A History of Anthropology

Second Edition

Thomas Hylland Eriksen

A History of Anthropology

Anthropology, Culture and Society

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A HISTORY OF ANTHROPOLOGY

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Series Preface

Anthropology is a discipline based upon in-depth ethnographic works that deal with wider theoretical issues in the context of particular, local conditions – to paraphrase an important volume from the series: *large issues* explored in *small places*. This series has a particular mission: to publish work that moves away from an old-style descriptive ethnography that is strongly area-studies oriented, and offer genuine theoretical arguments that are of interest to a much wider readership, but which are nevertheless located and grounded in solid ethnographic research. If anthropology is to argue itself a place in the contemporary intellectual world, then it must surely be through such research.

We start from the question: 'What can this ethnographic material tell us about the bigger theoretical issues that concern the social sciences?' rather than 'What can these theoretical ideas tell us about the ethnographic context?' Put this way round, such work becomes *about* large issues, *set in* a (relatively) small place, rather than detailed description of a small place for its own sake. As Clifford Geertz once said, 'Anthropologists don't study villages; they study *in* villages.'

By place, we mean not only geographical locale, but also other types of 'place' – within political, economic, religious or other social systems. We therefore publish work based on ethnography within political and religious movements, occupational or class groups, among youth, development agencies, and nationalist movements; but also work that is more thematically based – on kinship, landscape, the state, violence, corruption, the self. The series publishes four kinds of volume: ethnographic monographs; comparative texts; edited collections; and shorter, polemical essays.

We publish work from all traditions of anthropology, and all parts of the world, which combines theoretical debate with empirical evidence to demonstrate anthropology's unique position in contemporary scholarship and the contemporary world.

Professor Vered Amit
Dr Jon P. Mitchell

Preface to the Second Edition

It would not be correct to claim that anthropology has changed dramatically in the twelve years that have passed since the publication of the first edition of this book. However, there are several reasons why we felt that a thorough revision and update was in order by now.

First, the non-metropolitan anthropologies – from Brazil to Russia, from Japan to India – were treated cursorily and somewhat superficially in the first edition. This has now, at least to the best of our abilities, been rectified. Recent scholarship on 'other people's anthropologies' has been of great help here.

Second, there were a number of small errors, inaccuracies and ambiguities scattered around in the first edition. We cannot guarantee that they are all gone, but again, we have done our best.

Third, there have in fact been some slight changes or adjustments in the course that anthropology has been taking in the last few years. For example, the field of globalisation studies, an incipient and slightly naughty trend in the 1990s – naughty because it eschewed an assumed orthodoxy seeing anthropology as the study of small, fairly isolated societies – has grown into maturity. Few anthropologists today see problems in studying the relationship between the global and the local, and most do, in one way or another.

Fourth, as a matter of fact, the early chapters have been revised and modified just as much as the latter ones. Indeed it may be said that the remote past has changed just as rapidly as the recent past. Partly we see the past in a different light because of recent changes; partly new research and a deepened understanding reveals new details and formerly ignored patterns.

It is not true, in other words, that the past is safe and immune to change since what has happened has happened. As anthropologists interested in history have shown time and again, the past is something malleable and dynamic. Each generation has its own past. This is our anthropological past.

Thomas Hylland Eriksen
Finn Sivert Nielsen
Oslo, November 2012

Preface to the First Edition

This is an ambitious book, but not a pretentious one. It is ambitious in that it tries, within the space of relatively few pages, to make sense of the diverse history of anthropology. Our priorities, omissions and interpretations are bound to be contested, since there can be no single authoritative history of anything, least of all a sprawling, dynamic and disputed field like anthropology. Still, the book is unpretentious, since our aim throughout has been to offer a sober and balanced account of the historical growth of anthropology as a discipline, not to propose a radical re-interpretation of it.

There exists a growing scholarly literature on the history of anthropology, which this textbook does not try to compete with. Nevertheless, we know of no existing book with exactly the same scope as this one. The scholarly literature is often specialised, and existing textbooks on anthropological history are either more theoretically oriented or more committed to one or a few professional traditions. Although we may not always have succeeded, we have strived to give an impression of the parallel, convergent and interdependent developments of all major traditions in social and cultural anthropology.

The book is chronologically ordered. Beginning with the 'proto-anthropologies' from ancient Greece to the Enlightenment, it continues with the creation of academic anthropology and the growth of classical sociology during the nineteenth century. The third chapter concentrates on the four men who, by general consensus, are considered the founding fathers of twentieth-century anthropology, and the fourth chapter indicates how their work was continued, and diversified, by their students. The fifth and sixth chapters both deal with the same period – from about 1946 to about 1968, but concentrate on different trends: Chapter 5 discusses the theoretical controversies surrounding concepts of society and social integration, while Chapter 6 covers concepts of culture and symbolic meaning. In Chapter 7, the intellectual and political upheavals of the 1960s and 1970s are presented, with emphasis on the impulses emanating from Marxism and feminism. Chapter 8 deals with the 1980s, concentrating on the postmodernist movement and its close cousin, postcolonialism, two critical trends, which seriously

challenged the discipline's self-confidence; while the ninth and final chapter presents a few of the major post-postmodern trends that emerged during the 1990s.

We do not consider the history of anthropology to be a linear tale of progress. Some 'modern' controversies, for example, have occupied scholars since the Enlightenment and even earlier. At the same time, we believe that there has been a steady, cumulative growth in knowledge and understanding within the subject, not least with regard to method. Moreover, as anthropology responds to changes in the outside world, its substantial focus changes accordingly. Thus, the movement from the early industrial and colonial age to the information age of global modernities has led the subject through a series of transformations, yet essentially it continues to raise the same questions that were asked 50, 100 or even 200 years ago.

Oslo/Copenhagen, July 2001
THE & FSN

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Proto-Anthropology

How long have anthropologists existed? Opinions are divided on this issue. The answer depends on what you mean by an anthropologist. People around the world have always been curious about their neighbours and more remote people. They have gossiped about them, fought them, married them and told stories about them. Some of their stories were written down. Some were later criticised as inaccurate or ethnocentric (or flatly racist). Some stories were compared with others, about other people, leading to general assumptions about 'people elsewhere', and what humans everywhere have in common. In this broad sense, we start an anthropological enquiry the moment a foreigner moves into the neighbouring flat.

If we restrict ourselves to anthropology as a scientific discipline, some would trace its roots back to the European Enlightenment during the eighteenth century; others would claim that anthropology did not arise as a science until the 1850s, others again would argue that anthropological research in its present-day sense only commenced after the First World War. Nor can we avoid such ambiguities.

It is beyond doubt, however, that anthropology, considered as the science of humanity, originated in the region we commonly refer to as 'the West', notably in four 'Western' countries: France, Britain, the USA and Germany. Historically speaking, this is a European discipline, and its practitioners, like those of all European sciences, occasionally like to trace its roots back to the ancient Greeks.

HERODOTUS AND OTHER GREEKS

Thanks to research carried out by anthropologists, historians and archaeologists, we today believe that 'the ancient Greeks' differed quite radically from ourselves. In the classical city-states, more than half the population were slaves; free citizens regarded manual labour as degrading, and democracy (which was also 'invented' by the Greeks) was probably more similar to the competitive *potlatch* feasts of the Kwakiutl (Chapter 4), than to the institutions described in modern constitutions (Finley 1973; P. Anderson 1974).

Going back to the Greeks is thus a long journey, and we peer into their world through cracked and smoky glass. We catch glimpses of little city-states surrounded by traditional Iron Age farmland where family and kinship formed the main social units, connected to the outside world through a network of maritime trade relationships between urban settlements along the Mediterranean and Black Sea coasts. The trade in luxury goods and the free labour entailed by slavery brought considerable wealth to the cities, and the citizens of the *polis*, with their distaste for manual work, had at their disposal a large surplus, which they used, among other things, to wage war, and to build temples, stadiums, baths and other public buildings, where male citizens could meet and engage in philosophical disputes and speculations about how the world was put together.

It was in such a community that Herodotus of Halicarnassus (*c.* 484–425 BCE) lived. Born in a Greek colonial town on the south-west coast of present-day Turkey, Herodotus began to travel as a young man and gained personal knowledge of the many foreign peoples that the Greeks maintained contacts with. Today, Herodotus is mainly remembered for his history of the Persian Wars (Herodotus 1982), but he also wrote detailed travel narratives from various parts of western Asia and Egypt, and (based on second-hand information) from as far away as the land of the Scythians on the northern coast of the Black Sea, the Ethiopians, and the peoples of the Indus valley. In these narratives, far removed as they are from our present world, we recognise a problem that has pursued anthropology, in various guises, up to this day: how should we relate to 'the Others'? Are they basically like ourselves, or basically different? Most, if not all, anthropological theory has tried to strike a balance between these positions, and this is what Herodotus did too. Sometimes he is a prejudiced and ethnocentric 'civilised man', who disdains everything foreign. At other times he acknowledges that different peoples have different values because they live under different circumstances, not because they are morally deficient. Herodotus' descriptions of language, dress, political and judicial institutions, crafts and economics are highly readable today. Although he sometimes clearly got the facts wrong, he was a meticulous scholar, whose books are often the only written sources we have about peoples of a distant past.

Many Greeks tested their wits against a philosophical paradox that touches directly on the problem of how we should relate to 'the Others'. This is the paradox of *universalism* versus *relativism*. A present-day universalist would try to identify commonalities and

similarities (or even universals) between different societies, while a relativist would emphasise the uniqueness and particularity of each society or culture. The Sophists of Athens are sometimes described as the first philosophical relativists in the European tradition (several almost contemporary thinkers in Asia, such as Gautama Buddha, Confucius and Lao-Tze, were concerned with similar questions). In Plato's (427–347 BCE) dialogues *Protagoras* and *Gorgias*, Socrates argues with the Sophists. We may picture them in dignified intellectual battle, surrounded by colourful temples and solemn public buildings, with their slaves scarcely visible in the shadows between the columns. Other citizens stand as spectators, while Socrates' faith in a universal reason, capable of ascertaining universal truths, is confronted by the relativist view that truth will always vary with experience and what we would today call culture.

Plato's dialogues do not deal directly with cultural differences. But they bear witness to the fact that cross-cultural encounters were part of everyday life in the city-states. The Greek trade routes stretched from the Straits of Gibraltar to present-day Ukraine, they fought wars with Persians and many other 'barbarians'. The very term 'barbarian' is Greek and means 'foreigner'. To a Greek ear it sounded as if these aliens were only able to make unintelligible noises, which sounded like 'bar-bar, bar-bar'. Similarly, in Russian, Germans are to this day called *nemtsy* (the mute ones): those who speak, but say nothing.

Aristotle (384–322 BCE) also indulged in sophisticated speculations about the nature of humanity. In his philosophical anthropology he discusses the differences between humans in general and animals, and concludes that although humans have several needs in common with animals, only man possesses reason, wisdom and morality. He also argued that humans are fundamentally social by nature. In anthropology and elsewhere, such a universalistic style of thought, which seeks to establish similarities rather than differences between groups of people, plays a prominent role to this day. Furthermore, it seems clear that anthropology has vacillated up through history between a universalistic and a relativistic stance, and that central figures in the discipline are also often said to lean either towards one position or the other.

AFTER ANTIQUITY

In the classical Greek city-state, conditions were perhaps particularly favourable for the development of systematic science. But in the

ensuing centuries as well, 'civilised' activities such as art, science and philosophy were cultivated all around the Mediterranean: first, in the Hellenistic period, after the Macedonian, Alexander the Great (356–323 BCE) had led his armies to the northern reaches of India, spreading Greek urban culture wherever he went; then later, during the several centuries when Rome dominated most of Europe, the Middle East and North Africa, and impressed on its population a culture deriving from Greek ideals. In this complex, multinational society, it is not surprising to find that the Greek interest in 'the Other' was also carried on. Thus, the geographer Strabo (*c.* 63–64 BCE–*c.* CE 21) wrote several voluminous tomes about strange peoples and distant places, which sparkle with curiosity and the joy of discovery. But when Christianity was established as state religion and the Roman Empire started falling apart in the mid-fourth century CE, a fundamental change took place in European cultural life. Gone were the affluent citizens of the cities of Antiquity, who could indulge in science and philosophy, thanks to their income from trade and slave labour. Gone, indeed, was the entire city culture, the very glue that held the Roman Empire together as an (albeit loosely) integrated state. In its place, countless local European peoples manifested themselves, carriers of Germanic, Slavic, Finno-Ugric and Celtic traditions that were as ancient as those of pre-urban Greece. Politically, Europe fell apart into hundreds of chiefdoms, cities and autonomous local enclaves, which were only integrated into larger units with the growth of the modern state, from the sixteenth century onwards. Throughout this long period, what tied the continent together was largely the Church, the last lingering trustee of Roman universalism. Under the aegis of the Church, international networks of monks and clergymen arose and flourished, connecting the pockets of learning in which the philosophical and scientific traditions of Antiquity survived.

Europeans like to see themselves as linear descendants of Antiquity, but throughout the Middle Ages, Europe was an economic, political and scientific periphery. Following the rise of Islam in the seventh century, the Arabs conquered territories from Spain to India and, for at least the next seven centuries, the economic, political and intellectual centres of the Mediterranean world lay in sophisticated metropolises such as Baghdad and Cordoba, not in the ruins of Rome or Athens, nor in glorified villages such as London or Paris. The greatest historian and social philosopher of this period was Ibn Khaldun (1332–1406), who lived in present-day Tunisia. Khaldun wrote, among other things, a massive history of the Arabs and

Berbers, furnished with a long, critical introduction on his use of sources. He developed one of the first non-religious social theories, and anticipated Émile Durkheim's ideas about social solidarity (Chapter 2), which are today considered a cornerstone of sociology and anthropology. In line with Durkheim and the first anthropologists who utilised his theories, Khaldun stresses the importance of kinship and religion in creating and maintaining a sense of solidarity and mutual commitment among the members of a group. His theory of the difference between pastoral nomads and city-states may, with the wisdom of hindsight, be said to have been centuries ahead of its time.

A contemporary of Khaldun, Ibn Battuta (1304–1369), was in his way just as significant for the history of anthropology. Not a major social theorist, Battuta is considered to be the most widely travelled person of the pre-industrial world. Born in Tangier in present-day Morocco, Battuta's travels brought him as far east as China and as far south as present-day Tanzania. Battuta's main work, the *Rihla* ('Travels'), was completed in 1355. Although later scholars doubt the authenticity of some of the journeys described in the book, it is considered a major source of knowledge about the world known to the Arabs at the time, and of prevailing interpretations of other cultures.

In spite of the cultural hegemony of the Arab world, there are a few European writings from the late medieval period, which may be considered precursors of latter-day anthropology. Most famous is Marco Polo's (1254–1323) account of his expedition to China, where he allegedly spent 17 years. Another example is the great journey through Asia described in *The Voyage and Travels of Sir John Mandeville, Knight*, compiled by an unknown author in the fourteenth century. Both books stimulated the European interest in alien peoples and customs, although the reliability of their accounts must have been questioned already then (Launay 2010).

Then, with the advent of mercantilist economies and the contemporaneous Renaissance in the sciences and arts, the small, but rich European cities of the late Middle Ages began to develop rapidly, and the earliest signs of a capitalist class emerged. Fired by these great social movements and financed by the new entrepreneurs, a series of grand exploratory sea voyages were launched by European rulers. These journeys – to Africa, Asia and America – are often described in the West as the 'Age of Discovery', though the 'discovered' peoples themselves may have had reason to question their greatness (see Wolf 1982).

THE EUROPEAN CONQUESTS AND THEIR IMPACT

The 'Age of Discovery' was of crucial importance for later developments in Europe and the world, and – on a lesser scale – for the development of anthropology. From the Portuguese King Henry the Navigator's exploration of the West coast of Africa in the early fifteenth century, via Columbus' five journeys to America (1492–1506), to Magellan's circumnavigation of the globe (1519–22), the travels of this period fed the imaginations of Europeans with vivid descriptions of places whose very existence they had been unaware of. These travelogues, moreover, reached wide audiences, since the printing press, invented in the mid-fifteenth century, soon made books a common and relatively inexpensive commodity all over Europe.

Many of the early travelogues from the New World were full of factual errors and saturated with Christian piety and cultural prejudices. A famous example is the work of the merchant and explorer Amerigo Vespucci, whose letters describing his voyages to the continent that still bears his name were widely circulated at the time. His writings were reprinted and translated, but his descriptions of the Native Americans (who were called *Indios*, Indians, since Columbus believed he had found a route to India), reveal a much less scrupulous attitude to facts than in Herodotus' or Khaldun's writings. Occasionally, Vespucci seems to use the Native Americans as a mere literary illustration, to underpin the statements he makes about his own society. Native Americans are, as a rule, represented as distorted or, frequently, inverted reflections of Europeans: they are godless, promiscuous, naked, have no authority or laws; they are even cannibals! Against this background, Vespucci argues effectively for the virtues of absolutist monarchy and papal power, but his ethnographic descriptions are virtually useless as clues to native life at the time of the Conquest.

There were contemporaries of Vespucci, such as the French Huguenot Jean de Léry and the Spanish clergyman Bartolomé de las Casas, who gave more truthful and even sympathetic accounts of Native American life, and such books also sold well. But then, the market for adventure stories from distant climes seems to have been insatiable in Europe at this time. In most of the books, a more or less explicit contrast is drawn between the Others (who are either 'noble savages' or 'barbarians') and the existing order in Europe (which is either challenged or defended). As we shall see in later chapters, the legacy of these early, morally ambiguous accounts still weighs

on contemporary anthropology, and to this day, anthropologists are often accused of distorting the reality of the peoples they write about – in the colonies, in the Third World, among ethnic minorities or in marginal areas. And, as in Vespucci's case, these descriptions are often denounced as telling us more about the anthropologist's own background than about the people under study.

The conquest of America contributed to a veritable revolution among European intellectuals. Not only did it provoke thought about cultural differences, it soon became clear that an entire continent had been discovered which was not even mentioned in the Bible! This potentially blasphemous insight stimulated the ongoing secularisation of European intellectual life, the liberation of science from the authority of the Church, and the relativisation of concepts of morality and personhood. As Todorov (1984) argues, the Native Americans struck at the very heart of the European idea of what it means to be human. The Native Americans were humans, but they did not behave in ways that Europeans considered 'natural' for human beings. What was then human? What was natural? During the Middle Ages, philosophers assumed that God had created the world once and for all and given its inhabitants their particular natures, which they had since retained. Now it was becoming possible to ask whether the Native Americans represented an *earlier stage* in the development of humanity. This in turn led to embryonic notions of progress and development, which heralded a radical break with the static worldview of the Middle Ages, and in the later history of anthropology, notions of development and progress have at times played an important role. But if progress is possible, it follows that progress is brought about by the activity of human beings, and this idea, that people shape their own destinies, is an even more enduring notion in anthropology.

Thus, when the Europeans examined themselves in the mirror held up by the Native Americans, they discovered themselves as free, modern individuals. Among the most striking expressions of this new-found, subjective freedom, are the *Essais* (1580) of the French philosopher Michel de Montaigne (1533–1592). With an open-mindedness and in a personal style that were unheard of at the time, Montaigne speculates about numerous issues large and small. Unlike nearly all his contemporaries, Montaigne, in his writings about remote peoples, appears as what we today would call a cultural relativist. In the essay 'Of Cannibals', he even concludes that if he had been born and raised in a cannibal tribe, he would in all likelihood himself have eaten human flesh. In the same essay,

Montaigne invoked *le bon sauvage*, 'the noble savage', an idea of the assumed inherent goodness of stateless peoples, which is another part of the common heritage of anthropology.

In the following centuries, the European societies expanded rapidly in scale and complexity, and intercultural encounters – through trade, warfare, missionary work, colonisation, migration and research – became increasingly common. At the same time, 'the others' became increasingly visible in European cultural life – from Shakespeare's plays to Rameau's librettos. Every major philosopher from Descartes to Nietzsche developed his own doctrine of human nature, his own philosophical anthropology, often basing it directly on current knowledge and beliefs about non-European peoples. But in most of these accounts 'the others' still play a passive role: the authors are rarely interested in their lifeways as such, but rather in their usefulness as rhetorical ammunition in European debates about Europe, or about 'Man', usually synonymous with a 'Male European'.

A famous example was the great seventeenth-century philosophical debate between *rationalists* and *empiricists*. The former position was held by René Descartes (1596–1650), a Frenchman of many talents, who made substantial contributions to mathematics and anatomy, and is widely considered to be the founder of modern philosophy. Among anthropologists Descartes' name is almost synonymous with the sharp distinction he supposedly drew between consciousness and spiritual life on the one hand, and the material world and the human body on the other. However, the clear-cut 'Cartesian dualism' that is often criticised by anthropologists is a caricature of Descartes' thought. Descartes distinguished two kinds of substance: that of thought and mind, which had no spatial dimensions, and that of the spatially organised world. The latter could be partitioned up, measured and made subject to the laws of mathematics so its true properties might be revealed, the former could not. But by critical reasoning one could identify ideas that were axiomatically true.

The primary task of philosophy was to identify ideas that would form an unassailable basis for scientific knowledge of the external world. To achieve this, Descartes assumed an attitude of 'radical methodological doubt': any idea that may be doubted is uncertain, and thus an unsuitable foundation for science. Not many ideas survived Descartes' acid test. His famous *cogito ergo sum* ('I think, therefore I am') expressed his primary certainty: I can be sure that I exist since I know that I think. Descartes' philosophical system is derived from this axiomatic truth.

Descartes was not a social philosopher. Still, he was a child of his times. He asserted that the individual was the measure of all things. If God's existence can be proven, it must be on the basis of the individual's certain knowledge of himself. Even if God, through the inborn ideas, was the ultimate source of certain knowledge, it was the reason of individuals that separated true ideas from falsehood, applied true ideas on the world, and 'perfected' society 'from a semi-barbarous state … to civilization' (Descartes 1637: part 2).

Descartes' belief in reason, typified in the clear and consistent laws of geometry, was shared by his opponents, the British empiricists. The empiricists also attempted to establish a foundation for certain knowledge, but Descartes' notion of axiomatic ideas was unacceptable for them. John Locke (1632–1704), the first great Empiricist philosopher (Chapter 6), claimed that the human mind was a blank slate, *tabula rasa*, at birth. Our ideas and values have their origin in our experiences, or 'sense impressions', as they were called. *Tabula rasa* is a much used and abused term. Locke did not claim that people were born with no abilities at all. One had an inborn intellect. When sense impressions put their mark on the blank slate, the intellect combined them with other sense impressions to form ideas about the world that became points of departure for abstraction and generalisation. Here Locke is laying the groundwork of a human science that combines a universalistic principle (we are all born the same) and a relativistic principle (our differing experiences make us different).

Locke was a political liberalist and a confirmed democrat, and his philosophical empiricism is related to his political argument for the idea of 'natural law' (*lex naturalis*). Like 'Cartesian dualism' the notion that all humans have certain inborn rights goes back to the Middle Ages, when it was argued that natural law was established by God. Locke claimed that natural law was not a gift from God or princes, but a defence of the individual's needs. Thus, Locke's argument explicitly contradicted that of the rationalists, but his basic anthropology was similar to theirs. As in Descartes, the individual was the measure of all things. This was a radical view in the seventeenth century. Even when it was used to justify the power of princes (as Thomas Hobbes did), it had revolutionary force. All over Europe, kings and princes were confronted by the demands of an increasingly restive and powerful liberal bourgeoisie: demands that the Ruler be bound by law to respect the rights of individuals to property, personal security and rational public debate.

As in Descartes, the 'primitives' are a minor concern within this larger argument. They remain a category of contrast. 'Children, idiots, savages and illiterate people' are 'of all others the least corrupted by custom, or borrowed opinion', writes Locke. But, he continues, if we consider their behaviour, we see that they are helpless, they have no inborn ideas to support them. Therefore they must be 'improved' (Locke 1690: §27).

The legacy of these seemingly distant philosophical debates is still, as we shall see later in this book, evident in anthropology today. An empiricist stamp rests on British anthropology, a rationalist stamp on French *ethnologie*. On German anthropology completely different influences came to bear.

WHY ALL THIS IS NOT QUITE ANTHROPOLOGY YET

This brief review of the prehistory of anthropology has suggested that a number of issues that would later attain prominence in the discipline had been the subject of extensive debate since Antiquity. Exotic peoples had been described normatively (ethnocentrism) or descriptively (cultural relativism). The question had repeatedly been raised whether people everywhere and at all times are basically the same (universalism) or profoundly different (relativism). There had been attempts to define the difference between animals and humans, nature and culture, the inborn and the learned, the sensual body and the conscious mind. Many detailed descriptions of foreign peoples had been published; some were based on meticulous scholarship.

In spite of these continuities, we maintain that anthropology as a science only appeared at a later stage, though it is true that its birth was a more gradual process than is sometimes assumed. Our reasons for this are, first, that all the work mentioned so far belongs to one of two genres: travel writing or social philosophy. It is only when these aspects of anthropological enquiry are fused, that is, when data and theory are brought together, that anthropology appears. Second, we call attention to the fact that all the writers mentioned so far were influenced by their times and their society. This is of course true of modern anthropologists as well. But modern anthropologists live in a modern world, and we argue that anthropology makes no sense at all outside a modern context. The discipline is a product, not merely of a series of singular thoughts such as those we have mentioned above, but of wide-ranging changes in European culture and society, that in time would lead to the formation of

capitalism, individualism, secularised science, patriotic nationalism and cultural reflexivity.

On the one hand, then, certain topics have followed us throughout the time we have dealt with so far. On the other hand, from the fifteenth century onwards, a range of new ideas and new forms of social life have appeared, which will form the groundwork on which anthropology and the other social sciences will be built.

Two of these new ideas have been discussed above. First, we have seen that the encounter with 'the Other' stimulated European intellectuals to see society as an entity undergoing change and growth, from relatively simple, small-scale communities, to large, complex nations. But the idea of development or progress was not confined to notions of social change. The individual, too, could develop, through education and career, by refining his personality and finding his 'true self'. As the sociologist Bruno Latour (1991) points out, the idea of the autonomous individual was a prerequisite for the idea of society. Only when the free individual was established as 'the measure of all things' could the idea of society as an association of individuals put down roots and become an object of systematic reflection. And only when society had emerged as an object to be continuously 'improved' and reshaped into more 'advanced' forms could the independent, rational individual change into something new and different, and even 'truer to its nature'. And without an explicit discourse about these ideas, a subject such as anthropology could never arise. The seeds were sown in early modern philosophy, important advances were made in the eighteenth century, but it was only in the nineteenth century that anthropology became an academic discipline, and only in the twentieth century that it attained the form in which it is taught today. We shall now turn to the intellectual currents of the eighteenth and early nineteenth centuries, before recounting – in the next chapter – how the discipline of anthropology came of age.

THE ENLIGHTENMENT

The eighteenth century saw a flowering of science and philosophy in Europe. The self-confidence of the bourgeoisie increased, citizens reflected on the world and their place in it, and would soon make political demands for a rational, just, predictable and transparent social order. The key word was *enlightenment* (*Aufklärung*, *lumières*), literally shedding light on matters that had so far been left in the dark. As Locke and Descartes had argued, the free individual

was to be the measure of all things – of knowledge and of the social order: the authority of God and princes was no longer taken for granted. But the new generations of intellectuals developed these ideas further. They met in informal clubs and salons to discuss art, philosophy and social issues. Private letters and diaries evolved into newspapers and novels, and although censorship was still common in most places in Europe, the new media soon gained greater freedom and wider circulation. The bourgeoisie sought to free itself from the power of Church and nobility, and to establish in their stead a secular democracy. Traditional religious beliefs were increasingly denounced as superstitions – roadblocks on the way to a better society, governed by reason. The idea of progress also seemed to be confirmed by the development of technology, which made important advances at this time. New technologies made scientific measurements more accurate. Industrial machinery made a hesitant debut. Descartes' purely theoretical attempt to prove the universal truth of mathematics was becoming a practical issue of incalculable relevance. For if mathematics, the language of reason, could reveal such fundamental natural truths as Newton's laws, did it not follow that nature was itself reasonable, and that any reason-driven enterprise was bound to succeed? All these expectations culminated in the French Revolution, which attempted to realise the dream of a perfectly rational social order in practice, but was quickly superseded by its irrational opposite: the revolution devoured its children. Then the dreams, the disappointments, the paradoxes of the Revolution spread during the Napoleonic Wars in the early 1800s to all of Europe, deeply influencing the ideas of society that later generations would develop.

But we are still in the eighteenth century, the 'Age of Reason', when the first attempts were made at creating an anthropological science. An important early work was Giambattista Vico's (1668–1744) *La scienza nuova* (1725; *The New Science*, 1999), a grand synthesis of ethnography, religious studies, philosophy and natural science. Vico proposed a universal scheme of social development, in which all societies passed through four phases, with particular, well-defined characteristics. The first stage was a 'bestial condition' without morality or art, followed by an 'Age of Gods', of nature worship and rudimentary social structures. Then came the 'Age of Heroes', with widespread social unrest due to great social inequality, and finally the 'Age of Man', when class differences disappeared and equality reigned. This epoch, however, was in its turn threatened by internal corruption and degeneration

to 'bestiality'. Here, for the first time, we see a theory of social development that not only contrasts barbarism and civilisation, but specifies a number of transitional stages. Vico's theory would become a model for later evolutionists from Karl Marx to James Frazer. But Vico has an element that many of his followers lacked. Societies do not necessarily develop linearly towards constantly improved conditions, but go through cycles of degeneration and growth. This gives Vico's Enlightenment work a critical and romantic subtext, as in Rousseau (see below).

Vico was an Italian pioneer, but it was in France that the first steps were taken towards the establishment of anthropology as a science. In 1748, Baron de Montesquieu (1689–1755) published his *De l'esprit des loix* (*The Spirit of Laws*, 1977). This is a comparative, cross-cultural study of legislative systems which Montesquieu had first- or second-hand knowledge of, and from which he attempts to derive the general principles that underlie legal systems cross-culturally. Montesquieu pictures the legal system as an aspect of the wider social system, intimately entwined with many other aspects of the larger whole (politics, economy, kinship, demography, religion, and so on) – a view that has led many to describe him as a proto-functionalist (Chapter 3). According to Montesquieu, polygamy, cannibalism, paganism, slavery and other barbarous customs could be explained by the functions they fulfilled within society as a whole. Montesquieu also wrote the remarkable *Lettres persanes* (1722; *Persian Letters*, 1973), a collection of fictitious letters from two Persians describing France to their countrymen. He here exploits the 'strangeness' of cultural difference to parody France at the time of Louis XIV. The book is thought-provoking. Even today it remains controversial, since Montesquieu has been accused of being a proto-*Orientalist* (Said 1978, 1993), who unduly emphasised the exoticism of the Persians. This critique is undoubtedly justified, and Montesquieu's primary aim is clearly not to describe Persia but to criticise France. But the Persian letters also reveal an understanding of a problem in contemporary anthropology that might be referred to as *homeblindness*: our inability to see our own culture 'objectively', 'from outside'. Montesquieu employed a particular technique to overcome this problem: he described his own society from the point of view of an outsider, a technique that is widely used in anthropology today.

Yet another step towards a science of anthropology was taken by a group of idealistic French intellectuals. These were the Encyclopaedists, led by the philosopher Denis Diderot (1713–1784) and

the mathematician Jean Le Rond d'Alembert (1717–1783). Their aim was to collect, classify and systematise as much knowledge as possible in order to further the advance of reason, progress, science and technology. Diderot's *Encyclopédie* was published in 1751–72, and included articles by illustrious intellectuals like Rousseau, Voltaire and Montesquieu. The encyclopaedia quickly established itself as a model for later projects of its kind. It was a liberal and wide-ranging, not to say a revolutionary work, which was censored in many parts of Europe for its criticism of the Church. But the 17 volumes of text and 11 volumes of illustrations also contained other controversial material, such as detailed descriptions of mechanical devices developed by ordinary farmers and craftsmen. The fact that such matters were taken seriously in an academic work was unheard of at the time, and hinted that it would soon be legitimate to study the everyday life of ordinary people. The encyclopaedia also contained detailed descriptions of culture and social customs all over the world. One of its youngest contributors, Marquis de Condorcet (1743–1794), who was to die prematurely in a Jacobin jail, wrote systematic comparisons between different social systems, and tried to develop a synthesis of mathematics and social science that would allow him to formulate objective laws of social development.

The most influential contributor to the *Encyclopédie* was undoubtedly Jean-Jacques Rousseau (1712–1778). Contrary to his French and British contemporaries (but not unlike Southern Europeans such as Vico), Rousseau argued that development was not progressive, but degenerative, and that the source of decline was society itself. Starting from an initial, innocent state of nature, where each individual lived by himself in harmony with his surroundings, people went on to found institutions of marriage and kinship, and settled in small, sedentary groups. Eventually, these groups grew in complexity, and invented priests and chiefs, kings and princes, private property, police and magistrates, until the free and good soul of man was crushed under the weight of society. All human vices were the product of society's increasing demands on the individual, particularly the increasing social inequality that development entailed. 'Man was born free, but is everywhere in chains', he declares in *Du contrat social* (1762; *On the Social Contract*, 1978). But the false social contract could be replaced by a true one, based on freedom and democracy, and this is where Rousseau's importance becomes evident. An individual, says Rousseau, is free if he follows a law he has set for himself, and society can freely follow a law that was collectively adopted. But society consists of many subjects,

each with his own will. The true social contract therefore implies a particular relationship of *exchange*: the individual gives up his natural rights in return for rights as a citizen of society, which give greater and longer-lasting freedom. But the individual, though good, is often stupid. Great leaders are therefore needed, to establish good judicial systems, if necessary by subterfuge or force. Here we see the inspiration of one of Rousseau's greatest influences, the Italian political philosopher Niccolò Machiavelli (1469–1527).

The paradoxical passage above about the relationship of individual to society is packed with insights that would have heavy influence on future events. Most clearly this is seen in Karl Marx (Chapter 2), who was inspired by Rousseau's ideas about inequality and property, human nature and alienation. Rousseau's ideas about the exchange relation underlying the true social contract inspired Claude Lévi-Strauss' theory of society as a product of exchange (Chapter 6). More generally, Rousseau's elevation of 'primitives' at the expense of Europe's corrupted civilisation was an important precursor of anthropological cultural relativism, although for Rousseau, as for so many others, the 'primitives' were primarily a mirror image of his own society, a viewpoint that hardly stimulated empirical investigations of real (primitive or modern) societies.

Most importantly, though, Rousseau was a mediator between the French-dominated Enlightenment and the predominantly German Romanticism that took over the leading position in European philosophy toward the end of the eighteenth century. Here, Rousseau's admiration for the original human being was further developed, the first theoretical concepts of *culture* were put forth, and the outlines of scientific anthropology start emerging.

ROMANTICISM

While Enlightenment thinkers saw society as a law-bound association of reasoning citizens, Romanticism cultivated the creative, emotional individual, and the warm-blooded community of feeling – the nation. Romanticism is often said to displace the Enlightenment during the years of reaction after the French Revolution. But it may be more accurate, as Gellner (1991) suggests, to see the two movements as parallel flows, at times diverging or competing, at times intersecting and binding together. This is especially true of anthropology, which seeks not only to understand cultural wholes (a Romantic project), but also to dissect, analyse and compare them (an enterprise of the Enlightenment).

Romanticism spread everywhere in Europe, but its influence was greatest in Germany. In the eighteenth century, when France and England were strong, centralised states, Germany was little more than a diffuse linguistic area, embracing a medieval patchwork of independent principalities, free cities and multi-ethnic landscapes that it would take another hundred years to forge into a unified nation state. Germans had reason to speculate about what bound their nation together. The French could safely invoke the universality of human reason, as long as French fashion, language and nobility dominated the Western world and defined *what reason was*. One of the most popular German romantics even took a French pen-name: Jean Paul. Predictably, the politically fragmented, but culturally articulate Germans would at some point react to French domination.

1770 was a seminal year for this movement. It was when the young poet Johann Wolfgang von Goethe (1749–1832) – the soon-to-be spiritual father of the German nation – met the philosopher Johann Gottfried von Herder (1744–1803), who had recently made a major contribution to linguistics. Their meeting is said to be the spark that ignited the *Sturm und Drang* epoch of German cultural history, with its sharply reasoned philosophy and its worship of the poet's solitary yearnings and the people's deep and inscrutable fate. In 1784–91, Herder published his magnum opus, *Ideen zur Philosophie der Geschichte der Menschheit* (*Reflections on the Philosophy of the History of Mankind*, 1968), where he presents the ideas that had made him famous during the last 15 years as parts of a wider, continuous argument. He attacks French universalism as it was propounded, for example, by Voltaire, and argues that human experience is a totality that cannot be split into separate functions, such as reason, sense perception and emotion. Every people (*Volk*) shares a holistic, bodily experience, grounded in common history, common dependence on local natural environments and a national character (*Volksgeist*) that expressed itself through language, folklore and myths.

According to Herder, cosmopolitanism and cultural intermixture damaged the nation's moral integrity. This notion of *Volk* added fuel to the nationalist ideologies that swept like wildfire through nineteenth-century Europe. However, Herder is also considered the father of the anthropological concepts of culture and cultural relativism. During the many years he spent in Riga, he investigated Latvian folk traditions and poetry, and found a *Volksgeist* buried in them that was suppressed by (German-led) internationalism. It

is an important paradox that cultural relativism and nationalism both trace their origins back to Romanticism.

The greatest philosopher of the time was Immanuel Kant (1724–1804), whose contribution to European thought is too pervasive to fit into any philosophical school. Here we consider him a German Romanticist in order to highlight how his work was continued by the Romanticist Hegel. The Romantic element in Kant lies in his overcoming the split between sensual and rational knowledge. In his *Kritik der reinen Vernuft* (1781; *Critique of Pure Reason*, 1991) Kant argued that empiricism and rationalism were not opposed, but two sides of the same coin. Knowledge was both sensual and mathematical, objective and subjective. The problem was not a matter of choosing between extremes, but of demonstrating how they presuppose each other. After Kant's revolution, knowledge no longer consisted of mental images that reflected reality as it is in itself more or less adequately, but of mental *judgements* based on criteria that are subjective (they exist only in the mind), but also objective (they are universally present in every knowing mind).

We argue that these formulations made social science possible. We do not imply that Kant single-handedly laid the groundwork for the sciences of society. However, Kant established the preconditions for a species of social theory that has shaped anthropology deeply. A direct line leads from Kant, via Hegel, to Marx, Durkheim, Weber and the classical sociology that remains the core of anthropological theory to this day. Kant opened up a new field of intellectual endeavour by demonstrating that it was possible to produce scientific knowledge about society. In all the precursors of social science we have seen so far, we sense an underlying uncertainty about the very definition of the social. What kind of reality was society? What could we know about it? Some (with Vico) were attracted to the natural sciences, hoping to discover social laws similar to the laws of physics. Others (as Rousseau) saw their role as more artistic. Now Kant seemed to offer a third way. Knowledge is self-reflexive, the subject must be conscious of itself as a knowing subject in order to know the object. To study 'the world out there' is to study the encounter between the world and myself. Our meeting, gives the world a subjectively knowable form, that still is objective, since it derives from universal qualities inherent in understanding as such. As any anthropologist on fieldwork will tell you: to know the world is to contribute to its creation. Suddenly it seemed possible that those parts of the world that are not extended in space – Descartes' thought substance – could indeed be investigated scientifically. Still,

something hindered the direct application of Kant's insights to social science. This 'something' would be only be addressed by his successor, Georg Wilhelm Friedrich Hegel (1770–1831).

Kant's critical reasoning shook the foundations of Western thinking, and after his death there were many attempts to find loopholes in his logical construction and complete the revolution he started. Hegel's goal was to bring together Kant's idea of the universal preconditions of knowledge, and the particularistic orientation of Herder and the Romanticists. Kant's knowing subject existed outside context and history. It belonged to no concrete place or time. Hegel sought to reinstate it in the world by focusing on its 'spirit' (*Geist*) – a concept he developed in great and often cryptic detail in *Die Phänomenologie des Geistes* (1807; *Phenomenology of Spirit*, 2000).

Like Kant's knowledge, Hegel's spirit is self-reflexive: A subject can know another only by knowing itself as a knowing subject. Hegel adds intersubjectivity to this picture: A subject can only know itself when it is known by another knowing subject. 'Spirit' is the relationship between the knower and the known – two points with no independent existence, their only being is their relation (Habermas 1999).

From a social scientist's point of view, Kant's revolution was now complete. Knowledge of society is knowledge of 'spirit', of self-reflexive relations and patterns of relations. Hegel refers to this pattern as a whole, as the 'world spirit' (*Weltgeist*). It has its centres and peripheries, and changes in accordance with evolutionary laws. Later theoreticians have described it in various terms, as structure, function, solidarity, power, system, aggregate or discourse. Indeed, Hegel's far-ranging discussion of the dialectics of the world spirit's self-expression through history, was not only the first systemic description of sociality in motion, but the first systematic vision of a truly global humanity (Geana 1995).

But still, this is not social science. The communicative collective and the subjects participating in it are too abstract and lacking in context. Yet it is here we find the root of the idea of a socially constructed reality (Berger and Luckman 1966), which is our most important heritage from eighteenth-century European philosophy.

But this idea also had strong affinities with the nationalist movements that Herder had inspired, which had spread throughout Europe in the decades after 1800. Nations were precisely such socially constructed realities ('imagined communities', according to one modern authority) as Hegel had described, each with its

unique style and character. Ideally, the nation was a collectivity of the people, ruled by the people, in accordance with the people's deepest longings and needs. But although nationalism was inspired by Romantic philosophy, as a social movement it was a product of underlying historical processes: the political upheavals in the wake of the Napoleonic Wars, the sense of alienation brought about by industrialisation, the spread of revolutionary ideals of freedom, equality and brotherhood.

It was into this world of upheaval and transition that anthropology first emerged as an academic discipline. The first step was the establishment of the ethnographic museums. Collections of exotica had long existed at the European courts. One of the earliest, founded by Danish King Frederik III, dates back to 1650 and would later form the basis of the Danish National Museum. But systematic collection of ethnographica only started in the 1800s. Large national museums were established in London (1753), Paris (1801) and Washington, DC (1843), and these would all eventually develop influential ethnographic departments. Still, the first specialised ethnographic museums were established in German-speaking areas, notably Vienna (1806), Munich (1859) and Berlin (1868). This may seem surprising, as Germany and Austria had no colonial empires. Nevertheless, German academics had, in accordance with Herder's programme, begun to carry out empirical studies of the customs of 'the people'. They collected data on peasant life – folktales and legends, dress and dance, crafts and skills. The earliest museums were primarily concerned with *Volkskunde* (the study of peasant cultures at home) rather than *Völkerkunde* (the study of remote peoples). Thus, the institutionalisation of anthropology commenced in Germany, rather than in France or Britain – a fact that is often overlooked in accounts of the history of anthropology.

As the next chapter will show, German anthropology retained an important, in some respects a dominant, position throughout the nineteenth century, while in Britain a more peculiar 'Victorian' anthropology emerged.

2
Victorians, Germans and a Frenchman

Between the Napoleonic Wars (1792–1815) and the First World War (1914–18), we see the rise of modern Europe – and of the modern world. This was, above all, the age of the Industrial Revolution. In the 1700s, profound transformations had taken place in agriculture and manufacturing, particularly in Britain. Steam power and spinning machines had become widespread, and a growing class of landless peasants and urban labourers began to make themselves heard. But greater changes were ahead. In the 1830s, the first major railways were built; a decade later, steamships crossed the Atlantic on a regular basis; in 1838, Samuel Morse demonstrated the first functioning telegraph. It was becoming possible, on a scale the world had never seen before, to move vast quantities of information, raw materials, commodities and people across global distances. This, in turn, meant that production could be increased both in agriculture and manufacturing. Europe was made able to feed more people than ever before – by increasing production and expanding imports. In 1800, Britain had 10.5 million inhabitants. By 1901, there were 37 million, 75 per cent of whom lived in towns and cities. Peasants deserted the countryside, forced out by population pressure and rationalisation of agriculture, and migrated to urban centres like London or Paris, where they were re-socialised as workers. Conditions in the rapidly growing cities were hardly optimal: epidemics were common, and when the first British law against child labour was introduced in 1834, it affected only children under the age of nine. But conditions in the countryside were often even worse, as during the Great Famine in Ireland in 1845–52. In time, protests against these changes increased in frequency and scale. The most dramatic example was the French Revolution, but the Chartist revolt in Britain in the 1840s, the French, Austrian and Italian Revolutions in 1848–49, the Paris Commune of 1870, indicate the potential for violence that industrialisation unleashed. Along with the protests, a new, socialist ideology emerged. Its roots went back to social philosophers such as Rousseau and the proto-socialist Henri de Saint-Simon (1760–1825), and to the German

neo-Hegelians, but its decisive formulation came with Karl Marx, to whom we shall shortly return. For the nineteenth century was also the century of the working class. In 1867 male British workers won the franchise, and soon they would achieve even greater gains in the class struggle.

But the success of the labour movement would hardly have been possible without the train and the steamship. More than 60 million European emigrants were transported by rail and ship to the USA, Australia, Argentina, South Africa, Siberia and elsewhere, relieving population pressure, and permitting a long-term rise in standards of living for those who remained. Meanwhile, in the colonies, administrations spread European culture and institutions. This grand process of diffusion had variable effects. New power relations arose – between colonial administrator and Indian merchant, between plantation owner and black slave, between Boer, Englishman and Bantu, between settler and Australian aborigine. In the wake of these new manifold relations of dominance and dependence, new philosophies, ideologies and myths rose to defend or attack them. The campaign against slavery is an early example, and slavery was successfully abolished in the British and French dependencies in 1833. But racism, which first emerged as an organised ideology during the nineteenth century, was a response to the same processes. So was the internationalised science that now emerged. The global researcher becomes a popular figure – the prototype being Charles Darwin (1809–1882), whose *On the Origin of Species by Means of Natural Selection* (1859) was based on data collected during his famous six-year circumnavigation of the globe.

EVOLUTIONISM AND CULTURAL HISTORY

Global research demands global communications, hence it is hardly surprising that anthropology emerged as a discipline in these years. So did sociology. If anthropology was an outgrowth of colonialism, sociology arose from the changing class relations that industrialisation brought about in the West. All the classical nineteenth-century sociologists speculated on the nature of modernity and contrasted it with premodern conditions. Both disciplines shared a basic evolutionist orientation. Both studied society. And empirical data on premodern societies were delivered to both by anthropologists.

Despite such common interests, a deep split ran between the two subjects throughout the nineteenth century. The split expressed itself most profoundly in the German tradition. As we have seen,

the first ethnographic museums were established in Germany and Austria-Hungary, and this was where the foundations of German anthropology were laid. Here, the discipline was defined in Romanticist and humanistic terms, as a branch of cultural history rather than a social science, and the classical sociology that was also emerging in Germany at the time was deemed irrelevant and ignored. British anthropology developed later, was less museum-based, and identified more closely with the natural sciences. This undoubtedly stimulated greater openness toward the social sciences as well; on the other hand, little of note was happening in British sociology. In the United States the initial situation was similar, then German anthropology became the dominant influence, and sociology made no impression at all until much later. The split between the disciplines was least pronounced in France, and it was a Frenchmen who would finally restore the connection between them.

Theoretical discourse also differed markedly, though there were points of contact as well. Outside the German sphere of influence, anthropologists continued to develop the evolutionary ideas of the eighteenth century, proposing ever-new variations on Vico's theory of stages. As we have seen, the idea that non-European societies were 'less developed' goes back to the 1500s, thus, the evolutionism subscribed to by nineteenth-century anthropologists was hardly breaking news, though their theories gained in complexity and their data in detail. The added stimulus provided by Darwinism after 1860 did little to change this. We should note that at the time, evolutionism was not associated with racism. On the contrary, like all the leading anthropologists of the day, the evolutionists subscribed to the principle of the 'psychic unity of mankind', first formulated by the German cultural historian Adolf Bastian (see below). This principle states that human beings everywhere belong to the same biological species, and genetic differences between cultures are cosmetic. In fact, the entire anthropological project rests on this tenet. Social evolutionists and cultural historians alike based their work on systematic cross-cultural comparison. If cultural differences were biologically determined, anthropologists were out of a job.

Meanwhile, Continental sociology followed the lead of Kant and Hegel, and explored the 'socially constructed reality' opened up by the two philosophers. The founding sociologists realised this project in various ways, but they shared the idea of society as an autonomous reality that must be studied on its own terms and with its own methods, not as a branch of natural science. Like

the anthropologists, the sociologists asserted the psychic unity of mankind and deferred to some variant of evolutionist theory. Unlike the anthropologists, who classified and compared the external characteristics of societies all over the globe, sociologists were concerned with the internal dynamics of Western, industrial society. Unlike the anthropologists too, nineteenth-century sociologists engaged in a sophisticated discourse on the dynamics of social systems, This would have a fundamental impact on anthropology as well, but only in the early twentieth century.

Here we shall illustrate the differences between these two emerging traditions with the work of two of their most prominent pioneers: the American anthropologist Lewis Henry Morgan and the German sociologist Karl Marx.

MORGAN

Lewis Henry Morgan (1818–1881) grew up on a farm in New York, in the America of equality and opportunity that the French sociologist Alexis de Tocqueville would describe so prophetically in 1835 and with such ambivalent feelings. He was educated as a lawyer, and became a prosperous and active participant in local politics. An early champion of Native American political rights, he had been fascinated by Native Americans since his youth. In the 1840s, he lived with the Iroquois for some time, was adopted into one of their clans and given the name *Tayadaowuhkuh*: 'he who builds bridges'. Morgan may in this sense be considered the first anthropological fieldworker.

Morgan witnessed the destruction of Native American society – notably their political and economic institutions – as a result of the massive influx of European settlers, and believed that most of their immaterial culture would soon also be irretrievably lost. He considered it a crucial task to document traditional culture and social life before it was too late. This attitude, often referred to as *urgent anthropology*, was shared by the second great American anthropologist, Franz Boas (Chapter 3), and is widespread in research on indigenous peoples.

Morgan had close contact with the people he studied, sympathised with their problems, and published detailed accounts of their culture and social life. But he also made substantial theoretical contributions, particularly in his pioneering work on *kinship*. Morgan's interest in kinship dated back to his stay with the Iroquois. Later, he discovered surprising similarities and differences between their kinship system

and others in North America. He devised a large-scale comparative study of Native American kinship, eventually including other groups as well. Morgan created the first typology of kinship systems (cf. Holy 1996), and introduced a distinction between *classificatory* and *descriptive* kinship that remained unchallenged until the 1960s. To simplify greatly – descriptive systems (like our own) differentiate kinsmen in ways that closely approximate biological kinship. Classificatory kinship (as with the Iroquois) distributes individuals among broad kinship categories that may bear little or no relation to biological relations. In the first case, your in-laws never become family; in the second, they automatically do. But Morgan did more than formulate a theory; he grounded it in years of intensive study of existing kinship systems around the world. In his influential *Systems of Consanguinity and Affinity of the Human Family* (1870), the results of this research are presented, defining kinship, once and for all, as a primary anthropological concern.

For Morgan, kinship was primarily a point of entry to the study of social evolution. He argued that primitive societies were organised on the basis of kinship, and that terminological variations among kinship systems correlated with variations in social structure. But he also supposed that kinship terminology changed slowly and unevenly, and therefore contained clues to an understanding of earlier stages of social evolution.

In his magnum opus *Ancient Society* (1877), Morgan attempts a grand synthesis of all this work. He distinguishes three major stages of cultural evolution: savagery, barbarism and civilisation (with three sub-stages each for savagery and barbarism). His criteria for these divisions are mostly technological: his 'savages' were hunters and gatherers, 'barbarism' was associated with agriculture, 'civilisation' with state formation and urbanisation. In hindsight, it seems clear that Morgan's synthesis did not succeed. Even if his basic evolutionary scheme is accepted, the details remain hazy. At times, isolated technological features are accorded unreasonable weight – for example, pottery is *the* criterion of the transition between two sub-stages. Where does that leave the Polynesian chiefdoms, with their complex political systems, but not a trace of pottery? It is only fair to add that Morgan himself was conscious that his conclusions were often speculative, and critical of the quality of his (mostly secondary) data.

Morgan had considerable influence on later anthropology, particularly on kinship studies, but also on American cultural materialists and other evolutionist anthropologists from the

mid-twentieth century onwards (Chapter 5). But Morgan was read by sociologists as well. When Marx discovered Morgan toward the end of his life, he and his collaborator, Friedrich Engels, attempted to integrate Morgan's ideas in Marxian evolutionary theory. The unfinished results of this work were published by Engels in *The Origin of the Family, Private Property, and the State*, in 1884, a year after Marx's death.

MARX

The scope and aims of Karl Marx's (1818–1883) work contrast sharply with Morgan's, despite their shared commitment to materialist explanation. Marx's writings on non-industrial societies are scattered and tentative. It was through his analysis of capitalist society in his foundational work, *Das Kapital* (vols 1–3, 1867, 1885, 1896; *Capital*, 1906), that he made his lasting contribution to social theory. Though the political influence of Marxism has waxed and waned, Marxian theory has remained an important academic influence.

Born in the same year as Morgan, in an inconspicuous German town, into a wealthy Jewish family – his parents eventually converted to Lutheranism – Marx completed a university education in philosophy before embarking on a career as social theorist, pamphleteer, editor, journalist, labour organiser and agitator. He was actively involved in the revolutionary wave that shocked the European establishment in 1848–49, and in the Paris Commune of 1870. After the Commune, he became known as the leading theorist of the international labour movement.

Marx is beyond doubt the most influential social theorist who ever lived, and his influence may be traced in innumerable analyses in most of the humanistic and social sciences. Simultaneously, Marx was a prominent political actor who contributed substantially to the formation of the nineteenth century's labour movements and their offspring, from social democrats to Stalinists, in the twentieth century. The confluence of social theory and political activism runs deep in Marx, and gives his entire project a paradoxical and thought-provoking character (Berman 1982). Marx was an idealist Hegelian before he became a materialist, and his life's project consisted, it is tempting to say, in unifying these contradictory impulses. From Hegel he had the idea of development toward a utopian goal, but he replaces the evolution of the world spirit with social evolution from (simple) classless societies to (complex) class societies, broken by an

apocalyptic transition to the new, classless society of Communism. Here, it is not difficult to hear Rousseau between the lines. Marx also derived from Hegel the idea that evolution is driven by dialectics, that is, by conflicts – or 'contradictions' – that merge into higher unities, which again enter into conflict, and so forth. But in Hegel the conflicts are spiritual; in Marx they are material and social: between the diverse factors that participate in material production; between the social organisation of the system of production as a whole ('infrastructure') and the 'superstructure' of power and symbolism that the infrastructure sustains; between classes with conflicting relations to the system of production; between differing systems of production that collide.

The theory is so ambitious, and in many respects so ambiguous, that it was bound to raise problems when confronted with real-world complexities. An example is Marxian class analysis. Marx postulated that property holders and the propertyless constitute discrete classes with particular interests. The objective interest of the working class consists in overthrowing the ruling class through revolution. But the working class is only partly conscious of being exploited, since the true power relations are concealed by an *ideology* that justifies the existing order. Superstructural phenomena such as law, religion or kinship are typically infused with an ideological 'false consciousness' that pacifies the population.

But, asks the anthropologist, is this model applicable to non-Western contexts? How does it fit with Morgan's dictum that kinship is the primary organising principle in primitive societies? Is kinship part of the infrastructure? How can that be, if kinship is an ideology that *conceals* the infrastructure? Must the entire distinction between infra- and superstructure, the material and the spiritual, be abandoned? In what sense, if any, is ideology 'less real' than matter? Such issues have been raised with fervour and sophistication in anthropology, and a significant part of Marx's attraction today lies in his ability to generate questions such as these.

Marx himself was not oblivious to such problems. His extensive discussion of value formation is proof of this. The value of an object in itself, its concrete *use value*, its correspondence to real human needs, is transformed, under capitalism, into an abstract *exchange value*, its value *as compared to* other objects. 'Material' objects are transformed into 'spiritual' commodities, and the further this continues, the more abstract, absurd and alienated does the world seem. In such passages, 'value' becomes a deeply ambiguous concept, in which power and ideology, the material and the spiritual

merge seamlessly. Nevertheless, it remains doubtful whether Marx actually solved the problem he posed for himself. As anthropologists we note that his difficulties with bringing materialism and (Hegelian) idealism together are reminiscent of Morgan's problem with the materialist causes of kinship terminology. Only in the 1980s do we see a concerted effort to resolve this paradox.

BASTIAN AND THE GERMAN TRADITION

The decades before and after 1800 saw an unprecedented blossoming of German culture and science – in part inspired by Kant's philosophical revolution, in part by the national renaissance stimulated by Herder, in part by innovations in linguistics (Herder had an impact here too), and in part by Goethe's overshadowing presence as a poet, scientist and cultural paragon. Aside from poet-philosophers like Fichte and Schiller, the epoch saw many scientific advances: the theologian Friedrich Schleiermacher (1768–1834) gave the hermeneutic method its first modern formulation; the brothers Jacob and Wilhelm Grimm founded folklore studies; Alexander and Wilhelm von Humboldt revolutionised German natural science and established the basis of modern linguistics. The philosopher Friedrich von Schlegel (1772–1829) established comparative linguistics, whose success in untangling the history of the Indo-European languages was almost as sensational at the time as Darwin's evolutionism.

The common denominator for this milieu was a commitment to humanism. Some, with Goethe, sought a unified science, whose humanist outlook would provide an esthetic-emotional counterweight to 'hard' sciences such as physics. In this spirit, Schleiermacher expanded hermeneutics, an ancient method for interpreting texts, into a method for interpreting the world, equally applicable to the natural sciences as to philology.

The Germans were sceptical to evolutionism, with its universalistic claims and scientific pretensions. Here, even at its high point, the evolution was always seen through the lense of particularism of *cultural history*. This is clearly seen in the work of Adolf Bastian (1826–1905), known as the father of German anthropology. In 1851, Bastian, a polymath educated as a medical doctor, departed on his first great expedition to Central America, Eastern and Southern Asia, Australia and Africa. The eight-year journey confirmed Bastian's interest in ethnography, and on his return he published a famous three-volume treatise, *Der Mensch in der Geschichte* (1860, 'Man

in History'; see Koepping 1983), which promoted views on human psychology and cultural history that shared little common ground with the evolutionists, who studied a universal movement and ignored the concrete events of cultural history. Bastian completed nearly a dozen great expeditions, accumulating a quarter-century of travel that carried him several times around the world. Back home, he authored long, learned works, was appointed Professor of Ethnology at the University of Berlin, and became Director of the Imperial Museum, to which he contributed such rich collections from his own expeditions that a new museum, the famous Museum für Völkerkunde, was established in 1868 mainly to house them.

Bastian's travels taught him to respect the particularism of cultural diversity; nevertheless, he concluded, universalistically, that all cultures ultimately sprang from the same source. He opposed the idea of biologically distinct races and formulated the principle of the *psychic unity of mankind*, which, as we shall see, would influence anthropology profoundly. Bastian specified this general concept by positing that all humans share a limited number of elementary ideas – *Elementärgedanken*. In actual cultures these are influenced by local natural conditions and historical events, such as migrations – and by evolution, which, according to Bastian, proceeded differently in different culture areas. Thus, a few elementary ideas were differentiated into a vast number of 'folk ideas' (*Völkergedanken*), which in real cultures are woven into a unique *Kulturkomplex*. Through the 'culture complex' the folk's character may be glimpsed, its *Geist* ('spirit') – here Bastian's debt to Herder is evident.

So is his debt to the psychologist Wilhelm Wundt (1832–1920), at the University of Leipzig. Wundt, who is considered the father of experimental psychology, distinguished between individual psychological mechanisms (like sense perception) that could be investigated in laboratories with methods from the natural sciences, and more complex psychic functions that must be researched intuitively or by means of statistics. Most complex of all were processes of collective psychology. In his vast work, *Völkerpsychologie* (ten volumes; 1900–20), Wundt sought to reveal the underlying collective psychology of various cultures by means of comparative studies of rituals, myths and other culture traits.

Wundt's ethnopsychology remained incomplete; he never found the analytical means to penetrate beneath the surface of cultural expression. Yet, his attempt would have great repercussions. Prominent names in anthropology, such as Durkheim and Rivers (see

below), Mauss and Malinowski (Chapter 3) studied under Wundt or were inspired by him. Bastian also came under his spell and was led to search for formal differences between cultural expressions, and logical rules of transformation that might reveal how universal elementary ideas were transformed into local folk ideas. Like Wundt, Bastian died without bringing his work to completion.

Elements of evolutionism may be discerned in Bastian's thought, but it would be wrong to describe him as an evolutionist. He consolidated a tradition for studies of the cultural history of individual groups – as opposed to the abstract generalities of evolutionism – that would find a large following among German and American anthropologists, and elsewhere. In other respects as well, Bastian's influence was great. In Germany he was attacked and venerated by the next generation – the diffusionists (see below). One of his students, Franz Boas, would have a fundamental impact as the founder of American anthropology (Chapter 3). Bastian's principle of the 'psychic unity of mankind' was universally accepted in the discipline; when the British anthropologist Tylor appropriated it, he made it a basic tenet of British evolutionism. Bastian's concept of elementary ideas would gain a small but celebrated circle of admirers. It inspired the Swiss psychologist Carl Jung's theory of archetypes. In American anthropology it was developed by Edward Sapir and the ethnolinguists (Chapter 4). And in France, after the Second World War, when Claude Lévi-Strauss proposed a theory that made systematic sense of the transformations of cultural history, Bastian was an inspiration, though the deciding influence came from linguistics rather than psychology (Chapter 6).

TYLOR AND OTHER VICTORIANS

The year after the publication of *Der Mensch in der Geschichte*, the Scottish lawyer Henry Maine (1822–1888) published another seminal book, *Ancient Law* (1861). Maine here demonstrates how changes in legislation reflect wider social changes, and distinguished traditional societies whose legal systems are based on *status* from modern societies where they are based on *contract*. In status-based societies, rights are distributed through personal relationships, kinship and inherited rank. Contract society, in contrast, is based on formal, written principles which function independently of actual persons. The distinction between status and contract is still in use today, and many scholars have followed Maine's lead in

distinguishing two 'ideal types' – simple and complex societies – and have, naturally, been criticised for oversimplification.

Other evolutionist ideas that gained currency in the nineteenth century were later unanimously rejected. A prominent example was the theory of the original matriarchy, where women supposedly reigned, which was launched by the Swiss lawyer Johann Jakob Bachofen (1815–1887), in *Das Mutterrecht* (1861, 'Mother's Right'; see Bachofen 1968). Though no empirical basis for this theory has ever been found, it has remained so resilient that it needed to be demolished again as late as the 1970s, when it gained brief popularity among feminist anthropologists (Bamberger 1974).

A more central position in the discipline was occupied by Edward Burnett Tylor (1832–1917). Tylor grew up a Quaker, and was barred by his faith, ill health, and the early death of his parents from a university education. While recovering from tuberculosis in Central America, he discovered an interest in archaeology and was invited to take part in an expedition to the Toltec ruins in Mexico. In an era dominated by evolutionism, the step from prehistory to anthropology was short, and Tylor's work as an anthropologist would soon gain him (and the discipline) considerable prestige. In 1896, he was appointed the first British professor of anthropology, at the University of Oxford. In 1912, he was knighted. Tylor was still a young man when he published his first evolutionist synthesis, *Researches into the Early History of Mankind and the Development of Civilization* (1865); and his major work, *Primitive Culture* (1871), followed just a few years later. Tylor here proposed an evolutionary scheme reminiscent of Morgan's in *Ancient Society* (the two books were published in the same year). He shared Morgan's faith in the primacy of material conditions. Like Morgan, too, his knowledge of cultural variation was vast (Darwin refers to Tylor several times in his work on human evolution from the 1870s). But Tylor did not share Morgan's interest in kinship, and instead developed a theory of *cultural survivals*. Survivals were cultural traits that had lost their original functions in society, but continued in use, for no particular reason. Such traits were crucial to the effort to reconstruct human evolution. Tylor advocated a trait-by-trait comparative method, which allowed him to isolate survivals from the larger social system. Though influential at its time, the method was abandoned by the next generation of British anthropologists. Curiously, it reappeared in the mid-1970s, when the sociobiologist Edward O. Wilson, in an intellectual venture comparable to Tylor's, attempted to reconcile cultural variation and Darwinian evolutionism (Ingold 1986).

Tylor's most significant contribution to modern anthropology is his definition of *culture*. His much-quoted definition appears on the first page of *Primitive Culture*, and reads as follows:

> Culture, or civilization, taken in its broad, ethnographic sense, is that complex whole which includes knowledge, belief, art, morals, law, custom, and any other capabilities and habits acquired by man as a member of society. (Tylor 1871: 1)

Since Tylor's day, anthropologists have suggested many alternatives to this definition, but is seems we always fall back on its simple and all-embracing formulations. Neither Tylor's evolutionism nor his 'tolerantly paternalising ethnocentrism' (Stocking 1996: 10) finds expression in the definition, which is general enough to apply to any society, and neutral enough to pass as politically correct today. Where evolutionism distinguished societies on different stages of development, the definition of culture unites mankind: all societies have something in common – they are cultural. This is the point where Bastian's influence on Tylor is most evident. Tylor was the foremost proponent in the British tradition of Bastian's idea of the 'psychic unity of mankind', and his definition of culture is in fact more German than British. Paradoxically, however, it was most influential in American anthropology, while the American Morgan's focus on kinship gained a similar following in the UK. As late as in the 1970s, Meyer Fortes (Chapter 4) commented that American and British anthropology seem to have 'exchanged ancestors'. But as Stocking (1996: 13) points out, this is misleading. Tylor's definition had roots in German anthropology, and when it was accepted in the USA it was thanks to another German: Franz Boas (Chapter 3).

In the years between 1840 and 1880, as technological and economic change proceeded at unprecedented pace, a whole range of new problems was raised by sociologists and anthropologists. While Marx developed the first grand theory in sociology, comprising modernisation, value formation, power and ideology, and while Darwin formulated the principles of biological evolution, anthropologists were engaged in exploring the ramifications of evolutionism and cultural history, and in documenting the empirical variation cultures globally.

It was still uncommon for the anthropologist himself to carry out field studies, although Morgan and Bastian were prominent exceptions. A third exception was the Russian ethnographer Nicolay Nicolayevich Mikloukho-Maklay (1846–1888) who, in 1871, carried

out a 15-month intensive field study on the New Guinean coast, and laid the groundwork for a rich ethnographic tradition in Russia that goes unmentioned in most Western histories of the discipline (Plotkin and Howe 1985; Kuznetsov 2008). But the vast majority of anthropologists gathered their data through correspondence with colonial administrators, settlers, officers, missionaries and other 'whites' living in exotic places. Given the uneven quality of these data and the authors' vast theoretical ambitions, such studies tended toward the kind of speculation that Radcliffe-Brown (Chapter 3) would dismiss as *conjectural history*. Still, the learned books of the Victorians were theoretically focused and empirically grounded to an extent that had never been seen before.

The importance of kinship in this phase of the discipline's evolution cannot be overstated. Kinship terminology was a limited empirical field; nevertheless, mapping and understanding it was a humbling experience. The closer one looked at these strangely formal systems, the more complex they seemed. True, to the first practitioners of kinship studies, mostly lawyers by profession, the task seemed fairly simple. They looked for a 'legal system' that regulated behaviour in primitive societies, and kinship was the obvious candidate – an formalised empirical system of verbalised norms. At the end of the century, it was widely held that kinship was a kind of anthropologist's Rosetta Stone, that allowed primitive customs to be translated into scientific and comparative terms. The many legal scholars that were attracted to anthropology in this phase also made a lasting impression, and the idea of society as a juridical system governed by 'laws', 'contracts', 'norms', 'rules', and 'charters' remained, long after the demise of nineteenth-century evolutionism.

THE GOLDEN BOUGH AND THE TORRES EXPEDITION

For a couple of decades after the prolific 1860s and 1870s, little of importance was published in anthropology. In sociology too, there seems to have been a dearth – a notable exception being Ferdinand Tönnies' *Gemeinschaft und Gesellschaft* (1887; *Community and Society*, 1963), which proposed a dichotomy of the traditional and modern with more emotional and less juridical accent than Maine's. In the course of these years a new generation made its appearance and many of the leading figures discussed so far, including Marx, Morgan and Maine, were dead. The 'young lions' were waiting in the scenes for a cause to champion.

In anthropology, the first wave of institutionalisation had been completed in Germany and the UK, and similar developments were under way in France, the Netherlands and the USA. Independent national traditions were starting to crystallise, as different institutional and disciplinary concerns were raised in each country. In Germany, the diffusionists were establishing themselves, as a reaction set in against Bastian's broad programme of cultural history. In the USA and Britain, evolutionism remained the dominant paradigm, but researchers increasingly immersed themselves in empirically based studies of primitive kinship, religion, magic or law. In France, a new synthesis of sociology and anthropology was under way. Everywhere, however, there was an increasing realisation that the greatest obstacle to further development of the discipline was the lack of broad, detailed and reliable data. A general flight from lofty theories to field methods took place. The problem of data collection was not new. As early as in 1857, Tylor had taken the initiative to compile the first edition of what was to become the authoritative British work on field methods for nearly a century – *Notes and Queries on Anthropology*, which was reissued in four revised, and ever more detailed, editions. But the long-awaited methodological breakthrough only arrived after the First World War, when a radically new conception of anthropological fieldwork emerged.

The last great Victorian evolutionist was James George Frazer (1854–1941), a student of Tylor's who was celebrated far beyond anthropological circles for his masterpiece *The Golden Bough*, which was first published as a two-volume set in 1890, and later expanded to fill twelve tomes in its final edition, published in 1915. *The Golden Bough* is a comparative evolutionist investigation of myth, religion, beliefs and customs, with historical and contemporary examples drawn from all over the world. Like so many others, Frazer believed in a three-step model of cultural evolution: a 'magical' stage is replaced by a 'religious' stage which gives way to a 'scientific' stage. In his discussion of magic, Frazer follows a thread laid out by Tylor, who emphasised the need to identify patterns and universal traits even in 'obviously irrational' thinking. This part of his work had significant impact on later anthropology, mainly by way of Evans-Pritchard's classical monograph about witchcraft (Chapter 4). But his influence was greatest outside anthropology; three of his warmest admirers were the poet T.S. Eliot, the philosopher Ludwig Wittgenstein and the psychologist Sigmund Freud. Yet Frazer's fascinating and ponderous work was never followed up by later

research. It stands alone, a majestic monument to the towering ambitions and insecure empirical base of Victorian evolutionism.

More in tune with the times were the British participants in the Torres expedition, organised from the University of Cambridge in 1898 to the Torres Straits, between Australia and New Guinea. The expedition was to collect data about the traditional population of the islands in the area, and included, among others, the zoologist Alfred C. Haddon (1855–1940), later a pioneer in the use of anthropological film, the psychologist William H.R. Rivers (1864–1922) and the medical doctor Charles G. Seligman (1873–1940). All three would become notable anthropologists. In contrast to the individualistic anthropological fieldwork ideal that later became the norm, nineteenth-century expeditions were typically collective efforts with scholars from various disciplines responsible for various tasks. The Torres expedition was unique not in this, but in its high methodological standards and the excellent quality and impressive volume of the data collected. For these reasons, its participants are often referred to as the first true fieldworkers. 'Through their work', writes one commentator, 'British social anthropology was born' (Hynes 1999; see also Hart 1999).

Haddon planned the Torres expedition as an 'ideal' field project, where the participants would cover as broad a range of native life as possible: ethnography, psychology, linguistics, physical anthropology and musicology. He himself would take care of sociology and folklore, as well as material culture. Haddon would later become the dominant anthropologist at the University of Cambridge, where he defined the programme of the department where Frazer worked. For Seligman, who in later years became a central figure at the influential anthropology department at the London School of Economics, the expedition was the beginning of a career which would lead to several major field studies in the Sudan. He thus contributed to moving the regional focus of British anthropology from the Pacific Islands to Africa, which would prove to be an ethnographic goldmine. Seligman's major work from the Sudan, co-authored with his wife Brenda Seligman (Seligman and Seligman 1932), is still regarded as a classic in its field.

Rivers was perhaps the most intriguing member of the expedition. Until his early death in 1922, he was a professor at the University of Cambridge where he invested much effort in developing a psychological anthropology, a project inspired by Wundt and, like his efforts, too far ahead of its time to succeed. Towards the end of his life, Rivers, like his later student, Malinowski (Chapter 3),

came under the influence of Sigmund Freud's psychology. During the Torres expedition, Rivers concentrated on the mental abilities of the natives, especially their use of the senses. In 1908, he published a descriptive monograph, *The Todas*, based on fieldwork among a tribe in South India; and in 1914 *The History of Melanesian Society*, a comprehensive overview of the immense cultural variation of Melanesia, which he explained as a result of repeated waves of migration, an hypothesis that is still accepted, with modifications, by present-day archaeologists. Rivers found it difficult to reconcile the complexity of Melanesia with Tylor's evolutionism, and began to search for new theoretical approaches. His history of Melanesia represented a nearly unique move in Britain, away from evolutionism and towards the new German school of diffusionism.

Meanwhile, the institutional development of anthropology in Britain had stagnated. Fredrik Barth sums up a survey of the period from the Torres expedition to the publication of Malinowski's first monograph as follows: 'the formal institutional framework for the discipline of anthropology remained almost unchanged [from 1898 to 1922] – in other words,' Barth continues, with only slight exaggeration, 'such a framework continued to be absent' (Barth 2005: 20).

GERMAN DIFFUSIONISM

For many years, Germany was the intellectual powerhouse of anthropological research. Here were the learned cultural historians, the subtle theoreticians and the large, modern ethnographic museums – Bastian's Museum für Völkerkunde in Berlin was renowned as a model academic institution. As we have seen, linguistics played a prominent role in this milieu. Around 1800, Schlegel had demonstrated that many European and Asian languages could be traced back to the same, ancestral Indo-European root, while words were also loaned between languages and language families. With the close relations between linguistics and anthropology, it was inevitable that anthropologists would try to copy the prodigious success of the linguists, all the more so since linguistics offered a promising alternative to evolutionism. It was less unilinear and ethnocentric. It revealed the past as a complex web of influences and inheritances without overruling goals or universal stages. As evolutionism failed, the linguistic model gained ground in German anthropology and the diffusionist school emerged.

Diffusionists studied the geographical distribution and migration of cultural traits, and posited that cultures were to some extent random patchworks of traits with various origins and histories. Not all parts of a culture are necessarily linked into a larger whole. In contrast, most evolutionists held that societies were coherent, functional systems. True, evolutionists also recognised the existence of isolated, non-functional cultural traits (Tylor's *survivals*) and, in practice, these received a disproportionate amount of attention (considering they were atypical), since they were the key to reconstructing the social forms of the past. When the evolutionist perspective collapsed, the idea of societies as coherent wholes was also discredited (though it remained strong in sociology, and would soon reappear with renewed force in British and French anthropology). Now *all* cultural traits were potential 'survivals'. Like the evolutionists, the diffusionists used them to reconstruct the past, but their 'past' was no longer a unilinear movement of progress, but a tangle of long, crooked, often discontinuous stories of cultural encounters, migrations and mutual influences through thousands of years.

Technology and ideas have always travelled and people have been conscious of this. What was new about diffusionism, as it was practised in Germany around 1900, was its stringent and critical method. No effort was made toward grand theory; even Bastian was criticised as a crypto-evolutionist. Instead, explicit hypotheses about specific cases were advanced and discussed on the background of detailed and well-documented data. Like Rivers, many diffusionists worked in geographically limited regions, where it was possible to demonstrate convincingly that specific cultural traits had an identifiable history.

The heartland of classical diffusionism was in great museum cities such as Berlin, Vienna and Leipzig, from which it spread throughout Eastern and Northern Europe. In Britain and France (apart from individuals, like Rivers) it had little influence. As we have seen, German anthropologists at times worked within a more or less explicit evolutionary framework, but here diversity, which the evolutionists sought to reduce to a simple schema, was an analytical virtue. Scholars like Friedrich Ratzel (1844–1904), Fritz Graebner (1877–1934), Leo Frobenius (1873–1938) and the Austrian Wilhelm Schmidt (1868–1954) developed the most popular strand of diffusionism, the culture-circle (*Kulturkreis*) school. This had as its main hypothesis that all cultures had evolved, like language, from relatively few, prominent cultural centres (*Kulturkreise*), whose

influences overlie each other like geological or archaeological strata, and can be traced empirically. In certain cases, as in Graebner's studies from Oceania, as many as seven overlapping *Kulturkreise* were identified in the same society (*Kulturkomplex*).

In contrast to the evolutionists, the *Kulturkreis* scholars rested their research on a firm methodological base. This is clearly seen in Graebner's *Methode der Ethnologie* (1911, 'The Method of Ethnology'). The book was devastatingly reviewed by Franz Boas, who disapproved of the new, systematic diffusionism, and has never been translated into English. But here, if not elsewhere, Boas was clearly mistaken. Diffusionism discusses the mobility of individual cultural traits. Graebner deals with objections to this mode of enquiry, offers a clear definition of what a 'trait' is, and describes how it may be recognised as data by the researcher. He defines 'trait' in purely formal terms (the *form* of the ornament is constant; its meaning and usages vary), since form diffuses more easily than content. It follows that the best data are traits that have *no* function or meaning; the more arbitrary, the better the chances are that the trait comes from elsewhere.

As we shall see in the next chapter, both evolutionism and diffusionism were thoroughly thrashed by the next generations of anthropologists. But diffusionist research was more sophisticated than later anthropologists have generally cared to admit, and in the German-language area, particularly in Austria, the *Kulturkreis* programme retained its dominance into the 1950s.

From Germany diffusionism itself diffused, into Eastern and Northern Europe, along with German museum culture. In many cases, the scope of enquiry was restricted to local conditions, but expeditionary traditions were also maintained, most particularly in multi-ethnic Russia. Among Mikloukho-Maklay's many followers, three prominent names were Vladimir Ilich Jochelson (1855–1937), Vladimir Germanovich Bogoraz (1865–1936) and Lev Yacovlevich Sternberg (1861–1927), all of whom were exiled to Eastern Siberia by the Czar and used the opportunity to carry out long-term fieldwork among the indigenous peoples of the region. Around the turn of the century, they participated in a major Russo-American expedition to the indigenous peoples around the Bering Straits, organised by a German-American by the name of Franz Boas. These scholars were traditional cultural historians rather than modern diffusionists, but their schooling derived from Germany, where diffusionism was at its height, and diffusionism was part of their intellectual baggage. Later, these conditions were 'frozen' by the

communist revolutions and remained so for many years. Hence diffusionism remained a respectable, if somewhat cowed, theory in post-Stalinist Soviet Russia, with long traditions and high analytical and methodological standards. Even today, diffusionist concerns are by no means extinct, though 'modern' anthropological analysis is gaining territory (Chapter 9). In the West, a form of diffusionism reappeared in the tradition of *imperialism studies* that ultimately stems from Marx and Lenin, but has been resurrected under such headings as 'dependency studies', 'global system studies' and, most recently, 'globalisation studies' (Chapters 7 and 9). The Marxian influence adds power to the diffusionists' Herderian brew, with more potent and violent results.

THE NEW SOCIOLOGY

The new generation of British anthropologists who will be introduced in the following chapter, had good reason to distance themselves from evolutionism and diffusionism. They were convinced that they had discovered a theoretical alternative with greater potential than any previous theory of sociocultural variation. British anthropologists had discovered Continental sociology.

What is called 'classical sociology' in textbooks, usually refers to the *oeuvre* of a handful of (mostly German or French) theorists, who produced most of their work between the 1850s and the First World War. In the generation after Marx, these included Emile Durkheim and Max Weber, and also Georg Simmel (1858–1918) and Ferdinand Tönnies (1855–1936). Like Marx, these authors are still read for the intrinsic interest of their work (rather than as expressions of an historical *Zeitgeist*). Simmel (who has been experiencing a renaissance since the 1980s) is admired for his studies of modernity, the city and money. Both Durkheim and Weber are still important enough to generate book-length studies. But of all the classical sociologists, Durkheim was most significant for anthropology – in part because he himself was concerned with many anthropological themes and was instrumental in developing French anthropology, in part because of his influence on British anthropology through Radcliffe-Brown and his followers. In the USA, the influence of 'classical sociology' only made itself felt many years later, and was never as strong as in Europe. As with their German founder, Boas, the leading American anthropologists of the early twentieth century were oriented towards the humanist

disciplines of cultural history, linguistics and psychology rather than sociology.

DURKHEIM

Like Marx (and Boas), Emile Durkheim (1858–1917) came from a Jewish family. He grew up in a small town near Strasbourg, and his parents wanted him to become a rabbi. He did so well in school, however, that he was admitted to the prestigious École Normale Supérieure in Paris, which secured him a later academic career. During his education he had lost his religious faith and become part of a dynamic and critical intellectual milieu. He studied a year with Wundt in Leipzig, and Wundt's *Völkerpsychologie* would later influence his theory of collective representations (see below). Throughout his life, Durkheim was deeply concerned with moral issues, and he was a committed advocate of radical social and educational reform. In 1887, he was appointed lecturer in education and sociology at the University of Bordeaux, becoming the first French social scientist to hold an academic position. While in Bordeaux, where he stayed until 1902, when he moved to Paris, Durkheim wrote two of his most important works, *De la division du travail social* (1893; *The Division of Labour in Society*, 1964) and *Le Suicide* (1897; *Suicide*, 1951). He also founded the influential journal *L'Année Sociologique*, which he continued to edit after moving to Paris. As professor at the Sorbonne from 1906 till his death in 1917, Durkheim's influence on French sociology and anthropology was immense. With his nephew and intellectual successor Marcel Mauss, he wrote extensively on non-European peoples; a notable work in this regard was their co-written *Classification Primitive* (Durkheim and Mauss 1900; *Primitive Classification*, 1963), a study of the social origins of knowledge systems, which draws on ethnographic data, particularly from Australia. This book, which posits an intrinsic connection between classification and social structure, would become an important source of inspiration for anthropological studies of classification.

Unlike both diffusionists and evolutionists, Durkheim was not particularly interested in origins. He was concerned with *synchronic* (atemporal) rather than *diachronic* (temporal) explanation. Like the diffusionists, but unlike the evolutionists, he was deeply committed to an anthropology based on observable, often quantifiable data. Unlike the diffusionists, however, he was convinced that societies were logical, integrated systems, where all parts were dependent

on each other and worked together to maintain the whole. In this, he approached the evolutionists, who, like him, drew analogies between the functional systems of the body and society. Durkheim often described society as a *social organism*. Like Tönnies and Maine, but unlike Marx and Morgan, Durkheim subscribed to a dichotomous division of societal types – dropping all talk of 'stages' and 'evolution', he juxtaposed traditional and modern societies without postulating that the former would ever evolve into the latter. Primitive societies were neither 'survivals' from a dim past nor 'steps' along the path of progress, but social organisms that deserved to be studied on their own terms. But unlike Bastian and the *Völkerkunde* school, Durkheim was concerned, not with culture, but with society; not with symbols and myths, but with organisations and institutions, and, most fundamentally, with the force that held social groups together. He referred to this force as *solidarity*.

The book on the division of labour discusses the differences between simple and complex forms of social organisation. The former are based on *mechanical solidarity*. People support the existing social order and cohere into groups because they share the same everyday life, carry out the same tasks and experience each other as similar. In complex social organisations, *organic solidarity* prevails. Here, groups are held together by the fact that people are different and fulfil different, but complementary tasks, each of which contributes to the whole. Durkheim emphasises that the two forms of solidarity must be understood, not as different types of society, but as general principles of social integration that coexist in all societies. In *Suicide*, he considers what happens when solidarity breaks down. The world is emptied of moral content, the individual is disconnected from others and cast into a homeless social vacuum without norms (*anomie*). Marx had described a similar state, which he called alienation and traced back to the individual's loss of control over the products of his work. But where Marx sees alienation from the physical world, Durkheim sees alienation from the moral collective.

Durkheim's last, and most anthropological work, *Les Formes élémentaires de la vie réligieuse* (1912; *The Elementary Forms of Religious Life*, 1995) was published just five years before his death. Here, he grapples with the meaning of 'solidarity' itself, of the very force that keeps society together. Solidarity, Durkheim argues, arises from *collective representations*. These are symbolic 'images' or 'models' that are shared by a group and embody the group's shared experience. Such 'images' develop through interpersonal

relationships, but attain a supra-individual collective character. They make up an all-embracing reality, which to the members of society appear just as real as the material world. Hence, collective representations are subjective, they have power over the emotions. But they also have an inescapable, objective character. This duality alerts us to the fact that Durkheim is describing a 'socially constructed reality', as in Kant and Hegel. Religion becomes an important object of enquiry for Durkheim, because it is here, more than anywhere, that the collective representations are strengthened and made visible. This happens primarily in *ritual*, where religion is expressed through physical interaction, and the collective representations, with the solidarity they promote, are transformed into direct, bodily experience. Ritual hedges itself off from *profane* daily life, drawing a protective magic circle around its own, forbidden, *sacred* domain. This demarcation allows the experience of ritual to be intensified until an almost mystical union is achieved. When the memory of this experience returns to us in everyday life, it brings home to us that the world truly is as the collective representations describe it.

Religion had long attracted the attention of anthropologists, who had documented it in a wide range of empirical forms. The problem of understanding social integration in stateless societies had been an important (though often implicit) concern in evolutionism. Bewilderment at exotic symbols and customs was the original point of departure of anthropological enquiry. Now Durkheim seemed to offer an analytical tool that would bring all these interests together. The exotic could be understood as a system of collective representations that generated social solidarity. And religion, the most mystifyingly 'exotic' phenomenon of all, was the rational dynamo driving this process.

Although the first modern British anthropologists (Chapter 3) embraced Durkheim's theory enthusiastically and found countless applications for it, in the study of religion, law and – not least – kinship, Durkheim was not alone in reforming sociology around the turn of the century. But since Durkheim's influence was so strong, it would take many years before anthropologists started taking these alternatives seriously, just as it took time for them to face up to the implications of the work of Marx.

WEBER

Max Weber (1864–1920) grew up in a prosperous and authoritarian Prussian family, was educated at the universities of Berlin, Heidelberg

and Göttingen, and rose rapidly in the German academic world. He was appointed professor at the age of 31, and in the course of a few years, published several learned works about topics as diverse as the fall of the Roman Empire and agricultural problems in contemporary eastern Germany. From his mother, who was raised in a strict, Calvinist home, he had inherited ideals of asceticism and strict work discipline, which he put into practice in his academic life. In 1898, after only three active years, he suffered a mental breakdown and was able to resume work only after another five years had passed. Immediately after his recovery, Weber wrote the book that many consider his finest: *Die protestantische Ethik und der 'Geist' der Kapitalismus* (1904–05; *The Protestant Ethic and the Spirit of Capitalism*, 1976). This is a work of economic and cultural history which explores the roots of European modernity. Weber argues that the Calvinists (and other puritan Christians of the sixteenth and seventeenth centuries) formulated a view of life that corresponded closely to the image of the perfect capitalist. The Calvinists believed that human life was predestined, that certain individuals were singled out by God for salvation, but it was beyond human comprehension to understand who or why this would be. The God of Calvin was stern and demanded obedience, but refused to explain his reasons. According to Weber (and we sense that he is speaking from personal experience), this ambiguity, coupled to the merciless doctrine, created an unbearable tension in the life of the Calvinists. Looking for solutions to their self-imposed dilemma, it occurred to them that hard work coupled with a frugal lifestyle could only bring them closer to God's grace. Economic success, if they attained it, would be proof of this. However, the fruits of their labour were not to be enjoyed. Instead, they should be reinvested back into the enterprise, to generate a spiral of increasing profits to the glory of God. Calvinism gave people a legitimate reason to not spend the surplus of their work on personal consumption, but reinvest it – and reinvestment is, as Marx pointed out, the prime condition of a capitalist economy.

Weber's point is not that Calvinism was the *cause* of capitalism. There were many reasons why capitalism arose, and reinvestment was by no means an invention of Calvin's. The point was rather that the Protestant sects formulated an explicit ideology – a 'spirit' (*Geist*) – that justified and even glorified the capitalist ethic.

Weber's work was deeply influenced by hermeneutics, which since Schleiermacher's day had become an integral part of a well-rounded

education in the humanistic German academic milieus. Weber sought to interpret human action and describe the motivations behind it. In this sense he was an early representative of what has later been referred to as *methodological individualism*. This did not mean that he focused on individuals to the exclusion of the social whole. Hermeneutics teaches that human attention constantly shifts between part and whole, and Weber's individuals always participate in wider social contexts – in states and institutions, and in the long-term movements of history. It is the hope of *interpreting* the motivations of action that is expressed in the hermeneutic concept of *Verstehen* ('understanding, interpretation'). Weber offered an interpretive and empathic sociology that put itself in the place of actors and immersed itself in the choices they must make, given the sociohistorical conditions surrounding them. After the Second World War, Weber's focus on the individual and his interest in interpretation would inspire anthropologists as different as Fredrik Barth (Chapter 5) and Clifford Geertz (Chapter 6).

The empirical phenomenon that Weber himself was most interested in interpreting, was power. Power was an old theme in the social sciences. The classic was Machiavelli's *Il Principe* (1532; *The Prince*, 2008), which described the logic of power-holders with acute realism. Later, Marx established the material and bodily basis of power, and in Weber's lifetime, the philosopher Friedrich Nietzsche had described power as an expression of the will to live. Weber had read these authors, and was most closely allied to Nietzsche. Marx had linked power to control of the means of production, and thus with property. Weber instead saw power as rooted in the individual. Like Marx – and unlike Durkheim – Weber saw social life as full of conflict and in constant change. But Marx assumes change is caused by anonymous structural conflicts taking place in the deepest, unknowable regions of society, behind peoples' backs, so to speak. This was unacceptable for Weber, whose interest was in individuals pursuing values and striving for goals. Like his contemporaries, the diffusionists, Weber was opposed to abstract generalisations, such as Marx's structural concept of power. It was the particular, the historical coincidence, the concrete, lived reality, that attracted his attention. Weber could see well enough that power and property were often related, but he refused to generalise. Power, he asserted, was the ability to make someone do something he otherwise would not have done. Property is just one means by which it may be exerted. Weber

then goes on to describe, in his second great work, *Wirtschaft und Gesellschaft* (1922; *Economy and Society*, 1968), three *ideal types* of legitimate power. (The Weberian 'ideal type' is a simplified model that is applied to the real world to reveal specific aspects of its functioning.) Weber's ideal types of legitimate power are *traditional authority* (legitimised by ritual and kinship), *bureaucratic authority* (legitimised by formal administration), and *charismatic authority* (the power of the prophet or revolutionary). All three types may coexist within a single society. Now, the first two types look suspiciously like the primitive/modern dichotomy proposed by Maine, Tönnies or Durkheim. But the third type is an innovation. It bears witness to the fact that Weber, towards the end of his life, had read Freud, who, like Nietzsche, emphasised the irrationality of the human psyche. There exists a kind of power, Weber tells us, that is unpredictable and individual, driven by intuition and the command of loyalty, rather than on control of property (Marx) or stable norms (Durkheim).

Thus, for Weber, society was a more individual and less collective endeavour than for Marx or Durkheim. Society is not, as in Durkheim, a stable moral order. Nor is it, as in Marx, a product of ponderous collective forces that individuals can neither influence nor understand. It is an ad hoc ordering, generated as people with different interests and values meet, quarrel, and strive (if need be by force) to convince each other and arrive at a course of action. Thus, while Marx and Durkheim each developed a distinct brand of *methodological collectivism*, which studies society primarily as an integrated whole – Weber announced a *methodological individualism* that accepted that societies could be inconsistent and unpredictable.

By the turn of the twentieth century, the Continental sociologists were engaged in a lively discourse on issues of social theory, attaining levels of sophistication that anthropologists could not pretend to. In our own day, Marx, Durkheim and Weber are far more frequently cited by anthropologists than Morgan, Bastian or Tylor. Soon, the impact of Durkheim would shake anthropology deeply, while Weber and Marx still lurked in the scenes, only appearing as major influences after the Second World War.

Still, the heritage of nineteenth-century anthropology is richer than often supposed. Evolutionism never disappeared completely, and has had influential twentieth-century proponents. Diffusionism, as we hinted above, is perhaps still a force to be reckoned with. Many

concepts have survived and are still in use: Morgan's classificatory kinship, Maine's contract and status, Tylor's definition of culture, Bastian's psychic unity of mankind, are all 'survivals' (to use a native term) of Victorian anthropology. It is nevertheless only with the developments described in the next chapter that anthropology emerges in the form we recognise today.

3
Four Founding Fathers

The peaceful conditions that had prevailed in the West since the Napoleonic Wars, the steady advance of democracy and culture, the growing colonial empires, the dynamic economy and the scientific breakthroughs, had done their part to make ideologies of unilinear progress seem plausible, if not inevitable. A mere glance at the world seemed sufficient to confirm evolutionism in this age, which is called Victorian, after the long-lived British monarch. As Keith Hart puts it:

> The question Victorians asked was how they were able to conquer the planet with so little effective resistance. They concluded that their culture was superior, being based on reason rather than superstition … (Hart 1998)

By the turn of the twentieth century, optimism was more subdued. Freud's theory of the unconscious from 1900 and Einstein's theory of special relativity from 1905 frame a symbolic gateway to our time. These theories challenged the very substance upon which the bourgeois, Victorian age was built. In Freud's hands, the rational, free individual, the means and end of progress, dissolved into irrational drives and explosive sexuality; Einstein dissolved physics, the most prestigiously rational of the sciences, into dithering, explosive movement.

The phone, the car and the plane replaced the telegraph, train and steamboat. The colonial empires still grew, few things indicated that they would soon be supplanted by the postcolonial world (dis) order that shapes our lives today. The twentieth century transformed the world and the people in it. And as cannons roared over the trenches of Flanders, and the masses stormed the Winter Palace, and the Model T Fords rolled from the conveyor belts of Detroit; while the world economy collapsed, and Hitler and Stalin seized power, and Western women gained voting rights, and Europe moved toward a new war, and jazz became popular in Germany – while

these and so many other events transpired, anthropology was born as a modern social science.

At the turn of the twentieth century, the state of anthropology may be described as follows: A number of ambitious theories of long-term social change had been advanced, based on historical sources or contemporary reports, or (more rarely) on first-hand observation. Enthusiasm for these theories was in decline, as a pressing need for richer and more precise empirical data made itself felt. Meanwhile, sociologists had proposed a range of sophisticated social theories with clear implications for anthropological thinking – without anthropologists taking notice. Not only Marx, Durkheim and Weber were ignored, but the entire sociological field (names such as de Tocqueville, Simmel, Tönnies and Veblen come to mind). Anthropologists remained largely indifferent even to major achievements in science and philosophy. Darwinian evolution, Freudian dream analysis, Saussure's semiotics, Nietzsche's will to power and Husserl's phenomenology had yet to be impressed on the 'blank slate' of anthropology. All would be so before the end of new century.

The discipline of anthropology as we know it today developed in the years around the First World War, and we will uncontroversially describe its growth by focusing on four outstanding individuals – two in Britain, one in the USA and one in France. There were other national traditions, from Russia and Japan to the Netherlands and Brazil, and other scholars in the metropolitan countries who may have seemed as important at the time. It is only in hindsight we can assess the historical importance of past events; their contemporary significance may have been otherwise. And there is the flip-side of the coin: How many of Mauss' colleagues died in the First World War? How many of Boas' books were burned by the Nazis? How many ethnographers were silenced or killed in the Gulag? Much might have been different in anthropology, if not for the killing fields of the twentieth century.

The men whose work will form the backbone of this chapter were Franz Boas, Bronislaw Malinowski, Alfred Reginald Radcliffe-Brown and Marcel Mauss. Between them, they effected a near-total renovation of three of the four national traditions we have focused on so far – the American (led by Boas), the French (by Mauss) and the British (by Malinowski and Radcliffe-Brown). In German anthropology, little changed; cultural history and diffusionism were left unchallenged to the point of stagnation. However, Boas was a German and Malinowski a Pole, and both brought something of the

German tradition with them when they emigrated to the USA (Boas) and Britain (Malinowski). Thus, there is reason to argue that the four national traditions that arose in the nineteenth century have all contributed to the making of twentieth-century anthropology. Nor were these traditions alone. Since the end of the nineteenth cenntury, institutionalised anthropology had been spreading through Europe and beyond, with embryonic scholarly institutions appearing as far afield as Helsinki, Zurich and Belgrade, New Delhi and Tokyo, and more were soon to come.

The four founding fathers did not perceive themselves as a single movement and had no common programme. Both theoretically and methodologically the differences between them, and between the schools they founded, were great, and it was only after the Second World War that French, American and British anthropologists started looking for a common language. Still, all of the founders considered themselves anthropologists. There were many reasons for this. A professional anthropological identity had been established, linked with academic institutions that disseminated anthropological knowledge, conducted anthropological research and were acquainted with each other's work. Another reason for a common identity was that the traditions were in no way hermetically closed. Durkheim's influence on British anthropology is a famous case, but views were exchanged freely in letters and journals as the national traditions defined themselves and challenged each other. Before 1910, Rivers, in Britain, could conduct a lively debate with the American Kroeber on the use of psychological and sociological models in anthropology. There was also the shared background in nineteenth-century anthropology. Loud consensus held that evolutionism had been a dead end, but it was also silently acknowledged that evolutionists and cultural historians had laid the foundations on which the modern discipline rested.

The transition happened differently in the three countries. In Britain, there was a radical break. Radcliffe-Brown and Malinowski proclaimed an intellectual revolution and debunked their teachers mercilessly. In the USA and France, continuity was greater. In the USA, Boas remained a father-figure throughout the transitional period. In France, there was even less to-do: Mauss continued his uncle Durkheim's work with only slight differences in emphasis. In Britain it is sometimes claimed that Radcliffe-Brown and Malinowski single-handedly created modern anthropology, and there is some truth to this. But the story is also a myth, created in the heyday of British social anthropology in the mid-twentieth century, when it

seemed that Malinowski had created a method and Radcliffe-Brown a theory that together defined the project of anthropology.

THE FOUNDING FATHERS AND THEIR PROJECTS

In 1886, the oldest of the 'fathers', Franz Boas (1858–1942), found himself in New York. He was 28 and on his way home to Germany. He held a doctorate from Kiel, an academic position in Berlin, he had spent a year alone with the Baffin Land Inuit and participated in several ethnographic expeditions to northern and western Canada. Still, he chose to stay in New York, maybe because of his Jewish background; certainly in part because it brought him closer to the Native American peoples that fascinated him. He worked first as the editor of a scientific journal, later at a small university, and in 1899 was appointed professor at the prestigious Columbia University in New York City, where he remained until his death in 1942. During the intervening 43 years, he was teacher and mentor to several generations of American anthropologists.

Boas' message to his students was simple, but fundamental. He had been trained by German scholars who were critical to evolutionism, and remained until his death a kind of diffusionist, though Graebner's *Kulturkreis* theory was unacceptable to him (Boas 1911) – he claimed that Graebner underestimated human creativity. Boas was convinced that culture was extremely complex and that social theories were dependent on a firm and reliable empirical base, rather than on neat armchair theories that reduced reality to schemata. The first task of anthropology was to provide the data. *Then* one could discuss theory. Boas' respect for empirical facts was related to his *cultural relativism*. Each people, each nation, each tribe had its fate, its irreplaceable character, and it was the task of the anthropologist, as Boas saw it, to document and defend it (Boas was a prominent anti-racist). In this he was a loyal follower of Herder and Bastian.

Twenty-four years after Boas made his decision to stay in the USA, a young Polish intellectual from an upper-middle-class academic family in the venerable town of Kraków, Bronislaw Kasper Malinowski (1884–1942), moved from Leipzig to London. A few years before, he had defended a highly regarded doctorate in physics and philosophy in Krakow, then part of Austria-Hungary (now in Poland). In Leipzig, he had (like Bastian, Durkheim and Rivers before him) studied under Wundt, who convinced him that society had to be understood *holistically*, as a unity of integrated parts, and

that this was best achieved by synchronous (rather than historical) analysis. Meanwhile, his reading of *The Golden Bough* had inspired him to investigate 'primitive' peoples, and he moved to study under Seligman at the London School of Economics (LSE), which had a reputation for offering good conditions for exotic fieldwork. Four years later, Malinowski did a six-month field study on an island off the coast of New Guinea, which he regarded as a failure. After a short stay in Australia, spent reflecting on his methods, he returned to the same region, this time to the Trobriand Islands, where he spent nearly two years. After the war, he returned to Europe to write *Argonauts of the Western Pacific* (Malinowski 1922), possibly the single most revolutionary work in the history of anthropology. He attracted a small group of accomplished and enthusiastic students to the LSE, who would make fieldwork à la Malinowski anthropology's trademark. Malinowski died in the USA, in the middle of a study of social change among Native American peasants in Mexico.

Alfred Reginald Radcliffe-Brown (1881–1955) was Malinowski's age, but came not from Central European intelligentsia, but from the English lower middle class. He began his career as plain A.R. Brown. With economic support from his brother, who had made a career in the army, he embarked on medical studies, but was encouraged to move to Cambridge and study anthropology by his teachers – Haddon and Rivers – who had both participated in the Torres expedition. Radcliffe-Brown did fieldwork in 1906–08 on the Andaman Islands in the Bay of Bengal, and published a well-received field report in the diffusionist style. Just before Malinowski arrived at the LSE, Radcliffe-Brown read an early version of Durkheim's masterpiece, *The Elementary Forms of Religious Life*, and held a long lecture series on Durkheim at Oxford, and when his monograph, *Andaman Islanders*, was finally published (in 1922, the same year as *Argonauts*), it appeared more than anything else to be a brilliant demonstration of Durkheimian sociology applied to ethnographic material.

Radcliffe-Brown's later career was, like Boas' and Malinowski's, devoted to institution building. Unlike them, however, he spent long periods of his professional life as an academic nomad. He spent many years in Cape Town and Sidney, laying the groundwork for South African and Australian social anthropology, and his six years in Chicago influenced American anthropology profoundly. When he finally returned to Oxford to accept a Chair in Social Anthropology in 1937, it was as a celebrated exile, rather than an outsider. When Malinowski left for the USA a year later, Radcliffe-Brown quickly

gathered the reins in his hands and became the leading figure in anthropology in Britain.

In 1917, while Malinowski was still in the Trobriand Islands, while Boas was busy educating his students in New York, while Radcliffe-Brown was serving a short stint as director of education in the Polynesian kingdom of Tonga – Durkheim died. His nephew, Marcel Mauss (1872–1950), who had been Professor of Primitive Religion at the École Pratique des Hautes Études in Paris since 1902, had cooperated with Durkheim for two decades, and now took his place as leader of the *Année Sociologique* circle. It was not the easiest of times. Several outstanding colleagues of Mauss had perished in the First World War, and he would spend much of his professional energy completing and publishing their manuscripts. In 1925, he founded – with the anthropologists Paul Rivet and Lucien Lévy-Bruhl – the Institute d'etnologie at the University of Paris, which for the first time in France, offered and anthropological education. During the same period he published his most influential work, *Essay sur le don* (1923–24; *The Gift*, 1954). Mauss had his background in classical studies and comparative philology. He never did fieldwork, but his knowledge of global cultural history and contemporary ethnography was comprehensive. In his work, which is largely comparative, he cites Boas, Malinowski and Radcliffe-Brown along with a plethora of other authorities, many of them learned Germans of the cultural history tradition. Like the other 'founding fathers', Mauss was an institution builder, and the academic community he built seems to have mattered more to him than his personal career. He published little during his long lifetime, but he posed deep questions, cooperated freely, and bequeathed a rich heritage to his many students.

We have pointed out above that the four founding fathers had no common programme. However, they agreed on certain basics. They believed that anthropology was an holistic discipline. The idea of comparing individual culture traits, considered out of context, that was so crucial to evolutionism and diffusionism, was unacceptable. They agreed that anthropology must become a true science, where hypotheses were tested and theories put forth on the background of detailed, well-documented data. Natural science was to some extent a model, but linguistics and Wundtian psychology were closer to home. And they agreed that anthropological methods must be revamped. Whether fieldwork was carried out individually, in the British-Malinowskian style, or by teams, as was common elsewhere,

techniques of observation, interviewing and documentation must be critically reviewed and systematically applied.

Here the consensus ended. There was broad disagreement on how to realise these laudable goals in practice, and there were plenty of conflicts, on theoretical, methodological, institutional and personal issues. But here again the differences were great: the rivalry between Malinowski and Radcliffe-Brown is as legendary as the harmony that reigned under the benign dictatorship of Boas, though history has no doubt exaggerated both.

MALINOWSKI AMONG THE TROBRIAND ISLANDERS

Malinowski came to Oceania as secretary for the Torres expedition, just before the outbreak of the First World War. It has been claimed that he was virtually interned on the Trobriand Islands, since, as a citizen of Austria-Hungary, he was technically an enemy of Britain. He could have returned to Europe any time he liked, but he had a job to do (Kuper 1996: 12). One might gain the impression at times that Malinowski was a flighty romantic who 'just happened' to discover the principles of modern fieldwork. This was very far from the truth. He was a thorough and systematic ethnographer, with an unusual capacity for acquiring languages and outstanding faculties of observation (Firth 1957).

For Malinowski, it was essential to stay long enough in the field to be able to use the local vernacular as one's working language. Only thus could one gain direct access to the culture. Interpreters cut you off from the informal aspects of communication, in the worst case giving access only to information the interpreter consciously decides to give. Most anthropologists before Malinowski had been in this position.

Malinowski's 'participant observation' set a new standard for ethnographic research. No fact was too trivial to be recorded. Formal interviews and social aloofness would no longer do. As far as practically possible, the ethnographer must take active part in the ongoing flow of everyday life, avoiding specific questions that might divert the stream of events, and without restricting attention to particular parts of the scene. This was hard work: Malinowski lived alone in a hut in the middle of a Trobriand village for months on end – though he kept his tropical suit and hat immaculately white, and though his posthumously published diaries (Malinowski 1967) reveal that he often felt homesick, despondent and sick and tired

of 'the natives'. However, Malinowski was too good a fieldworker to restrict himself to unstructured methods. He collected accurate data on yam yields, land rights, gift exchange, trade patterns and political conflicts, and many other things, and carried out structured interviews whenever necessary.

What he did not do to any significant extent was to position the Trobriand Islanders within a wider historical and regional context. In this, he stands in striking contrast to Mauss, who was a specialist on the Pacific, with a broader and deeper knowledge of the region's cultural history than Malinowski, but had never actually been there.

Virtually everything Malinowski later published drew on his Trobriand data. He wrote about economics and trade, marriage and sex, magic and worldviews, politics and power, human needs and social structure, gardening and aesthetics. His descriptions run over several thousand pages, and demonstrate conclusively the potential of long-term intensive fieldwork. The sheer number of Trobriand institutions, beliefs and practices, showed beyond doubt that a 'primitive', 'simple' society, near the bottom of the evolutionist ladder, could be a highly complex and multifaceted universe in itself. Malinowski's work revealed, more convincingly than any theoretical argument, the absurdity of comparing individual traits. From now on, context and interconnection would be essential qualities of any anthropological account.

Argonauts of the Pacific, Malinowski's first major work, remains his most famous. The book was prefaced by Sir James Frazer, who lavished praise on the young Pole, clearly unaware that he, in an academic sense, was signing his own death warrant. It is a long book, but fluently written. Following a brilliant introductory chapter which outlines the aims and methods of social anthropology (or ethnology, as it was still often called), the author leads us through a tightly focused and extremely detailed examination of a single institution among the Trobriand Islanders, namely the *kula* trade, where symbolic valuables circulate over a large area between the islands of Melanesia. Malinowski describes the planning of expeditions, the routes followed, the rites and practices associated with them, and traces the connections between the *kula* trade and other Trobriand institutions, such as political leadership, domestic economics, kinship and rank. A younger contemporary and countryman of the novelist Joseph Conrad, Malinowski brought home news from 'the heart of darkness', in the form of nuanced and naturalistic images of the Trobriand Islanders, who in the end

emerge as neither spectacular, exotic nor 'radically different' from Westerners, but simply as distinctive. Never have Herder's ideals been more convincingly realised.

Malinowski's theoretical views were less enthusiastically received than his methods and ethnography. He referred to his theoretical stance as *functionalism*, though in fact he was rather eclectic. Social practices and institutions were functional in the sense that they fit together in a functioning whole, which they contributed to maintaining. But unlike the structural-functionalists who followed Durkheim, Malinowski saw individuals, not society, as the system's ultimate goal. Institutions existed for people, and it was their needs, ultimately their biological and psychological needs, that were the prime mover of social stability and change. This was another expression of methodological individualism, and in a collectivist academic climate dominated by Durkheimians, it was not favourably received. For a few decades after his death, Malinowski's star continued to fade, until disillusionment with 'Grand Theory' set in during the 1970s, leading to his rehabilitation on both sides of the Atlantic. Malinowski's attention to detail and the native's point of view may seem to exclude high-flying theories, and we note the similarity to Boas here, a sign of their common Germanic training. Malinowski differed from Boas, however, in his reluctance to engage in historical reconstruction. With Radcliffe-Brown he waged an anti-evolutionary – and anti-historical – campaign that was so successful that historical perspectives was more or less banned from British anthropology for half a century.

Malinowski called himself a functionalist, but his views differed greatly from those of the rival programme of structural-functionalism. For the Durkheimian structural-functionalists, the individual was an epiphenomenon of society and of little intrinsic interest – what mattered was to elicit the elements of social structure. These two lineages of British social anthropology – biopsychological functionalism and sociological structural-functionalism, whose roots may be traced to the German and British traditions – highlight a basic tension in the discipline, between what has later been referred to as *agency* and *structure*. The individual has agency in the sense that he or she is a creator of society. Society imposes structure on the individual and limits his or her options. The two viewpoints are, as Giddens (1979) points out, complementary. But in interwar British anthropology this was not seen. Malinowski and Radcliffe-Brown were considered diametrical opposites.

RADCLIFFE-BROWN AND THE 'NATURAL SCIENCE OF SOCIETY'

Where Malinowski, following Weber and Nietzsche, saw human motives and the logic of action, Radcliffe-Brown, following Durkheim, saw abstract mechanisms that integrated society. The 'mechanisms' that Radcliffe-Brown tried to identify were of Durkheimian origin, akin, perhaps, to collective representations, and thus to Wundt's ethnopsychology, whose influence, paradoxically, seems underlie both his and Malinowski's projects. But Radcliffe-Brown had an explicit hope of transforming anthropology into a 'real' science modelled on the natural sciences, which neither Durkheim nor Wundt shared. Here, in the name of science, we see Radcliffe-Brown resisting the German influence in Durkheim, as he resisted it in Malinowski. In *A Natural Science of Society*, his last book, based on a lecture series held in Chicago in 1937 and posthumously published in 1957, he indicates the tenor of his hope. Society is bound together by a *structure* of juridical rules, social statuses and moral norms, which circumscribe and regulate behaviour. Social structure exists, in Radcliffe-Brown's work, independently of the individual actors who reproduce it. Actual persons and their relationships are mere instantiations of the structure, and the ultimate goal of the anthropologist is to discover its governing *principles* (which, we remember, are kin to collective representations), beneath the veneer of empirically existing situations. This formal model, with its units clearly defined and logically related, clearly demonstrates the master's 'scientific' intent.

Social structures can further be partitioned into discrete institutions or subsystems, such as systems for distribution and inheritance of land, for conflict resolution, for socialisation, for division of labour in the family, and so on – all of which contribute to the maintenance of the social structure as a whole. This, according to Radcliffe-Brown, is their *function* and the cause of their existence. At this point, we have a problem. Radcliffe-Brown seems to claim that institutions exist *because* they maintain the social whole; that is, their function is also their cause. Such 'tautological' or 'backward' reasoning is generally frowned on in scientific explanations.

Such problems might have worried the structural-functionalists, anxious as they were to be regarded as proper scientists, but they did not. Radcliffe-Brown's linkage between Durkheimian social theory and ethnographic material, and his ambitions on behalf of the discipline, generated an attractive new research programme, to which talented young men and women flocked, which in turn

increased the theory's prestige. But what tipped the scales decisively in the new school's favour was the potential Radcliffe-Brown saw for Durkheimian analysis of kinship. Since Morgan, anthropologists had been aware that kinship was a key to the understanding of social organisation in small-scale societies. However, it was still unclear just what this key unlocked. Radcliffe-Brown's Durkheimian use of Maine's old idea of kinship as a juridical system of norms and rules made it possible to exploit the analytical potential of kinship to the hilt. A kinship system was easily understood as an unwritten constitution for social interaction, a set of rules for the distribution of rights and duties. Kinship, in other words, was once again becoming a key institution, this time as the engine (or heart, to use the biological analogies favoured by Durkheim) of a self-sustaining, organically integrated yet abstract entity called *social structure*.

With key in hand, structural-functionalists set out to study primitive societies: politics, economics, religion, ecological adaptation, and so on. Durkheimian kinship seemed to unlock it all. Kinship was a framework for the creation of groups or *corporations* in pre-state societies. The groups might have collective rights, for example, to land or animals. They might demand loyalty in case of war. They might settle disputes or organise marriages. It was these kin groups and their dynamics that the structural-functionalists studied, not their 'culture'. Radcliffe-Brown was not fond of the word 'culture'. The central issue was not what the natives were thinking, what they believed in, how they made their living or how they had become who they were, but how their society was integrated, what 'mechanisms' bound it together as a whole.

Radcliffe-Brown's critique of the 'conjectural history' of the evolutionists was harsh. In his view, contemporary arrangements existed because they were functional today, certainly not as 'survivals' of a bygone era. They made sense in the present or not at all. He was also scornful of the sometimes fanciful reconstructions engaged in by cultural historians and diffusionists. Where no evidence existed, there was no reason to speculate. Here, Malinowski and Radcliffe-Brown were in perfect agreement.

As a fieldworker, Radcliffe-Brown was less than notable. As he himself admitted, his Andaman material was inadequate. He failed to learn the local language and spent the bulk of his fieldwork in the town of Port Blair rather than in the forest with the Andamanese. His later fieldwork in Australia is linked to Daisy Bates, an eccentric Englishwoman who had lived for years among aborigines and had

extensive knowledge of them. Though it is unlikely that Radcliffe-Brown actually stole data from her, it is probable that her aid greatly facilitated his work (White 1993).

Malinowski and Radcliffe-Brown founded two 'lineages' in British anthropology, which were in some respects in competition, in others, complementary. With the consolidation of these schools in the 1930s, British social anthropology was well on its way to become an established academic discipline. The 'lineages' were hardly endogamous. British social anthropology was a small tribe where everyone knew everyone else. There were two corporate groups; one at Oxford, where E.E. Evans-Pritchard was appointed research lecturer two years before Radcliffe-Brown returned from Chicago in 1937, the other at the LSE, the stronghold of Malinowski, Seligman and, in the next generation, Raymond Firth. Nearly all social anthropologists educated in the interwar period were associated with one of these centres, but the centres were small and the distance between them insignificant, and students moved freely between them and followed lectures at both. Most of the first generation of students had studied initially with Malinowski, and were only later enamoured with the theories of Radcliffe-Brown. This group included Evans-Pritchard, Fortes and Max Gluckman. Students who remained 'Malinowskian' in their orientation included Firth, Audrey Richards, Edmund Leach and Isaac Schapera. But Malinowski's field methods were eagerly adopted by both groups, and everyone had to relate to Radcliffe-Brown's concepts of structure and function and the 'kinshipology' they fostered, for at least a decade after his death. Still, patronage was rife: only Firth among the Malinowskians attained a tenured position at a British university before 1950.

In demographic terms, the expansion of social anthropology was sluggish; on the eve of the war, there were fewer than 40 devotees throughout Britain, and only a handful held tenured academic positions. Nevertheless, the institutional expansion, both at home and in the colonies, was impressive. Radcliffe-Brown had no small part in this. During his long 'nomadic' period, he had established social anthropology departments in Cape Town and Sydney. During his stay in Cape Town (1921–25), he collaborated with an old student of Malinowski's, Isaac Schapera (1905–2003), who would later direct the department there for many years. Later, this department would be a critical voice throughout the years of Apartheid (1947–94), and though its orientation was increasingly Marxian in tenor, Radcliffe-Brown's influence remained palpable

throughout. After all, he had argued that *cultural* difference were not an argument for racial segregation, since the groups inhabited the same *society*, While in Sydney, Radcliffe-Brown stimulated the scientific study of aboriginal languages, and established Sydney's role as base camp for fieldworkers active throughout the Pacific. In Chicago, from 1931 to 1937, he contributed to the 'Europeanisation' of part of American anthropology (Chapter 4). Finally, Radcliffe-Brown's student M.N. Srinivas was instrumental in founding Indian social anthropology as a largely structural-functionalist discipline.

BOAS AND HISTORICAL PARTICULARISM

In time, British social anthropology would spread to many countries, both in Europe and further afield. Social anthropology was a branch of Durkheimian sociology, and its research priorities were social structure, social relations and social interaction, kinship, politics and economy. Problems that found no room within this definition (including all historical questions) were regarded as uninteresting. Tylor's old 'German' definition of culture faded into the background. As we have seen, Radcliffe-Brown regarded culture as a vague and unscientific term, and his lead was followed by most social anthropologists until the 1950s.

In the USA, the German influence was retained, and the discipline became known as *cultural* anthropology. Here, Tylor's broad definition of culture still ruled the field. In the German-American (and Tylorian) sense, culture is a far wider concept than society. If society is made up social norms, institutions and relationships, then culture consists of everything that humans have created, *including* society: material phenomena (a field, a plough, a painting), social conditions (marriage, households, the state), body techniques (dance, manual skills, sense perception), and symbolic meaning (language, ritual, belief). Anthropology was the humanist science, the science of humanity, and was, quite literally, concerned with everything human. Boas recognised that no individual could contribute equally to all parts of this vast subject (though he made heroic attempts himself), and advocated a 'four-field approach', dividing anthropology into linguistics, physical anthropology, archaeology and cultural anthropology (which included social anthropology). Students were trained in all four fields, later to specialise in one of them. Specialisation was thus an integral part of American anthropology from its inception, while in Britain and France a more generalistic approach remained the ideal, though in Britain,

at least, the restriction to 'society' was effectively a specialisation in its own right. As early as in the 1930s, there existed well-established research teams in the United States, specialising, for example, in Native North American languages.

Boas' own writings covered all four 'fields' broadly, with a slant towards the fourth field, cultural anthropology. He carried out long-term individual field research among the Inuit in 1883–84, but also worked with assistants who collected material on the north-west coast Kwakiutl and other Native American peoples. During fieldwork, he would make use of linguistically proficient members of the tribe under study, who would record, discuss and interpret the statements of informants. Some of these collaborators, notably the prodigious George Hunt (1854–1933), who co-wrote several of Boas' books on the Kwakiutl and supplied him with thousands of pages of ethnographic documentation, were later recognised as anthropological scholars in their own right.

Fieldwork, as Boas saw it, was often collaborative, and did not necessarily presuppose a lone researcher subjected to the long-term stresses of continuous 'immersion' in the field. Individual field stays were often short, and even during longer trips, he travelled extensively between local groups (some 5,000 kilometres during his year among the Inuit). Projects was frequently 'long term' in another sense, however, since stays were repeated many times over the years, often by a number of people who collaborated on the project (Foster et al. 1979; Silverman 2005). This methodological strategy had precursors in the German tradition, and was besides a natural approach, given that 'the field' was close at hand in the USA, not continents away, as in Britain.

Unlike his British contemporaries, Boas was not hostile to historical reconstruction. Native American cultural history was an integral part of his research agenda, and he included physical anthropology and archaeology as parts of the holistic anthropological enterprise. His basic orientation remained that of a cultural historian, and the school he is associated with is often loosely referred to as *historical particularism*. Like Bastian, he held that each culture had its own values and unique history, which anthropologists could attempt to reconstruct. He saw intrinsic value in the plurality of cultural practices and was deeply sceptical of any attempt, political or academic, to undermine this diversity. Writing about Kwakiutl dance, for example, he says it illustrates the culture's approach to rhythm, and cannot be reduced to a mere 'function' of society (as the 'British school' preferred). One must ask what the

rhythm *is* for the people who dance to it, and the answer can only be found by exploring the emotional states that generate and are generated by the rhythm (Boas 1927).

As a tireless critic of racism and the science inspired by it, Boas was a highly visible presence in American public debate. Against scholars who claimed that 'races' had different innate potentials for cultural development, Boas argued that culture was *sui generis* – its own source – and that inborn differences could not account for the impressive range of cultural variation that anthropologists had already documented. The term *cultural relativism*, which we have referred to above, was coined by Boas. In our own day, it is often asked whether relativism should be understood as a methodological or a moral imperative, and the answer is most often that it is method: without relativising your own culture, you can have no hope to understand another. To Boas, this would no doubt have seemed hair-splitting. For him, method and morality were two sides of the same coin.

Boas dominated American anthropology throughout four decades, but left no grand theory or monumental work that is read by succeeding generations of anthropologists. This reflects his distrust of lofty generalisations. During his years with Bastian, he was warned against the perils of empty theorising, and he took these warnings to heart. In his writings, he tries to identify the unique circumstances that generate particular cultures or cultural patterns, hardly ever jumping to general conclusions. He was cautious in his use of comparison, which all too easily established spurious similarities between circumstances that were fundamentally different. There is thus a sense in which Boas was a methodological individualist, in that he sought the particular instance rather than the general scheme. His lifelong scepticism of Durkheim confirms this.

Boas' students include almost all the important American anthropologists of the next generation (with some exceptions, to which we shall return). Among them (see below and Chapter 4) were Alfred L. Kroeber, who with Robert H. Lowie, his long-time colleague and fellow cultural historian, founded the Department of Anthropology at Berkeley; Edward Sapir, who founded the Department of Anthropology at Yale and the school of 'ethno-linguistics'; Melville Herskovits, who established Afro-American studies in the United States; Ruth Benedict, Boas' close friend and collaborator who founded the 'culture and personality' school; and Margaret Mead, the runt of the litter, who continued Benedict's

work, and became perhaps the most influential public figure in the history of anthropology.

As this list indicates, the cultural anthropology championed by Boas evolved in several directions during his lifetime. Further diversification followed in the 1950s, when Morgan was rediscovered and Radcliffe-Brown's associates at Chicago developed a brand of British-style social anthropology. Nevertheless, the legacy of Boas remains at the heart of American anthropology.

MAUSS AND THE TOTAL SOCIAL PRESTATION

One reason why *Argonauts of the Western Pacific* became such a classic was that the conditions it described were perfectly aligned with Mauss' insights in *The Gift*. The *kula* trade among the Trobriand Islanders was a gigantic example of Mauss' basic idea, that exchange creates mutually binding relationships. The juridical laws and moral norms that Radcliffe-Brown took for granted could thus be understood as products of an underlying process – an exchange of things and meaning that goes on continuously in human affairs. The elegance and simplicity of this thesis, and its fundamental consequences for our thinking about society, is a measure of how far French sociology had by now advanced. In contrast, British sociology was underdeveloped; this was perhaps one reason why social anthropology was such an instant success.

In France, Durkheim had built up a theoretically innovative, empirically based sociological discourse, that was open to researchers outside Durkheim's circle as well. The most famous of these outsiders was the learned academic Arnold van Gennep (1873–1957), renowned for his book *Les Rites de passage* (1909; *The Rites of Passage*, 1960), a comparative study of initiation rituals (in which persons move from one social status to another). The most widespread rites of passage are associated with birth, puberty, marriage and death. Anticipating Durkheim's sociology of religion, van Gennep argued that such rituals are dramatised expressions of the social order, which strengthen the integration of both initiates and spectators into that order. He claimed that initiation rituals were universally divided into three stages: separation, liminality and reintegration, a perspective to which we will return in discussing Victor Turner's work (Chapter 6).

Another contemporary of Durkheim and Mauss was the philosopher Lucien Lévy-Bruhl (1857–1939), who opened a field of enquiry that has stimulated later anthropologists, from Evans-

Pritchard and Lévi-Strauss to Jack Goody and Clifford Geertz. Lévy-Bruhl's thesis was that pre-literate and modern people think in qualitatively different ways. Pre-literal thought is not logical, but associative and poetical. Although Lévy-Bruhl's theory, first formulated in *Mentalité primitive* (1922; *Primitive Mentality*, 1978), was heavily criticised by his contemporaries, from Lowie in the USA to Schmidt in Germany, it framed an analytical field that has proven fertile later: the comparative study of thought styles, and the problems of intercultural translation associated with them. Like Frazer, whose work belongs to the same category, Lévy-Bruhl was most influential outside anthropology. He was embraced with enthusiasm by the surrealist movement, which equated 'primitive mentality' with freedom and creativity, and whose idealisations of 'primitive peoples' were not inhibited by empirical facts.

But mainstream French sociology and anthropology sprang from Durkheim, to whom lines are easily drawn even today, since there are fewer breaks in the French tradition than in Britain or America.

Like Radcliffe-Brown, Mauss based his work on Durkheim's theory of social integration. Unlike Durkheim, Mauss was particularly interested in non-European societies. His project was to create a comparative sociology, based on concrete descriptions of individual societies and institutions around the world – not unlike the general project of Malinowski, Radcliffe-Brown or Boas. But in contrast to Boas, Mauss sought to classify societies and establish structural similarities between them, in order to achieve a general understanding of social life. And in contrast to his British contemporaries, Mauss had no prejudice against historical material. The 'general laws' that Radcliffe-Brown looked for find no parallel in Mauss, who (with Boas) was careful not to generalise on an insufficient empirical basis. Though he never carried out fieldwork, his graduate courses at the Institute of Ethnology focused heavily on methods. Mauss insisted that students had to become ethnographers before they engaged in theory.

Mauss commenced his career with studies of religion and language, his specialisations were Indian religion and Sanskrit. In the decades before the First World War, he participated actively in the socio-anthropological milieu that had collected around Durkheim, which included several notable young scholars. Robert Hertz (1881–1915), who was only 34 when he died in the war, wrote seminal articles on death and on the significance of the right hand. Henri Hubert (1872–1927) published on sacrifice and magic, while Maurice Halbwachs (1877–1945), produced groundbreaking

work on social memory. With members of this group, Mauss became involved in one of the most ambitious research projects on record. Under Durkheim's leadership, the young scholars set out to explore social life in as broad terms as possible, from the socialisation of time and space, to embodied cultural categories, to the economy. This openness and breadth of scope remained an ideal for Mauss. But many participants in the project died during the war, and before it was over, Durkheim himself was dead, only 59 years old. Mauss spent years picking up the pieces, editing and publishing his dead colleagues' work, and continuing some of their projects. Like Boas, he left no great monograph or broad theoretical synthesis – even *The Gift* initially appeared as a long article in Durkheim's journal, *L'Année Sociologique*. But he wrote rich, dense essays that opened up entire research fields and are still read avidly today. Some of the most famous discuss the concept of the person, sacrifice, classification, death, totemism (with Durkheim), the body as a cultural category and nationalism. His most influential work, however, remains *The Gift*.

The basic idea in *The Gift* is simple: every gift wants something in return, every prestation demands a counter-prestation; gift exchange thus generates social relations; it is socially integrating. Gift exchange has a moral aspect, it binds people together in mutual commitment and is instrumental in the formation of norms, it appears to be voluntary, but is regulated by firm, if implicit, rules. It has a strategic and pragmatic aspect. It is manipulated by individuals seeking their own interests: generosity builds reputation, which is a resource in politics. Finally, the gift is an animator and meaning-creator. Things that are given and received become signs of other things and conditions than themselves. The ornaments that circulate in the *kula* trade, in themselves useless and meaningless, are said to have *mana* or *hau* (force, power). They have life, even a personality, because they have a history: the history of the exchanges they have been through.

Not everything can be exchanged, nor has equal *mana*. Modern money can systematically undermine the *mana* of some objects, while building up that of others through marketing. Compared to this, gift exchange clearly plays a subordinate role today. So while gift exchange occurs in all societies, its meaning and form varies. Mauss was particularly interested in a form of exchange he calls *préstations totales* ('total prestations'). Such gifts have such long and complex histories that they become compressed symbolic expressions of the social order as such. Christmas presents, perhaps

the closest approximation to *préstations totales* in our society, evoke family, society, community, capitalism, Christendom and paganism, among other things. But they are not total prestations in Mauss' sense, since their *mana* is restricted to a sentimentalised familial context. It is not *comme il faut* to use them as political weapons in elections.

The Gift asks how societies are bound together and how they are split apart. It discusses the circulation of things and meaning. Thus, it builds a bridge across two of the great theoretical divides in anthropology during the last 60 years: between structuralist and individualist analysis, and between materialistic and idealistic explanations. In an unsettling way, Mauss seems to unite the 'Grand Theory' of the 1970s with the deconstructionism of the 1990s, without a hitch.

Although he was not a prolific writer (he found writing hard), Mauss has had great influence both in France and abroad. One of his most famous students is Louis Dumont (Chapter 6), and in Britain he found a warm admirer in Evans-Pritchard, who in the long run found Radcliffe-Brown's restrictive Durkheimianism limiting. In Mauss he discovered a Durkheim who was less concerned with positivism and laws and more with the *Année* group's interpretive holism. Evans-Pritchard, too, found his way back to the German inspiration.

ANTHROPOLOGY IN 1930: PARALLELS AND DIVERGENCES

By 1930, communities of modern anthropologists were established in Britain, France and the USA, with contacts among more traditional, German-oriented anthropologists, not only in Germany itself, but in Scandinavia, Eastern Europe, Brazil, and elsewhere. Colonies of modern anthropologists were established in South Africa, India and Australia. The groups were still small. All in all there were no more than a couple of hundred professional anthropologists in the world, and to speak of 'schools' in this context may seem slightly out of place. Only eight years had passed since Malinowski published *Argonauts*. Radcliffe-Brown was still in Sydney, and would spend most of the 1930s in Chicago. Malinowski had few students, none of which had produced anything of importance so far. Diffusionism and 'armchair anthropology' flourished. Frazer had eleven years left as professor at Cambridge. In 1930, it could by no means be taken for granted that modern anthropology would survive in Britain, and the situation in France and the United States was not very different.

The founders of modern anthropology belonged to a small group, and had much in common in spite of their many differences. Most importantly, perhaps, they all sought to ground anthropology in a 'detailed study of customs, in relation to the total culture of the tribe that practised them' (Boas 1896, in Boas 1940: 272). The central point of this quotation is the idea that cultural traits could no longer be studied in isolation. A ritual is not a detached 'survival' of an hypothetical past. It must be seen in relation to the total society it is part of here and now. It must be studied in *context*. Anthropology is a *holistic* science – its aim is to describe societies or cultures as integrated wholes. So far, the four founders agreed, indeed, similar ideas were basic to Marxist, Durkheimian and Weberian sociology as well, and had gained wide support in academia by the turn of the century. We might even claim that the concept that 'society is a system' is the most fundamental of all sociological insights, and it should come as no surprise, therefore, that when it entered anthropology, it produced a theoretical revolution in which all four founders participated in one capacity or another.

Despite the diminutive size of the discipline, the differences between the national traditions were marked; in methods, theory and institutional organisation. Later, when the founders were dead, certain images of them, and of their mutual relations, took shape in the discipline. These images or myths are still widely dispersed and tend to allow certain of the more obvious qualities of each of the four men to overshadow all others. The reader should therefore bear in mind that academic relations among anthropologists are no less complex than human relations in general (Leach 1984). Thus, Boas and Mauss both agreed that there was no deep conflict between cultural history and synchronic studies, and both therefore retained an interest in diffusionism, while Radcliffe-Brown and Malinowski regarded such interests as 'unscientific'. This division clearly mirrors the fact that the two British anthropologists were engaged in a 'revolution', while there was a far greater sense of continuity in France and the United States. But other divisions were equally important. Radcliffe-Brown and Mauss agreed that their studies were part of a large, comparative sociological project, while Boas, the least sociologically inclined of the four, was suspicious of the 'French science' that Radcliffe-Brown preached in Chicago, and distrusted the comparative method deeply. Malinowski, for his part, seems to have avoided comparison altogether. In this case, the German heritage of Malinowski and Boas clearly unites them against the 'French school'. But this unity is also incomplete.

While Radcliffe-Brown and Mauss were committed methodological collectivists who delved into the secrets of 'society as a whole', Boas and Malinowski were (German) particularists. But Malinowski's particularism focused on the bodily needs of the individual, while Boas believed in the primacy of culture.

The purely personal qualities of the four men also influenced the new science of society. Boas effortlessly assumed the role of the benevolent father figure of American anthropology. Indeed, his popularity was so great during his long career that his obvious blind-spot, his distrust of generalisation, became the blind-spot of a generation. With few exceptions (including, famously, Benedict 1934), wide-ranging generalisations were absent from twentieth-century American anthropology until after the death of 'Papa Franz' and the end of the Second World War. In Britain, there was no such consensus. Well into their careers, Radcliffe-Brown and Malinowski were co-activists in the 'functionalist revolution' but, as their common enemy receded, their mutual antagonisms came to the fore, and their students (and students' students) eagerly reproduced their conflict (Chapter 4); Radcliffe-Brownians would speak derisively of the 'Malinowskian' monograph – packed with boring details, but with no workable ideas; while the Malinowskians would chide their Oxford colleagues for producing models that were so coherent as to be incompatible with the facts.

Finally, there were systematic differences between the three national traditions that were neither academic nor personal. Due in part to the prestige of Boas, in part to the fact that research money was more accessible in the United States, American anthropology quickly became a larger and more established discipline than in Europe. When the American Anthropological Association (AAA) was formed in 1906, it already had 175 members. As late as 1939, however, there were only some 20 professional anthropologists in the entire British Empire, and when the British Association of Social Anthropologists (ASA) was established in 1946, it had only 21 full members (Kuper 1996: 67; Stocking 1996: 427).

In France, the situation was different. The French academic system was more centralised than in the two other countries, and Paris attracted a large, talented and dynamic intellectual elite, which enjoyed considerable prestige. Membership in this elite was more important than disciplinary boundaries, and anthropologists were therefore involved in close cooperation and debate with sociologists, philosophers, historians, psychologists and linguists. Although anthropology was being institutionalised here as elsewhere, there

was not the same strong feeling of a new and revolutionary discipline taking form, defining itself as distinct from its predecessors, from other disciplines, and from other anthropological schools. Thus, French anthropology was in a sense both the most open-minded and the most elitist of the national traditions.

By the early 1930s, the three first schools of modern anthropology were firmly established. In the space of a short decade, the Victorian anthropology of Tylor and Frazer, the materialism of Morgan and the diffusionism of Graebner had collected a thick layer of dust. Much earlier theory still lay dormant, to be rediscovered by later generations, most notably the work of Marx and Weber; but as a whole, the enterprise of anthropology was perceived as fresh, new and exciting, as a key to a true understanding of the human condition. The groups in each country were small and highly motivated, in some cases (we have Mauss' and Radcliffe-Brown's followers in mind) almost reminiscent of religious cults.

In Europe outside France and Britain, the transformation of anthropological research made less progress. In Germany, diffusionists held sway until well after the Second World War, and it was only in the 1950s that social anthropology was established in Scandinavia and the Netherlands (see Hannerz and Gerholm 1982; Vermeulen and Roldán 1995, for partial histories of European anthropologies), although individual Dutch scholars had set their mark on the discipline since the nineteenth century. Cultural history in a vaguely diffusionist or evolutionist vein remained the norm, in Europe as elsewhere; at times sprinkled with ethnocentric prejudice and nationalist aspirations, at times advocating the dignity and rights of indigenous peoples and minorities.

4
Expansion and Institutionalisation

Jazz and Stalinism, *Kristallnacht* and Al Capone, blood on the horizon and bread-lines in the street, the consolidation of colonial empires and the birth of mass media, the crash of stock markets and rise of the welfare state. The 1930s quiver like an arrow waiting for release. Then war rolls across Europe and the world, a war even more terrible than its predecessor. Science gave us Hiroshima. Nationalism gave us Auschwitz. Now, if not before, the idea of the infinitely perfectible, rational individual was dead and buried. Or was it?

The war obliterated the last vestiges of the world in which the Victorians had lived and believed. The rational individual of the Enlightenment and the emotional community of the Romantics now seemed equally naive. Soon, the great colonial empires would also crumble, and with them their *raison d'être*, the White Man's Burden and *la mission civilisatrice*, the assumed obligation to spread European civilisation to every corner of the world.

Curiously, perhaps, it was in these years that anthropology blossomed into a mature discipline. The 1930s were a productive decade, when the founders' first students started making their mark on the field, and the founders themselves were still active. Neither did the Second World War seriously disturb this upswing. In the USA, university life went on nearly as before, and in Britain, too, scholars continued to work. Evans-Pritchard volunteered for military service in 1940–45 in Sudan and North Africa, but managed to collect data while in uniform, and even to publish a little-known monograph (1949) based on this work. Even in occupied France the situation was not intolerable, and the territories most affected by the war, the 'Bloodlands' occupied first by Stalin, then by Hitler, then again by Stalin (Snyder 2011) were marginal to the new anthropology anyway. Nevertheless, some questions were postponed 'for the duration' and funding for research and new positions was scant. After 1945, a new wave of radical reform would sweep through anthropology. The war coincided with the retirement of Mauss and Radcliffe-Brown (in 1939 and 1946), and the death of Malinowski

and Boas (both in 1942). With the old world gone and the founders out of the way, the time was ripe to state one's mind loud and clear. This story will be told in the next chapter, but first, we return to the 1930s.

A MARGINAL DISCIPLINE?

Anthropology was now faced with immediate challenges generated by its own success. 'The revolution', as Radcliffe-Brown and Malinowski called it, had been going on for more than a decade. The methodological, theoretical and institutional foundations of the refurbished discipline had been laid. Research programmes were defined, applications sent and funding obtained; professional friendships, enmities and alliances were built. The task now was to demonstrate the long-term viability of the discipline. Students had to be educated, journals edited, competent publishers found for monographs, conferences organised, the media addressed, politicians and planners convinced, and – not the least task – employment must be found for the growing number of graduates and researchers. In order to achieve this, the energy of the revolution had to be disciplined and channelled into predictable institutional routines. In Adam Kuper's history of British social anthropology (1996), the chapter dealing with this period is called 'From charisma to routine', quite as Weber might have put it: after a charismatic awakening, routinisation is bound to occur, however reluctantly. In anthropology, the period of consolidation lasted from the 1930s until the end of the 1940s. In Britain, the lead was taken by Radcliffe-Brown and his students; in the USA, Benedict, Mead, Kroeber and others ensured the continuation of Boas' sprawling programme, while in French anthropology glamorous figures such as Marcel Griaule and Michel Leiris lent the discipline an avant-gardistic and experimental touch.

As noted above, the new anthropology had a marginal identity from the outset. The founding fathers were themselves 'outsiders', and many of their successors ever since have been, like Radcliffe-Brown, nomadic, 'global scholars' who restlessly shuttled between universities and between home and field. Surprisingly many were also personally marginal. Some were of foreign origin, like Malinowski and Boas – or Kroeber, Sapir and Lowie, who were also from German-speaking countries. Some came from the colonies, like Fortes, Gluckman and Schapera from South Africa, Firth from New Zealand and Srinivas from India. Many, like Mauss,

Sapir or Alexander Goldenweiser, were Jewish. Several were women at a time when the academy was still a distinctly male domain – Mead and Benedict are well known, but Malinowski's students Audrey Richards (a pioneering Africanist) and Hortense Powdermaker (author of a classic on field methods), belonged to the same generation.

Unlike nineteenth-century evolutionism, twentieth-century anthropology was also marginal in the sense that it placed 'dirty heathens' on an equal footing with middle-class Westerners. Malinowski's holistic field method, Boas' cultural relativism and Radcliffe-Brown's search for universal laws of society suggested that all societies, or cultures, could be studied by the same methods and were essentially equivalent. Studying 'from below' had already become the hallmark of anthropological fieldwork. As opposed to the other social sciences, which often worked with large groups and aggregate populations, anthropologists studied the point of view of people on the ground, and were almost by instinct sceptical of decisions taken 'from above', by politicians and bureaucrats, who had no idea what life 'on the ground' was actually like. Nine out of ten anthropologists were, it seems, politically radical in one sense or another. The impeccable gentleman Radcliffe-Brown was known as 'Anarchy Brown' in his student days, due to his fascination with the Russian anarchist Kropotkin. Malinowski enjoyed comparing Trobriand Islanders favourably with Englishmen. Boas was an outspoken anti-racist, and even Mauss was an active socialist, though of a non-Marxist kind.

In fact, Boas' pacifism and his sustained (and successful) attack on academic racism made him unpopular among politicians and the senior administration at Columbia University (Silverman 2005: 264). At one point, his outspokenness seems to have led to a temporary freezing of funds for new positions at Columbia (Silverman 1981: 161). Boas had a towering position in American academic life. His student Margaret Mead's books, comparing middle-class Americans to Pacific Islanders, became bestsellers, and influenced American feminism and cultural radicalism deeply. And when Malinowski received standing ovations from packed auditoriums for his lectures about life in the Trobriand Islands during his 1926 tour through the USA, the message was clear: the potential of anthropology for cultural criticism and advocacy on behalf of native peoples was considerable.

It has been argued that particularly British anthropologists tacitly accepted the oppression of native peoples in Africa, Asia

and Oceania, and even that they cooperated actively with colonial administrations in return for research funds (see Asad 1973). In a scrupulously researched attempt to dig out the truth of this matter once and for all, Jack Goody (1995) concludes that the accusations are unfounded, and George Stocking (1995, ch. 8), the most authoritative historian of British anthropology, supports his view, as does Kuper (1996, ch. 4). They point out that several leading social anthropologists were explicitly critical of colonialism. Goody further shows that the Colonial Office and the various colonial administrations neither funded nor in other way encouraged anthropological research in particular areas or among particular groups. Funding for fieldwork was often obtained from American foundations. It is true that a handful of colonial administrators received some training from Malinowski, Radcliffe-Brown and the Cambridge anthropologists, and a few scattered examples exist of research commissioned by the Colonial Office. On the whole, however, colonial administrators were indifferent towards anthropology, and vice versa.

It might still be maintained that British anthropologists tended to pursue research interests that directly or indirectly legitimised the colonial project. The interest in political organisation in Africa, for example, seems a perfect match for the administrators of indirect rule (though there is little evidence of this research ever being taken seriously or put to use). The near-total absence of an interest in politics and economics among Boas' students may similarly reflect the fact that the Native North American tribes had seen their sociopolitical organisation destroyed; symbolic culture and memories were all that was left to study. It has always been the case, and remains a fact that research agendas are constructed in particular historical contexts, and that they themselves bear the imprint of these contexts, consciously or not.

The marginal position of anthropology is perhaps easy to understand. The discipline recruited a particular kind of person, who could thrive on (or at least tolerate) long-term fieldwork under uncomfortable or unglamorous conditions. Ever since Malinowski's stay among the Trobriand Islanders, this was the name of the game. Anthropology's subject matter was itself rather off the beaten track: kinship systems in Africa, exchange networks in Melanesia and North American ritual dances did not seem to belong to mainstream science.

With all the marginalising and individualistic tendencies, it is quite impressive that anthropology, during the years we are

now surveying, achieved respectable academic status. In the end, charisma was successfully converted into routine and institutional power. In the following we shall turn to how this process unfolded.

OXFORD AND THE LSE, COLUMBIA AND CHICAGO

In 1930, there was effectively only one academic centre of the new anthropology in Britain, at the LSE, where Malinowski presided under Seligman's benevolent gaze from 1924 to 1938. At the LSE, Malinowski taught nearly the entire next generation of British anthropologists: Firth, Evans-Pritchard, Powdermaker, Richards, Schapera, Fortes, Leach and Nadel are some of the most famous names. The dependency on a single person made the milieu vulnerable, but after Malinowski's departure for the USA, continuity was secured through Firth, a Malinowskian functionalist, who had been at the LSE since arriving there from New Zealand as a student in 1923. At Oxford, the old guard reigned until the mid-1930s, when Evans-Pritchard and later Radcliffe-Brown himself arrived to build up a more theoretically oriented environment. At Cambridge, once a powerhouse of anthropological scholarship, anthropology was almost extinct by 1930: after Haddon's retirement in 1926, only Frazer, an old and shy man, remained until the Second World War, and the new social anthropology was not properly introduced until the appointments of Fortes, Jack Goody and Leach in the 1950s.

In 1930, however, this was all in the future. Evans-Pritchard was on fieldwork in the Sudan, and would later take up a post in Sociology at the University of Cairo. Radcliffe-Brown was still in Sydney and would soon move on to Chicago, where he would spend six years. The most important institutional development in British anthropology in the 1930s was the founding of the Rhodes-Livingstone Institute in Livingstone, Northern Rhodesia (now Zambia) by a group of young scholars under the leadership of Godfrey Wilson. Among the first research fellows was a South African, Max Gluckman, who would in the coming decades direct a series of pioneering studies of social change in Southern Africa (Chapter 5).

Radcliffe-Brown's stay in Chicago in the 1930s was a fruitful one in that it stimulated the formation of a group of non-Boasian anthropologists at an excellent American university. The department he worked in was a combined department of sociology and anthropology founded in 1892, where the influence of European sociology (Weber, Simmel) was prominent from the start, and

pioneering studies of urban life, migration, ethnic relations and peasants had been carried out, using ethnographic methods, since before 1920. Radcliffe-Brown's sociologically inclined anthropology was met with great interest in this group, and he was a major source of inspiration for, among others, Robert Redfield, Sol Tax, Fred Eggan and Ralph Linton (Chapters 5 and 6).

The undisputed centre of American anthropology was nevertheless still in New York City, at Columbia University, where Boas presided. In 1930, he had just finished training his second batch of students. Of the first group, who had taken their doctorates in 1901–11, the German Kroeber and the Austrian Lowie had left for California to establish the Department of Anthropology at Berkeley. Another, the Ukrainian Alexander Goldenweiser, had found employment at the New School of Social Research in New York. A fourth, the German-born Edward Sapir, had founded ethnolinguistics and become professor at Chicago – and a fifth, the Pole Paul Radin (1883–1959), moved from university to university, writing innovative ethnographies where the informants were given space to express their views, and becoming a post mortem icon of postmodernist anthropology in the 1980s..

In contrast to this motley crew of European immigrants, Boas' second batch of students were by and large born-and-bred Americans. The most influential of them were Ruth Benedict, Melville Herskovits and Margaret Mead.

But the discipline was larger and more diverse in the USA than in Britain. The dominance of Columbia was far from complete, and, many influential anthropologists with no connection to Boas appeared in the country during the 1930s and 1940s. Robert Redfield (1897–1958) was one of them. His speciality was peasant studies, which he and his students pursued in Latin America, India and Eastern Europe. Another was Leslie A. White (1900–1975), whose teachers included Sapir and Goldenweiser. White established himself at the University of Michigan in 1930, where he developed a materialist neo-evolutionist theory in direct opposition to Boas. Around the same time, the sociologist Talcott Parsons (1902–1979) was employed at Harvard, where he would work for more than four decades on a grand synthesis drawing on both Weber and Durkheim, which would eventually involve some prominent anthropologists as well. Ralph Linton (1893–1953), a Polynesianist educated at Harvard in the 1920s, took exception to Boas for his pacifism and Benedict for her focus on emotions, and developed a social theory somewhat similar to that of Radcliffe-Brown. In 1937,

George P. Murdock (1897–1985) began his magnum opus at Yale University: the *Human Relations Area Files* (HRAF), an enormous database of cultural traits worldwide, which has been used and criticised by researchers for half a century.

As political conditions in Europe deteriorated and the Second World War approached, established European scholars appeared in the USA, and not just Jews from the Nazi-dominated areas, though they may have been the most numerous group. One was Malinowski himself, who spent a few years at Yale before his death in 1942, the same year as Boas. Another was the British anthropologist Gregory Bateson (1904–1980), who forged early links between structural-functionalism and the psychologically oriented anthropology of Benedict and Mead. Yet another was the Hungarian economic historian Karl Polanyi (1886–1964), who moved to New York in 1940 and was employed as an historian at Columbia, where he would inspire Julian Steward, a student of Kroeber and Lowie from Berkeley, to develop a materialist, historically oriented brand of neo-evolutionism at Boas' old department. Finally, the young Claude Lévi-Strauss fled from Vichy France and spent the war years in New York, where he developed his immensely influential theory of kinship (Chapter 6).

We will later discuss some of these events in greater detail. First, however, we must take a look at developments in France, where Mauss and his colleagues were realising their grand programme in practice.

THE DAKAR-DJIBOUTI EXPEDITION

Institutionalisation came late to French anthropology. It was only in 1926 that systematic training in the discipline started, when Mauss, Lévy-Bruhl and the South Americanist Raul Rivet (1876–1958), founded the Institute of Ethnology at the University of Paris. Among the first students, Marcel Griaule (1898–1956) deserves note. After participating in an expedition to Ethiopia in 1928, Griaule initiated and led the ambitious Dakar-Djibouti expedition (1931–33), financed by the French government, in which an interdisciplinary group of researchers traversed French colonial Africa from Senegal to Ethiopia. The expedition collected materials for the new Musée de l'Homme, which was opened under Rivet's leadership for the World Exhibition in Paris in 1937. The expedition brought home vast materials, and in this sense was a success.

But Griaule was dissatisfied with the methods used. He was the first French anthropologist to advocate long and intensive field studies of individual societies. During the Dakar-Djibouti expedition, Griaule had met the Dogon people in Mali, and this is where he decided to realise his fieldwork ideas in practice. The research group he started maintained a constant presence among the Dogon for 25 years, thanks to which they are among the most thoroughly studied peoples in the world. In 1938, Griaule published *Masques Dogons* ('Dogon Masks'), a complex book that is in part an empirical documentation of myths, masks and funerary rites, in part an analysis of the relationship between these and other aspects of Dogon society, culminating in a discussion of their notion of the person, one of Mauss' favourite themes.

In 1946, Griaule met an old hunter who initiated him into Dogon cosmology, a theme that would occupy Griaule for the rest of his short life. Before his death he had time to publish his conversations with the old hunter, in *Dieu d'eau: entretiens avec Ogotemmêli* (1948; *Conversations with Ogotemmêli*, 1975), a lively mix of interviews and ethnographic material that became a bestseller. But the full cosmology was only completed and prepared for publication posthumously, by his close collaborator, Germaine Dieterlen (1903–1999).

Griaule was the first French anthropologist to do full-fledged fieldwork, and from his position as the first professor of ethnology at Sorbonne he contributed greatly to raise the quality of methods in French research. He argued for long-term fieldwork. But this did not imply, as it did for Malinowski, that the ethnographer must work alone in the field. Griaule maintained the old expeditionary ideal of interdisciplinary teamwork. How otherwise could the Dogon have been studied so comprehensively? He was also an early advocate for audiovisual techniques that could register data in finer-grained detail than would otherwise be possible. The photographic and filmic documentation of the Dogon continued after Griaule's death and offers a unique picture into the changes their society went through over nearly 30 years.

Griaule's influence in French anthropology was fundamental. An entire generation was brought up with his methodological ideals, his focus on aesthetics, his interest for visual anthropology and Africa. This was where the French *cinéma vérité* school of ethnographic film originated. This school, led by Griaule's student Jean Rouch (1917–2004), produced films that showed the dialogue between film

team and informants and thus made the conditions of knowledge production explicit. Again, this is postmodernism *avant la lettre*.

Griaule's most original student was Michel Leiris (1901–1990) who, after studying ethnology, was drawn into the surrealist movement in the 1920s, later becoming part of the anti-surrealist circle around the philosopher Georges Bataille, and joining the Dakar-Djibouti expedition as Griaule's secretary. On his return from Africa, Leiris wrote *L'Afrique fantôme* (1934, 'Invisible Africa'), a philosophical and ethnographic account, full of sharp insights into the role played by the anthropologist in the field. In 1937, Leiris and Bataille joined up with another ethnologist and ex-surrealist, Roger Caillois (1913–1978), a former collaborator of Mauss', to form the legendary Collège de Sociologie that organised seminars and authored articles on power, mythology and the sacred, which would later inspire the French materialist anthropologists (Chapter 7), but also the experiments conducted by Leiris and Caillois in the borderland between anthropology, literature and psychology.

Thirty years before the Dakar-Djibouti expedition, and nearly 15 years before Malinowski's visit to the Trobriand Islands, Maurice Leenhardt (1878–1954) spontaneously embarked on one of the longest field studies in history, among the Kanak of New Caledonia in Melanesia. Leenhardt went there as a missionary in 1902 and remained until 1926, combining systematic field studies with zealous defence of Kanak culture against the destructive effects of alcohol and imperialism. Back in France, with the support of Mauss and Lévy-Bruhl, he was awarded an academic position in Paris, and in 1941 appointed to a professorship at the École pratique des hautes études. Leenhardt's work is remarkable not only for its comprehensiveness and ethnographic detail, but also for its subtle discussions of intercultural translation, a theme that received little attention in Anglophone anthropology until the 1970s.

In this institution-building phase, anthropology in France evolved with hardly any reference to developments in Britain or the USA. It is worth note, however, that French anthropologists played a key role in the establishment of modern anthropology in several Latin American countries. Alfred Métraux (1902–1963), a Swiss student of Mauss and a prominent South Americanist, founded the Institute of Ethnology at the Universidad Nacional de Tucumán in Argentina in 1928. And Mauss' colleague, Paul Rivet, spent the Second World War establishing anthropological institutions in Columbia and Mexico. In these countries, where the US influence

was also strong, French and American traditions met and merged at an early date.

CULTURE AND PERSONALITY

While Boas had very wide-ranging interests, his students tended to specialise. Each followed up a part of the master's total project, and could equally claim to be true Boasians. Yet it is often said that the direct line of succession goes from Boas to the *culture and personality* school of Ruth Benedict (1887–1948) and Margaret Mead (1901–1978). Benedict was a close friend of Boas and associated with him both institutionally and academically. She spent her academic life at Columbia and was de facto head of department for the last year before Boas' retirement, in 1937. She was bypassed for Boas' Chair, however, in favour of Ralph Linton, a former military man, whom the university administration preferred to a radical Boasian who was also a woman. Mead, based at the American Museum of Natural History in New York City, also taught at Columbia. Like Boas, both women were highly visible public figures. The books they wrote were read by millions all over the world, and contributed significantly to giving the new discipline of anthropology a public face. Mead was also an able lecturer to popular audiences, which increased her fame further.

Like many of Boas' students, Benedict and Mead paid little attention in their research to the political and economic issues that were so central to British structural-functionalist anthropology, converging instead on issues of psychology (personality, emotions, 'character') and culture (socialisation, gender roles, values), which were mostly ignored by their British counterparts. When Benedict argued that emotions and culture were connected, she was, from one viewpoint, merely following up Wundt's old interest in *Ethnopsychologie* and extending the tenure of German Romanticism in American anthropology, from another point of view, however, she was taking a radical step. It was commonly held, not least in British anthropology, that emotions had nothing to do with society. Culture was a collective phenomenon, while emotions were assumed to be individual. Mead and Benedict nonetheless argued that patterns of emotion could be shared, they were also parts of culture. Although much in their work has been criticised, there can be no doubt that they took the first effective steps towards establishing psychological anthropology as a recognised anthropological sub-field.

Benedict came from a solid, if unhappy, middle-class background, and it was not until she turned 30 that she took up anthropology, under the tutelage of Boas and Goldenweiser. She carried out some fieldwork among Native North Americans, but her influence derives mainly from two books which are not ethnographic monographs, but large-scale comparisons: *Patterns of Culture* (1934) and *The Chrysanthemum and the Sword* (1946). In *Patterns of Culture*, Benedict developed the idea that culture may be analysed as a macropsychological pattern. Rather than cataloguing the substance of cultures, she sought to identify the configuration of its collective 'personality', the 'emotional style' or 'aesthetics', which permeated action, emotion and thought. Benedict referred to this as *ethos*.

One of the main empirical contrasts in *Patterns of Culture* is between two Native North American tribes, the Zuni and the Kwakiutl. The Zuni had a strong sense of group solidarity, political leadership was non-authoritarian, rituals undramatic and child-rearing practices mild. The Kwakiutl, in contrast, were a people of excess and exaggeration – their famous *potlatch* institution, for example, to which both Boas and Mauss had devoted attention, was an aggressive, spectacular and boastful gift-giving competition. Benedict referred to the ethos of these ambitious, hedonistic individualists as *Dionysian*, while the serene Zuni were *Apollonian* (both concepts derived from Nietzsche, a nod to the German roots of her thinking). Benedict also attempts to explain how ethos relates to social practices and institutions, and in these passages her holism comes close to that of the structural-functionalists.

During the Second World War, Benedict was commissioned to write a report about Japanese national character. Her second bestseller, *The Chrysanthemum and the Sword*, was based on this material, and is to this day quite well regarded among East Asianists. The book describes the ethos of Japanese culture, and posits a fundamental psychological tension in this culture, between violence and aestheticism. With Mead, Benedict developed a methodology of 'studying culture at a distance' (by means of written sources, visual and artistic media, and interviews with émigrés), for use in cases where fieldwork was impossible (Mead and Métraux 1953). After the war, Benedict and Mead co-directed a large-scale comparative project based on this methodology, the Columbia University Research in Contemporary Cultures Project, funded by the US Navy, which engaged some 120 researchers in studies of seven Western and non-Western cultures. The premise of this work was that entire nations developed 'personality types' – national ethoses. This notion

was controversial in Mead's time, and is even more so today. Still, the idea of Herderian national character has never disappeared from anthropology, and it resurfaced in the new research on nationalism that began in the 1980s (Chapter 8).

Mead's parents were social scientists, and she grew up in a liberal family of intellectuals, constantly moving from place to place. While Benedict was a shy and sometimes self-effacing personality, Mead was a self-assured young woman of 24 when she embarked on fieldwork to Samoa. This study and the resulting monograph may have been the first Malinowskian, participant-observation-based field study leading to a holistic monograph carried out by an American anthropologist (Silverman 2005: 268). Mead would later do fieldwork in New Guinea, after which she married Gregory Bateson (see below). The meticulous photographic study from Bali, which Mead and Bateson conducted together (Bateson and Mead 1942), was an attempt to describe and analyse body language. The authors assumed that the 'collective emotions' of ethos would be expressed in body language, and that their study would add empirical substance to Benedict's claims. In the late 1970s a similar idea was proposed (and backed up with greater theoretical sophistication) by the French anthropologist Pierre Bourdieu, who referred to it, with a term from Mauss, as *habitus*.

Like Marx, Mead was simultaneously a researcher and an activist, and these two strands of her life were inextricably intermeshed. The comparison with Marx, far-fetched as it may seem, is not completely gratuitous. Mead was a guiding light of American feminism, and her ideas inspired American – and Western – liberal opinion in the postwar decades. Her work from the Pacific told young Americans that there were many solutions to the challenges of living, and young American women that gender roles could be changed (Marcus and Fischer 1986).

As an activist, Mead's message was simple: if culture shapes personality, then personality can be changed by changing culture. In her first bestselling book, *Coming of Age in Samoa* (1928), she compared the 'free' style of socialisation in Samoa with the strict, authoritarian style of the American middle class, arguing that Samoan girls were happier than their American counterparts. Along with *Sex and Temperament in Three Primitive Societies* (1935), her Samoan book belongs, with *The Golden Bough*, *Patterns of Culture*, *The Chrysanthemum and the Sword*, Gregory Bateson's *Steps to an Ecology of Mind* (1972) and Lévi-Strauss' *Tristes Tropiques* (1955), among the most widely read books ever produced by anthropologists.

Among colleagues, the reaction to Mead's work was often less than enthusiastic. However, the scholarly debate on Mead was complicated by political issues. Her (and Benedict's) foremost contemporary critic in the United States, Linton, was, as we have mentioned, politically opposed to the liberal agenda of Boas and his students, and his appointment to Boas' Chair at Columbia created long-lasting conflicts in American anthropology, which continue to confuse the debate to this day. Further afield, in Britain, Mead's (and Benedict's) books were augustly criticised for being unscientific: making unwarranted inferences about mental states and generalising on the basis of inadequate data. But even here, extraneous issues intruded. Evans-Pritchard most likely echoes the Oxford pub-talk of the time accurately when he describes *Coming of Age in Samoa* as 'a discursive, or perhaps I should say chatty and feminine, book with a leaning towards the picturesque, what I call the rustling-of-the-wind-in-the-palm-trees kind of anthropological writing, for which Malinowski set the fashion' (Evans-Pritchard 1951b: 96). As the quote implies, Mead was in part criticised because she was a (extremely successful) woman. In part, she was attacked as a crypto-Malinowskian, and the German influence underlying both her and Malinowski's psychological inclinations, lends credence to this view. Such issues have (Chapter 7) continued to occlude the more material questions: whether Mead's fieldwork was superficial and her substantial conclusions misleading, and whether her consistently culturalist version of psychological anthropology is valid.

Psychological anthropology was not only promoted by Benedict and Mead. Linton, who is best remembered today for his work in microsociology, also developed a form of psychological anthropology, in collaboration with psychoanalyst Abraham Kardiner. In *The Individual and His Society* (1949) they rejected Benedict's idea that cultures are 'personalities writ large' and proposed that specific child-rearing practices generate personality problems that are expressed in social organisation, which, in turn, amplifies the original problems. Critical of Radcliffe-Brown's teachings at Chicago, which he considered reductionist, Linton defended a wide definition of culture, in which psychology figured prominently.

CULTURAL HISTORY

While Benedict and Mead followed up Boas' and Wundt's psychological interests, others continued his work in cultural history. A prominent example was Alfred L. Kroeber (1876–1960), who

came from an upper-middle-class German Jewish family in New York, and was Boas' first student. After founding one of the great American anthropology departments in 1901 and building up one of the world's leading ethnographic museums, Kroeber continued to work at the University of California at Berkeley until his retirement in 1946. Like Boas, Kroeber was an anthropological jack of all trades, but his main interest was in cultural history, and he wrote several voluminous historical studies of European and non-European civilisations. Kroeber's long-time colleague at Berkeley, Robert H. Lowie (1883–1957), shared this interest, but added a hint of materialist evolutionism which would soon inspire their prominent student, Julian Steward, to more controversial conclusions.

In the course of his long academic career, Kroeber collected vast amounts of data on Native North Americans. His *Handbook of the Indians of California* (1925) is a 1,000-page ethnographic volume, and in his later work, *Cultural and Natural Areas of Native North America* (1939) he emphasises the importance of history to an understanding of the native cultures. Kroeber had at an early stage declared his dissatisfaction with the trait-by-trait comparative method that the evolutionists had pioneered and that was still in common use, particularly in German anthropology (even Boas used it at times). Trait-by-trait comparison was a superficial approach. Cultures were organic wholes (in a nearly Durkheimian sense) that could not be disassembled into their component parts without losing their meaning. Kroeber referred to the cultural whole as the *superorganic*, an integrated system that was more than biological yet seemed to have its own innate dynamics, almost to live its own life. Kroeber is often considered an extreme methodological collectivist. In his article 'The superorganic' (1917, reprinted in Kroeber 1952), he points out that innovations are often made simultaneously by different people in different locations. This, he argues, in a near-Hegelian vein, is evidence that cultures 'live their own life' and their *Zeitgeist* unfolds independently of individuals.

Though Boas had argued along similar lines back in 1896, both he and several other colleagues thought that Kroeber went too far. Culture was not an object independent of human beings. It must not be *reified*. Kroeber must have taken this criticism seriously, since he suggested a compromise towards the end of his career.

In the 1950s, Kroeber worked on several collaborative projects (with Parsons and others), and his last contribution to American anthropology was a large, interdisciplinary research programme, where anthropologists would study 'culture', while sociologists

studied 'society' (Kuper 1999). This project included among its participants two young men, David Schneider and Clifford Geertz, who will be introduced more fully in Chapter 6.

ETHNOLINGUISTICS

Another branch on the Boasian trunk was the synthesis of linguistics and anthropology established by Edward Sapir (1884–1939). Sapir was yet another German-Jewish immigrant to the United States, though he arrived as a child. He studied several Native American languages, spent 15 years at the Canadian National Museum in Ottawa, and was in charge of the ethnographic collections at the University of Chicago. He then moved to Yale University, where he founded and directed a new anthropology department until his early death. Sapir is regarded as the father of modern ethnolinguistics, and his *Language* (1921) is still a standard work of linguistic anthropology.

With his student and later colleague, Benjamin Lee Whorf (1897–1941), Sapir developed the so-called Sapir-Whorf hypothesis on the relationship between language and culture. It states that languages differ fundamentally in syntax, grammar and vocabulary, and these differences imply more profound differences in the language users' ways of perceiving and living in the world. A Hopi-speaker perceives another world than an native English-speaker. Hopi language is poor in nouns and rich in verbs, and promotes a worldview rich in movement and process, but poor in things. European languages, in contrast, have more nouns and fewer verbs, which would bias them in favour of a worldview focused on objects. The viewpoint has often been criticised, and some of the critique is reminiscent of the criticism levelled at Lévy-Bruhl's 'primitive mentality'. As Bateson once pointed out, the main problem with the hypothesis may be that it cannot – on some level or other – *not* be true. Obviously, language influences thinking; the question is only to what extent and in what ways that influence is expressed.

With Sapir, the study of language and culture, originally a German interest, passed on to America, and for years it remained an almost purely American specialisation in anthropology. But since the war, and particularly since the 1980s, the field has expanded dramatically. The Sapir-Whorf hypothesis first became an issue in British anthropology in the early 1960s, during the 'rationality debate' (Chapter 6). More recently, many ethnolinguistic issues have

cropped up in modified form in the burgeoning field of cognitive anthropology (Chapter 6 and 9).

Sapir was a mediator between American and European traditions. He was well-read in classical sociology and befriended both Radcliffe-Brown and Malinowski during their years in the USA. Among Boas' successors, Sapir was a source of theoretical inspiration. His view of culture would influence both Kroeber, Benedict and Mead, and through them most of American anthropology. While culture, for Boas, had been an integrated whole mostly in name, Sapir laid the groundwork for a concept of cultural integration similar to that of linguistics. He pointed out that cultural integration does not mean that members of a culture agree about anything, but that they share a common ground for disagreement. It is like language. We speak with the same grammar, even when we quarrel. The role of the anthropologist, as for the linguist, is to reveal what is hidden and taken for granted, beneath the visible expressions of culture. Here, we sense Sapir's debt to European sociology, and note that he anticipates ideas that Lévi-Strauss, among others, would soon take much further. In the great American debate about the concept of culture that started in the 1960s and continues today, the critics gladly thrash Benedict or Boas, but as a rule stay away from Sapir, who is less easily demolished.

THE CHICAGO SCHOOL

In Boas' lifetime, a range of research projects had thus been set in movement, which in one way or another continued his ideas. Some would in time diverge from Boas' own convictions – this was particularly true of the neo-evolutionist movement of the 1950s and 1960s. But even when the students disagreed with the master, his mark was still visible, at least indirectly, on most of what they did. Boas' interest in cultural history, archaeology, diffusion, language, symbols and psychology made American anthropology far more varied than the European traditions.

But, as we noted above, there were quite a few anthropologists in the USA whose intellectual lineage did not include Boas. The prime example was the group of social scientists that established itself in Chicago under the auspices of Robert E. Park (1864–1944) and William I. Thomas (1863–1947) and their collaborators in the interwar years. The urgent challenge for the Chicago sociologists was to understand ethnic relations in the seething cauldron of metropolitan Chicago – black people from the South, Jews,

Irishmen, Scandinavians, Italians, Poles, Lithuanians. Theoretically, the Chicago school addressed this issue by means of 'ecological theory': the city was approached as a microcosm inhabited by social groups and institutions. Methodologically, the challenges of the city were addressed by ethnographic method. The anthropologists here inhabited a milieu where interdisciplinary cooperation was the order of the day. Retrospectively, this research seems decades ahead of its time (Hannerz 1980). It was urban anthropology and migration studies at a time when anthropology was still synonymous with the study of small 'remote' communities; and it was ethnicity studies before the term 'ethnicity' had even been coined.

Apart from studies of the metropolis, the Chicago school is best known for its research on peasant societies in Latin America, Eastern Europe, and – somewhat later – India. Peasant studies had its roots in studies of rural communities by German and East European sociologists, historians and economists. Among these was the Russian Alexander Chayanov (1888–*c.* 1938), who developed a theory of peasant economics around the time of the First World War. Chayanov, who died in one of Stalin's purges, was almost unknown in the West until the 1950s. In part of his theory he asks why it is so difficult to get peasants to produce for profit, and concludes that they have a marginal, subsistence economy which makes them unwilling to take risks. Simple as it may seem, this formulation has had fundamental consequences for anthropological studies of underdevelopment. Another important East European, who directly influenced the Chicago school, was the Polish poet and rural sociologist Florian W. Znaniecki (1882–1958). Znaniecki and Thomas developed a close collaboration, and while Znaniecki was in Chicago, they finished their great cooperative effort, the monumental five-volume *The Polish Peasant in Europe and America* (1918–20). Polish sociology would later become known for its pioneering use of ethnographic methods.

In 1934, the first of Thomas and Znaniecki's successors was already in place in Chicago, Robert Redfield (1897–1958). Redfield, who was virtually born and bred in the Chicago school, was first influenced by the Boasians during Sapir's stay in Chicago from 1925 to 1931, and later by Radcliffe-Brown. Redfield did fieldwork in Mexico, and directed several projects in Central America. Theoretically, his research concentrated on two questions: first, could peasant societies be said to have their own culture, or is their way of life a mere product of economic necessity? Redfield responded that peasants had cultures, just like anyone else, and he

did not see their hybridised ways of life as any less authentic than other cultures.

Second, Redfield asked how data about local peasant life could be integrated with data about processes on the national, regional or global level. Since the term 'peasants', as used in anthropology, denotes communities of subsistence agriculturalists who are also marginal participants in non-local processes of politics and exchange, their communities cannot be studied as if they were isolated and self-sustaining. Redfield initially proposed to deal with this situation by means of a simple dichotomy between *folk culture* and *urban culture*, or 'little traditions' (local, orally transmitted) and 'great traditions' (non-local, literate). This classification was based on cultural criteria, and was hardly concerned with the economic and political aspects of peasant life, a fact that was criticised by the more materialistically oriented students of peasants who surfaced in the 1950s. In a famous controversy in American anthropology, Oscar Lewis undermined Redfield's conclusions by performing a re-study of the Mexican village where he had conducted fieldwork and arriving at very different conclusions (Redfield 1930; Lewis 1951).

Redfield would eventually modify his views, suggesting that folk and urban culture were not dichotomous, but opposite poles of a continuum, and incorporating processes of migration and cultural modernisation (individualisation and secularisation) into his model. However, he was not willing to abandon his emphasis on symbolic culture, a view he shared with many other American anthropologists. In fact, Redfield's view of culture was not very different from Benedict's. He was concerned with showing how the peasant way of life created a particular 'cultural character' or ethos, not, for example, with unearthing the power structures that dominated the peasantry.

In the postwar years, Chicago was would become the birthplace of the 'Second Chicago School', with its microsociological tradition that focused on detailed analyses of person-to-person interaction in limited, often short-term settings (for instance, within an institution). This approach, often referred to as *symbolic interactionism*, was developed by sociologists, two of whom would later exert considerable influence on anthropology. These were Erving Goffman (Chapter 5), known for his subtle studies of interaction ritual and role play, which would soon become part of the anthropological canon; and Raymond Birdwhistell (1918–1994), a pioneer in the study of cross-cultural communication and body language, who followed up Bateson and Mead's interest in these themes.

Why was the rest of the anthropological community so slow in taking up these new research interests? At one level, the answer is simple. Neither Chicago immigrants, semi-urbanised Polish peasants nor the staff of a modern hospital could be said to have a 'true' culture, and they were therefore 'unsuitable' as objects of anthropological enquiry. Long after the majority of anthropologists had lost their interest in grand theories about the 'original state of man' (as in Rousseau or Morgan), the notion that some cultures were more 'authentic' than others lived on. African tribes and threatened Native Americans were more attractive for anthropologists than the hybrid cultures created by modernisation. This preference was not explicit, and Radcliffe-Brown – for one – greatly admired the work of the Chicago school. But when all was said and done, the 'real primitives' carried greater professional prestige. And there were good reasons for such priorities. 'Real primitives' were rapidly disappearing from the face of the earth, and it was a pressing task to document their way of life before it was too late. One may still sympathise with this motivation today, since all generalisations about the human condition are dependent on the widest possible range of comparative data.

'KINSHIPOLOGY'

Despite the breadth of American research, it is not primarily this we associate with the anthropology of the 1930s and 1940s. Instead, we remember the great British structural-functionalists and their classical monographs, which not only stuck to 'real primitives', but described the structural principles underlying their lives in a manner that, in its formal elegance, was eminently 'civilised'. The authors of these studies were mostly former students of Malinowski's, some of whom were closer to Radcliffe-Brown than others. Until the 1950s, a few of the most prominent men (none of the women) in this group were appointed to leading positions at prestigious British universities. Most – particularly Firth's and Malinowski's loyal students – had to wait until the next decade.

One of Malinowski's star students, who would later become the main advocate of Radcliffe-Brown's structural-functionalism, was Meyer Fortes (1906–1983), a South African Jew, originally educated as a psychologist. Fortes' break with Malinowski in the 1930s was dramatic, and not exclusively motivated by academic differences. For example, Malinowski apparently asked Fortes in 1934 to make a written statement where he confirmed that he had borrowed all

his ideas from Malinowski himself (Goody 1995: 37). Whereas Firth, the mild-tempered New Zealander, reacted to Malinowski's outbursts with stoicism and a pinch of salt, Fortes was personally offended. At any rate, by the time of the publication of the seminal *African Political Systems* (Fortes and Evans-Pritchard 1940), it had become evident that Fortes' interests were far closer to Radcliffe-Brown's than to Malinowski's. His speciality was kinship, a topic Malinowski never wrote about in detail, although for years he had kept promising a book on kinship among the Trobriand Islanders. In 1932, Fortes began his first major fieldwork in the Gold Coast (now Ghana), and in the 1940s and 1950s he would publish widely on two of the largest and socially most complex peoples in this country, the Ashanti and the Tallensi. His *The Dynamics of Clanship among the Tallensi* (1945) is considered one of the high points of structural-functionalism. It was largely due to Fortes' efforts that the British anthropology of the period was often spoken of disparagingly as 'kinshipology'. Many, not least among the Americans, felt that all the attention lavished on kinship betrayed the holistic ambitions of the discipline.

In 1946, Radcliffe-Brown retired, just as his life's work, structural-functionalism, was reaching its zenith. Radcliffe-Brown had seen kinship as the engine driving primitive society, the glue that held it together and the moral universe in which it lived. This view was confirmed and strengthened through Fortes' studies, which – while based on massive ethnography – focused on Radcliffe-Brownian 'mechanisms' and structural principles. Fortes' close professional ally and friend, Edward E. Evans-Pritchard (1902–1973), who in 1946 succeeded Radcliffe-Brown as professor at Oxford, shared Fortes' commitment to structural-functionalist kinship studies in the first half of his career, but struck off in a different direction during the 1950s (Chapter 6). Like most British anthropologists of his generation, Evans-Pritchard had studied with Malinowski at the LSE, but his teachers also included the Sudan specialist Charles Seligman, of the Torres expedition. It was under Seligman's supervision that Evans-Pritchard carried out his field studies in the Sudan during the 1930s, mainly among the Azande and the Nuer. Evans-Pritchard and Malinowski were never close. Malinowski revelled in poetic, detailed and evocative descriptions of Trobriand life, while Evans-Pritchard had a passion for social theory and sharp, elegant and logically coherent intellectual analysis. Evans-Pritchard was an exception to the rule that the early anthropologists were 'outsiders'. This 'very English Englishman, despite his Welsh name',

as Leach (1984) describes him, indeed seemed to personify the British upper classes (though his father was indeed a Welsh-speaking clergyman), from which Malinowski, as a foreigner (though an upper-class foreigner), was forever barred.

Back in 1931, Evans-Pritchard had been duly impressed by Radcliffe-Brown, who had stopped briefly in Britain en route from Sydney to Chicago. However, he was never a mere student of the Durkheimian master. When Radcliffe-Brown returned to Oxford as professor in 1937, Evans-Pritchard already held a lectureship there, and had not only done extensive fieldwork among several peoples in the Sudan, but had worked for three years as professor of sociology in Cairo. His first book, *Witchcraft, Oracles and Magic among the Azande* (1937), was published in the year of Radcliffe-Brown's return, and immediately recognised as a masterpiece. The monograph deals with witchcraft and related beliefs among an agricultural people in southern Sudan, and the thrust of its analysis, one of the most celebrated and discussed in anthropology, is two-pronged: on the one hand, witchcraft is seen as a 'safety valve' that redirects social conflicts into harmless channels, an integrating device in the best Durkheimian tradition; on the other hand, it is a bold attempt to make sense of an alien world of thought, presented in its own terms. What is remarkable is the way it knits these two approaches together into a seamless whole. The belief system not only stabilises and harmonises the social order, but is rational and consistent, given the logical presuppositions of Zande thought. Evans-Pritchard emphasises the prosaic, self-evident and practical side of these beliefs. Thought and faith are not abstract, outside the concreteness of everyday life, but an inseparable part of that life. Some commentators (Winch 1958) have made much of the structural-functionalist dimension of Evans-Pritchard's work, arguing that he reduces witchcraft beliefs to their 'social functions'. Others (Douglas 1980; Feyerabend 1975) have emphasised the opposite, that Evans-Pritchard demonstrates that knowledge and belief are social products *anywhere*.

Evans-Pritchard's second major work appeared in 1940, the same year as the publication of the volume on African political systems that he edited with Fortes. *The Nuer*, a study of the political organisation of a patrilineal pastoralist people living just north of the Azande, is written more in Radcliffe-Brown's spirit. The book sets out to address a central problem in the anthropology of acephalous ('stateless', literally 'headless') peoples, namely how large-scale political mobilisation can occur in the absence of

centralised leadership. The book, which vividly evokes the lifeworld of the Nuer, is also a *tour de force* of 'kinshipology'. Conflicts are organised along kinship lines. The principle of *segmentary organisation* – 'it is me against my brother, me and my brother against our cousins, brothers and cousins against third cousins', and so on – was central to the analysis, which also demonstrated the conflict-reducing influence of relationships (such as marriages) that cut across the patrilineal system – an aspect fully exploited in a reanalysis by Gluckman (1956). In the final chapter, Evans-Pritchard elaborates his view of social structure, which he defines along the same lines as Radcliffe-Brown, as an abstract system of social relationships which continues to exist unchanged in spite of changes of personnel. The continuity from the Azande monograph is also notable. Evans-Pritchard considered kinship and witchcraft as two examples of 'modes of thought', and in both cases is concerned to show how thinking is related to what Pierre Bourdieu (1990) would later call 'the logic of practice'.

Fortes' and Evans-Pritchard's monographs on the Tallensi, the Ashanti, the Azande and the Nuer, were, after Seligman's pioneering work, crucial in transferring the regional focus of British social anthropology from the Pacific to Africa, but it should be kept in mind that other important British anthropologists also worked in Africa at the time – Richards among the Bemba, Schapera among the Tswana, Gluckman among the Zulu, Forde among the Yakö. *African Political Systems*, which included contributions from about a dozen British anthropologists, was a powerful demonstration of this new regional emphasis. In their much-quoted introduction, the editors distinguished three types of African societies: egalitarian, small-scale societies (largely hunters and gatherers); state societies (such as the Buganda kingdom); and the interesting, intermediate type, the lineage-based segmentary society, of which the Nuer became the paragon, which is decentralised but able to form large, cooperative groups for specific purposes (such as war). As we shall see, segmentary society preoccupied anthropologists for decades, and not only in Britain (see Sahlins 1968). During the great debates on kinship the 1950s and 1960s, the model presented in *African Political Systems* was subjected to criticism from many quarters. Some felt that it was simply too neat to fit the complexities of real life. Others disparaged it as evolutionism in disguise. Yet others (most prominently Lévi-Strauss) rejected its exclusive focus on *descent* as the main principle of kinship.

But neither was structural-functionalism a static doctrine; it was a theory that evolved and ultimately transcended itself. The 1940s open with the publication of two important books on African kinship – *The Nuer* and *African Political Systems*. The decade ends with three equally important publications, from the same region and with the same subject matter – Fortes' *The Web of Kinship among the Tallensi* (1949), Radcliffe-Brown and Forde's *African Systems of Kinship and Marriage* (1950), and Evans-Pritchard's *Kinship and Marriage among the Nuer* (1951b). The virtuoso self-assurance of the two first books contrasts sharply with the volumes that appeared in 1949–51. All three books distinguish, in Evans-Pritchard's words, between *kinship* and *descent*. Kinship belonged to the domestic sphere. Descent was public and jural. The *kinship system* stood as a stable corpus of categories that connected the domestic with the public. But the categories were not real, as Radcliffe-Brown had supposed, but 'ideal types' (in Weber's terms). They might be changed, reinterpreted or replaced, and the public and domestic spheres both contained factors that invited people to manipulate the system in their own interests: the man I claim as my cousin today, when my family needs help to tend the fields, may be distant kin tomorrow, when my father's inheritance is divided. The new studies of *domestic kinship* thus brought to the fore the tension between household-external and household-internal forces, between the strategic play of politics and everyday demands. The change was fundamental, and Radcliffe-Brown evidently never accepted it (Kuper 1996: 92). His stable, positivist social structure had become a web of forces and counterforces, ambivalent categories and criss-crossing loyalties. The sharp focus on political structure around 1940 was giving way to a broader understanding of the complex interaction between local and super-local processes.

One of the most important contributions of lineage research was that it made the informal aspect of political organisation and leadership explicit. Informal politics are not reserved for lineage-based societies, but play a role everywhere in the modern world. In the 1940s, anthropologists still resisted this generalisation. But views were changing. Kinship studies had commenced their long journey toward the intricate and self-reflexive subfield they are today.

FUNCTIONALISM'S LAST STAND

In 1930, the undisputed centre of British anthropology was at the LSE, where Malinowski presided with his many gifted students,

under the tolerant auspices of Seligman. In 1940, Oxford had become a Radcliffe-Brownian preserve and was rapidly moving towards hegemony. By 1950, Radcliffe-Brown's protégés had secured jobs at Cambridge, Manchester, and University College London, and the Malinowskians seemed to have lost the competition for academic control. But Malinowski's heritage was not dead. The LSE remained a Malinowskian stronghold, and many of Malinowski's talented students were active and respected voices in the field; the most famous, no doubt, besides Firth, being Edmund Leach (Chapter 5).

It was above all Raymond Firth (1901–2002) at the LSE who secured the direct continuity of Malinowski's programme. After initial work on the Maori of his native New Zealand, Firth arrived in London to study economics, but transferred to anthropology after attending Malinowski's very first seminar (Stocking 1995: 407). He did pioneering work in economic anthropology, which would later have considerable influence. But in spite of his theoretical contributions (Chapter 5), Firth was first of all an empirical researcher. Like his mentor, he regarded interaction and the ongoing flow of social life as more fascinating (and more 'real') than abstract structures. He published eleven books about the Tikopians, the inhabitants of a Polynesian island where he carried out long-term fieldwork on three occasions. His most famous monograph, *We, the Tikopia* (1936), is a 600-page volume which typifies both the strengths and the weaknesses of Malinowskian anthropology. Structural-functionalists were full of disdain for Firth's allegedly wishy-washy, all-inclusive accounts, which made no attempt to develop elegant models or even to accord some institutions priority over others. However, the book's long, detailed ethnographic descriptions documented the actual complexity of 'primitive' life far better than the stringent structural-functionalist accounts.

Firth's monographs are typical Malinowskian studies, along with Reo Fortune's work from Dobu Island (near New Guinea), Isaac Schapera's books from Bechuanaland (Botswana) and Audrey Richards' work from Northern Rhodesia (Zambia). These works all assumed that society was an integrated, functional system, but rarely specified the mechanisms of its integration in detail. The emergent Oxford anthropology appeared more polished, more scientific, more coherent and in every way theoretically superior. However, the last word had not yet been said: Firth, like Malinowski, was a methodological individualist who focused on the daily life of living people rather than on the abstract, legal (Radcliffe-Brown) or logical (Evans-Pritchard) principles that governed it. The ever-changing,

tactical play among individual actors is Firth's main concern, and this made him popular as an ancestor figure for the generation of methodological individualists that came to the fore in 1950s and 1960s.

SOME BRITISH OUTSIDERS

British anthropology was a tight-knit little community, closed, elitist and full of conflicts. Yet, in the course of few years, it produced some of anthropology's greatest classics. But the milieu was indeed closed, and tended to marginalise anyone who did not belong to either the structural-functionalist or the functionalist camp. A good example is A.M. Hocart (1884–1939), whose work on Pacific societies in the interwar years, influential at the time, was later forgotten, but has occasionally been rediscovered more recently. Hocart carried out research in the Pacific in 1909–14, primarily in Fiji, but also in Tonga and Samoa. His concerns were both historical and subtly structural, and he was far removed from both Malinowski's brisk pragmatism and Radcliffe-Brown's search for simple 'laws' and 'mechanisms'. His chief interest lay in ritual and social hierarchies, and he developed a comparative method that has more in common with French anthropology than with his British contemporaries. His innovative book on caste (Hocart 1938) was published in French translation before it appeared in English, and is more often cited in French than in Anglophone literature. Hocart never found academic employment in Britain, but succeeded Evans-Pritchard as Professor of Sociology in Cairo in 1934, where he remained until his early death.

Other outsiders include the Austrian Siegfried Nadel (1903–1956), a musicologist and Africanist, and a pioneer of psychological anthropology in Britain, who ended as professor at the Australian National University in Canberra. Another was Daryll Forde (1902–1973), who was even more marginal than Nadel, having studied archaeology in Britain and later cultural anthropology with Kroeber and Lowie at Berkeley. Returning to Britain with an unfashionable penchant for ecological studies, he found a powerful ally in Evans-Pritchard, and was appointed as Chair at University College London in 1945.

However, the most interesting young British outsider of the interwar years was Gregory Bateson (1904–1980). Bateson came from an academic, upper-middle-class family. His father, the famous biologist William Bateson, had named his son after Gregor

Mendel, the founder of genetics. Bateson was studying to become a biologist, when Haddon, during a conversation on a train bound for Cambridge, converted him to anthropology (Lipset 1982: 114), and he soon embarked on fieldwork in New Guinea. After an unsuccessful attempt at fieldwork among the Baining, Bateson studied the Iatmul, a lowland people whose *naven* ritual formed the backbone of his first (and only) ethnographic monograph, *Naven* (1936).

While in New Guinea, apparently in a canoe on the Sepik river, Bateson met Reo Fortune and his wife, Margaret Mead, who were doing fieldwork in the area. The description of this meeting (Mead 1972; M. Bateson 1984) has become a classic in the history of anthropology. The encounter was intense in every way. The three eagerly discussed anthropology and life in general, argued about the differences between the peoples they were studying, and fearlessly analysed their personal relationships. When the dust settled, Fortune and Mead were divorced, while Bateson married Mead and moved to the United States in 1939.

The meeting of Mead and Bateson highlights the relationship between British and American anthropology in these years. Bateson's admiration for Radcliffe-Brown's elegant intellectualism was challenged by Benedict's insight into psychology and emotions. What was the role of the anthropologist – to uncover sociological principles or describe the subtleties of human communication? Did the one exclude the other? Or did there exist a common language that could encompass both? Bateson's monograph is an expression of these dilemmas. The *naven* ritual of the Iatmul, involving gender reversal by both sexes and spectacular homosexual displays by the men, was approached by Bateson from three distinct analytical perspectives. The first was 'sociological and structural', inspired by Radcliffe-Brown. The second he called 'eidos' (a culture's cognitive and intellectual style), and the third 'ethos' (from Benedict). He found it very difficult to reconcile, not to say synthesise, these three approaches, and eventually abandoned the effort. *Naven*, as it was originally published in 1936, thus stands as an unsolved riddle. Only in 1958 did the book come in a second edition, with a long appendix, where Bateson tried to tie the disparate strands together.

Bateson's monograph was an ambiguous work that had little impact on contemporary anthropology. His British colleagues did not know what to make of it (Kuper 1996), but its prestige has increased as it has become clear that it anticipated several of the changes that have occurred in the discipline since the 1950s. Thus, Bateson is critical of the idea of 'function', which, in his

view, is teleological (it implies that the effect precedes the cause). Functionalist explanations should always be examined closely, to see whether they in fact specify all the links by which the 'purposes' and 'needs' of the whole are communicated to the individual actor. This will lead us to focus on process and communication rather than function and structure.

Bateson was an exceptional intellectual who still elicits admiring commentaries, some of them book-length (such as Harries-Jones 1995). After the war, his concern with communication and process would bring him into contact with brilliant scholars on the frontlines of many fields: psychiatrists, psychologists, ethologists, mathematicians, ecologists, biologists, and so on. He soon became an interdisciplinary figure, who made significant contributions to subjects such as psychology and communications theory (Bateson 1972), and pioneered the use of cybernetic models in anthropological explanation. Even before the Second World War, his 'photographic fieldwork' with Mead on Bali indicated his willingness to explore the limits of anthropology. During the war, Bateson contributed to Benedict and Mead's studies of 'national character' and started work on an influential theory of communication, both in anthropology and elsewhere (Chapter 6).

It seems appropriate to end this chapter with a few words about Bateson's early career. An iconoclast and an eccentric throughout his life, Bateson's first stab at theoretical synthesis was an attempt to bridge the gap between interwar American and British anthropology. He himself regarded it as a failure. This should remind us that the gulf between the two traditions was very real. While Americans steeped in Boasian cultural anthropology studied cultural history, collective psychology and ethnolinguistics, the British zoomed in on social life, status relationships, kinship systems and politics. Dominant tendencies in French anthropology followed a third path, to which we shall return in Chapter 6. Although every self-respecting historian of anthropology will claim that anthropology was, after all, one discipline, the Atlantic, and even the English Channel, were effective lines of demarcation in 1945. And while it would be simplistic to claim that these boundaries remained intact throughout the second half of the century, it would be naive to assume they had disappeared. The three national traditions continue to mark anthropology to this day, albeit increasingly enriched by mutual contact with other national traditions of anthropological enquiry –

some old and established, as in Russia, China, South Africa or the Netherlands; some more recent, as in Brazil, Scandinavia or Japan.

In the next two chapters, the chronological structure of this book will be temporarily upset. Both chapters deal with the 20-odd years between the end of the Second World War and the new radical movements that caught on in the late 1960s. While Chapter 5 presents the mounting critique of the established paradigms on both sides of the Atlantic and some new, less collectivistic, alternatives, Chapter 6 shows how the power of symbols and rituals became an issue in all three national traditions, bridging old divisions and creating new ones in the process.

5
Forms of Change

The guns are silent, the bombers grounded. Millions of refugees pick their way through Germany's ruined cities, across the scorched earth of Russia, Poland, Belarus and the Ukraine. France and Britain have been deeply shaken; their great empires will soon be only a memory. By contrast, the American economy is just settling down into superpower gear, wheeling out an ever-increasing plenitude of pink Cadillacs, TV sets, film stars and nuclear weaponry. To the East of Europe, the Soviet Union will compete successfully with 'the free world' in the production of military hardware, while the production of Cadillacs (pink or otherwise) lags ever further behind. McCarthy looks for communist spies; Beria looks for capitalist spies. The atmosphere is tense, as people peer into an unprecedented future, which, against the background of the horrors of the recent past, promises progress and Formica kitchens, or threatens with global annihilation.

Not only in the economy, but in most of the sciences as well – including anthropology – the USA was becoming the leading superpower, with more academics, larger research funds, better facilities, more journals and conferences than anywhere else. In the 1950s, academics in countries like Norway, who had hitherto published in German to reach an international audience, switched to English.

The racist views of Nazism had been discredited, and many felt that it was about time to abandon the concept of race in science as well. Almost, but not quite all, geneticists and biologists agreed that racial differences were not comprehensive enough to have any effect on culture. Virtually all social and cultural anthropologists held this view, as indeed they might, since their discipline had since Bastian and Tylor rested on the premise of the 'psychic unity of mankind'. When international anti-racist proclamations came to be written and signed, it seemed reasonable to involve anthropologists in the paperwork. In this way, a British émigré to the USA, Ashley Montagu (1905–1999), a Boasian with a PhD from Columbia, became the secretary at a United Nations Educational, Scientific and

Cultural Organisation (UNESCO) conference about race in 1950. The resulting document, 'Statement on Race', in no uncertain terms proclaimed that biological factors were of negligible importance in shaping human nature.

The winds of change following the Second World War were emphatically universalist; they announced the unity of mankind and equal human rights. To the extent that anthropologists related to this ideological trend – and many did – they were ambivalent. On the one hand, the culturalist, anti-racist views promoted not least by Montagu in a series of popular and influential books were by and large seen as uncontroversial, even trivial. Most anthropologists were probably also in favour of decolonisation, another universalist project. On the other hand, anthropologists steeped in cultural relativism found it difficult to accept the unwarranted missionary zeal seemingly inherent in the new, universalist rhetoric, whether it emanated from anti-colonial movements, the US State Department or the United Nations. In 1947, the American Anthropological Association (AAA) made a lengthy statement, published in *American Anthropologist* and written mainly by Melville Herskovits, which amounted to a warning against the cultural imperialism ostensibly inherent in the incipient Universal Declaration of Human Rights (AAA 1947). This statement indicates the strong position of the Boasian programme in American anthropology at the time (Wilson 1997; Wilson and Mitchell 2003). But the Americans were not alone. As late as in 1976, Lévi-Strauss stated that the Western concept of freedom was an historical project without universal relevance. In 1948 he attacked the Universal Declaration of Human Rights for being meaningless outside the West (Pace 1986).

Anthropologists were also sometimes co-opted by American government interests, and some delivered strategic knowledge to the CIA. Although far from uncontroversial, these practices – unlike politically radical views – rarely denied anyone tenure.

Shortly after the war, a powerful alternative to the Boasian consensus would develop in the USA. Its obvious debt to Marx was rarely acknowledged explicitly, since being a Marxist in the postwar USA was not a smart move for an academic who wanted tenure and research grants. Instead, its originators tended to look to Morgan as a founding father.

The year 1946 may be seen as a gateway to the lively, expansive period that anthropology now entered. The Norwegian anthropologist Fredrik Barth started his studies in that year, in Chicago. Decades later, he describes the atmosphere as electric,

with the university flooded with returning soldiers studying on GI grants, who supplied an influx of new blood into American academia that was unprecedented (Eriksen 2013). It was the year when the British formed the Association of Social Anthropologists (ASA), the year when Evans-Pritchard succeeded Radcliffe-Brown at Oxford and Kroeber retired from Berkeley after having taught there for 45 years, and it was also the year in which Julian Steward began to teach at Boas' old department at Columbia. 'The revolution' in the discipline was over, the initial phase of institutionalisation was perhaps over too, and change was again in the air. In the space of a few years, Steward's neo-evolutionist programme would challenge Boasianism on its home turf; Evans-Pritchard would repudiate structural-functionalism; Gluckman would become professor at the new anthropology department at Manchester, soon to be known for its political radicalism and its interest in social change; and Lévi-Strauss' great study of kinship, published in 1949, would change anthropology's discourse on its favourite institution forever.

Although anthropology branched off in many directions in the decades following the war, it also became more tightly integrated than before, thanks to the continuation – and internationalisation – of core debates. Differences remained, but mutual knowledge across national boundaries was becoming more widespread. The annual meetings of the AAA gradually turned into global meetings, and familiarity with each other's journals was increasingly taken for granted.

It would be futile to impose a simple linear narrative upon the complexities of the two decades following the war. It was a period when the New Guinean highlands replaced Africa as the most fashionable place to go for young fieldworkers, when the Caribbean and Latin America came into their own as ethnographic regions, when structuralism became a power to reckon with, interpretive anthropology had its breakthrough, and new forms of symbolic, political and economic analysis made their entry.

In this book, we have divided our treatment of the 1950s and 1960s into two thematic chapters. The present, and longest, deals with a turn that took place in British and American anthropology, toward the practical, often material, circumstances that are the precondition of social life. The next chapter will deal with an opposite turn, toward symbolic communication and meaning, which was most strongly expressed in France, but had important repercussions for the two other traditions as well. While this editorial separation reproduces a debatable dichotomy between society and

culture, it also highlights divergences and confluences among the evolving national traditions. American anthropology, for a while nearly synonymous with Benedict-Meadean studies of 'culture', sprang from an original holistic impulse, a Tyloresque 'culture', in which, naturally, both social organisation and the material world played a considerable role. Now, this breadth reasserted itself with the new materialists. French anthropology, which Durkheim had defined in a wide, sociological sense, had, through Mauss, arrived at the fascinating problem of exchange. Exchange, usually thought of in economic terms, can – *pace* Mauss – be redefined as communication. Enter Lévi-Strauss, and the focus of the discipline seemed to move from sociology to semiotics. Finally, the British, who stubbornly clung to the sociological definition of their subject, once again imported a French theory, as they had done before with Durkheim. There was continuity and change in these movements. The distinctions between the national traditions began to blur, but were not erased.

NEO-EVOLUTIONISM AND CULTURAL ECOLOGY

Although the emergent materialist school in American anthropology was explicitly opposed to Boas, several of Boas' own collaborators and students were closer to the new thoughts than one might think. At Berkeley, Kroeber was at least non-committal, and his colleague Lowie even expressed sympathy for the evolutionist project, though his most famous book, *Primitive Society* (1920), contains a scathing critique of Morgan's *Ancient Society*. Like Boas himself, Lowie was primarily a cautious, empirically oriented scholar, with an overwhelming respect for the facts. He was also a learned cultural historian, who rejected Benedict's ideas of 'national character' as vague and speculative, and regarded diffusionism as a more attractive explanation of cultural change than evolutionism, since its assumptions were simpler and easier to test against fact. But Lowie did not reject evolutionism completely. Though he declined to generalise on the subject, he appears to have accepted that cultures, in some cases, evolve along the same general lines, a view ambivalent, if not exactly opposed, to Boas' historical particularism. Lowie also introduced the term *multilinear evolution*, a concept later recycled to good effect by his student, Steward. Contrasting it with the *unilinear evolution* typical of nineteenth-century anthropology, Lowie held that evolution might proceed along different paths. Between these paths there were certain rough similarities, but also

considerable variations. When Steward embarked on his project of modernising evolutionism, he could thus draw on his teacher for inspiration and – at least tacit – support.

As Jerry Moore (1997: 166) points out, historical and evolutionist perspectives were more easily accepted in the USA than in Britain, where social anthropology had come to mean synchronous studies exclusively. Social change was not a concern either there or in France, where it would enter anthropology only in the 1960s through the British-inspired work of the Africanist Georges Balandier and his students. And, with the exception of Daryll Forde at University College London, a single, brilliant chapter in *The Nuer*, and Barth's 1956 article on ecology and ethnicity in Swat, ecology was also almost completely absent from British anthropology.

When neo-evolutionism came to the fore in the USA in the 1950s, this was largely due to the work of two men: Julian Steward (1902–1972) and Leslie White (1900–1975). Unlike most of his contemporaries, White, who taught for 40 years at the University of Michigan (1930–70), rejected the cultural relativist idea that cultures cannot be 'ranked' on a developmental scale, though he also emphatically rejected the moral connotations that the Victorian evolutionists had associated with such schemes. White was interested in general laws of cultural evolution. Like Malinowski, he held a bio-functionalist view of culture but, as he saw it, the function of culture was not to provide individual need satisfaction, but to ensure the survival of the group. His methodological collectivism might remind us of Radcliffe-Brown, but White did not believe in Durkheim's tenet that societies were autonomous entities with their own, self-sufficient dynamics. Societies were integrated in their ecological surroundings. White distinguished between technological, social and ideological aspects of culture (he was later to add 'emotional or attitudinal aspects' – a nod in Benedict's direction; see Sahlins 1976). The technological dimension was crucial; in fact, he argued that it *determined* the social and ideological aspects of social life (White 1949).

The originality of White's theory was modest, although his single-minded technological determinism was often expressed in original ways, as when, in *The Science of Culture* (1949), he defined the level of cultural development as the amount of energy harnessed by each inhabitant, measured through production and consumption. Such quantitative ambitions had been absent in nineteenth-century evolutionism and in Boasian cultural anthropology, but would become prominent among the new American materialists.

White's views met with considerable resistance. More than once, he was identified as a possible Communist during the McCarthy era of the 1950s. Among cultural anthropologists, White's ambition to turn anthropology into an accurate science of cultural evolution and the sociocultural effects of technology, was regarded as irrelevant. Nevertheless, White developed an excellent department at Michigan, and his students included luminaries like Marshall Sahlins (who later went to Columbia to study with Steward), one of the great figures of late-twentieth-century American anthropology.

Lowie, the crypto-evolutionist, had strong reservations about White's technological determinism, but encouraged Steward to pursue a version of materialist evolutionism, which, though less deterministic, had much in common with White's. Steward himself, after completing his PhD at Berkeley – a standard Native American study in the culture-and-personality style – worked as an archaeologist for years, before transferring to Washington, DC, where he directed the Institute of Social Anthropology at the prestigious Smithsonian Institution and edited the seven-volume *Handbook of South American Indians*, to which the French anthropologist Alfred Métraux made major contributions. Steward refined his theoretical approach during the 1930s and 1940s, and when he arrived at Columbia University in 1946, he brought along a mature theory which provoked his colleagues and inspired his students. During his six-year stay at Columbia (which coincided nearly exactly with Karl Polanyi's years there), Steward supervised a truly impressive group of graduate students, who were soon to put the mark of a new materialism on American anthropology. Elman Service, Stanley Diamond, Morton Fried, Eric Wolf, Sidney Mintz, Eleanor Leacock, Marvin Harris, Robert Murphy, Marshall Sahlins, Andrew Vayda, Roy Rappaport and others, all studied under Steward (or under his successor, Fried) and some participated in his many large and ambitious projects.

Steward was dissatisfied with the lack of theoretical ambition among Boasian anthropologists, and, like White, saw a potential for development of general theory in the study of technology and ecological conditions. Like Lowie, however, he was unenthusiastic about unilinear cultural evolution. Furthermore, where White distinguished three cultural subsystems, Steward opposed the cultural 'core' to 'the rest of culture'. The core consisted of technology and the division of labour – which corresponds nicely to Marx's definition of infrastructure, an influence that Steward, like

White, did not advertise too loudly. It would be their students who made the link with Marxism explicit in the less totalitarian1960s.

Steward's influence may have been even more powerful in archaeology than in anthropology, but at least three of his contributions have had a lasting impact, particularly on American anthropology. First, Steward founded modern *cultural ecology*. Though White, too, included environmental factors in his explanations, Steward regarded the totality of a society and its biological surroundings in roughly the same way as an ecologist regards an ecosystem. He saw society through the eyes of an ecologist. *Adaptation* was a core concept with Steward, who searched for institutions that concretely furthered a culture's survival in a given ecosystem. Some of these institutions were strongly determined by ecology, technology and population density; others were relatively unaffected by material conditions.

Second, Steward developed a theory of *multilinear evolution*, based on archaeological, historical and ethnographic evidence. Under particular conditions, such as irrigation agriculture in arid regions, he held that the cultural core would develop along roughly the same lines in different societies. By limiting his generalisations to a few important aspects of the cultures he studied and restricting the scope of his theory to societies with comparable natural preconditions, he succeeded in building up an evolutionism that did not lead to speculative generalisations that could easily be falsified. Neither Steward nor White regarded all aspects of the superstructure or symbolic realm as materially determined, unlike some of their predecessors as well as successors, Marxist and non-Marxist alike.

Third, Steward was, along with Redfield (whose orientation was definitely non-materialist) an important pioneer in *peasant studies*. Peasants (defined as subsistence farmers in complex societies who are partly integrated into non-local economies) have until recently made up the largest population category in the world. The lack of interest in them in pre-war anthropology confirms that the discipline was still hunting for the exotic at the expense of the typical. Steward's peasant research reached a high point during the large-scale Puerto Rico project he organised in the late 1940s. The project was one of the first area studies in anthropology, and was unique at the time for its integration of local and regional analysis. Here, for just about the first time in the history of anthropology, the nation state and the world market figure actively in the analysis. Steward's students would continue and refine Steward's interest in peasant societies in the coming decades, and make decisive contributions in turning

mainstream anthropological attention towards the Caribbean and Latin America in the 1960s and 1970s.

The most important result of White's and Steward's theoretical efforts was not their evolutionism, but the interest they inspired in the relationship between society and ecosystem. The emerging school of cultural ecology has often been described as just another form of functionalism, where the ecosystem replaces the social whole as the prime functional imperative. But this critique is only partly justified. The cultural ecologists were interested in cultural change, and, with the passage of time, developed a more sophisticated model of society than their British predecessors. In this, they were assisted by the great advances made in (biological) ecology during the 1950s, particularly as a result of the application of cybernetic models to problems of adaptation. In the 1960s, cultural ecology would prove a diverse source of inspiration among anthropologists. Bateson drew on models and ideas from cultural ecology in his contributions to general systems theory. Clifford Geertz, later known for his Weberian interpretive anthropology, published *Agricultural Involution* in 1963, a book on Javanese land tenure strongly influenced by Steward. Sahlins, who would also later move towards symbolic anthropology, began his career with several books which elaborated Steward's (and White's) interests, and, in a famous article on political leadership in the Pacific, saw the contrast between Melanesian big-men and Polynesian chiefs in evolutionist light, drawing on an analysis of household economy in accounting for political variations. The most consistent (and persistent) of Steward's and White's successors was nevertheless Marvin Harris (1927–2001) who, during the 1960s, developed his own brand of materialist evolutionism, which he referred to as cultural materialism (Harris 1979).

The high point of cultural ecology was, perhaps, Roy Rappaport's monograph *Pigs for the Ancestors* (1967), which quickly became a classic. Rappaport (1926–1997), a student of Fried's at Columbia and a friend and associate of Bateson and Mead, carried out fieldwork among the Tsembaga Maring of the New Guinean highlands in the early 1960s. He was particularly concerned with understanding a complex ritual cycle, involving warfare and mass slaughter of domesticated pigs, along with colour symbolism and the ceremonial planting of a *rumbim* tree. Applying a cybernetically inspired ecological analysis to the ritual, he demonstrates the intimate connections between Tsembaga adaptation to their surroundings (nature, but also neighbouring human groups) and

their worldview. Starting with the White-inspired premise that the availability of energy sources determines cultural adaptation, he ends with a subtle (and non-deterministic) analysis of the aesthetic language through which the Tsembaga conceptualise the ecology they inhabit. Many critics considered the book a kind of ecological structural-functionalism, with little space for individual motivation and independent cultural dynamics. Rappaport responded with a long postscript in the book's 1984 edition.

Another climactic point of cultural ecology movement, which also made manifest its great breadth and scope, was the 'Man the Hunter' symposium organised at the University of Chicago in 1966 (Lee and DeVore 1968). Concentrating chiefly on contemporary hunters and gatherers, most of the contributors – largely American cultural anthropologists – saw culture chiefly in terms of ecological adaptation. They argued that since hunting was the original livelihood of humanity, any general theory of society and the nature of Man would presuppose a close knowledge of the hunter's way of life. In addition to highlighting the familiar tension between culturalist and materialist accounts of culture and society, the symposium indicated how far from Boas and Benedict parts of American cultural anthropology had by now moved.

FORMALISM AND SUBSTANTIVISM

The emergent interest in material conditions was also expressed in other ways than through cultural ecology, and not only in the USA. From the 1940s onwards, economic anthropology was developed as a sub-discipline, in many cases in close tandem with peasant studies (Wolf 1966).

Anthropological studies of economics had a venerable history. Malinowski's *Argonauts of the Western Pacific*, Mauss's *The Gift*, and many lesser works focused directly on economic relations. Still, the pioneer in establishing economic anthropology as a sub-discipline was Raymond Firth (Chapter 4). Firth, who was first educated as an economist, had written detailed studies of the Maori and Tikopia economies (Firth 1929, 1939), which emphasised the pragmatic strategies of individuals. During the 1950s and 1960s, Firth continued this work, adding a theoretical emphasis to it that is often referred to as 'formalism' (LeClair and Schneider 1968). Formalism, which argues that classical economic theory may be applied cross-culturally, did not, however, crystallise into a distinct 'school' before it was challenged by 'the substantivist revolution'.

If Firth was the first formalist in economic anthropology, the apical ancestor of substantivism was the Hungarian economic historian and political reformer Karl Polanyi (1886–1964). After his exile he lived some years in London, then he moved to the USA in 1940, where he spent six years as professor of economics at Columbia (1947–1953) that coincided with Steward's term at the anthropology department there. In *The Great Transformation* (1944), Polanyi argues that what we call 'the economy' does not exist in pre-capitalist societies at all, and that classical economic theory can therefore legitimately be applied only to capitalist economies.

The heated debate between formalists and substantivists involved anthropologists, historians and economists, and continued well into the 1970s, when it concluded inconclusively with the rather banal realisation that the schools were complementary. Nevertheless, the issues raised were fundamental, and, in various guises are still around today. Roughly speaking, the formalists assumed that an economy might be described as a particular *kind* or 'form' of action that individuals have everywhere and are always engaged in – action in which the individual strives to achieve the maximal benefit for himself or his household. As long as we are dealing with maximisation, we are dealing with economic phenomena. Moreover, since maximisation is not limited to specific empirical contexts, but can occur in any kind of social interaction, the economy may be regarded as a *universal aspect* of human social life. This attitude, which (despite its universalist definition) is compatible with methodological individualism, would be embraced by many of the critics of structural-functionalism in the 1950s and 1960s. In contrast, substantivism argues that the economy is not a universal 'form' of action, but is defined by its *substance*. In Polanyi's words, it is an 'instituted process' (Polanyi 1957). The economy is contained by and limited to specific, historical institutions, the main categories of which are production, circulation (exchange) and consumption.

The strength of formalism was its emphasis on the pragmatic choices of individuals – which brings the variable and unpredictable aspects of economic action to the fore. The strength of substantivism lies in its ability to describe economic systems as being of different kinds and having different economic rationalities (formalism only accepts one economic rationality: maximisation). Substantivists would be more open to theories of historical watersheds, fundamental differences between cultures and irreversible change. Formalists would be more gradualistic. Polanyi himself, in a typology that

was later expanded by Sahlins (1972), distinguished three basic types of economy: *reciprocity*, *redistribution* and *market exchange*. In reciprocity, which is typically found in small, non-hierarchical, kinship-based societies, there is no short-term calculation of profit or loss, and – as Mauss pointed out in *The Gift* – it is the giver rather than the receiver who wins prestige. In *redistribution*, typical of traditional chiefdoms, goods are collected at a centre, from which they are distributed to the population on the basis of the centre's priorities – often in conspicuous displays of 'generosity'; here again, the giver wins. Only in market exchange, typical of capitalist societies, is this relationship reversed: the receiver wins, accumulates value and reinvests it in an endless cycle of profit maximisation, in which money plays a pivotal role. Each of these three 'ideal types' is based on particular institutions (kinship, the state, money), and may be found mixed with the others in empirical societies. There are elements of market exchange in kin-based societies, just as there are elements of gift exchange in our own economy. Polanyi, however, focused particularly on situations in which one or the other type of economy was dominant, and thus arrived at a vaguely evolutionist model of social development incorporating three stages (a common model, as we saw in Chapter 2).

To the formalists, like Firth and Herskovits, this evolutionist tendency was anathema (Frankenberg 1967), and they tried to show that maximisation regulated economic activities everywhere. The substantivists regarded this as ethnocentric (Sahlins 1972), and their favourite classic was Mauss, who had emphasised the differences between dominant logics of action in different types of society.

The controversy fizzled out as Marxian economic thinking (which sought to incorporate both substantivist and formalist approaches) gained currency. But analogous controversies have continued to surface in other parts of anthropology, for example in discussions of ritual, where Leach (1968) defined ritual not (substantively) as confined to a particular kind of institution, but as a formal communicative *aspect* of any action. Similarly, the debate on alliance and descent theory (Chapter 6) opposed a view of kinship as a (formalist) 'alliance building activity' to an idea of kinship as a method of formation of (substantial) 'corporate groups'. It might even be argued that postmodernism (Chapter 8) was a peculiar type of formalism, inasmuch as it sought to do away with essentialisation – the tendency to regard diffuse formal aggregates of process as distinct (substantive) 'things'.

THE MANCHESTER SCHOOL

By 1950, thanks to institutional expansion and retirements, the leading figures in postwar British social anthropology were nearly all firmly in place at central academic institutions: Firth succeeded Malinowski at the LSE in 1944; Daryll Forde became professor at University College London in 1945; Evans-Pritchard at Oxford in 1946, Gluckman at Manchester in 1949 (a couple of years after leaving the Rhodes-Livingstone Institute), Fortes at Cambridge in 1950, and Schapera at the LSE, also in 1950. Leach was appointed to a lectureship at Cambridge in 1953.

There were important differences within this elite, which must nevertheless have seemed a tightly integrated clan seen from the outside, not least from the sprawling field of American anthropology. In a debate with Firth in 1951, George P. Murdock accused the British of closing ranks and refusing to engage in the discourse of global (by which he perhaps meant American) anthropology (Stocking 1995: 432ff.). However, the internal differences in British anthropology were real enough. Fortes, Evans-Pritchard and Forde remained associated with structural-functionalism, although the latter two increasingly chose alternative paths – Evans-Pritchard repudiated Radcliffe-Brown's ideals of a 'natural science of society' (Chapter 6), while Daryll Forde, who was drawn to ecological anthropology during his student days at Berkeley, pursued this interest at University College London, where he founded and led a department that was unique in Britain for its ecological and material orientation. Firth, Richards and Leach developed very different brands of Malinowskian functionalism; in Leach's case with a structural twist. Finally, Gluckman and Schapera occupied a middle ground. They were self-declared structural-functionalists, but their thematic interests were closer to those of Leach and Firth, who, like them, were vitally concerned with the study of social change. Of these leading figures, Gluckman, Leach and Evans-Pritchard would be most directly involved in changing the face of British anthropology.

The 1950s and 1960s saw fundamental changes in the British tradition. Some of the most important, notably the turn 'from function to meaning', will be dealt with in the next chapter. Here we shall concern ourselves with a second change of importance, the turn 'from structure to process'.

As we saw above (Chapter 4), structural-functionalism had, during the 1940s, undergone a transformation. Social change was

no longer a non-theme, nor were conflict or process. Nevertheless, some scholars were dissatisfied with the slow pace at which this reorientation proceeded, others were simply overwhelmed by the reality of change among the peoples they studied. There was a growing consensus that structural-functionalism, for all its virtues, was just not very good at giving meaning to change. And change, as became increasingly evident, was everywhere, not only in the colonised societies, where it was all too obvious: even uncontacted primitive groups (for example, in New Guinea) existed in a state of imperfect cohesion and instability. Change seemed to be an essential part of the human condition.

It is hardly surprising that the issue of social change first came to the fore in British anthropology among scholars studying people who were themselves undergoing rapid, unpredictable and irreversible change. The prime example of this was the research conducted at the Rhodes-Livingstone Institute (later also at the University of Manchester), which focused on the effects of urbanisation, labour migration and rapid population growth in Africa. This research was pioneering, both in its methods and its subject matter, and was in no small measure responsible for the demolition of structural-functionalism – ironically, perhaps, since the school's main theorist, Gluckman, was a self-proclaimed protégé of Radcliffe-Brown.

The Rhodes-Livingstone Institute (RLI) was founded by Godfrey Wilson (1908–1944) in 1938 and led by him until he was forced to resign by the colonial administration (due to his radical political views and his pacifism) in 1942. He was succeeded by Max Gluckman, another radical. Wilson is best remembered for his discussion of 'acculturation' in *Essay on the Economics of Detribalisation in Northern Rhodesia* (1941–42). He predicted that colonialism would ultimately result in massive cultural change and 'detribalisation'. Years later, several leading anthropologists at the RLI would argue against this view, claiming instead that the effect of urban life on identity was *re-tribalisation* (Mitchell 1956; Epstein 1958), since the migrants were, in the new complex setting, continuously reminded of their identity as members of one group as opposed to the many others surrounding them in their new, urban environments. Like Wilson, many scholars at the RLI studied the mining towns of Northern Rhodesia's (now Zambia's), Copperbelt. Their research, proceeding over many years, demonstrated conclusively that traditional social forms, such as kinship, might be maintained and even strengthened under conditions of rapid change – 'returning to life' in the modern world, pregnant with

new meaning. A famous study in this genre was Peter Worsley's *The Trumpet Shall Sound* (1957), an analysis of messianistic *cargo cults* in Melanesia, which he describes as creative responses to the sudden and short-lived Western influence that many of the islands in the region experienced during the war. Similar perspectives have later proved useful in studies of ethnicity and nationalism (Chapter 7). Worsley, a card-carrying member of the Communist Party, was denied a research permit, first to Africa ,then to New Guinea, and had to base his study on existing literature.

Under the successive leadership of Wilson and Gluckman, the RLI opened up several new research fields associated with social change. The transformation of tribal life was investigated – many fieldworkers followed the same tribal groups both in town and in their traditional settings. They studied ethnicity or 're-tribalisation' (Mitchell 1956; Epstein 1958). They investigated race relations in the mining towns, at a time when racial discrimination was still considered the domain of sociologists in British anthropology. They also engaged in applied research, which was unheard of at the metropolitan departments.

The methods employed were equally innovative, indeed, it may be argued that the methodological contribution of the Manchester school was the most significant in anthropology since Malinowski. The practical problems of studying social life in the disorganised, conflict-ridden mining towns were formidable, and Malinowski's magisterial fieldwork on the tiny Trobriand Islands offered few clues. Some scholars experimented with quantitative methods, hitherto uncommon in anthropology: James Mitchell, Arnold Epstein and Elizabeth Colson used statistics and regression analysis in attempts to generate accurate data on social distance and network structure. 'Network analysis', pioneered by John Barnes (1954) was designed to trace the changing formal and informal relationships between people who were not firmly localised. The idea of 'scale', formulated by Wilson, allowed data to be sorted as relevant to local, regional, national or global levels of social organisation. The most important innovation was undoubtedly Gluckman's own 'situational analysis' (see below), which his student Jaap van Velsen (1967) formalised under the sobriquet 'extended case method'. These essential tools of anthropological methodology remain in active use today.

The parallels to the Chicago school are striking, and the British-Rhodesian researchers were perhaps aware of their work. But they were still primarily British social anthropologists. The success of the Manchester school was premised on the fact of colonialism

– which Wilson and Gluckman both actively opposed – and on the opportunities it opened up for alliances between metropolitan university departments and key research stations in the periphery. The department in Manchester could offer many of its students three-year research fellowships at the RLI (similar arrangements existed between the University of Cambridge and the East African Institute of Social Research at Makerere in Uganda, led successively by Aidan Southall and Audrey Richards).

What the Manchester anthropologists demonstrated, above all, was that change was not a simple object of study. One could not, as the structural-functionalists seem to have assumed, understand change simply by describing the social structure as it existed before and after the change, and postulating simple transformational rules, which 'explained' what had happened in the meantime. Gluckman and his colleagues showed that when local effects of global processes are investigated empirically, they dissolve into complex webs of social relations, which are in ceaseless change and influence each other mutually. This was the idea behind Barnes' *social networks* (ultimately derived from Georg Simmel), as opposed to Radcliffe-Brown's Durkheimian (and far more static) 'social structure'. Thus, an idea of change as rule-governed transformations between given states was slowly giving way to an idea of change as fundamentally unpredictable – a result of countless individual relations, each of which was reflexive and variable.

Max Gluckman (1911–1975) had taken his PhD at Oxford under Evans-Pritchard and Fortes. He came to the RLI in 1939, and served as director there from 1942 to 1947, when he was denied entry to Northern Rhodesia for his radical views and his championship of miners' rights, and returned to England. Gluckman's background, as a left-leaning Jew from South Africa, did not give him much in the way of automatic support from the British academic establishment, and it was thanks to Evans-Pritchard's and Fortes' support that he gained the Chair at Manchester in 1949. Much of Gluckman's research in southern Africa dealt with legal and political conflicts and their resolution (Gluckman 1965). After his move to Manchester, Gluckman's influence on the RLI continued throughout the 1950s, and for several years after Zambian independence.

Gluckman's theoretical orientation was a unique blend. As his student Bruce Kapferer (2006) puts it, he was primarily a structural-functionalist and a Durkheimian, in that he, like Durkheim and Radcliffe-Brown, sought the underlying structures of social life, and the 'logics of action' imposed by them on human consciousness. To

this Durkheimian basis, Gluckman added the notion of the critical event, the turning point, the *crisis*. In this he was inspired by the Austrian psychologist Sigmund Freud, whose detailed descriptions of atypical critical cases revealed underlying motivations in the subject, and by Marx, whose ideas of contradiction and resolution (dialectics) added historical context to the critical event. The crisis could either be resolved (as in carnivalistic rituals that allow people to air their frustrations in a controlled environment), or it could resist resolution, resulting in irreversible changes, in which, however, the past always continued to play an active role. In either case, Gluckman emphasised, change was incessant and omnipresent, only coming to a point in crises. This theoretical stance implied a particular and innovative methodology – *situational analysis* – which became the hallmark of the entire Manchester school: a crucial event or situation is isolated and examined in increasing detail and over ever-widening 'scales' of context, to produce a 'bottom-up' view on the large-scale dynamics of macrosocial units (regions, nations, the world market) that were inaccessible to Malinowskian fieldwork.

Gluckman's relationship to Malinowski was ambiguous. He acknowledged the master's eye for detail and his methodological flair, but was critical of the lack of generalisation and historical depth in his work (Kapferer 2006). He shared the Malinowskians' concern with social change, but saw change more as a product of confrontation between disparate elements of structure than as brought about by the tactical choices of individuals. While change for Malinowski himself was merely disruptive, Gluckman saw *structures in change* that contained the seeds of a new order. Thus, in his contribution to *African Political Systems*, he focused on the tensions between the traditional political system of the Zulu and the colonial administration imposed upon them, and asked what were the results of this critical conjunction. Unlike many of his contemporaries, Gluckman was acutely aware of the conflict-ridden nature of all societies, which were kept in one piece imperfectly and through hard work. Adam Kuper (1996) notes that Gluckman's and Malinowski's lineages practically converged in the late 1950s, through the remarkably similar work of Frederick Bailey and Fredrik Barth on political strategies. Characteristically, however, the (Manchester-schooled) structural orientation in Bailey's work (1960) on caste-climbing and local politics in Orissa, India, is absent from (Cambridge-schooled) Barth (see below).

Gluckman's interest in the crisis led him to do important research on ritual. The idea that ritual can contain crisis and strengthen

social cohesion is present as early as in Durkheim's sociology of religion. In Gluckman's work, and still more pointedly in that of his student, Victor Turner (Chapter 6), this basic idea inspires a notion of ritual as a locus of dynamic processes of meaning-making and social change.

METHODOLOGICAL INDIVIDUALISTS AT CAMBRIDGE

The Manchester school was important in reorienting British anthropology – from integration to process, from continuity to change. Still, no one had yet confronted structural-functionalism head-on. The mainstream were cautious. Their intellectual lineage stemmed from Durkheim, and the metaphor of society as a functionally integrated organism was implicit in their work throughout the 1950s, however innovative it otherwise might be. Malinowski's students, unhampered by axiomatic notions of social integration, had an easier time with social change. Malinowski himself had given the individual primacy over society, leaving more room for improvisation, variation and creativity than in structural-functionalism, which was increasingly seen as a straitjacket as the 1950s drew on.

Like his teacher, Raymond Firth was not primarily a theorist, but an ethnographer, and it was as such he maintained the need for a theory of social change. Wherever he went on fieldwork – among Maoris in New Zealand or Tikopians in Polynesia, among Malay fishermen or Englishmen in London – he saw social change taking place, and he saw the individual as the crucial agent of change. In his major theoretical work, *Elements of Social Organisation* (1951), Firth tries to nudge social anthropology towards a more flexible view of society. The 'kinshipology' of Radcliffe-Brown's students was at the height of its prestige. Evans-Pritchard was still a loyal structural-functionalist, and all major departments except at the LSE were directed by anthropologists of structural-functionalist persuasions. It would have been impossible for Firth to ignore these facts. He therefore nods critically, but respectfully, to the core notions of 'function' and 'structure'. He does not exclude the possibility of a stable social structure, but proposes a complementary concept, to capture the dynamic, shifting character of social action. That concept, borrowed from none other than Radcliffe-Brown, is *social organisation*. While social structure refers to the stable arrangements of society, social organisation is the actual flow of social life, in which individual interests meet, conflicts and compromises grow,

and the pragmatics of everyday life may deviate very considerably from the normative social structure without destroying it. Or in other words, action does not follow directly from norms, but passes first through a filter of tactical and strategic choice.

The discrete and diplomatic Firth was the mediator of his generation. He bridged gaps within the British school, gently preparing the ground for the more aggressive assaults on structural-functionalism that were to come, while maintaining an active dialogue with American anthropologists during a period when sustained contacts across the Atlantic were rare.

It was Cambridge that became the hotbed of the radical Malinowskians, who would eventually not only question the idea that society is inherently cohesive, but try to shift the attention of the discipline away from social wholes and towards individual action. Cambridge, a backwater of British anthropology until Fortes accepted the Chair in 1950, would for the next two decades simultaneously be a stronghold of structural-functionalism – primarily through Fortes and his student Jack Goody (1919–) – and a centre of radical innovation. In 1949, Fortes published the article 'Time and social structure: an Ashanti case study', a structural-functionalist *tour de force*, which showed that while the social structure of households unfolded over time, structure would *seem to change* while in fact merely repeating a well-known movement. (Here Fortes is expanding on the interest in 'domestic kinship' that we noted in Chapter 4.) In 1958, the cue was taken up by Goody in his edited volume *The Developmental Cycle of Domestic Groups*, which explores the mutual influence of kinship, resources, and structural changes imposed by the changing composition of households in the course of their development. Goody had done fieldwork in Ghana, but made his name by his bold, provocative – and increasingly unfashionable – comparative syntheses. Fortes and Goody both responded to the spirit of the times: even if the change they described was illusory, it was still (in the short term) change.

Edmund Leach (1910–1989), perhaps the most formidable personality among the young Cambridge anthropologists, pursued very different interests. A former student of Malinowski and Firth, Leach was educated as an engineer before he became an anthropologist. He was appointed to his position at Cambridge in 1953. In 1956, another influential student of Malinowski's, Audrey Richards (1899–1984), also came to town, to direct the new Centre for African Studies. Richards, who had done extensive fieldwork among the Bemba of Northern Rhodesia, had been an early critic

of Evans-Pritchard's work on the Nuer, arguing that the facts on the ground were too complex to fit his models. Like Firth, Richards did pioneering work in economic anthropology before the war (Richards 1939); her early work on nutrition makes her one of the founders of medical anthropology (Richards 1932); and in the year she went to Cambridge, she published an influential study of female initiation rituals (Richards 1956). It was through Richards' offices that the connection between Cambridge and the East African Institute at Makerere in Uganda was forged (see above). Other anthropologists associated with Cambridge in the 1950s included Gluckman's student, John Barnes, later professor at Canberra, and Fredrik Barth (1928–), a Norwegian who spent two years at Cambridge, where he took his PhD and wrote the classic *Political Leadership among Swat Pathans* (1959), a title echoing Leach's own seminal *Political Systems of Highland Burma* (1954). There was, in other words, a group of political anthropologists at Cambridge in the 1950s who had a weak sense of loyalty towards the dominant orthodoxy.

The focus on politics (a subject rarely foregrounded in Fortes' and Goody's domestic kinship-oriented work) was part of the legacy of structural-functionalism (Chapter 4). It could be argued, in retrospect, that in stressing the importance of political institutions, structural-functionalism was digging its own grave. Politics is 'the art of the possible', not 'the art of the legal'. It is about stretching rules – and breaking them, when opportune – not about stable allegiance to shared moral norms. Sooner or later, political anthropology would have to come to terms with the inherently manipulative dimensions of politics.

At Cambridge, Leach and Barth both saw this point, but in a somewhat different light than their colleagues at Manchester. Barth wrote about politics in Swat, north-east Pakistan, as a process fuelled by individuals' interests and strategies. His approach drew on the then new science of cybernetics (see below), which described complex systems in formal terms such as 'generative process' and 'feedback'. Barth eloquently demonstrates how the individualistic choices of antagonistic leaders maximising their own interests, generate a stable system of two alliances ranged against each other, while individuals, for tactical reasons, constantly defect from one block to the other. Social structure here has no primacy at all. It is a mere epiphenomenon of individual choice subject to the practical constraints of kinship, household structure and ecology. A comparison of *Political Leadership* and *The Nuer* indicates the shift that had occurred in British anthropology over the interim

20 years. Both books deal with stateless societies and the problem of integration; both discuss the politics of segmentary societies. However, the analyses differ in almost every other respect: Evans-Pritchard saw social structure as an overarching principle; individual maximisation is the only principle for Barth. Evans-Pritchard portrays his people with the aesthetics of a still life; Barth with that of a bustling tableau.

Leach's monograph, upon which Barth's was in some respects modelled, represents yet another approach to this theme. *Political Systems of Highland Burma* was based on fieldwork among the Kachin and Shan of northern Burma. Fieldwork was interrupted by the Second World War, when Leach enrolled in the Burma army and his fieldnotes were lost. The book, accordingly, contains few verbatim statements by informants and few accounts of actual people. However, whatever it loses in empirical detail it gains in analytical focus, and it is perhaps the most influential of all the monographs of the 1950s.

Political Systems, like *Political Leadership,* is a book about tension and conflict in politics. Leach thus shares the interests of his main antagonist in British anthropology, Max Gluckman, though their approaches differ radically. Leach was not the first anthropologist to study the relationship between myth and political process, but he was probably the first to argue that both are unstable and open to widely differing interpretations. The Kachin operate with two distinctive models of the political order; one egalitarian (*gumlao*), the other hierarchical (*gumsa*). Leach showed that the political organisation of Kachin villages oscillated between these poles in the long term, and that ambivalent elements in their marriage system, their economic organisation and their myths could be invoked and exploited to justify both. Malinowski had not been wrong in assuming that myths are 'social charters'. But in Leach's version, they were charters for trouble.

In the first chapter of his book, Leach made an important distinction between models and reality that was pertinent at a time when the validity of Evans-Pritchard's models of Nuer society was the object of heated debate. Models, Leach argued, are idealisations. They may be useful in analysis, as simplified points of reference for more realistic – and dynamic – descriptions. But even in wholly 'traditional' (and presumably stable) societies, norms cannot be translated directly into action, as Evans-Pritchard ostensibly claimed. The norms were focal points of ambiguity and stress produced by the meeting of opposed interests, and used by them to further their

own purposes. Leach is not entirely clear regarding the distinction between models devised by the anthropologist and native models, and this gives his conclusions a somewhat speculative flavour. The book nevertheless delves into the complex interrelationship of myth and power, revealing the space for individual manoeuvre that their intersection opens up. Leach demonstrated that social life is intrinsically volatile, that cultural categories are contested and open to interpretation, and stressed the legitimating functions of myth in politics, as Malinowski had done before him. The book played a pivotal role in establishing a research programme that is still very much alive as we write.

Towards the end of the 1960s, the atmosphere at Cambridge lost some of its edge, as the many talented students left to start their own careers. After Richards' departure, in 1967, only Leach, Goody and Fortes remained of the original group. Leach's interests had migrated from politics to symbolism (Chapter 6), Goody pursued his large-scale comparisons, with emphasis, by now, on literacy and the state. Fortes was approaching retirement. Barth had moved to the University of Bergen, Norway, in 1961, where he founded and led the Department of Social Anthropology at the recently established University of Bergen, and spent the 1960s building up a group of researchers devoted to the actor-oriented analysis he had pioneered, and in the process laid the groundwork for contemporary Scandinavian anthropology. The contributions of this department would range from studies of development issues in the Sudan to studies of entrepreneurship in northern Norway, and – increasingly – to ethnic relations. In 1966, Barth published a booklet called *Models of Social Organization*, a spirited attempt to demolish the Durkheimian concept of society once and for all. He argued that social structure is nothing but an aggregate generated by 'transactions' – pragmatic, strategic exchanges between maximising individuals. On the one hand, the constant negotiation of transactions generates a value consensus, on the other, it generates statistical regularities in the distribution of various kinds of action: 'social forms', Barth called them, which was the closest he ever came to a concept of social structure. This work, heavily inspired by the formal modelling techniques that were gaining currency in economics and political science at the time, was probably the harshest attack on structural-functionalism to date, and created considerable debate. In 1967, Barth convened a conference that would lead to the book for which he is most widely known today: *Ethnic Groups and Boundaries* (Chapter 7).

ROLE ANALYSIS AND SYSTEM THEORY

The study of social interaction, which had always been a mainstay of British anthropology and became even more prominent with the new methodological individualists, had never attained the same position in the USA, where culture occupied centre stage. As noted earlier, however, there were important exceptions. The work of the Chicago school and the formalist economic anthropologists come to mind, as well as the psychologically oriented contributions of Ralph Linton (Chapter 4). It was Linton (1937) who introduced the later common distinction between *status* and *role*, which (at the level of the individual) corresponds closely to Firth's later distinction between social structure and social organisation (on the level of society). Status, in Linton's terminology, is defined by moral norms, expectations from other individuals, and the actor's formal position in a structure of relations. Role, in contrast, is the enactment of status in actual behaviour. While the status is static, a given, much like the script of a drama, the role is dynamic. It rests on status, as the actor's performance rests on the script, but cannot be reduced to it. Role enactment demands and enables active interpretation and inventive departures from the norm. Linton was also the first to write systematically about the difference between acquired and ascribed status and about role conflict.

Nevertheless, the social theorist who is best known for his role theory is the Chicago microsociologist Erving Goffman (1922–1982), who carried out detailed studies of interaction and communication in small-scale settings in modern society, and developed a subtle conceptual apparatus for describing the rituals and routines of everyday life. In contrast to Parsons – the dominant sociological theorist in the USA at the time – Goffman consistently focused on the actor, on his or her motivations, strategies, decisions and concerns. In *The Presentation of Self in Everyday Life* (1959) he introduced his *dramaturgical* perspective, which took the metaphor of an actor on a stage much further than Linton ever did. Adding terms like role distance, stigma, under- and over-communication, deference and demeanour, frames and interaction ritual to the vocabulary of the social sciences, Goffman showed how each actor had ample room for manoeuvre within the constraints laid by status. His subtle observations of people interacting in everyday situations while observing, interpreting and communicating their (spontaneous, self-conscious or faked) intentions and reactions to themselves and

each other – raised to new heights our understanding of the intense reflexivity that characterises human social life (Goffman 1967).

Unlike much of the work undertaken by American anthropologists in the 1950s and 1960s, Goffman's fresh, lucid and often provocative writings crossed the Atlantic quickly, where they were happily deployed in the war against structural-functionalism, though Goffman's thinking is often very close to Durkheim's. In the USA, in contrast, his influence was at first largely confined to sociology.

Another innovation of the early postwar years had a somewhat similar fate. *Cybernetics*, the theory of complex, self-regulating systems (computers are a prime example), was developed in the late 1940s by an interdisciplinary group under the leadership of the mathematician Norbert Wiener (1948), and gained instant practical importance in computer design. Ecologists, biologists, psychologists, economists and scholars in a number of other sciences quickly put the theory to use. Cybernetics entered anthropology at an early stage, thanks to Bateson, who was associated with Wiener's group. Cybernetics focuses on relationships of circular causation or *feedback*, where 'cause' and 'effect' mutually influence each other. It studies the flow of *information* in such circuits. By linking circuit to circuit with 'logical switches' (which direct the flow along specific paths), a vast, interconnected web conveying meaningful impulses is formed. The ecosystem and the body are examples of such webs, and, as Bateson realised, there is in principle no reason why society should not be described in the same way. The result is a kind of functionalism, and indeed, it may be claimed that cybernetics renders at least part of the criticism against functionalist tautology obsolete. Cybernetically inspired anthropology differs from functionalism, however, in that all the internal connections of the system must be specified explicitly.

In a number of articles that were later collected in *Steps to an Ecology of Mind* (1972), Bateson devised a theory of human communication that he applied (at times, whimsically) to such diverse subjects as aesthetics, ecological flexibility, animal communication, schizophrenia and the constitution of the Self. An important contribution was his concept *metacommunication*, which denotes messages embedded in normal communication that inform the recipient that he is receiving information *of a specific kind*. By framing our messages in this way, we define for each other the context to which they belong (this is love; this is play; this is politics).

In this aspect of his thinking, Bateson resembles Goffman, and like Goffman, he was ignored by most American anthropologists in his

day. However, and again like Goffman, he would exert an unfocused but pervasive influence on anthropologists in most countries during the rest of the twentieth century and into the new millennium.

A changing world demands theories tailored to the study of change. This was the basic challenge faced by anthropologists, both in Britain and in the USA. In both cases too, the challenge emerged against a background of hegemonic social theories that described an idealised image of society (social structure) or culture (ethos). Both groups of innovators therefore reacted with a focus on the practical side of life. However, if they shared an interest in the practical, material processes of change, they disagreed deeply as to how these processes ought to be studied. In the USA, the rediscovery of Marx and Morgan implied a focus on institutions, structural analyses of inequality, the conditions for development and underdevelopment, and other aspects of large-scale change. From Benedict's individualist, psychological anthropology, Steward, White and their students moved toward large-scale historical processes. In Britain, the opposite was the case: attention moved from the collective to the individual. The leading orthodoxy, structural-functionalism, was collectivist in its orientation, and was attacked not only for offering a static, frozen image of the world, but also for not leaving the individual space for manoeuvre. While in the American analyses, change proceeded from institutional and historical processes, the typical agent of change in Britain was a calculating strategist or an innovative entrepreneur. Moreover, while the American evolutionists saw power (with Marx) as an outcome of global economic dynamics, the British interactionists (with Weber) saw it as a political resource subject to individual competition. Thus, the movement towards 'change' was far from uniform.

If this chapter has shown how economics and politics were reconceptualised during the 1950s and 1960s, the next chapter will discuss how new theories of symbolic meaning transformed the subject. Here, too, developments in the USA and Britain differed, though the problems raised were similar. Yet the single most important theorist was French.

6
The Power of Symbols

In the first decades of the twentieth century, the younger generation of anthropologists carefully distanced themselves from the search for historical origins that was characteristic of the nineteenth century. They sought an anthropology based on synchronous analysis and meticulous observation. By the 1950s, a new generation was rediscovering change, either as an evolutionary movement (in the United States) or an outcome of individual action (in Britain). All the while, other anthropologists were looking for new ways to study meaning and symbolism.

Research on the meaning of symbols was nothing new. In the United States, in particular, the 'discovery' was not at all subversive. The most important young American symbolic anthropologists, Clifford Geertz and David Schneider, who worked together at Chicago until Geertz moved to Princeton in 1970, saw themselves as inheritors of the Boasian tradition, who had added Parsonian system theory to Boas' concern with symbolic culture. In Britain, the situation was very different. There, the study of meaning was still associated with Frazer, who had explored the meaning of magic and the evolution of belief in *The Golden Bough*. Durkheim had studied religion, but chiefly in its ritual aspect, rather than as a meaningful system: the organisational practice of religion, rather than its content. Weber's interpretive sociology, notably his studies in the sociology of religion, was not well known. The study of meaning, in the British context, was tainted with evolutionism and best avoided. Culture, as a meaningful system, was widely seen as a footnote to the more fundamental workings of social organisation. The great exception was Evans-Pritchard, who had followed Frazer in his study of Azande witchcraft, before he became a prime mover of structural-functionalism. In 1950, he would turn apostate and lead British anthropologists into a new future. In France, a third path was taken. When Lévi-Strauss' structuralism became known in the 1950s, it was widely hailed as the crowning achievement of the tradition from Durkheim and Mauss. But was it? French intellectuals would spend years discussing this question.

FROM FUNCTION TO MEANING

We turn to the British situation first. It would be wrong to say that the interest in meaning was absent from British anthropology. After all, rituals, myths and kinship were studied, none of which were intelligible without attention to their meaning. Many scholars, however, saw beliefs and symbols as mere expressions of social structure. A late but influential example of this approach is the widely cited article by Jack Goody and the literary theorist Ian Watt, 'The consequences of literacy' (Goody and Watt 1963), which argued that the introduction of writing irreversibly changed both the social structure and the structure of reasoning (or cognitive style; Bateson would have called it *eidos*) of society. This article and the edited volume it introduces (Goody 1963) are definitely concerned with meaning, but largely with its social and political implications, not as an independent system with its own dynamics. Evans-Pritchard's interests were more radical than this.

He could afford to be radical. When he succeeded Radcliffe-Brown to the Chair at Oxford in 1946, he had already authored two hugely influential monographs and co-edited a book – *African Political Systems* – that defined the mainstream research agenda of British anthropology for two decades. Ten years later, the companion volume, *African Systems of Kinship and Marriage*, edited by Radcliffe-Brown and Forde, had much less impact. Evans-Pritchard was beyond doubt the most powerful social anthropologist of the time. He was president of the Royal Anthropological Institute, a member of most committees of importance and knew everyone. When, in his Marett lecture, 'Social Anthropology: Past and Present' in 1950, he repudiated structural-functionalism and distanced himself from his teacher, this was headline news that was impossible to ignore for the anthropological community. In the lecture he claimed, on the one hand, that it would be nonsense to believe that synchronous studies could yield insights of the same complexity and depth as historical studies; on the other, that in terms of method, social anthropology had more in common with history than with the natural sciences. He was rejecting two of the mainstays of structural-functionalism. In his later work, Evans-Pritchard abandons the search for 'natural laws of society' and attempts, more realistically, to understand the meaning of particular social institutions. His second Nuer book, *Kinship and Marriage among the Nuer* (1951b), was more descriptive and less theoretically focused than *The Nuer*. The elegant but simple models of *The Nuer* had in no small measure provoked the turn

from structure to process described in the previous chapter. The new book went deeper, its conclusions were more ambiguous – and it never received the same attention.

In 1958, the philosopher Peter Winch published *The Idea of a Social Science and its Relation to Philosophy*, a book that would exert considerable influence on anthropological self-reflection, especially in the UK. Winch argued that it was impossible to establish objective knowledge about cultural phenomena, since their meaning was defined by the cultural universe of which they were a part. He adopted a strongly relativist position, arguing that there exists no context-independent point of reference from which to compare and evaluate other cultures, except for our common experiences of universal bodily processes, such as 'birth, copulation and death' (Winch quotes T.S. Eliot at this point). Anthropology was, in Winch's view, a Western cultural oddity on a par with the institution of witchcraft among the Azande, and anthropologists had no right to consider their access to knowledge privileged. Winch used the Azande monograph as an example of the naive belief in the superiority of science. Evans-Pritchard presented a 'scientific' explanation of the 'obviously mistaken' belief in witches. What if the tables were turned? How can we know that a witchcraft-based explanation of the 'obviously mistaken' belief in science would be less true? Winch's book was the starting point of a long and influential debate about rationality and cultural translation, to which both philosophers and anthropologists contributed (B. Wilson 1970; Hollis and Lukes 1982; Overing 1985).

It is worth noting that Evans-Pritchard seems to have arrived at a similar position as Winch independently. The third volume of the Nuer trilogy, *Nuer Religion* (1956), is more interpretive than explanatory; the author declares at the outset that his ambition is to make sense of the Nuer worldview, not to explain it sociologically. In this, he is in accord with his Oxford colleague and close associate in this period, Godfrey Lienhardt, whose later work on the Nuer's neighbours, the Dinka, was similarly interpretive (Lienhardt 1961). Understanding and translation had become more pressing issues for Evans-Pritchard than explanation and the search for general 'laws'. But it is also true, as his student Mary Douglas (1980) says, that his entire oeuvre – from the Azande book onwards – was marked by continuity. Even *The Nuer*, which is often described as a paragon of orthodoxy, is at heart an evocative, even a poetic, book.

While the renewed focus on change in British anthropology is often described as a turn 'from structure to process', the change in

Evans-Pritchard's position was a turn 'from function to meaning'. Two of his intellectual descendants would, in the decades following the Marett lecture, with particular success combine a sociology of integration with an interpretive method concerned with symbolic meaning.

The first of these was Gluckman's student Victor Turner (1920–1983). During the 1950s and 1960s, Turner developed a perspective on symbols and social cohesion which has remained persistently influential up until our day. Unlike Leach, he was mainly concerned with ritual, not myth; and where Leach saw myths as focal points for conflict, Turner regarded rituals as ultimately cohesive (though not unchanging). As Durkheim had seen, rituals offered splendid material for the ethnographer since they expressed the central values and tensions of society in intensely concentrated form. Though Turner's approach to ritual would increasingly focus on symbolism, he always sought to combine it with an underlying Durkheimian notion of social integration. In one of the most influential British monographs of the 1950s, *Schism and Continuity in an African Society* (1957), based on fieldwork among the Ndembu of Northern Rhodesia (Zambia), the main problem is a classic one: how do matrilineal societies (like the Ndembu) solve the problem of integration? While succession, inheritance and group membership are united in a single principle in patrilineally organised societies, such rights were distributed in accordance with several different criteria in matrilineal systems, seemingly predisposing them for ambivalence and instability. In his discussion of Ndembu, whose social structure he describes as unstable, Turner (inspired by Gluckman's notion of crisis) introduces the concept of the *social drama* – often a rite of passage – in which underlying norms are given symbolic expression, and thus contribute to bring a fragmented world together.

While Turner's monograph remained within the structural-functionalist paradigm, his discussion of social drama suggested that a break was under way. In a series of articles written in the late 1950s and early 1960s (published as *The Ritual Process* in 1967), Turner developed his influential theory of ritual communication. In 'Betwixt and between: The liminal period in rites-de-passage', he introduced the concept of *liminality*, later a staple of anthropological studies of ritual. Taking his cue from van Gennep's early work on rites of passage (Chapter 3), Turner regards the ritual, and in particular the initiation ritual, as a process of transformation whereby a person moves from one defined state to another, with an intervening period

of uncertainty and crisis. It is this state of crisis – the *liminal* period – that is the focus of the ritual, which the ritual first creates and later seeks to control, by imposing the values of society upon the wavering individual who, for a short but critical time, finds herself 'betwixt and between'. In the borderland between social statuses, neither old nor new rules apply, and the individual is compelled to reflect on her personal situation, her place in society and indeed the legitimacy of society as such. Thus, liminality is a critical and creative state of being, and change a potential of any ritual. And yet, the initiate is nearly always reintegrated into society.

The continuity with both Durkheim and Gluckman is strong in Turner's work, which nevertheless stands out through its emphasis on the individual, on the multivocality of symbols, and on the creative potential of the crisis. Turner's emphasis on multivocality implies that symbols themselves might be tension-filled sources of change, and that the same symbol might mean different things to different people, thus creating a sense of community among people who are otherwise very different. This perspective would later be mined by others, not least scholars of nationalism (Chapter 8). By placing the liminal crisis at the core of every ritual, Turner introduced a basic uncertainty to the very heart of 'the sacred', which in Durkheim was the ultimate guarantor of social cohesion.

Another Africanist of structural-functionalist descent who would propel social anthropology towards the study of symbols, was Evans-Pritchard's student, Mary Douglas (1921–2007). Douglas studied the Lele of Kasai, Belgian Congo, in the late 1950s. This brought her into contact with French and Belgian anthropologists, and she would in time become a bridge-builder between the French and British traditions. Her most influential early work was not the monograph from her fieldwork, but a comparative study of symbolic boundaries and classification, *Purity and Danger* (1966). The book combines an almost orthodox structural-functionalism with a symbolic analysis drawing on structuralist and psychoanalytical models. An anthropological bestseller on a par with *Patterns of Culture*, *Purity and Danger* can be seen as a British counterpart to Benedict's book. In both books, the concern is with group identity and values; but whereas Benedict restricts herself to the symbolic aspects of culture, Douglas links symbols to social institutions as a classical Durkheimian. She sees symbols as means of social classification, which distinguish categories of objects, persons or actions, and keep them separate. The order of the classificatory system reflects and symbolises the social order, and 'intermediate',

'unclassifiable' phenomena come to represent a threat to social stability. Snakes (animals without legs) and substances that pass in and out of the body, are problematic. Foodstuffs are often ordered in hierarchies of 'pure' and 'polluted', which have nothing to do with their nutritional value. Bodily waste is universally polluting and potentially dangerous, since it symbolically challenges our humanness. Where Barth, for example, might see an unorthodox, unclassifiable person as a potential entrepreneur who may bring about change, Douglas might see the same person as a classificatory anomaly. The contrast hints at the differences between symbolic and actor-centred perspectives, as they appeared in British anthropology in the 1960s.

Both Douglas and Turner would refine and expand their perspectives through the next decades. Douglas would do pioneering work on the cultural dimension of consumption (Douglas and Isherwood 1979), risk perception and modern technology, and institutional anthropology. Turner, who moved to the United States in 1961, would develop his ideas of liminality into a general theory of ritual *performance* (Turner 1969, 1974, 1987). Though Turner died in 1983, his influence continued to grow during the 1980s and 1990s, when his concern with performative play and reflexivity would be welcomed by the postmodernist movement in anthropology, and by anthropologists concerned with bodily experience, emotions and the symbolic dimensions of power (Chapter 8). Turner's intellectual itinerary passes from fairly orthodox structural-functionalism to a radical focus on aesthetics and performance, but he remains at heart a Durkheimian – though his version of Durkheim differs radically from Radcliffe-Brown's.

ETHNOSCIENCE AND SYMBOLIC ANTHROPOLOGY

While anthropologists like Evans-Pritchard were busy distancing themselves from the natural sciences and reflecting the ideals of the German cultural historians, others made the opposite move. This was not only true of the American cultural ecologists and British methodological individualists, but also of many linguistic anthropologists in the USA.

Several of Edward Sapir's successors explored semantics and language structures with exact methods. Some devised quantitative research strategies that allowed them to measure frequency patterns of linguistic terms and correlate them across languages, in cooperation with psychologists, mathematicians and other workers in the

emerging interdisciplinary field of cognitive science (Chapter 9), itself heavily inspired by cybernetics (Chapter 5). Among the foremost of these were Harold Conklin, Charles Frake and Ward Goodenough, who all contributed to the development of *ethnoscience* in the 1950s. Ethnoscience was concerned with describing 'cultural grammars', by identifying the building blocks of semantic universes or systems of knowledge. They drew on both the culture and personality school's interest in socialisation, on formal linguistics and on the comparative study of classification, to which Sapir and Whorf (and, before them, Durkheim and Mauss) had contributed. In its most technical form, ethnoscience appeared as *componential analysis*, which broke down meaningful expressions into more elementary 'features', to allow precise definitions of meaning.

In its original form, ethnoscience died out during the 1960s, but the issues it raised have later been pursued in cognitive anthropology (D'Andrade 1995; Shore 1996). In both schools, the relationship between the universal and the culturally specific was of concern. Colour classification was an early, and relatively simple, field to be explored in this way. There were parallels between ethnoscience and the rationality debate in Britain, on the one hand, and Lévi-Straussian structuralism, on the other. Unlike Winch or Lévi-Strauss, however, the ethnoscientists worked inductively, amassing huge amounts of data which were processed by the massive, sluggish computers of the day.

In the 1950s, American anthropology was still dominated by Boas' students, who produced predictable work in the culture-and-personality tradition, often merged with the Durkheimian and Weberian ideas that were gradually gaining acceptance in the USA, largely through the work of Talcott Parsons. One of the most interesting monographs of this period was Kluckhohn's *Navaho Witchcraft* (1944), which resembles Evans-Pritchard's Azande monograph, in that it combines sociological analysis with psychological perspectives.

After Boas' death, the *pater familias* of American anthropology was Kroeber. In 1952, he published, with Clyde Kluckhohn (1905–1960), *Culture: A Critical Review of Concepts and Definitions*, which discusses 162 definitions of culture, and ends by recommending the abandonment of Tylor's and Boas' all-embracing concept in favour of a definition limited to cognitive (symbolic, meaningful) culture.

Thus, the turn towards meaning that took place in Britain had its parallel in the USA, not least thanks to Parsons' influence. From

his powerful position at Harvard, Parsons dominated American sociology in the 1950s. He had monumental visions for the social sciences, and was well connected with funding agencies. He suggested a 'temporary division of labour' between sociology and anthropology, in which sociologists would study power, labour and social organisation, while anthropologists (in accordance with Kroeber and Kluckhohn's recommendations from 1952) would focus on the symbolic and meaningful aspects of social life. In an article jointly authored by Parsons and Kroeber in 1958, this 'truce' (as the authors called it) was programmatically endorsed (Kuper 1999: 69). Although the majority of twentieth-century American anthropologists had already gravitated towards the symbolic, the 'truce' attempted to narrow the discipline's scope still further. From Kroeber's point of view, this was a last-ditch attempt to escape the influence of social science and maintain anthropology's humanistic, German roots. As the growth of Steward's programme shows (Chapter 5), he had at best partial success at this.

GEERTZ AND SCHNEIDER

Two postgraduates who received funds through a joint Parsonian-Kroeberian programme at Harvard were Clifford Geertz (1926–2006) and David M. Schneider (1918–1995). Both took part in interdisciplinary projects during their PhD studies – Schneider doing fieldwork on Yap, in Micronesia, Geertz on Java. Both at the time endorsed the cognitive definition of culture, with Geertz, in his early work, carefully distinguishing between two 'logics of integration': society, or social structure ('causal-functional' integration), and culture, or the symbolic realm ('logico-meaningful' integration). Each subsystem, he argued, could in principle be studied independently of the other.

In the 1960s, Geertz and Schneider emerged as the most important American symbolic anthropologists (along with Turner, who was by now in the USA), with research programmes that were sharply opposed to the materialist agendas of Steward's students, such as Wolf or Sahlins (for a while a colleague of Geertz at Chicago). They agreed that Kroeber and Parson's 'truce' had become a straitjacket, but instead of defining anthropology as a branch of sociology, they expanded – as Boas no doubt would have done – the field of culture as a symbolic system. Culture was a system of Weberian interpretations (Chapter 2) – of one's own acts as well as others'. They claimed that action was impossible without interpretation – that

is, without meaning – and thus made substantial inroads into the 'exclusive domain' of sociology. In Britain, this view was often misunderstood as a claim that meaning could be studied without reference to social organisation, which to *social* anthropologists seemed patently absurd.

Schneider's best known work is *American Kinship* (1968), a study of American kinship terms based on interview data collected by his students. The 'American Kinship Project' was the result of cooperation between Firth and Schneider. The two anthropologists had spent a year together at Stanford University in the late 1950s, and agreed that it would be interesting to extend anthropological kinship studies to modern society. They inaugurated a comparative project of middle-class kinship in London (Firth) and Chicago (Schneider). Although the comparative aspect of the project was never realised and the two studies were published separately, Schneider's book became a milestone in kinship research; in part because it demonstrated that kinship studies in complex societies were both possible and interesting, and in part because it forced a fundamental reassessment of anthropological notions of kinship.

After Evans-Pritchard's defection and the appearance new perspectives on politics and ritual at Manchester and Cambridge, kinship remained the last stronghold of structural-functionalism. Then, in 1962, John Barnes published the article 'African models in the New Guinea Highlands' (reprinted in Barnes 1990), which concluded that the theory of segmentary lineages, which had been applied so successfully in Africa, could not be transferred to the New Guinean context without seriously distorting the data. The problem lay not the kinship terms themselves. It was possible to interpret the New Guinean material *as if* it were African, but such an interpretation flew in the face of native understandings of kinship as well as their practices.

Schneider's book made a similar statement, but its conclusions were more radical. While Firth, in his London study, had catalogued a fairly standard range of kinship terms, Schneider's informants were asked to give information about anyone with whom they had any kind of kin relationship. A broader view of kinship was thus made possible. It emerged that kinship was a symbolic universe, through which informants moved tactically and strategically. This implied that the idea of kinship as a biologically based metaphor of human relations was seriously at fault, hardly a new observation, but in Schneider's view, a culture could in principle construct kinship entirely from scratch, without any reference to blood ties

whatsoever. Furthermore, within each 'culture of kinship' each kinship term derived its meaning from the semantic network of which it was a part, which was unique for the culture in question. Even elementary kinship terms, like 'father', had different meanings in different kinship cultures. Kinship was in effect situational. This undermined the entire project of comparative kinship studies that had survived since Morgan.

Schneider's redefinition of kinship from social structure to culture has parallels in the work of Geertz. Geertz' association with Parsons at Harvard has been mentioned. However, Geertz was influenced from many directions: from European sociology, from Boas, even from Steward's cultural ecology. Geertz' early work dealt with a wide variety of themes, from ecology (1963a) and economy (1963b) to religion (1960). His oft-cited and eloquent article on 'thick description' (1964, reprinted in Geertz 1973) expresses his methodological credo, and challenges anthropologists to describe the world in the same dense detail and with as many overlapping contextualisations as the natives experience. Among the European sociologists, Geertz was inspired by both Durkheim and Weber, as well as by Alfred Schütz (1899–1959), an Austrian social scientist who pioneered interpretive studies of modern everyday life. A decisive intellectual impulse in Geertz' mature work came from the French philosopher Paul Ricoeur (1913–2005), who saw society, culture and action as 'texts' that anthropologists – and the actors themselves – must 'read' and interpret (Ricoeur 1971). As mentioned above (Chapter 2), the interpretive method of hermeneutics had a long history before Schleiermacher's postulate (Chapter 2) that a text is simultaneously an assemblage of individual parts and a seamless whole, and that interpreting the text involves a movement to and fro between these two poles.

In the 1960s, Geertz introduced this idea, with latter-day refinements, to Anglo-American anthropology. As opposed to British anthropologists who focused on the individual as a (normatively or strategically motivated) *actor*, Geertz introduced the individual as a *reader* of the world. Against their idea of society as rationally constituted and of the individual participating in it through rational activity, Geertz posited that the world contains an infinity of meanings and that the subject must actively *interpret* this vast multivalence to find meaning in the world. In the lecture 'Religion as a cultural system' (1966, reprinted in Geertz 1973), he argued that religion is not primarily a functionally integrated subsystem of a social whole, but a means for individuals to make sense of the

world. In 1973, Geertz' most important early articles were collected in *The Interpretation of Cultures*, and his reputation has been on the rise ever since. During the 1980s, he was viewed as a kind of postmodernist *avant la lettre*, although it seems obvious to the present authors that this is at least a partial distortion.

LÉVI-STRAUSS AND STRUCTURALISM

The son of a prosperous Jewish couple of the cultured middle classes, Claude Lévi-Strauss (1908–2009) studied philosophy and law in Paris in the early 1930s and associated with the intellectual circle around the existentialist philosopher Jean-Paul Sartre. In 1935–39, he taught at the University of São Paolo in Brazil, and organised short field trips to several peoples of the Amazon region. As a Jew, he fled from France during the Second World War, and upon the intercession of Métraux and Lowie was offered a position at the New School of Social Research in New York City, where he stayed until 1946. While in New York, he was intimately affected by Boasian anthropology (Boas died of a heart attack in his arms, during a formal dinner), commenced a lifelong friendship with Margaret Mead, and met the important Russian-American linguist, Roman Jakobson (1896–1982), whose structural linguistics would become a mainstay of Lévi-Strauss' later work. He received his doctorate in Paris in 1947, and published his dissertation in 1949 as *Les Structures élémentaires de la parenté* (*The Elementary Structures of Kinship*, 1969). It was a book that would revolutionise kinship studies. Six years later, Lévi-Strauss published the ultimate anthropological travelogue, *Tristes Tropiques* (1955), a wide-ranging, beautifully written and intricately composed narrative, so full of suggestive and haunting passages that a summary would be useless. Three years later came a collection of theoretical articles, *Anthropologie structurale* (1958; *Structural Anthropology*, 1963a). Together, these three books established Lévi-Strauss' reputation as a formidable thinker with enormous ethnographic and theoretical knowledge.

By now, he had also established *structuralism*. Structuralism is a theory that seeks to grasp the general qualities of meaningful systems – most famously, in Lévi-Strauss' own work, kinship systems and myths – and ultimately reveal fundamental properties of the human mind. Such systems consist of elements, but the elements are not delineated categories or objects. They are *relationships*. A kinship system, for example, is a meaningful system and thus consists of

relationships, rather than positions ('statuses'). A father is not a father in himself, but only in relation to his children.

The idea that meaning was relational was not in itself new. It was an important component of Jakobson's structural linguistics, as well of the semiotic linguistics established by Ferdinand de Saussure before the First World War. In both, meaning derives from the relationship – the contrast or difference – between linguistic elements (phonemes, words, signs). Relational meaning was also central to cybernetics – as Bateson liked to say, meaning is a 'difference that makes a difference' (Bateson 1972: 453). Finally, and most importantly, relational meaning is implicit in Mauss' discussion of the gift. Here, objects are charged with magical power by the relationships through which they move. It is the exchange that gives the gift its meaning (Lévi-Strauss 1950).

The advantage of reducing meaningful systems to structures of contrasts is that the flow of time within the system is frozen. Living language becomes a static grammar. The confusing enactment of kinship in practice is reduced to a lucid, formal structure. Roughly, structuralist analysis consists, first, in unearthing this structure; second, in deducing its underlying principles – its 'logic'; and finally, in arriving at a universal 'logic of logics' of human communication. The technicalities of this process need not concern us here, but we shall briefly outline how it was expressed in *The Elementary Structures of Kinship*.

Structural-functionalism's reputation rested to a great extent on its analyses of segmentary lineage systems, which seemed to prove beyond reasonable doubt the role of kinship as the prime organisational principle in tribal societies. Lineage theory, in turn, presupposed a primary emphasis on *linear* kin relationships (grandfather–father–son), while *lateral* relationships (husband–wife, sibling–sibling) were often downplayed. All of this was upset by *Elementary Structures*. In Lévi-Strauss's view, kinship was not primarily a mode of social organisation, but a meaningful system, a system of relationships, and the primary relationship was not the 'natural' bond of blood (parent–child), but the socially constructed bond between husband and wife. The first cultural act takes place when a man gives his sister in marriage to another man and ties are forged between persons who are *not* biologically related. Marriage is the point of indeterminacy in biological kinship – one cannot choose your parents, but one *must* choose one's spouse. For Lévi-Strauss, this choice is the fissure through which meaning enters kinship, transforming tribal society from biology to culture.

Clearly, the integrity of this choice must be safeguarded. It must not appear to be determined by nature. You must not marry your siblings; they are 'too close', 'too natural', it would be too much like marrying yourself. It would do nothing to 'open up' your world, to give it meaning by relating it to *something else*.

In marriage, as practised in tribal societies, women are exchanged between groups of men, and a meaningful relationship is formed between these groups – a *lateral* kin relationship which Lévi-Strauss refers to as an *alliance*. From this, the logic of kinship is deduced – that is from *lateral*, rather than linear, kin relations. The result is a theory diametrically opposed to lineage theory, that places alliance above descent, contrast above continuity, arbitration above norms, meaning above organisation. In a rather brisk letter written near the end of his life, Radcliffe-Brown told the Frenchman that they would probably always talk past each other. Still, Lévi-Strauss expresses more respect for Radcliffe-Brown than for Malinowski, 'for whom culture is merely a gigantic metaphor for digestion' (Lévi-Strauss 1985). Radcliffe-Brown and Lévi-Strauss had a common interest in uncovering the hidden structures that governed thought and social life, and a common ancestor in Durkheim – though they belonged to very feuding segments of his lineage.

Lévi-Strauss' further writings are hard to summarise. His books are long, erudite, packed with facts, and held together by some at times very technical thinking. Thus, *Le Totémisme aujourd'hui* (1961; *Totemism*, 1963b) seems to be a discussion of the concept of totemism (which is debunked), but it is also (among other things) an ambiguous critique of the Western opposition of nature and culture. *La Pensée sauvage* (1962; *The Savage Mind*, 1966) discusses a fairly standard 'primitive' versus 'modern' dichotomy, reminiscent of Durkheim or Weber, Maine or Tönnies, but starts with a seemingly endless inventory of the detailed knowledge that 'primitives' have of their natural surroundings, and ends with a critique of Sartre's theory of history. In the first chapter of this book, 'The science of the concrete', Lévi-Strauss establishes the basis of 'savage' or 'mythical' (in contrast to 'modern' or 'scientific') thought. Both are equally complex and equally rational, but their governing rationalities differ. The *bricoleur* starts with the world that is directly accessible to his senses. He relates the objects found in this world to each other, and builds structures of meaning out of them, that are narrated, for example, in myths. Thus he creates *structure out of events*. The engineer, in contrast, creates *events out of structure*. He starts with

a blueprint, an abstraction that the senses cannot perceive, and by manipulating it he changes the real world.

The Savage Mind marks the transition from Lévi-Strauss' 'kinship period' to his 'mythology period'. The most remarkable work of this latter period is the *Mythologiques*, a vast, four-volume compilation and analysis of Native American myth, published between 1967 and 1974, which traces the transformation and convolutions of mythical themes throughout the continent. The sheer complexity of this work has limited its influence, just as the brevity, if not simplicity, of *The Savage Mind* has made it exceedingly popular.

EARLY IMPACT

Lévi-Strauss' impact on Anglo-Saxon anthropology was limited before the 1960s, and his early work was belatedly translated into English. *The Elementary Structures of Kinship* appeared in translation only in 1969, and for a long time the book was largely known indirectly, through an introduction written by a Dutch anthropologist – the founder of an older structuralist school – J.P.B. Josselin de Jong (1952). Despite the dearth of translated texts, Lévi-Strauss was from the first a controversial and influential author. In France, structuralism became an alternative to Marxism and phenomenology in the 1950s, and the impact of structuralism on general intellectual life was at least as pronounced as in anthropology. Important non-anthropologists such as Roland Barthes, Michel Foucault, Louis Althusser and Pierre Bourdieu were introduced to structuralism at a tender age and eventually rebelled against it – and their rebellion was in turn noted and debated by anthropologists, who imported these authors into the discipline's canon (Chapters 8 and 9).

In Britain, Leach was the first of the leading anthropologists to be attracted to Lévi-Strauss. Lévi-Strauss had commented quite extensively on kinship among the Kachin, and Leach immediately recognised the relevance of his conclusions for his own data. In structuralism, Leach discovered a sophisticated alternative to structural functionalism, and in 1970, he wrote an introduction to Lévi-Strauss, which substantially increased knowledge of his work in the English-speaking world. The Oxford anthropologist Rodney Needham, who had studied with Josselin de Jong in Leiden, was another early spokesman for Lévi-Strauss in Britain, although he had certain reservations from the beginning (Needham 1962). These were further strengthened by an unfortunate exchange with

Lévi-Strauss himself, who repudiated Needham's interpretation of his kinship theory in no uncertain terms in the preface to the English edition of his kinship book. Needham continued to develop structuralist thought about classification and kinship in innovative directions, but without referring again to Lévi-Strauss. Most Anglo-American anthropologists were nevertheless deeply suspicious of structuralism. They were provoked by Lévi-Strauss' abstract models and deductive thinking. Many regarded his work as useless because it could not be tested empirically.

Lévi-Strauss' kinship theory (often referred to as *alliance theory*, as opposed to structural-functionalist *descent theory*) was already debated in Britain during the 1950s (although misunderstandings were inevitable because of the lack of translations). Within structural-functionalism, there had been growing dissatisfaction with descent theory, which seemed unable to account for kinship systems that were non-unilinear. The structuralist focus on exchange and alliance seemed to have the potential to resolve these problems, since it accorded greater weight to lateral than lineal kin relationships; thus it was widely embraced by anthropologists working in societies without clear-cut unilineal descent groups. In a famous debate in the journal *Man*, in 1959, Leach defended Lévi-Strauss' views, while Fortes defended the descent model. But Leach too may have misunderstood Lévi-Strauss' intentions, which were less sociological and more cognitive than his British colleagues tended to believe. Like the formalist–substantivist controversy in economic anthropology, the debate on alliance versus descent in kinship studies slowly petered out towards the end of the 1970s. By then, there was a tendency to see the two systems as complementary (as Morgan did), and Lévi-Strauss himself proposed a theory of kinship that seemed to incorporate both perspectives (Lévi-Strauss 1987b).

In France, Louis Dumont (1911–1998) developed his own brand of structuralism, by combining impulses from Lévi-Strauss with classical European sociology (Durkheim, Tönnies) into an influential theory of social integration and symbolic meaning. Dumont, who is particularly known for his erudite study of the Indian caste system, *Homo Hierarchicus* (1968), posited that caste was a cultural system of classification, rather than a functional means of social organisation (a view not dissimilar to Needham's). He emphasised the irreducibility of Indian (Hindu) categories, in explicit opposition to political anthropologists like Barth, who had described caste in purely sociological terms, and argued that strategic actors were driven by the same kinds of motivations as Europeans.

THE STATE OF THE ART IN 1968

By 1968, anthropology had become a very diverse discipline. The 'Man the Hunter' symposium had just been held, demonstrating the power of ecological anthropology. The interpretive anthropology of Geertz had begun to make its influence felt. Peasant studies in Latin America and the Caribbean had become a mainstay at some American departments. Barth's radical 'transactionalism' rubbed shoulders with the creative revitalisation of structural-functionalism carried out by Douglas and by Gluckman's students. The rationality debate was on, formalism confronted substantivism, alliance theory battled with descent theory, while structuralism loomed on the horizon, and young radical Marxists and feminists waited in the wings for their share of the academic pie. New journals, conferences and workshops, monograph series and institutions devoted to anthropological research made important contributions to the growth and diversification of anthropology.

The demographic expansion had been formidable. In 1950, a mere 22 PhDs were awarded in the USA. By 1974, the number had increased to 409, a level that remained stable until the mid-1990s (Givens and Jablonski 1995). However, the discipline had grown not only in complexity and size, but in geographic dispersal. Scandinavian, Italian, Spanish and Dutch anthropology became part of the European mainstream – in the first case, the British influence was strongest; in the second and third, the main impulses came from France; in the fourth there was a little bit of both. In several Latin American countries, notably Mexico, Columbia, Brazil and Argentina, indigenous anthropologists influenced by the Boas school, by Steward and his students, and by French anthropology, conducted research on Native Americans, peasants and city-dwellers. But in spite of strong non-metropolitan milieus such as Leiden and Bergen, the discipline remained centralised. In Britain, Oxford, Cambridge and London still held the reins, though Manchester and University College London were becoming powers to be reckoned with, and anthropology was taught in several other places as well, from Sussex to Edinburgh. In the USA the dispersal was greater, since the numbers were greater, but prominent universities such as Columbia, Yale, Harvard, Berkeley, Stanford, Michigan and Chicago still had the lead. In France, all roads led towards the prestigious institutions in Paris.

The 1950s and 1960s also saw considerable diversification of the core regions for ethnographic research. During the 1920s and

1930s, British anthropologists had shifted from the Pacific to Africa, American anthropology saw a less pronounced move from Native North America to South and Central America. In France, both Africa and the Pacific had been important since the 1920s and, after the war, Georges Balandier further strengthened the African orientation (Balandier 1967), while Dumont and his students flocked to the Indian subcontinent and Oceania. By the 1960s, the mountainous New Guinean highlands had become an extremely fertile area for ethnographic research, and with this change came new perspectives on gender relations, warfare, exchange and kinship. For although anthropological research may be carried out anywhere, each region confronts ethnographers with new questions.

In spite of occasional dialogue, there was still little contact between the three dominant national traditions. As we have repeatedly pointed out in the last two chapters, research interests were frequently similar, but the theoretical approaches were sufficiently different to make direct discussion difficult. Firth and Schneider had to abandon their comparison of kinship in London and Chicago. Lévi-Strauss debunked Needham's interpretation of his work. As mentioned, Kroeber and Kluckhohn presented 162 competing definitions of culture, a term that few British anthropologists had discussed since Tylor. There was much individual movement going on, though mostly in a westward direction: Bateson, Turner and Polanyi had settled in the USA, and Lévi-Strauss spent the war there. Many others were to make the move later, particularly from Britain. The national traditions were nevertheless still relatively bounded.

Language differences played their part in this. The belated translations of Lévi-Strauss delayed the acceptance of structuralism by at least a decade in most of the English-speaking world, and research published in less prestigious European vernaculars than French generally fared even worse. Throughout much of the Third World (a term introduced into English by anthropologist Peter Worsley in 1964; in French *le tiers monde* had been used, with a slightly different meaning, since the 1950s), these problems were exacerbated by the lack of adequate economic resources in academia. Finally, political conflicts delayed the internationalisation of the discipline. In the former colonies, hostility was sometimes directed at anthropology as such, thus inhibiting and sometimes even halting its spread. With decolonisation, the relationship between metropolitan institutions and their colonial counterparts was often severed. In Europe two decades earlier, the Iron Curtain had effectively prevented academic contacts between

East and West. Anthropology was becoming a global discipline, as scholars increasingly started publishing in English, but even in the West, scholars in, say, Stockholm, could draw inspiration from metropolitan anthropologists, but feel certain that their own work would never be read outside of Scandinavia unless they chose to publish in a foreign language, that is, in English (Gerholm and Hannerz 1982).

With the next chapter, we rapidly approach the present, and we begin to recognise research interests that still have high priority in the new millennium. The radical Marxism of the 1970s lies at the root of various present-day research agendas. The radical feminism of that decade has been transformed into sophisticated gender studies. Research on ethnicity in complex societies has continued, and later spawned a burgeoning interest in nationalism. The new discussions of fieldwork methods that saw the light of day after 1970 were soon to be drawn up into wider debates on reflexivity and field ethics, which still elicit professional interest. On the other hand, the political awareness that was so powerful in anthropology during these years eventually receded, along with the optimistic hope that anthropological insight could change the world. Nevertheless, while anthropologists in 1968 were still grappling with problems that would soon seem outdated, several of the concerns of 1978 remain important today as well.

7
Questioning Authority

The Cuban missile crisis, the Berlin Wall, the civil rights movement, the Prague spring; hippies in Haight-Ashbury, student riots in Paris; the Beatles, the Moon landing, Kennedy's assassination, the Vietnam War – all this is emblematic of the 'Sixties', as the time is known in the West. But the radical political climate to which these events contributed did not come to bloom before the end of the decade, and belongs, strictly speaking, to the ten years *following* 1968. Certainly this was true in academia, where students shout their slogans but tenured professors remain as the years go by. Anthropologists, always a radical bunch, may have searched their souls more deeply than many other academics, but were no less subject to the academic routine. Enter the 1970s, the forgotten decade, sandwiched between Flower Power and the Iron Lady, between Richard Nixon's election victory and John Lennon's death, the decade when world population hit 4 billion, when we saw the Yom Kippur War breed the Middle East oil crisis, the CIA-assisted coup in Chile, the founding of Microsoft, the deaths of Mao and Elvis (overdue and premature, respectively), the first Polish pope and the Iranian Islamic Revolution. It was a decade of revolutionary dreams that would soon enough be crushed under the wheels of history – in anthropology as elsewhere.

As we get closer to our own time, we need to alert the reader, again, to the inevitable bias of in any book such this. As time goes by, the sheer size of the discipline must necessarily force us to be either extremely selective, or too superficial to be informative at all. By 2012, the American Anthropological Association alone had more than 10,000 paying members, and around the globe there are countless regional centres of academic and applied anthropology, each with their specific research traditions. No historian in the world could do justice to this growing multiplicity – which, by the end of the 1970s, was already well advanced.

In this chapter, we deal mainly with two of the most powerful intellectual currents to arise from the radicalisation of academia: Marxism and feminism. Both were insistently present everywhere in

anthropology in the 1970s, until disillusion set in and new agendas were set. However, gender and power had arrived in anthropology, separately and in tandem, and they were destined to stay.

But an account of the 1970s which concentrated exclusively on radicalisation would leave out some very important features of the anthropology of that period: it was also the decade when ethnicity studies came into their own, when sociobiology became a household word (to abhor or to emulate), when economic anthropology had its golden age. French anthropology had re-entered the international scene with Lévi-Strauss, and now a whole troupe of Frenchmen appeared beside him, with messages that were not only politically radical, but intellectually complex. It was a decade of controversies, and it is the first decade in which anthropology had grown so vast and interconnected that it was no longer possible for a single anthropologist to follow the entire discipline. For the authors of this book, this is the decade in which it was no longer possible to trace more than the most important trends and connections in the discipline. This chapter should nevertheless give an impression of some of the milestones of that decade.

THE RETURN OF MARX

Of the previous generation of anthropologists, Steward, White and Gluckman were the most decisively influenced by Marx – Steward in his materialism, White in his technological determinism, Gluckman through his interest in crisis and conflict. However, references to Marx in their work were all but absent. There are scattered references to Marx and Marxist theory in the work of a few Anglophone anthropologists in the 1950s and early 1960s, including Eric Wolf and Stanley Diamond in the USA, and Peter Worsley in Britain. But the ideological climate of the time was neither congenial nor receptive to Marxists; this was especially so in the USA, and not much easier in Britain. A card-carrying English Communist like Worsley had serious difficulties obtaining research permits and finding employment, before finally securing a job in sociology at Manchester, supported by Gluckman, who had had similar experiences himself (Chapter 5).

In Britain, the USA and France, all this changed quickly in the 1960s, certainly among students. Marxist theories of alienation, of ideology as false consciousness, of the infrastructure–superstructure distinction and the concept of contradiction, entered the academic vocabulary in the late 1960s, and many young anthropologists

began to engage intellectually with the old, hoary theory of social classes and historical change. But grafting Marxist theory onto contemporary anthropology was no easy task. As explained in Chapter 2, Marxism was chiefly a theory of capitalist society. Its attempts to describe and compare other modes of production and engage in long-term cultural history (the latter mainly undertaken by Engels after Marx's death) were influenced by Victorian evolutionism and Hegel's philosophy of history. Anthropology had been moving for almost a century since then. If anything at all held the sprawling discipline together in the mid-1960s, it was a commitment to empirical variation, a distrust of simplistic, universalist models, and an ingrown cultural relativism. The global pretensions of Marxism and its antiquated empirical base did nothing to attract anthropologists.

But Marx had a compelling vision of the modern world, which, in a context of ever more visible global disparities, seemed no less relevant to the 1970s than the 1870s. The Vietnam War, seen by many as a war of imperialist expansion, was the main item of foreign news for years. On fieldwork, too, anthropologists were increasingly exposed to such injustices, and many were eager to contribute to their demolition, as Wilson and Gluckman had done a generation before them. Now as then, Marx was the sociologist who spoke most eloquently of these problems, and so it was to Marx that the young revolutionaries flocked. No matter that Marxism was more than a social theory; that it had become the official state ideology of a substantial portion of the world and was thus a resource of political power. To a Marxist in particular, this should have been a fact of deep significance, though it rarely was. Instead, the power structures of the Communist states reproduced themselves in the organisations outside those states that fought for freedom from other power structures. Meanwhile, in Western academia, devastating battles took place between Maoists, Trotskyites, Stalinists, Anarcho-Syndicalists, and so on, ad infinitum, who nonetheless all united to face the common enemy, frequently personified in the local anthropology professor. Out of all this commotion grew the academic Marxian anthropologies.

There were several distinctive strands of Marxian anthropology. One, which we could label cultural Marxism or superstructure studies, arrived so late on the scene that it was post-Marxist before it was established in anthropology in the 1980s. This brand of Marxian theory was inspired by Antonio Gramsci's critical studies of ideology and hegemony and the Frankfurt school's (Adorno,

Horkheimer), critique of the commodification of culture, and entered anthropology via Edward Said's *Orientalism* (1978), a book that criticised 'exotifying' European representations of the Middle East (Chapter 8). Take Said's critique, mix with Michel Foucault's post-structuralism and add a dash of deconstructionism à la Derrida, and the result is the heady cocktail that would hit anthropology in the 1980s.

The two main flavours of Marxian anthropology were structural Marxism and political economy. It is a testimony both to the breadth of Marx's work and the scope of anthropology that there was little contact between these schools, and that the questions they raised were strikingly different.

Finally, there was also a fourth brand of Marxism, that followed up Marx's emphasis on the person as a productive, creative material body in a material world. We shall return to this 'sensual Marxism', with its roots in German Romanticism, towards the end of this chapter.

STRUCTURAL MARXISM

A harbinger of the new era was a paper published in 1960 by the French anthropologist Claude Meillassoux (1925–2005), who presented an unmistakably Marxian analysis of subsistence production in agricultural societies. Originally an economist and a businessman, Meillassoux had studied anthropology with Georges Balandier (1920–), a lone voice of sociologically oriented anthropology in structuralist-dominated France, who did fieldwork among the Guro of the Ivory Coast in the late 1950s. His research from the start had a Marxian orientation: not only did it concentrate on economic life, it tried to map the dynamics between the social *relations* of production and the technological and environmental *means* of production in Guro society. Meillassoux's article represented the first evidence of an emerging French Marxist anthropology. He would later develop a typology of 'pre-capitalist modes of production' in Africa, but, unlike his younger contemporaries, he was mainly a committed empirical researcher, and would be increasingly critical of the abstract theories that would soon dominate French Marxist anthropology. Indeed, among the French Marxists, Meillassoux was the one most sympathetic to the British school. In a preface to the English translation of his 1975 book *Femmes, greniers et capitaux* (*Maidens, Meal and Money*, 1981), he writes that Balandier had introduced him to 'the best of

current anthropology – that is British anthropology' and goes on to praise the work of Schapera, Gluckman, Monica Wilson and others. However, he notes, functionalism 'was based more on a sort of legalistic empiricism than on a thorough analysis of the content of economic and social relationships' (1981: viii), and he adds that it concealed economic exploitation by allowing kinship to permeate the field of enquiry. A main task for Meillassoux, then, was to extricate economics from kinship. This was not easy when writing about societies organised on the basis of kinship (Chapter 2), and he eventually proposed a mode of production new to the Marxist canon, which he called 'the domestic mode of production', based on the family and household. Interestingly, Sahlins, in his *Stone Age Economics* (1972), developed a nearly identical concept, but with a different aim: rather than reconciling African economies with Marxist orthodoxy, he sought to rescue economic anthropology from the perils of formalism, where the individual maximiser was the universal actor. In Sahlins' view, the household taken as a unit was not a maximising actor, and drawing on both Chayanov's early peasant studies and Mauss' theory of reciprocity, he argued that household-based production is not a means of maximisation, but a way of procuring necessities. To this Meillassoux answered that the household was not a productive, but at reproductive unit, it created the labour that was exploited by whatever system dominated it.

An enduring obstacle in Marxist theory for the new French anthropologists was the notion that power ultimately rests with control over the means of production, that is ownership of tools, fields, machinery, and so on. Since, in traditional African societies, such ownership is most often not individual but accorded to kin groups, there was a problem in locating power in the system. Meillassoux concedes, seemingly contradicting Marx, that 'power in this mode of production rests on control over the means of human reproduction – subsistence goods and wives – and not over the means of production' (Meillassoux 1981: 49).

Structuralism had little influence on Meillassoux. Others were more ingenious in forging links between Marx, anthropology and current intellectual sensibilities, including not only Lévi-Strauss' work, but also the original interpretations of Marx proposed by the philosopher Louis Althusser. When Althusser's *Pour Marx* and *Lire Le Capital* (the latter co-written with Étienne Balibar) were published in 1965, they had a major impact not only on French intellectual life, but on the new generation of anthropologists. Althusser's Marxism seemed to suit anthropology, since it introduced a measure

of flexibility in the infrastructure–superstructure relationship. A conventional reading of Marx would state that the infrastructure (material and social features of the process of production) determines the superstructure (everything else in society). In non-capitalist (or 'pre-capitalist') societies, it was often very difficult to see how this came about. Non-Marxist anthropologists simply would not believe it; it contradicted everything they had ever learned. In British anthropology, kinship was assumed to be fundamental; in American anthropology, culture – whatever that meant, but certainly not economics. And Lévi-Strauss (who explicitly but confusingly had called himself a Marxist) was concerned with the superstructure exclusively, as was Dumont, whose view that the values of society ultimately determine its power structure was directly opposed to that of the Marxists (Parkin 2005).

Althusser, who wrote extensively on ideology, made it legitimate to study rituals and myths as devices of domination. He further held that in a given society, any social institution can be dominant in the sense that it de facto dominates, but whether it does so or not will always ultimately be determined by the infrastructure. In medieval Europe, for example, the Church was the dominant institution, but the Church's dominance was 'in the last instance' determined by the feudal mode of production – and ultimately served the ends of that mode of production.

In the hands of the most famous of the French Marxist anthropologists, Maurice Godelier (1934–), the influences from Marx, Althusser and comparative ethnography merged with an equally strong admiration for the work of Lévi-Strauss. Originally educated as a philosopher, he was converted to anthropology by Lévi-Strauss and has done extensive fieldwork among the Baruya of New Guinea. The Baruya, with their non-monetary economy based on subsistence and barter, indicated important differences between capitalist and non-capitalist societies. Unlike Meillassoux and several other French Marxist anthropologists, who regarded structuralism as neo-Kantian, idealist mystification, Godelier – who worked with Lévi-Strauss in the early 1960s – saw structuralism as a real scientific advance. In his view, the Marxian concept of contradiction could make structuralism more historical, while the conceptual apparatus of structuralism was indispensable in locating the hidden mechanisms of society and culture. At one stage, Godelier even went so far as to suggest that Marx was a structuralist *avant la lettre* (1966, republished in Godelier 1977).

Godelier was also – like Meillassoux – concerned with kinship. Since kinship seemed to be 'everywhere' in traditional societies, he reasoned that it had to be seen as part of both the superstructure and the infrastructure (Godelier 1975). Rather than looking for particular institutions that took care of economy, ideology and so on, he proposed a 'formalised Marxism' that instead looked for *functions*. Such formulations further indicate the need felt by many of these researchers for a more flexible Marxian theory.

Much of the structural Marxian scholarship dealt with modes of production. Marx and Engels' old idea of an 'Asiatic mode of production' was unearthed and eagerly discussed, and notions of one or several 'African modes of production' were widely debated, following the research of Meillassoux and others on that continent. These debates died out towards the end of the 1970s, along with most anthropological attempts at grand typologies.

British Marxist anthropology was largely an offshoot of the French 'structural' variety. Since Lévi-Strauss, on the eve of the late 1960s radicalisation, was acknowledged as the worthiest partner in argument outside Britain for British theorists, the attraction of structural Marxism for young British radicals was easy to understand. (Of the alternatives, the older Marxism of the Manchester school seemed outdated by this time, and American Marxist anthropology was perceived as close kin to human ecology, a poorly understood and practically non-existent field in Britain, except at Forde's department at University College Loondon.) Symptomatically, the most important British Marxist anthropologist, Maurice Bloch, was of French origin.

The fundamental problem with Marxism in anthropology was, and is, that it is essentially a theory of capitalism, and that its account of 'pre-capitalist societies' was based on speculation and inadequate data. To reconcile orthodox Marxism with ethnographic research required a strong will, and as Jonathan Spencer (1996: 353) points out, when competent ethnographic analysis was carried out by Marxian anthropologists, 'it did become more obviously cultural, but looked less and less convincingly Marxist'. Nevertheless, many French Marxist anthropologists, notably Godelier, continued to publish anthropological work with a distinctly Marxian flavour through the 1980s and 1990s. Others, like Bloch and Marc Augé (Chapter 9), eventually changed their research priorities. Though Joel Kahn and Josip Llobera, in a review article from 1980, wrote that it was too early to 'produce a definitive critique' of the

movement (Kahn and Llobera 1980: 89), it seemed that movement had somehow expired while the article was in press.

THE NOT-QUITE-MARXISTS

While French Marxist anthropologists were often politically active, within or outside the Communist Party, this was rarely the case for the new generation of American Marxist or Marx-inspired anthropologists – though in retrospect it is easy to see that their efforts had more direct bearing on political issues of global injustice than the more academic contributions of their French contemporaries.

Marxist anthropology in the USA developed among Steward's, White's and Fried's students in the early postwar years. It began to make its mark by the end of the 1960s, flourished in the 1970s and peaked only in the early 1980s. The concerns of these scholars, inspired by the practical bent of Steward's research, continue to play an important role in anthropological studies of power and underdevelopment today. Though this generation of materialist Americans included some of the most important figures of the 1970s, some (like Marvin Harris) never really became Marxists, while others (like Marshall Sahlins) followed complex intellectual itineraries of their own, passing through a Marxist phase but eventually abandoning it. Sahlins (1930–), originally an evolutionist trained first by White, then by Steward, engaged creatively with the European Marxist debates about modes of production and forms of subsistence, arguing, in a famous, almost Rousseauesque contribution to the 'Man the Hunter' symposium, that hunter-gatherers were 'the original affluent society' (1968, reprinted in Sahlins 1972), and that economies of scarcity were the result of the inequalities imposed by the agricultural revolution. In 'On the sociology of primitive exchange', the centrepiece of his subsequent collection of essays (*Stone Age Economics*, 1972), Sahlins argued that the logic of generalised reciprocity, or sharing, was the norm in tribal societies, where the calculating, 'economising' actor of the formalist economic anthropologists was conspicuously absent. But as early as in this book, marked by Marxian concerns as it is, Sahlins was more convincing in his culturalist arguments than in his attempts to show causal connections between modes of production and symbolic culture. Then, in 1976, in his important theoretical treatise, *Culture and Practical Reason*, Sahlins angrily criticised Marxism for its reductionism and for not treating symbolic culture as an autonomous realm.

The itinerary followed by Sahlins – from cultural ecology via Marxism to symbolism and history – was no mere personal idiosyncrasy. Several other American anthropologists followed similar (but rarely identical) paths. Columbia-trained Andrew Vayda, whose research priorities moved, between the 1960s and 1980s, from a strong version of cultural ecology to a cognitivist and almost postmodern attitude to theory, is one example (Vayda 1994). A collaborator of Vayda in the 1960s and a long-time associate of Bateson, Roy Rappaport, also moved from a materialist to a cybernetic vision. In the long series of postscripts to the 1984 edition to *Pigs for the Ancestors*, Rappaport reveals decreasing commitment to ecological determinism and increasing sophistication in his Batesonian analyses of webs of communicative feedback.

With Marvin Harris (1927–2001) the situation was different. Though a student at Columbia under Steward, he remained committed to the Boasian mainstream in his student years, discovering White's and Steward's work only in the mid-1950s. Following fieldwork in Mozambique, when he was confronted with the miseries of Portuguese colonialism, Harris' politics were radicalised and his analytical interests sharpened. Over the next decades, Harris would develop his own research programme, or 'paradigm' as he might have called it, based on the notion that the material facts of economy and ecology *determine* culture – not merely 'in the last instance' but directly. In a famous article from 1963, he discusses the sacred cow of Hinduism, and concludes that the cow's special status may seem an exotic feature of Hindu religion, but is in fact a perfect example of economic and ecological rationality. Accusations of functionalism, to which Harris paid no heed, were inevitable. Later in the 1960s and 1970s, Harris' materialism became more pronouncedly non-Marxist, and in his main theoretical work, *Cultural Materialism* (1979), he spends half the book repudiating what he regards as inferior research programmes – from sociobiology and Marxism to 'eclecticism'. Harris was the strongest positivist materialist in American anthropology, and he saw the Marxian insistence on a 'dialectical relationship' between infrastructure and superstructure as a mystifying and non-scientific device. His opponents, and they were many, would class him variously as a crude evolutionist or a vulgar Marxist with no understanding of the subtler aspects of society. Harris published a popular textbook and, in 1968, a history of anthropology (*The Rise of Anthropological Theory*), which describes the history of the discipline more or less as a unilinear evolutionist narrative (with

minor branches and dead-ends), culminating unsurprisingly with cultural materialism. He also wrote several popular books, among which *Why Nothing Works* (1981), an amusing and provocative exposé of the irrationality of modern life, is a perennial favourite.

Even in Britain, the debate between Marxism and cultural ecology was activated. Studies of 'habitat' (ecology), pioneered by Daryll Forde and continued at his department at University College London, had otherwise hovered on the outskirts of respectable university life for decades. Now, cultural ecology finally found a few tenured adherents, and it soon became clear that although cultural ecology and Marxism pose many of the same questions, they answer them in profoundly different ways (Burnham and Ellen 1979). In a pyrotechnical demolition of Rappaport's work on the Tsembaga Maring, Jonathan Friedman (1979) – who had re-analysed Leach's Kachin work in a structural Marxist framework in his PhD dissertation – argued that Rappaport's ecological analysis of Tsembaga ritual fell into the classic traps of functionalism, by positing a 'great ecologist in the sky' as an omniscient subject regulating pig populations as needed. Rappaport's response was a sophisticated Batesonian plea for the unity of 'mind', which argued that in the conceptual universe of the Tsembaga there was *no difference* between the material and the symbolic – hence no 'great ecologist' or 'functionalism' was needed; the local symbolic vernacular was in effect a specialised discourse on ecology.

POLITICAL ECONOMY AND THE CAPITALIST WORLD SYSTEM

The leading proponent of Marxist or Marx-influenced anthropology in the USA was arguably Eric Wolf (1923–1999). The 'peak' of American Marxist anthropology alluded to above was the publication of his major work, *Europe and the People Without History* in 1982, a magisterial enquiry into the complex economic, cultural and political effects of colonialism on the peoples studied by anthropologists, and their responses. Here Wolf concentrates, as in much of his work, on features of the lives and histories of non-Europeans that had been neglected by generations of anthropologists. Wolf, originally an Austrian, was another of Steward's and Benedict's Columbia students, and he retrospectively pointed out that both his teachers had, 'each in their own way, intensified my own interest in how subgroups and regions came to be welded into overarching nations' (Wolf 1994: 228). A participant in Steward's Puerto Rico project in the late 1940s, Wolf later worked

on peasant issues in Mexico and published an important synthetic work, *Peasants*, in 1964. Opposed to the single-society approach favoured by most contemporary anthropologists, Wolf throughout his life explored how the destinies of localities are intertwined with larger-scale national and global processes. More often than not, the engine of these processes is economic profit, and the result is capital accumulation at the centre and exploitation in the peripheries. Peasants were, more than other groups, the victims of this exploitation. Deprived of land and producing for the global market at often ridiculous wages, they tended to live in poor countries, whose national autonomy was undermined because of unequal access to the world economy. Wolf was one of the first anthropologists who questioned the very concept of 'society', and preferred to think in terms of interconnected networks and social fields.

Wolf was not alone in pursuing interests in *world-system theory*, imperialism and underdevelopment during the 1970s. Along with Marx, anthropology had discovered Lenin, whose theory of imperialism was a logical add-on to Marx, and an alternative to the prevalent, but fading views of the civilising effects of colonialism. In a period when rural sociology was rapidly expanding, not least in Latin America, Marxist political economy seemed a natural part of any Third World-oriented scholar's field kit, particularly since social scientists were now engaged in development issues on an unprecedented scale (Grillo and Rew 1985). The most ambitious attempt at synthesis along these lines during the 1970s was Immanuel Wallerstein's neo-Trotskyist *The Modern World System* (Wallerstein 1974–79), a massive study of the evolution of a tripartite world of centres, semi-peripheries and peripheries, with accumulated resources flowing from the outer reaches to the centres. On a less grand scale, it was also the decade of Johan Galtung's structural theory of imperialism (Galtung 1971), which showed how global inequality was maintained through alliances between elites in the centres and the peripheries.

The 1970s also saw the rise of *dependency theory*, a close cousin of world-system theory. While theorists of development had formerly held that all societies would eventually 'catch up' with the West, a crypto-evolutionist position that was anthropologically unpalatable and hardly empirically correct, sociologists and economists like Andre Gunder Frank and Samir Amin, writing about Latin America and Africa, respectively, sought to demonstrate that exchange between rich and poor parts of the world – whether

de facto colonies or not – amounted to capital accumulation in the North and deprivation in the South. However, the dependency theorists tended to be non-anthropologists, and the main exception, Peter Worsley, seemed merely to prove this rule. Though Worsley had been Gluckman's student, he worked in a sociology department, and most of his work was in a sociological vein. It was not that anthropologists were cynical accomplices of world imperialism, that their cultural relativism had led them to moral nihilism, or that they were oblivious to the suffering of the world. On the contrary, many anthropologists had gone to great lengths to assist 'their' people, who were most often indigenous populations with 'authentic cultures'. But by the 1970s, the study of the millions of urban poor and semi-modern peasants had become a growth industry for anthropological research, and many anthropologists became engaged in development work and aid projects, with all the ethical, methodological and political dilemmas that this entailed.

The problem of anthropology's relation to issues of neocolonialism and Third World exploitation was at least fourfold. First, as suggested, the poor masses of the tropics were – outside the milieux inspired by Steward and Gluckman – generally not considered relevant to anthropological attention. They were 'too acculturated', and though ethnographic studies of modern people had existed throughout the twentieth century, it was only in the 1970s that they started to become common. As yet, the theoretical and methodological framework to deal with such groups was poorly developed. Second, the single-people approach favoured for both theoretical and methodological reasons by both the Boasians and the British, could not easily be reconciled with a concern with global political economy, though Steward's Puerto Rico project seemed to indicate a middle way. Third, most anthropologists related with reserve, if not hostility, to colonialism, which they often – for want of better alternatives – expressed by ignoring it (the only anthropologists to discuss such issues before the 1960s belonged to the Manchester school). This passivity was now heavily criticised. One of the most hotly debated books in British anthropology in the early 1970s was the Saudi-born anthropologist Talal Asad's edited volume *Anthropology and the Colonial Encounter* (1973), which argued that anthropology and colonialism had developed in collusion for several countries. Though many of Asad's claims have later been controverted, there was enough substance in his argument to stimulate significant self-searching in the discipline. Fourth, and not least important, the notion of 'development' was

– and is – difficult to swallow for anthropologists, who had been taught for generations to be sceptical of notions of social evolution. Lévi-Strauss said that he regarded himself as a 'fourth-worlder' as opposed to a 'third-worlder', meaning that he defends the small, vulnerable and unique peoples against not just the onslaught of Westernisation, but also against the development schemes of Third World governments (Eribon and Lévi-Strauss 1988). In this, he probably speaks for much of the anthropological community of the 1970s.

These issues, thorny as they were, could be overcome, as Wolf's work shows. Years earlier, Redfield had argued that peasants 'had their own culture', and though the quest for 'authentic culture' implicitly remained strong in anthropology, there were no strong academic arguments for not studying the hybridised, mixed cultures of, say, Latin America and the Caribbean, or for that matter, Belgrade and Hong Kong. A combination of in-depth fieldwork with a wider systemic and historical analysis was also perfectly viable, though it did not legitimise over-reliance on non-ethnographic material. The relationship to colonial authorities was irrelevant by the 1970s. What remained, then, was the problem of 'development', which seemed as difficult to overcome as the French Marxists' problems with infrastructural determination in tribal societies. Many of the most creative anthropologists who worked with issues of political economy in the 1970s had encountered this problem. In most cases they argued, true to the principles of anthropology, that development had to be defined from within, as an 'emic' (native) category. At the same time, they regarded the fact of global capitalist expansion as an objective, homogenising and unifying force in the world; indeed, Wolf, Mintz, Wallerstein and Worsley were active supporters of the globalisation studies that made their debut in anthropology in the 1990s (Chapter 9).

There was a distinct regional focus on 'the backyard of the USA' in the 1970s anthropological research informed by Marxist political economy and world-system theory. This regional focus tended, like Steward's earlier efforts, to stimulate academic anthropology and rural sociology in the countries under study. Unlike most places in the Third World, countries like Argentina, Mexico and Brazil offered established academic facilities, with a steady output of potential 'native' collaborators for Western anthropologists, with whom they could work on an equal intellectual footing. This was good news for the many young, politically committed Western anthropologists,

who carried out fieldwork among Latin American peasants within a Marxist theoretical framework (Melhuus 1993).

While French structural Marxism today appears as a dead end, it left a lasting imprint on the profession. It forcefully directed attention to the complex interweavings of local and global strands of inequality and power, resistance and survival; it grappled resolutely with historical change and the difficult relationship between 'development' and culture. Most important of all, perhaps, it directed the attention of a blandly Durkheimian or Boasian mainstream to the imperative material conditions for life. In the work of Godelier and Meillassoux, we may see the beginnings of a theory that addresses these issues in a wider perspective, and tries to wrest new meaning out of the distinction between the material and the ideational.

We have stated above that the boundary between structural Marxism and political economy was practically watertight. This was not always true in practice, and there are examples of 'crossover' scholars. The Argentinian anthropologist Eduardo Archetti (1943–2005) was one such case. Archetti did undergraduate studies in sociology in Argentina before going on to study with Godelier in Paris in the late 1960s, while Sidney Mintz, a Caribbeanist and another of Steward's students, was also teaching there. His main research interest was in the underlying logic of peasant societies and their relationship to the outside world. Under Godelier's supervision, he carried out fieldwork on peasants in Argentina, and wrote a doctoral thesis on which theories of underdevelopment and dependency made a deeper imprint than structural Marxism. When, in the mid-1970s, he began to teach at the University of Oslo, the professor who hired him reminisces that 'we took him on because we needed someone to teach the latest fads in French structural Marxism' (A.M. Klausen, personal communication). This, to remind us that intellectual trajectories are rarely simple, and boundaries are rarely clear-cut.

FEMINISM AND THE BIRTH OF REFLEXIVE FIELDWORK

In 1954, under the pseudonym of Elenor Smith Bowen, the American anthropologist Laura Bohannan published *Return to Laughter*, a candid and personal account of a (fictional) American woman anthropologist on fieldwork among the Tiv of Nigeria. The pseudonym was deemed necessary, because it was not

considered proper to speak publicly about the personal aspects of fieldwork, the doubts and mistakes, fortuitous circumstances and general disjointedness that lurked behind Malinowski's 'participant observation'. In 1966, Malinowski's old student Hortense Powdermaker published *Stranger and Friend: The Way of the Anthropologist*, which describes a life of fieldwork in the most varied localities. Again, the impression is that fieldwork is perhaps not quite the orderly data collection that Radcliffe-Brown envisioned. Then, in 1967, Malinowski's personal diaries from his Trobriand fieldwork were edited and published – and created an immediate scandal. The master himself, it seemed, had been no more than mortal. He had been homesick, had cursed the natives, masturbated and felt sorry for himself. How could anyone claim, after this, that he produced 'objective knowledge'? Later, many have defended Malinowski, notably Barth (2005), himself a prolific fieldworker, by pointing out that Malinowski had the good sense to separate his private rants and frustrations – which were perfectly understandable – from his scientific work.

Only a few years later, the participants in the 'rationality debate' would grapple with these questions philosophically, but in the meanwhile a group of young American female anthropologists responded more practically to the issue at hand. In 1970, the year Bryan Wilson's edited *Rationality* came out, the edited volume *Women in the Field: Anthropological Experiences* (Golde 1970) appeared. Each of the many essays in this hefty volume recounts the concrete circumstances under which the author's field research had taken place, and reflects on the effect of her experiences on the quality of her data. The essays are different, reflecting a wide variety of practices and experiences in the field, but they all agree on one thing: the fact that they were women had a profound effect on the conclusions they brought home with them. In this way, the idea of 'positioned' fieldwork emerged, the idea that the anthropologist can learn significant lessons about her own data by reflecting on her personal role in the field. Thus, *Women in the Field* put two debates on the agenda: How should self-reflexive fieldwork be conducted? And what role does gender play in social systems?

The first question was responded to with a series of detailed, practical accounts of how concrete fieldwork situations had in fact been tackled. A wonderful example of the genre is Rosalie Wax's *Doing Fieldwork: Warnings and Advice*, published in 1971, where the budding anthropologist is told, in no uncertain terms, What You Are Getting Into.

The second question – how can gender be studied anthropologically? – introduces the first male participant into this hitherto completely female account. He was the British anthropologist Edwin Ardener (1927–1987), who published 'Belief and the problem of women' in 1972. An Oxford anthropologist who carried out extensive fieldwork in Cameroon and Nigeria, Ardener's most important theoretical papers (collected in Ardener 1989) deal with social anthropology and linguistics, and with problems of translation, generalisation and intelligibility. He was also concerned with power, not least who had power to define the category systems of a society. Add Ardener's sustained interest in language – rare in British anthropology – and his contribution to the gender debate should come as no surprise. (His wife, Shirley Ardener, was a leading figure in British feminist anthropology at the same time, and edited two important collections in the 1970s (S. Ardener 1975, 1978).)

Ardener posited that '[t]he problem of women has not been solved by social anthropologists'. He emphasised that his 'problem' was not why women everywhere had lower rank than men, though that was also an important issue. His concern was with the conspicuous absence of women in most of the (presumably holistic) classics of anthropology, even in books written by female anthropologists (he mentions Richards' *Chisungu* (1956) as an exception). An important cause for this absence was general male bias in Western society, but this did not explain everything. Ardener argued that fieldworkers, whether male or female, more easily found rapport with male informants than with women, since men dominated the public sphere and were used to addressing outsiders. The cultural models ethnographers brought home were often formulated by men. Ardener described women as *a muted group*, not because they were silent, but because their statements were not easily translated into meaningful fieldnotes. The argument recalls sociolinguistic studies of class, race and language in the USA, that had shown that black and working-class children did badly in school because they were used to expressing themselves in 'context-dependent' language (Giglioli 1976). Ardener's paper and the debate it provoked raised similar questions in Britain as Golde's book had in the USA.

Two years later, the response came, again from a group of American women anthropologists, in the form of another edited volume, this time in a more theoretical vein: *Woman, Culture and Society* (1974), edited by Michelle Z. Rosaldo and Louise Lamphere. Two of Ardener's questions were dealt with there.

First, Ardener had presupposed – as a premise for the mutedness of women – that societies generally distinguish between a private and a public sphere, where men control the latter and women occupy the former. Rosaldo discusses this distinction comparatively, and confirms Ardener's view by showing that women's efforts are in most societies confined to the immediate vicinity of the home, due to the physical constraints imposed by frequent childbirths and long periods of lactation. Men have a wider radius of action and have time to spare to constitute a public sphere around such activities as ritual, politics and trade.

Second, towards the end of his paper Ardener noted that women are often associated with 'wild' nature, while men are considered essentially 'cultural'. Sherry Ortner, who would later write about the Sherpas of Nepal and publish several influential theoretical articles, responded with the classic 'Is female to male as nature is to culture?' (Ortner 1974). She writes that 'each culture, in its own way and on its own terms', regards women as 'in some degree inferior to men' (1974: 69), and describes the train of symbolic associations that connects the socially oppressed to the non-social world.

The enduring impact of *Woman, Culture and Society* – which was widely read throughout the next two decades – was at least partly due to its distinctly un-revolutionary tone. It was a collection of articles on gender, not a political manifesto. Along with several later anthologies discussing women and (increasingly) gender relations, it contributed to permanent changes in the research priorities of anthropology in the 1980s and 1990s.

It is not easy to give an overall assessment of the impact of feminism on anthropology. It is often difficult to discern the theoretical impulses from feminism against the rising tide of postcolonial and postmodernist work. More concretely, the emphasis on 'women's worlds' led to classical monographs on gender relations in various societies. We might mention Jean Briggs' self-reflexive account of emotional life among Canadian Inuit (1970), Annette Weiner's ambitious re-study of the Trobriand islanders (1976), Michelle Rosaldo's discussion of language and emotion among the headhunting Ilongot of the Philippines (1980) and Robert and Yolanda Murphy's gender-sensitive monograph on the Mundurúcu of Brazil (1985). These and other books challenged the orthodox view of traditional societies. In 1967, it was still acceptable to classify non-agricultural societies under the heading 'Man the Hunter'. In 1981, the collection *Woman the Gatherer* was published (Dahlberg 1981), which showed that women's work – gathering – provided

well over half of the food the 'hunters' ate. Though the importance of gathering had been pointed out before, it was by now no longer meaningful to speak of 'hunting societies'. They are either 'hunters and gatherers' or 'foragers'.

The influence of feminism is deeper and broader than these examples can show. As we will see, feminist anthropology laid the agenda for the next decades' studies of the anthropology of the body, of kinship and gender, of resistance among the oppressed, and of exchange. It is hard to imagine the numerous reworkings of the concept of power we have seen since 1980, without the feminist influence. The 1970s were also the decade when female students in large numbers entered the discipline. Women in anthropology had been muted (though not completely, as the careers of Benedict and Mead, Douglas and Richards show). Now they were about to speak.

ETHNICITY

A third trend in 1970s anthropology was ethnicity studies. This had several independent sources. The first was the work of the Chicago anthropologist George DeVos (1923–2010) and his associates at Berkeley (DeVos and Romanucci-Ross 1975). Inspired by the pioneering work of the Chicago school (Chapter 4), DeVos did extensive work on ethnic minorities and immigrant assimilation in the USA, Japan and elsewhere. A committed interdisciplinarian with a background in psychology, he remained loyal to the basic tenets of the culture and personality school, and was a major force behind the establishment of psychological anthropology as a sub-discipline. 'Ethnic identity' (a term DeVos introduced) was for him a matter of cultural psychology and self-identification.

Another, more sociological tradition studied 'plural societies'. As defined by the British-trained Jamaican anthropologist Michael G. Smith (1965), the term referred to societies in which clearly separated ethnic groups coexisted. Smith, originally a West Africanist who wrote increasingly from the West Indies, saw the constituent groups of plural societies as tight-knit, culturally distinctive corporations, competing fiercely for power. A controversial theme was whether such ethnic groups *are* in fact as discrete as Smith claimed, since they have often been subjected to intensive integration processes, not least in the Caribbean. This recalls the Chicago school's discussion of the American melting-pot as well as the Manchester school's problems with de- and re-tribalisation in Africa (Chapter 5). Then as now, the debate led to no definite conclusion.

Two other schools of ethnicity studies would be more influential, particularly in Europe. Both had their roots in British postwar studies of social change, and conceived of ethnicity as a factor in the strategic choices of individuals and groups. Both also emphasised the instrumental, political dimension of ethnic relations rather than their cultural content.

Around 1970, several monographs by British Africanists on urbanisation and social change appeared. Abner Cohen, yet another of Gluckman's students, published *Custom and Politics in Urban Africa* (1969), a study of trade and ethnicity in West Africa, which showed how Hausa merchants from northern Nigeria monopolised cattle trade through the use of networks based on kinship, ethnicity and, notably, religion. Writing from East Africa, David Parkin showed, in *Neighbours and Nationals in an African City Ward* (1969), how Luo tribal loyalties were transformed into modern ethnicity after migration to Nairobi. In these and other studies from the same period, the continuity with the Manchester school was strong. Indeed, Mitchell himself wrote an important contribution to Cohen's edited collection *Urban Ethnicity* (1974b). In Cohen's introduction, and in his theoretical volume *Two-Dimensional Man* (1974a), this continuity is abundantly clear. Among the influences that may be discerned are Victor Turner's insistence on the multivocality of symbols, Mitchell's discussion of the transformation of tribal loyalty into modern ethnicity, and Gluckman's original fusion of structural-functionalism and social conflict. Cohen nevertheless went further than his mentors, with his explicit focus on the dual – emotional and political – character of ethnic symbols, and his observation that political entrepreneurs manipulate such symbols to gain and guide the loyalty of their followers. As opposed to DeVos and Smith, Cohen went far in divorcing ethnicity from culture, stating, for example, that 'City men' (London bankers) might well be seen as an ethnic group. It was the social function of ethnicity, rather than its cultural content, that interested him.

The most widely influential of the ethnicity studies from this period was Barth's edited collection *Ethnic Groups and Boundaries* (1969). Based on a conference in 1967, and including contributions from leading Scandinavian anthropologists – including several of his former students – Barth's book, and particularly his introduction, was one of the most widely quoted works in academic anthropology during the last decades of the twentieth century. Like Cohen, Barth argued that ethnicity was chiefly a social and political, rather than a cultural, phenomenon. Barth, however, went on to say that it is

'the ethnic *boundary* that defines the group, not the cultural stuff that it encloses' (1969: 15). The relationship *between* groups, not the culture *of* groups, is the basis of ethnicity. The similarity with Lévi-Strauss' and Bateson's ideas of meaning (based on difference or contrast) is easily seen, and indicates the common interest of all these authors in formalist, rather than substantivist, concept formation, and in cybernetics. In Barth's book this formalism implied that ethnicity should no longer seen as an expression of shared culture, language, history or territory, but a result of social processes generated by individual choices. Ethnicity arises when groups meet and their interaction generates a boundary between them. The empirical studies that make up the body of Barth's volume explore this idea, and analyse the economic, political and demographic communication that takes place across ethnic boundaries. The studies demonstrated that significant cultural differences could exist within a single ethnic group, while different ethnic groups could have near-identical cultures. All that mattered, it seemed, was that the groups perceived themselves as different, and interacted on that assumption. Barth's formalist stance on ethnicity (all ethnic groups are defined by a universal *kind* of 'ethnic behaviour'), mirrors his preoccupation with formalist economics (Barth 1967). However, the success of formalism in ethnicity studies has been far greater.

Barth himself would later come to view his contribution to ethnicity studies as a preamble to the deconstructivist movement in anthropology (Barth 1994), where the very notion of cultural wholes with substantial content was questioned on epistemological, theoretical and methodological grounds (Chapter 8). If the leaders of ethnic groups virtually fabricated cultural differences for strategic reasons, what then remained of Boas' conception of cultures as unique and authentic? It may be objected that Barth's basically positivistic agenda is irreconcilable with the aesthetic and eclectic vision of postmodernism. Still, the practical effect of the two theories was in this context similar.

Soon it also became clear that Barth's understanding of ethnicity was compatible with the new interest in nationalism and globalisation that appeared in anthropology in the 1980s and 1990s. In some of that work, several of the trends discussed in this chapter came together: power and inequality from Marxism and feminism, the global context from Marxism, 'muted' discourses and reflexivity from feminism, cultural deconstruction and identity from ethnicity studies. And though the leading European schools reduced ethnicity mainly to a tool of politics, more complex analyses soon appeared

which emphasised the importance of subjective identification and ontological security for ethnicity. Two early books that developed these aspects were Manchester anthropologist A.L. Epstein's *Ethos and Identity* (1978) and A.P. Cohen's *The Symbolic Construction of Community* (1985).

PRACTICE THEORY

In 1984, Sherry Ortner, who had contributed to Rosaldo and Lamphere's volume ten years earlier, published the article 'Theory in anthropology since the Sixties'. Here she described a new, overarching theoretical paradigm that had been emerging in anthropology during the previous two decades, which she called 'practice theory'. Practice theory was, according to Ortner, an outgrowth of several long-term tendencies in the discipline. The old controversy between actor-oriented and structure-oriented approaches during the 1950s had lost much of its appeal, and the search was on for a new synthesis. The Marxian and feminist work of the 1970s was fresh in mind; neither were the debates of the 1950s and 1960s forgotten: on change and symbolism, formal models and rationality. Practice theory was not a formal school, but a loosely defined tendency encompassing many different research agendas, with a common meta-theoretical interest in unifying methodological individualism and collectivism, and in understanding human beings, not as abstract 'social positions' or 'roles, but as physical bodies in a material world, under pressure from historical and macrosocial processes, but resisting them. Though Ortner's references were mainly to American anthropologists, her conclusions were descriptive of important trends in European social science as well.

The idea of a social theory that could unify actor- and structure-orientation, as well as sociological and cultural perspectives, was not new, and had been approached – variously – by both Bateson, Geertz and Barth. The term 'practice' (or more philosophically: *praxis*) is derived from Marx, whose description of the human body as exploited by and resisting power underlies his value theory, and is one of the most powerful statements in the social sciences. The feminists, too, with their emphasis on power and gender, forced the body to the forefront of analytical attention, as did the new sub-discipline, medical anthropology, which became one of the fastest-growing specialisations in the discipline in the 1980s and 1990s.

Some of these concerns would lead to a hesitant *rapprochement* of anthropology and biology in the 1990s (Chapter 9). In the 1970s,

however, they attracted the attention of several leading lights of European social theory, two of whom will briefly be treated here, while the third will also be discussed in the next chapter.

In 1979, the sociologist Anthony Giddens (1938–), who has been described as 'Britain's best-known social scientist since Keynes', published *Central Problems in Social Theory*, a collection of essays that leaned heavily on Marx and Althusser, while also citing interaction theorists, such as Goffman and Barth. Giddens' explicit aim was to unify two dimensions of social life, which he referred to as *structure* and *agency*. In his *chef-d'œuvre*, *The Constitution of Society* (1984), Giddens develops this theory further.

The concept of agency, which in Giddens' work evoked a conscious, strategic actor, acting within the structural constraints imposed by power on his body, is nearly identical to Ortner's concept of 'practice'. Practice is also the preferred term in the work of the French sociologist and anthropologist Pierre Bourdieu (1930–2002). Born to a lower-middle-class family in a provincial town in France, Bourdieu studied in Paris (with Michel Foucault and Jacques Derrida; see Chapter 8), and did fieldwork among the Kabyles, a Berber group in Algeria, during the War of Algerian Independence in the 1950s. He was deeply influenced by Marx and Lévi-Strauss, Mauss, Durkheim and Weber, and his project has been to unify these influences into a simple but sensitive instrument for the study of human societies. Bourdieu wrote on a wide range of subjects, including class, sports, art, taste, architecture, power, gender and exchange, and his influence on anthropology has been wide-ranging and profound. His most influential work in the 1970s, *Esquisse d'une théorie de la pratique* (1972; *Outline of a Theory of Practice*, 1977; see also Bourdieu 1990), uses Kabyle ethnography extensively, but is basically a sustained theoretical meditation on the relationship between collective norms, social power and individual agency, as these are expressed through and by the human body.

Two aspects of Bourdieu's theory will concern us here. First, the idea of *habitus*, which he borrows from Mauss and the German sociologist Norbert Elias. *Habitus* is the permanent internalisation of the social order in the human body. The body inhabits a material world, a world of power, and a social world of people. The structural constraints inherent in these worlds are impressed on the body, forming permanent dispositions:

> … schemes of perception and thought, extremely general in their application, such as those which divide up the world in accordance with the oppositions between the male and the female, east and west, future and past, top and bottom, right and left, and so on, and also, at a deeper level, in the form of bodily postures and stances, ways of standing, sitting, looking, speaking, or walking. (Bourdieu 1977: 15)

Habitus is a pervasive *aesthetics* of action, which determines the actor in the manner of a dance – you cannot break out of it without loss of grace. At the same time, style, like dance, may be practised more or less skilfully, it may be utilised creatively, and opens infinite possibilities for variation and improvisation. Harking back to Bateson, and beyond him to Benedict, the concept of *habitus* seems to give tangible reality to the vague and general idea of ethos, by coupling it to power and the material world.

In the second part of his book, Bourdieu develops a model of symbolic culture, in which he distinguishes *doxa* and *opinion* as two basic forms of knowledge. *Doxa* refers to that which is taken for granted, which is beyond discussion and in many cases cannot even be articulated by members of society. Opinion, in contrast, refers to those aspects of culture that are open to scrutiny, discussion and dissent.

A third theorist with a profound impact on anthropological research on embodied practices, to whom we shall return in the next chapter, was the French philosopher and historian Michel Foucault (1926–1984). In 1975, Foucault published an acclaimed study of the rise of the modern prison system in Europe, which rested heavily on the concept of *discipline*. Discipline, like *habitus*, is structure and power that have been impressed on the body, forming permanent dispositions. Foucault, however, stresses the violence of this 'impression' more strongly than Bourdieu, and gives a more vivid sense of the cost of modernisation for whoever is subjected to it. This aspect of Foucault's work had a fundamental influence on the anthropological studies of power and violence that came to the fore during the 1980s and 1990s (Chapters 8 and 9).

In sum, the practice theorists opened up a whole new field of enquiry for anthropology by focusing on the human body as the central fact of social existence. This interest connected them – directly or indirectly – to another group of researchers, who had been exploring the interface between biology and sociology. This group included Victor Turner, whose late work on performance and

ritual had a strong body-orientation. It included Bateson, who (with Mead) had worked on body language on Bali, and had inspired such anthropologists as Ray Birdwhistell, who did technical work on the minute details of non-verbal communication. It also touched on the work of psychologists, linguists and cognitive anthropologists who had been exploring inborn or deeply imprinted linguistic and perceptual aptitudes (Chapter 9). Finally, it connected to the work of a group of biologists and physical anthropologists who created a major stir in the discipline in the late 1970s through an attempt to redefine anthropology as a branch of the study of evolution (E. Wilson 1975). The resistance among mainstream anthropologists to this work is illustrated by the fact that when Turner's posthumous 'Body, brain and culture' was published in 1987, his editor deemed it necessary to preface it with a long introduction, explaining that Turner had *not*, in his dotage, become a sociobiologist.

THE SOCIOBIOLOGY DEBATE AND SAMOA

In spite of its evolutionist overtones, Marxist anthropology was grudgingly acknowledged as a legitimate project by most of the senior figures in anthropology. Feminist anthropology was by and large welcomed as an elaboration of some of the perennial disciplinary concerns, and practice theory, particularly in Bourdieu's version was, if anything, embraced too uncritically. Not so with sociobiology. It was met with extreme hostility, and traditional antagonists – cultural materialists and hermeneuticians, British political anthropologists and French structural Marxists – temporarily joined forces to exorcise the evil spirit. The centrepiece of the controversy was the biologist Edward O. Wilson's book *Sociobiology* (1975). Most of it deals with non-human forms of 'social organisation', but in the final chapter, Wilson (who is considered the world's leading authority on ants) proposes to include the social sciences in the grand endeavour of evolutionary biology. He sees culture essentially as an adaptation in the biological sense; its main function consists in ensuring the production of offspring, and in order to understand what people are up to and how societies work, one has to see their activities in the light of the 'hardware' of their genetic apparatus. In Wilson's view, cultural phenomena such as religion, cooperation and morality should be seen as biological adaptations. In the intellectual circles of the time, where feminism and Marxism loomed large and Durkheim's spirit still moved upon the face of the waters, such bio-determinism inevitably caused uproar. At a public meeting in

1978, a member of the audience poured a pitcher of ice-cold water over Wilson's head as he entered the podium to speak, and others chanted: 'Wilson, you're all wet now!' The event serves to illustrate the emotions that seemed to be at stake.

As the reader will be aware, the idea of biological determinism was not new to anthropology. It had been fought off by Bastian's postulate of the 'psychic unity of mankind', then again by Boas in the face of racism and eugenics. Since the war, the demon had lain low, but after the early 1960s, a few anthropologists, and quite a few human behavioural biologists, started to think seriously about developing a Darwinian science of culture. Popular books with tantalising titles such as *The Naked Ape*, *The Imperial Animal* and *The Territorial Imperative* appeared in the late 1960s, claiming for biology areas that had been monopolised by cultural relativists and other social scientists for most of the twentieth century. These books were academically lightweight, and caused less anxiety than irritation among professionals. With the publication of Wilson's book, and three years later, his *On Human Nature*, social and cultural anthropologists had a target worthy of sustained attack.

And attack it they did. Even many evolutionary biologists, including Richard Lewontin and Stephen Jay Gould, went out of their way to discredit the view of humanity proposed by Wilson. In anthropology, Marvin Harris – who might perhaps have seemed a suitable ally for the sociobiologists – wrote a chapter on the movement in *Cultural Materialism*, where he concluded that the cultural variation in the world could be accounted for by reference to ecological, demographic and technological factors, and the sociobiological proposals were either trivial or wrong. Sahlins, who had just completed his anti-reductionist *Culture and Practical Reason*, responded with a small book, *The Use and Abuse of Biology* (1977), published well before the debate in the journals had come to an end. In the book, or pamphlet, Sahlins pursues several arguments. One is that sociobiology is a kind of social Darwinism, an ideology of individualism and competition masquerading as 'real science'. Another, more technical argument concerns Wilson's and his followers' concept of 'kin selection'. This principle states that a person's loyalty and willingness to make personal sacrifices is contingent on genetic kinship, so that one would be more inclined to make sacrifices for close genetic relatives than for others. Obviously, a cultural anthropologist would have misgivings about this kind of view, and Sahlins spends nearly half the book showing that the ways of reckoning kin vary immensely worldwide, and that there is

no necessary connection between genetic proximity and the social solidarity entailed by kinship. In a rejoinder to Sahlins, Richard Dawkins (in the second edition of *The Selfish Gene*, 1983) argues that cultural representations may vary, but this does not mean that practices vary accordingly.

The debate did not end there. Lévi-Strauss politely demolished sociobiology in his *Le Regard éloigné* (1983; *The View from Afar*, 1985), pointing out that the idea of 'inclusive fitness' was an empty explanatory category since it was so flexible in practice that it could apply to anything at all. In *Evolution and Social Life*, published a decade after *Sociobiology*, Tim Ingold (1986) devotes much attention to the book and the ensuing controversy. With an air of exasperation, he remarks that Wilson (in *On Human Nature*) unwittingly reinvents the nineteenth-century evolutionists' comparative method in his attempt to create a biologically based social science from scratch (Ingold 1986: 71).

Just as the sociobiology debate was cooling down, at least in the anthropological mainstream, the publication of a monograph on Samoan social life led to its resurgence. In 1928, Margaret Mead had published *Coming of Age in Samoa*, an intimate description of young girls growing up, which did much to force the idea of cultural relativism into the public eye in the West. In the interwar years, it had been instrumental in discrediting the then-powerful eugenics movement, which favoured 'selective breeding' of humans in order to improve their culture. The book was also an important inspiration for American feminism, and was often referred to by the new anthropological feminists of the 1970s.

In 1983, the Australian anthropologist Derek Freeman published a book-length assault on Mead's research, *Margaret Mead and Samoa: The Unmaking of an Anthropological Myth* (1983). Freeman had been doing research on Samoa on and off for decades, and at first, as he explains it, he had taken Mead's views on Samoan society for granted. Only gradually did he start wondering at the discrepancy between what he saw around him and the descriptions given by Mead. Nothing seemed to fit: gender roles, socialisation and sexuality were not at all as Mead had described them. In his book, he attributes these discrepancies, in part, to wishful thinking; in part to Mead's having been cunningly misled by her informants. Freeman's own analysis appeared to show that Samoa was a dreadful place to grow up. Rape, suicide and mental disorder were common, and – in stark contrast to Mead's romantic portrait of free sexuality among adolescent Samoans – there was an extreme cult of virginity.

Mead's academic work had been criticised before. It was generally agreed that her fieldwork in Samoa – as a young woman of 24, less than a decade after Malinowski's return from the Trobriand Islands – had been methodologically questionable, and that the monograph was probably not a major scientific work. The virulence of Freeman's attack, and the fact that he chose to publish it after Mead's death in 1978 (rather than face the matriarch's wrath; he had been working on the book for decades), contributed to the almost unanimous pro-Mead reactions the book provoked among American anthropologists. But the 1970s had been full of re-analyses, from Talal Asad's demolition of Barth's work from Swat (1972), to Jonathan Friedman's accusations of functionalism in Rappaport's study (1979), to Annette Weiner's respectful but thought-provoking follow-up critique of Malinowski (1976). None of these caused such a stir as Freeman's critique of Mead. Not only, it seems, was he criticising Mead; he was being obnoxious to the memory of an icon of liberal humanism, and, worst of all, he was doing it as a biologically inclined anthropologist. Freeman was not treated politely by his fellow anthropologists, and he quickly became *persona non grata*; but he continued his crusade until his death in 2001.

Interestingly, other Samoan specialists did not, on the whole, take Freeman's side. One might have thought they would. Was he not the real scientist of the two, working doggedly for decades, collecting voluminous materials, learning the language – while Mead was never more than a visitor with time for no more than fleeting engagements with her informants? Maybe so, but the specialists were cautiously equivocal in assessing the relative merits of Mead's and Freeman's research. One non-specialist pointed out, slightly tongue-in-cheek, that Mead's point that nurture was stronger than nature had been proven in the intervening years, since American society had moved from a very puritan view of sexuality to a more liberal view, not least because of the influence of her ideas. Some regional experts praised Freeman for having provided a more complete picture of Samoan society, while Lowell Holmes (1987), who had himself replicated Mead's work in the early 1950s, concluded by stating, in reference to his own work, that he would have loved to thrash Mead, but he was unable to do so: the resemblance between the real Samoa and the picture she had drawn was too strong.

Although the subject matter of this controversy is in itself bound to be engaging to any anthropologist, what interests us most in the context of the history of the discipline, is the degree of passion, not

to say aggression, it aroused. Even during the recent fierce debates about descent versus alliance in kinship, the assumed universality of male supremacy, the true Marxist meaning of infrastructural determination or the autonomy of culture, the antagonists rarely moved beyond irony in castigating their opponents. With the sociobiology debate and related issues concerning nature versus nurture, the politeness of anthropological debate was discarded, and the pitcher of cold water was never far away.

Before we return to these (and other) issues, we must take the plunge into the turbulent and turgid waters of postmodernism. Regarded by some as a source of salvation, by others as a dead (and excessively wordy) end, by still others as a tunnel with a light in the distance, the diverse intellectual currents summarised as 'postmodernism' had few concerns in common, but were responses to a particular historical situation.

8
The End of Modernism?

If every age has its ambience, that of the 1980s is unmistakable. The decade seems to roll in on us in a heavy cloud of AIDS, black leather, urban decay and crack. The sound of The Cure from a Walkman, drifting down the street, past the pale guy on the corner with his spikes and golden mohawk. Or watch the girls in tight pastels swooning to Michael Jackson and dancing till dawn – while the first clunky personal computers hit the home market, and the pale moon shines down on you from a sky that now contains ozone holes and greenhouse gases – weird phenomena, that an astute anthropologist will soon call *hybrids*. Another hybrid is buried in an unmentionable sarcophagus close to the small town of Chernobyl in the Ukraine. Reagan and Thatcher; Nicaragua and Afghanistan. Indira Gandhi assassinated, Saddam Hussein elected. Olof Palme assassinated, Mikhail Gorbachev elected. Right at the middle of the decade, deep-sea divers locate the sunken *Titanic* on the North Atlantic ocean floor, and plans are discussed for rescuing parts of the vast, luxurious wreck. But then the Soviet Union proclaims *glasnost* and *perestroika*, unilateral arms cuts, freedom of speech; and the world watches as the great Communist superpower falters and crumbles under its own weight. In 1989, the Berlin Wall goes down, and is sold piecemeal to individuals and corporations all over the world. Democracy and capitalism are triumphant. Nelson Mandela is freed.

The 1980s brought domestic political developments that few academics were happy with. The public sector was undermined, and universities restructured with a view to becoming more efficient. The future funding of 'useless' disciplines like anthropology became more uncertain than for decades. Competition and individualism were proclaimed from the rostrums and enforced in the lecture halls. After the outgoing, aggressive academic life of the 1970s, the 1980s seemed enclosed: claustrophobic or soul-searching. Disillusion was widespread among a generation of anthropologists who had recently thought they could change the world. By the end of the decade, some were saying that 'anthropology as we know it' was (or should

be) dead and buried, while others continued their with their job, sending students off on fieldwork and keeping the institutions going – organising conferences, editing journals, reviewing monographs, working on projects, applying for grants, and so on.

By 1980, anthropology had become a diverse and lively discipline with a number of clearly delineated research traditions. Despite the recent upheavals brought about by Marxists and other subversives, and despite the nearly constant self-criticism that anthropologists had practised for a decade or more, the leading theorists still commanded deference and respect. They were the generation who had cut their teeth in departments headed by people like Kroeber, Redfield and Herskovits, Steward, Firth, Evans-Pritchard and Gluckman, during the first postwar years. The rising star of American anthropology was Clifford Geertz, who had moved from Chicago to Princeton in 1970, two years after Turner *came* to Chicago and received his professorship there. Geertz was by now firmly established as the leading American symbolic anthropologist, admired for his eloquent and subtle interpretations. His contemporary, Sahlins, arrived in Chicago in 1973. He had abandoned neo-evolutionism in favour of Boasian Marxism (if such a thing can be!), but would soon move on to develop his own brand of structuralism, with particular focus on collective memory. All three phases of his work had their admirers. Schneider, also at Chicago, would soon (in 1984) proclaim that the concept of kinship was as good as meaningless; while Wolf, who published his magnum opus on the local impact of colonialism in 1982, had a sizeable following at City University of New York. Harris would move from Columbia to the University of Florida in 1982, after publishing his theoretical manifesto on cultural materialism in 1979 – the year that Bateson, pursuing his interdisciplinary interests in California, published his first and only major synthetic work, *Mind and Nature*, only months before his death in 1980.

Among British anthropologists, several left for the United States – among them Mary Douglas and F.G. Bailey. Douglas continued to produce important work in the borderlands between structuralism and structural-functionalism – some consider the little-known *Cultural Bias* (1978) her best book ever. In the next decade, she would publish *How Institutions Think* (1987), a remarkable defence of structural-functionalism at a time when, in most people's eyes, it was safely relegated to the mists of history. In Britain itself, Needham and Ardener had their followings at Oxford; Needham with his more Dutch than French brand of structuralism and an

ethnographic focus on South-East Asia; Ardener, the Africanist, with his 'post-structuralist' concern with language and cognition. At Cambridge, Leach retired in 1978, though he continued to attract attention for his theoretical views, influenced in about equal measure by Malinowski and Lévi-Strauss, until his death in 1989. Goody retired in 1984 and was succeeded by Ernest Gellner (1925–1995), a Paris-born Czech philosopher – who had joined the anthropology department at Cambridge in the early 1980s. Gellner was attracted to Malinowskian functionalism while still a student at the LSE, had converted to anthropology and published his first and only field study on – Moroccan saints (1969). Barth, probably the most versatile Malinowskian fieldworker in the history of the discipline, had recently completed studies in New Guinea and Oman, and was looking forward to new fieldwork in Bali and, later, Bhutan (previous fieldwork had been done in Iraq, Sudan, Norway, Pakistan (Swat) and Iran). In the mid-1970s, he had relocated from Bergen to Oslo – and from his old interests in economy, ecology and politics to studies of knowledge. In 1987, he published *Cosmologies in the Making*, a regional study of knowledge traditions in Highland New Guinea, seen from a processual and generative perspective.

In France, all roads still seemed to lead to Paris, where Lévi-Strauss officiated. He had witnessed the phenomenal rise of structuralism during the 1950s, and then the relentless attack on it by the younger generation, headed by Foucault and Derrida. He wrote new books, though he had few new students by now. Bourdieu had moved to Paris from Lille in the early 1960s, and became an international figure with the translation of *Outline of a Theory of Practice* into English, in 1977. Dumont, whose reputation as an anthropologist had been on the rise since *Homo Hierarchicus* was published in English in 1970 (until then, he had been considered, outside France, as a 'mere' South Asianist), had gone on to write challenging works on hierarchy and values, individualism and collectivism, 'the West' versus 'the East', and would acquire quite a following during the 1980s.

By 1980, it could no longer be said that anthropological research was limited to certain 'core regions', or to studies of exotic, non-Western cultures. In the wake of the methodological revolution of the 1970s, fieldwork in Western countries had become commonplace, and the 1980s would see a considerable output of publications in this genre, including Marianne Gullestad's *Kitchen-Table Society* (1984) – a study of working-class urban Norwegian women; and Katherine Newman's *Falling from Grace*

(1988) – a study of downward mobility among the American middle class under Reagan. Urban anthropology, pioneered by the Chicago and Manchester schools, had been established as an entirely respectable enterprise.

The older generation of anthropologists entered the 1980s with mixed emotions. For some, the retreat from political commitment seemed a betrayal of all that was sacred to anthropology. Others saw a chance to get back to work after a decade of stormy political debates. For yet others, it was the long-awaited opportunity to get rid of the old idea of anthropology as a natural science and institute a new humanism. An example of the latter was Victor Turner, who, in his posthumously published *The Anthropology of Performance*, writes of the 'systematic dehumanizing of the human subjects of study' in anthropological accounts, 'regarding them as the bearers of an impersonal "culture," or [as] wax to be imprinted with "cultural patterns," or as determined by social, cultural or social psychological "forces," "variables," or "pressures" of various kinds' (Turner 1987: 72). Turner had come a long way since his student years with Gluckman. In this book he calls for an experimental, playful anthropology that addresses the entire human being, as a living, sensual body. Turner welcomed postmodernism (though he disliked the label) because it, at least in some of its forms, offered a freedom from abstract systems and formal models, whether actor-oriented or structural, sociological or cultural. Formal models obscured the exuberance, creativity and humour of human life, and placed the scientific mind above real people.

There is a paradox hidden here, which we shall briefly point out. On the one hand, a theoretical hardliner like Bourdieu seems to be saying much the same thing as Turner. His concept of *habitus* has the express purpose of bringing out the richness of human interaction – by focusing on the *body* – which is just what Turner recommends. On the other hand, Bourdieu's whole project, with its totalising ambitions and its involuted, formal argument, seems to contradict Turner's intentions completely.

Among the younger generation too, the views and interests were diverse. It is enough to leaf through some of the monographs they wrote to be convinced of this. Take Gananath Obeyesekere's *Medusa's Hair* (1981), a psychoanalytically and medically inspired discussion of spirit possession in Sri Lanka; or Steven Feld's *Sound and Sentiment* (1982), a jazzed-up structuralist essay on music, natural sound and emotion in Papua New Guinea; or Katherine Verdery's *Transylvanian Villagers* (1983), tracing three centuries

of political, economic and ethnic change in a Romanian peasant community; or Henrietta Moore's *Space, Text and Gender* (1986), a study – inspired by Ricoeur and Marx – of gender, symbolism and power among the Kenyan Marakwet. Maurice Bloch's *From Blessing to Violence* (1986), the greatest and last structural Marxist thesis, treats history and power as expressed through an initiation ritual in Madagascar; while Bruce Kapferer's *Legends of People, Myths of State* (1988), delves into the cognitive underpinnings of Sri Lankan and Australian nationalisms, drawing on Dumont's notions of hierarchy and Turner's theory of ritual performance.

We could continue in this vein indefinitely, but we have seen enough to appreciate the wide range of issues and places discussed in these ethnographies. But we should also note their pervasive theoretical eclecticism. The influence of Marxism and feminism is often seen; and there is a tendency to focus on the body, on power, on ritual – but the authors seem more willing than before to throw in a dash of Lévi-Strauss without swallowing his whole perspective, or to apply action-based network analysis in essentially Durkheimian studies of social integration. Steven Feld's monograph, moving at will through the theoretical landscape, is perhaps the best example in the bunch. Postmodernism proclaimed the 'death of the grand narrative', it 'deconstructed' the great synthetic projects, leaving the fragments spread out on the ground. So it is happy days for individualists, in anthropology as elsewhere, and every anthropologist with respect for him- or herself, seems to create a private analytical toolbox, never to be recycled by anyone else, except in fragments.

A glance at some of the more explicitly theoretical works of the decade seems to confirm this impression. Take Anthony Cohen's *The Symbolic Construction of Community* (1985), a slim book on local identity based on data from Shetland, and on Barth's model of ethnicity – as opposed to Marilyn Strathern's *The Gender of the Gift* (1988), a vast and intricate work on exchange and gender among the Hagen of New Guinea, engaging with a wide range of theorists, including Mauss and Lévi-Strauss; or take Roy Wagner's *Symbols that Stand for Themselves* (1986), a neo-Lévi-Straussian excursus on symbolic creativity in European philosophy and Papuan ethnography – as opposed to Arjun Appadurai's edited work, *The Social Life of Things* (1986), a comparative discussion of consumption and value transformations in global economic systems, drawing on the value theories of Marx and Simmel.

All these varied and incompatible projects took place against the background of a more general academic movement. Ideas often called 'post-structuralist' were spreading. Foucault was becoming a household name among anthropologists. Heavy controversies turned around issues of representation, reflexivity and the very possibility of an anthropological science. If the 1970s were a decade of commitment, the 1980s were an age of doubt. And – partly as a result of the very individualism and eclecticism we have noted – this doubt also affected the integrity of the various national traditions in the discipline. Their century-old boundaries were beginning to blur.

THE END OF MODERNISM?

By the mid-1980s, many younger, particularly American, anthropologists spoke about a crisis in anthropology, a crisis in how anthropologists described – or 'represented' – the people they studied (Fabian 1983; Clifford and Marcus 1986). To various degrees, they accused the discipline of 'exoticising' the 'Other', of maintaining an untenable 'subject–object distinction' between the observer and the observed, which, it was argued, continued the 'othering' enterprise of colonialism by maintaining an indefensible, asymmetrical 'distinction' between 'Us' and 'Them'.

Their writings might be heavy with jargon, but the critics had much to offer in the setting of the 1980s. Anthropologists and others had argued that Western culture, and particularly the Western scientific and intellectual tradition, is heavily biased toward control, as embodied most visibly in the 'controlled circumstances' of physics laboratories (Latour 1991). As a science, anthropology shares this 'disposition' (as Bourdieu might call it) to control its objects of study. The mere planning of a research project assumes this. It is obvious that care must be taken at all stages of the project to keep the amount of – well – 'othering' at a minimum.

An early expression of this new sensitivity was Paul Rabinow's *Reflections on Fieldwork in Morocco* (1977), which, in line with the self-reflexive model of fieldwork that arose earlier in that decade (Chapter 7), describes the difficulties caused by power asymmetries (he had money, they did not), sketchy mastery of the language (he spoke good French, but bad Arabic) and his attempts to justify his project to himself.

But the postmodernist movement was less straightforward than this. Indeed, one might well ask if it should be considered one movement at all, since its main proponents often held widely

diverging views. There were in fact many different strains of 'postmodernism' (which was fully in keeping with the spirit of 'postmodernism' itself). Let us now trace the history of a few of these tendencies.

In the previous decade, Marxism and feminism had paved the way for the postmodern critique of anthropology. They had shown that knowledge and power were interconnected and worldviews were never neutral. However, the Marxists and feminists themselves presumably inhabited some kind of meta-level from which they could safely and critically observe and analyse the world. Strip away that, and what you get is postmodernism. It is as if one were to take away the authority of scientific observation and description from the Boasians and Malinowskians. All that remained would be an indefinite number of versions of the world.

The term 'postmodern', whose roots go back to Spanish literary criticism in the 1930s, was first defined in philosophy by the French philosopher Jean-François Lyotard (1924–1998), in his *La Condition postmoderne* (1979; *The Postmodern Condition*, 1984). Lyotard saw the postmodern condition as a state where there were no longer any overarching 'grand narratives' that could be invoked to make sense of the world. Different voices competed for attention, but never merged. The book, an unlikely bestseller, was intended as a critique of the standardising and 'flattening' effect of computerised information retrieval systems on intellectual discourse. It described a particular historical situation in the West, in which new technologies, new power relations and ideologies reigned. But postmodernism was itself an ideology (an -ism), an analytical perspective and an aesthetic that described the world (whether the world of the postmodern period itself, or any other world) as discontinuous and fragmented – a world of local, individual voices, rather than of hegemonic schools and ideologies. In architecture, film, literature and art, this attitude was eagerly embraced, resulting in a number of eclectic, collage-like productions, often playing ironically on nostalgic evocations of the styles and fashions of bygone days. In anthropology, the attitude was quickly associated with uncompromising cultural relativism, going well beyond the standards of all but most marginal anthropologists. All worlds and worldviews were equal – as long as they did not attempt to dominate each other. Each world was constituted by an independent 'language game' (a term Lyotard derived from the philosopher Ludwig Wittgenstein), and we are condemned to live in different worlds, without any overarching language to bring us together. Visions of democracy or universal human rights

were, as certain anthropologists had previously held, part and parcel of a culture-specific, Western ideology, and could never be value-neutral. We are reminded of Herder's critique of Voltaire (Chapter 1); the role of Voltaire being played notably by the German social philosopher Jürgen Habermas, who developed a theory of *Herrschafftfrei* ('authority-free') communicative action in the 1970s.

The direct impact of Lyotard on anthropology was limited. Of greater importance to the new generation of anthropologists was Michel Foucault. A philosopher and critical social theorist, Foucault's main works deal in part with the conditions of knowledge (1966), in part with the history of mentality (1972), in part, as we have seen (Chapter 7), with power and the body in the modern world (1975). His influence has been profound, on a par with Bourdieu's. When the *Times Higher Education Supplement* ranked the most-cited authors of books in the humanities and social sciences in 2007, these two shared the first place, with their old co-student Derrida as a somewhat distant third. Surprisingly, perhaps, Goffman comes out number six, in front of Weber, Freud and Kant, and way before Durkheim and Marx and the only full-blooded anthropologist in the brood, Clifford Geertz. Foucault showed, through historical studies of the treatment of deviance (insanity, criminality and sexuality) in Europe, how the taken-for-granted frameworks for understanding and acting upon the world have changed historically. He used the term *discourse* to delineate such frameworks. This term had been used by linguists for years, but in Foucault's usage it meant specifically a public exchange of ideas, in which certain questions, agendas and definitions – so-called 'discursive objects' – evolved as the result of power struggles between participants, and imposed themselves on the sensual human body. In his ruthless, intensely beautiful prose, often drawing on military analogies in its descriptions of discursive power and bodily discipline, Foucault spoke of the discourse as establishing a *regime of knowledge*.

At first sight, this theory might not seem to pose a challenge for mainstream, relativist anthropology, but rather to confirm its importance, in contrast to quantitative social science. However, anthropologists reading Foucault, notably Paul Rabinow (1989), stressed that anthropology was itself a regime of knowledge. Foucault's attack on power therefore not only struck at the cultures that anthropologists studied, but at anthropology itself. Courses in the history of anthropology could no longer depict it as a value-neutral accumulation of knowledge and experience, but would have to see it as a *genealogy* of discursive objects ('cultures'

or 'actors') that were constituted, debated and challenged through the impersonal discursive flow, and imbued with authority by the power contained in discourse.

The anthropological work inspired by Foucault in the 1980s belongs to two distinct categories: on the one hand, ethnographic studies of discursive power, such as Lila Abu-Lughod's work on gender and politics in the Middle East (1986); and on the other hand, critiques of anthropological enquiry itself (Clifford 1988). Either way, the Foucauldian perspective was compatible with views that had previously been espoused by Marxists, postcolonialists and feminists. All knowledge was *situated*, and more often than not it served to justify existing power structures. Moreover, as we indicated above and shall argue below, the perspective had a limited, but nevertheless striking affinity with a number of existing anthropological agendas, most notably with the cultural relativism of Boas and Benedict, but also with certain strands of British interactionist anthropology. Both Geertz and Barth would claim that their (deeply opposed) approaches were precursors of postmodernism. American interpretive anthropologists and European students of ethnicity were (along with feminists and some erstwhile Marxists) among the first to display an interest in postmodern thinking.

When Foucault studied at the École Normale Supérieure in Paris during the 1950s, he was part of a group influenced by the structural Marxist philosopher Louis Althusser and the semiotician and literary critic Roland Barthes. In the 1960s, this group of 'post-structuralists' vehemently attacked Lévi-Strauss, in part for his disregard for power, in part for the elegant sterility of his formal models. Jacques Derrida (1930–2004), a student of Foucault, soon to be the leading figure of this movement, expanded this critique to Western philosophy as a whole. He developed a method of analysing texts that exposed the hierarchical assumptions inherent in them, which he referred to as *deconstruction*. To deconstruct a text is to locate the centre of power in it, and then look for unnoticed, marginal expressions, which escape power and allow the reader to interpret the text in new ways. The paradoxical nature of this project – given that deconstruction itself had to be done in the form of written texts – was evident to Derrida, and his deconstructions therefore sought to deconstruct themselves. This made for an involuted, extremely self-reflexive style of writing, full of allusions, contradictions and irony, which in Derrida's own work is also meticulously exact – but in many of his admirers seems obscure at best, and at worst imbues

the text with a 'centralism' that is directly opposed to Derrida's aims. Derrida himself, who grew up in the periphery, as a 'pied-noir' Jew in French Algeria, had an intrinsic interest in the 'margins', and later in life would dedicate much energy to such causes as the abolition of apartheid.

Transferred to anthropology, Derrida's method effectively entails the end of ethnographic authority. There is no privileged, fixed 'I-view' from which one can make neutral statements of any kind. Every concept is slippery, every description can be contested. Again, these problems were hardly alien to anthropology. Ever since the 'rationality debate' (Chapter 6) and the new reflexivity's entry into fieldwork (Chapter 7) similar problems had been at the forefront of anthropological discussion, and before that as well they had been raised periodically. What was new about Derrida's proposal, was the suggestion that *any* text could be deconstructed. In other words, Winch's criticism of Evans-Pritchard's representation of Zande witchcraft was no more stable and valid than the text it criticised. In Derrida's philosophy, there is no fixed point of reference, no 'Archimedean point', to use a favourite postmodernist term. The potential for self-criticism, already significant in anthropology, was raised to unheard-of heights.

Though some anthropologists attempted (and usually failed) to follow Derrida straight through to the bitter end, there were also more moderate reactions. Thus, in 'The decline of modernism in social anthropology', Evans-Pritchard's old protégé Edwin Ardener (1985, republished in Ardener 1989) developed the idea that social anthropology was inextricably linked to *modernism*, loosely defined as an artistic and intellectual movement which sharply distinguishes modernity, on the one hand, from all other forms of human existence, on the other. Anthropological modernism, as epitomised, for instance, in Evans-Pritchard's work, built on several premises, including a clear subject–object distinction (the active fieldworker versus the passive informant), a 'primitivist' notion (traditional societies are stable, integrated wholes), and an idea of timelessness (the society under study is presented as 'the Nuer', not 'the Nuer in 1936'). Ardener argued that these premises were no longer tenable, and as a result, modernist social anthropology (functionalism, structural-functionalism, structuralism) had lost its momentum and legitimacy around 1980. Ardener's fear was that anthropological fieldwork would as a result be discredited, and texts would be produced that were nothing but commentaries on other texts.

THE POSTCOLONIAL WORLD

But other, related concerns were also on the rise, and contributed to the postmodernist movement in anthropology. One of these was the postcolonial movement in the arts and humanities, which challenged the right of metropolitan intellectuals to define who 'the natives' were and what they were like, and, more generally, questioned the aesthetic and intellectual authority of metropolitan judgements. Derrida had been close to such concerns, but two earlier writers would also influence anthropology profoundly. These were Frantz Fanon (1925–1961) and, on a less philosophical but more anthropological note, Vine Deloria (1933–2005). Fanon, a Martiniquan medical doctor and writer, published two books with a lasting impact on thought about power and identity in unequal group relationships. In *Peau noire, masques blancs* (1956; *Black Skin, White Masks*, 1986), Fanon performs an Hegelian analysis of the relationship between black and white man in the colonies. The book is a downright painful psychological portrait of the sense of inferiority and humiliation imposed on black people, who were convinced by their white masters that their only hope was to become white – but whose skin *could never* become white. The only escape was to hide behind the mask of 'the native', cloyingly obeying the master's wishes, while all the time, behind the mask, living an altogether different life. This book anticipated similar concerns in anthropology by nearly three decades. It was subtle and terrible, and later led Fanon to declare, in *Les damnés de la terre* (1960; *The Wretched of the Earth*, 1967), the need for a black revolution.

Vine Deloria was a Lakota Sioux Professor in Native American Studies, a theologian, lawyer and activist, whose much discussed book, *Custer Died for Your Sins* (1970), was an impassioned attack on all kinds of liberal (and not so liberal) authorities, who spoke about and on behalf of Native North Americans, thereby effectively preventing them from speaking for themselves. Deloria was furious with the Boasian anthropologists, whose relativism condemned Native Americans to eternal exoticism, and prevented them from attaining equality with whites.

In spite of these and other books written by non-anthropologists (the Kenyan author Ngũgĩ wa Thiong'o is another example), the 1980s postcolonial movement was effectively launched by an American professor of Literature of Palestinian origin, Edward Said. His *Orientalism* (1978) became a benchmark study, not only because of its originality, but because of its huge readership and influence. In

the book, Said argued that representations of 'Orientals' in Western academia were permeated by ambivalent fascination and disgust at the 'irrational', 'sensuous' and 'mysterious' East – an ambivalence that went back to nineteenth-century colonialism, but could trace its roots much further back (Chapter 1). Said argued that to Europeans 'The Orient' was a flexible location, consisting of very many and very different real societies, strung out across two continents from Morocco to Japan. Citing an infamous remark by Marx on Asians to the effect that 'they cannot represent themselves, so they need to be represented', Said held that Western studies of Asians, including anthropological monographs, had created an 'essentialised' – or 'reified' – image of their way of life, based on a simplistic and misleading dichotomy between 'Us' and 'Them', where the West represented science and rationalism, the East its negation.

Said's critique, which focused largely on work dealing with his home area (West Asia and North Africa), was dismissed by many regional specialists who felt that he discredited serious scholarly work and glossed over the diversity in Oriental studies. Nevertheless, the argument stung the increasingly self-critical anthropological community, and its concerns overlapped with those of the postmodernists. Said questioned the simple, unequivocal representations of 'whole cultures' that were common in anthropological research (though he seemed to approve of Geertz), and stressed the notion that knowledge was always 'positioned' (dependent on the social positions of known and knower). As in postmodernism, there appeared to be no privileged position from which neutral assessments of other peoples could be made.

'Postcolonial studies', which emerged as an academic discipline during the late 1980s, addressed the issues brought up by Said, Fanon and others, among them two influential theorists of Indian origin: the literary critic Gayatri Chakravorty Spivak and the cultural theorist Homi K. Bhabha. Both were more explicitly postmodernist in their approach than Said, but they shared his concern with suppressed voices – the illiterate, women, low castes, blacks – and with affording them a place in the sun by deconstructing the hegemony of white, Western, male knowledge.

The postcolonial perspective had an ambivalent reception in anthropology. On the one hand, anthropologists might with some justification feel that their discipline served as an antidote to Orientalism, since it was the only non-regional academic discipline to have its chief focus outside Europe. Had not a major objective of anthropology since Malinowski and Boas been to

offer sympathetic interpretations of non-European worldviews, and had not many important anthropologists – from Morgan and Boas onwards – defended powerless peoples against the forces of destruction, sometimes at great personal cost and to the detriment of their careers? The answer was yes, and yet many – within and outside the profession – could agree that anthropology often had a patronising tendency to represent others 'who were unable to represent themselves', and that the holism of many classical analyses projected an image of 'the other' as uniformly passive and changeless – an essentialised object of scientific inquiry. Thus, though Said goes unmentioned by Ardener, the latter's article on the decline of modernist anthropology has parallels with *Orientalism*.

In coming years, the debate continued. In 1983, the Dutch anthropologist Johannes Fabian published *Time and the Other*, which argued that anthropology tends to 'freeze' the peoples it describes in time. In 1990, the historian Ronald Inden, in his influential book *Imagining India*, demonstrated the relevance of the Orientalist critique for South Asian studies. Finally, in *Occidentalism* (Carrier 1995), several anthropologists and sociologists showed that not only do Westerners have stereotyped images of 'the East', but stereotypical images of 'the West' are widespread in the rest of the world.

In parts of the Third and Fourth World, partly as a result of the critique of Orientalism, anthropologists have become increasingly unpopular with national authorities as well as local intellectuals. They are seen as hunters for exotica and intellectual adventurers – part of the problem rather than the solution for people who struggle from day to day to be allowed to represent themselves on their own terms, as respected members of the global community.

Anthropologists responded in a variety of ways to these critiques. A lasting effect on the discipline has probably been that traditional cultural relativism, as highlighted by the Boasians, has become a difficult position to maintain. It is no longer politically correct for anthropologists to state publicly that they are, for example, opposed to the Universal Declaration on Human Rights for relativist reasons (as the American Anthropological Association did in 1947). Furthermore, as a result of the sustained self-criticism and the at times bitter confrontations with vocal representatives of studied peoples that took place in the 1980s, the attention of anthropologists was increasingly drawn towards the large-scale processes of global history. The systemic and historical approaches favoured by anthropologists like Wolf and Mintz seemed more and more

relevant for many young scholars, while synchronic, single-society studies increasingly seemed outdated and ethically problematic (Chapter 9). Finally, the Orientalism debate stimulated attempts to conceptualise the specificity of anthropological writing about specific regions. Thus, in 1990, British anthropologist Richard Fardon edited a volume with many prominent contributors which discussed the growth and transformation of 'regional traditions in ethnographic writing'. In his introduction, Fardon (1990) points out that such traditions, which typically associate an ethnographic region with specific analytical interests (exchange in Melanesia, lineage studies in Africa, and so on), are expressions of scholarly priorities which often have less to do with empirical conditions in the regions being described, than with established hierarchies in anthropology itself. However, he also stresses that such traditions have typically grown out of long-term exposure to the regions themselves through fieldwork, and they are therefore not arbitrary, but contain important insights into actual conditions.

Although its main objectives were arguably political rather than epistemological, the postcolonial critique of anthropology 'from the outside' to a great extent overlapped with the 'reflexive turn' that came from within the discipline itself in the 1980s, particularly in the USA. A handful of books published in the latter half of the decade may be seen as representative of this movement, and we now move on to a brief consideration of their message and impact.

A NEW DEPARTURE OR A RETURN TO BOAS?

What we might retrospectively call the postmodernist movement in American anthropology was associated with the work of a fairly small group of scholars. The core included non-anthropologist James Clifford, a historian of anthropology with leanings towards literary studies; and anthropologists Stephen Tyler (a convert to postmodernism from ethnoscience), George Marcus, Michael Fischer, Renato Rosaldo and Paul Rabinow. Others associated with the movement included Fabian, Richard Handler (a student of Schneider's studying discourses of nationalism), Lila Abu-Lughod (an Arabist), and Akhil Gupta and James Ferguson, who co-wrote and edited an important work on the discursive construction of space and narrative in the 1990s. In spite of differences (regarding, for example, the possibilities and limitations of ethnography), these and other scholars shared a number of concerns. They were uncomfortable with the reified 'othering' typical of classical

modernist anthropology, and sought to redress this, often by advocating 'experimental ethnographies', where the informants participated as equal partners in the production of knowledge (Clifford and Marcus 1986; Marcus and Fischer 1986). They were critical of the Boasian (and, more recently, Geertzian) idea of cultures as integrated wholes with deep historical roots. Inspired by Foucault and cultural Marxists like Antonio Gramsci (1891–1937), they were also concerned with modes of representation and the power implied by particular styles of writing.

The year 1986, in many ways an *annus mirabilis* for this movement, saw the publication of two important books and the launching of a new journal edited by Marcus, and entitled – with youthful arrogance – *Cultural Anthropology*. The first book was Marcus and Fischer's *Anthropology as Cultural Critique*, subtitled *An Experimental Moment in the Human Sciences*. Arguing that the discipline suffered from 'a crisis of representation', they went on to present several of the problems outlined above, and to affirm the importance of reflexivity (positioning the anthropologist's knowledge) and wider systemic concerns (incorporating an understanding of world history and economics in ethnographic analyses). They argued that a main objective of the discipline should be to engage in cultural criticism 'at home', and that an appropriate way of achieving this was by *defamiliarisation* – creating a sense of 'strangeness' by pointing out the similarity of the readers' own world to distant and 'exotic' cultures. In their view, the 1980s were a period of unique potential for the fulfilment of anthropology's promise as an instrument of cultural critique. The discipline was in disarray; the broad postwar consensus had broken down on both sides of the Atlantic; the postcolonial movement had generated uncertainty; grand theories had lost their appeal. In this situation, 'experimental ethnographies' could contribute substantially to critical self-reflection on Western society.

Anthropology as Cultural Critique, which emphasised continuity with the concerns of anthropologists like Mead, Sahlins and Douglas, was less radical than the edited volume *Writing Culture* (Clifford and Marcus 1986). Although the latter book contained about a dozen chapters written by different scholars, who represented various positions in the postmodernist debate, it was received as a single-minded assault on the dominant concept of culture. The contributors unanimously distanced themselves from the idea of culture as an 'integrated whole', questioned the rhetorical devices of 'scientific' anthropology, and argued the merits of both 'dialogic'

methods (the main inspiration here being the Russian literary critic Mikhail Bakhtin), and historical contextualisation in the increasingly problematic art of anthropological representation.

The hugely influential *Writing Culture* was followed two years later by Clifford's *The Predicament of Culture*, which could be summarised as a long, historically based argument against essentialism. In the same year, Geertz published a small, elegant book called *Works and Lives: The Anthropologist as Author*. This was a collection of essays about famous anthropologists that focused on the rhetorical and literary aspects of their writings, and was based on a lecture series which antedated *Writing Culture*. Ardener's predictions now seemed to be confirmed from the other side of the Atlantic: the anthropological quest had been brought to a close, since it no longer dealt with living people, but with texts; not with cultures, but with their representations. Of course, this view had limitations. Never had so much fieldwork been done, in so many different places, as in the 1980s. Yet it remains a fact that some of the most hotly debated writings of that decade were reflexive texts, which deconstructed ethnographic authority, questioned the ethical legitimacy of turning 'natives' into data, and ultimately challenged the validity of ethnographic representation as such.

The connection between American postmodernist anthropology and literary studies was strong. Both looked to recent French philosophy for inspiration, and the young anthropologists, steeped in Geertzian hermeneutics, were predisposed to see cultures as texts. Their break with Geertz consisted largely in problematising the subject–object relationship between anthropologist and informant, and in not seeing cultures ('cultural texts') as integrated wholes. However, these problems were not foreign to Geertz himself. He once compared an integrated culture to an octopus, a loosely coordinated animal with a weak brain that does not always know what each of its arms is doing; and in *Works and Lives*, he effectively deconstructs classical anthropological texts, as imaginative, historically situated works. Indeed, from a distance, American postmodern anthropology might resemble an extreme form of Boasianism, nothing entirely new. Geertz is the key figure here. Though the younger anthropologists, who congregated around *Cultural Anthropology*, had an ambivalent relationship to Geertz' hermeneutics, he was their favourite discussion partner. There was continuity between his view of anthropology and the radical deconstruction advocated in *Writing Culture*, but eertz himself felt that the younger generation had gone too far, and coined the term 'epistemological hypochondria'

to describe the excessive self-criticism that prevented people from doing good ethnographic work.

Many anthropologists would agree with Geertz or even go further in their criticism than he. Some of the more explicit critics were Steven Sangren (1988), who saw the 'reflexive turn' as a retreat from anthropology's proper mission, and Jonathan Spencer (1989), who argued that anthropology might be better viewed as a style of working than as a form of writing. Marvin Harris, in one of his many position papers, thoroughly thrashed (some would say parodied) the likes of George Marcus, who seemed to favour an increase in 'the number of experimental, personalistic, and idiosyncratic field studies carried out by untrained would-be novelists and ego-tripping narcissists afflicted with congenital logo-diarrhea' (Harris 1994: 64).

Ernest Gellner, in a little book devoted to the defence of rational science in the face of the two-pronged threat of closed-world fundamentalism and boundless postmodern relativity, berated the American postmodernists, singling out Clifford and Rabinow as his main targets, for their sloppy thinking and poorly defined concepts, and ultimately for being more interested in their own interpretations than in understanding the world (Gellner 1992). While Gellner saw Geertz as an important precursor of the postmodernist movement, he noted that Geertz, at least, was still trying to 'say something about something', as Geertz himself once put it. Against the critique of Great Divide (or 'Big Ditch') theories (that contrast 'Us' with 'Them', 'modern' with 'primitive', and so on), Gellner confirmed his position as a modernist in Ardener's terms. Elsewhere, Gellner (1993) also voiced misgivings about postcolonialism, for undermining scientific truth claims, confusing ideology and analysis, and not understanding that the 'problem of power and culture … is too important to be left to lit[erary] crit[icism]' (Gellner 1993: 4). Finally, Gellner hinted that there was an element of careerism in the postmodern movement, remarking that '*Sturm und Drang and Tenure* might well be their slogan' (1992: 27). While there is undoubtedly something to this, the same might of course be said of all innovative movements in anthropological history, from the Malinowskian–Radcliffe-Brownian 'functionalist revolution', via Steward's evolutionist and Barth's interactionist 'revolutions', to the 'revolution' in nationalism studies that Gellner himself had sponsored.

It was also often noted that many of the ideas inspiring American postmodern anthropology had originated in France, and that the Americans tailored the French masters to fit their anthropology,

often trivialising their views in the process. Foucault, in the American reading, became a theorist of discourse; Derrida a philosopher of relativism. Again, this may be true, but it is important to situate American anthropology itself as well. Boas, a German, had in his day understood very well the potential for ethnic and racial conflict in the sprawling, multinational United States, and events since his death have not proven him wrong. Clifford and Marcus may seem as crude as McDonald's to highbrow intellectuals from Paris, but relativism, even extreme relativism, was an understandable stance in a country split apart by a history of Afro-American slavery, Native American genocide and immigration from every part of the world.

Some critics doubted that postmodernism was as radical a departure from anthropological tradition as it claimed to be. However, this argument was double-edged, and was also used by some of the postmodernists to legitimise their project. Thus, Kirsten Hastrup, a Danish student of Ardener, whose work has been relentlessly anti-positivist, and has in some ways represented a European counterpart to American postmodernism, argued that anthropology *had always been* a postmodern science, ever since it started to confront the West with images of other lifeworlds (Hastrup 1995). Though Hastrup may have overstated her case, there are definite affinities between the deconstructive efforts of the postmodernists and several previous trends in anthropological history. Thus, there are precedents to postmodernism in the 'rationality debate' of the 1960s (Chapter 6) and in the 1970s revolution in fieldwork (Chapter 7), and debunking the objectivity of ethnographic method had been an anthropological parlour game on both sides of the Atlantic at least since the Second World War. But above all, of course, there is continuity with the historical particularism of Boas and the German Romantic tradition. In general, American anthropologists, who were steeped in this tradition, were more favourably disposed to postmodernism than their European colleagues, who were the heirs of confirmed positivists such as Radcliffe-Brown (Kuper 1996: 189). Schneider's deconstruction of kinship studies had nothing to do with postmodernism, but was the work of a devoted Boasian and a lifelong supporter of Parsons' sociology. Later, his work would be cited with approval in Britain too, by the Czech-British anthropologist Ladislav Holy in his textbook on kinship (Holy 1996), but Holy's point of reference is not Boas, but the methodological individualism of the British anti-structural-functionalist movement of the 1950s and 1960s (Chapter 5). This movement also often regards itself as a precursor

of postmodernism. When Barth, in the mid-1960s, demolished the concept of social structure and posited that stable social forms were unintentional aggregate outcomes of individual decisions, this was (in a sense) a deconstructive argument which closely paralleled the postmodernists' deconstruction of Boasian and Geertzian concepts of integrated cultural wholes.

Nevertheless, the postmodern critique of anthropology, with occasional support from feminist and postcolonial scholars, did represent something new, though its originality was often exaggerated at the time. The newness, as far as anthropology was concerned, lay mainly in the reflexive emphasis on styles of writing, in the rejection of a neutral and non-positioned authorial voice, and (most fundamentally) in the application of reflexivity to anthropology itself. After postmodernism, anthropology could no longer see itself as a privileged discourse with access to objective truth about the peoples it studied.

OTHER POSITIONS

Postmodernism and 'the reflexive turn' were not the only show in town in the 1980s. For most anthropologists, it was largely business as usual, as the discipline continued to grow and diversify into increasingly specialised sub-fields. Postmodernism – a new and untested departure – often had a noncommittal reception. Thus, in two turn-of-the-century introductory texts to anthropological theory, by Robert Layton (1997) and Alan Barnard (2000), postmodernism is afforded a prominent place, with about as many pages of text as structural-functionalism or structuralism. However, both authors are careful not to pass definite judgement on its merits and lasting effects on the discipline. In the massive *Companion Encyclopedia of Anthropology* (Ingold 1994), there is scarcely a mention of the movement at all.

However, during the 1980s and early 1990s, on both sides of the Atlantic, a very large number of anthropologists might be said to belong to the fringes of postmodernism, sympathising with some of its views, but incorporating them in established anthropological theory. These were largely scholars who, unlike Gellner and Harris, felt that their discipline was an interpretive activity whose claims to enduring truth were debatable. The most obvious example is Victor Turner, whose theory of performance would inspire many anthropologists studying ritual and related phenomena (Turner and Bruner 1986). Another example was the Melanesianist Roger

Keesing, who, in his last academic papers before his early death in 1993 (Keesing 1989, 1994), contended that the classical conception of culture had been mistaken. He now argued that 'his' people, the Kwaio, had no homogeneous, more or less stable culture, and that their ideas of their own culture were both politicised and influenced by ethnographic writings about themselves. Judith Okely and Helen Calloway's edited volume, *Anthropology and Autobiography* (1992), also paralleled some of the concerns of the *Writing Culture* group, but focused less on texts and more in the tradition of the reflexive fieldworkers of the 1970s. These latter scholars were the heirs of the anti-positivist, hermeneutic trend in British anthropology which began with Evans-Pritchard's late work. They turned the tools of hermeneutics inward, towards anthropology itself, to look critically at the juncture of knowledge production and personal experience. As in Keesing's case, the concerns of these authors were developed independently of the American postmodernists; Okely, for one, had produced a trenchant criticism of 'scientism' in anthropology back in the mid-1970s (Okely 1975). Another, unjustly neglected work of this general type, was the American anthropologist Robert Ulin's magisterial but little-read volume on cultural translation and rationality, *Understanding Cultures* (Ulin 1984). The book parallels the concerns of the *Cultural Anthropology* group, but instead of embracing postmodernism, applied an historicising hermeneutic method (as opposed to Geertz' more ahistorical hermeneutics) inspired by the German philosopher Hans-Georg Gadamer.

One of the most important anthropologists to be inspired by the deconstructive method was Marilyn Strathern, a Melanesianist at Manchester, who succeeded Gellner as professor at Cambridge in the 1990s. Strathern published several influential books in the late 1980s and early 1990s. In her magnum opus, *The Gender of the Gift* (1988), she explored concepts of personhood and exchange in Melanesia, and argued that Melanesian culture had been misread by Europeans who imposed their own concepts and prejudices on it. On a more general level, Strathern contended that classical theories of exchange and identity were defective in that they did not take gender into account. Is it meaningful, she asked, to say that objects are exchanged between two 'persons', or that a 'person' has a certain identity, if these 'persons' are *always* gendered? This critique brought feminist perspectives close to the heart of several foundational theoretical debates in anthropology, and thus increased their legitimacy in the discipline greatly. Later, in *After Nature* (1992), Strathern performed a comparison of concepts

defining personhood, society and kinship in Melanesia and England, making an argument that at once deals with a substantial issue (new reproductive technologies) and with the (reflexive) relationship between anthropological and native concepts.

Strathern represents a 'postmodern' approach that is perhaps more significant in the long run than the programmatic work of the American postmodernists. The same might be said of the somewhat similar project of the American Melanesianist Roy Wagner, who published *The Invention of Culture* in 1975, an influential theoretical essay that anticipated some of the central issues of postmodernism. Wagner here argued that cultures were symbolic constructions with an inherent capacity for change, innovation and reflexivity. In 1986, he further elaborated these themes in *Symbols that Stand for Themselves*, a complex account of symbolic transformation and continuity, which combines the stringent analysis of Lévi-Strauss with a reflexive and processual perspective reminiscent of postmodernism.

Wagner was one of a number of authors during the 1980s who started to explore the repercussions of applying *phenomenology* (a school of thought founded by the German philosopher Edmund Husserl) to anthropological analysis. An early proponent of such an approach was the British anthropologist Tim Ingold, who had previously done work on ecological adaptation. In 1986, Ingold published a major theoretical and historical work, *Evolution and Social Life* (Ingold 1986), where he developed a framework for the study of humanity in its social, cultural, biological and environmental dimensions, without reducing one to another. In at least two ways, this book is similar to the project of the American postmodernists. First, Ingold announces the need to 'clear out some of the accumulated conceptual debris of a century of social and evolutionary theorizing'; and, second, he emphasises that we 'cannot afford to maintain the illusion that we stand, like gods, aloof from the world' (1986: 376). The latter attitude clearly recalls ideas proposed by major phenomenologists such as Martin Heidegger (1889–1976) and Maurice Merleau-Ponty (1908–1961). Like these philosophers (who rejected the subject–object dichotomy on different grounds than the postmodernists), Ingold argues that people are intimately connected to the non-human world they inhabit. His solution to the subject–object paradox is therefore almost the opposite of the Americans'. Discarding postmodernism as intellectual aloofness, he instead proposes to bring anthropology

closer to other 'life sciences' (such as biology) – quite the opposite of comparing cultures to literary texts.

Ingold's movement towards biology parallels that of quite a number of anthropologists who, from very different perspectives, have sought to establish links between anthropology and the natural sciences during the 1990s. We shall return to this trend in Chapter 9; here, however, it is necessary to point out two related movements, that were already entering their boom years during the 1980s.

During the 1970s, Western countries increased their budgets for developmental aid to the Third World dramatically. The aid lobby became a power to be reckoned with in global politics, and anthropologists were increasingly drawn into the planning, implementation and evaluation of aid projects. Through UNESCO, the World Health Organisation, the World Bank, the European Union and other international organisations, through the broad range of NGOs that started engaging in aid-related work, and through the various national ministries of developmental aid that were formed at this time, the practical expertise of anthropologists was becoming increasingly in demand. From the start, the problem was to find niches where anthropological knowledge could be meaningfully deployed. The organisations were often dominated by representatives of 'hard' professions such as economy, law and engineering, who regarded concepts such as 'culture' and 'identity' with scepticism. Nevertheless, anthropologists quickly made their mark in a number of areas. The increasing interest in peasant studies and economic anthropology during the 1970s clearly bears witness to this, and as the years passed, and the practical problems of developmental aid became more apparent, anthropological viewpoints were increasingly taken into account. The successes should not be exaggerated (technocratic, economic and political interests still dominate aid work), but it should be noted that several key anthropological standpoints have gained wide acceptance. Anthropologists were among the first to argue the need to orient aid work towards smaller-scale projects, towards women (as the stable nucleus of households in many poverty-stricken areas) and towards ecological awareness – viewpoints which today are widely accepted, in theory, if not always in practice. The fate of other viewpoints has been more doubtful, and the fault lies not only with the development agencies: In 1973, Keith Hart coined the term *the informal sector* to designate economic activities that elude planners and statisticians: market exchange without ledgers and receipts, barter and mutual exchange of services, bootleg production, illegal migrant labour,

drug dealing and prostitution … the range is huge, and the values astronomical. Such activities are often of life-and-death importance in poverty-stricken urban areas, particularly in the South. To Hart, it seemed that anthropologists were the only category of 'experts' who would be able to discover and describe the informal sector convincingly. He may be right about this, but expertise in the informal economy has rather diminished than increased during the four decades that have passed since he wrote his article, due to a generally declining interest in economic anthropology.

One of the areas where anthropologists have most successfully forged alliances with dominant aid professions is in the field of health and nutrition. In the 1980s, when many anthropologists were disillusioned with aid work and the term 'post-development' was emerging (Ferguson 1990), *medical anthropology* grew to become the fastest expanding sub-field in the discipline. The roots of medical anthropology go back to the pioneering work of Audrey Richards in the 1930s, and to the efforts of a number of dedicated professionals who followed up and expanded her interests in the next few decades. An example is Ronald Frankenberg (1929–), who studied under Gluckman. Frankenberg did fieldwork in Central Africa and England (where he wrote about football, among other things), and has later written widely on questions of health and healing (Frankenberg 1980). He acted as a consultant on a number of aid projects, and did pioneering theoretical work on such issues as the conception of time in medical practice and the psychiatric understanding of children. During the 1980s he was drawn towards phenomenology and parts of the postmodernist movement, particularly the work of his old colleague and friend, Victor Turner, on performance, which inspired Frankenberg's interest in the uses of ritual in (traditional and modern) healing. Frankenberg is an example of how 'deconstructionism' can be put to 'constructive' use. His criticism of the essentialisation of concepts of disease and mental health in the medical sciences has inspired many to consider the social processes that generate such concepts.

In general, it might be said that medical anthropologists contribute an understanding of social context to standard medical work. The effect of a programme of regular health checks for pregnant women may, for example, be significantly enhanced by knowledge of the women's conceptions of propriety, their work schedules, the authority structures within their households, their kinship obligations, their conceptions of how disease expresses itself and what it means. Many prominent medical anthropologists are

themselves medical doctors or psychiatrists, which gives them a high level of professionalism and increases their legitimacy among medical professionals as well.

In the United States, where medical anthropology grew significantly during the 1980s and 1990s, one of the most influential figures has been Nancy Scheper-Hughes, at Berkeley. Scheper-Hughes, a student of Hortense Powdermaker's (Chapter 7) and a former civil rights activist, has done work on psychological anthropology and gender-related health issues in Ireland and Brazil (Scheper-Hughes 1979, 1992), and is at present working on a study of violence and democracy in South Africa. In 1987, she and Margaret Lock wrote the programmatic article 'The mindful body: a prolegomenon to future work in medical anthropology', which charted out an agenda for future applied and theoretical research in the field. In an interview, she described her fieldwork in Brazil in the following terms:

> … I started out with the question of the high incidence of infant mortality. What did that mean for women who had to face four or five, six, in some cases eleven deaths in a row? What did it mean for their understanding of motherhood? What did it mean in terms of their sense of optimism and hope? And what did it mean for the children who lived? How were they raised, given the spaces that they were made to fill [after] all the children who had died before? … I decided I would track these deaths … I did interviews with … close to a hundred women, getting them to tell me the context around each of the deaths of their children and what they thought were the causes of death. But also interviewing children, because children in northeast Brazil are the ones who bury the dead and they're the ones who form the procession. What do they think about death? (Scheper-Hughes 2000)

A third example from this sub-field is Arthur Kleinman, professor of medical anthropology at the prestigious Department of Global Health and Social Medicine at Harvard, who has done extensive work on cross-cultural psychiatry, particularly in China (Kleinman 1980; Kleinman and Good 1985). Kleinman, who has engaged in a wide variety of consultancy work, has in recent years been Director of the World Mental Health Project, a very large-scale consultancy project, sponsored by the Carnegie and Rockefeller Foundations, and organised under the auspices of the United Nations. Kleinman has also been one of the pioneers in the study of 'social suffering', which addresses the 'consequences of war, famine, depression,

disease, torture – the whole assemblage of human problems that result from what political, economic, and institutional power does to people' (Kleinman et al. 1997).

As several of the above studies indicate, medical anthropologists are by no means limited to applied research, indeed, with the recent *rapprochement* between anthropology and the natural sciences, the establishment of body-oriented theories of practice, and the growing interest in phenomenology, medical anthropologists have made very significant contributions to fundamental research in a wide variety of fields, during the 1990s. As we remarked in our discussion of Marilyn Strathern above, such engagement with anthropology's basic theoretical discourse tends to indicate that a sub-field (feminist or medical anthropology) has reached a mature stage, and is no longer a marginal 'special interest' in the discipline.

Finally, we shall briefly discuss a third important research field that came to the fore in the 1980s, namely the study of *nationalism*. Like medical anthropology, though for very different reasons, nationalism studies were less vulnerable to the postmodern epistemological critique than many other parts of anthropology. The research did not posit the existence of 'discrete, homogeneous cultures' existing in a timeless 'ethnographic present'. Rather, it explored a particular feature of modernity, in whose name the existence of such cultures was claimed, at least partly for strategic reasons, by political and cultural elites. As we have seen, the concept of culture had the same historical roots (in Herderian Romanticism) as political nationalism, and anthropologists who sought to deconstruct nationalist ideologies, thus had many interests in common with postmodernist deconstructors of culture. Though both empiricist and comparative in orientation, and thus potentially open to postmodernist attack, nationalism studies generally went unchallenged: they were neither essentialist (they deconstructed indigenous essentialisms), ahistorical (they located their object of study firmly in history) nor neocolonial (many major studies of nationalism focused on Western societies). Furthermore, as nationalistically motivated political conflicts spread worldwide during the 1990s, anthropological studies of nationalism seemed increasingly relevant. And the excessive violence of many nationalist movements, led some nationalism scholars to contribute to studies of the effects of violence (as in medical anthropology; see Malkki 1995), and to studies of power (as in political anthropology; see Kapferer 1988).

The interdisciplinary flourishing of nationalism studies that took place during the 1980s was inspired by three books that were

published in the same year. The first was Gellner's *Nations and Nationalism* (1983), where a main argument was that nationalism was a functional, cohesive ideology in an otherwise fragmented and alienating modern world. The second was historian Benedict Anderson's *Imagined Communities* (1983), which concentrated on the symbolic features of nationalism, comparing it to phenomena like kinship and religion. The third was historians Eric Hobsbawm and Terence Ranger's co-edited *The Invention of Tradition* (1983), which demonstrated that many presumedly 'ancient traditions' were in fact invented by colonial authorities or other elites to create cohesion where it would otherwise be absent. Often inspired by these books, anthropological research on nationalism seemed to confirm Ardener's view that modernism occupied 'an almost precisely datable span from 1920 to 1975' in social anthropology (Ardener 1989 [1985]: 197). Research on nationalism and, more generally, identity politics was seen as a form of postmodern anthropology. Powerful monographs on nationalism, like Bruce Kapferer's *Legends of People, Myths of State* (1988), combined classical concerns of anthropology – the meaning of myth, the problem of social cohesion, the power of symbols – with an attempt to come to terms with contemporary identity politics, its violent imagery and frequently violent practice, its creation of enemy images and its relationship to the State. Here again we see the movement of a sub-field from a relatively marginal position in the discipline, into its mainstream.

Although the debates on postmodernism stole many of the headlines during the 1980s, it is perhaps (as Derrida might say) in the margins of the postmodern movement, rather than among its most prominent spokesmen, that its most lasting contributions must be sought. Studies of exchange and identity, studies inspired by phenomenology, studies of health and nationalism, were all brought to the fore during this decade, and would deeply influence the anthropology of the 1990s and into the new millennium.

9
Global Networks

Lack of historical distance precludes a proper review of the last decades, whether of the general cultural ambience or of the specific enterprise of anthropology. In both regards, it is nevertheless obvious that some of the trends of the 1980s were consolidated towards and beyond the millennium. Uncertainty, or ambivalence, became a standard feature (some would say an affectation) of intellectual life, certainly in the humanities and social sciences; although trends representing a more positivist view, seeking less equivocal answers to the perennial questions of anthropology, were also on the rise. In particular, we have sociobiology and its successor, evolutionary psychology, in mind, but grand theory has also made a reappearance in the form of cognitive science. Nevertheless, Henrietta Moore, one of the most influential British anthropologists at the turn of the millennium (who in 2008 succeeded to the William Wyse professorship at Cambridge – occupied by Marilyn Strathern before her, and before *her*, by Gellner, by Goody, by Meyer Fortes, and before *him*, by the great pre-Malinowskian ethnographer John Henry Hutton, and before *him* by the linguist and diffusionist Thomas Hodson) introduced her *Anthropological Theory Today* with the sentence: 'It is very tempting to begin a book of this kind with the statement that there is no such thing as anthropological theory' (Moore 1999: 1). She goes on to note that the critical projects of the 1970s and 1980s had led, in the 1990s, to a widespread retreat from theory to ethnography and, in some cases, 'even from the project of anthropology itself' (1999: 1). She goes on to state that there is no longer (if there ever was) anything like a single anthropology and that the status of theory as such is increasingly questionable. 'Theory is now a diverse set of critical strategies which incorporates within itself a critique of its own locations, positions and interests: that is, it is highly reflexive' (1999: 9).

The impression one may get from this and other attempts to provide broad overviews of contemporary anthropology, is that the discipline is hopelessly fragmented and in profound disarray. For several reasons, this view is seriously misleading.

First, practising anthropologists rarely feel that their discipline has been deconstructed. This is because they usually work within a field of specialisation which is clearly delineated and continues to raise research questions that can be addressed with the methods at hand. This, on the other hand, means that anthropology as a generalist's discipline is probably on the wane. As early as the late 1960s, the subject had become too diverse for a single person to be familiar with it in its entirety.

Second, at a purely quantitative level, anthropology has been phenomenally successful during the last decades. The increase in the number of publications and conferences has been formidable, anthropological engagement in applied research has never been more widespread, and in many countries, anthropology had become a very popular undergraduate subject by the turn of the millennium. It makes sense to study anthropology in today's interconnected world, where travel, electronic communication and migration have brought people of different cultural backgrounds together more closely than ever.

Third, academic and applied anthropology, in more or less 'modern' versions, has spread from its original heartland in Britain, France and the USA to most, if not all, countries in the world. The spread has not always been easy, and in many cases modern, 'sociological' anthropologists are still a beleaguered minority, facing a more traditionalist consensus, often with nationalistic, and sometimes overtly racist, overtones. There exists a global anthropological identity, which, though embattled and disputed, commands the loyalty of anthropologists today.

Fourth, and partly as a result of this, anthropology has, half a century after Margaret Mead's heyday, attained near-global public attention, and key anthropological concepts and perspectives have made inroads into the global public sphere. This is nowhere more apparent than with the concept of culture. There is a broad and steadily-growing global awareness, among policy-makers, businesspeople, journalists, politicians, other academics and in the general public, that culture and cultural difference are *important*, and that anthropologists may be able to tell us *why* they are important and what their practical consequences are. Anthropologists today are the stewards of generations of anthropological thinking, and have a responsibility for it, vis-à-vis the public. As 'culture' becomes a global term, anthropologists are committed to assess and criticise the use and abuse it is put to. In the decade following the terrorist destruction of the Twin Towers in Manhattan, with its obsessive

focus on 'cultural' factors, such as religion, ethnicity and migration, this is particularly true. Sometimes, the services of anthropologists are called upon by the state, and not only in relatively uncontroversial contexts to do with, for example, development or migration. During the wars in Iraq and Afghanistan, anthropologists were 'embedded' among US forces to facilitate communication and basic cultural understanding, and this led to heated controversy in the anthropological community in the early 2000s.

There are obvious reasons for the continued relevance of anthropology today. Just as Marxian sociology offered a key to understanding the hidden agendas of class- and gender-related oppression in the 1970s, anthropology supplies an understanding of cultural variation in an age when multiculturalism, identity politics, intercultural terrorism, failed states, international migration, ethnic discrimination and nationalist wars stand high on the agenda of politicians, grassroots movements, NGOs and the media almost everywhere.

TOWARDS AN INTERNATIONAL ANTHROPOLOGY?

Speaking from the vantage point of 2013, the demographics of the discipline have become increasingly complex, though anthropology remained larger and more diverse in the USA than anywhere else. In the early twenty-first century, the American Anthropological Association had about 10,000 members, while the British Association of Social Anthropologists had less than a tenth that number, and the European Association of Social Anthropologists had around 2,000. The Brazilian, Japanese national anthropological associations, two of the largest in the world, each gathered about 2,000, while associations in Russia, India and Mexico together may have had another 3,000 members. Hundreds of other organisations existed around the world, some specialising in sub-fields that are marginal to the concerns in this book. All told, there may be as many as 25,000 anthropologists worldwide. This may seem an impressive number when we consider the minute size of the anthropological community a mere century ago, and indeed it is. However, the picture is not quite as rosy as it looks. The increasingly straitened financial circumstances of many universities in recent years have led to programme cuts and reduced teaching even at prestigious universities, and at less well-endowed institutions conditions are often critical. These are global concerns, but the noticeable downturn in the USA and Britain are of particular concern, since so many

of the resources (teaching facilities, libraries, museums, journals, publishers, research funds) upon which the global community depends, are located here.

Of the many national traditions we see today, some go back to the nineteenth century (for example, Russia, Italy, India), a larger group dates to the interwar years (China, Brazil, Mexico), a third and fourth wave came in the 1970s and 1990s. Quite a few have experienced longer or shorter hiatuses, due to political conditions (most prominently Russia and China, but also a number of Latin-American countries, where research was halted or severely restricted under various dictatorships.

The dominant position of the English language in academic discourse, which has been on the rise since the postwar years, was further strengthened in the 1990s and early 2000s. English supplies the global anthropological community with a lingua franca, but obscures the fact that most anthropology, outside the English-speaking world, is neither taught, discussed or published in English, and that proficiency in English, even in countries like Norway, is often inadequate for participation in scholarly debate. In Brazil, there are at least twice as many practising anthropologists as in Britain, but with few exceptions, their publications are unknown to people who do not read Portuguese (Peirano 2008; Ribeiro and Escobar 2006). Similarly, there are major bodies of anthropological and/or ethnographic literature in Spanish, Russian and Japanese, and anthropological publication in the national vernacular is on the rise in most other countries in the world. The nearly 600 members of the German Anthropological Association predominantly read and publish in German. The trouble is not only language-related. There is a major English-language anthropological literature in India, which is scarcely known outside specialist circles. One reason for this is that many Indian anthropologists address local applied concerns that have little audience outside India. Another reason is that global research agendas are generally set in the leading Western countries and may not fit the established priorities of Indian researchers. Finally, access to major publications is unequal. Your chances, as a Ph. student, to have your paper published in a prestigious journal, are greater if you study at Oxford or Chicago than at Dibrugarh or Karnatka.

Since the early 1990s, non-metropolitan European anthropologies have received increased attention from the centres. One important event was the foundation of the European Association of Social Anthropologists (EASA) in 1988. Formed at the initiative of Adam

Kuper at Brunel University, an important purpose of the EASA was to bring anthropologists from (Protestant/Germanic) Northern Europe and (Catholic/Romance) Southern Europe together. Then, in late 1989, while the anthropologists were busy planning the first EASA conference (to be held in Coimbra, Portugal in 1990), the world was struck by what would become the defining event of the 1990s: the fall of the Iron Curtain (soon to be followed by the dissolution of the Soviet Union) and the concomitant cultural and intellectual opening-up of most of Central and Eastern Europe. The changes that followed in the wake of these upheavals would be manifold and paradoxical – and would from the start open up a new ethnographic region of continental dimensions for anthropological research. Modern and traditional anthropologists and ethnologists from Latvia to Serbia were back on the map of European anthropology, in some cases (as in Estonia) with few local traditions to build on, in others (like Poland), with rich pre-existing heritages. But for the EASA planners, the immediate concern was the opportunity for intensified academic contacts with colleagues in these countries. Relationships were formed with anthropological traditions that had almost been forgotten in Western academia for decades. In the former Soviet Union, an ethnography was discovered that combined German diffusionist traditions with Marxian evolutionism (Dunn and Dunn 1974; Gellner 1980; Kuznetsov 2008). In Poland, the Chicago-inspired methodology of Znaniecki (Chapter 4) had been further developed into a sophisticated urban microsociology (Wedel 1986), and ethnography in Yugoslavia and later ex-Yugoslavia began to find its feet after the bloody breakup of the federation (Boskovic 2008). Hence, the EASA from the first faced not only the challenge of forging bonds between North and South in Europe, but between East and West.

Through the participation of anthropologists from all over the continent in EASA's conferences, and through recurrent panels on the history of European anthropology (Vermeulen and Roldán 1995), a picture of the discipline's European past has emerged which is far more complex than depicted in this book. Swedish ethnology, Polish surrealism, Slovene *Volkskunde*, Lithuanian semiotics, Slovak structuralism, are only a few examples of possible new genealogies for European anthropology. And outside Europe the situation is not much different. There is much of anthropology's history we have not yet told, and many potential futures we have not taken into account. Two glimpses of other traditions will serve to illustrate this point.

As we have seen (Chapter 2), pre-revolutionary Russian anthropology shared important features with the German cultural history tradition. This focus was retained during the 1920s and early 1930s, but most Soviet ethnographers were at this point also practical workers, engaged in such tasks as literacy work, schooling and health care. Anthropologists were in these years instrumental in developing the first written languages for many non-literate minorities of the Soviet Union. By the late 1920s, 'Russian ethnology resembled Western European and American sociocultural anthropology' (Kuznetsov 2008: 26). Then, under Stalin, both applied and theoretical ethnography were ruthlessly repressed, many practitioners were exiled or killed, and the profession was effectively reduced to empirical documentation and Marxist dogmatism. The 1960s and 1970s saw a resurgence of theoretically informed research with some degree of openness to international influence. A sophisticated theory of ethnicity with a decidedly non-Barthian slant was developed by Yuliy Bromley at the Moscow Academy of Sciences (Banks 1996); innovative work was done, for example, on statistical modelling of cultural cohesion; and large-scale ethnosociological surveys were carried out by Yuliy V. Arutyunyan (Moscow). In the West there was little knowledge of this work, in spite of the dedicated efforts of a few individuals, such as Stephen P. Dunn, a former student of Morton Fried's at Columbia, who founded and for 25 years edited *Soviet Anthropology and Archaeology*, a journal of translations that published a wide variety of Soviet scholarly literature in English. After the dissolution of the Soviet Union, Russian anthropology has undergone a painful process of diversification (Tishkov 1992). While anthropologists of the older generation often occupied leading positions at the dominating institutions in Moscow, St. Petersburg and Novosibirsk, and had vested interests in perpetuating Soviet traditions, many younger anthropologists (some never educated as anthropologists at all) looked to the West for inspiration (Condee 1995; Kuznetsov 2008). In St Petersburg, the foundation of the new European University, funded by the Hungarian-American billionaire George Soros, has to some extent crystallised this opposition, with many Western-oriented anthropologists clustering around Nikolai B. Vakhtin at the new university. In applied settings, we may find fertile cooperation going on between anthropologists, sociologists, psychologists, historians and others, and Western anthropological methods and theories are discussed with informed interest.

Brazilian anthropology presents a rather different picture. Here, as in Russia, the primary influence was German. Foundational studies of Native Brazilian American religion and cosmology were carried out by the self-taught German anthropologist Curt Nimuendajú (1883–1945). While the coastal tribes had been described many times during the last centuries, Nimuendajú (the name was given him by the Apapocuva Indians) decisively shifted the regional focus to the inland. He published more than 60 titles and did years of fieldwork. The interwar years was dominated by another German, Herbert Baldus (1899–1970), whose studies of Native South Americans were also very extensive. In the 1930s, he was awarded an academic position in São Paolo, where taught many students and established São Paolo as the centre of South Americanist studies in Brazil. It was Baldus who attracted Lévi-Strauss to the city, where he played a key role in building a modern anthropological research tradition, and São Paolo thus also came to be a major centre of French influence in Brazil, attracting many French anthropologists and utilising French methodologies and theoretical approaches. Meanwhile, the Brazilian-born Gilberto Freyre (1900–1987), who studied with Boas at Columbia and who became a highly visible journalist and politician, started up the first courses in cultural anthropology at the University of Rio de Janeiro, in 1935. Freyre opened up the field of Afro-Brazilian studies, wrote on poverty and oppression in Brazilian history, and established of a centre of US-style anthropology in Rio. Rio and São Paolo, with their different profiles, continued to dominate Brazilian anthropology into the postwar years. Meanwhile, substantial numbers of Brazilian anthropologists were educated, many going abroad (typically to the United States or France) for graduate studies. The Brazilian Association of Anthropology was established in 1955; by 1960, it had several hundred members. Brazil was becoming one of the largest and most dynamic anthropological communities in the world.

During the military dictatorship (1963–85), there were attempts to limit the scope of anthropological research and to close down institutions that were troublesome for the regime, but these were mostly fended off. Later, Brazilian anthropology has expanded substantially, with a wide range of themes are researched today, from urban anthropology and institution studies, to cultural hybridity. Studies of indigenous tribes and their modern transformations continue to be a major focus of research, and other research as well is almost exclusively carried out in Brazil. Brazilian anthropologists have actively supported the rights of their country's indigenous

tribes against governmental and commercial interests, and against anthropologists who overstep ethical boundaries in their research. As Mariza Peirano (2008) puts it, Brazilian anthropology – diverse, broad-ranging, applied and basic by turns – is an 'anthropology with no guilt': Brazilians carrying out research in Brazil do not represent a former imperial power, nor are they white people from the North building careers by studying poor people in the South. They engage with social and political conditions in their own country. But Brazilian anthropology has in latter years also given us outstanding international scholars like Roberto DaMatta (1995), who has published on urban life, carnival and Brazilian identity (at present based in the USA), and Eduardo Viveiros de Castro (1992), who has done groundbreaking work on shamanism among Amazonian Indians (and remains in Brazil).

These sketches of Russia and Brazil give at least a hint of the range of variation among the national anthropologies that are increasingly making themselves noticed internationally. They also indicate the greatest limitation of the present book. Our narrative has concentrated heavily on anthropology as it emerged during the nineteenth and twentieth centuries in three language areas: German (until the interwar years), French and English (British and American). We have made this choice, because these traditions have effectively defined the mainstream of methodological and theoretical development in the discipline. In part, this is a matter of definitional power, and if, say, the best of Brazilian or Russian anthropology had regularly been translated into French and English, the history of the discipline as a whole might well have been different.

Nevertheless, English is increasingly becoming anthropology's lingua franca, and lack of English-language proficiency is becoming a severe handicap. At the biannual EASA conferences, both English and French are official languages, but English is by far the most used; even many French anthropologists present their work in English on these occasions. Young scholars in countries with strong anthropological traditions are increasingly encouraged to publish their work in English. While there are perfectly sound academic reasons for this, the situation also creates a peculiar power asymmetry, since the English of non-natives is a poorer instrument of communication than their native language. Another, related question – on which there ought at least to be a qualified anthropological opinion – is whether the current linguistic convergence will ultimately lead to homogenisation or heterogenisation – to de-tribalisation or re-tribalisation, in the Manchester school's terms. On the one hand, a

wider variety of scholars, writing from different national traditions, are exposed to each others' work through the medium of a shared language. On the other hand, the very transformation of that work into a foreign language inevitably removes some of its qualities (Wierzbicka 1989; Eco 2003). Any anthropologist who works in two languages – and the present authors both belong to that group – knows that the richness and nuance of expression one cultivates in one's native tongue is difficult to achieve in a foreign language, which may even lack concepts for what one wants to say. Few are endowed with the linguistic skills of Malinowski, and even he was never truly 'at home' among the Trobriand Islanders.

TRENDS FOR THE FUTURE

Taking into account both the increasingly complex geography and history of the discipline, and its current lack of theoretical coherence, we shall now very tentatively outline some trends in contemporary anthropology. As of 2012, continuity with the past was still surprisingly strong – which may be reassuring or disquieting, depending on one's point of view. Participant observation remained the method of choice for procuring reliable and detailed data, though it was increasingly supplemented by other methods, and it was taken for granted that fieldwork in complex societies raises its own methodological issues. The idea that the world is socially and culturally constructed was also still shared by most anthropologists – albeit with a variable accent on postmodern relativism. The last two decades saw a general backing off from extreme postmodern positions, as the postmodern revolution, like others before it, was assimilated into the mainstream. There was a return to a more balanced ethnographic 'realism', a need to assert that anthropological knowledge may well be relative, but it is no less relevant for that. All-out particularism was fading into more pragmatic interests in empirical *interactions* between the universally human and the culturally particular. As a result, some old controversies resurfaced in a new guise; and some new research involved the rediscovery of older work. During the 1980s and 1990s, Mauss was rediscovered in at least three different contexts: as a theorist of the morality of exchange (Thomas 1991; Weiner 1992), as a theorist of personhood (Carrithers et al. 1985), and as a theorist of the body (Mauss 1934). All three fields remain major concerns into the twenty-first century.

However, there were also some new features to contemporary anthropology, which cannot go unmentioned. Let us note some

prominent examples. First, as seen in the previous chapter, any simple distinction between us and them, or observer and observed, has become difficult to defend. 'Natives' are perfectly capable of 'representing themselves', and are increasingly hostile to anthropological attempts to dictate who they are. This has contributed to a sharpened awareness of ethical issues in anthropology, that has increased ever since the American Anthropological Association – at the height of the revolution in fieldwork – published the first version of its 'Statement on ethics' in 1971. Today, ethical considerations form a routine part even of student work. Ethics also became important because 'anthropology at home' is no longer a curiosity, but a perfectly normal part of the enterprise – and ethical dilemmas are more clearly seen when we encounter them close to home.

Second, simple 'modern–traditional' dichotomies have also become indefensible, whether for epistemological or empirical reasons. Indeed, it seems to the present authors that this aversion to anything that even smacks of evolutionism is so strong in anthropology that it may amount to a blind spot. As the neo-evolutionists and neo-Marxists demonstrated in the 1960s and 1970s, it is easy to document empirical differences between small-scale hunter-gatherer societies and modern post-industrial societies, in terms of quantitative measures such as energy-flow per capita or power differentials. Such questions should concern us, since *homo sapiens* have inhabited the Earth as hunter-gatherers for some 150,000 years, while modern society is very recent. Since only about one-tenth of a per cent of human history has been spent in 'modern' societies, it stands to reason that general theories of human sociality are dependent on insights into the difference between 'primitive' and 'modern' social systems.

Third, the world has seen a phenomenal growth in transnational connections of every kind – from migration to tourism, from international wars on terrorism to the Internet. This acceleration of sociocultural mobility across all traditional ethnographic regions has led many anthropologists to question the often taken-for-granted link between groups of people and bounded geographical localities to which they 'belong'. The whole concept of space needs rethinking, as anthropologists increasingly study globally dispersed groups, such as refugees and migrants, workers in multinational firms or internet communities. The classical synchronic, single-site, single-society studies that used to be the hallmark of anthropology are becoming increasingly rare, and anthropologists are expected as a matter of course to contextualise their work both historically

and regionally. *Multi-sited fieldwork* (Marcus 1998) is no longer experimental and daring, but part and parcel of the methodological toolbox. We shall shortly return to some of the theoretical thinking that has emerged from this.

Fourth, and as part of this 'rethinking of space', we have seen a renewed interest in the physical territories occupied by people, whether ecosystems, cityscapes, or virtual realities – all of which was considered irrelevant by the extreme social constructionists of postmodernism. We sense an affinity between this interest in the physical environment, and the orientation towards the (physical) human body that was emphasised by the practice theorists (Chapter 8), and, indeed, Bourdieu places equal emphasis on the physical surroundings and the physical body in his theory of *habitus*. This suggests that a new *rapprochement* between anthropology and the physical world is taking place on very different terms than during the cultural ecology movement of the 1960s.

Finally, the general undermining of the concept of culture that had been going on since the 1960s, had, by the end of the 1990s, successfully discredited the old notion of 'a people' sharing 'a culture'. Thus, on the one hand, the idea of the social whole has been weakened, as 'society' dissolves into dispersed and overlapping networks or social fields. On the other hand, the idea of the material and bodily world has attained greater prominence. This perhaps suggests a long-term drift away from Durkheimian notions of society as a self-sufficient system, towards various kinds of alliances with the natural sciences.

This theoretical trend is one of two we have singled out for consideration in this final chapter of our history of anthropology. The second trend covers studies of globalisation and place. Our choice of these two subjects is obviously debatable, and there are other trends we might have discussed with equal justification. There has been very significant work done in anthropological research on exchange, both in the traditional heartlands of the discipline (such as Melanesia: Weiner 1992; Godelier 1999; or comparatively: Wyschogrod et al. 2002) and 'at home' (a North London street: Miller 1998; or the global economy: Hann and Hart 2011). These interests interface with issues in economic anthropology, which has lain dormant for two to three decades waiting for theoretical advances such as these (see Carrier 2012 for an overview). Work on economic consumption, inspired by the rediscovery of Georg Simmel since the 1980s, is already seeing something of a boom. The potential for cooperation with economists that arises from this, is

particularly great in our days, when 'new institutional economists' – the distant offspring of Polanyi's substantivism (Chapter 5) – are winning Nobel Prizes. Fundamental work is also being done on symbolism, history and power, inspired in particular by the work of Marx, Gramsci and Foucault (Herzfeld 1992; Trouillot 1995; Gledhill 2000; Comaroff and Comaroff 2006). An offshoot of this research is a growing interest in the anthropology of political violence (Malkki 1995; Nordstrom and Robben 1995; Tambiah 1996) and human rights issues (Wilson 1997; Wilson and Mitchell 2003); new research areas which may become prominent in the future of the discipline.

Our decision to focus on globalisation studies and studies of biology and culture does not imply that we consider these fields more important than the above. We do, however, consider the two trends to be particularly interesting in the context of the history of the discipline – in part, because they both 'push the envelope' of mainstream anthropology in noticeable ways; in part, because both have been the subject of major debates during the last decades. The two trends also present us with a number of interesting contrasts and overlaps. Roughly, we might say that they respond to the present state of anthropology and the world in two very different ways. The first seems to retreat from history and current complexities and to pose once again the old question 'What is a human being?' – thereby revitalising the nature/nurture controversy, which was in its day constitutive of the discipline. The second returns us to two other classical questions, 'What is society?' and 'What is culture?' – but now in a context of global flux and flow.

BIOLOGY AND CULTURE

Two complementary questions may be asked about the nature of humanity (Ingold 1994): 'What is a human being?' (answer: 'A small twig on a branch of the great trunk of evolution'; 'A close relative of chimpanzees', and so on) and 'What does it mean to be a human being?' (a question that generates a range of different answers, from Dostoevsky to Renoir, from Lévi-Strauss to Goffman). In twentieth-century anthropology the latter question has predominated over the former. Grasping the native's point of view was essential both for Boas and Malinowski, and Mauss and Radcliffe-Brown were both concerned with the nature of society rather than the nature of the human species. Explanations seeing culture and society as results of external forces, be they environmental or genetic, were

always minority views, though sometimes influential. During the last two decades, we have seen a revitalisation of the relationship between anthropology and several of the natural sciences. This movement is ultimately driven by the fact that the natural sciences are making use of increasingly complex models, that may be able to provide realistic simulations of biological, and to some extent even mental and social, processes. In the foreseeable future, the direct application of such models to qualitative social science will be impossible, since the models are dependent on quantitative input, nevertheless, as we saw in our discussion of cybernetics (Chapter 5), models from mathematics or natural science at times be used with profit as *metaphors* of social process.

It is the complexity of the new models that is their prime attraction for many anthropologists. With complex systems theory, natural science itself seems to have abandoned the unilinear world of cause and effect in favour of a probabilistic, multistranded universe that seems more familiar to social scientists – and also increases understanding for the social sciences among many natural scientists. However, there is still substantial distrust between these branches of academia, and misunderstandings are common, which impedes the exchange of ideas. Much work still remains to be done – on both sides of the divide – before true cooperation becomes possible.

In 1979, Bruno Latour and Steve Woolgar published their seminal *Laboratory Life*. This was a traditional, single-sited monograph based on Malinowskian fieldwork – carried out in a high-tech biochemical laboratory in California. The authors were careful, from the outset, to separate their task from that of the scientists they studied. While the biochemists were unearthing knowledge about the physical world 'out there', Latour and Woolgar asked how such knowledge became a social fact: how did a scientist recognise a 'result' when he saw it, how did 'results' circulate within the laboratory collective, to be criticised or accepted, defended, related to other 'results' and published? In their answer to these questions, the authors develop the rudiments of what Latour (1991) would later call 'actor-network theory', which links persons, objects and ideas in a network, where constant 'translations' (person to object, object to idea, and so on) take place. In the monograph's introductory chapter, by now a classic, the authors describe their entry into the laboratory, which is consciously exotified, to make it seem as 'unfamiliar' as a New Guinea tribe to the reader.

In the wake of this study, quite a number of sociological and anthropological studies of science, often referred to as 'STS' (studies

in technology and science) have appeared, ranging from general accounts of the embeddedness of science in large-scale economic and political processes to micro-studies of particular research milieux. However, the implicit critique of natural science that many of these studies conveyed, and their often post-structuralist concerns with knowledge regimes and ideology, did little to improve relations between anthropologists and their subjects of research. Latour and Woolgar generally avoided these pitfalls; nevertheless, their work was mostly either criticised or ignored by biologists and physicists. More recently, another controversy demonstrated that tensions are still considerable. In 1996, physicist Alan Sokal published an article entitled 'Transgressing the boundaries: Toward a transformative hermeneutics of quantum gravity' (1996) in the journal *Social Text*, where he argued that theoretical physics should be seen as a social construction and not as 'objective truth'. Shortly afterwards, he announced that the article had been a hoax; an incomprehensible argument full of postmodernist jargon and foggy thinking, and that the whole exercise had been an attempt to expose the vacuous anti-intellectualism of the postmodern cultural elite. While the Sokal affair did not involve anthropologists (his targets were chiefly literary theorists and philosophers), it offered an acute illustration of the continuing gap between humanistic and scientific approaches.

It is worth noting, however, that Latour is hardly a strong social constructionist. Indeed, in *We have Never Been Modern* (1991) he asserts that it is as futile to reduce physical science to social science as vice versa. Instead, he tries to develop an analytical language to describe the 'translations' that constantly take place between the two, ostensibly separate, fields. On the one hand, Latour's theory thus constitutes a frontal attack on the axiomatic separation of society and nature that has been constitutive of *both* the natural and the human sciences since Descartes. On the other hand, it calls attention to the *hybrid* nature of all scientific endeavours, to the fact that knowledge is transformed as it moves out of the laboratory or the fieldwork situation, into global networks of translation and re-re-translation.

This is worth keeping in mind in the following, where we will be discussing two broad families of anthropological approaches that engage (often through interdisciplinary work) with the natural sciences. The first family is linked to the expanding interdisciplinary field of *cognitive science*. 'Cognition' (which we might define as all mental processes associated with the acquisition and management of knowledge, including perception, memory, judgement, concept

formation, language use, and so on) is an old anthropological interest, which had taken many forms in the history of the discipline: from the *Ethnopsychologie* of Wundt and Durkheim's collective representations, through the Sapir-Whorf hypothesis, the 'rationality debate' and Lévi-Straussian structuralism, to American ethnoscience and the specialisation often referred to as cognitive anthropology (D'Andrade 1995). Since the 1980s, several of these concerns have converged with work in linguistics, psychology, neurology, evolutionary biology, artificial intelligence research and general systems theory, to form the new field of cognitive science.

Cognitive science remains a field in formation, which utilises a wide variety of methods (from computer-assisted tomography to participant observation), and poses a number of complex and potentially groundbreaking questions. New simulation models for complex systems have stimulated many branches of this research and, with software becoming more complex and hardware more powerful day by day, may in the long run change the way we understand human mental processes fundamentally. This information, which would have given Descartes pause for thought, has not been wasted on funding agencies worldwide, which have invested heavily in several branches of cognitive science. Influential research institutes and centres of excellence have been established, and extensive experimental and field-oriented research is carried out. All this means that cognitive science offers countless tantalising and suggestive questions, but so far – from an anthropologist's point of view – mostly tentative and fragmentary results.

The long-term impact of cognitive science on anthropology is hard to assess. However, it is bound to make an impression that cognitive scientists consider it proven that humans are not born as cognitive 'blank slates'. Ever since Durkheim, anthropologists have tended to defend the *tabula rasa* postulate to an extent that even Locke did not (Chapter 1). Human mental processes were socially constructed, and could thus be adapted freely to an infinite variety of conditions. In contrast, the new research demonstrates that our mind and sensory apparatus are specialised instruments, with specific potentials and limitations. Clearly, if positive knowledge of how these instruments function is forthcoming, it will be of the greatest interest to anthropology.

The incipient interest in cognitive science in anthropology itself was indicated by an early, influential study by Scott Atran (1990; see also Atran and Medin 2008), which develops themes from both Durkheimian sociology and ethnoscience, and argues that

there are inborn ways of classifying the natural world that are universally human. Otherwise, anthropologists working within the framework of cognitive science represented a wide range of persuasions. On the one hand, Lévi-Strauss' former student Dan Sperber (1996) developed an hypothesis of symbolic systems as networks of mental representations and public 'productions', in which a form of Darwinian evolution takes place. On the other hand, a number of theorists suggested that neurology might contain clues to an understanding of universal aspects of human cognition (Turner 1987; Bloch 1991; Borofsky 1994). Finally, such scholars as Bradd Shore, Dorothy Holland and Naomi Quinn (Holland and Quinn 1987; Shore 1996) adhered to varieties of *schema theory* or *prototype theory*, which postulate that cognition is organised around a limited number of prototypical 'elementary meanings', which are composites of biological hardwiring and social construction: 'up' and 'down' may be universal human categories (*Elementärgedanken*, as Bastian would have called them), but their meaning is understood differently (as *Völkergedanken*) on a Polynesian atoll and in the Andes.

A somewhat similar view is expressed in two books co-written by the linguist George Lakoff and the philosopher Mark Johnson (1980, 1999), in which the idea is advanced that human cognition and knowledge build on metaphors *based on* bodily experience. Bodily experience, which clearly has a universal component (we are all born with two hands), is also clearly particular to individuals and societies. Moreover, bodily experience is intimately known to each of us, and is therefore a fruitful source of analogies with other experiential fields, which are imbued with the ambience of particular bodily experiences. Lakoff and Johnson's work has been particularly important in subfields such as medical anthropology and the anthropology of knowledge.

A final example of this genre is the linguist Anna Wierzbicka's comparative work on concept formation in various European languages. In an influential comparison of Russian and English words for emotional states, she demonstrates that the two languages draw the conceptual separation of mind (or soul) and body in the two languages (Wierzbicka 1989). Such work might seem to be no more than a revival of the (relativistic) Sapir-Whorf hypothesis (Chapter 4), but in fact has a universalistic component, in as much as Wierzbicka seeks to discover 'semantic universals'.

The above examples – and many others could be cited – suggest a tentative revival of universalism in anthropology and a reversal

of postmodern trends, with their rejection (as some would have it) of anything reminiscent of scientific pretensions in anthropology. No matter how they stand on the nature–nurture issue, and here cognitive anthropologists differ, they consider cultural representations as 'enrichments of an intuitive ontology' (Boyer 1999: 210) and are intent on revealing the hardwiring underlying the soft and shifting 'cultural stuff'.

The universalism of the second approach we have chosen to highlight is more pronounced, though it is related to cognitive science, as witnessed in recent research on religion as a cognitive and evolved phenomenon (Whitehouse and Laidlaw 2007). The attitudes of anthropologists towards evolutionary theory, or neo-Darwinism, were – and are – contentious. Some see the accounts as dehumanising and politically irresponsible, or as attempts to reduce the richness of experience and sociocultural variation to genetics. Others see them as poorly supported by evidence and therefore impossible to assess. Yet others see great potential in the fusion of Darwinism, cognitive psychology and ethnographic research.

In anthropology, the sceptics have dominated the scene, and have marshalled support from a venerable lineage of anthropologists. Before the war, Boas, Malinowski and Radcliffe-Brown (Kuper 1999: xiii–xiv) were relentless critics of biological determinism, and the often associated racist pseudo-science eugenics, which many Darwinian biologists had supported (Malik 2000; Gingrich 2005). After the war, the orthodox view on both sides of the Atlantic was that biological accounts of human nature were irrelevant to anthropology. Anthropologists, indeed, rarely studied human nature as such, generally restricting themselves to accounts of particular societies or cultures, and maintained that society and culture were perfectly intelligible in terms of their historical development and internal dynamics. When universal questions were broached, they were generally content to postulate some abstract idea of human sociality (for example, 'maximisation' in formalist economic anthropology, or 'binary oppositions', as in Lévi-Strauss). Nevertheless, most anthropologists appear to work with an implicit theory of human nature, though they may hesitate to make it explicit. But if the postulate of the 'psychic unity of mankind' is accepted, which it must be to enable comparative research, then it would clearly be useful to be able to specify the scope and characteristics of this mental unity. This, after all, was Bastian's and Wundt's project (Chapter 2).

As we have seen, biological accounts of human nature briefly re-emerged in mainstream intellectual life in the mid-1970s (Chapter 7), when they were rejected by anthropologists. Since the early 1990s, however, sociobiology has re-emerged in a more sophisticated form, with greater potential for coalitions with social scientists – but again, anthropologists have generally rejected their advances. Nevertheless, the debate has become less aggressive; perhaps because of the 'cognitive turn' in sociobiology, perhaps because anthropologists have become more receptive to universalistic issues.

The project of a Darwinian social science, which would account for aspects of human social life and culture in terms of the evolutionary history of the species, can roughly be divided into two approaches (Knight et al. 1999: 1–2). One, 'evolutionary anthropology', addresses the biological evolution of the human species as well as the sociocultural evolution of human behaviour. Sociocultural evolution is often understood in the light of models of 'cultural transmission' (or 'communication', as anthropologists would call it), with an accent on learning. Certain practitioners within this extremely broad field have attempted to establish that human solidarity is determined by genetic distance. And while many anthropologists were put off by early claims that parents would be more willing to make sacrifices for biological kin than for adopted kin, newer research linking genetic distance to interethnic conflict, for example by Frank Salter (2004), has had more success. Many anthropologists feel that generations of anthropological research on social cohesion and conflict be taken seriously before general propositions of this kind are advanced.

The other approach, known as 'evolutionary psychology' (in fact a branch of evolutionary anthropology), 'has focused less on the functional consequences of behaviour than on the cognitive mechanisms believed to underpin it' (Knight et al. 1999: 2). This school is more hesitant at drawing inferences from the existence of particular behaviours or beliefs to their immediate adaptive value, and is more concerned with cognition and classification than with 'sex and violence'. The benchmark publication for this new synthesis was the omnibus work *The Adapted Mind* (Barkow et al. 1992), and the most dedicated proponents of the theory have been anthropologist John Tooby and psychologist Leda Cosmides. Deliberately avoiding the contentious label of sociobiology, they developed a theory of the human mind which saw it as composed of specific domains that originally evolved as adaptive responses to the 'environment of evolutionary adaptation': the environment in which the human

species originated (most likely the upland savannahs of the East African Rift Valley), and their adaptation to that environment, as hunters and gatherers. The defining features of the human mind were thus originally adaptive (enhancing the species' potential for survival), but in contemporary contexts may well be maladaptive. Again, there would seem to be good reason for anthropologists to welcome positive knowledge in this area – but so far the research of these scholars is too inconclusive and fragmented to be of much use. Moreover, though several evolutionary psychologists have tried to account for the relationship between biological evolution and cultural change (Boyd and Richerson 2005), the school has not developed a theory of cultural change, which makes it seem singularly inappropriate in today's world. In this respect, it is worth noticing that the neurolinguist Steven Pinker, whose *Blank Slate* (2004) was an unanimous defence of an evolutionary perspective on the human mind, later published a book about the decline of violence (2012), which indicated that human nature might be more malleable than he had previously thought.

At the end of the day, and in spite of numerous refinements, evolutionary psychology is still seen by most anthropologists as a form of biological reductionism, and has made few inroads into the mainstream of social and cultural anthropology.

In 2000, this and other issues were thrust into the public eye. In 1968, the US anthropologist Napoleon Chagnon, who had conducted long-term research on the Yanomami in Brazil and Venezuela, published *Yanomamö: The Fierce People* (1968), which became a bestseller used in introductory courses to anthropology worldwide. Unlike Mead's bestseller on Samoa, Chagnon portrayed the Yanomami as extremely warlike and aggressive, traits he explained in sociobiological terms. The book, embraced by sociobiologist E.O. Wilson, was resented by many anthropologists. In 1988, the Brazilian Anthropological Association raised ethical issues concerning Chagnon's research and asked the American Anthropological Association to review his case. This was not done. Scandal erupted in 2000, when *The New Yorker* magazine, renowned for its meticulous fact-checking, published an article by the journalist Patrick Tierney, accusing Chagnon and his collaborators of causing a deadly epidemic of measles among the Yanomami as part of their sociobiological research. In response to new demands from their Brazilian colleagues, the AAA reviewed Chagnon's case, clearing him of most, but not all, charges. The highly visible debate that ensued included reviews by several other academic bodies as well,

which agreed that Chagnon was not at fault, and Tierney had wilfully distorted the facts, but the AAA only rescinded their report in 2004, dropping the remaining accusations against Chagnon. In 2011, Alice Dreger, an historian who had researched the case for two years, confirmed that there was no substance in the accusations at all, and accused the AAA of subjecting one of its members to an harrowing review process on the basis of questionable journalism, without checking the facts or inviting Chagnon to defend himself.

Two points in this complex case (see Borofsky 2004 for a review) are worth noting in the present book. First, the AAA's action against Chagnon was influenced by several factors, including his sociobiological convictions, pressure from the powerful Brazilian anthropological community and the self-critical attitude of metropolitan anthropologists fostered by postmodernism. These were all extraneous to the case at hand, but highly relevant within the context of the history of anthropology. Second, that the prestige of Tierney's highbrow publishers and the virulent, lowbrow debate on the internet that followed, conspired to damage not only Chagnon's reputation, but the reputation of anthropology in the USA and globally.

Issues of research ethics, advocacy and the obligations of anthropological professional organisations were in this debate fused with the theoretical debate on evolutionary anthropology, and the still broader issue of how anthropologists are enmeshed in an increasingly interconnected world. Theoretical debates that could previously be contained within the anthropological community, are today liable to expand into globalised and uncontrollable controversies, with potentially catastrophic results for individual researchers, informants, and the discipline as such. The generalised self-criticism of postmodernism is of little help in this situation, which instead demands of us an enhanced consciousness of the globalised world of which we are a part.

GLOBALISATION AND THE PRODUCTION OF LOCALITY

For a while during the 1990s, it seemed that hardly a conference in the social sciences failed to include the word 'globalisation' in its title. Before the late 1980s, the term had hardly been used (Robertson 1992: 8); then, suddenly, it was on everybody's lips, and the interest (so far) seems to continue. Many anthropologists were active in defining the field and developing new research agendas, new journals were inaugurated (such as the Chicago-based *Public*

Culture and the British *Global Networks*), and a flurry of books were published, often with words like 'global', 'culture', 'modernity' and 'identity' in their titles. The most influential edited volume was, in this early period, Mike Featherstone's *Global Culture* (1990), which was followed by Scott Lash and Jonathan Friedman's *Modernity and Identity* (1991), sociologist Anthony Giddens' *Modernity and Self-Identity* (1991), Ulf Hannerz's *Cultural Complexity* (1992), Friedman's *Global Identity and Cultural Process* (1994) and Arjun Appadurai's *Modernity at Large* (1996), to mention a few of the most widely read books in those years that were marked by the end of the Cold War, the expansion of the internet and the advent of mobile telephony.

Globalisation can be provisionally defined as any process that relativises the geographical distance between locations. The spread of, say, human rights concepts, consumption patterns, information technologies, pop music, nationalist ideologies and (as we have seen) anthropological debates across the world are processes of globalisation, as are international capital flows, the AIDS pandemic, the illegal drugs and arms trade, the growth of transnational academic networks, Islamic and right-wing terrorism, or the migrations through which, for example, Caribbean or Palestinian communities are established in Britain, Canada and the USA. Such processes are dependent on the development of global infrastructures (long-distance transportation, communication technologies, and so on), though anthropologists are quick to point out that the sociocultural effects of, say, inexpensive plane tickets, satellite television or Facebook are unpredictable and highly diverse (Eriksen 2003, 2007).

Globalisation itself is not new, and anthropologists have studied it for many years under other names. Indeed, one might say that Herodotus and Ibn Khaldun (Chapter 1), were early representatives of the trend. Still, it may reasonably be argued that phenomena such as the emergence of poly-ethnic urban societies in the West, the spread of modern educational systems in the Third World, the global dissemination of Western lifestyles and political ideals, the new forms of organisation permitted by the internet, or the growing politicisation of essentialised ethnic identities, deserve to be regarded as truly new, setting new agendas for theory and method in anthropology. It seems beyond doubt that the *speed and volume* of contemporary flows of information, people and goods are unprecedented in human history; though long-distance networks

of trade, kinship, ritual exchange and political conflict have existed – on a smaller scale – as long as human society itself.

It may seem trivial, but we also need to emphasise the distinction between *globalisation itself* – a complex of actually occurring sociocultural processes, and *globalisation studies* – a family of anthropological approaches to these processes that attained prominence during the 1990s. As regards 'globalisation itself', the most prominent event of the last decades was undoubtedly the fall of the Iron Curtain. For anthropology this had wide-ranging repercussions. As mentioned above, Western and Eastern anthropologists were soon rubbing shoulders at conferences, trying to make sense of each others' conceptions of the discipline and the world – while a new ethnographic region was 'opened up' to anthropological enquiry. Its recent shared past had established a measure of commonality throughout the region (embodied, for example, in bureaucratic, educational and scientific conventions, ideology and social memory), overlying a patchwork of local traditions of diverse extraction, which asserted themselves with renewed force upon the sudden collapse of central authority. To the Western anthropologists who soon engaged in fieldwork in the 'post-socialist region', these conditions seemed sufficiently unique to prompt the development of innovative theoretical and methodological approaches. In 1991, the American anthropologist Katherine Verdery published the influential article 'Theorizing socialism: A prologue to the "transition"'. Drawing on the research of a handful of Western anthropologists who had done fieldwork in the region prior to 1989 (see Halpern and Kideckel 1983 for an overview), on the work of Eastern European scholars (such as the Hungarian economist János Kornai), and on the theories of Karl Polanyi (Chapter 5), Verdery develops a holistic model of pre-1989, 'socialist' society, which describes it as a distinct social type, with similarities to feudalism. Later research has in part followed up Verdery's model (Humphrey 1996–97), in part developed along different paths (Ries 1997). In 2002, the Max Planck Institute for Social Anthropology was opened in Halle, Germany, and rapidly established itself as an important research centre for the study of post-socialist societies under the leadership of Chris Hann and Günther Schlee. 'European universities' with Western-style curricula, initially financed by George Soros, were opened up in St Petersburg (see above) and Budapest.

The collapse of the Iron Curtain illustrates several points, and at the same time introduces us to the anthropological study of globalisation. On the one hand, we see global socioeconomic

processes that lead to the collapse of a regional political system. On the other, we see anthropologists (themselves agents of globalisation) penetrating into a pristine 'field', defining it as an 'ethnographic region', forging alliances with local scholars, and attempting to establish a respectable 'regional tradition of ethnographic writing' (Fardon 1990). Thus, while globalising processes erased the barriers around the region, anthropologists were busy localising themselves in it, proclaiming its uniqueness, and developing theory tailored to it. However, the theory thus developed is not itself much concerned with globalisation. Indeed, its emphasis on typologies and mechanisms of social integration harks back to the 1970s and earlier. This typological bent further strengthens the argument that the region is distinct, and thus a legitimate object of study for a new sub-group of anthropologists.

Globalisation has local effects, which are unpredictable, and may be autonomous to the extent that they directly oppose globalisation. This realisation was an important point of departure for the globalisation researchers that emerged in anthropology from the 1990s. Ranging from Melissa Caldwell's study of the cultural significance of McDonald's in Moscow (2003) to Anna Tsing's (2005) rich ethnography of Indonesian environmental activists and James Ferguson's (1990, 1999) influential studies in 'post-development'; from John and Jean Comaroff's research on the commercialisation of identity in South Africa (Comaroff and Comaroff 2009) to Ulf Hannerz's study of foreign correspondents (2004), connectedness, always partial, and transnational flows have struck at the heart of anthropological enquiry, prompting a reconsideration of central concepts – most obviously, 'society' and 'culture' – on firmer empirical ground than that of the postmodernists.

As we have said, the anthropological interest in globalisation was not without precedent. The studies of ethnicity and nationalism that emerged during the 1970s and 1980s (Chapters 7 and 8) clearly anticipated the globalisation school, inasmuch as the rise of nationalism was historically associated with modernity and the state, and ethnic movements were typically products of change and modernisation. Likewise, there are clear continuities with the 1970s interest in 'political economy' (Chapter 7). Indeed, two of the grand old men of this school, Eric Wolf (Steward's student) and Peter Worsley (Gluckman's student), had published major books in the early 1980s (Wolf 1982; Worsley 1984) dealing with cultural aspects of global capitalism and the universalisation of modernity. Still further back, there are continuities with the Marxian

theory of imperialism, with the peasant studies pioneered by the Chicago school and Steward, and with the studies of urbanisation and colonialism conducted by the Manchester school. This is not least evident in the work of John and Jean Comaroff, professors at Chicago, whose intellectual itineraries from Cape Town via Manchester to Chicago illustrate these connections perfectly.

In the influential work of the Swedish anthropologist Ulf Hannerz, such continuities are reflected. While his first monograph (1969) was a study of American ghetto life largely in the Chicago tradition, and his first major theoretical work (1980) was an appraisal of urban anthropology, his most important contribution in the 1990s was a discussion of the field, methods and potentials of globalisation studies (1992). Here, the concept of culture was redefined to signify flow, process and partial integration, rather than stable, bounded systems of meaning. This concept of culture was compatible with the still-dominant postmodernist sensibilities, as was Hannerz's definition of globalisation – as global aspects of modernity, rather than a monolithic 'global village'. Such adjustments made globalisation studies more palatable to traditional-minded anthropologists, but were also tailored to make sense of a world where bounded, stable cultures were hardly prevalent. Hannerz coined the term 'cultural creolisation' to describe the intermingling of two or more formerly discrete traditions; another term for the same phenomenon was 'cultural hybridity' (Modood and Werbner 1997).

Finally, like nearly every anthropologist writing in this field, Hannerz stresses that global processes have local consequences. Globalisation does not entail the disappearance of local difference; instead, a battle is waged with unpredictable, and often innovative, outcomes. Indeed, the neologism *glocalisation* has been proposed to stress the local aspect of globalisation (Eriksen 1993b).

For anthropologists like Hannerz, globalisation studies were simply an extension of existing research into a new empirical context. For others, globalisation seemed to pose a number of new questions, to which they responded with new theoretical formulations.

An example of the latter is the 'actor-network theory' proposed by Bruno Latour. Originally developed in a study of scientific practice (see above), the theory's emphasis on 'hybrids' and processes of 'translation' that occur when persons, objects or ideas flow from context to context, seemed ideally suited to a globalised world. When deployed methodically along the lines suggested by classical network theory (Chapter 5), and informed theoretically by the discussions of exchange that emerged in the 1990s, actor-network

theory seemed a formidable tool for analysis of phenomena that influence each other without making up a coherent system.

Arjun Appadurai is another anthropologist who has contributed to a dedicated theory of globalisation. In his edited volume on economic anthropology from 1986, he develops ideas of value transformation in global networks that are ultimately inspired by phenomenological concerns. Both Appadurai and Hannerz were concerned to retain the virtues of ethnography, even when considering large-scale processes. Hannerz wrote about 'cosmopolitans and locals in world culture', giving primacy to the locals, and Appadurai's article on 'The production of locality' argued that locality and local identity had always been a scarce resource and continued to be so in a period of accelerated globalisation. By linking locality production to ritual, he gave his theory a classical twist (and potentially redefined the study of ritual).

Though globalisation research was largely an Anglophone speciality, important contributions to the field were made by the French anthropologist Marc Augé, who had studied ritual and politics in West Africa in a structural Marxist mode during the 1960s. In small ethnographic studies of the Paris Metro and the Luxembourg Gardens (1986, 1985), and most influentially in his later theoretical books, including *Non-lieux* (1991; *Non-Places*, 1995), Augé discussed the fate of classical anthropological notions of place, culture, society and community in the postmodern era of flux and change. Arguing that the stability of 'place' could no longer be taken for granted, Augé parallels many of Appadurai's concerns (Appadurai 1996). In a work reminiscent of his postmodern countryman, Jean Baudrillard, Augé discusses dreams and imagination under different informational regimes, drawing on his West African research as well as on recent global developments (1999). Godelier also engaged with the discourse of globalisation, publishing on the neoliberal capitalist hegemony as well as raising the fundamental question 'What is a society?' on the basis of data from the Baruya, a New Guinean people he had studied since the 1960s, who were now embedded in capitalism and could no longer be said to form an autonomous 'society' (2009).

The kinship between globalisation studies and postmodernist deconstructionism was evident in the work of several anthropologists, perhaps most poignantly in Strathern's *Partial Connections* (1991), which argued that neither societies nor symbolic systems are coherent wholes, and cited globalisation research (notably Hannerz) in support of this view. The multiplicity of voices, the erasure of

clear distinctions between 'cultures' or 'societies', the eclectic attitude to research method, and the insistence that the world was inhabited by 'hybrid' objects, people and concepts, were some of the shared notions. Among the leading postmodernist anthropologists, George Marcus (1998) advocated comparative studies of modernity as a framework for an updated, reflexive anthropology. In some studies of the global–local interface, the paradoxes of widespread cultural reflexivity and the global spread of icons and institutions of modernity, gave empirical grounding to the often abstract claims of the postmodernists.

In spite of the continuities with previous research, the emergence of globalisation studies may signify the final demise of the classical anthropological notions of 'culture' and 'society', that have shown remarkable resilience in the face of near-continuous criticism ever since the 1960s. Characteristically, the Colombian anthropologist Carlos Londoño Sulkin (2012) explains, in his ethnography of Amazonian Indians, that he describes them as 'the people of the Centre' rather than with an ethnonym, since the boundaries of their identity are made fuzzy by mobility, intermarriage and cultural change. 'Culture' and 'society' are thus again under threat, and the reason is not so much the intrinsic value of the globalisation theories themselves, as an empirical reality where even the ideal type of the 'authentic' culture seems increasingly anachronistic. The actor-network models mentioned above may be a preview of the kind of concepts that will in the end replace the classical ones. They portray a world of 'partial connections' and ever-changing and hybridising 'discursive objects', employed and deployed by human carriers with reflexive ideas of their own identity, among which 'culture', derived from anthropology, may figure prominently. Indigenous peoples like the Sami of northern Scandinavia or the New Zealand Maori actively debate the relative merits of ethnographic studies of their cultures; people in Trinidad may be familiar with M.G. Smith's (1965) theory of cultural pluralism (and actively object to it); Australian aborigines draw on classical ethnographies in presenting their 'culture' to the authorities; Pacific Islanders copyright rituals in order to stop anthropologists from broadcasting video recordings of them, or else accept payment to masquerade as savages in some reality show. In an era of cultural reflexivity, anthropologists may end up in the 'hybrid' situation of studying not people's cultures, but their quasi-anthropological representations of their cultures.

Enthusiasm for globalisation studies was not universally shared in the discipline. For some, it was a case of the emperor's new clothes: globalisation was just a fancy name for neo-imperialism, cleansed of its political dimension. But while it is true that power relations have not always been visible in globalisation research, they have not been absent either. Power is a major issue in Appadurai's work, as well as in the research on comparative modernities that has been produced or stimulated by the Comaroffs (for example, Comaroff and Comaroff 2006). Inspired by work on power and resistance (Chapter 7) by such authors as James C. Scott (1985), Anthony Giddens (1979), Eric Wolf (1969) and ultimately Marx, the Comaroffs have argued, inter alia, that traditional rituals, such as witchcraft, may – under the impact of the stresses inherent in global processes – mutate into virulent forms that stimulate mass violence.

Other objections levelled at globalisation studies were that anthropology should continue to emphasise the local and the unique, and that the prophets of globalisation exaggerated the reach of modernity. However, as we pointed out above, a recognition of global interconnectedness does not preclude a concern with the local – indeed, the fragmented local cultures of the globalised world seem to invite a particularist, even a Boasian, approach. The most famous anthropologists associated with American cultural relativism in the last decades of the twentieth century, namely Geertz and Sahlins, both wrote essays that situated the impact of modernity on traditional societies within the larger framework of their respective projects. Sahlins wrote about the ironies of identity politics in Melanesia; 'as the New Guinean said to the anthropologist: if we didn't have *kastom* [custom], we would be just like white men' (1994: 378). He described the commercialisation and politicisation of identity in the Pacific, decrying the 'Hawaiian' culture presented to tourists by Hawaiians recreating themselves 'in the image others have made of them' (1994: 379). However, as he emphasised, in continuity with his earlier work, 'what needs to be studied ethnographically is the indigenization of modernity – through time and in all its dialectical ups and downs' (1994: 390). Geertz, in a similar vein, wrote ironically that difference 'will doubtless remain – the French will never eat salted butter. But the good old days of widow burning and cannibalism are gone forever' (1994: 454). He nevertheless saw no contradiction between the emergence of a global world of chaotic interconnections and his overall research programme, which he summarised as 'grasping an alien turn of mind' (1994: 462).

Both essays, however, conveyed a distinct sense of discomfort. Both authors saw that the era of classical anthropology was gone, displaced by a world of fragmentation, reflexive modernity, blurred boundaries and uncertain futures.

Methods, conceptualisations and research agendas change. The boundaries between anthropology and other disciplines are fuzzy in places, and interdisciplinarity, as we have seen, is on the rise. Globalisation studies link up with political theory, human geography, macrosociology and history; evolutionary approaches with psychology, biology and neurology. Eclecticism in theory and method has been characteristic of the last decades of anthropological history. Yet it can still be said that some of the classic tensions of anthropology, some of the 'differences that have made a difference' and that have defined the space in which anthropology arose and thrived, are still intact.

First, it still makes sense to distinguish between anthropology as a generalising social science and as one of the humanities, aiming for interpretive richness. Second, it makes sense (though there are plenty of crossovers) to distinguish anthropologists of *society* (focusing on agency, social structure, politics, and so on) from anthropologists of *culture* (focusing on symbols, mental structures, meaning, and so on). We deliberately avoid the terms 'social' and 'cultural' anthropology, which refer to an American-European divide that only in part overlaps with this distinction. Third, it is still reasonable to distinguish approaches that see society and culture as historical phenomena and approaches that search for unchanging structures and patterns.

Many, if not most, anthropologists place themselves at the crossroads of one, two or all of these polarities, but most feel the magnetism of the poles and are occasionally forced to take a stance. Boas himself oscillated between scientific and humanistic ambitions on the discipline's behalf, and his cultural relativism is often highlighted to the extent that it overshadows his beliefs in the scientific pretensions of anthropology.

Insofar as these tensions have not been resolved, the intellectual space that defined anthropology in the first place is still intact, notwithstanding 'the end of modernism' and the soon-to-be-final demise of 'primitive' society. The idea of the primitive may be gone, and the notion of a world of discrete cultures rendered obsolete; but the larger questions – 'What is society?', 'What is culture?', 'What is a human being?', 'What does it mean to be human?' – remain

unanswered. Or rather, they are still answered in many different ways; sometimes complementary, sometimes conflicting. It is only if these conflicts are made explicit, in anthropology itself and in dialogue with other disciplines, that the discipline of anthropology can continue to thrive, for, as this book has shown, anthropology has throughout its history depended on controversy.

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