RAYMOND FIRTH *Hugh Firth*

Raymond William Firth
1901–2002

RAYMOND FIRTH drew his first breath in Auckland on 25 March 1901, and his last in London, on 22 February 2002, nearly 101 years later. He became an anthropologist, working chiefly in the Pacific, Malaysia and London, in the fields of economics, religion and kinship. He held permanent teaching posts at Sydney (1930–2) and at the London School of Economics (1932–40, 1944–68). During the Second World War he served in Naval Intelligence; he became secretary of the Colonial Social Science Research Council in 1944–5, and was a founding member of the Association of Social Anthropologists of the United Kingdom and Commonwealth in 1946. He was elected to the British Academy in 1949 as an economist, later helping to create the Social Anthropology section. He was elected corresponding or foreign member of the American Academy of Arts and Sciences, of the American Philosophical Society, of the Royal Society of New Zealand, and of New South Wales, and of the Royal Danish Academy of Sciences and Letters. In retirement he held eight visiting posts at American and Pacific universities. He was knighted in 1973 and received Honorary Doctorates from Oslo, Michigan, East Anglia, Australian National University, Chicago, British Columbia, Exeter, Auckland, Cracow and London. He was appointed Companion of the New Zealand Order of Merit in 2001. In 2002 he was awarded the Leverhulme Centenary Medal of the British Academy but did not live to receive it in person.

Firth was a patient and generous teacher whose many graduate students remained loyal throughout their lives; he was an able and

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purposeful administrator of great integrity: no one alive can remember him doing a mean or malicious or self-interested act. In anthropology he was resolutely humane and empirical: his aim was always to convey the variety and complexity of people's experience, and to show how his theory was based on that understanding. He had many academic friendships outside anthropology, was well-read in several disciplines, and was affable and generous with his learning. Without being puritan, in later life he took his pleasures somewhat austerely, with a preference for romanesque architecture, for example, and early music. He married Rosemary Upcott, daughter of a distinguished official in the Treasury, in 1936. Their son Hugh was born in 1946.

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Firth's father Wesley was a Methodist by religion and a builder by trade. His children (Raymond; Gretta, 1905–7; Cedric, 1908–94) were born in Remuera, now part of Auckland City. The family moved to Mauku in 1910 and Firth attended primary school three miles away: he walked there barefoot (boots reserved for Sundays) until his father gave him a horse for transport in 1911. In 1914 he became the first pupil from that school to win a Junior National Scholarship and in 1915 moved to Auckland Grammar School, staying during term with friends of his parents. His further schooling was supported by a Senior National Scholarship, he went on to Auckland University College. In his notes on his early life Firth was at pains to remark that he never got a first prize, and that he came low down in the order for his university scholarship. He attended full-time, unusually, because his father 'believed firmly in unfettered education'. Whatever he thought at the time, Firth (in his seventies or eighties when he wrote the notes) seems to say that he became a scholar and researcher of such eminence against all odds, not merely social and geographical ones. He records only one triumph at this stage: in 1919 his economics paper was marked at 35 per cent and he scraped through, while the examiner failed nearly all the other candidates. The examiner was J. M. Keynes, and the bare pass felt like a distinction.

If his academic performance fell short of brilliance it was not from lack of energy and curiosity. In his school days Firth had travelled widely in New Zealand (school trips on SS *Clansman*) and was captivated by landscape and people. In 1920 he took up geology and began to learn Maori, taking conversation lessons from the Auckland court interpreter. His MA thesis (economics and history) was on the kauri gum industry.

The fossilised resin of the ancient forests of *Agathis australis* had been used for a variety of purposes by the Maori; it became a commodity after European settlement, exported as an ingredient in varnishes until superseded by synthetics in the 1920s. To do this work Firth visited the kauri forests and interviewed the gum-diggers. It got him a First Class, it was published, and was the basis of his first article, published in the *New Zealand Journal of Science and Technology*.

In 1923 he began to teach at Auckland Grammar School and at a Methodist Sunday school while reading for the University Diploma in Social Science. He was drawn more and more to the study of the Maori, and gave early papers to the Anthropology and Maori Race section of the Auckland Institute. His horizons were expanding: he could have become a schoolteacher–scholar, a Methodist figure-head, a pillar of New Zealand society. Instead he began to prepare himself for a wider world. He broke his early engagement to a local woman, daughter of friends of his parents; his father arranged for him to take lessons to diminish his Kiwi accent, and in 1924 he went to study in England: still no scholarship, but a Free Passage awarded by the Senate of Auckland University, and an allowance of £250 p.a. from his generous and committed father. He arrived in London in September and began reading for a thesis on the economies of Polynesia. This (as with many graduate students) was gradually transformed. In fact, on his first day he failed to meet his designated economics supervisor, and having told the School Secretary (Jessie Mair, future wife of Beveridge, mother of Lucy Mair) that he was also interested in a secondary way in anthropology, she sent him to Seligman's office, and Seligman introduced him to Malinowski. In the course of 1925 he narrowed the field to Maori, and under the influence of Seligman and Malinowski and his fellow graduate students, changed to 'a more anthropological approach'. In 1926 the Rockefeller Research Fund committee (administering funds entrusted to LSE) made an award to Malinowski to employ Firth as a research assistant at £2 10*s*. a week, for twenty hours' work. He got his Ph.D. in 1927, and planned fieldwork of an anthropological kind.

He arrived in Sydney in November of that year, meeting Radcliffe-Brown and Hogbin, and decided on Tikopia as his research base. It took him two months to arrive there, and he stayed for fifty weeks. On his return to Australia he was appointed Lecturer in the University of Sydney and remained there for two years. They were the 'golden years' of his early life: congenial company, an active and provocative intellectual life in the university and amongst Sydney's advanced thinkers. In

particular he liked and admired Radcliffe-Brown for his pioneering analytical work on Australian Aborigines' kinship systems, and for his glamorous slightly avant-garde social persona. He also realised that Radcliffe-Brown thought more clearly and reasoned more cogently than Malinowski. When Radcliffe-Brown left Sydney for Chicago in 1931, Firth remained as Acting Professor, Acting Chairman of the Department and (the beginning of a long association) Acting Editor of *Oceania*. In 1932 Malinowski offered him a lecturership at the London School of Economics. Firth left Sydney and arrived at LSE in January 1933. In London he led the life of a busy and energetic young academic. He taught, he published, he gave lectures to outside bodies such as the Workers' Educational Association. On Saturday afternoons he played badminton with Beveridge (Director of LSE), Hayek and others, followed by tea at the Waldorf. Or he week-ended, visiting Lucy Mair's family (she was by then a lecturer at LSE) which unconventionally often included Beveridge. He went on holiday in Wales with Evans-Pritchard; on walking tours in the Cotswolds with Michael Postan the economic historian; he spent part of the summers walking in the Dolomites and touring continental Europe, he and others from the Department using a pension near Malinowski's house in Oberbozen as their base.

Malinowski died in 1942. He had been a dominating influence, in work and play, on the young Firth—as teacher, host, employer, patron, colleague. Malinowski could be domineering; he could make appallingly unfunny jokes; he never understood economics—his reading in the sociology of economic life led him to a reactive development of idiosyncratic concepts that lacked the clarity necessary to interest economists in his work. Firth respected the ethnographer for the rest of his life (so far as completeness is concerned the Tikopia corpus is a response to the challenge of Malinowski's works on the Trobriands); his practice as a teacher in seminars seems to have evolved from Malinowski's graduate seminars, although Firth was more concerned perhaps to give each member an opportunity to contribute, his own role was therefore less dominant and less volatile. Firth also maintained the international character of the graduate body at LSE: they welcomed students from all parts of the world, most of whom returned to become prominent in their own countries. In a speech on indigenous anthropologists in 2001, Firth was able to cite the work of eight from Polynesia alone; and of those eight at least half had been students at LSE during Firth's or Malinowski's reign.

He met Rosemary Upcott in 1935. Busy, sociable, polemical, to some

extent in the public eye, their engagement was noted in the *Evening Standard* (6 Jan. 1936) 'Dr Raymond Firth, lecturer in social anthropology at the London School of Economics, who has often expressed pronounced views on marriage and divorce, is engaged . . .'. Before their marriage Rosemary had enjoyed an *amitié* with Edmund Leach, then a young engineer. He had gone to work in China for four years, and had developed an interest in anthropology. On his return Rosemary introduced him to Raymond and to LSE where he became a graduate student in 1937. That became a four-sided friendship when Leach married Celia Buckmaster in 1940, and it was of such strength it survived the many intellectual and academic provocations made mostly by Leach in later years.

In 1939 (having failed to get funding for research in China) the Firths learned Malay and arrived in Kelantan via Penang in August. They stayed in the fishing village of Pernpok until the fall of France, when they returned to England via Australia, New Zealand, Panama and Halifax. Crossing the Atlantic their convoy was attacked by the German pocket battleship the *Admiral Scheer*. The sole escort vessel *Jervis Bay*, an armed merchant cruiser, conducted a heroic diversionary action and was sunk with the loss of 190 men. But during the battle, as night fell, most of the rest of the convoy managed to escape; and, although the *Rangitiki* had been reported lost with all hands, they arrived eventually at Milford Haven.

Firth moved at first to Cambridge, where LSE had removed to safety, but in early 1941 he joined the Admiralty's Naval Intelligence division, producing the Handbooks for the Pacific Islands. He also went to the United States in 1942, to assist in the American effort to map and describe the new theatre of war. At this time he prepared *Malay Fishermen, their Peasant Economy* for publication. He turned to London and to Naval Intelligence at Chatham House in 1943, maintaining some academic activity. In June 1944 he was appointed secretary to the new Colonial Social Science Research Council (CSSRC), set up following the Hailey Report of 1938 to provide an empirical basis of knowledge for colonial development after the war. He was employed half-time by the Colonial Office and half-time by the Admiralty.

Later that year he was offered the chair of Anthropology at LSE, in succession to Malinowski, and accepted pending the end of the war. He returned full-time to LSE in December 1945, joining Audrey Richards as a member (rather than functionary) of the CSSRC. At this time Evans-Pritchard was elected to the Chair of Social Anthropology at Oxford. Firth was on the Board, which at one stage offered the position to him.

Firth declined: he had just been appointed to LSE; he believed that Evans-Pritchard was the most suitable candidate.

In 1947 Firth was invited to advise the Australian National University on the creation of a Research School in Pacific Studies, and in March 1948 visited Canberra with Sir Keith Hancock and J. F. Foster as well as the scientists Sir Howard Florey and Sir Marcus Oliphant. The discussions continued into 1949, and in 1950 Firth visited the USA to discover the range of Pacific studies there. He visited ANU again in 1951, was acting Director of the Research School of Pacific Studies, and was invited to become permanent director. He agonised; and after consultation with Rosemary (who had said she would do what he wanted), eventually accepted. Rosemary was then very upset, so Firth changed his mind and declined the appointment. He was able nonetheless to make a field trip to Tikopia (with James Spillius) in March 1952. He was invalided out in September, suffering from acute pneumonia, but was able to visit his family in New Zealand after a convalescence in Australia. In effect the Canberra offer was Firth's last serious temptation to leave LSE, although he was offered a chair at Harvard in the following year. He had become Fellow of the British Academy in 1949, and was President of the Royal Anthropological Institute in 1953–4: he was immersed in teaching and administering in London, making relatively short trips to seminars or to lecture for at most a month or so at (among others) UNESCO, the universities of Chicago and New York, and at Burg Wartenstein. He made a short study of kinship in east London in the early fifties. A six months' fellowship at Palo Alto in 1959 was of particular importance to him: he was able to have his family with him for part of the time; he became more deeply understanding of American anthropology, and made enduring friendships with the social scientists who were also in fellowship. But London became the permanent base for his work. He and Rosemary led a busy life outside the School, going to the theatre, opera and concerts. For instance, in March 1961 they went to plays on six evenings: Shakespeare, Shaw, Marlowe, Fry, Sophocles; he recorded no frivolous entertainment in his diaries.

In his last years at LSE he was able to visit Auckland again, to see his father (who died in 1977, aged 104), and to make a short field-trip to Kelantan (1963), visiting Singapore, Auckland, Montreal and Ann Arbor (1967). After his retirement he made more extensive visits. He was visiting professor at Hawaii for the academic year 1968–9: Alice Dewey, the head of department, had been his student. He spent periods of four to six months as visiting professor at British Columbia (Cyril Belshaw was his

former student there), at Cornell, City University of New York, ANU and University of California, Davis. These were all appointments with serious teaching obligations: in each of these places he was welcomed and fêted, but made a point of showing that he was an active and up-to-date anthropologist: not a relaxed panjandrum collecting his laurels, but a serious contributor to the work of his hosts and their students, giving good measure for the honour they did him.

The most striking example of this was in 2001 at the party held in the New Zealand High Commission in London to celebrate his one hundredth birthday. He received the Polynesian Society's Nayacakalou Medal, named for a Fijian anthropologist and politician who had at one time been Firth's doctoral student. Firth, thanking the Society's representative, said that he understood that previous recipients had given a lecture in return. He did not intend to give a lecture, but if he were to, his title would be 'The Creative Contribution of Indigenous People to Their Ethnography'. He expressed doubts about the claim that indigenous people could have an anthropology that was the product of their own culture (an 'indigenous epistemology'): 'I am firmly convinced that the routes to knowledge are not exclusive, but universally shared.' Ethnography was a different matter. Not only had all anthropologists always been indebted to local experts in all social matters, but some anthropologists were natives of the societies they studied: he cited eight of special interest to members of the Polynesian Society, pointing out that they had sometimes perhaps controversially used their insights to 'attempt to redress asymmetry in the current society'. But 'for me ethnography and social anthropology in general as they have developed have been the creation of both alien Western and indigenous contributors' (*Journal of the Polynesian Society*, September 2001, 241–5). Even from a younger person it would have been a remarkable performance: knowledgeable about the personnel, sharp on the issues, restrained, good-humoured—and touched with pride that so many of the people concerned had been his pupils, or Malinowski's.

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Firth visited Tikopia for nearly twelve months in 1927 and again for a few months in 1952, with James Spillius. He made a further one month visit in 1966. The island is quite small (about 3 sq. miles) and then had a population of 1,278 (1,750 in 1952). His main publications derived from this fieldwork were *We the Tikopia*, mainly on kinship and social organisation

(1936, and various subsequent editions); *Primitive Polynesian Economy* (1939, 2nd edn. 1967); *The Work of the Gods in Tikopia* (1940, 2nd edn. 1967); *Tikopia Ritual and Belief* (1967); *Rank and Religion in Tikopia* (1970), and *History and Traditions of Tikopia* (1970). His visit with Spillius resulted in *Social Change in Tikopia*: *a restudy after a generation* (1959), which included an important account of Tikopia responses to natural disaster. They had suffered hurricanes in January 1952, and consequent famine. Firth showed that Tikopia maintained neighbourly and ceremonial exchanges—in extremis at a token level—above consumption: civility, in short, is more important than naked self-interest, a counter-example to set against fictional (*Lord of the Flies*) and ethnographic (*The Ik*) suggestions that civilisation is a shallow veneer over 'savagery'. His more specialised publications, with co-authors, include *Tikopia String Figures* (1970, with Honor Maude), *Tikopia Songs* (1990, with Mervyn Maclean, including a tape cassette), and *A Tikopia–English Dictionary* (1985, with Ishmael Tuki and Pa Rangiaco). He was especially proud of this last volume, which attested his command of the language, used an extensive system of cross-referencing that indicates the semantic range of words and encapsulated connections amongst Tikopia concepts and institutions. These books and monographs stand alongside innumerable articles and notes and published letters of which the last were 'Tikopia dreams: personal images of social reality' (2001) and 'Linguistic and social patterns of separation and reunion' (posthumously, in 2003).

This may seem to be a rather extensive publication on a rather small number of people, and Firth was aware that his work was cited and sometimes criticised by colleagues whose command of the corpus was scarcely complete. His reply was succinct: we should not imagine that a thousand people living in a territory one eightieth the size of Rutlandshire had less life, less activity, less work to do and fewer dilemmas than smart but callow graduate students lounging around in the LSE canteen. In fact the seven main volumes are vivid, fresh and are not repetitive (Firth marked items in his field notebooks to show that he had used them in published work). He was especially concerned to show that Tikopia lived complex lives, faced moral and political dilemmas, wondered what to do in changing circumstances, and did not always do the same tasks or fulfil their obligations in standard repetitive ways. That concern was not compatible with a terse mode of writing, and his style of anthropology (sharply distinct from that of many of his contemporaries) seemed to demand expansiveness.

Firth's work in Kelantan (fieldwork 1939–40; *Malay Fisherman: their Peasant Economy*, 1946; 2nd edn. rev. and enlarged, 1961; reissue 1998) is not so extensive: his wife Rosemary undertook the research on domestic organisation and kinship matters (published as *Malay Housekeeping*, 1943; 2nd edn. 1963) and war curtailed fieldwork. But it was a detailed account of economic activity which expanded his range: Malay fishermen were dependent on markets and market operators, had relations of debt and credit. They had a peasant rather than a primitive or modern economy, and Firth knew that his analysis of the bargaining between fishermen and their merchants was pioneering. His third main area of ethnographic inquiry was in London. In the 1950s he made a study in the east end of London which was well received: his short book *Two studies of kinship in London* (1956) was an inspiration to Wilmot and Young who founded the Institute of Community Studies, and developed Firth's ideas, not always on lines of which he approved. Further work on kinship in north London resulted in a monograph with Anthony Forge and Jane Hubert, as well as useful papers on research methods describing how they had conducted these innovative inquiries.

Firth also published work in economic anthropology more generally. *Primitive Economics of the New Zealand Maori* (1929) was his Ph.D. Thesis, written before he went to Tikopia. He edited *Capital, Saving and Credit* (1964, with Basil Yamey) and *Themes in Economic Anthropology* (1967). He was always attached to the categories of Western economists, and used 'labour', 'capital', 'property' freely in his analyses of non-Western economies. He never argued (as for example Polanyi and Sahlins did) that different kinds of economy were based on different principles and needed different kinds of economics. He was at pains to describe how concepts of proven worth such as 'property' might vary from place to place and from time to time. That in turn might lead economists to a more nuanced understanding of their matter. In his last years he was much concerned that economists had begun to write about culture, and that some anthropologist should comment sharply on their works.

In 1972 Firth gave the inaugural Radcliffe-Brown lecture 'The Sceptical Anthropologist? Social Anthropology and Marxist views on society' (*Proc. Brit. Acad.*, 58) in which he argued that Marx's views on primitive society and economy were 'amorphous'. Engels and Lafargue, he said, represented an 'out-dated arid evolutionary position'. Marx's account of pre-capitalist economic formations, crucially under-informed, was no use at all as an account of primitive, Asiatic or communalist societies; but even myths might be useful, though perhaps only as points of

contestation or departure. He was politely scornful of the mainly French anthropologists who argued that 'kinship . . . serves as both infrastructure and superstructure': the point was not to find equivalents of Marx's fundamental categories in this or that primitive society, but to ask Marxist questions about real people doing real things. In particular, Firth thought that anthropologists' attention might be drawn to aspects of non-western society that their traditional training had hitherto led them to ignore. For instance, it was no harm to explore occasions of conflict and contradiction rather than to be set on discovering cohesion and harmony. In the sometimes bitter arguments about the role of social anthropology in imperial and colonial domination he took the line that British anthropologists had (within the limits of their training and avocation) been as impartial and balanced as it was reasonable to expect: they were not colonial officers, but more like the factory and health inspectors whose reports on conditions in nineteenth-century England had informed the work of both Marx and Engels, and whose competence, accuracy and freedom from bias were essential to socialist or any other kind of analysis.

Firth's lecture showed great learning not only in the canons of Marxist literature, but in the fragmented and often tiresome writings of the *groupuscules marxisants* of the 1960s. But he remained Firthian: he was interested for example in the attempts, by Salisbury among others, to measure objective labour value in non-monetary economies, and to compare the anthropologists' assessments (based on time spent) with 'the natives'. He was interested in what could be tested, and in what that might contribute to our understanding of human action. His combination of learning with empiricism brought to bear on manageable conceptual issues was typical of his work. It was as if he thought that an attack on high theoretical systems was futile: it was more sensible to put major or all-encompassing intellectual constructs to one side, and to examine the bricks to understand their strength and usefulness; so, not Marxism–Leninism, but labour-value and the actual forms of production in Asia.

His account of property rights in Tikopia is classic. 'Enquiries as to land ownership in Tikopia elicit a description in one of four different ways', indicating a series of overlapping and reversionary rights. Ownership in any of its four versions was not exclusive: people might borrow land, especially but not exclusively for seasonal crops, with only retrospective token acknowledgement of an 'owner's' rights. The topic is introduced in *We the Tikopia* and discussed rigorously in *Primitive Polynesian Economy*—in the chapter firmly and provocatively entitled 'Property and

Capital in Production'. He did not look for analogues of the components of western economic systems: he took the concepts, and showed that they were complex, flexible, and more varied than western economists believed.

Another example of Firth's determined occupation of the middle range is his remarks on Marcel Mauss's *The Gift*. Mauss had proposed three obligations: to give, to receive, to make a return. Firth denied that they were in fact 'universally mandatory'. Empirical investigation showed that each of the three contained 'significant areas of choice and uncertainty', and showed too that people did not in fact always meet their obligations. Mauss, concerned to establish why people everywhere sought to make a return for gifts received, had proposed that all gifts partake of an archetypical gift which he thought he had discovered as a survival in Polynesia. They spoke of *hau*, the 'spirit of the thing given', which sought always to return to its origin and which made recipients of gifts uncomfortable if this need of *hau* was unrequited. Most sceptics might take a radical line, arguing against the possibility that a universal phenomenon could be explained by Polynesian ethnography. Firth, however, showed that *hau* did not mean what Mauss claimed it did, and that it was much more limited in effect than Mauss had thought. If you wanted to explain the need to return a gift you had to look at the sanctions others could apply to a defaulter—loss of status, loss of future gifts, loss of ritual or religious standing. Firth seems never to have had a root-and-branch instinct, but undermined the grandiose propositions of his predecessors and contemporaries with empirical reasoning.

A final example, from Firth's explorations in religious and conceptual anthropology, is in his article 'Twins, Birds and Vegetables' (*Man*, NS I i. 1966). Lienhardt had reported that Dinka occasionally said that some men were lions, and appeared to mean that they were essentially lions who took human form. Similarly, Evans-Pritchard had written about Nuer that they said 'twins are birds'. The question was what they meant or thought: Evans-Pritchard maintained that the identification was not metaphorical, but was part of a 'complex analogical representation which requires to be explained in more general terms of Nuer religious thought'. Firth proposed that in each case the 'are' implied an identification of men and lions, twins and birds, and that the evidence in neither man's work was sufficient to explore the possible meanings. He was constructive: Tikopia evidence (reviewed at some length and with acute precision) suggested that there were at least three ways in which people might identify themselves or others, or things, with spiritual beings, and he proposed

that lions were one kind, twins another. If you examined the psychological and social patterns to distinguish the kinds of identification, you might then suggest what consequences that might have for general analysis of, for example, totemism. Evans-Pritchard replied perhaps rather evasively in the correspondence pages of *Man*: if Firth had shown him the article in advance he would have been able to discuss Nuer ideas in detail, and to make suggestions about Firth's reading of his work; as matters stood he could only correct some matters of fact. Firth's reply was 'that in dealing with such a delicate and difficult matter as description of belief we need as much evidence as possible, both of what the people concerned say and of what they have been observed to do. When the people themselves do not state their beliefs in direct terms, the indirect evidence needs to be even more carefully marshalled, with the investigator's generalisations supported by concrete data.'

Firth, by inclination and ability, worked always with middle-range ideas. When many of his contemporaries spoke of structure or structuralism, he spoke of organisation. His friends at the time saw him as bridging the alleged abyss between Radcliffe-Brown's structuralism and Malinowski's functionalism, but in later life Firth denied this: he had not sought to mediate. He had achieved a distinct position which arose he said from his training in economics and from his experience of Tikopia. Structures may very well exist, but they are inaccessible to observation. What could be seen and conveyed to others were the week-to-week or year-to-year arrangements that men and women made to meet obligations and to satisfy social and material needs. Social organisation required coordination and agreement; it depended on imprecise rules ('room for manœuvre' as his colleague Lucy Mair called it), and it required time. Social organisation is the 'systematic ordering of social relations by acts of choice and decision'. Such acts followed on from others; situations differed one from another, were never exactly replicated, and the choices and decisions were not always the same—they might be, for instance, cautious or tentative solutions to dilemmas. Firth thought that structure was the outcome of repeated acts of organisation: it consisted of precedents, each of them an approximation to some set of expectations about how people should do things properly. It was not a permanent and constraining univocal controller, but set conventional more or less fragile limits to the range of things people might choose from, and was itself affected by the organisational choices made each and every day, week, year. Firth diminished structure: it was a distillate of past practice, was never precise enough to

eliminate the need for choice and could hardly be permanent or positively constraining. You might think Firth could have abandoned 'structure' altogether: 'organisation' is sufficiently explanatory. But that would have required him to make a full-scale assault on an item of high and imprecise theory: that was not his style, and he was content to elaborate the intermediate range. Firth laid the foundations of this pattern or habit of thought in the 1920s, and although he developed and expanded it in successive works, he maintained it against persuasive fashion for seventy years.

Firth characteristically used the past tense in his ethnography:

> . . . at the time of an incision ceremony in Rofaea, Pa Niukapu made a double journey to Matafana and back after dark in pouring rain to see how his children were. He knew they were sleeping with their grandmother, in no discomfort, but he wished to be assured of their well being. As he was a mother's brother of one of the initiates he had to return again to Rofaea to sleep. (Firth, *We the Tikopia*, 1936)

It is vivid, located in time and space and weather, conveying the contingency of action and the intersection of motives and proscription. By describing several rituals, boat-building parties, feasts—and indeed, how Tikopia coped in the aftermath of a hurricane—he was able to build up a picture of what was distinctive in economy, politics, kinship, religion; to suggest its fluidity and adaptability. If you compare the account Evans-Pritchard gives of a similar sort of event, the differences are marked and clear:

> Each village acts independently in arranging for its boys to be initiated. After the operation the boys live in partial seclusion and are subject to various taboos. . . . Only age-mates of the father of the initiate in whose homestead the feasting takes place attend it: others keep at a distance lest they see the nakedness of their kinswomen and mothers-in-law. (Evans-Pritchard, *The Nuer*, 1940)

The two passages deal with initiation, but in very different ways. Evans-Pritchard described the general and habitual practice of 'Nuer', in the ethnographic present tense. His actors were villages or age-sets. He wished to insert his account of the ritual practice into an account of a structure of relations among age-mates (which is itself part of a structure of lineages and tribes). And his use of the ethnographic present here and elsewhere allowed him to elide implicitly into an account of what he claimed were enduring principles of social structure. Firth wrote in the past tense, and was concerned with knowledge, with motives and experience: named people in named places, expressing purpose and doing things within a framework of permission and prohibition.

Evans-Pritchard and Firth had been friends since Firth's arrival at LSE. Firth drank his first glass of wine with Evans-Pritchard (at l'Escargot in Soho in September 1924); they went on holiday together. The first sign of a rift occurred after Firth had supported Evans-Pritchard in the election to the Oxford Chair of Social Anthropology in 1946. As early as 1937 Firth had proposed (in a memorandum to the Colonial Office on 'the utilisation of anthropological services') 'the appointment in each territory of a specific Government Anthropologist'. Post-war, he and Audrey Richards, members of the CSSRC, argued that research to support HM Government's efforts at development in the colonies should be based in universities in the colonies. The researchers would have a secure local base; the universities would acquire multi-disciplinary teams with local members as well as semi-attached expatriates who would be in close touch with local administrators. The model in Richards's mind became the Institute for Social Research at Makerere, of which she was the first Director (1950–6). Firth had made extensive tours in West Africa (July–October 1945) and in Malaya and Singapore (July–October 1947) to review the possibilities, and was wholly supportive.

Evans-Pritchard took another view: the research should be based in Britain. Young researchers would be trained, for example, in 'pure' anthropology in British universities, and would then do fieldwork overseas, returning after a year or two to write up their theses. In 1948 Evans-Pritchard persuaded the newly-created Association of Social Anthropologists (of which he was Chairman, Firth Hon. Secretary) to send a deputation to the Colonial Secretary mandated to argue for the Britain-based scheme. They were well-received, but the government opted for the proposal from Richards and Firth. Firth referred to this as 'a mild contretemps', and he bore no lasting malice. It was, however, the first step towards a deterioration of relations. They were on mismatched good terms: Evans-Pritchard acknowledged Firth's personal qualities and kindnesses, but increasingly mistrusted his anthropology. Firth had been active in securing Evans-Pritchard's election to the British Academy, and Evans-Pritchard wrote 'This, I fancy, could only be your doing, and it is chiefly for that reason I am accepting. This adds to your many acts of generosity, none of which I forget' (13 June 1956). But at the same time he found Firth's anthropology lacking in grand ambition. Moreover, the LSE Department under Firth acquired a character as pragmatic, involved with government, busy in the world in ways which were inimical to pure anthropologists. Evans-Pritchard disapproved, and wrote that Firth 'had chosen mammon'. For his part Firth acknowledged Evans-Pritchard's

intellectual gifts, with reservations. He can hardly have been comfortable with an anthropology that was conceived to reveal 'a structure of relations among relations'. The ideas in Evans-Pritchard's *Witchcraft, Oracles and Magic* were not wholly original—they were 'perhaps more commonly shared than he imagined'; he had not done justice to Margaret Mead; he paid scant regard to the work of his juniors in the discipline. Firth was uncomfortable with Evans-Pritchard's religiosity, and with the personality cult that grew up in the coterie that surrounded him. Oxford anthropologists in the 1950s and 1960s did cultivate the idea that their work was æsthetically and intellectually on a higher plane than anthropology elsewhere. In spite of these faults he regarded Evans-Pritchard as the most brilliant man of their generation, and admired him for it. But Firth seems to have received rather few acts of generosity or good will after the mid-1950s.

Edmund Leach, too, disapproved of the changing nature of the LSE Department. Leach had been a graduate student in 1938–9, and again in 1946–7; thereafter a lecturer. But he left for a post in Cambridge in 1953, expressing his dissatisfaction. It is clear that Leach thrived on controversy, perhaps especially with friends and colleagues in nearest proximity, and that he was an enthusiast for new ideas and schemata. Firth recognised and admired his qualities, but responded always with *distinguos*. To take an example from towards the end of Leach's life: in 1987 at the conference of the Association of Social Anthropologists Leach maintained that all ethnography was 'fiction': human creativity determined the presentation of fieldwork to a scholarly audience, and to pretend that it was in any sense objective was a fundamental mistake. Firth's off-the-cuff response was measured. (The story that Firth began his reply 'You may well speak for yourself but not for other ethnographers' does not correspond to the memory of those present, and is uncharacteristically waspish.) Ethnographers were human, he said, and it was necessarily true that their creativity was involved in their writings. But not all creativities were the same: anthropologists were trained quite differently from novelists or poets. They were obviously influenced by assumptions current in their own societies, indeed he had pointed this out himself in 1969. If you wished to distinguish good ethnographers from bad ones, you did so by reference to the reality they gave an account of. All this he delivered in calm and unprovoked terms, knowing quite well that Leach at any rate for part of the time did indeed think that the true excitement of anthropology lay in spinning fine theories derived as it might be from topology, from structuralism, from

communication theory or, in this case, from relativistic postmodernism. To Leach's perpetual and exhilarating exuberance Firth responded as the bourgeois who refused to be *épaté*. In its own way this was as provoking to Leach as Leach had hoped to be to Firth.

Firth's principled refusal to abandon the middle range exasperated the fireworks men and women of the three decades 1950–80. Marxists, as well as structuralists and postmodernists and the many other more aleatory -ists of the time, all got the treatment: learned, calm, gentle, empirical, coupled with acute caution about highfalutin theory. They thought and said that Firth was atheoretical, and Leach (typically) wrote that Firth's aspiration to write anthropological theory was like a clown's desire to play Hamlet. It is certain that Firth was less flashy than some of his pyrotechnical contemporaries; certain too, that they could not recognise that what Firth did was firmly theoretical (if it didn't flash for them it wasn't theory). His emphasis on organisation, on motives and dilemmas, his proposal that structure was the outcome of continuously renewed organisation were important elements of an established and thoughtful theoretical position. It is at present a matter of speculation whether Firth noted that, twenty years later, many British anthropologists applauded Bourdieu's invention of the concept *habitus*, giving special emphasis to human motives and dilemmas, and proposing that structures grew out of perpetually modified acts of conformity and convention.

Firth was brought up a Methodist, abandoning his ancestral religion sometime in the 1920s, perhaps between his first glass of wine (1924) and his lecture on *The Soul* to the Sydney Free Thought Society in 1932. He told Peter Loizos that fieldwork in Tikopia had changed him profoundly: Tikopia had no Methodist restrictions, and still managed to live relatively orderly lives, and had a sense of morals and made moral judgements.

He became a 'practising humanist' and a member of the Rationalist Press Association. He nevertheless wrote constantly about religion. Evans-Pritchard had been received into the church of Rome in Benghazi in 1944, and came eventually to declare that people without faith could not really understand religion. Firth's response was that losing a faith (as he had done) might be as good a key to understanding as acquiring one (as Evans-Pritchard had). In his three volumes on Tikopia religion, and in his numerous lectures and articles—the nine most significant in Firth's estimation collected in *Religion: a Humanist Interpretation* (1966)—Firth asserted that religious activity, concerned with gods, was therefore

concerned ultimately with ineffable ideas. But it also served more mundane purposes: it purported to answer otherwise unanswerable questions, and thus brought psychological comfort and reassurance to believers. Religion often encapsulated moral ideas, regulating conduct. Ritual mobilised economic goods, and a church could be 'a sociological force of great impact'. Both ritual and a church can stimulate artistic creativity of a sublime order as well as violence and oppression. All this was susceptible to analysis by sociologists and social anthropologists. In short, religions were not mysterious: Firth was interested in what religionists in all their variety found mysterious, but thought that this would be discovered and described using plain language, common sympathy and respect, together with scrupulous ethnography. Of course, no understanding was perfect; but in essence understanding religions was no different from understanding economies or polities. He thought both that religions were human creations, made from specific intellectual, experiential, æsthetic resources, and that Durkheim's grand dictum that societies created their gods in their own image was 'oversimplified'.

In this he was true to himself and to his tried methods of analysis. Although he had no faith and thought that the prohibitions of Methodism were tiresome, he retained to some extent the style of his early Christianity-moulded childhood. Then, he said, he had found immense happiness in simple things: a party could be a real party with only tea and lemonade. His tastes in later life were less austere, but he enjoyed himself seriously. And perhaps because of his awareness of his lost faith he treated religious issues rather reverently; certainly his language became more portentous.

Firth was a centenarian. His contemporaries, who witnessed his formative years, predeceased him; we can know little of that time, or of the personal course he ran to become the man we knew. His early married life, with many absences abroad, was perhaps not always easy. But he and Rosemary achieved an intellectual and emotional conjugality 'in argument and agreement' and she was 'the most important personal influence' on his life, for more than sixty years (Rosemary died in 2001). From his writing we can see that his general approach to anthropology was formed fairly early: he was an organisation man from the 1930s, both in his theory and in his administrative activities. He maintained the intellectual position of the empiricist, the theorist of the middle range, firmly, calmly, sensibly for the next seventy years. In administration he was a consistent and fair-minded advocate for anthropology at home and abroad. In the School he built a serious and humanely engaged international

department of great renown. His students were intensely loyal, but he never asked them to become Firthians.

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Fellow of the Academy

Note. In writing this memoir I have been greatly assisted by John Drury, Hugh Firth, Jean La Fontaine, Peter Loizos, David Mills, and David Parkin. They are not responsible for errors, but they have added greatly to its depth and range.

Firth's papers are mainly deposited in the archives of the London School of Economics; a complete bibliography is retained by the School.