RETROSPECT*

by

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Abstract: In this paper, the author reviews his intellectual and professional career, tracing the principal influences on his academic and intellectual development. He also outlines the main themes of his research: on human-animal relations, the comparative anthropology of hunter-gatherer and pastoral societies, and relations between biological evolution and human history. In his most recent work he has attempted to build a synthesis between phenomenological, ecological and developmental approaches, in philosophy, psychology and biology respectively, linking them to an anthropological theory of skilled practice.

Key-words: Intellectual autobiography; anthropology in the UK; biology and culture.

I was born in 1948, the youngest of four children. I enjoyed an overwhelmingly happy childhood which imbued me with a love of the Kent countryside where I grew up, and a passion for steam trains. With the constant assistance of my mother, who dealt with routine derailments, I became a keen railway modeller. However I saw rather little of my father. As professor of botany at Birkbeck College, London, he often had to stay late to teach his classes, and was rarely home before my bed-time. Nevertheless, I doubt whether any other single person has exercised so great an influence on my life and character.

My father was enthralled by the beauty of nature. But his way of celebrating that beauty was to study it. His was a homely science: the sort you could do by going for walks in the countryside armed with a collecting tin, by peering down a microscope at what you had found, and by tracing out what you saw with pen and ink, using a contraption made out of an old lamp and a sheet of glass mounted on

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copies of the Encyclopaedia Britannica. As a child, I spent hours with the Encyclopaedia. I also loved to thumb through the pages of my father's copy of D'Arcy Wentworth Thompson's monumental book, *On Growth and Form.* Added to that was my growing collection of mathematical and scientific books. By the age of eleven I was experimenting with the mathematics of soap bubbles and writing a paper on the cycloidal patterns traced on a surface by the point of a spinning top. But then I was sent off to boarding school.

For the first three years of boarding school life I was homesick and miserable. My consolation lay in music: I had an inspiring piano teacher and began learning the cello. By the age of fourteen I was taking my 'ordinary level' exams. I did well in maths and science subjects – except biology, which I loathed. The two sixth-form years were much happier. I had wonderful teachers in all of my school subjects, particularly in physics. We were made to feel directly involved, on the cutting edge of the search to understand the mysteries of matter, energy and the universe. I became interested in geology – especially volcanoes, and after taking my advanced-level exams went camping to Iceland with a couple of school- friends. I left school only a month after celebrating my seventeenth birthday.

At a loose end as to what to do, I worked as a warehouseman in a local supermarket and saved up to travel abroad. My dream was to go north, to Finland and Norway. In May 1966 I sailed to Helsinki, whence I travelled to Lapland. The ice was breaking up after a winter of exceptional severity, and many roads were impassable. I was determined, however, to reach the settlement of Sevettijärvi, 100 kilometres off the main road, where – according to my guide-book – there lived a still primitive tribe of Lapps, known as Skolts. When I got there I had no idea what to say or do: acutely embarrassed, I ran for it, spent the night in a derelict cabin, and returned the next morning whence I had come. This ill-fated trip had, however, ignited my curiosity, with far-reaching consequences. After that, I travelled widely in Lapland, fetching up at a farm on the north Norwegian coast where I worked for a couple of months before returning home in time to start my first term at Cambridge.

I had never given a second thought to what subjects I would study at Cambridge. It was assumed that I would take the Tripos in Natural Science. After the excitement of school science, however, lectures at Cambridge were an intense disappointment. For the first time I began to wonder why I was studying science, and what I would do with it. I found much of what I was expected to do intellectually claustrophobic, dedicated to the regimented and narrow-minded pursuit of lines of inquiry that seemed remote from experience. I don't think I ever became radically hostile to science, as did many of my contemporaries, but I could see no future in it for myself. I wanted to study something in which there was more room to grow, where I could discover the world and myself at the same time.

Looking through the list of subjects then on offer at Cambridge, one possibility leapt to my attention. It was social anthropology. My tutor thought it just the subject for misfits like me. It appealed to me (rather as D'Arcy Thompson's biology had done before) as a kind of pure mathematics of real life. My father arranged a meeting with the anthropologist Jean LaFontaine, then a lecturer at Birkbeck College, and she recommended that I read Fredrik Barth's classic, *Political Leadership among Swat Pathans.* I was entranced by the book, and was hooked. Having completed my first year of natural science in autumn 1967, I commenced all over again as a first-year student in the Faculty of Archaeology and Anthropology. This entailed taking courses in physical anthropology and archaeology as well as social anthropology, so that I had my share of measuring fossil skulls and sorting stone tools.

I remain a believer in the integration of the three fields of anthropology – social, physical (or biological) and archaeological – and this initial training at Cambridge may have had something to do with it. Yet my teachers in these three fields had virtually nothing to do with one another. So far as the social anthropologists were concerned, the only thing that held the three fields together was an obsolete theory of progressive evolution. Unbeknown to me, I had stumbled into anthropology at the time when structural-functionalism – the ruling paradigm for the previous two decades – was about to collapse. But it had not collapsed yet. Every good anthropologist, we were told, should carry a copy of A. R. Radcliffe-Brown's *Structure and Function in Primitive Society* in their breast pocket (my own copy of the book came from my sister, who had been presented with it as a school prize). American cultural anthropology was virtually taboo, and anything with a whiff of evolutionism was banished from the curriculum. Once, out of sheer curiosity, I picked up a copy of that strange little book by Marshall Sahlins and Elman Service, *Evolution and Culture.* I was quite excited by it. But on mentioning it to my tutor, I received a firm rebuke and was instructed never to touch such stuff again! This experience only strengthened my resolve to prove my teachers wrong, and to find a way of thinking about evolution that would enable us to reintegrate the biophysical and sociocultural dimensions of human existence through an emphasis on processes in the very long term. Though I cannot claim to have an entirely satisfactory answer yet, I have been working at it ever since.

The intellectual landscape of Cambridge anthropology in the late sixties was notably fractured. On the one side was the towering figure of Edmund Leach who at that time was aggressively championing his own idiosyncratic version of Lévi-Straussian structuralism. On the other side, Meyer Fortes was struggling to understand why people love one another even when they say they don't, and why they hate one another despite their public displays of amity. In between them stepped the figure of Jack Goody, beard, tie and gown aligned at every possible angle to the vertical, largely incoherent in lectures, but unleashing such a torrent of ideas as to leave one

breathless. By 1970, the year in which I graduated, structural-functionalism was cracking up, but no-one knew what the alternative might be. For a very brief period, it seemed that the answer might lie in what was known as social network theory. There were two varieties of this. The first emanated from the 'Manchester School' of Max Gluckman, the second from the 'transactionalism' that Fredrik Barth and his followers were propagating from their base at the University of Bergen, in Norway. I became an enthusiast for the Barthian approach: it had, after all, been Barth's work that brought me into anthropology in the first place. I was impressed by its crystalline lucidity, and its economy of expression.

I never doubted that I would proceed to postgraduate research, and the time came when I had to decide where I would be based, and where my fieldwork would be. Usually, Cambridge-trained fieldworkers were expected to go south, to tropical Africa or Asia. To the astonishment of my mentors, I wanted to go north. Fortunately, a suitable supervisor had appeared at Cambridge in the form of John Barnes, recently appointed to the University's first ever Chair of Sociology. Barnes was riding the crest of a wave of interest in network theory, as one of its leading proponents. With a background in the Manchester School, he had developed his ideas about networks through the analysis of material from subsequent fieldwork in rural Norway and was well known in the Barthian camp. It was arranged that I should spend some time in Barth's department in Bergen, both prior to my departure to the field and immediately following my return.

For my fieldwork I returned to Sevettijärvi and to the Skolt Saami community that I had first visited five years previously. I had been there once again, in summer 1969, with an international voluntary work-camp, where our task had been to build concrete potato cellars. Drawing on connections from that time, and equipped with a bicycle, notebook and camera, I was soon deeply absorbed in the intricacies of reindeer herding. Indeed the sixteen months I spent with the Skolt Saami probably shaped my outlook far more than I ever realised. Though it is hard to trace the links directly, I doubt whether I would be so interested in issues of skill, environmental perception and human-animal relations, or whether I would be addressing these issues in the ways I have done, had it not been for this formative field experience. But of course I did not know this at the time.

Returning from the field in 1972, it took some time to catch up with what had been going on. Anthropology had gone through tumultuous times. Barth had abandoned Bergen for the United States, transactionalism looked like a doomed cult confined to the followers he had left behind, network theory had crashed – taking the remnants of structural-functionalism with it, people were feeling disoriented. So what had happened? Among other things, inspired by political developments in Europe, social anthropologists had rediscovered Karl Marx.

At first this made no impression on me. I was deep into explaining fluctuations in reindeer numbers, unraveling patterns of kinship, and understanding the machinations of what was then the new politics of the 'Fourth World'. I completed my doctoral dissertation in 1975, and a monograph based on it was published the next year. Beyond a critique of the notion of 'minority culture', which got me into a certain amount of trouble with Saami politicians who were trading in this notion at the expense – I thought – of the local communities they claimed to represent, neither the thesis nor the book had any grand theoretical ambitions. But in the meantime my life had moved on. After a brief spell in Cambridge, followed by a year at the University of Helsinki in 1973-4, I had been offered my first proper job as Lecturer in Social Anthropology at the University of Manchester. Arriving in September 1974, I was to remain there for the next 25 years.

At Manchester, I was required to teach a course called 'Environment and Technology'. The idea had got around that I was strong on ecological anthropology. In reality I knew virtually nothing about it, and had to learn it from scratch. It was a question of saving face by keeping one step ahead of the students. The evolution of this course, which I taught more or less continuously until 1991, is almost indistinguishable from the evolution of my own thinking over this period. From the start, we were reading the work of scholars once thought unmentionable in British social anthropological circles: Julian Steward, Leslie White, Marvin Harris. I was even able to get my own back on my undergraduate teachers by including the book they had once banned – Sahlins and Service's *Evolution and Culture* – on the reading list! But we were also reading the new wave of studies coming out of, or inspired by, the neo-Marxist movement: work by Maurice Godelier, Claude Meillassoux, Emmanuel Terray, and of course Marshall Sahlins. And we were investigating the parallels and contrasts between ideas of evolution and transformation to be found in Marxism, Darwinism and classical cultural ecology. It was all very exciting. But to my departmental colleagues 'Environment and Technology' was always considered way out, on the edge of the known continent of anthropology. Any invocation of concepts from biology or evolutionary theory was treated with deep suspicion.

Aside from all the theory, during the early years students sat through a lot of lectures about reindeer. I had found in Lapland that while living animals belonged to people, as expected in a pastoral society, the animals themselves were virtually wild, and were mustered by means of techniques resembling those of pre-pastoral reindeer hunting. This forced me to recognise that neither hunting nor pastoralism could be understood in purely technical or ecological terms, but only as historically specific conjunctions of technoecological and social relations of production. Putting this in a Marxian framework, I developed a model to account for the transitions from hunting to pastoralism, and from pastoralism to the ranch-like system of herd

management that I had observed in Lapland. In 1979-80 I carried out more fieldwork in Lapland, this time among Finnish people with a background in farming and forestry. Like the Skolt Saami, many of these people had been resettled as a result of the redrawing of the Russo-Finnish frontier in the aftermath of the Second World War, and I wanted to look comparatively at the long-term consequences of this post-war resettlement. My field material, however, has still to be properly written up, for as soon as I returned to Manchester my thoughts turned back to theory.

My book on reindeer economies and their transformations had appeared in 1980. Reactions among social anthropological colleagues were indifferent; curiously, it was among prehistoric archaeologists that the book had its greatest impact. It spoke to their concerns with long-term socioeconomic change, especially in Palaeolithic and Mesolithic Europe. This marked the beginnings of a dialogue with prehistorians that has continued to this day, and that has profoundly shaped my conception of the affinity between archaeology and anthropology. Our methods may differ, but in our concerns with time, landscape and the persistence and transformations of human ways of life in the long term, we are at one. But the reindeer book also set me off in three other directions.

The first was in the study of human-animal relations. I had been dissatisfied with the anthropological tendency to treat animals merely as the symbolic objects of an exclusively human discourse. It was clear to me that animals were sentient beings with whom we humans relate socially, just as we do with one another. We needed an anthropology that did not confine social relations to human relations. At the same time we needed to re-examine the grounds on which human beings have been conventionally distinguished from other animals. This led me to revisit the literatures on non-human primates and human evolution. I had to engage with the writings not just of prehistoric archaeologists but of biological anthropologists as well. But I also had to read literature in psychology, specifically in what was then the emergent field of animal psychology. The idea that non-human animals might have minds of their own, once anathema in psychology, had suddenly become fashionable, with the result that old questions concerning what was truly distinctive about human cognition had resurfaced in a new guise. One of the classic criteria of human distinctiveness was toolmaking, another was language. I wanted to know more about the connections, in human evolution, between language, toolmaking and cognition.

The second direction was towards a comparative anthropology of hunter-gatherer and pastoral societies. For some time I visited the anthropological 'camps' of both hunter-gatherer and pastoral studies, but progressively veered to the former. Students of hunting and gathering, it seemed, were still asking the fundamental questions about qualities of sociality, relations with animals and the land, the significance of place and movement, the origins of property and inequality, and so on, to which I

wanted answers. In 1986 I brought out a book of essays dealing with these questions, among others, entitled *The Appropriation of Nature.* All of them set out from the premise that every human being is simultaneously a biological organism, caught in a web of ecological relations, and a social person, constituted within a nexus of social relations. The challenge, I thought, was to understand the dialectical interplay, over time, between these two kinds of systems, social and ecological.

Finally, I felt that there were issues about the meaning of evolution that needed to be resolved. In particular, I wanted to clarify the relation between biological evolution and human history. Is history a process that is 'added on' to an evolved biological baseline? Or is it simply a continuation of the evolutionary process into the domain of human affairs? To tackle these questions meant looking at the way the idea of evolution had been handled in the disciplines of biology, anthropology and history from the late nineteenth century to the present. This turned out to be a major project. The book I published in 1986, *Evolution and Social Life,* representing the fruits of my work to that point, was already twice as long as it should have been. And far from wrapping things up, it had only opened up greater uncertainties about how social and biological understandings could be brought together. Something, I felt, was wrong about the dualism between person and organism, and correlatively between social and ecological relations, around which my previous thinking had been organised. What was needed, I realised, was a different biology.

In 1991-92, I enjoyed a couple of years of leave. Much of this time was taken up with editorial tasks: I was editing the journal *Man* as well as a massive *Companion Encyclopedia of Anthropology* (published by Routledge in 1994). But this period of leave also gave me a moment to take stock. The ship of anthropology appeared to have capsized. Exponents of the 'literary turn' were drowning in their own, increasingly incomprehensible texts, while a few dogged survivors still hung to overturned lifeboats of scientific objectivity. Their protestations, however, left me cold. I felt that I was embarked on another voyage altogether. For the influences that had reshaped my thinking did not come from within anthropology. They came from biology, psychology and philosophy. In biology, I had been impressed by the work of the few scholars – mostly developmental biologists – who were seeking to go beyond the straitjacket of neo-Darwinian thinking. In psychology, nothing has influenced me more than my encounter with the 'ecological psychology' of James Gibson and his followers. In philosophy, I have drawn endless inspiration from dipping into the phenomenological work of Maurice Merleau-Ponty. I was struck by the parallels between Merleau-Ponty's critique of Cartesian science, Gibson's critique of cognitivism and the critique from developmental biology of neo-Darwinism. Putting these critiques together, I thought, offered the prospect of a powerful synthesis. Establishing this synthesis became my agenda for the 1990s.

I emerged from my cocoon of leave in 1993 to head the Manchester department, replacing Marilyn Strathern – who left for Cambridge in the same year. Balancing the administrative burdens this entailed, I had the satisfaction of developing two new advanced courses – 'Culture, Perception and Cognition' and 'Anthropology of Art and Technology' – that gave me the space to develop and try out my new ideas. These were exciting times, but opportunities to write were very limited. Relief came through a two-year award from the British Academy (1997-99), enabling me at last to assemble the fragments of my thinking into one large volume. Entitled *The Perception of the Environment,* it was published in 2000.

In 1999 I left Manchester to take up a newly created chair of anthropology at the University of Aberdeen. My task, here in Aberdeen, is to establish – more or less from scratch – a programme of teaching and research in the anthropology of the North. I feel I have come full circle, not just to my roots in northern circumpolar ethnography, but also to the kind of homely, experience-near science that I absorbed through my childhood. Here I am, back to a biology that owes as much to D'Arcy Thompson as to Darwin, a psychology that is as much 'on the ground' as 'in the head', and an anthropology that knows no absolute division between the person and the organism, or between social and ecological relations. And my ambitions remain as they always were: to establish a broad view of anthropology that overcomes the narrow specialism into subfields; to campaign against reductionist and intolerant approaches to culture and society, and to find a way to re-embed our experience as whole human beings within the continuum of organic life.