

sideration what would happen in situations other than the one which holds at present, and one important source of evidence about such alternative situations is the past. Professor Ginsberg has said that the concept of 'social class' denotes not a real group but a *potential* one. One should notice that this is true of very many sociological concepts.

Another consideration is that in studying the pattern of cause and effect in a given society, anthropologists are not so much interested in the initial conditions of a sequence, as in the generalisation specifying the relation between the condition and the consequent states of affairs. This is so, and should be so; though in order to use this as a satisfactory analysis of anthropological explanation, one would have to go further and take into account the fact that the generalisation itself is something that needs explaining, as in other sciences though in a manner which may be peculiar. For one thing, the generalisation itself may describe a regular connexion which is a state of affairs characteristic of a society, and this fact itself be the consequent in a more general sequence; but in order to know this we must also know the antecedent, and thus *historical* evidence becomes absolutely necessary.

I do not wish to give the impression that I criticised Leach's argument as an example of ordinary error. If it is erroneous, it is so in a way in which interesting philosophical doctrines are. On the contrary, it is perhaps the most lucid statement of a certain kind of Idealism that I know, and teachers of philosophy could profitably use a selection of his statements as a means of explaining to their students what such Idealism is about. They certainly could do no better than use Leach's book if they wish to illustrate how philosophical problems can spontaneously grow out of first-order work in an actual science. Moreover, the criticised argument is used to point a moral which is in fact a good one; and philosophical error analogous to his own is as present, if not as clearly stated, in the views he criticises as in his own.

NOTES

1 Cf. Ronald Fletcher, 'Functionalism as a Social Theory', *Sociological Review*, n.s. 4, July 1956, 31-46.

2 Cf. a forthcoming article by Mr Paul Stirling.

3 The doctrine that ritual and belief systems mirror the social organisation in which they occur has wide currency amongst social anthropologists. It is, in fact, a very suggestive and illuminating idea, helpful in formulating questions for research and in organising material. As a formally maintained doctrine it has logical defects of which anthropologists may not be sufficiently aware. Mainly: the idea of 'reflection' or 'mirroring' presupposes, as a minimum

requirement, that there be a one-one correspondence between the two mutually reflecting systems. But neither system—neither social organisation, nor a system of ritual or belief—consists of easily separable, identifiable, countable 'parts'. The principles of individuation of such parts are largely arbitrary. It follows that any anthropologist who wishes can always, as far as this condition goes, with some ingenuity or ruthlessness interpret *any* material so that it fits the 'reflection' thesis.

4 Cf. C. Lévi-Strauss, *Les Structures Élémentaires de la Parenté*, Paris, 1949, p. 27.

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SOCIOLOGY AND
SOCIAL ANTHROPOLOGY

The distinction between sociology and social anthropology is itself a social rather than a logical one. In other words, the distinction can best be understood not by looking at some neat dividing line in the subject matter of the disciplines or in their method, but in the concrete, and hence untidy, factors which operated in various times and places to cause people to class themselves as 'sociologists' and as 'social anthropologists'. This is highlighted by the fact that this distinction, and similar distinctions, are drawn differently in different countries. If the facts or logic of the case imposed the distinction, one should expect it to be drawn similarly in most places. To say this is not to claim that there is no logical content at all to the distinction; merely that, as so often, logical, pragmatic, opportunist and accidental factors all contribute to the drawing of a distinction between two groups of people.

What are the logical or substantive elements in the differentiation? What are the various watersheds along which the frontier is or has been located? I shall list a few obvious ones.

The first distinction which springs to mind is the contrast Advanced/Primitive. This is certainly the first rule of thumb by means of which one distinguishes the concerns of sociologists from those of anthropologists. Nevertheless, it gives rise to formidable difficulties, both theoretical and practical, if it were to be treated as an internationally recognised frontier. The practical difficulty is this: anthropologists do not cease to be anthropologists, in their own estimation or that of others, when they study, for instance, middle-class kinship in London and Chicago. At a pinch, villages in Western Ireland, Wales or Italy, or even the working classes of industrial cities, could be classed as a kind of honorary savage. But middle-class professional families?

The theoretical objection can be formulated, in simplest terms, as follows: the classification of societies into advanced and primitive, or more elaborate versions of such a classification, presupposes at the very least a tacit acceptance of an evolutionist view of human societies. What happens if such a schema is rejected? Any workable

definition of the subject must surely refrain from prejudging the truth or falsity of theories *within* the subject. Are not anthropologists (or, for that matter, sociologists) entitled to reject evolutionism?

Of course, one might attempt to make the delimitation concrete and specific, and consequently free from attachment to any evolutionist doctrine of 'stages' of unilineal development. One might draw the line between large and small societies, or between complex and simple. But the small societies which concern the anthropologists are sometimes disconcertingly complex, and at least one reasonably interesting theory of modern society ('Mass Society') makes it out to be simple, at least to the extent of being composed of similar elements. Moreover, anthropologists do not lose interest in a community when it is incorporated, politically or otherwise, in a world-wide social network.

Could it be claimed that anthropologists are concerned with societies as totalities, whereas sociological researchers isolate various *aspects* of society? There are various factors which point in this way. Anthropologists are able to concern themselves with a whole society when that society is small, and they are *obliged* to do so when that society is previously relatively unknown, so that the characterisation of any one aspect of it requires at least the sketching in of the rest of that society. Sociological researchers *cannot* do this in large complex societies, and *need not* do so in as far as some general knowledge of society can be taken for granted. Moreover, anthropologists have recently been less inclined to do comparative work, and hence had less need to isolate 'aspects' for comparison, whereas sociologists, when theorising at all, have had to do this. Despite all this, one could nevertheless not draw a definitive boundary here. A typical anthropological thesis is also concerned with specific aspects of a society; and sociologists sometimes attempt to see a society globally, and the most all-embracing and global of theorists have counted as 'sociologists'.

Perhaps one should attempt to differentiate the two disciplines in terms of their attitude to time? For many people outside sociology and for a few within it, the paradigm of a sociological theory is still an account of 'stages'; whereas what characterises many anthropologists is the preference for the synchronic method. But although I shall have a good deal more to say about this crucial question of time, clearly no such simple frontier can be drawn here either. It would condemn sociologists who are evolutionists, or anti-evolutionists, to the wrong side of the frontier, and it would ignore the quite effective efforts of anthropologists to deal with change over time.

One might at times be tempted to see the distinction not in the kind of theory employed, but in the attitude to theory as such or in terms of its very presence or absence: one might be led to this by the

manner of speaking adopted by some anthropologists, who distinguish between ethnography and sociology and mean by the latter the general, theoretical conclusions drawn from field observation. But again, this will not do. Heaven knows, there are untheoretical sociologists too.

Or, again, one might be tempted to seek the distinction in the type of method employed. Anthropology suggests above all field work, participation, intensive pursuit of the social reality under the social appearance and a careful mapping of both, a pursuit of latent functions. Sociology suggests extensive rather than intensive research, general observation, comparison. But it is hardly necessary to repeat that, once again, this cannot give us an acceptable boundary.

One might say that a different contrast underlies the two disciplines. Sociology is born above all from preoccupation with social change within Europe, from an attempt to understand how the European present and future arose from the European past. Anthropology, on the other hand, was born of the contact between Europeans and others, and the question is not 'how did we emerge from our ancestors', but rather 'how did they get left behind or diverge from our path?'

Anthropology was born of the interest in the contrast between western and savage man. Sociology was born of the contrast between the present and the past of western man. This highlights the somewhat paradoxical fact that anthropology began by being *more*, not less, past-oriented than sociology. Present academic social structure bears witness to this: in university curricula, anthropologists are still often linked with archaeologists. Anthropologists were differentiable from sociologists even before the days in which this differentiation was conceived in terms of the cult of field work: but in the early days, the differentiation was in terms of a concern with the distant past as opposed to a closer past (in both a literal and a geographical sense). Of course, when the concern with primitive societies as surrogate time-machines was replaced by concern with them as exemplars of social structure as such, officially this preoccupation disappeared: but it is still there, somewhere in the background. Since the Second World War, of course, both these two contrasted contrasts were replaced by one all-embracing one, that between modern industrial society on the one hand, and *both* 'feudal' and 'pre-industrial' and 'oriental' civilisation, *and* tribal societies, on the other. This strikingly illustrates one of my main points—perhaps an obvious one—that the contrast with which we are concerned is not merely not a neat one, but also a highly unstable one: it fluctuates in time and place, according to background intellectual doctrine, and according to general social preoccupations.

There used to be a joke to the effect that whereas in Oxford, dons

were preoccupied only with what past thinkers had taught, in Cambridge they were preoccupied with the teaching of Cambridge dons in the recent three decades. The truth actually is that whereas in Cambridge they are preoccupied with the teaching of Cambridge thinkers in the past three decades, in Oxford they are preoccupied with the teaching of Oxford dons in the past two years. Some similar dialectic exists perhaps between sociology and anthropology. The distance of the horizons, the range of concern, the centre of intellectual gravity has shifted, expanded, shrunk and varied, and though this question of the temporal horizon is crucial, no simple characterisation of it will do justice to the facts. Nor is there anything regrettable in the changes which have occurred.

It is not, then, in some neat distinction of subject or method, or even in a less than neat conjunction of such distinctions, that we must seek the boundary, but in the actual social structure, ethos and history of the two disciplines, and this moreover will vary from country to country. From actual observation, I can only speak concerning Britain or a part of it. In Britain, it is or was relatively easy to distinguish an anthropologist from a sociologist, though possibly the differences between one kind of a sociologist and another are even greater than those which separate both of them from anthropologists. But just *this* is the crucial distinction: the valid joke in Britain is that the two disciplines mirror the type of society with which, in the popular imagination, they are associated. Anthropologists study tribal societies and they are a tribe; sociologists study anomic modern societies, and they are notoriously and excessively anomic. Anthropologists have a coherent and cohesive tradition and a great deal of similarity in training and outlook. Sociologists come in all shapes and sizes, and some are so far removed from each other that they do not even engage in any sustained dialogue, or sometimes any dialogue at all.

If, then, no neat boundary separates the two subjects, one should nevertheless be able to distinguish and characterise the two cultures, the sociological and anthropological: but the characterisation will have no universal validity, either in time or in space. The only characterisation I can offer is based on observation of the two disciplines in Britain in the recent past. Sociologists are still in a heroic age: no consensus, no central authority, and the populace affiliates itself to heroic figures who, each of them, can carve their own intellectual principality. By contrast, anthropologists enjoy a consensus and a moral community which knows how to impose its norms on its members. They have a fairly stable internal segmentation.

What are those norms? The 'structural-functional' method, and a certain shared attitude to time. I shall say more of these, and I shall not dwell on the familiar matter of homogeneous recruitments, initia-

tion by field work and so forth, with which one would have to be concerned if one wished to explain the precise social mechanism by which those norms are imposed and maintained, by means of which they are internalised, by means of which each individual anthropologist acquires a deep inner investment in the traditional approach which ensures that he is sufficiently like his fellows to communicate with them easily and to produce comparable and hence reasonably cumulative work.

First of all: *time*.

Modern anthropology springs from Malinowski and is characterised by a synchronic attitude towards the interpretation of society. Simplifying: earlier anthropology was distinguished by a greater concern with the past, and a preoccupation with a more distant past, whereas modern anthropology is characterised by a disregard of the past.

Now I happen to be fully aware that this is a gross simplification, and moreover one which is by now vehemently repudiated by many anthropologists, including some who have earned the right to speak with authority about their own subject. Nevertheless, the simplification contains an important truth, and it seems to me that the qualifications and repudiations which have been made are the *wrong kind* of qualification and repudiation. I know that one distinguished anthropologist has defined his subject as a kind of history,¹ and that another one has carefully demonstrated how patiently and attentively anthropologists do look at the past of the societies which they interpret.² Notwithstanding all this, and for reasons which will be stated, I think that the synchronic approach is the correct starting point for understanding what is really happening in the discipline.

The first point which it is necessary to make is that the manifest and latent reasons given for the synchronic approach are not identical. By manifest reasons I mean those which were actually given in so many words; by latent reasons I mean those underlying reasons which are, first of all, valid (in my view), and which also through their cogency and validity had the effective consequence of making anthropological research so very fruitful, effective and cumulative, and which consequently sustained the application of the method. I trust that it is as legitimate to apply distinction between latent and manifest function to anthropologists, as it is legitimate for them to apply it to the peoples they investigate. But it is worth stressing that my notion of the 'latent', in this context, is doubly loaded: it suggests both logical validity, *and* social effectiveness. The two of course do not necessarily or generally go together, but in this case, providentially, they did.³

To begin with, a brief sketch of the manifest reasoning underlying the synchronic approach. First, there was the inaccessibility of the

past in an illiterate society without records. The consequence of this state of affairs, it was asserted, was that reconstructions of tribal history were mere speculative reconstructions, which it was impossible to check and which consequently had little or no scientific validity. The trouble with this is that, whilst partially true, it is only partially true, and no one really knows just how big the two parts are, and in any case there is no reason to assume that the past is equally accessible or inaccessible in all places. Can one really say that there is no element of validity at all in the attempts by archaeologists to reconstruct past social structure by the method of their craft, or in the attempts to reconstruct history from systematically collected oral traditions? And if there is at least some validity, some possibility of valid results, who is to say that in some places, at least with greater ingenuity or greater luck, or with respect to some particular problems, the results should not be very fruitful? It is always dangerous to claim that something cannot be done. Especially if it is manifest that *some* of it *can* be done.

Coupled with the rejection of the past on the grounds of lack of records and the undesirability of uncheckable speculation, there was also a more interesting reason. This is a theory of social causation, and is perhaps best expressed in the slogan associated with Malinowski—'there are no survivals'. One can easily grasp the underlying idea. When people do things, they have motives for doing them—*now*. The motives as well as the action are in the present. Human actions are not inert objects, which can be left behind by the past like artefacts (though the magpie anthropologists who had assimilated customs to artefacts might have thought so); they have to be caused or willed anew each time they occur. Hence their explanation must be sought in the circumstances, inclinations and so forth which brought them into being at the time they were done, and not somewhere in the past. Is that not obvious?

One is at this point reminded of the metaphysical doctrine that the world is recreated anew each instant (say by the Deity), because manifestly the past has not the power to create the present. For one thing, the past is now quite inert: for another it no longer exists. How could it contain a hidden mystic power to generate and control the present? We cannot maintain ourselves in being, it is fortunate accident that we persist: the miraculous gift of existence is granted us anew every instant that passes. And so it is. The metaphysical intuition which generates this doctrine can also be applied specifically to human societies and institutions, and I do believe that it is one of the factors underlying the synchronic predilection.

But the trouble here again is that we are dealing with something which is only partially true. But in the case of the 'lack of evidence' argument, we are dealing with something which was 'partially true',

and indeed also 'partially untrue', in a very straightforward sense. The inertness and ineffectiveness of the past, on the other hand, is true and untrue in a much more complicated manner, being true in some senses and not true at others. We are here not in a situation where we can say 'It is true to such and such a degree, because there is evidence at this point but no evidence at another, and evidence concerning this problem but not that'. The sorting out of the true and the untrue elements requires some more complicated distinctions. Here again, we see that the thesis—in this case, the thesis of the impotence of the past—cannot be wholly true. Any social process consists of a series of events in which people react one day to what somebody else may have done the previous day, and so on. Of course, in one sense the past may be claimed to be wholly dead: I can only react today to something someone else did or said yesterday, if that action or utterance of his of yesterday had left some kind of mark in the present, literally present, situation. But it *has* left a mark: that literally present situation would be different if what had happened yesterday had been other than it was. Deny this and you end up with a quite absurd position, a series of wholly discrete and discontinuous incidents, quite independent in their content and structure of each other.

But if this extreme position is not intended, what is meant? Is there a kind of sociological 'specious present', within which interaction is permitted? If so, how long is it? A few days, a season, a year, a generation? From the viewpoint of the metaphysical intuition which underlies the argument, the intuition that only the present can operate in the present, a miss is as good as a mile: if the events of yesterday are relevant, then the invocation of events of any past period, however distant, are in principle permissible . . .

There is another way of highlighting the fact that the idea underlying the slogan 'there are no survivors' cannot be pushed to the limit, that it must be allowed to have some exceptions—and, of course, once this is allowed, the question arises concerning how many exceptions are to be allowed and how important they are. Take an example which, it is said, was discussed in Malinowski's seminar: those peculiarly pointless buttons which are found at the end of the sleeve on male coats in our society. *Prima facie*, these are of course splendid candidates for the status of being a 'survival': one assumes that, in the sartorial past, they performed some function, but they really do nothing whatsoever now. Yet, at the same time, it is not at all difficult to find some kind of synchronic, functionalist, Malinowskian explanation of their presence. They are, for instance, part of that sartorial elaboration which, through its very pointlessness, manages to sift out in our society those who have the resources and leisure for attending to their clothes from those whose poverty or

occupation prevents them from doing this. In a society in which low status accrues to those who can not attend to pointless sartorial niceties, the buttons on one's sleeve do make a contribution, albeit a humble one, to the maintenance and expression of social stratification.

So far so good: an explanation of this kind is plausible, and some explanation along those lines may well be true. But the 'functionalist' explanation really only applies to a certain aspect of the phenomenon: to its formal aspect, so to speak, the fact *some* pointless sartorial elaboration is necessary, an elaboration which requires time and/or money and which thus helps to segregate the possessors of either from those who are deprived of them. Such an explanation might be quite powerful in the sense of being pretty specific, in narrowing down the 'functional requirement' very specifically: the explanation might for instance deduce from independent evidence about our society just the precise amount of pointlessness that is required. It might show that our society is just sufficiently egalitarian, mobile and utilitarian not to permit great excesses in pointlessness, whilst at the same time requiring a kind of minimal modicum of it. It might then be shown that things rather like buttons on sleeves fit the requirement precisely.

But this kind of explanation, useful though it may be in the hands of a skilled practitioner, does not explain why it is *just buttons* and not something else, containing exactly the same amount of pointlessness, which is employed. Why *ausgerechnet* buttons on sleeves? Obviously there is an infinity of possible adornments, which would satisfy the requirements. Why just buttons? Why just at the bottom of the sleeve?

Here surely the functionalist anthropologist will be driven to say something like this: the structure of society or a social situation is explained synchronically, but the culture, the precise symbols that happen to be employed and so on, can be determined historically, by the past. A certain degree of elaborate sartorial pointlessness is required by this social situation, but the symbols or tools employed for it are determined by the accident of the past.

But this concession, which I think is inevitable, again operates as the thin edge of a wedge. If some cultural content is allowed to be determined by the past, where is one to draw the line? If some cultural content is determined in that way, why not a lot of it, as indeed is likely to be the case? And if a lot of it is so determined, is it really plausible to say that at no time do the accidents of cultural *content* have a crucial influence on the structural *form* of the society? The distinction between structure and culture is an enormously important one, and highly valuable in field work and in analysis: but it is not a sharp one, and it would be a daring anthropologist who would maintain that 'culture' is always causally powerless.

So once again, for different reasons, one finds that the powerlessness of the past in the strong and exceptionless sense cannot be maintained, and if qualifications and modifications are introduced, it is not clear how much is left of the original thesis.

There is another consideration, curiously seldom noticed: there is a certain contradiction between the synchronic method and functionalism—notwithstanding the fact that so many anthropologists embraced both, and indeed considered them to be mutually reinforcing. In some ways, no doubt they are. But in one way, they are in contradiction. Functionalism in a way amounts to this: when interpreting an institution, look for the ways in which it contributes to social stability. Functionalism as a *method* consists of the requirement that one should seek these contributions to stability; functionalism as a *doctrine* consists of the view that all existing institutions do make such a contribution.

But all this presupposes that we know the society in question to be *stable*. This means, of course, that we suppose it to have been the same in the past as it is in the present and, indeed, that we expect it to continue in the same condition in the future. But how on earth can one say, almost in the same breath, that one does not know anything about the past of an illiterate tribal society (there being no records), *and* that one knows it to have been the *same* in the past as it is in the present? How indeed. One can say it in the same breath, provided one does not say it in the same words. The presumptuous doctrine claiming knowledge of past stability was not put in these words, but was tacitly incapsulated in the very notion of 'function', meaning, roughly, contribution to *stability*, whereas the more modest principle of disclaimer of speculative reconstructions of the past was asserted, bravely, in so many words. This deception was not, of course, deliberate: it just happened.

What I am saying is that the reasons for the timeless approach, in as far as overtly formulated, were inadequate, imprecisely formulated (and then false on a strong interpretation and inadequate on a weak one), and in some respects downright self-contradictory. Yet underneath, there were other, cogent, valid and important reasons. What were these? They are connected with what seems to me a very valid perception concerning social causation: a kind of sociological rejection of action at a distance.

The trouble with traditional evolutionism was that it had an in-built tendency towards a vicious kind of abstraction. I am not saying that this vicious tendency always and necessarily had to manifest itself: but it was a strong tendency, strong enough to vitiate much if not most of the work of evolutionists. It amounted to a tendency to seek causal connections at too high a level.

Evolutionism was concerned with the Great Path. Consequently,

and this is an important point, it tended to take stability for granted. It was the great *change* which had to be explained, and hence stability seemed a kind of inertia, requiring no special explanation. Consequently, it is mainly interested in seeing and explaining how one Big Stage causes the next one. But does causation really occur at this level of abstraction?

The answer is—Yes, it does; but before we look at the causation at *this* high level of social abstraction, we must first of all look at the more atomic level of social interaction. Consider an imaginary and very, very stable society, leaving aside the question whether really stable societies exist. An evolutionist with a grand vision might pass this society by, for it is of no interest to him since the time it was generated by the preceding 'stage' or until the time when it begins to generate the next 'stage'. But can such a neglect be justified? This stable society is not, after all, in any kind of social rigor mortis. Bend over the ant-heap, look at it carefully and in detail, and obviously its members must be quite active—perhaps even very, very active. A persisting society, even or especially a stable one, consists of people doing the many things required to keep themselves alive, to reproduce themselves, to maintain order amongst themselves, to ward off the various shocks which an external social or political environment invariably gives to any society—all these things have to be *done*. Is there not a problem here of how this is managed?

Malinowskian anthropologists set about answering precisely this kind of question. They found themselves small-scale, technologically primitive societies, assumed them to be stable (on the somewhat self-contradictory grounds that they did not know their past and consequently could not assert them to be *unstable*), and proceeded to do immensely valuable work in so doing. The explanations they put forward had to be in 'structural-functional' terms, for their terms of reference precluded (rightly) the invocation of something external to the present society altogether, such as its past condition. The explanations then must be 'structural', in as far as they must be about the relationship of the parts of the society to each other. (There is nothing else in terms of which it could be.) They must also be 'functional', for the problem is 'how does the society maintain itself in a condition of stability', and the answer must be in terms of how each individual institution or custom etc. contributes to this effect, and how it in turn is kept in place by the other institutions, etc. Not only are explanations in terms of the past excluded, but the method itself also automatically excludes explanations in terms of unique events, such as the occurrence of an idea or of an outstanding personality, for such a *deus ex machina* explanation would not be a real explanation of stability unless a mechanism was specified which caused its reproduction regularly, and of course if such a mechanism *is* specified, the

crucial event ceases to be unique. (It is then acceptable to the method, of course.)

The point towards which I am working is that the 'structural' method, which I am not defining very precisely, but which implies a good look at the self-maintenance properties of organisations (and ignoring supposedly unique events, treating everything anonymously instead), is profoundly implied by the 'timeless' approach, but—and this is enormously important—it does not imply it in turn. In other words, the historic service performed by the shock of timelessness introduced by Malinowski was to make people into structuralists: when they became structuralists, they could then cease to be timeless.⁴

Once the habit of looking at molecular causal connections, so to speak, within a society has become second nature for the social investigator, it can easily be reapplied to *unstable* situations as much as to stable ones, to situations obtaining in *the past* as much as in the present. It in no way requires a rejection either of change and development, or of concern with the past. It had been injected, forcefully, into anthropology as part of a timeless attitude: once it is securely present, it has no need whatever of that timelessness.

Causation does of course occur at both molecular and molar levels (or perhaps one should say *many* levels). Institutions, customs, activities interact and produce the stability, *or* change, of the society, as the case may be, and also in another sense total states of a society produce the subsequent states: but the nexus existing between total states cannot be fully explained without the prior specification of the molecular interactions of which it consists, and whose existence it presupposes.⁵

This shows that those anthropologists who want to save anthropology from the charge of timelessness are somewhat misguided. No doubt they are quite right in their facts, and anthropologists have never neglected the past where evidence was relevant. But this throws out the baby with the bath water. Timelessness was most valuable in throwing out evolutionist pseudo-history: and it is all to the good that it never threw out genuine history as well. But the elimination of evolutionism was a great achievement, and the defence against a minor and not very important charge obscures that achievement. Since the shock of the timeless approach, and thanks to it, concern with molecular causation, and an unwillingness to take stability (*or* change) for granted, have become second nature with anthropological thinking; and this too is an enormous achievement. Why obscure it by being worried with a minor and inaccurate charge?

To sum up the argument: anthropologists are most interestingly distinguishable by their attitude to time. The extreme formulation of that attitude is invalid and yet was, through its very extremity,

valuable: for though mistaken in itself, it brought with it the habituation, indeed the profound internalisation, of the 'structural-functional' method.

If one believes this method to be most valuable, as I do, it is perhaps desirable to define it, and the crucial notion of 'structure', in greater detail than has been done so far—for, so far, it has really been made equivalent in this argument with something like 'attention to molecular causation'. Hence such a more detailed account must be the next step in the argument.

What is a functional system, or at any rate a stable functional system, 'in equilibrium'? It is a system of interacting parts such that a stable order is maintained, this in turn being defined so that any change going beyond specified limits will be prevented by mechanisms within the system. This definition already highlights the fact that the notion of a functional system is rather elliptical, and becomes determinate only if the limits which must not be transgressed are clearly specified. The limits cannot be *too* narrow, for some change characterises any system made up of living beings—if only the change consisting of a turnover in personnel due to the passage of generations. The limits cannot be too broad, of course, without making the attribution of functionality tautological.

As indicated, such a system may but need not be teleological. Indeed, whether or not it is may sometimes be just a matter of phrasology. For example, many children believe the world itself to be a functional system, in which cats were created to keep down mice, and dock leaves were created to keep in check the pain caused by stinging nettles. Now this belief can be formulated in a causal manner, by simply saying that the whole system is kept within certain limits through the effect of the behaviour of cats on mice, and of dock leaves on the pain generated by stinging nettles. Alternatively, it can of course be formulated (and generally is) in a purposive manner, in terms of what cats or dock leaves 'are for'. The content of the two assertions need not differ, though of course it can differ in as far as the attribution of purpose also contains the idea that there is some mind responsible for the creation of the whole system, a mind for which the purpose in question was a decisive consideration.⁶

Now the idea of a self-maintaining order is an interesting one and deserves some further elaboration. An order is 'self-maintaining' not only relatively to the permitted limits of change, as indicated, but also, and in somewhat different sense, it is relative to the amount of permitted external impact, i.e., the amount of external impact which it can, as it were, assimilate or digest or react against. A snail, for instance, is presumably a reasonably self-maintaining organism, but when crossing a road it cannot resist the external impact of a steam-roller. Most systems, excluding the universe as a whole, must count

with some external impact, and the attribution of self-maintenance must, once again, contain the specification of just how much external impact can be accommodated. A good deal follows from this point. For instance, it might be argued in defence of the applicability of the 'functional method' in modern circumstances, that the functional interpretations developed concerning traditional society must be tested by the adaptability of traditional institutions in modern circumstances.⁷

I doubt whether this particular defence is generally acceptable. The kind of steam-roller effect which the modern world has must be well beyond the range with which traditional institutions can, in general, cope, or can indeed be expected to.

Another consideration is of course—how much external impact can it tolerate, and how much internal potential for disruption does it possess? A perfect example of a functional equilibrium is a *vacuum*. One may reflect that the most elegant solution for the Creator would have been to create *absolutely nothing*, thus saving Leibniz his question as to why there was anything rather than nothing. Why indeed? There is something inelegant about creating something, and then needing other things to balance it, with the corollary that creating this rather than that opens the Creator to the inescapable charge of arbitrariness and partiality. It really would have been much more elegant to leave the whole thing entirely vacant, for ever and ever, no nettles and no mice.

This is something which does of course occur to the child if it becomes sceptical. Had not the Deity created mice, It need not have troubled Itself with the creation of cats. If only the Deity had not created stinging nettles, It need not have bothered with dock leaves. The world may be functional, but it is clumsily so. It contains one thing to counteract another, when it would have been much simpler to have neither one nor the other.

The functionalist anthropologist may not be tempted to play the part of a Leibnizian philosopher and ask why there is anything at all: he may well be content to find out how one thing sustains another and so on in a circle, and leave unasked the questions of why the society exists at all. This only highlights the no doubt trite observation that institutions and activities are not functional in themselves, but only in relation to each other. Perhaps we do not need to justify the circle as a whole, but we do need to establish that there is a circle. And to give an account of the method, we must specify the general nature of the relationship by means of which one institution sustains another. The activities of the one 'cause' the activities of another, or keep them within the appropriate limits. But just *how*?

The very best model for a functional system in equilibrium is an absolute vacuum, which after all has no potential disturbances either

inside or outside. The next best approximation is some rigid immobile homogeneous body which, excluding inner corrosion, is strong enough to resist external impact up to a certain level of vehemence. But neither of these resembles a social system, which after all consists of disjointed moving parts. A good model for a social system in equilibrium is perhaps one of those situations in chess in which neither player has any choice and the whole situation is repetitive. *This* is a social system 'in equilibrium': it is not at the mercy of the will of the participants, but perpetuates itself *whatever* they do, within the limits which are open to them.

One might say this of extreme functionalism: it takes the stalemate in chess as the paradigm of a society.

But the chess situation differs from a social equilibrium in one very important respect: and the highlighting of this particular feature is the main merit of using the chess situation. In chess, the rules which limit the movements of the players are supplied and given *from the outside*. What corresponds to this given element in the social situation?

A social system is like the game of chess in which the activities of the players generate and sustain not merely the situation in which they find themselves, *but also the rules of the game itself*. Nothing in the very nature and constitution of things prescribes the playing of this or that game, the rules of which would then lead to stalemate situations (in the case of stable societies) or progressive situations (in the case of developing ones). Or rather: very nearly nothing. *Nature* does impose certain limits. This provides part of the answer: compatibility, rules by which some things are required, are *in part* stated by nature. No complex of institutions which precludes the nourishment and physical reproduction, for instance, of a given population, can be self-maintaining. Certain rules of compatibility and incompatibility are thus supplied by the physical basis of human existence. But only some: for if one thing is obvious, it is that the natural environment plus the need or desire to survive do not uniquely determine social structure. It is simply not the case that given the same physical environment, and the same size of population, only one social structure is possible. What else, then, narrows down the range of possibilities?

In the case of the vacuum or a homogeneous inert mass, the question of 'compatibility' hardly arises. But in a complex made up of parts, what is it that makes one part compatible with another, or what makes one part 'sustain' another? In this context, we think too much in a spatial metaphor: we think of a jigsaw puzzle, where compatibility is easily understood. But institutions are not shapes occupying space, whose compatibility or mutual support are easily understood. They are activities, and above all, repeated activities. It

is important to stress here that for these things, the notion of compatibility is far from self-evident.

We tend to take social causation for granted. But it is in fact a rather puzzling phenomenon. There is here no push or pull. Somebody does something in one place, and *in consequence* somebody else does something else in another place. A man fires a shot and six runners set off on a track. A man raises a signal and an engine driver starts a train. What is the link?

When a man passes food to another and thus enables him to survive, or pushes him to his death, or even when he impels him to do something by a threat, there is a kind of intelligible physical causation present. But nothing of this kind is present in the examples cited. The physical world is perfectly conceivable in which the man fires the starting gun and the runners choose not to run. The connection has at any rate no immediate physical basis. If, then, nature did not supply the connecting rules, who or what did?

One is tempted to say, as the first attempt at a reply, that the *concepts* of the social order in question dictate the connection, or its conventions, or something of that kind. But that won't quite do: what dictates and sustains those concepts or conventions? Anyway, they are not always effective. We are, when facing a social system, facing something very odd indeed: a system whose parts interact by means of connections which *it itself generates*. (I am not here concerned with the question of how we discover social causal connections. I believe we discover them in the same way in which we discover any others, though we may be guided in our search by insight, by *verstehen*. I am concerned with what a social or cultural connection *is*, as opposed to a natural one, rather than how we discover or establish it.) How is the connection maintained?

The first and less puzzling sense in which institutions can be connected with each other, or the society of which they are a part, and have effects which contribute towards the 'explanation' of that society (be it stable or not), is the one arising when a given institution has, for instance, the consequence of safeguarding the food supply in a simple physical sense. Here a 'rule of the game' is supplied by nature, to the effect that a society does not persist unless its members are fed, and a 'move' is made, in the form of the working of an institution or complex of institutions which helps satisfy the need in question. But, as indicated, the rules are not always so supplied by nature. What happens in the other cases?

First, consider what may be called the Idealist solution. It would run something as follows: the concepts of the society themselves acquire a force as great as the rules supplied by nature herself: as great, or almost as great, as the rules demanding the supply of food, the conditions of procreation and so forth. It is the concepts of the

society itself which supply some of the 'rules of the game', analogous to the rules of chess in our previous example. When we say that a society is a stable functional system in which the various institutions sustain each other and check each other (or, for that matter, if we say that it is an unstable system leading through the interaction of its parts to a changing end result), the *nexus* between one institution, activity, etc., and its social effect, is provided by the *ideas* of the society itself. Just as nature, a set of data supplied from the outside, decrees that the consequence of the availability of food is the possibility or reality of survival, so culture, the set of ideas of the society itself, decrees for instance that the consequence of one situation (e.g., a certain transgression has been committed) is a certain consequence (e.g., a certain punishment is applied according to certain rules to certain people connected directly or indirectly with the transgression). It is a set of connections like these, dependent on the *ideas* or *concepts* of the society, which leads to the additional interplay of cause and effect (over and above that supplied by nature), and the play of all the complementary chains produced in this effect leads to a stable end result (or an unstable one, as the case may be).

There is an obvious and immediate problem here: the concepts which, as it were, provide the glue between one activity and its social consequences, which determine that *this* should cause *that*, socially speaking, are themselves in a very important and real sense institutions of the society in question. They in turn must be sustained and, for the matter, checked, protected from developing cancerous growth. This makes the system complex, but that is not necessarily an objection: societies *are* complex. There is one well-known short-cut available here, which happily and rightly has, on the whole, dropped out of anthropology: to claim that one sustaining mechanism is sufficient to explain all the concepts which abound in a given society, namely the mechanism of education. The argument is that, particularly in the case of primitive societies, men are so flawlessly indoctrinated, 'conditioned' into the concepts of that society that no one can conceivably think or act outside them. In fact, primitive societies are not such perfect specimens of a retrojected 'Brave New World'.

Eliminating this short-cut, we are left with a picture of causal connections in a society, either based on a nexus supplied by nature, or supplied by culture in the form of a concept of the society, these concepts then being in turn sustained in various ways, so that the social system as a whole is a by-product of natural *and* cultural connections, where the reliability of the cultural connections themselves is a by-product of the system.

But, in this unqualified form, this picture is part of what I've called the Idealist approach and, when left unqualified, incorrect. Basically,

it exaggerates two things: first, the power of concepts to guide the behaviour of men and, secondly, the length to which societies can indulge their free fantasy in living by this or that concept. The whole picture, as sketched in so far, would suggest that there is no limit to the kind of connections between activities which a society can impose in virtue of having, or creating, the appropriate concept. For instance, to take a relatively extreme example, a society such as Erewhon could exist, in which people were punished for illness but treated medically for criminal acts, and other societies could exist in which connections were not merely inverted versions, of what we are familiar with, but, in our eyes, totally arbitrary. There would, on this version of the theory, be no restrictive rules on what kind of concepts can exist.

I do not believe that the conceptual and social worlds are so limitlessly flexible. But what then is the alternative to the rejected, idealist picture? The alternative can, appropriately, be called the Materialist account. It would run something as follows: social causation remains very close to physical causation. Examples of physical causation are the nourishing effects of food, or the debilitating effects of undernourishment. When one social event is causally connected with another, what is happening is in principle similar to these examples. There are also things which cannot be physically defined, being conceptual artefacts, so to speak, of the society in question: some of these things might be called 'ideology' or 'culture' or something like that. These things are effected by physical causes but do not significantly react back. The real linkages in a social system, whose sustained interaction leads to stability or change in a society, are between things susceptible to causal connections, such as those summed up as hunger, fear, etc.

The materialist approach amounts to saying this: that which corresponds in sociology to the externally given rules of chess in the stalemate situation, are rules supplied by the physical environment of man (including, of course, those governing his own physiology, etc.). The most important and influential version of sociological materialism is somewhat less extreme than this, in as far as it seems to be saying something like this: the by-product of the interaction of man and environment, and man and man, is the production of certain tools. These, then, form part of the system and have effects of their own, and it is indeed the effects of type of tools which are the crucial factor in understanding and determining the shape of social systems.

How does the so-called structural-functional method stand with respect to the two extremes on the spectrum, from the idealist to the materialist answer, concerning the stuff and nature of social connections? It is, I believe, much closer to the materialist end than to the idealist one, without for all that being in the very least identical with it.

How could one characterise this method? I would like to sum up once again the formal framework within which, on my account, it must be fitted: a social system is an aggregate of parts; its 'parts' are human activities, etc., which interact with each other to produce a certain result; and the *manner* of their connection, the nexus by which they interact, is itself either one further institution of the society in question, or something supplied by the natural environment. We have here a structure in which *some* of the bricks are supplied by nature, but many are, as it were, themselves supplied by the structure and help to hold other parts of it in place.

It is worth noting that the (in my view rightly) fashionable concept of *structure* combines the valid aspects of the underlying intuitions of both materialism and idealism. The sensible aspect of the intuition of materialism I take to be this: our explanatory models should be built up in an orderly and systematic and economical way, in such a way that the properties of the model as a whole should as much as possible follow from the properties of its parts and their arrangement, and that these more primary properties, as it were, should be limited in number, clearly defined and, as far as possible, 'intelligible'. (Rigid materialism, of course, may well have been wrong in trying to work with a very restricted set of such properties, reducing them in as far as possible to the impenetrability of extended matter.)

Idealism, on the other hand, sets out from the perception that in social, semantic, psychological and other systems the *context* provided by the system as a whole is essential: the essence of the part is its role in the system. To take the simplest example, from the theory of meaning: a name is not just a relationship between a sound and a thing. The 'meaning' of a sound, which makes it a name, derives from the fact that it is part of a system in which other names exist, or at least are possible, and have a given role, and that this (not necessarily delimited) system of names relates to a whole set of things, isolated from the continuum in which they occur in accordance with certain principles, and so forth. In other words, one cannot even perceive that something has a name without first of all understanding the system within which it exists and is named.

The notion of 'structure' as used by anthropologists incorporates both these insights. The models are built up with care and with a minimum of invocation of explanatory notions or alleged connections which are not deducible from fairly elementary and manifestly powerful human tendencies. At the same time, the whole orientation of 'structural explanations' drives the investigator towards 'placing' any given activity in the context of the system of which it is a part, which gives it its 'meaning', and towards trying to make that large system explicit.

The same point can probably also be made concerning the ideas

underlying Information Theory, cybernetics, etc. The 'Idealist' insight is powerfully present: the models invoked presuppose, fundamentally, that the meaning of a message is its place within a system of alternatives and the rules or pattern of that system. There is no 'echo' theory of meaning here. On the other hand, of course, the fact that this approach leads to the building of concrete physical models highlights its 'materialist' connections—the system is material and its properties depend on the properties of its parts, and there is no suggestion that the system is itself somehow outside the world. In as far as these systems are 'idealistic', they could be described as a kind of mechanised idealism.

Under the impact of explicit and implicit procedural rules, modern anthropologists have developed a distinctive and easily recognisable style of reconstructing the social systems of the societies with which they are concerned. There are certain things an anthropologist will tend *not* to do. He will not rely on allegations of strange motives, a strange concept, or on inertia. Finding a strange custom, he will not invoke the alleged fact that the locals believe something or other (from which belief the custom is meant to follow): he has far too strong a sense of the fragility of belief, and also of the fact that the belief itself needs to be sustained, notably by the very custom which is justified in its name, and cannot constitute an independent and adequate explanation. Similarly, he will not suppose that the locals have been fashioned into possessing some strange and specific motives from which the set custom follows: the same kind of tacit reasoning excludes explanations of this kind. Least of all would he suppose that the custom can be explained by the supposition that it was once established in a specific situation and has since persisted by sheer inertia. (This kind of account, which of course is often built into the local legends themselves, he will contemptuously dismiss as 'just-so' stories when used by old-fashioned anthropologists.)

These things he will refrain from doing. But there are also certain things he positively will do. One is what I would like to call Power Accounting, or a Power Balance-Sheet. I believe that a Power Balance-Sheet is implicitly present in every good anthropological account of a given society. This consists of showing how the persistence of a given political or economic, etc., system is the result of the interplay of given forces in the given environment, observing the negative rules mentioned above—i.e., without placing too much explanatory strain on the assumption of an automatic persistence of strange beliefs, etc. The assumption is that people are very roughly similar all over the place, and are not perfectly socialised, i.e., are not total slaves of either the overt or the tacit norms of their society. Men will go off any kind of social rails. A Power Balance-Sheet shows how the system maintains itself even on the assumption of a reasonable

amount of deviance (and, incidentally, a reasonable amount of external disturbance as well).

The negative rules cited above do indeed contain an assumption which can be summed up as the very rough generalisation that people are much the same everywhere. Of course, in fact they are not: but it is a sound methodological rule, built into this method, to minimise the invocation of individual differences. To minimise is not necessarily to exclude altogether: but the requirement to try to find an explanation within the Balance-Sheet first of all is an *excellent* requirement.

The requirement that eccentric beliefs or aims should not too easily be invoked by way of explanation is not the same as their exclusion altogether. The method consists really of placing the onus of proof heavily on the side of the demonstration that strange beliefs, etc., are really present, operative, and sustained by other social factors. If this is indeed established, as it sometimes may be, *then* the further invocation of the eccentricity is permissible.⁸

The structural-functional method, as I am describing it, was not really internalised in the soul of the anthropologist by means of the official summaries of it which exist, but by the real education of the anthropologist: field work, and the subsequent systematic discussion of field work results in seminars by his peers and seniors. It is in these two crucial anthropological activities that the set of rules which I am trying to make explicit was forged and sustained. The method, and its deep internalisation and persistent effectiveness, was the causal, rather than the logical, consequence of the cult of field work.

The ideas of Malinowski, taken in isolation and coldly, may not be particularly original and hence, in that sense, not particularly important. But his position in the history of the social sciences is perhaps, in one respect, rather like that of Lenin in the history of political thought: it is impossible, or at any rate pointless, to investigate his ideas without at the same time being concerned with the institutions which were engendered by them. The importance of Malinowski lies perhaps in his fusion of a certain set of ideas into a kind of whole *and*, above all, in setting up the institutions, the traditions and the ethos which perpetuated the *application* of those ideas in cumulative and profitable research.

Two interesting theories of knowledge were associated with the cult of field work: knowledge by total immersion, and cognition by trauma. I have some sympathy for the former, the Baptist theory of knowledge, so to speak, but am rather sceptical about the second, at any rate when it is generalised. Many people have experienced the trauma of alien mores, without thereby gaining social understanding. Does the trauma work only if it is anticipated? And, in that case, is it genuinely traumatic?

The situation here is parallel to what I claim holds concerning the modern anthropologists' attitude to time: a mistaken doctrine of the self-sufficiency of the social instant served as premise, and overt stimulus, for a perfectly valid attitude towards social explanation. Similarly, an attitude to field work, justified by arguments which were not always cogent, and an attitude which itself may be exaggerated, led to the internalisation of what seems to me a valid type of social explanation. Of course, field work is an excellent thing, partly because it is interesting and enjoyable and partly because it brings in material which otherwise would not be available and which is generally of a far higher quality than that supplied by non-anthropologists. But the analytic method which is internalised by means of the cult of field work and its subsequent discussion by peers has merit quite independently of whether it was indeed preceded by, and internalised by means of, field work; and it can in fact also be applied to non-field-work material.

An additional note about the Power Accounting: the field work habit not only forces the anthropologist to account for the persisting situation (or the situation believed to persist) in terms of the operating forces, but it also forces him to make a kind of survey of all the forces operating in the society and show how they spend themselves. He must ask: why does *this* sub-group not break out, what constrains an individual in this position, etc., etc. This is implicit in the habit of spending a lengthy time in a fairly small community, taking a sociological census, and so on. This imposes a further check of facile and excessively abstract, context-less explanations. What is there must be included in the Power Balance-Sheet, just as the final result at the end of the Balance-Sheet must follow from what is documented in a recognised field work manner.

We can now see how this method differs from what I've called the Materialist and the Idealist accounts of how a social system is built up, and how it is rather closer to the materialist approach than the other. It might be called a 'multiform materialism', to differentiate it from the materialism which carried the doctrine, or the suggestion, that one material base implies one type of 'superstructure'—in other words, a doctrine of a one-one relation. The 'structural-functional method' certainly is not materialist in the sense of carrying any suggestion of such a one-one correlation. It differs from the materialist approach in being quite willing, if the evidence warrants it, to allow explanation in terms of institutions which are social, cultural artefacts, which are not simply dictated by the interplay between nature and an imaginary pre-social man. But it is close to the Materialist approach in placing a heavy onus of proof on any such claim, and thereby placing a healthy restraint on sociological fantasy. All this is, of course, closely connected with the cult of field work,

which itself curbs fantasy.⁹ One might object at this point: is this, then, all that the 'structural-functional' method amounts to?—an abstention from various kinds of facile explanations, a requirement that the accounting of social forces be thorough, and that the resultant situation be worked out, and effectively be compatible with the forces which are operating, and documented as such; and a cautious but not rigid approach in allowing cultural artefacts to play a part similar to the forces and requirements of nature? Is this really *all* there is to it?

This is indeed, I suspect, a good deal of what it does amount to: but I reject the implication that that isn't a great deal. It may not seem a great deal when cosily and briefly summed up in this manner. But its systematic implementation and application in the study and account of societies *is* a great achievement: indeed, the achievements of social anthropologists bear witness to this. The great achievement of Malinowskian social anthropology was perhaps the establishment and perpetuation of those social institutions *within* the anthropological community which led to the persistent, compulsive, thorough, cumulative applications of these relatively simple rules. Anthropologists were of course also aided by the general characteristics of the societies with which they were concerned: the communities in question were often small, and hence the kind of 'accounting' described could indeed be attempted on the basis of, say, two years of field work. The communities were indeed 'simple' in the sense that, relatively speaking, they wore their social hearts on their sleeves: not in the sense that what their members said corresponded to the reality of the situation, but that the reality of the situation itself could not be hidden in quite so many clouds of ambiguity as it is in complex 'developed' societies. All these advantages are often denied to the sociologist. But, given that the anthropologist did frequently have these advantages, nevertheless modern anthropology must be credited with having found the tools for exploiting them brilliantly.

The merits of the method have not changed or diminished (though there is no further need to tie them either to an attitude of timelessness or to a cult of field work). On the contrary, it seems to me that progress in sociology is conditional to a large extent on applying a similar attitude to large, complex and rapidly changing societies.

What has changed is not the merit of the method, but the external environment which once so greatly favoured the implementation of the method. Roughly speaking, tribes are getting rarer, and the colonial system has (almost wholly) disappeared.

What are the implications of this? In the beginning, I stressed that the differentiation between sociology and anthropology, and hence the relationship between them, was itself as much a social as a logical matter. It follows that the future fate of the method I have attempted

to analyse must itself be seen in social terms, i.e. in terms of its fate as operating in effective contemporary conditions, as much as in abstract logical terms.

The connection between modern social anthropology and the colonial system is obvious and has been commented on.¹⁰ Firstly and most obviously, the colonial system made field-work-based anthropology possible by making residence in tribal societies safe, by making the tribal societies relatively accessible (but not so accessible as to destroy them rapidly) and, often, by providing a certain protection for tribal institutions. Colonial administrations were not the same in all places, but in sufficiently numerous colonies they were willing to maintain traditional structures, from one motive or another, provided practices too deeply repugnant to the European moral sense were not indulged and sometimes even without such a proviso. Colonial administrations sometimes had their own version of functionalism, unaided by anthropologists. Some anthropologists are liable to overrate the originality of the functionalist doctrine of the meaningfulness and usefulness of even surprising tribal institutions in their particular social contexts.¹¹ It would have been surprising if this idea had been novel, for after all it had been the stock-in-trade of conservative political theory for quite some time. The ideas of Burke could be applied to tribal societies as much as to European ones—indeed, in view of the fact that tribal societies were assumed not to have been disrupted by a few centuries of rapid change, they might be supposed to be *more* applicable. What was true of England might, after all, apply equally well to northern Nigeria. Far from it being the case that anthropologists obligingly supplied the colonial administration with an ideology for using the tribes against the newly emergent, disrupted and hence revolutionary classes, it might well be that the anthropologist had brought back his functionalism in part *from* the district officer, who had picked it up from a conservative political background. Be that as it may: the anthropologist's employment of functionalism was considerably superior to the use made of it by conservatives, for conservatives had invoked it as an omnibus *carte blanche* justification of any unspecified archaisms in their own society, whereas anthropologists had used it in a concrete and specific manner to work out what I've called the Power Balance-Sheet of the communities they were studying.

The colonial system aided the field anthropologist not merely by providing him with security and transport. The anthropologist may not have been the intellectual lackey of colonialism, but he was in various very important ways ideologically inoffensive and hence there was no reason not to tolerate him. The anthropologist's aim was to find out how a tribal society worked, to draw the Power Balance-Sheet of the community he was studying, to describe how it *really*

worked. For at least two reasons, whatever he found or claimed to have found out was unlikely to be ideologically offensive to the new ultimate power holders, the colonial administration. For one thing, he was naturally, at any rate in the earlier years, concerned with finding out how the tribal society had worked prior to the colonial interference, and hence he tended to abstract from the goings on of the administration, imagining things in their state of sociological purity. If, in all good faith, you abstract from the administrator, you naturally are not likely to say things about the administrator which will annoy him. Secondly, the administration had no ideology of its own about how the tribe 'really functioned', and hence could hardly be upset by any findings on this topic (especially if, as indicated, the findings were liable to abstract from the activities of the administration itself).

There were other factors still. A colonial administration tended to be a genuine bureaucracy: that is to say, its functioning tended to follow set rules, and a member of it, however junior, who observed these rules was reasonably safe. Even if he was not safe, the possession of his post was not something so enormously superior to the professional openings available to him at home as to make him desperately keen at all costs to maintain it. This is not to say that intrigues or conflicts were absent in its ranks (though, on the other hand, the opportunities for intrigue amongst territorially dispersed district officers must be less than amongst bureaucrats in a ministry, with adjoining offices); but, nevertheless, the individual member of the bureaucracy was not involved in a crucial struggle to maintain his position, he was not precarious in the face of those he administered, and he did not need to involve them one way or the other in his intrigues, such as they were.

The situation is different in the post-colonial period. Take an independent ex-colonial country with a multi-party system (they do exist, contrary to popular belief): though the country probably subscribes in a nominal manner to the doctrine of civil service impartiality, in fact the civil servant, or the local administrator, is almost certainly involved in a very serious political struggle, in which the career stakes from his own viewpoint are very great indeed. He is unlikely to welcome an independent observer and busybody who amongst other things provides locals opposed to himself with the possibility, or the illusion, of a new channel of information and communication with the capital.

Or consider one of the more typical one-party states. The same facts operate, in as far as there may be conflict within the one political system, and the anthropologist, being outside the local system of sanctions and authority, provides a disturbing break in the authority structure. Moreover, both his existence, his views and, finally, the

report he publishes may be in conflict with what is ideologically required to be true.

It is not so much that under the colonial system the anthropologist had on his side the prestige of belonging to the dominant race, whereas now he does not: he still often does, by virtue of his contacts and his familiarity with the modern world, possess a privileged position amongst the local population. It is rather that the privileged position he now occupies tends, in the political setting, to act against him, whereas in the past it acted on his behalf. It is for this reason that the post-colonial world is not particularly favourable to anthropological research.

But these practical difficulties might be overcome by a skilful and determined field worker. Or, at least, they might be overcome in favourable circumstances. It is not the practical difficulties which constitute the biggest and most significant change in the working conditions of the anthropologist. The really important change is taking place in the nature of his subject matter, in the societies he is studying.

To begin with, a point by way of introduction: it is generally assumed that the forces of the modern world destroy small intimate communities, and substitute large structures. In one sense, this is not true. A colonial occupation, or a modernisation drive by a newly independent country, does not always rapidly destroy, for instance, the kinship structure or family organisation of a given people. An anthropologist can arrive, decades after the effective incorporation of the local community in a wider political system, and find these molecular social patterns, so to speak, relatively undisturbed. It is the *larger* units, the political achievements on a grander scale, which tend to disappear most rapidly, be it because they are rivals of the new institutions or because their functional prerequisites are more precarious. There are of course exceptions to this: the emirates of northern Nigeria, or the kingdom of Buganda spring to mind. But by and large, it is the large-scale groupings and institutions of the traditional world which disappear most easily (and where they do not, to what extent is there a real social continuity, as opposed to a merely nominal one, in the institution surviving from the traditional world into the modern one?).

Perhaps this generalisation will not survive careful scrutiny. But suppose it does; what follows? Are the modern political and other large-scale institutions which replace 'tribal states', where these existed, as amenable to anthropological enquiry as were those traditional ones which they replaced? In other words: is the district officer, or the secretary of the local branch of the political party, as amenable to anthropological interpretation as the chief or the cult priest?

The answer must be: *ultimately*, yes. 'Ultimately', it *must* be so if I am right in my contention that socio-anthropological method,

'structural-functional' interpretation is simply the paradigm of correct sociological method in any context. If this is so, then in the long run it must of course be applicable. It simply embodies the recognition that social life has at its base the repetition of certain activities, and that these cycles of activity, as well as changes in the pattern of the cycles, must be causally explained, and that the system of sanctions or incentives which canalise the concrete doings of individuals into grooves compatible with these cycles must themselves be explained without facile invocation either of nature or of conscious intention and belief, and so forth.

But, in the short run, the world of the district officer and of the secretary of the political party does differ very significantly from that of the chief and of the cult priest. The point is this: the technological limitations of 'primitive' society were a considerable help in narrowing the range within which one could seek the causal mechanisms which maintained the social structure. What are the sanctions, the multiple swords of Damocles, hanging over any society? Above all, starvation, anarchy, external aggression. A sociological account must explain how, outside the Garden of Eden, both life and order are maintained by a society in an indifferent or hostile environment. The means of production and of coercion available to primitive society are, by definition, very limited. This considerably simplifies the search for explanations. This also, in my view, gives a very special interest to those larger groupings and institutions and political structures which are sometimes evolved in primitive society: the achievement is so much greater, its mechanisms so much more interesting. A skyscraper built of mud is more interesting than one built of concrete.

Modern society differs from this in that it can allow itself a kind of sociological fantasy. Where productive and administrative techniques are so very powerful, the society can, from accident or ideological predilection, build up structures which are not the simplest or optimal means of attaining certain effects. There is here a clear analogy with the notion of 'functionalism' in architecture. Where technology is limited, one can see how the materials 'dictate' a given style of building. Where technology is as powerful as it now is, it is only the architect's preference for simplicity, in other words a particular aesthetic doctrine, which keeps him within 'functional' solutions. He can easily allow himself non-functional solutions, if his taste or that of his clients happens to require it. It is in this sense, amongst others, that functional interpretations are not immediately and easily applicable to modern societies. They have far too much technological and administrative leeway.

It is significant that anthropologists think of 'functionalism' as essentially a descriptive doctrine, whereas architects think of it as a normative or prescriptive one. 'Functionalism' seems descriptive in

sociology and prescriptive in architectural aesthetics. The reason is of course that modern building materials allow architects ample scope *not* to be functional, so that if they proceed 'functionally' it is from preference and not from necessity. The same may yet happen to societies. Not quite yet, perhaps: one still hears, for instance, that industrial society has certain kinds of functional preconditions which are not compatible, for instance, with strict Muslim religious observances, or that the functioning of secular Israel is not easily squared with strict religious Jewish observance. But the time may come when the progress of automation will make the strictest industrial discipline compatible with the greatest Muslim religious rigorism, and when a massive electronic automated *shabes-go'i* will make an industrial theocracy feasible. In other words, it is possible that fully industrial society will exceed in ritual and doctrinal fantasy anything achieved by 'primitive' society.

The present relevance of this point may very well be challenged, and I would not wish to be dogmatic about the extent to which it is now significant. It might be challenged as follows: the administrative power of modern societies should not be overrated. Consider those numerous and striking failures to mobilise populations in accordance with the wishes of the administration. Just because the central power possesses means of coercion, bribery, propaganda, information and communication, etc., that does not mean that these provide it with effective and adequate levers for organising the society according to its wishes. The actual life of societies is outlined within limits set not by deliberate plan, by what I called sociological fantasy, but by social reality. Is it not just for this reason that sociologists are now so fashionable, so very much in demand as advisers on the implementation of social reform and development?

There is clearly a good deal of truth in this counterassertion. The reply to it in turn consists of two points. First, and not very interesting, is that this is a matter of degree. The fact that the power of deliberate manipulation is not limitless, and in certain cases totally absent, despite the presence of modern administrative and technological means, does not mean that it is non-existent, and that it can be ignored in those many cases and areas where it has radically transformed the situation. Secondly, and more interestingly, there is this consideration: there is now a rather different relationship between the nominal and the real social structure of any given society. There is a sense in which, in traditional societies, the nominal political or religious structures were also parts of the real structure: however distinct from the real structures and however illusory or divergent from them in content, nevertheless the nominal structure usually had an important part to play. Its illusions or ambiguities were 'functional', in contributing something to the real functioning of the society which

they were in and described. The society could not allow itself the luxury of what I called sociological fantasy.

This is not so in modern societies. The real functioning may still be subject to very real limitations (though less so than in the past), and these are inadequately explored. But it is too easy to build up nominal hierarchies of authority on ideological precepts, social theories and so forth. It is very easy, the limits are so wide as to be hardly discernible, and there is very little presumption of 'functionality'. It is for these reasons that modern nominal structures are so very much less interesting than the old ones: it is not just romanticism which makes some anthropologists more interested in the old structures of a tribe than in a ministry or a district office. The former may be sociologically more significant: it may reveal the limits of what can be done, organisationally, with certain limited means. If assumed to persist over time, there is a strong presumption of functionality.

This brings one to the problem of typicality. The following objection is sometimes raised against anthropological method: an anthropologist will pick his village and investigate it intensively, and then present his findings as the structure of *the* Ruritanian village, without having any real basis for his explicit or implicit contention that the village he chose is typical of Ruritanian villages in general. There is a twofold answer to this: first, typicality is not at issue. Of course, an anthropologist should not make claims of typicality where he has no evidence to support such a claim, but a single Ruritanian village is of interest irrespective of whether it is typical or not. Given the limited resources available to Ruritanian villages, any one village constitutes important evidence of what can be done, given those resources. Secondly, the limitation of resources itself constitutes some evidence of typicality, even where there is no survey of the traits of Ruritanian villages in general. If one knows the general ecological conditions obtaining in Ruritania, and the institutional and conceptual devices available in Ruritanian culture and language generally, one automatically has *some* evidence of the limits of possible structures to be found in Ruritanian villages. (This evidence may be misinterpreted, and should of course, whenever possible, be supplemented by a genuine survey: but all the same, it does constitute some evidence.)

Both these arguments are of course very much weakened for a modern context. Where the presumption of functionality is weaker, good evidence of typicality matters more. There is much less of a presumption of a kind of limiting achievement within given means, and there is also much less of a presumption that one can know the limits of the cultural tools available: there is too much instability, change and heterogeneity in the modern world. For these reasons, knowledge of typicality becomes very much more important, and investigation of community in isolation correspondingly less valuable.

Typicality in space brings one back to the question of typicality in time, and the question of the presumption of stability as a tacit premise of 'functional' interpretations. In my earlier account of what underlay this assumption, I concentrated on the *logical* considerations: on the assumption of stability as a camouflaged form of the insistence on a realistic assessment of causal connections, which, once appreciated as such, can indeed be dissociated from that assumption of stability which had introduced it, and equally applied to unstable situations. (The latent function of a-historism, I claimed, was a realistic, structural approach to social conditions. Once this latent function becomes manifest, the old manifest rationale can be dropped.) But this (to my mind) admirable premise was not the only consideration present. There was also an empirical matter. Primitive societies were assumed to be stable in as far as they lacked the technical means for being *unstable*. They were assumed to lack the means to get anywhere from their present conditions, and a similar argument could be extended backwards: no special means were available in the recent generation to make the present condition an exceptional and temporary one but, on the contrary, it could be assumed to be somehow 'normal' for the society in question. Now this argument is something of a *non sequitur*: for although technological power generates rapid change, not all rapid change is generated by technological power. As no one put forward this argument *explicitly* (as far as I know), no one can be specifically charged with it, though I believe it was tacitly operative in helping to make the stability assumption acceptable. But there is, once again, an element of truth in it, despite its unacceptability as a generalisation. The element is this: stability can be assumed as a first approximation where there is no evidence to the contrary in primitive societies, whereas the strongest possible presumption against it exists in modern contexts. In modern contexts, positive evidence for stability would be required before one could really assume that recent generations were in a condition similar to the present one.

When this is true, the realistic assessment of causal connections, which on my argument is at the heart of sound anthropological and sociological method, cannot begin by simply looking for the way in which current practices contribute to their own perpetuation, because this perpetuation cannot be presumed to hold, and the method cannot proceed without positively trying to ascertain what the past situation was. To resume the earlier formulation, typicality cannot be assumed either over time or over space. Take this in conjunction with the fact that social structures are not the limiting exploitations of given means, for the means of the modern world are as yet unexploited, together with the fact that the divorce between nominal and real structures is sharper and different in kind from that which obtained in traditional

societies, and you can see that the anthropologist working in modern conditions lives in a world very different from that which formed his method and traditions a decade or so ago.

SUMMARY OF ARGUMENT

One of the crucial things about social anthropology is its method.

This method, when reduced to its bare bones, may sound obvious. Nevertheless, obvious though it may be, few men who investigate societies or theories about them have succeeded in not sinning against it. The distinctive and important thing about social anthropology is the creation of a tradition which sustains the effective application of this (theoretically obvious) method, and which minimises the danger of sinning against it. It is a tradition which itself contains institutional checks enforcing the 'right approach'.

The method consists in the first place of seeking causal connections between various institutions and activities. (Only in appearance is it teleological.) Obvious though this may seem, the effective implementation of this requirement distinguishes social anthropology from many styles of enquiry which only seek causal connections in an ineffectual and unrealistic manner. Furthermore, the method requires that the causal connections themselves should be explained: leaving aside those connections which are imposed by nature, there is nothing inherent in the nature of things which requires that one activity should have the modification of another one as its effect. If it does have such an effect, this is itself a social fact. It in turn requires support from other activities or institutions. Anthropological method requires that this circle be, as far as possible, closed. *This* is the essence of functionalism. Functionalism is not really a doctrine about what societies are like: it is rather an obligation placed upon anthropological enquiry. It contains excellent recipes for avoiding facile ways of achieving this end.

This method and its application was engendered and sustained by various factors within the anthropological tradition and in the wider world in which it operated. Some of these factors were doctrinal and, it so happens, mistaken, but the abandonment of these mistaken ideological props does not require the abandonment of the method itself. (Examples: the doctrine of social stability, built into 'functionalism', or the doctrine of the irrelevance of the past.) Others were customs of the anthropological community itself, such as the institutionalisation of field work. This is a good but not a necessary thing, and can again if necessary be abandoned without the abandonment of the method itself. Others still were social conditions of the wider world, notably those prevailing under the 'colonial system', and these are in any case disappearing. Thus many of both the doc-

trinal and socio-environmental props of the method are disappearing. These are general features of the modern world which make the application of the method more difficult.

Thus the application of the method to new situations and in new conditions is more difficult than it was in the past. It is, however, eminently desirable.

NOTES

1 Cf. E. E. Evans-Pritchard, *Essays in Social Anthropology*, London, 1962, Essays 2 and 3.

2 Cf. I. Schapera, 'Should Anthropologists be Historians?', *Journal of the Royal Anthropological Institute*, 92, July 1962, 143-56.

3 Latent functions were not discovered by anthropological functionalists. Long before, they were familiar to philosophers as the Cunning of Reason.

4 This shows how mistaken it is to attack 'functionalism' along the lines adopted by Kingsley Davis, who argues ('The Myth of Functional Analysis as a Special Method in Sociology and Anthropology', *American Sociological Review*, 24, December 1959, 757-72) that 'functional' explanations do not differ from any other kind of explanation in sociology and that consequently functionalism is a myth. Functional explanations may indeed not differ from other causal explanations: but the whole point is that the doctrine and method against which functionalists were reacting was, in a very important way, vacuous in its supposedly causal explanations. Functionalism thus differed from *inadequate* causal explanations.

5 This point in no way prejudices the order in which various connections are noted or established. One may perceive a molar connection without having fully worked out or understood a molecular one, and of course vice versa.

6 It is interesting to note that primitive societies tend to have their own evolutionism and their own functionalism. They interpret the world purposively, but they also explain its causal arrangements in terms of antecedent events, whose effects somehow continue to pervade the world.

7 This was argued to me in conversation by Dr Ioan Lewis.

8 It is sometimes claimed that the anthropologist, as such, has no opinion about the validity of, e.g., witchcraft beliefs. (This claim is to be distinguished from the stronger, and even more mistaken, view that local concepts are never mistaken.) This seems to me incorrect. In fact, the

anthropologist knows full well that witches do not exist, and that *consequently* he is obliged to explain how witchcraft beliefs are sustained. True beliefs do not require to be explained nearly as much—though they too need a social explanation. There are social explanations of true beliefs just as there are of false beliefs, but when beliefs fly in the face of evidence the social mechanism presumably needs to be that much stronger. But it is precisely the anthropologist's awareness of the false elements in local belief, that helps him select the areas requiring special explanation.

9 There is a puzzle about the Materialist approach which I do not quite know how materialists themselves face. Their position treats the social, cultural, conceptual artefacts as a kind of epiphenomenon. But if some class of human activities is mainly or largely epiphenomenal, why should it exist at all? What need is there to have an excrescence which echoes, but does not in turn have significant effects? Is it just a kind of causal accident, it just so happens that society generates it, without it being in any way essential to the system? I suspect Materialism is ambivalent and inconsistent at this point: it both treats the 'superstructure' as epiphenomenal, and yet also as pretty essential to the maintenance of the system—which suggests that it does have crucial effects after all. Anyway, if it is epiphenomenal, is its *specific* content irrelevant? Could a society of one kind have *any* kind of cultural superstructure? And, if not, can the superstructure be epiphenomenal? And there are distinctions to be drawn: what is epiphenomenal—the fact that there is a superstructure at all, or the specific cultural content it has? And, if the latter, to what degree of specificity is it determined by the substructure?

10 Cf., for instance, Peter Worsley, *The Trumpet Shall Sound*, London, 1957, p. 260.

11 Cf. Dr Ian Hogbin, 'Malinowski's Theory of Needs in R. Firth, ed., *Man and Culture*, London, 1957, pp. 245-64; see esp. p. 248.