Being Alive

Essays on movement, knowledge and description

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ANTHROPOLOGY IS *NOT* ETHNOGRAPHY

Acceptable generalisation and unacceptable history

The objective of anthropology, I believe, is to seek a generous, comparative but nevertheless critical understanding of human being and knowing in the one world we all inhabit. The objective of ethnography is to describe the lives of people other than ourselves, with an accuracy and sensitivity honed by detailed observation and prolonged first-hand experience. My aim, in this final chapter, is to demonstrate that anthropology and ethnography are endeavours of quite different kinds. This is not to claim that the one is more important than the other, or more honourable. Nor is it to deny that they depend on one another in significant ways. It is simply to assert that they are not the same. Indeed this might seem like a statement of the obvious, and so it would be were it not for the fact that it has become commonplace – at least over the last quarter of a century – for writers in our subject to treat the two as virtually equivalent, exchanging anthropology for ethnography more or less on a whim, as the mood takes them, or even exploiting the supposed synonymy as a stylistic device to avoid verbal repetition. Many colleagues to whom I have informally put the question have told me that in their view there is little if anything to distinguish anthropological from ethnographic work. Most are convinced that ethnography lies at the core of what anthropology is all about. For them, to suggest otherwise seems almost anachronistic. It is like going back to the bad old days – the days, some might say, of Alfred Reginald Radcliffe-Brown. For it was he who, in laying the foundations for what – in the early decades of the twentieth century – was the new science of social anthropology, insisted on the absolute distinction between ethnography and anthropology.

He did so in terms of a contrast, much debated at the time but little heard of today, between *idiographic* and *nomothetic* inquiry. An idiographic inquiry, Radcliffe-Brown explained, aims to document the particular facts of past and present lives, whereas the aim of nomothetic inquiry is to arrive at general propositions or theoretical statements. Ethnography, then, is specifically a mode of idiographic inquiry, differing from history and archaeology in that it is based on the direct observation of living people rather than on written records or material remains attesting to the activities of people in the past. Anthropology, to the contrary, is a field of nomothetic science. As Radcliffe-Brown declared in his introduction

to *Structure and Function in Primitive Society* – in a famous sentence that, as an undergraduate beginning my anthropological studies at Cambridge in the late 1960s, I was expected to learn by heart – 'comparative sociology, of which social anthropology is a branch, is ... a theoretical or nomothetic study of which the aim is to provide acceptable generalisations' (Radcliffe-Brown 1952: 3). This distinction between anthropology and ethnography was one that brooked no compromise, and Radcliffe-Brown reasserted it over and over again. Returning to the theme in his Huxley Memorial Lecture for 1951 on 'The comparative method in social anthropology', best known for its revision of the theory of totemism, Radcliffe-Brown insisted that 'without systematic comparative studies anthropology will become only historiography and ethnography' (1951a: 16). And the aim of comparison, he maintained, is to pass from the particular to the general, from the general to the more general, and ultimately to the universal (ibid.: 22).

The distinction between the idiographic and the nomothetic was first coined in 1894 by the German philosopher–historian Wilhelm Windelband, a leading figure in the school of thought, then known as neo-Kantianism. Windelband's real purpose was to lay down a clear dividing line between the craft of the historian, whose concern is with judgements of value, and the project of the natural science, concerned as it is with the accumulation of positive knowledge based on empirical observation. But he did so by identifying history with the documentation of particular events and science with the search for general laws. And this left his distinction wide open for appropriation by positivistic natural science to denote not its opposition to history but the two successive stages of its own programme: first, the systematic collection of empirical facts; and secondly, the organisation of these facts within an overarching framework of general principles. It was left to Heinrich Rickert, a pupil of Windelband and co-founder with him of the neo-Kantian school, to sort out the confusion by pointing out that there are distinct ways, respectively scientific and historical, of attending to the particular, to each of which there corresponds a specific sense of the idiographic (Collingwood 1946: 165–170). One way treats every entity or event as an objective fact, the other attributes to it some meaning or value.[1] In so far as a geologist setting out to reconstruct the history of a rock formation, or a palaeontologist seeking to reconstruct a phylogenetic sequence on the basis of fossil evidence, necessarily deals in particulars, the reconstruction could – in the *first* of these senses – be deemed to be idiographic. Moreover the same might have been said (and indeed was said) of attempts, predominantly by North American scholars and going under the rubric of ethnology, to reconstruct chronological sequences of culture on the evidence of distributions of what were then called 'traits'.

It was in this sense that Radcliffe-Brown could set aside North American ethnology, which he associated primarily with the work of Franz Boas and his followers, as an idiographic enterprise wholly distinct from his nomothetic social anthropology conceived as a search for general laws governing social life (Radcliffe-Brown 1951a: 15). But while Boasian ethnology was thus being portrayed in Britain as historical rather than scientific, on the other side of the Atlantic it was being criticised for being scientific rather than historical. This critique came from Alfred Kroeber. Thoroughly conversant with the writings of the neo-Kantian school, Kroeber called for an anthropology that would be fully historical and therefore idiographic in the *second* sense. It must, in short, attend to particulars in terms of their value and meaning. Yet no particular – no thing, or happening – can have value and meaning in itself, cut out from the wider context of its occurrence. Each has rather to be understood by way of its positioning within the totality to which it belongs. Thus while

preserving the singularity of its phenomena rather than allowing them to be dissolved into laws and generalisations, the historical approach – in Kroeber's words – 'finds its intellectual satisfaction in putting each preserved phenomenon into a relation of ever widening context within the phenomenal cosmos' (Kroeber 1952: 123). He characterised this task, of preservation through contextualisation, as 'an endeavour at descriptive integration' (1935: 545). As such, it is entirely different from the task of theoretical integration that Radcliffe-Brown had assigned to social anthropology. For the latter, in order to generalise, one must first isolate every particular from its context in order that it can then be subsumed under context-independent formulations. Kroeber's disdain for Radcliffe-Brown's understanding of history, as nothing but a chronological tabulation of such isolated particulars awaiting the classificatory and comparative attentions of the theorist, bordered on contempt. 'I do not know the motivation for Radcliffe-Brown's depreciation of the historical approach', he remarked caustically in an article first published in 1946, 'unless that, as the ardent apostle of a genuine new science of society, he has perhaps failed to concern himself enough with history to learn its nature' (in Kroeber 1952: 96).

The sigma principle and the totality of phenomena

Though I am not sure that the terms are the best ones, the distinction between descriptive and theoretical integration is of great importance. For the two modes of integration entail entirely different understandings of the relation between the particular and the general. The theoretician operating in a nomothetic mode imagines a world that is, by its nature, particulate. Thus the reality of the social world, for Radcliffe-Brown, comprises 'an immense multitude of actions and interactions of human beings' (1952: 4). Out of this multitude of particular events the analyst has then to abstract general features that amount to a specification of form. One of the strangest attempts to spell out this procedure appears in a book ominously entitled *The Theory of Social Structure* by the great ethnographer and anthropologist Siegfried Nadel, posthumously published in 1957. Introduced by his friend and colleague Meyer Fortes (in Nadel 1957: xv) as a work 'destined to be one of the great theoretical treatises of twentieth-century social anthropology', it was soon forgotten. Its peculiarity lay in its author's use of notation drawn from symbolic logic in order to formalise the move from the concreteness of actually observed behaviour to the abstract pattern of relationships.

Let us suppose, Nadel postulated, that between persons A and B we observe diverse behaviours denoted by the letters a, b, c … n, but that all index a condition of 'acting towards' – of A acting towards B and of B acting towards A. We denote this condition with the colon (:). It then follows that a formal relationship (r) exists between A and B, under which is subsumed the behavioural series a … n. Or in short:

$$\begin{aligned} &A\ r\ B, \text{ if} \\ &A\ (a, b, c \ldots n) : B, \text{ and } \textit{vice versa} \\ &\therefore r \supset \Sigma\, a \ldots n \end{aligned}$$

(Nadel 1957: 10)

My purpose in recovering this formulation from the rightful oblivion into which it quickly fell is only to highlight the sense of integration epitomised in the last line by the Greek

'sigma', the sign conventionally used in mathematics to denote the summation of a series. The abstract relation, here, takes the form of a covering statement that encompasses every concrete term in the series.

When Kroeber spoke of 'descriptive integration', however, he meant something quite different: more akin, perhaps, to the integration of an artist's picture on the canvas as he paints a landscape. To the artist's gaze, the landscape presents itself not as a multitude of particulars but as a variegated phenomenal field, at once continuous and coherent. Within this field, the singularity of every phenomenon lies in its enfolding – in its positioning and bearing, and in the poise of a momentarily arrested movement – of the entangled histories of relations by which it came to be there, at that position and in that moment. And as the artist tries to preserve that singularity in the work of the brush, so, for Kroeber, does the anthropologist in his endeavours of description. This is what he meant when he insisted that the aim of anthropology – as of history – must be one of 'integrating phenomena as such' (Kroeber 1935: 546). The integration he was after is one of a world that already coheres, where things and events occur or *take place*, rather than a world of disconnected particulars that has to be rendered coherent, or joined up after the fact, in the theoretical imagination. Thus what Kroeber called the 'nexus among phenomena' (loc. cit.) is there to be described, in the relational coherence of the world; it is not something to be extracted from it as one might seek the general features of a form from the range of its concrete and particular instantiations. For precisely that reason, Kroeber thought, it would be wrong to regard the phenomena of the social world as *complex*. Contemplating the landscape, the painter would be unlikely to exclaim 'What a complex landscape this is!' He may be struck by many things, but complexity is not one of them. Nor is it a consideration in the regard of the historically oriented anthropologist. Complexity only arises as an issue in the attempt to reassemble a world already decomposed into elements, as a picture, for example, might be cut up to make a jigsaw puzzle. But like the painter, and unlike the puzzle builder, Kroeber's anthropologist seeks an integration 'in terms of the totality of phenomena' (ibid.: 547) that is ontologically prior to its analytical decomposition.

Yet if the anthropologist describes the social world as the artist paints a landscape, then what becomes of time? The world stands still for no one, least of all for the artist or the anthropologist, and the latter's description, like the former's depiction, can do no more than catch a fleeting moment in a never-ending process. In that moment, however, is compressed the movement of the past that brought it about, and in the tension of that compression lies the force that will propel it into the future. It is this enfolding of a generative past and a future potential in the present moment, and not the location of that moment in any abstract chronology, which makes it historical. Reasoning along these lines, Kroeber came to the conclusion that time, in the chronological sense, is inessential to history. Presented as a kind of 'descriptive cross-section' or as the characterisation of a moment, a historical account can just as well be synchronic as diachronic. Indeed it is precisely to such characterising description that anthropology aspires. 'What else can ethnography be', asked Kroeber rhetorically, 'than … a timeless piece of history?' (1952 [1946]: 102). The other side of this argument, of course, is that the mere ordering of events in chronological succession, one after another, gives us not history but science. Boas, whose painstaking attempts to reconstruct the lines of cultural transmission and diffusion over time had been dismissed by Kroeber as anti-historical, was perplexed. He confessed to finding Kroeber's reasoning utterly unintelligible (Boas 1936: 137). Back in Britain, however, Kroeber's understanding of

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 sociologism, 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'

(Weiss 1969: 7). That is why, to return to my earlier criticism of Leach, a mechanical analogy can offer no account of social *life*. A machine can be constructed from parts, but machines do not live. And this brings me from the meaning of the social to the second of my key terms, namely 'life'. By this I do not mean an internal animating principle that is installed in some things but not others, distinguishing the former as members of the class of animate objects. Life, as Weiss observed, 'is process, not substance' (1969: 8), and this process is tantamount to the unfolding of a continuous and ever-evolving field of relations within which beings of all kinds are generated and held in place. Thus where Radcliffe-Brown drew an analogy between organic life and social life, I draw an identity. Organic life *is* social, and so for that matter is the life of the mind, because the order to which it gives rise is implicate.[4]

In this distinction between explicate and implicate orders lies an echo of the contrast I drew earlier between theoretical and descriptive modes of integration. To recapitulate: the theoretical mode works through the summation of discrete particulars, according to the sigma principle, so as to arrive at covering statements of the general form of social relations. The descriptive mode, on the other hand, seeks to apprehend the relational coherence of the world itself, as it is given to immediate experience, by homing in on particulars each of which brings to a focus, and momentarily condenses, the very processes that brought it into being. Though both modes of integration aspire to a kind of holism, their respective understandings of totality are very different. The first is a totality of *form*: it implies the closure and completion of a system of relations that has been fully joined up. The second, however, is a totality of *process* that, since it is forever ongoing, is always open-ended and never complete, but which is nevertheless wound up in every moment that it brings forth. Now as I mentioned earlier, I am not convinced that the terms 'theoretical' and 'descriptive' are entirely appropriate for these two approaches. The trouble is that the very notion that description is a task somehow opposed to the project of theory has its roots in the first of the two modes. It harks directly back to Radcliffe-Brown's division between ethnography and anthropology: respectively idiographic and nomothetic, descriptive and theoretical. Yet in the opposition between descriptive data and theoretical generalisation the act of description is itself diminished, reduced to a mechanical function of information pick-up. The second mode, on the other hand, refuses this reduction, recognising – as the first does not – that any act of description entails a movement of interpretation. What is 'given' to experience, in this mode, comprises not individual data but the world itself. It is a world that is not so much mapped out as taken in, from a particular vantage point, much as the painter takes in the landscape that surrounds him from the position at which he has planted his easel.

It follows that any endeavour of so-called descriptive integration, if it is to do justice to the implicate order of social life, can be neither descriptive nor theoretical in the specific senses constituted by their opposition. It must rather do away with the opposition itself. What then becomes of my initial distinction between ethnography and anthropology? Have I not argued myself out of the very position from which I began? I have certainly argued against the simple alignments of ethnography with data collection and of anthropology with comparative theory. If there is a distinction between ethnography and anthropology, then it must be drawn along different lines. Let me return for a moment to Radcliffe-Brown. In his 1951 lecture on 'The comparative method in social anthropology', he had a word or two to say about armchairs. It is told that long ago, in the days before fieldwork had become established practice in anthropological research, scholars sat in their libraries, ensconced in comfortable armchairs, as they carried out their comparative work. By the middle of the

twentieth century, however, the 'armchair anthropologist' had become an object of derision, whose airy speculations were brushed aside by a new generation for whom fieldwork was paramount. For Radcliffe-Brown this was a matter of regret. A social anthropology that aspires to systematic comparison, and that is not content to rest on its ethnographic laurels, must, he thought, allow space for the armchair (Radcliffe-Brown 1951a: 15). Now whether our anthropological ancestors actually sat in armchairs as they worked, I do not know. But the reason why this particular piece of furniture has earned its central place in the disciplinary imagination is plain. For it seems to cocoon the scholar in a sedentary confinement that insulates him almost completely from any kind of sensory contact with his surroundings. Being-in-the-armchair, if you will, is the precise inverse of being-in-the-world.[5]

Here is where I differ from Radcliffe-Brown: I do not think we can do anthropology in armchairs. I can best explain why in terms of the difficulty that I, along with many colleagues (Sillitoe 2007: 150), routinely face in introducing what our subject is about, especially to novice students. Perhaps it is the study of human societies – not just of our own society, but of all societies, everywhere. But that only begs further questions. You can see and touch a fellow human being, but have you ever seen or touched a society? We may think we live in societies, but can anyone ever tell where their society ends and another begins? Granted that we are not sure what societies are, or even whether they exist at all, could we not simply say that anthropology is the study of *people*? There is much to be said for this, but it still does not help us to distinguish anthropology from all the other disciplines that claim to study people in one way or another, from history and psychology to the various branches of biology and biomedicine.

What truly distinguishes anthropology, echoing our conclusion from the last chapter, is that it is not a study *of* at all, but a study *with*. Anthropologists work and study *with* people. Immersed with them in an environment of joint activity, they learn to see things (or hear them, or touch them) in the ways their teachers and companions do. An education in anthropology, therefore, does more than furnish us with knowledge *about* the world – about people and their societies. It rather educates our *perception* of the world, and opens our eyes and minds to other possibilities of being. The questions we address are philosophical ones: of what it means to be a human being or a person, of moral conduct and the balance of freedom and constraint in people's relations with others, of trust and responsibility, of the exercise of power, of the connections between language and thought, between words and things, and between what people say and what they do, of perception and representation, of learning and memory, of life and death and the passage of time, and so on and so forth. Indeed the list is endless. But it is the fact that we address these questions in the world, and *not* from the armchair – that this world is not just what we think *about* but what we think *with*, and that in its thinking the mind wanders along pathways extending far beyond the envelope of the skin – that makes the enterprise anthropological and, by the same token, radically different from positivist science. We do our philosophy out of doors. And in this, the world and its inhabitants, human and non-human, are our teachers, mentors and interlocutors.

Anthropology as art and craft

In a recent, somewhat wistful essay, Maurice Bloch (2005) asks rhetorically, 'Where did anthropology go?' Echoing a complaint that has rumbled on ever since the collapse of the nineteenth-century certainties of evolutionary progress, he worries that in the absence of

any 'generalizing theoretical framework', anthropology is left 'without the only centre it could have: the study of human beings' (ibid.: 2, 9). He suggests a return to functionalism, taken in a broad sense as an understanding grounded in the circumstances of real human beings, in specific places, and embedded in the wider ecology of life. I am sympathetic, having myself put forward something similar under the rubric of the 'dwelling perspective' (Ingold 2000a). As Bloch (2005: 16–17) says of his functionalism, this is not a theory so much as an attitude – let us say, a way of knowing rather than a framework for knowledge as such. Fundamentally, as a way of knowing it is also a way of being. The paradox of the armchair is that in order to *know* one can no longer *be* in the world of which one seeks knowledge. But anthropology's solution, to ground knowing in being, in the world rather than the armchair, means that any study *of* human beings must also be a study *with* them. Indeed, Bloch offers a fine example of how this might be done, recalling a discussion of a deeply philosophical nature with his hosts during fieldwork in a small Malagasy village. He describes the discussion as a seminar (ibid.: 4). I am sure we can all recall similar conversations. They shape the way we think.

A moment ago I referred to the work of Hallowell – a profound contribution to the philosophy of the self, consciousness and perception. As we know, however, this philosophy was shaped more than anything by endless conversations with his hosts, the Ojibwa people of north-central Canada. One thing he learned from them is particularly worthy of consideration here. It concerns dreaming. The world of one's dreams, Hallowell's mentors told him, is precisely the same as that of one's waking life. But in the dream you perceive it with different eyes or through different senses, while making different kinds of movements – perhaps those of another animal such as an eagle or a bear – and possibly even in a different medium such as in the air or the water rather than on land. When you wake, having experienced an alternative way of being in that same world in which you presently find yourself, you are wiser than you were before (Hallowell 1955: 178–181). To do anthropology, I venture, is to dream like an Ojibwa. As in a dream, it is continually to *open up* the world, rather than to seek closure. The endeavour is essentially comparative, but what it compares are not bounded objects or entities but ways of being. It is the constant awareness of alternative ways of being, and of the ever-present possibility of 'flipping' from one to another, that defines the anthropological attitude. It lies in what I would call the 'sideways glance'. Wherever we are, and whatever we may be doing, we are always aware that things might be done differently. It is as though there were a stranger at our heels, who turns out to be none other than ourselves. This sensibility to the strange in the close-at-hand is, I believe, one that anthropology shares with art. But by the same token, it is radically distinguished from that of normal science, which defamiliarises the real by removing it altogether from the domain of immediate human experience.

Turning from its underlying sensibilities to its working practices, anthropology is perhaps more akin to craft than art.[6] For it is characteristic of craft that both the practitioner's knowledge *of* things, and what he does *to* them, are grounded in intensive, respectful and intimate relations *with* the tools and materials of his trade. Indeed, anthropologists have long preferred to see themselves as craftsmen among social scientists, priding themselves on the quality of their handiwork by contrast to the mass-produced goods of industrial data processing turned out by sociologists and others. Rarely, however, have they sought to spell out exactly what craftsmanship entails. Rather ironically, introducing an edited volume entitled *The Craft of Social Anthropology* published in 1967, Max Gluckman explained that its

purpose is to provide a guide to modern fieldwork methods. The contributing authors, who broadly represented the so-called 'Manchester School' of social anthropology, had all tried, wrote Gluckman, 'to set techniques in the framework of theoretical problems, so that those who use the book may remind themselves of what they are aiming at when they collect their material' (Gluckman 1967: xi). The irony is that the language of data collection, hypothesis testing and theory building used throughout the book could hardly be further removed from the practice of craft, and in fact the term, so prominently displayed in the book's title, is never mentioned again. That anthropology is a craft seems to have been something that its contributors simply took for granted. A decade previously, however, C. Wright Mills had concluded his book *The Sociological Imagination* (1959) with an appendix that tackles the issue head-on. Apart from its presumption that all social scientists are men, Mills's essay 'On intellectual craftsmanship' remains as relevant today as it was fifty years ago. Though addressed to social scientists in general rather than anthropologists in particular, it contains more words of wisdom than any number of theoretical treatises and methodological manuals.

This is how Mills begins:

> To the individual social scientist who feels himself a part of the classic tradition, social science is the practice of a craft. A man at work on problems of substance, he is among those who are quickly made impatient and weary by elaborate discussions of method-and-theory-in-general; so much of it interrupts his proper studies.
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> (1959: 215)

Thus the first thing about intellectual craft, for Mills, is that there is no division between method and theory. Against the idea that you start by setting a theoretical agenda, and then test it empirically by means of data collected in accordance with standard protocols, Mills declares: 'Let every man be his own methodologist; let every man be his own theorist; let theory and method again become part of the practice of craft' (ibid.: 246). The second thing about intellectual craft, then, is that there is no division, in practice, between work and life. It is a practice that involves the whole person, continually drawing on past experience as it is projected into the future. The intellectual craftsman, as Mills puts it, 'forms his own self as he works towards the perfection of his craft' (ibid.: 216). What he fashions, through his work, is a way of being. And thirdly, to assist him in this project, he keeps a journal, which he periodically files, sorts and scrambles for new ideas. In it, he notes his experiences, his 'fringe-thoughts' that have come to him as by-products of everyday life, snatches of overheard conversations, and even dreams (ibid.: 216–217). It is from this heterogeneous reservoir of raw material that the intellectual craftsman shapes his work.

Mills's portrayal of craftsmanship certainly seems to fit, so far as anthropology is concerned. I am confident that most anthropologists would be happy to sign up to it, even if it goes against the grain of much of what has been published on the subject of theory and method. But what has become of ethnography? If theory and method are to come together again in craft, as Mills recommends, then should not every anthropologist be his or her own ethnographer, and vice versa? We can still recognise today the figure of the 'social theorist', sunk in his armchair or more likely peering from behind his computer screen, who presumes to be qualified, by virtue of his standing as an intellectual, to pronounce upon the ways of a world with which he involves himself as little as possible, preferring to interrogate the works of others of his kind. At the other extreme is the lowly 'ethnographic

researcher', tasked with undertaking structured and semi-structured interviews with a selected sample of informants and analysing their contents with an appropriate software package, who is convinced that the data he collects are ethnographic simply because they are qualitative. These figures are the fossils of an outmoded distinction between empirical data collection and abstract theoretical speculation, and I hope we can all agree that there is no room for either in anthropology. But what of the detailed descriptions of other people's lives, informed by prolonged fieldwork, that are characteristic of ethnography at its best? Should we not leave some space for them? Indeed we should. But something happens when we turn from the being *with* of anthropology to the ethnographic description *of*. And to explain what this is I must return to the notion of description itself.

Writing and correspondence

Earlier I likened the anthropological mode of descriptive integration to the integration of a landscape painting as it takes shape upon the artist's canvas. In painting, as also in drawing, observation and description go hand in hand. This is because both painting and drawing entail a direct coupling between the movement of the artist's visual perception, as it follows the shapes and contours of the land, and the gestural movement of the hand that holds the brush or pencil, as it leaves a trace upon a surface. Through the coupling of perception and action, the artist is drawn *in* to the world, even as he or she draws it *out* in the gestures of description and the traces they yield.[7] As I have already mentioned, there is much in common between the practices of anthropology and art. Both are ways of knowing that proceed along the observational paths of being *with*, and both, in doing so, explore the unfamiliar in the close at hand. But by and large, ethnographers neither paint nor draw. As noted in the last chapter, the entire debate that has accompanied the so-called 'crisis of representation' has been founded on the premise that the graphic part of ethnography is not drawing but writing. Moreover it is writing understood not as a practice of inscription or line-making but as one of verbal composition, which could be done just as well on a keyboard as with a pencil or pen. It is for this reason that James Clifford, for example, can assert that description involves 'a turning *away* from dialogue and observation towards a separate place of writing, a place for reflection, analysis and interpretation' (Clifford 1990: 52).

There is nothing intrinsically wrong with this, but the separation deserves to be noted. Conventionally we associate ethnography with fieldwork and participant observation, and anthropology with the comparative analysis that follows after we have left the field behind. I want to suggest, to the contrary, that anthropology – as an inquisitive mode of inhabiting the world, of being *with*, characterised by the 'sideways glance' of the comparative attitude – is itself a practice of observation grounded in participatory dialogue. It could perhaps be characterised as a *correspondence*. In this sense, the anthropologist's observations answer to his experience of habitation. The correspondence may be mediated by such descriptive activities as painting and drawing, which can be coupled to observation. It can also, of course, be mediated by writing. But unlike painting and drawing, anthropological writing is *not* an art of description. We do not call it 'anthropography', and for good reason. The anthropologist writes – as indeed he thinks and speaks – *to* himself, to others and to the world. This verbal correspondence lies at the heart of the anthropological dialogue. It can be carried out anywhere, regardless of whether we might imagine ourselves to the 'in the field' or out of it. Anthropologists, as I have insisted, do their thinking, talking and writing

in and with the world. To do anthropology, you do not have to imagine the world as a field. 'The field' is rather a term by which the ethnographer retrospectively imagines a world from which he has *turned away* in order, quite specifically, that he might *describe it in writing*. His literary practice is not so much one of *non-descriptive correspondence* as one of *non-correspondent description* – that is, a description which (unlike painting or drawing) has broken away from observation. Thus if anyone retreats to the armchair, it is not the anthropologist but the ethnographer. As he shifts from inquiry to description he has of necessity to reposition himself from the field of action to the sidelines.

It has long been customary to divide the process of anthropological research into three successive phases: of observation, description and comparison. In practice, as Philippe Descola has pointed out, this three-phase model offers 'a purified definition of operations that are most often intertwined' (Descola 2005: 72). One cannot say where one ends and the next begins. An overall movement is nevertheless assumed from ethnographic particulars to anthropological generalities. It might seem from the foregoing that I have reversed this order, placing anthropology before ethnography rather than after it. But that is not really my intention. I do not believe that anthropology is any more *prior* to ethnography than the other way round. They are just different. It may be hard to carry on both at once, because of the different positionalities they entail, but most of us probably swing back and forth between them, like a pendulum, in the course of our working lives. My real purpose in challenging the idea of a one-way progression from ethnography to anthropology has not been to belittle ethnography, or to treat it as an afterthought, but rather to liberate it, above all from the tyranny of method. Nothing has been more damaging to ethnography than its representation under the guise of the 'ethnographic method'. Of course, ethnography has its methods, but it *is not* a method. It is not, in other words, a set of formal procedural means designed to satisfy the ends of anthropological inquiry. It is a practice in its own right – a practice of verbal description. The accounts it yields, of other people's lives, are finished pieces of work, not raw materials for further anthropological analysis. But if ethnography is not a means to the end of anthropology, then neither is anthropology the servant of ethnography. To repeat, anthropology is an inquiry into the conditions and possibilities of human life in the world; it is not – as so many scholars in fields of literary criticism would have it – the study of how to write ethnography, or of the reflexive problematics of the shift from observation to description.

This is a message that has critical implications for the way anthropology is taught. Too often, it seems to me, we disappoint our students' expectations. Rather than awakening their curiosity towards social life, or kindling in them an inquisitive mode of being, we force them into an endless reflection on disciplinary texts that are studied not for the light they throw upon the world but for what they reveal about the practices of anthropologists themselves and the doubts and dilemmas that surround their work. Students soon discover that having doubled up on itself, through its conflation with ethnography, anthropology has become an interrogation of its own ways of working.[8] As educators based in university departments, most anthropologists devote much of their lives to working with students. They probably spend considerably more time in the classroom than anywhere they might call the field. Some enjoy this more than others, but they do not, by and large, regard time in the classroom as an integral part of their *anthropological* practice. Students are told that anthropology is what we do with our colleagues, and with other people in other places, but not with them. Locked out of the powerhouse of anthropological knowledge construction,

all they can do is peer through the windows that our texts and teachings offer them. It took the best part of a century, of course, for the people once known as 'natives', and latterly as 'informants', to be admitted to the big anthropology house as master collaborators, that is as people we work *with*. It is now usual for their contributions to any anthropological study to be fulsomely acknowledged. Yet students remain excluded, and the inspiration and ideas that flow from our dialogue with them unrecognised. I believe this is a scandal, one of the malign consequences of the institutionalised division between research and teaching that has so blighted the practice of scholarship. For indeed, the epistemology that constructs the student as the mere recipient of anthropological knowledge produced elsewhere – rather than as a participant in its ongoing creative crafting – is the very same as that which constructs the native as an informant. And it is no more defensible.

Anthropology is *not* ethnography. Ethnographers describe, principally in writing, how the people of some place and time perceive the world and how they act in it. In our dreams we might once have supposed that by adding up, comparing and contrasting the ways that people of all places and times perceive and act, we might be able to extract some common denominators – possible candidates for human universals. Any such universals, however, are abstractions of our own, and as Whitehead was the first to point out, it is a fallacy to imagine that they are concretely instantiated in the world as a substrate for human variation.[9] With its dreams of generalisation shattered, where should anthropology go? Should it continue to accumulate disparate but thematically oriented ethnographic case studies between the covers of edited volumes, in the hope that some kinds of generalisation might still fall out? Should it abandon its project for the work of philosophers who have never mustered the energy or the conviction to leave their armchairs? Should it, on the other hand, join with the literary critics in their own arcane ruminations on the ethnographic project? Anthropology has tried all these things. Yet every direction leads off at a tangent from the world we inhabit. It is no wonder, then, that anthropologists are left feeling isolated and marginalized, and that they are routinely passed by in public discussions of the great questions of social life. I have argued for a discipline that would return to these questions, not in the armchair but in the world. We can be our own philosophers, but we can do it better thanks to its embedding in our observational engagements with the world and in our collaborations and correspondences with its inhabitants. What, then, should we call this lively philosophy of ours? Why, *anthropology*, of course!