



E. E. EVANS-PRITCHARD

## EDWARD EVAN EVANS-PRITCHARD

1902-1973

### I

EDWARD EVAN EVANS-PRITCHARD, known to most of his colleagues as E-P, died on 11 September 1973, nearly 71 years old. He was Professor of Social Anthropology in the University of Oxford from 1946 until 1970, succeeding A. R. Radcliffe-Brown, the first holder of the chair. With his colleagues in the Institute of Social Anthropology he established Oxford as one of the leading centres of research and teaching in that discipline. His high reputation rests however not only on his achievements as the leader of the Institute but also on his published work, including several major contributions which appeared before he moved to Oxford. With these he reached a very wide audience, evidenced by the numerous translations of his books into other languages. Although his experience of anthropological fieldwork was confined to Africa it embraced several diverse groups of people in the Sudan, Kenya, and Libya. His writings were firmly rooted in his African research, but were widely seen to have implications for the study of peoples everywhere. These implications were in part made explicit in his works on method and theory, particularly those that appeared in the earlier part of his career. He is remembered principally for his discussions of witchcraft and religion, and for his analysis of the political structures of stateless societies, but he dealt more briefly with a wide range of substantive topics. Towards the end of his life he declared himself to be disenchanted with theory and attempts to establish social anthropology as a science, stressing instead its task of recording for the benefit of posterity those social institutions and practices which would soon disappear. Those who knew him personally will, however, perhaps remember him most vividly as very much a character in his own right, a man *sui generis*.

E-P's father, Thomas John Pritchard, came from Caernarvonshire. He studied at Hertford College, and, 'after a slightly stormy career at Oxford—in which his son followed in his

footsteps' (1973b, p. 36), entered the Anglican ministry. He served as a curate in Liverpool and while there married Dorothea Edwards, of Anglo-Irish descent. Because of his poor state of health he moved to Sussex and filled curacies in Lewes and Eastbourne before becoming honorary curate of St John's Church, Withyham. The couple settled in Crowborough, where their first child Thomas was born, followed two years later, on 21 September 1902, by Edward Evan, a couple of months premature. It seems that his mother had private means, for sometime after E-P's birth the family moved into a larger house, named Lea after his mother's family estate in Ireland. By 1907 E-P's father had changed his name to Evans-Pritchard, his wife's mother's father being Eyre Dixon Evans, JP, of Liverpool. While still only five years old, E-P was sent as a boarder to The Grange, a preparatory school near Crowborough, partly because his brother was lonely there. It was, according to E-P's own account, 'one of the best schools in a social sense—stables, golf course, beagles—but not, I would say, from the point of view of scholarship' (1973e, p. 18). The family remained in Crowborough until 1913 when his father became vicar of Waterperry, near Wheatley in Oxfordshire. The move to Waterperry provided E-P with his first experience of living in a close-knit rural community, a period in his childhood that he recalled with much affection.

In 1916 E-P went to Winchester College as a Commoner. When he visited the school many years later, he showed manifest pleasure at seeing old haunts. He wrote approvingly of how at Winchester he had been implanted with what some people, but not he, had come to regard as old-fashioned sentiments: loyalty, honour, duty, disinterestedness. On the other hand he disliked his housemaster and while he enjoyed games he was not good at them. He studied the humanities and also, for a time, chemistry but, after causing an explosion, he directed his attention to history. Looking back he wrote 'though I was very happy at Winchester during my four years there, I only just managed to scrape through by the magnanimity of the Headmaster'. He left in 1921 to go to Exeter College, Oxford, as an honorary Scholar. By this time his parents had moved to Calverton, near Stony Stratford, where his father was rector until his retirement in 1923. He died in 1929 following an operation, aged 72. Although uninterested in anthropology, he was sympathetic towards his son's professional interests. E-P described his father as 'a simple, humble, pious man' who, 'If he had any bias it was against

dissenters' (1973b, p. 36). His mother, who outlived her husband by many years, seems to have had a greater influence on E-P. She was hostile towards anthropology and tried to persuade Seligman not to accept E-P as a student, wishing him to succeed his godfather in a Sussex legal practice. Later, however, she provided him with support when he faced difficulties over his conversion to Catholicism.

At Oxford E-P read modern history. If the statements of his contemporaries are to be believed, he combined work with a good deal of play. One described him as a 'rogue elephant Wykehamist', always ready for a slash at the Establishment. Yet although not seeing eye to eye with his tutor, he developed a great sense of loyalty to his college and a high regard for its Rector, R. R. Marett. He gained second class honours in 1924, after being vivaed for a first. He had already decided that, like Marett, he would specialize in anthropology. A contemporary photograph of a fancy-dress party he attended (Powell 1976, pl. 21) shows him wearing an Arab burnous, suggesting that his sights were already set on the Middle East. He read some of the works of E. B. Tylor and Sir James Frazer and thus, he later wrote, 'I became a social anthropologist in embryo. But there was a snag. I did not want to become, I was going to say, just an intellectual. I wanted a life of adventure too, and fieldwork seemed to be the solution to combine both' (1973e, p. 18). He also read *Primitive Society* by Lowie, the only general introduction to social anthropology available at the time. E-P selected this as the one book to be taken into the field; Max Gluckman, he noted, made the same choice (1972d, p. 42; 1973p, p. 241).

After graduation he moved to the London School of Economics. E-P delighted in relating how incomprehensible this decision appeared to his Oxford tutors. But to gain training in field research a move was necessary, for although there were several men in Oxford besides Marett who were working in areas near to social anthropology—E-P mentioned Balfour and Dudley Buxton—none had engaged in fieldwork. Seligman at LSE, who had done research in the Torres Strait, Papua, Ceylon, and the Sudan, was the obvious choice as teacher. In the same year, 1924, Malinowski joined the teaching staff at LSE and his postgraduate seminars soon became the focus of anthropological teaching in the School. E-P attended these seminars but soon fell out with Malinowski (cf. Stocking 1984a, p. 167).

In 1909 C. G. Seligman, in collaboration with his wife Brenda, had begun what we would now call ethnographic survey research

in the southern Sudan. Their investigations continued until, after their third expedition in 1921-2, ill health forced them to abandon the possibility of further work in the field. Seligman arranged for E-P to continue the survey. His research was financed by the government of the Anglo-Egyptian Sudan, with the active support of the Civil Secretary, Sir Harold MacMichael. E-P started from Khartoum on his first field trip about the end of October 1926 (Cunnison and James 1972, p. x). His duties, as described later by MacMichael in 1929 when E-P was about to begin work among the Nuer, called for the production of a report 'couched in easily comprehensible terms and dealing with those social and cultural aspects of his study a knowledge of which is essential to the administrator' (Ahmed 1973, p. 268). E-P certainly met this requirement, though in a manner that probably neither Seligman nor MacMichael expected.

The Seligmans, like many field anthropologists of their generation, relied on gaining ethnographic information through interpreters and did not spend extended periods investigating single communities. On the other hand, as well as ethnographic data they collected measurements of the physical characteristics of the people they encountered. In both respects they conformed to the orthodox mode of data collection for British professional ethnographers. For two centuries or more missionaries in many parts of the world had reported on the ethnographic results of their years of pastoral work among indigenous communities. In the nineteenth century pioneer ethnographers lived for considerable stretches of time among American Indian groups. Yet it was Malinowski's stay in the Trobriand Islands during part of the First World War that, in retrospect, came to be taken, at least among British anthropologists, as marking the transition from survey research, typified by the work of the Seligmans, to what is often called participant observation, the prolonged study of a small community by an observer who lives as closely as possible with the people who are being studied.

E-P expressed his dissatisfaction with survey research in several of his writings, though whether he had already reached this view before going to the Sudan, following his participation in Malinowski's seminars at LSE, is not clear. Had he done so, it is certain that he would not have stated his view in public. He conformed to expectations even to the extent of having 'taken around with me callipers and a height-measuring rod. I did this to please my teacher Professor Seligman. I have always regarded, and still regard, such measurements as lacking scientific value,

even being almost meaningless; but so it was at that time' (1973p, p. 242). On the other hand, he never linked his own preference for intensive inquiries to Malinowski's fieldwork. Indeed throughout his professional career E-P continued to publish the results of brief investigations he had made among peoples for whom better data were unavailable. His first published paper, on the Ingassana tribe (1927), was based on only about a month's observations, while forty-five years later he was still publishing scraps of information, in this case the existence of rock drawings he had seen at a desert oasis in 1938 (1972s). He enunciated the view 'that the fieldworker should put on record in some or other form all the information he possesses', adding characteristically 'but *vita brevis*' (1967h, p. 180). Yet after his retirement he sorted out and destroyed many of his papers, deciding what information was worth preserving.

In 1927 E-P arrived for the first time among the Azande and remained with them for three months. This provided him with the data for his London doctoral dissertation, 'The social organization of the Azande of the Bahr-el-Ghazel province of the Anglo-Egyptian Sudan', submitted in the same year. Further study of the Azande led to the publication in 1937 of *Witchcraft, Oracles and Magic among the Azande* (1937g). This was his first book, and arguably remains his most substantial contribution to our understanding of social thought and social action. As Seligman wrote in his Foreword, 'Dr Evans-Pritchard has given us good measure, pressed down and running over.' A French version appeared in 1972 and a German translation in 1978, based on the abridged English edition of 1976. In this book E-P demonstrated how Zande beliefs and practices about witchcraft formed a closed system within which men and women took rational decisions. He showed in detail how apparent failures and contradictions were not explained away, as a sceptical onlooker might expect, but rather were perceived as evidence of the continuing validity of the system. As well as the book he published several articles on various aspects of Zande social life before turning his attention to reporting on the Nuer. In 1954 he resumed publishing on the Azande and continued until he died, his collection of texts, *Man and Woman among the Azande* (1974a), appearing posthumously. In all, between 1927 and 1930, he spent about twenty months among the Azande, his work being supported financially by the Sudan government, with supplementary grants from the Royal Society and the Laura Spellman Rockefeller Memorial Fund. Some of his later publications on

the Azande were produced in collaboration with Zande men at the University of Khartoum during the 1960s where he served as an external examiner.

Early in 1930, at the request of the Sudan government, he began a study of the Nuer in Upper Nile province, against whom the government had recently carried out punitive operations (Johnson 1979). E-P had originally intended to continue with his work among the Azande but agreed to the new plan. However, meeting with suspicion and lack of co-operation from both the Nuer and officers of the civil administration, he decided after three and a half months to abandon the inquiry. His decision was precipitated by an incident when government troops surrounded the cattle camp where he was staying and took hostages, demanding that the inhabitants should find and hand over to the government the prophets they were seeking (Johnson 1982, pp. 231-6). E-P wrote, 'I felt that I was in an equivocal position, since such incidents might recur, and shortly afterwards returned to my home in Zandeland' (1940k, p. 11). He then went back to England and while there his father died.

His return to the Sudan for the dry season of 1931 was marked by quarrels between MacMichael in Khartoum and C. A. Willis, the Governor of Upper Nile province, about where E-P should resume his work. Eventually he was allowed to visit the eastern Nuer area. Willis remained unco-operative, climatic conditions were severe, and after five and a half months, in June 1931, E-P had to retire to hospital with severe malaria. He returned to England to convalesce. It was not until 1935 that he was able to return to the Nuer, for a trip of seven weeks' duration, terminated by sickness. He visited them for the last time in 1936 on his way back from fieldwork in Kenya but again after seven weeks had to give up because of fever. His total time spent among the Nuer was about a year. Articles on the Nuer began to appear in 1933, while his last major contribution to Nuer ethnography, *Nuer Religion*, came in 1956 (1956g), being preceded by *The Nuer* (1940k) and *Kinship and Marriage among the Nuer* (1951g) as well as numerous journal articles. E-P readily acknowledged the difficulties he had encountered in his inquiries among the Nuer, and the consequent shortcomings in his account of their social institutions. Of his first Nuer book he said 'I am amazed that it has ever appeared at all' (1940k, p. 9).

He made one other substantial study of peoples in the Sudan. In 1935, while waiting for permission to carry out a study of the Galla in Ethiopia, he spent two and a half months among the

Anuak, the results of his inquiries appearing as a monograph published in a series sponsored by the London School of Economics (1940h). During the Second World War E-P increased his command of the Anuak language and his understanding of their institutions and he compiled an Anuak dictionary and grammar (Lienhardt 1974, p. 300). His analysis of *The Divine Kingship of the Shilluk* (1948a), another Sudanese people, is based on a study of the literature rather than on field research (cf. Arens 1979).

After six years of intermittent research in the Sudan, E-P still had no prospect of permanent employment in Britain, a state of affairs he later attributed to Malinowski's dislike for him (1973e, p. 19). Financial support from the Sudan government for further field research was not forthcoming (Johnson 1982, p. 239). While at the London School of Economics E-P had met and liked the Australian anatomist Elliot Smith and his anthropological disciple W. J. Perry. Elliot Smith had earlier worked in Egypt and it was through his good offices that in 1932 E-P became Assistant Professor of Sociology at the University of Egypt in Cairo. He was placed in the Department of Geography, where he flourished. He was able to 'improve my Arabic, to learn some Galla and Matokki, and also to do a good bit of desert camel-travel' (1973e, p. 19). It was in 1932 that he first met some Sanusi, in exile from Libya, whose social institutions he was later to describe at length. His main publications while in Cairo were, apart from papers on the Azande and Nuer, theoretical essays on the views of Tylor, Frazer, Lévy-Bruhl, and Pareto (1933a, 1934c, 1936e).

E-P resigned his professorship in 1934. He then returned to England where, with Marett's support, he was appointed to a research lectureship in African sociology at Oxford. With sponsorship from Elliot Smith he also became Honorary Research Assistant in the Department of Anatomy and Embryology at University College London, an appointment he recalled later with delight. He gave occasional lectures in Oxford and at UCL. His major piece of field research was to have been a study of the Galla in 1935, supported by a two-year Leverhulme research fellowship, but because of the Italian invasion of Ethiopia he had to abandon the plan, and instead spent time among the Anuak and then again with the Nuer. In 1936 he went to Kenya as a member of an expedition from Oxford intending initially to study the Masai. This plan was vetoed by the Kenyan government, as was his second proposal for a study of the Turkana.



Instead he spent six weeks among the Nilotic Luo and then went back to the Sudan for his final visit to the Nuer (Beidelman 1974a, p. 2). In 1936 a chair of social anthropology was established in Oxford. E-P was a strong candidate, but the electors decided in favour of an older and academically more experienced man, Radcliffe-Brown, who was backed by Malinowski (cf. Stocking 1984a, p. 170).

In 1939 E-P married Ioma Gladys Heaton Nicholls, daughter of a South African politician and at the time a graduate student of anthropology at Oxford. There were to be three sons and two daughters of the marriage. E-P had a small private income of £300 a year (1973p, p. 236), presumably derived from his mother's property, and his lectureship, which was funded by the Rockefeller Foundation, provided him with an annual salary of £300. He wrote 'In despair of ever getting a post I could live on in England I had accepted a Chair at Yale' (1973e, p. 20). This was to have been a temporary appointment for one year in the first instance. War was declared just before he was due to sail for America. He thereupon resigned and joined the Welsh Guards, but was forced to return to civilian status by the Oxford university authorities on the grounds that he was in a reserved occupation. He managed to get back to the Sudan, despite the war, on the pretext that he was to carry out further research there. Meanwhile Meyer Fortes had taken over the research lectureship in Oxford, and, ironically, Malinowski the professorship at Yale.

In the Sudan E-P initially intended to work on problems of administration among the Dinka (Johnson 1982, p. 242), though he seems also to have begun a study of an Upper Egyptian (Quft) village (1973f, p. 7). Before he could begin Italy declared war. By October 1940 he had been commissioned in the Sudan Auxiliary Defence Force with the rank of Bimbashi. To comply with the regulations of the Force he was first given a nominal position in the Sudan political service so that he could become eligible for commissioned rank. Later he defended himself against the criticism that he had served as a member of a colonial administration by emphasizing that his appointment to the political service was a mere formality to enable him to achieve his main objective, an active part in the war. Joining the Sudan Auxiliary Defence Force brought him into conflict with the university authorities in Oxford and he lost his lectureship, despite his plea that 'if Socrates fought at Thebes and Descartes

in the Low Countries, why shouldn't I have a little excitement in Ethiopia and elsewhere?' (1973e, p. 22).

He was attached to the Gila Force, and recruited fifteen Anuak men, armed with 'rifles of a last century model and 50 rounds apiece' (19730, p. 471). Knowing the country, the people and their language, he gave, he said, 'a very liberal interpretation to my instructions'. He found that he had 'to lead by example rather than by command, for they are rugged individuals and very obstinate. I learnt that if, after discussion of the course of action I proposed they refused to agree to it, I could attain my object by proceeding to carry out the proposed operation myself, whereupon all eventually followed suit' (19730, p. 474). One may wonder whether E-P had his colleagues in the Institute in mind when writing those words. Contemplating the start of hostilities, he wrote to Fortes from Akobo 'I view the prospect with complete equanimity. I have always felt that to be killed in a war would be a very suitable ending to my rather peculiar career. One's life should have a certain form and dramatic sequence; otherwise it is uninteresting to oneself' (22 October 1940). He was actively engaged on patrol and offensive operations from 28 October 1940 until May 1941, though he found time to write a short Foreword to a book written by a friend (1941). In a letter to Fortes he said 'I have taken like a duck to water to guerilla warfare, but I am inclined to be so reckless that I get raspberries from my commander that would win him first prize at any horticultural show. . . . What is a war for except fighting? I suppose I am 17th century in my ideas of it' (17 December 1940). He then left the Sudan and went on leave to South Africa. At one stage in the campaign he had as many as fifty Anuak under his command. He was mentioned in a despatch.

In South Africa he joined his wife, who had recently given birth to a stillborn son. While there E-P was offered a commission as colonel in the South African army by General Smuts, an offer he 'politely refused'. He returned to the Middle East, intending to join the Long Range Desert Group. Instead, after attending a course in the use of high explosives in Palestine, he joined the Spears mission in Syria as political officer in the Alawite territory. After disagreeing with his French colleagues he asked to be relieved and spent some time in Iraq, where his wife was working with the British embassy. Their first liveborn child, a daughter, was born in Baghdad. In Iraq, as in Syria, E-P was mainly occupied with plans for post-occupational sabotage in the event

of German victories. He then returned to Cairo and rejoined the Sudan Auxiliary Defence Force in time for the battle of El Alamein (October 1942). In order to avoid spending the rest of the war on garrison duties he arranged to be transferred, in November 1942, to the (third) British military administration of Cyrenaica and served as Governor of Cyrene district. Here, he said, 'I was quite hopeless as an administrator of the sort required' and transferred to become Tribal Affairs Officer. In this capacity he spent over two years 'wandering with my camels and horses in desert and semi-desert with interludes in the forests' (1973e, p. 21). From this period among the Bedouin of Cyrenaica came his book *The Sanusi of Cyrenaica* (1949i) as well as several lesser publications. E-P then returned to London and, to avoid being posted to Germany, tried unsuccessfully to join a naval intelligence unit in Oxford. However in June 1944 E-P had been appointed to a university lectureship in Cambridge. In March 1945 this post was upgraded to a readership and, according to E-P, J. H. Hutton, then Professor of Social Anthropology in Cambridge, was able somehow to get him out of the army to take up the appointment. At the same time he refused a readership in Oxford but in October 1946 returned there to succeed Radcliffe-Brown as Professor and Fellow of All Souls.

E-P was based in Oxford for the rest of his life. He carried out no further field research but in 1950 served as a visiting professor in the Department of Anthropology, University of Chicago, and in 1957-8 for a year as a Fellow at the Center for Advanced Study in the Behavioral Sciences, Palo Alto. His wife died tragically in 1959; a junior fellowship was later established in her name at her college, St Anne's. In 1960 E-P spent a few weeks in Ghana as an educational adviser to the government and visited west Africa again in 1965. He reached the retiring age for his chair in 1970 and was knighted in 1971. From 1963 to 1965 he was Sub-Warden of All Souls.

E-P received many honours. No less than eight Festschriften were presented to him, with a posthumous volume from his former Oxford colleagues. He received honorary doctorates from Chicago, Bristol, and Manchester, and other honours from the universities of London, Wales, and Aarhus. He was awarded the Rivers memorial medal of the Royal Anthropological Institute in 1936 and in 1963 was its Huxley Memorial Lecturer. E-P was awarded the Viking medal of the Wenner-Gren Foundation in 1961. He was a Chevalier of the Légion d'honneur, an honorary member of the Institut française de sociologie, a foreign honor-

ary member of the American Academy of Arts and Sciences and a member of the American Philosophical Society. In 1956 he was elected to the British Academy but resigned his fellowship in 1972, ostensibly to save expense but in fact in protest at the way in which other fellows had secured the election of a colleague (*pace* Kuper 1981). He wore his honours lightly.

## II

E-P's influence on the development of social anthropology has stemmed largely from the works he produced during the first half of his career, notably *Witchcraft among the Azande*, *The Nuer*, and his jointly edited *African Political Systems*. His influence as a teacher, and a leader of his profession in Britain, was exerted during the second half when, after a successful year in Cambridge, he moved to Oxford, along with several members of the Cambridge group he had begun to form. Before the war graduate students at Oxford were able to study for a Diploma in Anthropology, covering all branches of the discipline as well as social anthropology, and others were enrolled as doctoral candidates in social anthropology. There was no undergraduate teaching in the subject. While E-P was a research lecturer he had drawn up with his friend and colleague Fortes a plan, supported by Radcliffe-Brown, for 'research into problems of modern political development in Africa' (1940m). Because of the Second World War, the plan was not implemented, though Fortes made use of the grant from the University and the Rhodes Trust to make inquiries in west Africa. After succeeding Radcliffe-Brown, E-P put forward in 1949 a proposal for the establishment of an Honours School of Anthropology, available to undergraduates. The proposal was rejected by the General Board of the University, one of its four reasons being 'that it is not clear that a satisfactory education can be obtained from a School so predominantly confined as that envisaged by the present plan to the study of man in a primitive or uncivilized state'. Consequently the staff of the Institute of Social Anthropology had no option but to continue to concentrate its efforts on postgraduate teaching. This restriction, which did not apply to any other such school in Britain, in practice gave to Oxford an advantage that E-P and his colleagues exploited fully. They were able to concentrate their efforts on research and professional training and to escape the load of undergraduate teaching. E-P's leadership is com-

memorated with a portrait head in bronze by Ian Scott on display in the Institute.

Beyond the precincts of Oxford E-P took an active part in establishing social anthropology as an academic discipline in Britain and other Commonwealth countries, distinct from, though still part of, the wider tradition of anthropology. The emergence of a significant group of social anthropologists trained by Malinowski and Radcliffe-Brown led to the formation of a professional association, a move initiated by E-P. The first meeting of the Association of Social Anthropologists was held in Oxford in 1946, when E-P was elected Chairman and Secretary-General. He later became Life President. He sought to establish, under the aegis of the Association, a British journal analogous to the French *L'année sociologique*, but unfortunately without success (Firth 1986, pp. 4-8).

In the Preface to his first collection of previously published papers (1962g), E-P set out a typology of his writings which mirrored the stages of professional training undergone by graduate students in Oxford. First came papers treating anthropology and anthropologists in a general way; then essays based on other people's observations; and finally papers reporting on his own observations in the field. As already noted, most of what he published before 1945 falls into the third category, while his Cairo essays, though based on the arguments of others rather than on their observations, may be assigned to the second category. It was only after he had moved back to Oxford after the end of the war that publications in the first category began to appear.

When E-P was beginning his career as an anthropologist, the dominant theoretical orientation in anthropology, at least in Britain, was functionalism, as propounded by either Radcliffe-Brown or Malinowski. In his later years, the reigning fashion was for structuralism, with the label 'structural functionalism' coming into use to indicate a preference for Radcliffe-Brown rather than Malinowski. E-P was never a fully committed adherent to any of these positions, though his writings contain passages supportive of all of them. His early letters to Fortes express his mistrust of Malinowski's functionalism and he may indeed have had good grounds for objecting to Fortes's (1973, p. x) claim that 'his first publication on Zande magic [1929a] is pure Malinowski' and therefore functionalist (1974d). Nevertheless some of his other articles published at that time were undeniably written in conformity with one or other version of functionalism. For

instance, his short note on kinship (1929c), which led to an exchange with Hogbin, might well be described as 'leading on from Radcliffe-Brown', while in his reply to Hogbin E-P (1932b) spelt out a pattern of relations which in essence is the atom of kinship, as later defined by Lévi-Strauss, though in both papers he showed a more sensitive appreciation of the substantive content of these relations than is apparent in Lévi-Strauss's structural formulation. On the other hand, his statement that 'Generally it is possible to show what vital biological and social needs any institution satisfies and to explain it in terms of what it does, that is to say in terms of its functions' (1931d, p. 145) would surely have met with Malinowski's approval. Like many other critics of functionalism, he continued to make assertions in his writings that were at least compatible with a functionalist analysis. In E-P's later programmatic statements functionalism is rejected so emphatically that an American commentator could criticize the work of Fortes by accusing him of 'clinging to the functionalist baby which Evans-Pritchard has almost washed out of existence' (Harris 1968, p. 554).

While most of E-P's publications written before the outbreak of war are concerned primarily with reporting the findings of his researches in the Sudan, he was also during this period committed to the goal of social anthropology as a generalizing science. In proceeding towards generalizations which would apply to all societies whatsoever, an essential first step was to be the comparative method, whereby societies with several features in common are scrutinized with the intention of discovering some regularity in the way in which they differ from one another. The most explicit of E-P's writings in this mode is his article on the Nandi-speaking peoples of Kenya (19401). In the 1950s, at professional gatherings, this was an article that some of his colleagues, still committed to the search for valid generalizations, often cited as an example to be admired and emulated, while E-P remained silent at the reference to an intellectual stance he had already begun to query.

While still subscribing to this search he set out his proposed analytical method in a short article on religion:

One must not ask 'What is religion?', but what are the main features of, let us say, the religion of one Melanesian people; then one must seek to compare the religion of that people with the religions of several other Melanesian peoples who are nearest to the first in their cultures and social institutions; and then after a laborious comparative study of

all Melanesian peoples, one may be able to say something general about Melanesian religions as a whole. One can only take this long road. There is no short cut. (1953h, p. 217)

During the earlier part of his career, he followed the rest of the profession in referring to these generalizations, particularly those that applied to all societies without limitations of time or space, as 'laws'. He wrote 'Social anthropologists of today . . . assume that there are certain constant relations between social facts and that the discovery of these correlations and their formulation as sociological laws is the purpose of the science' (1946h, p. 411). Yet in the same article E-P had already begun the task of publicly distancing himself from this quest for 'general sociological laws'. He commented on the assumptions that such laws exist and would be useful to discover by saying, 'So long as social anthropologists recognize that they are assumptions no harm is done. Even if sociological laws may have to be regarded as fictions they have heuristic value' (1946h, p. 412).

By 1960 he had adopted the view that the comparative method might perhaps be used in anthropology, not to reconstruct the stages of social development, but to discover laws of functional interdependence, and to reveal how ethnographic facts are related to one another and hence what they mean (1960j, pp. 14-15). Since no laws of functional interdependence had yet been propounded in social anthropology, he maintained, therefore it was not worth while discussing whether there can be any such laws, or what their significance would be if there were. Gluckman (1975, p. 27) reports a conversation he had with E-P shortly before he died in which he maintained that the kind of work in social anthropology that would endure was the solid ethnographic corpus, exemplified by the writings of Isaac Schapera, whereas 'superficially exciting analyses, inadequately grounded in "facts"', would become 'intellectual curiosities'.

Much better known than the Nandi article is the 'Introduction' to *African Political Systems* which he wrote with Meyer Fortes (1940i). This short chapter had a profound effect on the work of anthropologists for a couple of decades or more, particularly those working in Africa, or in societies lacking hierarchical centralized political institutions, or who were trained in Oxford. Yet in fact this seminal piece seems to have been the work mainly of Fortes rather than of his co-author. Following talks in 1937 with Schapera in Cape Town, Fortes proposed to E-P that they should jointly edit a book (Fortes 1956). Radcliffe-Brown wrote

the Preface but the editors then thought that something more was called for. Fortes drafted the Introduction and sent the manuscript to E-P who made only minor alterations. During the 1950s, when the results of new inquiries among stateless peoples were being eagerly discussed in anthropological seminars, this essay provided the essential analytic framework. However, apart from providing a short preface (1958d) for a symposium on this topic edited by two younger Africanists, Middleton and Tait, E-P himself took little part in the debate. He returned to the broader theme of the comparative method in his Hobhouse Memorial Lecture (1963d).

Like many of his colleagues, E-P considered it was necessary on some occasions to stress what Radcliffe-Brown (1952), but not E-P, would have called the ideographic or descriptive task of ethnographic inquiry, focusing on what is distinctive or unique, and at other times the nomothetic or generalizing goal of social anthropology as a science, emphasizing similarities. He was certainly not alone among his colleagues in seeking to sustain both prongs of the discipline, but it is easy to take one item from his writings, ignoring the rest of the corpus, and draw a false inference about his views. He was aware of the diversity of stances he had from time to time supported in print, and defended himself characteristically by proclaiming that he hoped that it was true that he had changed his viewpoint, for 'consistency is surely the worst of all vices in science' (1970m). Firth (1975, p. 8) describes E-P as 'a great polarizer', sharpening points of disagreement with his colleagues, but we should add that he shifted sometimes from one pole to another.

Although E-P's attitude towards sociological laws and generalizations certainly changed over time, some of the fluctuations in his attitude were more apparent than real, and were a product of his continual use of the term 'science'. For Radcliffe-Brown, the advocate of a 'natural science of society', there was no essential difference in goals and correct methods between the physical or natural sciences and the social sciences. It is easy to read E-P's methodological pronouncements, particularly those that appeared in his earlier years, as if he meant by 'science' that version of the concept adopted by Radcliffe-Brown. Even Fortes (1953, pp. 30-2), in his inaugural lecture at Cambridge, maintained that E-P 'splits reality into twain', with the material world alone being susceptible of scientific study, whereas opposed to the material world are moral systems which can be apprehended only subjectively. If Fortes, as he says, found it difficult to follow



E-P's argument, it is not surprising that others as well were baffled. In some of E-P's statements, social anthropology is unequivocally a social science, a category to which, on occasion, he also assigned history. In the same spirit he argued that 'if social anthropology is to be combined with another departmental subject, ideally it should be with sociology' (1976h, pp. 173, 175). On the other hand in his series of broadcast lectures he declared that he regarded social anthropology as one of the humanities, in contrast to those of his colleagues who looked on their discipline as a natural science (1951f, p. 7). It was not until his Hobhouse Memorial Lecture (1963d) that he spelt out in detail his perception of the unbridgeable gulf between natural and social science

In his Oxford inaugural lecture E-P dwelt on the use of scientific method in anthropology (1948f), while two years later, in his Marett lecture (1950d), he maintained that anthropology was not and could not be a natural science. Some commentators, including those who wrote to *Man* following publication of the second lecture in that journal, have interpreted the clear contrast between these two pronouncements as marking a sharp break in E-P's thinking. Dumont (1975, p. 334), for instance, speaks of E-P exploding 'the bomb whose fuse had been burning for at least ten years'. Kroeber (1951) expressed his approval, while Radcliffe-Brown (1952) wrote to express his puzzlement. Evidence for a shift in outlook over a longer time span may be seen in the contrast between his programmatic statement of 1936, putting social anthropology firmly among the social sciences (1937e), and his 'Preface' to *The Azande* (1971g, v-xii), portraying anthropology as a substitute for history based on contemporary documents. Yet it seems more likely that, just as he did not, in his view, make a sudden conversion to Catholicism, there was also no sudden conversion to a new goal for anthropology. Most of the divergent interests staked out in his various programmes for anthropology tended to persist throughout his career, but the emphasis given to them varied from occasion to occasion, with many fluctuations from the long-term trend. The most striking piece of evidence that E-P did not see his variously stated goals for anthropology as incompatible is that in his radio lectures, which were presented to the lay public as an introduction to anthropology (1951f), portions of both his inaugural and Marett lectures are included as part of a unified interpretation of the aims of the discipline. This was in conformity with the view he had advanced in the Marett lecture that by realizing itself to

be a kind of historiography, 'and therefore ultimately of philosophy or art', social anthropology would be given the opportunity 'to be really empirical and, in the true sense of the word, scientific' (1950d, p. 123). Furthermore, many years after the event, he revealed that the views put forward in his inaugural lecture did not represent his own position at the time but those of his predecessor Radcliffe-Brown, who had asked him to present them on that occasion (1970m).

Given his training in history, it is not surprising that when E-P became an anthropologist he should retain a strong sense of the value of historical analysis. At the time he entered the subject there was a strong antipathy against historical studies, fuelled by the excesses of speculative reconstruction of the past by W. H. R. Rivers in his later writings and by other examples of what Radcliffe-Brown called 'conjectural history'. Ethnographic analysis tended to be conducted in synchronic terms and the results to be further divorced from time, if not also from space, by being written in the so-called 'ethnographic present' tense. Yet despite his later emphasis on an historical perspective in anthropological inquiry, E-P still subscribed to an approach indifferent to space, if not also to time, as late as 1946. In his review of Fortes's *Dynamics of Clanship*, he wrote that the Tallensi 'happen to live in the Gold Coast. The might just as well have been living in Golders Green for all Dr Fortes, or any good anthropologist, cares' (1946m, p. 907). Twenty-five years later, his views were significantly different. He commented that when he began his inquiries among the Azande, 'in the anthropological climate then prevailing, I might have regarded their traditions as of merely antiquarian interest and therefore not worth recording' (1971g, p. x). Fortunately he did make substantial records of these traditions.

There were two prongs to his thinking on this matter. On the one hand, where historical documents are available, they are to be utilized for their comparative value and for the determination of diachronic processes. On the other hand, where contemporary documents from the past are lacking, quite different procedures have to be followed to gain some idea of how the present situation has come about. Documents were available to throw significant contemporary light on the past of the Sanusi and E-P made good use of them. Indeed, Worsley (1961, pp. 30-1), writing in a Marxist journal, describes *The Sanusi* as 'probably still the outstanding study of a revolution by a British social anthropologist'. For the other peoples E-P studied in the field,

historical documents were scanty at best, though later research has drawn persuasively on historical material bearing on relations between the Nuer and their neighbours (Sacks 1979; Kelly 1985).

With peoples lacking a documented past, the technique was, in E-P's words, 'to start with well-authenticated information about the present situation and then to try to explain it by what we can glean of the past from verbal traditions and literary sources. . . . This may seem like standing history on her head but such must to a large extent be the procedure in a reconstruction of the history of primitive peoples' (1971g, p. ix). He undertook this task for the Azande but not for the Nuer. Indeed E-P has been criticized for underestimating the changes brought about in Nuer society by the military campaigns conducted against them by the Sudan government shortly before E-P began his research (e.g. Anderson 1968, p. 48). Johnson, however, in his detailed study of these events, while identifying some mistakes of perception made by E-P concerning Nuer prophets and Nuer-Dinka relations, attributes more blame to other commentators on the Nuer who have taken statements made by administrators at their face value. He writes, 'They have been treated as equal in validity to Evans-Pritchard's work. All Nuer source material has assumed a timeless quality. . . . It is ironic that Evans-Pritchard's material has been treated in this way, for he repeatedly complained of the lack of historical method in modern anthropology' (1979, p. 18). Yet later Johnson (1981, p. 508) comments that E-P himself accepted the reports of explorers and administrators rather too readily at their face value. Nevertheless it cannot be denied that the sparse style in which *The Nuer* is written makes it easy for the busy or lazy reader, who looks at nothing else written about the Nuer, to fall into the trap of treating the Nuer as if they lived in some timeless anthropological never-never land rather than in the Sudan of the early twentieth century. This quality of *The Nuer* is well captured in the indubitably apocryphal story that E-P corrected the proofs of the book while sitting on the deck of a ship and that every time he encountered a fact, he plucked it out and threw it overboard.

E-P stressed the importance of scrutinizing carefully all documentary sources, and wrote: 'Every authority should be put in the witness box, if not in the dock, that it may be estimated whether what he alleges will stand the test of probing.' Yet he recognized that anthropologists faced a difficulty which most historians, writing about a remoter past, escaped. Their authori-

ties are usually dead, whereas for anthropologists, 'Our main authorities are often alive, or their children and friends are; and we may wish to avoid offending them' (1971d, pp. 174-5). This formulation, though it seems from the context to refer primarily to indigenous 'authorities', may explain E-P's reluctance to criticize Seligman and Radcliffe-Brown, and even Malinowski, in print while they were still alive.

The relation between the disciplines of anthropology and history was not seen by E-P as one-sided. He used the occasion of his Manchester lecture on *Anthropology and History* to turn on its head not history but Maitland's (1911, p. 295) classic remark about anthropology. The lecture ends with the call: '... history must choose between social anthropology or being nothing' (1961b, p. 20). This remark was made in Manchester, and not, we may note, in the common room of All Souls.

E-P was concerned not only with the historical dimension in anthropological inquiry but also with the history of anthropology itself. This interest first showed itself in what came to be known as the 'Cairo essays' (1933a, 1934c, 1936e), copies of which circulated among colleagues and students for many years in mimeographed form. These essays, sympathetic yet acutely critical, contrast sharply with the publications being produced at the same time by other members of Malinowski's seminar, which were more closely focused on reporting the findings of recent fieldwork. Nevertheless, the theories of these writers were discussed in the seminar (Firth 1975, pp. 2-4; Leach 1980). In Fortes's view, E-P's interest in Lévy-Bruhl was stimulated by Radcliffe-Brown (Fortes 1973, p. x), though Malinowski had also drawn attention to his writings. The essays also show E-P's characteristically ambivalent attitude towards his intellectual predecessors; on the one hand devoting considerable effort to a close study of their writings and praising their pioneering efforts, while on the other hand emphasizing the falsity of their conclusions. Thus for instance he ended his critique of Pareto's work with the comment that he did 'not consider Pareto's contribution to sociology of great importance' (1936e, p. 192). Later he published a similarly ambivalent account of Comte's sociological theories (1970e) and the same attitude marks his posthumously published *History of Anthropological Thought* (1981b). This ambivalence may perhaps be seen as a consequence of the difficulty of reconciling his sentiments as historian and scientist; the present cannot be understood without a knowledge of the past, and uncovering the errors of the past has heuristic value for the

present. In his introduction to the *History* Gellner (1981, p. xv) captures that difficulty by describing E-P as 'Not . . . a prophet, but rather an intellectually restless, ever-questing, sceptical Hamlet', though while E-P was unmistakably restless and sceptical, he had none of Hamlet's gloom. Yet although E-P wrote at much greater length than most of his colleagues about his intellectual forebears, he under-emphasized, in Gellner's (1986, p. 69) view, his substantial debt to Durkheim. Indeed, the references in his writings to predecessors and contemporaries from whom he had drawn inspiration are remarkably few. Douglas (1980, p. 29) has drawn attention to the contrast between E-P's 'punctiliousness in gratefully acknowledging every missionary's or district officer's evidence' and his 'virtual silence as to his intellectual debts'.

In his later writings, E-P turned away from the attempt to establish generalizations valid for all societies, that task on which Radcliffe-Brown had set his sights, and instead concentrated on conveying to an educated but not necessarily professional audience an account of the ways of acting, speaking, and feeling found among cultures other than its own. Writing in a book published posthumously he said, 'It has seemed to me that anthropologists (include me if you wish) have, in their writings about African societies, dehumanized the Africans into systems and structures and lost the flesh and blood. It may be somewhat of an experiment, but in these texts I am asking Azande to say in their own way what they want to say' (1974a, p. 9). He offered a book 'with only the barest introduction and commentary'. The book, entitled *Man and Woman among the Azande*, was also to be seen as 'a contribution to that problem of how men and women get along together in any part of the world' (1974a, p. 10). No doubt he would not have advocated such a non-interventionist role for the anthropologist if he had been discussing texts collected from a community about which little information had appeared in print; he could afford to skip yet another analysis of Zande culture. In fact he did provide brief comments on witchcraft, marriage payments, and blood-brotherhood but only reluctantly, on prompting from his friend Lucy Mair. Clearly, by that time in his life, his preference was strongly for publications that allowed people 'merely to tell, in their own way of telling it' (1974a, p. 10). Further indication of his non-interventionist stance is given by his proclaiming himself to be the editor, rather than the author, of the collections of texts.

This retreat from comment may perhaps be seen as the

continuation of a shift away from the analytic stance of the Cairo essays, which was already substantial by 1956 when *Nuer Religion* appeared. Indeed, Fortes (1977, p. 132) commented even on the earlier Zande publications that E-P 'sticks strictly to the manifest level, taking Zande statements of motives at their face value and refraining from speculating about underlying psychological mechanisms'. On the other hand Sperber (1985, pp. 14–29) has demonstrated the wide range of assumptions implicit in E-P's descriptions and interpretations of Nuer actions and statements.

E-P put a considerable effort into the publication of texts. A series of monographs, the Oxford Library of African Literature, began to appear in 1964, edited by E-P, his colleague Godfrey Lienhardt, and W. Whiteley, a linguist. He established, with assistance from All Souls, the Oxonian Press, which published privately printed Zande texts. Yet despite his claim to let Africans themselves show 'how they talk and think' (1971p, p. 9), he never really advocated non-intervention by the anthropologist. As in so many other contexts, his proclamations on the importance of texts have to be read along with other statements which substantially qualify them. For in an article written about the same time as the preface to the Zande collection, E-P insisted that 'It is essential to realize that facts are in themselves meaningless. . . . It is useless going into the field blind. One must know what one wants to know and that can only be acquired by a systematic training in academic social anthropology' (1937f, p. 1). Likewise he wrote 'one does not easily find what one is not looking for' (1972r, p. 226).

Although *Witchcraft among the Azande* appeared as early as 1937, its effect on anthropological research was delayed for longer than might have been expected. It was the publications of 1940, *The Nuer* and *African Political Systems*, which attracted the attention of the immediate post-war generation of young anthropologists, so that as late as 1963 E-P, writing a Foreword to a collection of papers on East African witchcraft, complained that there had been 'disappointingly few' studies of witchcraft since the publication of his Zande book (1963m). In her contribution to this collection Douglas (1963) stresses that although witchcraft beliefs generate processes for constructively re-aligning social relations, they also have seriously disruptive effects. She elaborates this bifocal analysis in her Festschrift chapter 'Thirty years after *Witchcraft, Oracles and Magic*' (1972). Another Festschrift, *The Allocation of Responsibility*, edited by Gluckman (1972),

examines the implications of E-P's analysis of Zande witchcraft in a diverse set of contexts, as well as recording its impact on the work of social anthropologists, economists, political scientists, and others associated with the University of Manchester. Many studies of witchcraft beliefs and practices elsewhere in Africa have now been published (Gillies 1976, pp. xxi-vii), taking E-P's Zande book as their point of departure.

In 1964 the philosopher Peter Winch published an article drawing heavily on *Witchcraft among the Azande*. Thirty-five years after the appearance of E-P's first article on Zande witchcraft, so it seemed, his analysis had been discovered by the philosophers. A lively and broad-ranging discussion about rationality and theories of causation followed, in part drawing on Lévi-Strauss's *Pensée sauvage* (1962b) as well as on E-P's work (cf. Hollis and Lukes 1982). The same aspect of E-P's writings, identified as a theory of social accountability, forms the main theme of Douglas's book on E-P in the *Modern Masters* series (1980). E-P himself, however, took no public part in the debate; his current concerns were elsewhere.

Although *The Nuer* is the best-known and most immediately controversial of E.-P.'s books, his last major work on the Nuer, *Nuer Religion*, is likely to remain for longer a focus of debate and argument. His early studies of magic led naturally to wider analyses of belief and religion, coupled with his increasing articulation of his own religious faith. Yet perhaps we should not be surprised that after over three hundred pages of closely argued exposition and analysis, E-P ends his book with the sentence, 'At this point the theologian takes over from the anthropologist' (1956g, p. 322). Nuer religion, 'ultimately an interior state', remains inaccessible to anthropological explanation. The book, nevertheless, became a model and inspiration to many younger anthropologists, notably his pupil Lienhardt in *Divinity and Experience* (1961), and also, as with all his other major works, attracted detailed criticisms from those who followed in his footsteps in the Sudan (e.g. Beidelman 1981).

One feature of Nuer belief attracted particularly wide attention. In 1923 H. C. Jackson, an administrative officer in the Sudan, reported that the Nuer believed that there was a close connection between twins and birds (Jackson 1923, p. 142). In 1936 E-P wrote that among the Nuer 'twins are also spoken of as birds' (1936a, p. 235). He discussed this custom further in *Nuer Religion* (1956g, pp. 128-33) and his wife Ioma published a short story based on this identification, partly prompted by their own

twin sons. The relation between Nuer twins and birds was then discussed by Lévi-Strauss (1962a, pp. 114–19) and other anthropologists, notably by Gellner (1962) and Firth (1966). A lively exchange of letters to the editor of *Man* followed, as well as articles elsewhere (cf. Beidelman 1981, p. 132, fn. 47). E-P confined himself to a single letter dealing mainly with points of fact, avoiding the philosophical issues raised by others, issues he had indeed dealt with in his book.

The great interest aroused in the 1960s among British social anthropologists by the writings of Lévi-Strauss on the structural consequences of prescriptive marriage rules led many to forget an earlier debate about the social consequences of customary marriage payments in which E-P participated. One of his earlier articles is a plea for a better understanding of what he asked his colleagues to call 'bride-wealth' rather than bride-price (1931c). He followed this with an article on the same topic drawing directly on his research among the Azande, attacking the proposition that 'the function of bride-wealth is to stabilize marriage' and asking querulously whether it was true 'that the relations between husband and wife persist through what amounts to economic blackmail?' (1934d, p. 173). It is interesting to note that in this article E-P anticipated the notion of a marriage cycle later to be popularized by Lévi-Strauss, though with wives exchanged for daughters and spears rather than for sisters. His data on the frequency of Nuer divorce and its relation to the payment of bride-wealth led to another wide-ranging debate, in which Gluckman (1950) and many others participated, on the stability of marriage in societies of different structural types.

The term 'lineage' had been used, by both anthropologists and other writers, for a great variety of social forms. It was Radcliffe-Brown who suggested to E-P (1946m, p. 908; cf. Fortes 1978, pp. 2, 6–7; 1979, p. viii) that this was the word to use for the agnatic groups found among the Nuer, a suggestion taken up also by Fortes in his work on the Tallensi. The term then acquired a specialized connotation, and a considerable body of literature was produced describing and discussing the characteristics of lineages, particularly those found in stateless societies. In this corpus, although the 'Introduction' to *African Political Systems* and Fortes's books on the Tallensi are important points of reference, the *fons et origo* is E-P's *The Nuer* (cf. Kuklick 1984, pp. 73–6). Fortes, writing as late as 1979, maintains that 'No single work of post war British Social Anthropology has had, or continues to have, the same seminal influence on research and theory as



Evans-Pritchard's *The Nuer* (1979, p. vii). Discussion of this work may be said to have culminated in a review article 'Reading *The Nuer*' in the international journal *Current Anthropology* (Karp and Maynard 1983; Glickman 1985), with contributions from eleven scholars. Yet although comments on lineages in general, and the Nuer system of lineages in particular, began to appear even in the early 1950s, E-P did not respond.

In the last few years there has been active discussion by anthropologists about how, or whether, their professional expertise can and should be applied to facilitate social changes. It is sometimes forgotten that this is a topic that was discussed at length in the decade before the war. It was comparatively neglected in the immediate post-war period when opportunities and funds for research guided mainly by intellectual curiosity, rather than governmental exigency, became available in quantity. E-P contributed substantially to the pre-war debate, which was conducted in what in retrospect appears as a naïvely unproblematic colonial context. Nevertheless E-P identified two distinct issues in the debate: judgements about the moral status of features of indigenous cultures, and the practical value of anthropological insight for the implementation of social change. Early in his career he argued that the 'description of institutions and the formulation of scientific laws concerning them, on the one hand, and ethical evaluation of them on the other' should be kept severely apart (1933b). In a paper addressed to a Catholic audience, E-P outlined two divergent positions taken by various of his anthropological colleagues. One view was that the anthropologist should refuse to make moral judgements about the social life of the community being studied, leaving that task to others. The other view was that, since in fact it is impossible for the anthropologist to avoid being influenced in his descriptions and analysis by his own religious and political values, it is better to give 'a definite and explicit bias to his writings'. E-P rejected both of these views and urged that the field worker should make, but also keep apart, two different kinds of judgement: on the significance of social facts to scientific theory, and on the significance of the same facts to moral theory. He called therefore for some Catholic moral philosophers to study social anthropology (1964h, pp. 412-14).

But while moral evaluation might be important, for most anthropologists the pressing problem in the short term was the proper handling of the implications for public policy, if any, of their research findings. Here E-P was more cautious than he was

with Catholic theologians; one of his maxims imparted to graduate students was: 'He who sups with the Administration needs a long spoon.' He insisted that he had never been a government anthropologist, even though the Sudan government had supported his research financially and logistically, and that the Sudan government had never once sought his advice (1946a, pp. 97-8). Nevertheless the Sudan administration recognized the value of ethnographic research. E-P referred approvingly to his wartime service in Cyrenaica, where he had been a full member of the military administration and was able to influence policy toward the Arab population, though his assertion that during this period he 'did no anthropological research at all' has to be read in a very narrow sense. His ambivalence in this context has been well captured by one of his pupils with the label of a 'reluctant imperialist' (James 1973). For the brief but memorable period when he was in command of Anuak warriors, he put his anthropological expertise to good use. Writing to Fortes from Anuak territory, he asked, 'Is it not a Marxist principle that study of social facts should be related always to practical ends? Certainly when one tries to use Anuak society as an instrument in defeating the Italians many of its most essential qualities appear in disconcerting ways!' (31 January 1941). On this topic, as with many others, it is easy to miss the complexity of E-P's views by quoting selectively from his writings. For instance, in his 1946 article on 'Applied anthropology' he said that he saw no incompatibility between the anthropologist's roles as researcher and adviser (1946a). His own experience in Cyrenaica supports this view. But only a few years later, in a radio talk, he argued that anthropologists should eschew questions of policy to avoid compromising scholarship (1951f, pp. 120, 123).

### III

E-P carried out all his field research in a colonial context, when the Sudan was known as the land of Blacks ruled by Blues, and before 'development' had acquired its contentious post-war meaning. Yet even in the Sudan of the 1920s he was aware of the possible consequences for his informants if he published all he knew of their acts. Among the Azande he joined the Mani so-called 'secret society', or closed association as E-P preferred to call it, though he was unsure whether or not the association was still regarded as illegal by the Sudan government (1931d, p. 109). To protect members of the association from punishment he did

not mention their names in his publications. The illegal status of the association prevented him, he said, from making a thorough investigation, yet the account he did publish (1931d; 1937g, pp. 511-39) might well be judged thorough by ordinary standards. On the other hand he published nothing at all about other illegal Zande associations, giving the somewhat surprising reason that to do so would offend European sentiments (1937g, p. 511).

In the heady period immediately after the end of the war when the Institute was responding actively to the new leadership supplied by E-P, it was fashionable to say that in social anthropology there were no textbooks, and could never be any; the discipline was far too subtle to be captured in an elementary handbook. In particular, the techniques of field research could not be reduced to a cookbook recipe. Indeed, the most well-known summation of the research task in the field was E-P's own laconic maxim: 'the method of study is to get to know well the persons involved and to see and hear what they do and say' (1954g, p. x). This prescription was backed up with the practical advice that in the field the ethnographer must have two tables, one for meals and the other for work. But later in life E-P began to dwell on his own past. He was well aware of how different were the field conditions faced by younger generations of anthropologists and commented in 1973 that he would not 'care to try to do research in most of the Arab lands' (1973f, p. 9). He noted that earlier field workers—he mentioned Radcliffe-Brown and the Seligmans—had published little about the material and physical circumstances in which they collected information, and that therefore their findings could not be fully evaluated. This was the rationale for publishing a couple of articles (1973f, 1973p) on field research as he had known it, which gave to a wider audience facts about E-P's life that hitherto had been gleaned only by his friends from numerous conversations, usually both hilarious and bibulous.

As might be expected, he stressed the importance of a command of all the relevant languages, both the vernaculars spoken by members of the community being studied and the written languages of the scholarly literature about the community. E-P was proud of his linguistic ability and of the number of languages he could master, including the Nuer language which of necessity he had to learn without the help of any interpreter or written grammar. He published several articles on various features of the Sudanese languages he had used in his field research but these were offered only as a by-product of his

inquiries into other matters. Indeed, discussing his fieldwork in a letter to Fortes he said, 'I early jettisoned language and material culture since something had to go or I should have had a fit with overwork' (25 November 1934). Although E-P sometimes tried to give the impression to an unwary stranger that he never read anything, in fact he was very widely read and compiled extensive multi-lingual bibliographies. A colleague once described him as 'a secret reader'.

In the field, his relations with the people he studied differed widely. He found the Nuer so unco-operative that when he returned to the Sudan in 1935 he considered abandoning any plans for further work among them. He told Fortes that with the Nuer, 'I spent most of my time fishing as information just occasionally trickled in. In fact I cannot make up my mind whether to cut my losses and start on the Anuak or try the Nuer once more' (24 December 1934). He did go back and, he later maintained, his difficulties in obtaining information in ways he had used successfully among the Azande were compensated for 'by the intimacy I was forced to establish with the Nuer. . . . Azande would not allow me to live as one of themselves; Nuer would not allow me to live otherwise' (1940k, p. 15).

Participant observation, the term now commonly used to describe the method of fieldwork exemplified by E-P's studies among the Azande, implies both observation and participation by the research worker. E-P carried out all his field research before the arrival of tape recorders and other aids to observation. He was unsuccessful with the camera and relied on others for many of the striking illustrations to his books. He never used even a notebook in public, believing 'that somehow a notebook came in between them and me and broke our contact. I memorized what I saw and heard and wrote it down when I got back to the privacy of my abode' (1973f, pp. 5-6). He did however take down texts in seclusion. In contexts that he could not observe at first hand, he relied on the reports of those who had participated in the events he wanted to know about. A notable instance of this procedure was his use of his Zande servant, Kamanga, who began training as a witch doctor, something E-P decided it would not be effective to do himself. By agreement with his teacher, Kamanga reported back regularly to E-P as the training progressed (1937g, p. 151). In a letter of advice to Fortes about how to make inquiries in the field he said: 'I think it is a mistake to try to understand natives through the intellect alone but that one must throw one's notebooks for a fortnight at a time into one's

boxes and simply live with them, taking part in their motor and emotional expressions. In this way one gets a *real* perspective and not merely a *book* or *recorder's* perspective. You may say this is all rot. However I should try it—just write nothing for 15 days—it is a rare discipline and I found it provided me with my most fruitful periods—not in quantity of material but in quality' (25 November 1934; his emphasis).

#### IV

E-P's relations with other anthropologists were complex. Much more than the members of many other professions, social anthropologists enjoy gossiping about one another. E-P enjoyed this activity whole-heartedly but for him it was an oral activity, to be indulged in with his colleagues and students in the pub after work, whereas in what he wrote other rules applied. He was markedly reluctant to express open criticism of the work of colleagues during their lifetime, and showed a respect for the past more pronounced than was found among most of his contemporaries. For example, in a comment on his failed attempt to establish an Honours School in Social Anthropology at Oxford, and thus to begin undergraduate teaching, he admitted to being 'always in two minds in the matter', but that 'I felt that I was bound to do so, as this had been the aim of Tylor and Marett; and even Radcliffe-Brown advocated it at the end of his tenure of the chair' (1959c, p. 123). While committing himself to laudatory remarks when the occasion seemed to demand them, he usually refrained from engaging his colleagues in critical debate in print, and yet allowed himself in informal conversation to elaborate in fanciful detail on their defects as he saw them. His professional colleagues were able to accept these comments with the stiff dose of salt they needed, but inexperienced students were sometimes led astray and mistook them for E-P's sober opinion on the state of the discipline.

In an article published after his retirement from the Oxford chair, E-P described the survey work done by the Seligmans, reported in their book *Pagan Tribes of the Nilotic Sudan* (1932), as 'largely superficial and dubious' (1971d, p. 146). Much of the information on which the book is based was supplied by other investigators, E-P among them. He defended himself against the charge of unwarrantedly remaining silent about the defects of the book until long after its publication, and after both the Seligmans had died, by saying that 'I did not see in their final

form the parts for the contents of which I had some responsibility—I would certainly not have accepted some of what was said had I done so. . . . I could not bring myself to making public their errors during their lifetime' (1971d, p. 130). E-P had in fact done rather more than remaining silent about errors, for in 1940 he referred to their researches published in *Pagan Tribes*, as 'brilliant' (1940k, p. vii).

Despite substantial differences in outlook, throughout his career E-P remained a loyal friend of both Charles and Brenda Seligman and published his criticisms of their work only after they were dead. With Malinowski, however, he soon quarrelled, though his criticisms remained unpublished for many years, long after Malinowski's death (cf. Gellner 1981, p. xxvi). In an article published in 1973, a few months before he died, E-P wrote disparagingly of the training for field research that he received while at LSE. 'My teacher, Seligman told me to take ten grains of quinine every night and to keep off women.' Asking Malinowski for advice about how to do fieldwork, he was simply 'told not to be a bloody fool' (1973f, p. 1). He dismissed the advice he got from Westermarck, Haddon, and Flinders Petrie in a similar fashion. But this defamatory catalogue, an example of a style of writing that he followed with increasing frequency in his later years, has to be matched against another account that he published only a month or two earlier, in which he said that he learnt from Malinowski more than from anyone else (1973e, p. 19). In a collection of Zande texts that appeared after his death he acknowledged his debt by saying, 'I must pay my respects to B. Malinowski, who was the first to impress on me the importance of recording texts in the vernacular among primitive and illiterate peoples' (1974a, p. 11). His passion for texts was, he said, 'inflamed by Malinowski' (1973f, p. 10). Indeed, though E-P invariably described himself as a pupil of Seligman (or even as the only pupil), he did from time to time proclaim himself to be also a pupil of Malinowski (e.g. 1947h, p. 714), though Fortes (1973, p. ix) comments that E-P, along with Schapera, 'never considered themselves to be his pupils in the same sense' as did Firth, Richards, Mair, and Wedgwood. Yet the quarrel was long-lasting and E-P asserted that two or three years after moving to LSE he was barred from the institution by Malinowski. He did in fact give occasional lectures at the School during 1928–31, at the invitation of Seligman, and in 1931 was appointed as Temporary Assistant in Anthropology (cf. Leach 1980). Firth recounts an extraordinary incident that occurred

when Malinowski and E-P, together with Isaac Schapera, were jointly editing a Festschrift for Seligman (1934e). Malinowski begged Firth to serve as a fourth editor so that his own name would not have to appear in alphabetical order next to E-P on the cover of the book (Firth 1981, p. 121). E-P made many critical, and indeed sometimes scurrilous, statements about Malinowski and his anthropological stance, some of which have been listed and rebutted by Firth. In a letter published posthumously, E-P asserted that one of his earliest articles (1929a), a comparison of magic among the Azande and as practised in the Trobriand Islands, where Malinowski had carried out research, had been altered by Malinowski before publication so as to suggest that 'any originality my article might appear to have was due to him' (1974d). Malinowski, on the other hand, seems not to have voiced his dislike of E-P in print, though in a private letter to one of his students in 1940 he mentioned E-P among 'some of my pet aversions in the anthropological world' (Ardener 1980, p. 127). In assessing these defamatory comments, Firth offers the sobering observation: 'when what are ostensibly scientific judgments—either positive or negative—are passed in a circle of people who have known each other personally, they must be regarded as a function of the total situation and may have to be interpreted in the light of considerations not always immediately apparent' (Firth 1981, p. 122). This caution applies particularly to interpreting E-P's relation to Malinowski (cf. Burton 1983).

E-P met Radcliffe-Brown for the first time in August 1931. Fortes (1973, p. ix; 1978, p. 17) says that E-P had already been favourably impressed with Radcliffe-Brown's *Andaman Islanders* (1922), though later E-P described the book as 'very bad'. While Radcliffe-Brown was still alive, E-P avoided public criticism and displayed the same quality of silent loyalty that characterized his attitude towards Seligman. He contributed to Radcliffe-Brown's Festschrift (1949l) and, with Eggan, wrote an appreciative Foreword to a collection of Radcliffe-Brown's articles (1952g). *Kinship and Marriage among the Nuer* is dedicated to Radcliffe-Brown 'whose vast knowledge and illuminating analyses of primitive kinship systems have placed all students of the subject in his debt' (1951g, p. vii). E-P nominated Radcliffe-Brown for the Rivers Medal of the Royal Anthropological Institute and supported him when he was dying in straightened circumstances in London. In the obituary notice he wrote for *The Times* E-P spoke of Radcliffe-Brown's pre-eminence in British social anthropology and stated that he was more at home and happier in the

common room or smoking room of All Souls than anywhere else (1955g). But later E-P became more outspoken in his comments on his predecessor in Oxford. In the end, he stated in print that he doubted whether Radcliffe-Brown 'had ever read a history book in his post-school life' (1970d, p. ix). He said that Radcliffe-Brown 'was not a success at Oxford. Personally and in private urbane, he was inclined when talking to colleagues to combine arrogance with vanity. This can be done at Oxford, but only in the Oxford manner' (1973e, p. 23).

The anthropologist of his generation with whom E-P had closest relations was Meyer Fortes, whom he met when they were both attending Malinowski's seminars in London in the 1930s. He corresponded with Fortes when the latter was beginning field research in west Africa, offering him detailed advice about how to proceed. They collaborated in editing the symposium on African political systems (1940i) that had such a sustained influence on post-war British social anthropology. Fortes (1945, p. xiv) said that he looked upon E-P as his 'older brother in anthropological studies' and the two men remained friends for life. In a review of Fortes's first book, *The Dynamics of Clanship*, he asserted that 'in anthropological fieldwork one should never study peoples, only problems', and described the book as 'by far the most comprehensive and illuminating study yet made of a lineage system' (1946m). Later he reacted sharply to a suggestion made by a reviewer of *The Azande* (*TLS*, 1971) that there had been a clash of personalities between Fortes and himself.

When E-P resigned his professorship in Cairo in 1935, he was succeeded by A. M. Hocart, on E-P's recommendation. Hocart, with whom E-P declared himself to be in 'close intellectual harmony', had like him become interested in anthropology while an undergraduate at Exeter College. He had been associated with Elliot Smith, though without subscribing to the anatomist's heliocentric diffusionist views. E-P regarded his sponsorship of Hocart as 'paying back, as it were, my debt to Elliot Smith' (1971b, p. 38) and maintained that, in the long run, Hocart, who was a scholar, might prove to have exerted on British anthropology an influence comparable to that of Malinowski and Radcliffe-Brown (1967a; 1970d, p. ix).

Elliot Smith himself nowadays has few disciples among professional anthropologists. Yet throughout his career E-P maintained a charitable view towards the intellectual objectives, though not toward the analytical procedures and alleged findings, of the diffusionists. His review of Collingwood's *Roman Britain* (1932d)



shows how E-P considered that the phenomenon of cultural and social diffusion should be studied, and while recognizing that Perry and Elliot Smith came nowhere near reaching this standard, he remained sympathetic to what they said they were trying to do. This sympathy was not merely derived from the help he had received from them before the war in London and Cairo. Towards the end of his life he showed support for the far-from-mainstream journal, *The New Diffusionist*, which he saw as an 'endeavour to restore the historical perspective in anthropology' (1971b, p. 39).

## V

Though the anthropologist in the field may be completely committed as an observer, his or her participation can never be entirely whole-hearted, as E-P recognized. He lived in mud huts among the Azande, he was a guest in their tents among the Bedouin, and among the Nuer he acquired a herd of cattle 'as the price of my acceptance, or at any rate tolerance' (1973f, p. 2). But 'One cannot really become a Zande or a Nuer or a Bedouin Arab, . . . one always remains oneself, inwardly a member of one's own society and a sojourner in a strange land' (1973f, p. 3). This limitation on the extent to which an outsider can participate causes many practical difficulties but raises more serious questions in the realm of belief. E-P was born in the Anglican tradition and then became a Roman Catholic, so that, as Fortes (1980, p. vii) points out, he could make a clear contrast between the Zande belief in witchcraft, which he studied but did not share, and the Nuer belief in God or *kwoth*, spirit, which in a sense he did share, despite the substantial differences between Nuer religion and Christianity. Indeed, he wrote, 'I would say that I learnt more about the nature of God and our human predicament from the Nuer than I ever learnt at home' (1973f, p. 5). Yet in the field it becomes necessary for the anthropologist to act as if the beliefs of the community are true: 'You cannot have a remunerative, even intelligent, conversation with people about something they take as self-evident if you give them the impression that you regard their belief as an illusion or a delusion' (1973f, p. 4). Hence among the Azande E-P let his own life be governed by the decisions of the poison oracles in the same way as did his neighbours. 'If one must act as though one believed, one ends in believing, or half-believing as one acts' (1973f, p. 4). Acting as though one believes, however experienced the actor,

requires at least some sympathy with the belief and for a while some of E-P's more zealous followers maintained that only an anthropologist who believed in some kind of god was competent to undertake a field study of religious belief. E-P himself took a more cautious view and distinguished between the 'study of religion as a factor in social life', a task that could be undertaken by an atheist, and the study of religion as 'essentially of the inner life', which, quoting the words of W. Schmidt, E-P held could be 'better done by one in whose inward consciousness an experience of religion plays a part' (1965g, p. 121). Yet this proposal for a division of labour sits uneasily with the final sentences of *Nuer Religion*, quoted earlier; although the book was written, as Lienhardt (1986) says, 'from an explicitly theistic viewpoint', E-P the anthropologist eschewed the study of Nuer religion as 'an interior state'. On the other hand, with reference to the study of mysticism, which seems to belong unequivocally to 'the inner life', E-P quoted with approval Dr Samuel Johnson's comment 'He who drives fat oxen need not himself be fat' (1970k, p. 101).

E-P was received into the Roman Catholic Church in 1944 in the cathedral of Benghazi, but he made it clear that in his case conversion was no sudden event. Indeed, he had earlier made two unsuccessful attempts to be received into the church, but 'was put off by the catechism and some of the arid scholastic, theological and tortuous arguments'. 'It was', he wrote, 'a slow maturing; accumulating much sin, I regret to say, on the way to the final plunge' (1973b, p. 37). This was a plunge not from the atheism to which the majority of social anthropologists of his generation subscribed, but from a less closely specified belief in God. From childhood on, E-P had always enjoyed reading and reciting poetry, and while working in the Sudan he was sometimes known to other Europeans as 'The Poet' (Johnson 1982, p. 236). Two of his poems appeared in print, one a translation of an elegy on the execution of a Bedouin martyr (1949k). The other, 'The beautiful names of God' (1945e), was first published in Arabic and English in Benghazi shortly after his conversion. This poem was subsequently reprinted in three Islamic journals as well as in the English Dominican journal *Life of the Spirit*. It was a muslim rosary that he habitually fiddled with.

His sympathy with Islam was congruent with the political sentiments of the earlier part of his career, when he was decidedly pro-Arab. Later, however, following the failure, as he saw it, of the Arab states to defend Palestinian interests, he became more sympathetic towards the plight of Jews, and signed

a letter to the *Times* in support of persecuted Jews in Syria. Although he was the Aquinas Lecturer in Oxford in 1959, Lienhardt (1974, p. 303) comments: 'He was not what is called a "good Catholic" . . . and with what seemed to some a paradox maintained that only in the Catholic Church could one find freedom of thought, since only those who knew what it was to affirm belief in so much knew what scepticism really could be.' (There is a striking similarity between this paradox and E-P's well-known comment that 'it may be partly just because the agnatic principle is unchallenged in Nuer society that the tracing of descent through women is so prominent and matrilocality so prevalent' (1951g, p. 28).) He complained about the 'colonialist religiosity' of some Christian missionaries (1969c, p. 815). In the last few years of his life he became interested in mysticism and, in a remarkable paper read to a student audience, argued in almost lyrical language that, in some important sense, mystics of all religious persuasions, Hindu, Buddhist, Muslim, and Jewish as well as Christian, experience the same transcendental reality (1970k). E-P commented to Fortes, who was in the audience, that 'It must have been apparent to you, if not to them, that this is my inner life' (23 May 1972). Indeed, in more mundane terms he had argued many years earlier that religious beliefs that are universal, such as survival after death, are not therefore any the less likely to be true (1947h, p. 715). In April 1972, five months before his seventieth birthday, he wrote to Fortes to say that 'I shall from then give most of my time to religious thought and duties'; but the same short letter also announced his plans for a farewell visit to South Africa in 1973, a trip he never made. In E-P's own words: 'I have no regrets. Bad Catholic though I be, I would rather be a bad one than not one at all' (1973b, p. 37).

It would be easy to say of E-P that he was a man with charisma. Unfortunately, popular overuse of this term has long since robbed it of the theological and sociological specificity it once had. E-P described the first person to inspire him to become an anthropologist, Marett, as a boaster, but this was something that E-P was not, for the stories he delighted to tell were usually about others; when he described his own exploits it was to enhance his reputation not as a pillar of society or as an intellectual giant but as an *enfant terrible*. He enjoyed his own eccentricity, as the incident in an Oxford café related by Lienhardt (1974, p. 302) illustrates delightfully. Shortly before he died he started to write an autobiography, saying ' . . . either one writes about oneself as one knows oneself to be like inside

oneself, which would be an embarrassment to one's family and friends and would involve others in disrepute, or one presents a masked image of oneself and plays the role of both clown and liar. . . . I have made the second choice, to be a liar, not with regard to facts but in failure to disclose them all, the more intimate ones' (cf. Beidelman 1974b, p. 555). In his final years his health was endangered by too much alcohol and he temporarily became blind from too much tobacco. His friends became worried about his style of living but to little effect. As he wrote to Fortes 'I won't look after myself. What is the use of trusting in God if you can't leave it to him' (12 August 1972). Firth has said that E-P 'was not noted for his charity' and that he 'was an expert in the glancing blow' (Firth 1981, p. 121), but it is also true that throughout his career he remained true to the proposition he had enunciated in 1940: 'No man of science could wish for a finer tribute than the weight of evidence gathered to refute his opinions' (1940b, p. 25). There is now a substantial corpus of critical comment focused on E-P's Zande and Nuer writings and it is striking how often younger anthropologists do indeed pay tribute to him both as teacher and ethnographer. He was often exasperated by critics who appeared to have read only one item of his writings, or who had taken a passage out of context, but was warmly supportive of his juniors who had worked through the same sources as he had used and who, with their own field observations, were ready to challenge him in print.

E-P delighted in the use of words, particularly in the succinct expression of complexity and ambiguity, earning from his American colleague David Schneider (1965, p. 74) the barbed tribute of a reference to 'those special gems of paradoxical obfuscation for which Evans-Pritchard is justly famous'. Geertz (1983, p. 62), who analyses E-P's prose style in detail, refers to 'the maddening brilliance of it'. Lienhardt (1974, p. 303) notes: 'I think that he got more pleasure out of writing his article on "Sanza" [1956c], Zande double-talk, than out of most, delighting as he did in subtleties, sometimes the dangerous subtleties, of human relationships.' E-P himself explained *sanza* by saying that 'a Zande very often is deliberately evasive and obscure in his talk in order to say what he wants to say without actually saying it, even to the point of saying the opposite of what he means' (1974a, p. 14).

For a summation of his personality we perhaps cannot do better than to use the words that he used himself in his obituary notice of his friend and colleague, J. H. Driberg, another wayward student of Seligman and Malinowski, to whom E-P

had dedicated his Azande monograph and who, like E-P, had attended The Grange preparatory school, Crowborough, and been a warrior in the Sudan: 'exceptional linguist, . . . inspiring tutor, stylist, and brilliant talker—at his best *splendide mendax*— . . . His was a rare spirit and his weaknesses were consistent with the heroic in his personality and further endeared him to his friends. . . . He had *baraka*' (1947c, p. 12).

J. A. BARNES

In preparing this memoir I have been greatly helped by the preliminary work done by Meyer Fortes who had started to assemble material for the memoir but who died before he could begin to write it. He had gathered together many, though by no means all, of the letters he had received from E-P, spanning the years 1932 to 1972, as well as E-P's drafts for his autobiography. Fortes also elicited recollections of E-P from Sir Raymond Firth and several others who had known him well. I first met E-P in 1939 and last saw him in 1973. I am much indebted to Sir Keith Hancock, Dr Ian Hogbin, Dr Godfrey Lienhardt, Professor M. N. Srinivas, and Mrs Kathleen Turnbull for telling me about E-P's life and work. I am very grateful to Frances Barnes, Dr Marie Reay, Professor Basil Sansom, and Professor Isaac Schapera for their comments on an earlier draft of the memoir.

In 1964 E-P compiled a list of his publications (19641). This list was then edited by T. O. Beidelman and published in a Festschrift volume (1971q). It was later brought up to date and issued, together with a biographical note by Beidelman, as a separate booklet (1974e). In the references printed below, I include the items mentioned in the memoir, using the identifying labels from the 1974 list, together with those posthumous publications that have appeared subsequently. The 1974 list, with about 390 items, has many mistakes and inconsistencies; fortunately few of these are serious. I include eighteen items I have not cited where a user of the list might be led astray.

*Writings by Evans-Pritchard*  
(selected items only)

## Abbreviations

AA	American Anthropologist
AS	African Studies
BFA	Bulletin of the Faculty of Arts, University of Egypt
BSOAS	Bulletin of the School of Oriental and African Studies
JASO	Journal of the Anthropological Society of Oxford
JRAI	Journal of the Royal Anthropological Institute
SNR	Sudan Notes and Records

- 1927 A preliminary account of the Ingassana tribe in Fung province. *SNR*, x. 69–83.
- 1929a The morphology and function of magic. *AA*, xxxi. 619–41.
- 1929b The Bongo. *SNR*, xii. 1–61.
- 1929c The study of kinship in primitive societies. *Man*, xxix. 190–1.
- 1929f Witchcraft (mangu) amongst the Azande. *SNR*, xii. 163–249.
- 1931c An alternative term for 'bride-price'. *Man*, xxxi. 36–9.
- 1931d Mani, a Zande secret society. *SNR*, xiv. 105–48.
- 1931e See 1932f.
- 1932b The nature of kinship extensions. *Man*, xxxii. 12–15.
- 1932d Review: Collingwood, *Roman Britain*. *Man*, xxxii. 220–1.
- 1932f Heredity and gestation, as the Azande see them. *Sociologus*, viii. 400–14.
- 1933a The intellectualist (English) interpretation of magic. *BFA*, i. 282–311.
- 1933b Review: McLeod, *Origin and History of Politics*. *Man*, xxxiii. 73.
- 1934c Lévy-Bruhl's theory of primitive mentality. *BFA*, ii. 1–36.
- 1934d Social character of bride-wealth, with special reference to the Azande. *Man*, xxxiv. 172–5.
- 1934e EVANS-PRITCHARD, E. E., *et al.* (eds.). *Essays Presented to C. G. Seligman* (London: Kegan Paul, Trench, Trubner).
- 1936a Customs and beliefs relating to twins among the Nilotic Nuer. *Uganda Journal*, iii. 230–8.
- 1936e Science and sentiment: an exposition and criticism of the writings of Pareto. *BFA*, iii. 163–92.
- 1937e Anthropology and the social sciences. In DUGDALE, J. E. (ed.), *Further Papers on the Social Sciences* (London: LePlay House Press), pp. 61–74.
- 1937g *Witchcraft, Oracles and Magic among the Azande* (Oxford: Clarendon Press).
- 1940b Lucien Lévy-Bruhl, 1939. *Man*, xl. 24–5.
- 1940h *The Political System of the Anuak of the Anglo-Egyptian Sudan*. (Monographs on Social Anthropology, 4, London: Lund Humphries).
- 1940i FORTES, Meyer, and EVANS-PRITCHARD, E. E. (eds.). *African Political Systems* (London: OUP) [Introduction, pp. 1–23].
- 1940k *The Nuer: a Description of the Modes of Livelihood and Political Institutions of a Nilotic People* (Oxford: Clarendon Press).
- 1940l The political structure of the Nandi-speaking peoples of Kenya. *Africa*, xiii. 250–67.

- 1940m EVANS-PRITCHARD, E. E., and FORTES, M. Memorandum on a plan of research into problems of modern political development in Africa. *Hebdomadal Council*, clxxv. 185-96.
- 1941 Foreword. In McPHERSON, J. W., *The Moulids of Egypt (Egyptian Saints-days)* (Cairo: N. M. Press), pp. ix-xi.
- 1945e A hymn: We adorn this issue with the ninety-nine attributes of God. *Barqua Weekly Review*, lxii (3 March). Reprinted, *inter alia*, 1946, as The beautiful names of God. *Life of the Spirit*, iii. 1-3.
- 1945-9 A select bibliography of writings on Cyrenaica. *AS*, iv. 146-50; v. 189-94; viii. 62-5.
- 1946a Applied anthropology. *Africa*, xvi. 92-8.
- 1946c Hereditary succession of shaikhs of Sanusiya lodges in Cyrenaica. *Man*, xlvi. 58-62.
- 1946h Social anthropology. *Blackfriars*, xxvii. 409-14.
- 1946i G. O. Whitehead. *SNR*, xxvii. 242-3.
- 1946m Review: Fortes, *Dynamics of Clanship*. *BSOAS*, xi. 906-08.
- 1947c Jack Herbert Driberg: 1888-1946. *Man*, xlvi. 11-13.
- 1947h Does anthropology undermine faith? *Listener*, xxxvii. 714-15.
- 1948a *The Divine Kingship of the Shilluk of the Nilotic Sudan* (CUP).
- 1948f Social Anthropology: an inaugural lecture delivered before the University of Oxford on 4 February 1948 (Oxford: Clarendon Press).
- 1949i *The Sanusi of Cyrenaica* (Oxford: Clarendon Press).
- 1949k Translation of an elegy by Ahmad Shauqi Bey on the occasion of the execution of Sidi 'Umar al-Mukhtar al-Minifi. *Arab World* (London), xix. 2-3.
- 1949l Nuer rules of exogamy and incest. In FORTES, Meyer (ed.), *Social Structure* (Oxford: Clarendon Press), pp. 85-103.
- 1950d Social anthropology: past and present. *Man*, l. 118-24.
- 1951f *Social Anthropology* (London: Cohen and West).
- 1951g *Kinship and Marriage among the Nuer* (Oxford: Clarendon Press).
- 1951j Some features of Nuer religion. *JRAI*, lxxxii. 1-13.
- 1952a See 1951j.
- 1952g EVANS-PRITCHARD, E. E., and EGGAN, Fred. Foreword. In RADCLIFFE-BROWN, A. R., *Structure and Function in Primitive Society* (London: Routledge and Kegan Paul), pp. v-vi.
- 1953c The sacrificial role of cattle among the Nuer. *Africa* xxiii: 181-98.
- 1953h Religion in primitive society. *Blackfriars*, xxxiv. 211-18.
- 1953j See 1954h.
- 1954g Foreword. In PITT-RIVERS, Julian, *The People of the Sierra* (London: Weidenfeld and Nicolson), pp. ix-xi.
- 1954h Foreword. In HOWELL, Paul. *A Manual of Nuer Law* (London: OUP), pp. v-vii.
- 1955g [unsigned] Prof. Radcliffe-Brown: leader of British anthropology. *The Times*, 27 October, p. 14, col. B.
- 1956c Sanza, a characteristic feature of Zande language and thought. *BSOAS*, xviii. 161-80.
- 1956g *Nuer Religion* (Oxford: Clarendon Press).
- 1958c An historical introduction to a study of Zande society. *AS*, xvii. 1-15.
- 1958d Preface. In MIDDLETON, John, and TAIT, David (eds.), *Tribes without Rulers: Studies in African Segmentary Systems* (London: Routledge and Kegan Paul), pp. ix-xi.

- 1959c The teaching of anthropology at Oxford. *Man*, lix. 121-4.
- 1960h The organization of a Zande kingdom. *Cahiers d'études africaines*, i(iv). 5-37.
- 1960j Introduction. In HERTZ, Robert. *Death and The Right Hand* (London: Cohen and West), pp. 9-24.
- 1961b *Anthropology and History* (Manchester: Manchester University Press).
- 1962g *Essays in Social Anthropology* (London: Faber and Faber).
- 1962h Ideophones in Zande. *SNR*, xliii. 143-6.
- 1963d The Comparative Method in Social Anthropology (Hobhouse Memorial Lecture, 33, London: Athlone Press).
- 1963m Foreword. In MIDDLETON, John, and WINTER, E. H. (eds.), *Witchcraft and Sorcery in East Africa* (London: Routledge and Kegan Paul), pp. vii-viii.
- 1964l A bibliography of writings by E. E. Evans-Pritchard, mimeographed.
- 1965g *Theories of Primitive Religion* (Oxford: Clarendon Press).
- 1965j FORTES, Meyer, and EVANS-PRITCHARD, E. E. Values in African tribal life. In MCEWAN, Peter James Maxwell, and SUTCLIFFE, Robert Baldwin (eds.), *The Study of Africa* (London: Methuen), pp. 55-7 [part of Introduction to 1940i].
- 1967a Foreword. In NEEDHAM, Rodney, *A Bibliography of Arthur Maurice Hocart (1883-1939)* (Oxford: Blackwell), p. 5.
- 1967h Social anthropology and the universities in Great Britain. *Universities Quarterly*, xxi. 167-81.
- 1968 Descent and kinship. In BOHANNAN, Paul, and MIDDLETON, John (eds.), *Kinship and Social Organization* (Garden City, New York: Natural History Press), pp. 151-4 [part of 1951g].
- 1969c The perils of translation. *New Blackfriars*, l. 813-15.
- 1970d Foreword. In HOCART, A. M., *Kings and Councillors* (Chicago: University of Chicago Press), pp. ix-xi.
- 1970e *The Sociology of Comte: an Appreciation* (Manchester: Manchester University Press).
- 1970k Some reflections on mysticism. *Dyn*, i. 99-115.
- 1970m Social anthropology at Oxford. *Man*, NS, v. 704.
- 1971b Recollections and reflections. *New Diffusionist*, i. 37-9.
- 1971d Sources, with particular reference to the southern Sudan. *Cahiers d'études africaines*, xi. 129-79.
- 1971g *The Azande: History and Political Institutions* (Oxford: Clarendon Press).
- 1971l John Millar (1735-1801). *JASO*, ii. 131-3.
- 1971p *Zande Texts*. Part 9. [Oxford: Oxonian Press for All Souls College].
- 1971q The writings of E. E. Evans-Pritchard. In BEIDELMAN, Thomas Owen (ed.), *The Translation of Culture* (London: Tavistock), pp. 417-34.
- 1971r The failure of the comparative method. In GRABURN, Nelson, (ed.), *Readings in Kinship and Social Structure* (New York: Harper and Row), pp. 187-9 [part of 1963d].
- 1972d Food for thought. *New Diffusionist*, ii. 42-4.
- 1972g Adam Ferguson (1723-1816). *JASO*, iii. 14-23.
- 1972h Henry Home, Lord Kames (1696-1782). *JASO*, iii. 77-80.
- 1972o Condorcet (1743-94). *JASO*, iii. 105-9.
- 1972r Zande string figures. *Folklore*, lxxxiii. 225-39.
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