

what a historical or ideographic anthropology would look like fell on the more sympathetic ears of E. E. Evans-Pritchard.

In his Marett Lecture of 1950, 'Social anthropology: past and present', Evans-Pritchard virtually reiterated what Kroeber had written fifteen years previously about the relation between anthropology and history. These were his words:

I agree with Professor Kroeber that the fundamental characteristic of the historical method is not chronological relation of events but descriptive integration of them; and this characteristic historiography shares with social anthropology. What social anthropologists have in fact chiefly been doing is to write cross-sections of history, integrative descriptive accounts of primitive peoples at a moment in time which are in other respects like the accounts written by historians about peoples over a period of time...

(Evans-Pritchard 1950: 122)

Returning to this theme over a decade later, in a lecture on 'Anthropology and history' delivered at the University of Manchester, Evans-Pritchard roundly condemned – as had Kroeber – the blinkered view of those such as Radcliffe-Brown for whom history was nothing more than 'a record of a succession of unique events' and social anthropology nothing less than 'a set of general propositions' (Evans-Pritchard 1961: 2). In practice, Evans-Pritchard claimed, social anthropologists do not generalise from particulars any more than do historians. Rather, 'they see the general in the particular' (ibid.: 3). Or to put it another way, the singular phenomenon opens up as you go deeper into it, rather than being eclipsed from above. Yet Evans-Pritchard was by no means consistent in this view, for hardly had he stated it than he asserted precisely the opposite: 'Events lose much, even all, of their meaning if they are not seen as having some degree of regularity and constancy, as belonging to a certain type of event, all instances of which have many features in common' (ibid.: 4). This is a statement fully consistent with what, following Nadel, we might call the sigma principle of comparative generalisation, and flies in the face of the Kroeberian project of descriptive integration, or preservation through contextualisation.

In defence of Radcliffe-Brown

The problem is that once the task of anthropology is defined as descriptive integration rather than comparative generalisation, the distinction between ethnography and social anthropology, on which Radcliffe-Brown had set such store, simply vanishes. Beyond ethnography, there is nothing left for anthropology to do. And Radcliffe-Brown himself was more than aware of this. In a 1951 review of Evans-Pritchard's book *Social Anthropology*, in which the author had propounded the same ideas about anthropology and history as those set out in his Marett lecture (see Evans-Pritchard 1951: 60–61), Radcliffe-Brown registered his strong disagreement with 'the implication that social anthropology consists entirely or even largely of ... ethnographic studies of particular societies. It is towards some such position that Professor Evans-Pritchard and a few others seem to be moving' (Radcliffe-Brown 1951b: 365). And it was indeed towards such a position that the discipline moved over the ensuing decade, so much so that in his Malinowski Lecture of 1959, 'Rethinking anthropology', Edmund Leach felt moved to complain about it. 'Most of my colleagues', he

grumbled, 'are giving up in the attempt to make comparative generalizations; instead they have begun to write impeccably detailed historical ethnographies of particular peoples' (Leach 1961: 1). But did Leach, in regretting this tendency, stand up for the nomothetic social anthropology of Radcliffe-Brown? Far from it. Though all in favour of generalisation, Leach launched an all-out attack on Radcliffe-Brown for having gone about it in the *wrong way*. The source of the error, he maintained, lay not in generalisation per se, but in comparison.

There are two varieties of generalisation, Leach argued. One, the sort of which he disapproved, works by comparison and classification. It assigns the forms or structures it encounters into types and subtypes, as a botanist or zoologist, for example, assigns plant or animal specimens to genera and species. Radcliffe-Brown liked to imagine himself working this way. As he wrote in a letter to Claude Lévi-Strauss, social structures are as real as the structures of living organisms, and may be collected and compared in much the same way in order to arrive at 'a valid typological classification' (Radcliffe-Brown 1953: 109). The other kind of generalisation, of which Leach approved, works by exploring a priori – or as he put it, by 'inspired guesswork' – the space of possibility opened up by the combination of a limited set of variables (Leach 1961: 5). A generalisation, then, would take the form not of a typological specification that would enable us to distinguish societies of one kind from those of another, but of a statement of the relationships between variables that may operate in societies of *any* kind. This is the approach, Leach claimed, not of the botanist or zoologist, but of the engineer. Engineers are not interested in the classification of machines, or in the delineation of taxa. They want to know how machines work. The task of social anthropology, likewise, is to understand and explain how societies work. Of course, societies are not machines, as Leach readily admitted. But if you want to find out how societies work, they may just as well be compared to machines as to organisms. 'The entities we call societies', Leach wrote, 'are not naturally existing species, neither are they man-made mechanisms. But the analogy of a mechanism has quite as much relevance as the analogy of an organism' (ibid.: 6).

I beg to differ, and on this particular point I want to rise to the defence of Radcliffe-Brown who, I think, has been grievously misrepresented by his critics, including both Leach and Evans-Pritchard. According to Leach, Radcliffe-Brown's resort to the organic analogy was based on dogma rather than choice. Not so. It was based on Radcliffe-Brown's commitment to a philosophy of process. On this he was absolutely explicit. Societies are *not* entities analogous to organisms, let alone to machines. In reality, indeed, there are no such entities. 'My own view', Radcliffe-Brown asserted, 'is that the concrete reality with which the social anthropologist is concerned ... is not any sort of entity but a process, the process of social life' (1952: 4). The analogy, then, is not between society and organism as entities, but between social *life* and organic *life* understood as processes. It was precisely this idea of the social as a life process, rather than the idea of society as an entity, that Radcliffe-Brown drew from the comparison. And it was for this reason, too, that he compared social life to the functioning of an organism and *not* to that of a machine, for the difference between them is that the first is a life process whereas the second is not. In life, form is continually emergent rather than specified from the outset, and nothing is ever quite the same from one moment to the next. To support his processual view of reality, Radcliffe-Brown appealed to the celebrated image of the Greek philosopher Heraclitus, of a world where all is in motion and nothing fixed, and in which it is no more possible to regain a passing moment than it is to step twice into the same waters of a flowing river (Radcliffe-Brown 1957: 12).

What his critics could never grasp, according to W. E. H. Stanner (1968: 287), was that in its emphasis on continuity through change, Radcliffe-Brown's understanding of social reality was thoroughly historical. Thus we find Evans-Pritchard, in his 1961 Manchester lecture, pointing an accusing finger at Radcliffe-Brown while warning of the dangers of drawing analogies from biological science and of assuming that there are entities, analogous to organisms, that might be labelled 'societies'. One may be able to understand the physiology of an organism without regard to its history – after all, horses remain horses and do not change into elephants – but social systems can and do undergo wholesale structural transformations (Evans-Pritchard 1961: 10). Yet a quarter of a century previously, Radcliffe-Brown had made precisely this point, albeit with a different pair of animals. 'A pig does not become a hippopotamus... On the other hand a society can and does change its structural type without any breach of continuity' (Radcliffe-Brown 1952 [1935]: 181). This observation did not escape the attention of Lévi-Strauss who, in a paper presented to the Wenner-Gren Symposium on Anthropology in 1952, deplored Radcliffe-Brown's 'reluctance towards the isolation of social structures conceived as self-sufficient wholes' and his commitment to 'a philosophy of continuity, not of discontinuity' (Lévi-Strauss 1968: 304). For Lévi-Strauss had nothing but contempt for the idea of history as continuous change. Instead, he proposed an immense classification of societies, each conceived as a discrete, self-contained entity defined by a specific permutation and combination of constituent elements, and arrayed on the abstract coordinates of space and time (Lévi-Strauss 1953: 9–10). The irony is that it was from Lévi-Strauss, and not from Radcliffe-Brown, that Leach claimed to have derived his model for how anthropological generalisation should be done. Whereas Lévi-Strauss was elevated as a mathematician among the social scientists, the efforts of Radcliffe-Brown were dismissed as nothing better than 'butterfly collecting' (Leach 1961: 2–3). Yet Lévi-Strauss's plan for drawing up an inventory of all human societies, past and present, with a view to establishing their complementarities and differences, is surely the closest thing to butterfly collecting ever encountered in the annals of anthropology. Unsurprisingly, given its ambition, the plan came to nothing.

I do not pretend that Radcliffe-Brown's approach was without contradictions of its own. On the contrary, it was mired in contradiction from the start. Much has been made of Radcliffe-Brown's debt to the sociology of Emile Durkheim (1982 [1917]), and for Durkheim, of course, societies *were* self-contained entities, each with its own individuality, which could nevertheless be classified in terms of the possible combinations of their constituent parts.² But where Lévi-Strauss took this principle of discontinuity to its logical extreme, Radcliffe-Brown – influenced as much by Whitehead's (1929) philosophy of organism as by Durkheim's sociology – moved in the opposite direction, to re-establish the principle of continuity. This attempt to refract the process ontology of Whitehead through the classificatory epistemology of Durkheim, though brave, was bound to fail. Inevitably, social life reappeared as the life of society, emergent form as pre-existent structure, the continuity of history as the alternation of stability and change (Ingold 1986: 153–154). Indeed there was no way in which Durkheim's first rule of sociological method, *to consider social facts as things*, could be squared with Radcliffe-Brown's idea of social life as a continuous and irreversible process. Nevertheless I have found more inspiration in this idea of the social as a life process than in all the criticisms that have been levelled against it put together. Divested of the deadweight of Durkheim's sociology, I believe it is an idea that we can and should take forward from Radcliffe-Brown in forging a conception better suited to

our times of what a genuinely open-ended and comparative anthropology could be. Quite simply, it would be an inquiry into the conditions and possibilities of social life, at all times and everywhere. To be more precise, I need to explain what I mean by both 'social' and 'life'.

Social life and the implicate order

In a series of seminars presented at the University of Chicago in 1937, subsequently transcribed and published under the title *A Natural Science of Society*, Radcliffe-Brown dwelt at some length on the distinction between social science and psychology (Radcliffe-Brown 1957: 45–52). The matter was for him absolutely clear-cut. Psychology studies the mind, and mind is a system of relations between states internal to the individual actor. They are, so to speak, 'under the skin'. Social science, however, deals with relations between individuals, not within them. 'The moment you get outside the skin of the individual', Radcliffe-Brown declared, 'you have no longer psychological, but social relations' (ibid.: 47). The deep-seated assumption that mind is an internal property of human individuals that can be studied in isolation from their involvement with one another or with the wider environment continues to reverberate within the field of psychology. It has, however, been widely challenged (see Chapter 6, p. 86). One of the first to issue such a challenge was the great pioneer of psychological anthropology, A. Irving Hallowell. In an extraordinarily prescient paper on 'The self and its behavioral environment', published in 1954, Hallowell concluded that no physical barrier can come between mind and world. 'Any inner-outer dichotomy', he maintained, 'with the human skin as boundary, is psychologically irrelevant' (Hallowell 1955: 88). Fifteen years later, Gregory Bateson made exactly the same point. Mind, Bateson insisted, is not confined within individual bodies as against a world 'out there', but is immanent in the entire system of organism–environment relations within which all human beings are necessarily enmeshed. 'The mental world', as he put it, 'is not limited by the skin' (Bateson 1973: 429). Rather, it reaches out into the environment along the multiple and ever-extending sensory pathways of the human organism's involvement in its surroundings. Or as Andy Clark has observed, still more recently, the mind has a way of leaking from the body, mingling with the world around it (Clark 1997: 53).

I invoke the word 'social' to signify this understanding of the essential interpenetrability or commingling of mind and world. Far from serving to demarcate a particular *domain* of phenomena, as opposed – say – to the biological or the psychological, I take the word to denote a certain ontology: an understanding of the constitution of the phenomenal world itself. As such, it is opposed to an ontology of the particulate that imagines a world of individual entities and events, each of which is linked through an external contact – whether of spatial contiguity or temporal succession – that leaves its basic nature intact. In the terms of the physicist David Bohm (1980), the order of such an imagined world would be *explicate*. The order of the social world, by contrast, is *implicate*. That is to say, any particular phenomenon on which we may choose to focus our attention enfolds within its constitution the totality of relations of which, in their unfolding, it is the momentary outcome.³ Were we to cut these relations, and seek to recover the whole from its now isolated fragments, something would be lost that could never be recovered. That something is life itself. As the biologist Paul Weiss put it, in a 1969 symposium on the future of the life sciences, 'the mere reversal of our prior analytical dissection of the Universe by putting the pieces together again ... can yield no complete explanation of even the most elementary living system'