RADCLIFFE-BROWN LECTURE IN SOCIAL ANTHROPOLOGY

SOCIAL ANTHROPOLOGY: A NATURAL SCIENCE OF SOCIETY?

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As a start let me emphasize the query at the end of my title. In my view the scientific status of social anthropology is very much an open question.

In part my argument will be historical. I am concerned with changes in the theoretical attitudes of social anthropologists so dates are relevant.

A. R. Radcliffe-Brown died in 1955. His career as a professional anthropologist had begun in 1906.[1] His publications were not extensive. Apart from two important items which I shall mention presently, virtually all his significant published work had been written before 1949.[2]

My contrasted hero Claude Lévi-Strauss began his anthropological career in 1932.[3] He has written a great deal. His distinction as a theorist first became apparent as the result of work published in the United States between 1943 and 1945. His status as an international celebrity dates from 1955, the year that saw the publication in French of *Tristes Tropiques*,[4] a collection of autobiographical reflections on the nature of anthropology.

The practical overlap between these two figures is thus very minor. Lévi-Strauss has recognized a limited debt to Radcliffe-Brown; there was no feed-back influence the other way.

The work of Radcliffe-Brown was an important influence in British social anthropology from about 1910 onwards and over the decade 1945–55 it was dominant. It was a weakening influence thereafter.

Lévi-Strauss's work began to affect British social anthropology some time around 1950 and, despite persistent denunciation,

[1] For outline of Radcliffe-Brown's career see Firth (1956).

[2] See bibliography in Fortes (1949).

[3] For skeletal outline of Lévi-Strauss's career see Leach (1970).

[4] Lévi-Strauss (1955).

became a dominant influence about ten years later. This influence has not yet been replaced.

Perhaps I can best explain my theme by analogy with another pseudo-science. In the summer of last year a Congress of 1,500 Freudian psycho-analysts assembled in London and, if press reports are to be believed, devoted most of its energies to internal feuding. The argument was not really about what psycho-analysts do but about the models they should use in the interpretation of their observations.

On the one side there were the conservative traditionalists who held that the old Freudian model, which assumes an identity between psychic phenomena and the forces and quantities of the empirical sciences, is still, with minor modifications, quite good enough. On the other there were the radical reformers, led by a contingent from Paris, who rejected the assumption that causal-deterministic principles derived from the physical sciences can be usefully applied to the study of human beings who exercise conscious choice. These latter, the radical reformers, deployed the language of contemporary semiology. Psycho-analysis was to be regarded as a 'biological theory of meaning'. Psycho-analytic theory needs to be reformulated in terms of communication theory . . . and so on.[1] All of which will sound very familiar to the professional social anthropologists in my audience.

To those outside the profession the definitional limits of what social anthropology is often seem very obscure. This does not worry the professionals themselves. Social anthropology *is* what social anthropologists *do*; academic debate is all focused around styles in interpretation and explanation.

The concept of 'explanation' is itself very slippery. Academics of all kinds clearly feel that they are engaged in a kind of jigsaw puzzle activity. 'Explanation' consists of fitting together a number of isolated pieces of information so that they begin to look as if they formed part of a larger pattern. There are many different kinds of explanation and all of them are provisional, that is to say, they express what is probable rather than what is certain, though many things are so highly probable that we can ignore the uncertainty.

As indicated by the Congress of Psycho-analysts, debate about the nature of explanation leads to polarization.

One such polarity is that which divides *conservatives* and

[1] Fuller (1975).

radicals. *Conservatives* will cling desperately to their accustomed models and persuade themselves, in defiance of all probability, that all newly discovered facts will fit in with what they think they know already. *Radicals* have a prior commitment to destroy whatever model of reality had seemed acceptable to their predecessors; they therefore concentrate all their attention on the lack of fit between the new information and the old. As Thomas Kuhn and Michel Foucault have both argued in their different ways, the two sides never really carry on a dialogue; they talk past one another.[1] In the end the conservatives die off and the counter-culture viewpoint of the radicals becomes a new orthodoxy. With hindsight, historians then recognize an intellectual revolution.

Another major polarity is that which distinguishes *rationalists* from *empiricists*. In terms of my jigsaw puzzle analogy, *rationalists* start out with a clear-cut idea of the picture they are going to construct and then search around for pieces to fit into it. *Empiricists* work the other way round. They start with a pile of pieces which they assume will fit together and develop their picture *ad hoc* as they go along.

The radical rationalists are obsessed with the need to develop new theories without regard for the utility of old ones, while the radical empiricists constantly demand the collection of new facts, the implication being that if only you have enough facts you will be able to see that reality is chaos and that all search for systematic order is futile self-deception.

By contrast a conservative empiricist tends to get hooked on to a model which he formulated early on out of the first pieces of practical evidence that he happened to pick up. Thereafter he will either ignore all exceptions or resort to statistics to show that they are so unusual that they can be ignored. But for a conservative rationalist facts of any kind are just a nuisance. He may even adopt the stance that since explanation is only concerned with models in the human mind the empirical evidence does not matter at all.

My professional colleagues in this audience will, I think, agree that the practice of social anthropology, as it has developed over the past forty years, has exhibited each of these several polarities in very clear-cut form. In particular Lévi-Strauss often behaves as a prototypical conservative rationalist while

[1] Kuhn (1962) on changing paradigms; Foucault (1966) on changing *epistemes*.

Radcliffe-Brown was the perfect example of a conservative empiricist.

The old-guard conservative empiricists are not yet all dead and it may well be that by the time they are gone the whole discipline of social anthropology will have disappeared and transformed itself into something else—a general theory of linguistics for example—but meanwhile it may be of interest to take a look at the transformation in process.

Most forms of explanation in contemporary social anthropology can be typecast under one or other of four labels: functionalist, structuralist, Marxist, and structural-functionalist. In part a particular author's characteristic style will be determined by the nature of his interests and his position on the rationalist-empiricist continuum.

Orthodox *functionalist* explanation of the 'everything fits together like the gearwheels of a watch' variety is especially compatible with a relatively detached interest in the political and economic organization of small-scale local communities and appeals to those who are themselves, by temperament, far out on the empiricist wing.

Structuralism, which tends to reinterpret interpersonal economic and political transactions as acts of communication, appeals most strongly to those who feel that the elementary forms of the religious life are of greater fundamental interest than primitive economics. Its practitioners claim that the ultimate objective of social anthropology is to gain an understanding of the workings of the human mind. Its appeal is to those who are rationalist by temperament.

Marxist explanation of the sort which Raymond Firth has labelled 'gut Marxism' is likely to take over whenever an anthropologist who has started out with functionalist assumptions tries to give his economic and political interests a wider spatial and temporal context. This is especially the case if he allows his human sympathies to get the better of his supposedly scientific judgements, so that participant activity replaces participant observation. But 'cerebral Marxism', which has all the hallmarks of an intellectual religious exercise, is often hard to distinguish from structuralism.[1]

Finally, *structural-functionalism* is a style congenial to those who still hope that there might some day emerge a *social science* with

[1] I have borrowed the distinction between 'gut' and 'cerebral' Marxism from Firth (1972).

clear cut causal deterministic laws as precisely spelled out as those of Newtonian physics. As is the case with Marxism, rationalists and empiricists both feel that they can accommodate themselves to this style of argument but then engage in bitter dialectical debate with one another.

Structural-functionalism was in large measure the personal invention of A. R. Radcliffe-Brown who is commemorated in the lecture series to which I am now contributing. Twenty years ago, as I have indicated already, it was the dominant viewpoint in British social anthropology.

I would hesitate to pronounce a funeral oration and declare that structural-functionalism is now completely dead, but it is certainly on the way out. My concern for the remainder of this lecture will be to consider what has become of the explanatory fashion which Radcliffe-Brown first established.

So that brings me to my title.

Radcliffe-Brown's book *A Natural Science of Society*,[1] with no question mark, was published in 1957 and is a posthumous work. Its history is as follows.

In 1931, in the course of a highly peripatetic academic career, Radcliffe-Brown reached the University of Chicago in the status of Visiting Professor of Anthropology. He stayed on in the capacity of Distinguished Scholar. In the spring of 1937 he was still at Chicago but had just been appointed to the newly established Chair of Social Anthropology at the University of Oxford which he was due to take up the following October. From this prestigious position he took the floor at a senior faculty seminar to deliver his views on the scientific status of social science in general. Radcliffe-Brown spoke without manuscript and almost without notes but the proceedings were transcribed in shorthand. With Radcliffe-Brown's approval the resulting digest of this stenographic record was later put on sale in mimeograph form in the faculty bookshop. The text ran to 80 pages of single-space type and carried the full title: 'The nature of a Theoretical Natural Science of Society: Notes on a Discussion in a Seminar at the University of Chicago, 1937'.[2]

From time to time over the next fifteen years or so Radcliffe-Brown expressed his intention to revise the text for formal publication but since he never got round to doing so it must be assumed that, despite the *ad hoc* circumstances of its production, he considered the existing text to be a fair representation of his

[1] Radcliffe-Brown (1957). [2] Radcliffe-Brown (1948).

opinions. The posthumous, 1957, book version is substantially the same as the mimeograph.

The fact that Radcliffe-Brown talked rather than wrote *A Natural Science of Society* gives it a special kind of historical importance. Radcliffe-Brown's academic influence depended much more on what he said than on what he wrote. He had a large imposing physical presence, he was very fluent, and he was superficially knowledgeable about a great variety of subjects. In his talk he put himself forward as a polymath know-all, and got away with it far more often than he deserved. But it was, I think, precisely because a great many of the things he *said* in this way were never written down and made available for close inspection that his 'influence' over his immediate disciples persisted in the way it did. If they had more generally *read* rather than listened to the kind of argument that is presented in *A Natural Science of Society* they would surely have been less impressed?

But let me emphasize again. I am not now concerned with what was right or wrong about Radcliffe-Brown's argument but rather with what has become of that argument in the context of the kind of debate which goes on among social anthropologists in 1976.

I do not propose to give you a full digest of Radcliffe-Brown's book though I shall presently discuss certain features of it. But first let me say something about Radcliffe-Brown's over-all viewpoint.

He was emphatically a conservative. He claimed explicitly that the 'theory' that he expounded in Chicago in 1937 was directly derived from the pages of the early volumes of Durkheim's *L'Année sociologique* published in Paris before the First World War. On other occasions, when anxious to claim priority over Malinowski, he maintained that the whole essence of his system had already been incorporated in a course of lectures delivered in Sydney in 1910!

In practice the cast of his argument was probably not quite so inflexible as he pretended but there can be no doubt that when Radcliffe-Brown talked about 'natural science' he was recapitulating ideas which he had picked up in his undergraduate days at Cambridge right at the beginning of the century. That was the hey-day of museums. Science teaching was focused around showcases exhibiting specimens classified by types—fossils, rocks, insects, stuffed birds, caged animals in zoos—fixed entities, changeless, everlasting.

But although Radcliffe-Brown laid great stress on the supposed empirical basis for what he called 'the abstract structural principles of the social system', his theorizing fitted pretty badly with the available evidence even in 1910. It did not fit at all with what was known by 1937.

Radcliffe-Brown did not try to escape from this difficulty, as some of his successors have done, by claiming that his structural principles apply only to statistical norms rather than particular cases. Instead he slipped, perhaps almost unconsciously, into a semi-rationalist position. He had started out by talking about facts; he ended up talking about concepts. But it was the showcases of Cambridge museums at the beginning of the century which provided the model for those concepts. 'The fundamental problems' of a theoretical social science, he declared, 'must depend on the systematic comparison of a number of societies of sufficiently diverse types'.[1]

This simplistic, nineteenth-century view of the relationship between the natural order of things and the taxonomies which scientists employ to describe them bears directly on my thesis that there has recently been a basic shift in the epistemological assumptions that anthropologists make about their subject-matter.

From 1960 onwards Lévi-Strauss has made a number of unexpectedly complimentary remarks about Radcliffe-Brown's contributions to anthropological theory. In particular, he has claimed that the latter's 1951 Huxley Lecture applies to the analysis of Australian totemism a transformational view of structure closely akin to Lévi-Strauss's own, and that this represented a major break-through in Radcliffe-Brown's thinking.[2]

To those who were present on that occasion it seemed that Radcliffe-Brown was in fact saying precisely what he had said a great many times before. Indeed only one year *after* this event, in a letter addressed to Lévi-Strauss in 1952, Radcliffe-Brown roundly declared:

> I use the term 'social structure' in a sense so different from yours as to make discussion so difficult as to be unlikely to be profitable.[3]

I suspect that it was this *ex cathedra* statement more than anything else which led Radcliffe-Brown's most immediate disciples to ignore Parisian heresy for the next twenty years.

[1] Radcliffe-Brown (1957), p. 141.
[2] Lévi-Strauss (1962*a*), pp. 155–64, cf. Radcliffe-Brown (1951).
[3] Radcliffe-Brown (1952).

However, if we look at the available evidence it would seem that Radcliffe-Brown's ideas about structure, although significantly different from those which Lévi-Strauss eventually developed, were nothing like *so* different as he himself seems to have supposed.

In the letter in question, Radcliffe-Brown explains what he means by structure as an empirical pattern of consistency by reference to the shapes of sea shells. I will quote at some length. Radcliffe-Brown writes:

> While for you [Lévi-Strauss] social structure has nothing to do with reality but with models that are built up, I [Radcliffe-Brown] regard the social structure as a reality. When I pick up a particular sea-shell on the beach, I recognise it as having a particular structure. I may find other shells of the same species which have a similar structure, so that I can say there is a form of structure characteristic of the species. By examining a number of different species, I may be able to recognise a certain general structural form or principle, that of a helix, which could be expressed by means of a logarithmic equation. I take it that the equation is what you mean by 'model'. I examine a local group of Australian aborigines and find an arrangement of persons in a certain number of families. This I call the social structure of that particular group at that moment of time. Another local group has a structure that is in important ways similar to the first. By examining a representative sample of local groups in one region, I can describe a certain form of structure. . . . The structural form may be discovered by observation, including statistical observation, but cannot be experimented on. . . . In dealing with Australian kinship systems, I am really only concerned with arriving at *correct* descriptions of particular systems and arranging them in a *valid* typological classification. I regard any genetic hypothesis as being of very little importance, since it cannot be more than a hypothesis or conjecture.[1] [My italics.]

Notice the fundamental point that Radcliffe-Brown is here taking it for granted that his sea-shell *species* are naturally existing, real entities naturally distinct from one another, and not, as Buffon would have argued,[2] simply the arbitrary product of the application of rules of taxonomic classification.

[1] Radcliffe-Brown (1952).

[2] 'Il n'existe, dit il, réellement dans la nature que des individus; les genres, les ordres et les classes n'existent que dans notre imagination.' This frequently cited opinion is given in P. Flourens, *Histoire des travaux et des idées de Buffon* (Paris: Paulin) (1845), as if it were a direct quotation from C. G. L. de Malesherbes, *Observations sur l'histoire naturelle de Buffon et Daubenton* (Paris) (1798), t. 1, p. 38. The quotation does not in fact appear at that reference but it corresponds in a general way to Malesherbes's detailed account of Buffon's views about natural and artificial classifications.

Though written in 1952, the general style of the argument is indistinguishable from that of the 1937 Chicago discourse. However, in this 1952 case, the actual metaphors employed, with their references to forms, helical shells, logarithmic equations, and conjectural genetic hypotheses make it almost certain that the immediate source of Radcliffe-Brown's phraseology is D'Arcy Thompson's *On Growth and Form*, first issued in 1916 and revised in 1942. I do not know when Radcliffe-Brown first read Thompson's classic or how often he consulted it afterwards, but the echoes in 1952 seem very clear.[1]

Let me comment briefly on this latter book.

Like Radcliffe-Brown, Thompson posed as an empiricist. He starts with the precise description of material objects and moves step by step towards analytical and mathematical generalization. But he wrote this way because he was a biologist addressing other biologists. He in fact called himself 'a biologist with an inkling of mathematics' while insisting that the real task was for mathematicians who would eventually have to develop a general systems theory into which practical reality would be seen to fit.

In his final chapter entitled 'On the theory of transformations and the comparison of related forms', which I can recommend to you all, Thompson makes clear what sort of mathematics he had in mind.

Considering the immense changes in biological viewpoint that have come about as a result of the discovery of the genetic code and of recent developments in the mathematical theory of classification, Thompson's arguments have stood up amazingly well. But what is especially interesting and relevant for my present purposes is that the first reference to Thompson in the whole of Lévi-Strauss's massive four-volume *Mythologiques* comes fifteen pages from the end of the final volume.[2] He there quotes from Thompson's own concluding paragraphs:

[1] Thompson (1916–42). Professor Fortes, who knew Radcliffe-Brown very well during the latter part of his life, tells me that he is not aware that Radcliffe-Brown ever referred to Thompson either in talking or writing, but he also tells me that he himself was long ago struck by a parallelism between some of Radcliffe-Brown's formulations and those of Thompson. He suggests that 'some of his thinking went back to sources he had in common with Thompson in the first decade of this century'. In any case the similarity between the condensed argument in Radcliffe-Brown's 1952 letter and the extended exposition in Chapter XI of Thompson's book seems to me much too close to be entirely accidental.

[2] Lévi-Strauss (1971), p. 604.

A 'principle of discontinuity' is inherent in all our classifications, whether mathematical, physical or biological; and the infinitude of possible forms, always limited, may be further reduced and discontinuity further revealed by imposing conditions, as, for example, that our parameters must be whole numbers of proceed by *quanta*, as the physicists say.[1]

It is on the basis of this quotation that Lévi-Strauss launches into his final coda, in the course of which he recapitulates in splendid oratory all the essentials of his structuralist thesis that binary coding is the basic universal principle of both nature and culture. No précis could do justice to the metaphysical exultation with which the reader is invited to recognize how the wonders of nature are grasped by the metaphors and metonymic associations of the human mind through the transformations of common structures.

I certainly do not propose to guide this audience through the labyrinth of rhetoric by which in the course of the final six pages we move rapidly from the big bang origin of the universe to the reproductive mechanism of orchids, pass by a renewed denunciation of Sartre, return again to D'Arcy Thompson, reflect on the binary structure of the visual mechanism of cats, and end up with a contemplation of Hamlet contemplating a universe of not-being. But what is here said, in convoluted form, about the principle of discontinuity inherent in all classifications is very relevant to my present theme.

Very roughly, Lévi-Strauss seems to argue as follows. He agrees with Thompson that, in the sensible world out there, the processes of evolution, physical and genetic, operated upon by mathematical constraints of probability and quantum jumps, has produced a discontinuous field. He would accept Thompson's comment that 'We cannot transform an invertebrate into a vertebrate by any simple or legitimate transformation . . . not by anything short of reduction to elementary principles'.[2] On the other hand, at the level of elementary principles, *everything* is possible. The universe of myth, which is the creation of the human mind, differs from the sensible world out there in the following respects: the discontinuities are arbitrary not given, they are unambiguous and binary with no fuzzy overlap at the edges, there are no transformational constraints, any pattern can (at least in theory) be transformed into any other.

In practice it does not really work out like that. The concepts which make up the tidily organized universe of myth are

[1] Thompson (1942), p. 1094. [2] Lévi-Strauss (1971), pp. 615–21.

modelled on percepts which we receive through our senses. But what we perceive is not just a mirror of what really is. Perception is already worked upon by the intellect before it passes messages to the brain. To some extent at least we see what we expect to see. For example, seventeenth-century scientists working with early microscopes managed to see fully formed homunculi within the human spermatozoa which they examined. One consequence of such intellectual modification of perception is that when we project our conceptual image of the world back on to the world out there in order to work upon it, the constructed discontinuities of the one do not fit with the natural discontinuities of the other. The world of reality out there then appears to us to be fuzzy at the edges, disjointed in the wrong places. Ritual, according to Lévi-Strauss (though perhaps I misrepresent him), is a procedure we adopt to overcome the anxieties which are generated by this lack of fit between how things really are and how we would like to think about them.[1]

The argument between the Radcliffe-Brownian structural-functionalists and the Lévi-Straussian structuralists, around which my present lecture is focused, turns on just this point, the lack of fit between ideal categories and empirical discontinuities. Should recognized discontinuities of social structure be thought of as naturally existing—like the distinction between vertebrate and invertebrate—or should they be seen as arbitrary impositions of the human intellect, like the left and right sections of a straight line notionally bisected in the middle?

Before going back to Radcliffe-Brown's Chicago seminar let me draw your attention to one or two other features of his simile between *species* of sea shells and varieties of Australian kinship systems.

It must be remembered that at this point in time Radcliffe-Brown must have read Lévi-Strauss's *Elementary Structures of Kinship* which had appeared in 1949.[2] The first part of that book contains an account of Australian kinship systems which is quite close to the version which Radcliffe-Brown had himself published in 1930,[3] but then, by an adroit variation of parameters, Lévi-Strauss tries to persuade his readers that the Australian systems, regarded as a set, can be treated as a particu-

[1] Lévi-Strauss (1971), p. 603. But the whole discussion of pp. 596–603 is relevant. Since my own view of the relation between myth and ritual, thought and action, is very different from that of Lévi-Strauss it is very likely that I have misrepresented his argument.

[2] Lévi-Strauss (1949).

[3] Radcliffe-Brown (1930–1).

lar transformation of a much more general phenomenon, that of 'alliance', which is manifested in an entirely different form in another set of kinship systems located empirically in mainland eastern Asia. It is this kind of conjuring trick that Radcliffe-Brown is objecting to when he says: 'Structural form may be discovered by observation, including statistical observation, but cannot be experimented with.'[1]

Moreover, as I have remarked already, it is a fundamental point in Radcliffe-Brown's argument that species differences in nature are 'real' and not just something that has been imposed on the data to suit the convenience of taxonomists. It is against this background assumption that there is one, and only one, correct way of providing a true scientific description of the contents of the external world that he applies the analogy to human societies. 'I am really only concerned with arriving at *correct* descriptions of particular systems and arranging them in *valid* typological classification'.[1] It would not have been in any way surprising if Radcliffe-Brown had argued this way in 1910, but he wrote this in 1952!

Let us get back to Chicago in 1937.

I won't attempt to summarize the whole argument about a theoretical natural science of society, but will pick out some key points.

At the outset Radcliffe-Brown insists that the starting-point of all scientific inquiry is observation rather than speculation or inspired hunch. The scientist always moves from the particular to the general, not the other way round. The selection of modes of generalization is to some extent arbitrary but this is where the skill of the great scientist lies; genius is a matter of finding the *right* rules and the *right* abstractions.

Explicitly we are told that 'the method of science is one involving observation, classification, and generalisation, not as separate processes but as parts of a single complex procedure'.[2]

The prototype exponent of this method of science is declared to be Galileo. What Radcliffe-Brown tells us about his hero's intellectual standpoint is contradicted by the historical evidence but Radcliffe-Brown's own position emerges quite clearly: 'Systems of *a* man, *a* cell, *a* society, exist. They are real concrete phenomena. [The concepts] "man", "cell", "society" exist in the abstract, but not as abstract *systems*. You never have abstract

[1] Radcliffe-Brown (1952) as quoted above.

[2] Radcliffe-Brown (1957), p. 28. In the citations which follow I give the pagination in the 1957 book and not the 1948 mimeograph.

social systems.'[1] In other words, the Durkheimian metaphor 'a society is like an organism' is taken to be true in a literal empirical sense. It is not an abstract analogy. Relations are empirical facts out there in the world, not ideas in the mind.

The difficulties of this position were just as obvious to Radcliffe-Brown as they are to everyone else. His way of dealing with the matter was to pile on additional metaphors and hope that his listeners' scepticism would be worn down in the process. Since his audience has been assured that 'the first step in the development of any science is taxonomic' it follows that the would-be social scientist must classify. But what should he classify? On this occasion Radcliffe-Brown talked about animals instead of sea shells. I quote again: 'What is a zoologist doing when he is defining a lion? He is giving you the characteristics of all systems which fall into the class lion. All he has to do is to look at certain animals, perhaps not even dissect them, and he is then able to classify them quite soundly.'[2] From this base Radcliffe-Brown then develops the inference that 'a society' is a self-perpetuating natural system strictly comparable to an animal species.

Notice that it is lion as species rather than any individual lion that provides the model for a society. This is because both a species and a society exhibit continuity through time. Radcliffe-Brown refers to the genetic relationships between 'a father lion, a mother lion, a son lion' and finally to 'the inner relationships of the lion through periods of time and through a series of reactions'[3] but there is no mention of the lion's adaptive relationship to its environment or to other species. The whole discussion focuses on the natural separateness of a class of real objects. Definition of the class is treated as discovery. The implication is that in specifying the characteristics of a human society we are recognizing (discovering) the distinctiveness of something which is there already.

The argument is at best defective, at worst fraudulent. The listener naturally supposes that he is being asked to believe that one society differs from another society as the species lion differs from the species elephant. But the constituents of the species lion are individual lions while the constituents of the species elephant are individual elephants, and even if you work downwards to more and more general comparisons, lion and elephant remain immutably distinct, even to the level of

[1] Ibid., p. 31. [2] Ibid., p. 32. [3] Ibid., p. 33.

chromosome and gene. But, on the same basis, the constituents of *a* society are individual human beings and as such are completely interchangeable between any one society and any other.

You may perhaps think that this weakness of analogy is so obvious as to be trivial but this is not so. Radcliffe-Brown's simplistic comparison between 'a society as a system' and 'an animal species as a system' carries with it the fundamental racialist assumption that was so deeply embedded in nineteenth-century anthropology, the belief that not only is every primitive tribe a naturally separate thing in itself but that the constituent members of such tribes are likewise naturally distinct as kinds, so that each tribe is quite properly described as a separate race.

There is no suggestion in any of Radcliffe-Brown's published work that he harboured racialist sentiments of this kind yet it would seem that he had never really thought through the implications of his early anthropological training and this failure to fit his model to his own experience seems to have inhibited his thinking to a very marked degree. It is surely very surprising that despite his emphasis on the notion of system and his fondness of biological analogies, he never seems to have shown any interest in the Darwinian notion of adaptation through continuous natural selection. It was the separateness and differences between human societies that interested him, not their interactions with each other or with the rest of their environment.

Incidentally, it is only during the last ten years or so that anthropologists have begun to break out of this straight jacket and to think seriously about human ecology, not just as a relationship between a society and a static environment but as a whole set of continuously adaptive sub-systems within an unbounded matrix. It has long been obvious that man is always modifying any environment he occupies, often at a precipitate rate, but anthropologists have been very slow to recognize that any modification of the environment is also, by feedback, a modification of the human social system within it.

I dare say Radcliffe-Brown would have accepted this proposition but he preferred to concern himself with what he considered to be more fundamental problems.

Some time in the future the study of history in process might become a proper subject for anthropological investigation, but that time had not yet arrived: 'it is absolutely necessary to study separately how societies persist in maintaining their type in spite of internal change and how societies change their type.... The first major task of analysis I conceive to be the synchronic study of the society. Such a study is more fundamental than the diachronic one.'[1]

So let us get back to the basic analogy: *a* human society is like an animal species, both need to be viewed as closed, on-going, self-perpetuating systems.

The most obvious difficulty about this metaphor lies in the problem of boundaries. A lion, considered as an individual, is a free-standing natural entity separated from its environment by a natural skin. Until we get down to rather sophisticated levels of physiology there is really no problem about deciding on the difference between the inside and the outside of a lion.

But this is not the case with species. Modern research in genetics and applied probability theory shows that clear-cut boundaries between one species and another do not emerge as a natural inference from empirical facts. It is only the *idea* of species that is unambiguous. By *definition* members of one species do not interbreed with members of any other species. But definition is not discovery. A species is a rational construct, not a 'real concrete phenomenon'. Leaving aside the new complexities which have been introduced through the techniques of genetic engineering, it has always been the case that the species of empirical reality lack objective homogeneity and are blurred at the edges. The way that the human observer slots empirical individuals into one species category rather than another is determined by his definitions, not the other way round.

But what happens to this concept of a defined natural boundary in the case of the so-called organic analogy of society?

We have been told that the social scientist must establish a taxonomy of naturally existing social systems. He must therefore be able to know where one system ends and another begins. But what is there in social affairs which might be considered to constitute a natural boundary in this sense? Admittedly modern sovereign states have national frontiers. There are objective criteria by which the traveller can know whether he is in France or Switzerland but this sort of thing is not the norm for human society as a whole.

In his Chicago discourse Radcliffe-Brown was in no hurry to tackle this seemingly fundamental issue but he did get round to

[1] Radcliffe-Brown (1957), p. 88.

it in the end. He then pretended that there was no problem. I will quote again at some length:

> I am suggesting that the most expedient abstraction we can make of *a* society is to take a territorially delimited group which seems to be not only clearly marked off from other groups but which is also sufficiently homogeneous in most respects of the behaviour of its individuals, if not in all of them, so that the similarities can be discovered and constitute a material which can be adequately described.
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> The anthropologist does this fairly simply with savage tribes. He generally takes the abstractions made by the savages themselves. I go into a savage country and say 'Who are you?' As a matter of fact, what I say is, 'What language do you talk?'. They give me the name of their language, 'We are the Kariera people'. They have given themselves a name. Then I ask 'Do these people over the river speak Kariera also?'—'Yes'. 'Are these people over the hump Kariera?'—'No'. They will offer details, and they will mark off for you a definite territory and people who talk the same language and say those are Kariera. On the whole, language usually constitutes the line of demarcation. There is a single region which can be described as Kariera by the fact that Kariera is spoken there.[1]

Radcliffe-Brown then goes on to admit that 'in certain regions of Africa it becomes difficult to decide what unit to take'. Then, in the next paragraph, he qualifies his argument still further: 'I am insisting again that the procedure contains an arbitrary element', and finally, in complete anticlimax, we are told 'a society is a body of people, in certain relations, which we study as a unit—as a *conceptually* isolated system—to compare with other similar units.... I do not believe there is any more precise definition which can be given.'[2]

Notice that the method of verbal delivery has allowed Radcliffe-Brown to contradict himself quite directly. At p. 31 of the printed text we were told that 'a society is a system' and all systems are 'real concrete phenomena'. 'You never have abstract social systems.' But now at p. 60 'a territorially delimited group' is 'the most convenient abstraction we can make of a society'. Admittedly he has covered himself by pointing out at p. 31 that we use the notion of abstraction in more than one sense, but he has fudged the argument just the same!

Furthermore, having first led his listener to suppose that the coincidence of territorially delimited group and language group is normal for the Australian aborigines as well as for 'most savages', we end up by discovering that what we have to com-

[1] Radcliffe-Brown (1957), pp. 60–1. [2] Ibid., p. 62.

pare are not 'concrete phenomena' at all but '*conceptually* isolated systems'.

The trick is a verbal trick. In print the self-contradiction is obvious, but by skilful oratory—(and one must suppose that the passages I have compared were probably spoken on different days of the week)—the listener is led to imagine that the facts on the ground (the concrete phenomena) are really quite close to the idealized set of conceptually isolated systems.

I have not drawn attention to these passages simply as a means of poking fun at the ghost of Radcliffe-Brown, but because they illustrate very well the semi-rationalist stance which Radcliffe-Brown finally came to adopt. Radcliffe-Brown remained consistently conservative; he never qualified his original view that his hypothetical social science would reveal an orderly universe governed by natural laws. But in the light of experience he came to modify his original expectation that if he arranged his empirical facts in their proper natural classes then the natural regularities would be revealed. The connection between the facts on the ground and the natural order of things was evidently more complicated than he had supposed. But having thus qualified his empiricism he ended up talking about an imaginary world of ideal social types.

This shift of view was not peculiar to Radcliffe-Brown; it was predictive of what was to happen in social anthropology as a whole.

I do not want to suggest that we have *all* become out-and-out rationalists in the Lévi-Straussian manner. On the contrary, and particularly in Cambridge under the guidance of Professor Fortes and Professor Goody, the tradition of British empiricism has been most staunchly upheld, but there is now a much greater willingness to recognize that the way we cut up the empirical cake for the purposes of analysis is a matter of convenience rather than something that is given by nature, and that however we choose to make discriminations between one social system and another there will always be a fuzziness at the edges, and that it is in this fuzzy boundary area, where our typological assumptions do *not* fit, that the problems of real theoretical interest are likely to be found.

A case in point is provided by Professor Goody himself who has devoted a whole series of publications to the relationship between local group nomenclature and rules of inheritance in an area of North-Western Ghana where a zone of patrilineal inheritance and a zone of matrilineal inheritance abut. Professor

Goody does not contradict himself, but his emphases have changed. At the start, despite a guarded dissociation from the views of his mentors, Radcliffe-Brown and Fortes, he was much concerned to distinguish types of social system in a Radcliffe-Brownian manner.[1] But at the beginning of his latest paper on this theme he lays stress on the inhibitions that have been imposed upon social anthropologists by their general commitment to the 'idea that they are examining "societies", "social structures" or "cultures" which operate in some sense as "systems", as boundary maintaining units'.[2] He then goes on to apply himself specifically to the problem of boundaries—with what happens, for example, when as a result of intermarriage across a jural frontier such as this, an individual has relatives in both camps.

Perhaps, even by my insults,[3] I have myself contributed something to this change of view. Like other British social anthropologists Professor Goody has moved beyond the phase of butterfly collecting—that is of docketting types of society—to a more rewarding investigation of the actual processes of historical development.

Let me go back to Radcliffe-Brown in Chicago. You will have noticed that in my earlier quotation from *A Natural Science of Society* Radcliffe-Brown refers to named groups of Australian aborigines such as the Kariera as 'savage tribes'. Comparably Malinowski regularly referred to the Trobriand Islanders as 'savages'.[4] Nobody worried about such usages at the time, but when Lévi-Strauss's *La Pensée sauvage*, which should perhaps have been decoded as 'thought in the wild', was put out as *The Savage Mind* nearly all English-speaking anthropologists of my acquaintance were appalled.[5]

This new squeamishness is another indicator of changing views. The use of the term 'savages' by Radcliffe-Brown and Malinowski was again a part of the nineteenth-century tradition by which it was taken for granted that anthropologists are primarily concerned with 'primitive' peoples, species apart, who are *ipso facto* inferior, and who can on that account be treated as

[1] Goody (1956), pp. iii, 16–26; Goody (1957), *passim*.

[2] Goody (1969), (1970).

[3] Leach (1961), p. 3. The repudiation of the idea that societies 'operate in some sense as "systems", as boundary maintaining units' is the central theme of Leach (1954).

[4] e.g. Malinowski (1932).

[5] Lévi-Strauss (1962*b*), (1966).

experimental objects, like animals in zoos. Radcliffe-Brown's analogy 'a social system is like a species' was wholly consistent with this tradition.

By contrast, the highly elaborated techniques of field-work by participant observation, which have gradually been developed out of Malinowski's original innovating procedures, are wholly inconsistent with this sort of purported 'objectivity'. The modern anthropologist is studying intercommunicating human beings, friends, fellow anthropologists, people who are in intimate personal relationship with himself as well as with one another, not specimens in glass bottles. How can you study people who answer back and change their minds if you persist in thinking of them as specimens dissected on a laboratory bench or observed at a distance through a microscope?

Whatever links there may be between social anthropology and natural science it is certainly not *that* sort of natural science. So one palpable necessity is a change of metaphor, and it may be worth considering the metaphors which the natural scientists themselves now tend to employ.

Radcliffe-Brown's schoolboy conviction that the central concern of all science is the discovery of natural laws was, after all, abandoned by the natural scientists themselves a great many years ago, certainly long before 1937! The universe, physical, chemical, and biological, which scientists now seek to understand is *not* a changeless vista of the Great Chain of Being governed by immutable laws imposed by the nature of Nature at the beginning of time. It is an evolving system in which the relations between the ever-changing constituent elements are constantly assuming new patterns in new combinations. In a universe of this sort the most interesting events, the events that generate change, are those which are statistically improbable.

The improbable, change-generating events occur at random against a background of imperfect order. We now know that, in general, biological systems reproduce themselves with quite extraordinary precision and just how this comes about is certainly very interesting. But we also know that every now and again the precision breaks down, and that is much *more* interesting!

With the increasing attention paid to uncertainty, natural scientists have come to recognize that mechanical analogies are quite inappropriate. Ordinary machines cannot make mistakes. So the mathematical general systems theory which D'Arcy Thompson envisaged is now discussed in the language of communication engineers; serious experts speculate about the attributes of an ideal computer which might be endowed with 'artificial intelligence'.[1]

And this rationalist, mathematical view of a universe of intercommunicating entities possessing the essentially *human* characteristic of intelligence has begun to feed back into the scientist's perception of empirical things.

In molecular biology the error-making replicating link is described as 'message bearing RNA'. Such language is surely highly significant? Everyone's prototype model for a message-generating entity is the human mind. So instead of social anthropology becoming a theoretical natural science of society, biology seems to have become a theoretical social science of nature!

Radcliffe-Brown would, I imagine, have reacted to this inversion of his basic premiss much as he reacted to Lévi-Strauss's rationalist use of the concept of structure; yet in some ways these developments make the notion of a Natural Science of Society more rather than less plausible.

I have stressed throughout this lecture that we are all of us *both* rationalist and empiricist. Individual bias apart, we are all concerned with the interplay between ideal constructs and the way they are interpreted in social behaviour. Once that is recognized it should be obvious that a typology of mechanically articulated modes of social integration such as Radcliffe-Brown envisaged becomes as irrelevant as the theory of phlogiston. But if our concern is with these *two* levels, with how things are thought about versus how things really are, with jural rules versus what men actually do, with myth versus ritual, with practices versus praxis, then it makes sense to say that, in some very general but not easily specifiable sense, the whole of human culture operates 'like a language'.

This at least gives the social anthropologist a reasonably specific set of problems—what are the limits of the analogy 'culture operates like a language'? Hopefully at the end of the day he will find that in fact there are no limits—that is to say that the language-like character of human culture is a quality shared by biological systems in general, and that the ultimate theoretical natural science of society turns out to be a kind of general linguistics which incorporates non-verbal communication at one extreme and Darwinian notions of natural selection at the other.

This is more than verbal rhapsody.

[1] e.g. Winograd (1973).

At the beginning of this lecture I suggested that most contemporary work in social anthropology can be typecast as relying on one of four major types of explanation, functionalist, structuralist, Marxist, and structural-functionalist. The weakness of functionalism in its earlier forms, as developed by Malinowski and his pupils, was similar to that of the structural-functionalism of Radcliffe-Brown. It conceived of human society as consisting of closed, discrete, integrated systems with fixed boundaries functioning within a stable environmental matrix. But a modified functionalism, which views the environmental matrix as itself part of a total network of pliable unbounded relationships—an *ecological* anthropology as its practitioners describe it—is, both in method and in aim, not at all unlike 'ethology', that is the scientific zoological study of the evolution of the behaviour of wild animals in their natural environment. So it might almost seem as if we were back once more at Radcliffe-Brown's biological analogy between animal species and types of society.

But anthropologists need to handle this new vogue in socio-biology with extreme caution. In its more sensational forms it has the effect of reducing social anthropology to a crude behaviourism.[1] Men are once again reduced to the status of impotent machines activated by laws of nature over which they exercise no control. Moreover socio-biological model building embodies very obvious racialist presuppositions—every social group has its proper social station, just as every animal species has its proper environmental niche.

On the other hand, an ecological anthropology, which takes into account the fact that, as man modifies his environment, he also modifies himself and his society, could, in principle, take over from both Vico and Karl Marx the doctrine that:

> It is a truth beyond all question that the world of civil society has certainly been made by men and that its principles are therefore to be found within the modifications of our own human mind.[2]

If a natural science of society must for ever be searching for cosmic laws of nature—supernatural forces in face of which Man is impotent—then I for one am quite uninterested. But a scientific anthropology which could explain not only how man

[1] Wilson (1975) only refers in passing to the implications of socio-biology for social anthropology, but his references to Robin Fox, Lionel Tiger, Desmond Morris, and Robert Ardrey as anthropologists who are working on the right lines, from his point of view, is not reassuring.

[2] Vico (1744) (para. 331 in the Bergin–Fisch translations).

could live in his environment without destroying it, but also how the conscious control of civil society could establish a fit between what is actually the case and what we desire—that would be quite another matter.

But that again implies a humanization of natural science, and here I am an optimist.

Any type of anthropological explanation which concentrates attention on the actual processes of economic transactions within small scale, face-to-face communities necessarily takes us back to Malinowski's 'principle of reciprocity', and from there it is only half a step to Mauss's 'general theory of gift exchange' and to Lévi-Strauss's transformation which reinterpreted the reciprocity of economic exchange as the reciprocity of interpersonal communication.[1]

And indeed, in their different ways, contemporary functionalist, structuralist, and Marxist social anthropologists are all tending to converge on a type of explanation which views social behaviour as a network of communication. So far, so good; but if anthropologists are to make the most of this kind of metaphor they need to acquire a much better understanding not only of structural linguistics but also of the thought processes of computer programmers and communication engineers.

The two key scientific principles that are common to all these fields are *transformation* and *feed-back*. What happens when a message in one language is translated into another language? What is the nature of bilingualism? What happens when we switch codes either verbal or non-verbal? These are not just transformational processes; the responses are interactional and cybernetic.

Each of us regularly adapts his or her behaviour to a whole range of special situations. According to where we are, we speak differently, we act differently; but at the same time we learn by experience. In this cultural sense all of us are polylingual to a most marked degree. We know very little indeed about how such code switching is accomplished or how it comes about that some transformations are acceptable as within the bounds of normality while others are rejected as incomprehensible and foreign, or how what was once abnormal later becomes normal. But these are scientific problems which should be capable of scientific investigation and analysis.

[1] Malinowski (1926), especially chapters 2 and 3; Mauss (1924); Lévi-Strauss (1949), chapter 5.

You see, I have come back once again to the problem of social boundaries. Radcliffe-Brown's formula that 'language usually constitutes the line of demarcation' needs to be heavily qualified, but the related statements by D'Arcy Thompson and Lévi-Strauss remain very much to the point.

The empirical transformations of structure which occur among things in the world out there are subject to limitation and discontinuity. In principle, the operations of human thought are subject to no such limitation. But such freedom of thought is only 'in principle'. The practical, scientific, dare I say Marxist, task of social anthropology is to understand better how, at any given time, our modes of thinking are in fact conditioned by the state of the environment in which we operate. Where Radcliffe-Brown talked about taxonomy, we should be thinking about feed-back. That too is a part of a theoretical natural science of society.

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