is always squarely on the individual, on the psychological mechanism of reinforcement, and on the relevance of the specific conditions under which behaviour occurs. For these reasons, the decision-making approach impresses me as perhaps the most promising recent development in anthropological science.

Such achievements, scattered though they are, suggest that the future may witness the emergence of a genuine science of anthropology—liberated from unwarranted qualifying adjectives like 'social' and 'cultural'. For some years I have noticed the gradually weakening hold of the traditional mythologies on my younger colleagues. The two which I have discussed are merely the oldest ones and those from which I have personally had the greatest difficulty in extricating myself, but others arise from time to time to confuse us. Some have suggested that I should have paid equal attention in this paper to a third type of anthropological mythology which has recently been gaining adherents on both sides of the Atlantic. I refer to those who postulate structural properties in symbolic systems and call attention, for example, to conceptual parallels between marriage rules and food taboos.

I have not done so for two reasons. First, I do not fully command the relevant literature because the works I have read have notably failed to generate sufficient stimulation to induce me to explore the rest. I suspect that my scepticism stems primarily from an awareness of how vastly easier and pleasanter it is to speculate than it is to validate. Second, I am by no means convinced that what is postulated is actually a scientific myth. At least some of the proponents insist that the symbolic connexions they note are strictly metaphorical rather than causal, which would remove their theories from the realm of science to that of the humanities. If this is the case, their alleged discoveries are as scientifically irrelevant as are the latest fashions in literary criticism.

Before closing, I might note that the theoretical position which I have outlined for the interpretation of social and cultural behaviour bears a close family resemblance to that generally accepted by biological scientists to account for the development and differentiation of living organisms. Biologists, too, postulate a universal set of underlying mechanisms, which have largely been worked out by the science of genetics, and they conceive of these as operating in conjunction with widely varying environmental conditions to fix selectively and perpetuate the mutations which prove relatively adaptive. It is unlikely that this parallel is accidental, for man, despite what we are wont to call his culture and his society, is nevertheless fundamentally a biological organism. Nor is it in my opinion accidental that biologists have arrived at their present theoretical consensus only after painfully trying out and discarding a series of illusory concepts, such as those of vitalism, which seem to me strictly comparable to what I have called the myths of anthropological theory, and which a lifetime of professional trial-and-error has finally forced me to reject.

In conclusion, I would like to relate an anecdote which is famous in the unwritten history of the Department of Anthropology at Yale. Almost exactly forty years ago, when the late Edward Sapir was conducting a seminar on primitive religion, he had a student who came from the society later studied by John Beattie, the Banyoro of Uganda. This student, in reading a rather pedestrian paper on the religion of his own people, happened to mention that in his country the shrines of the war god were tended exclusively by priestesses. At this, Sapir pricked up his ears and interrupted to comment that, since war is the most masculine of all occupations, it seemed remarkable that the cult of the war god should be conducted by women only.

'Why should this be?' he inquired, and proceeded, on the spur of the moment, to propound a possible interpretation, highly complex and liberally seasoned with Freudian and other symbolism. The students sat upright in fascinated attention. As he was concluding, an alternative explanation occurred to him—equally brilliant, equally complex, and equally symbolic—and he developed this in like fashion, while the students perched on the edge of their chairs, utterly entranced by this double demonstration of his virtuosity. When he came to the end, he turned to the African to inquire the extent to which either hypothesis accorded with Banyoro culture, but, flushed with enthusiasm at his own performance, asked him instead which interpretation was the correct one.

'Actually,' replied the student, 'neither is correct. The explanation is really quite simple. You see, when war occurs in my country, all the men go out to fight, and no one is left except women to tend the cult of the war god.'

This anecdote might well stand as an allegory of both the fascination and the falsity of all forms of anthropology's mythology.

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