Alternative Histories of British Social Anthropology

Adam Kuper

I

There are obvious parallels – as well as interactions – between the beginnings of 'British social anthropology' and the more or less contemporary development of the Vienna circle of psychoanalysis, the launch of Durkheim's *AnneÈ Sociologique*, or the formation of the *Annales* school of social historians led by Lucien Febvre and Marc Bloch. In each case a small network of marginal intellectuals, attracted by charismatic leaders, created what amounted to new disciplines in the emergent social sciences. New objects of study, methods of research, and applications were pioneered. The achievements are clear enough, but it is not easy in any of these cases to work out precisely how the trick was done.

Undergraduate lecture courses on the history of British social anthropology conventionally begin with a contrast between Malinowski and the old-style ethnographers, and emphasise the radically innovative way in which he went about his fieldwork in the Trobriand Islands between 1915 and 1918. The great symbolic image, inevitably flashed on the screen, is of Malinowski pitching his tent in the village. We quote his slogan:'The anthropologist must relinquish his comfortable position on the verandah, where he has been accustomed to collect statements from informants'(1926: 147). From that point on, we tell our students, ethnographers had to 'go out into the village', cultivate a garden, join in the dances, exchange gifts, and generally muck in. Above all, the people now had to be studied as they really are rather than as they might once have been.

There is, of course, more than an element of myth-making here. Certainly the Trobriand study became the touchstone for Malinowski's students, but some of them remarked that their teacher did not always follow his own rules in the field. Michael Young has worked out that 'his tent was folded for almost half his time in Kiriwina' (Young, 2004: 502.) And it could be argued that Malinowski's methodological originality lay elsewhere, in his insistence that ethnographers had to provide what he called concrete documentation, such as maps, measurements, multi-coloured synoptic charts, gardening diaries, and texts in the vernacular. 'The main principle of my work in the field,' he wrote in a note to himself in the Trobriand Islands, 'avoid artificial simplifications. To this end, collect as concrete materials as possible: note every informant; work with children, *outsiders, and specialists. Take side lights and opinions.*' (Young, 2004: 560. Cf. Rold·n, 1994).

Equally, Malinowski insisted on introducing theory into the very process of what he called the

construction of facts. After all, like Franz Boas, he had written a doctoral thesis that addressed issues in the philosophy of science. This was an account of Mach's positivism, though he ended up with a more permissive empiricist doctrine, 'nothing without experience'. Working in the Trobriands, he sometimes felt himself 'almost swamped by detail', but his philosophy of science indicated that experience had to be shaped, and that theory must come before description. (Young, 2004: 79-90.) The description of facts required precise concepts that only theory could provide. The ethnographer should build up synthetic facts, informed by theoretical considerations, and tackle strategic problems ('problems, not peoples').

To be sure, an off-the-shelf ethnographer's aide was available, and he had made use of it while writing up the results of a short apprentice field study in Mailu, on mainland New Guinea, organising his material to fit the standard format of the Royal Anthropological Institutes's check-list for fieldworkers, *Notes and Queries in Anthropology.* It was a convenient solution, which facilitated cross-cultural comparisons, but compiling an mechanical inventory of customs and beliefs could not bring out the connections between different activities and institutions. Malinowski's aim in his Trobriand fieldwork was to tease out the various strands – magic, economics, kinship, politics – that were woven together in even the most essential work, like house building, sailing, or gardening.

From our contemporary perspective, Malinowski's fieldwork in Kiriwina is often seen as marking the decisive breach between the generation of Haddon, Rivers and Marett and the new social anthropology, even if the precise nature of his ethnographic innovations might be put in question. However, conventional histories also remark upon a more or less contemporary theoretical shift, for which Radcliffe-Brown is sometimes given equal credit, together with Malinowski. Since the 1860s, ethnographic data had been collected in order to address historical and geographical questions. In the first decade of the 20th century, Durkheim became the key influence on the new anthropologists, displacing Darwin, or Humboldt. The new research questions had to do with the workings of social institutions in the here and now, not with historical reconstructions. But Malinowski and Radcliffe-Brown famously did not agree on sociological theory. Malinowski was always an individualist, Radcliffe-Brown a collectivist. In his *Essay on the Gift*, in 1925, Mauss rewrote the *kula* ethnography in terms of Durkheimian collective sentiments. Malinowski in turn recast reciprocity as a matter of enlightened self-interest in his *Crime and Custom in Savage Society,* in 1926 .

Leaving aside their fundamental disagreements on Durkheimian sociology, however, it is misleading to represent Malinowski or even Radcliffe-Brown as being unequivocally committed to sociology. Rivers had argued in his paper 'Sociology and Psychology' in 1916 that anthropologists could operate either as sociologists or as psychologists. 'Those who follow one path will devote themselves to the study of the

body of customs and institutions which make up social behaviour, while those who follow the other path will inquire into the instincts, sentiments, emotions, ideas, and beliefs of mankind.' (Reprinted in Rivers, 1926: 6.) In a letter to Rivers, Radcliffe-Brown responded that 'the only difference between us is at what stage in the progress of sociology we should take up the fundamental psychological problems. I wish to take them up at once, whereas you wish to postpone them.' (Kuper, 1989: 79.) But what psychology did they have in mind? Rivers was a cognitive psychologist who later became a Freudian. Both Radcliffe-Brown and Malinowski were drawn to the theory of sentiments of the now forgotten English psychologist A. F. Shand, whom Malinowski described as 'one of the greatest psychologists of our time'. (Malinowski, 1927: 240. See Kuper, 1990.)

The histories then move on to the professionalisation of the discipline. The students of Malinowski and Radcliffe-Brown established new departments, debated variants of functionalist theory, and wrote the classic monographs of the 1930s and 1940s. Finally, the narratives close with the change of the guard in the 1970s, when Malinowski's students retired. The students of his students came into what is generally admitted to have become a rather diminished inheritance. The theoretical paradigm established in the 1920s and 1930s finally imploded, although Malinowski's methodology – or some selective and perhaps mythical version of this method – remained the defining procedure of field research. But awkward questions remained about this process of institutionalisation and professionalisation, none more awkward than those that have to do with the imperial dimension. Was it simply coincidence that the crisis of British social anthropology occurred just as Britain wound up its African empire?

II

If this is the conventional practitioners' account, there are, to be sure, a growing number of professional histories, that take a longer perspective and draw on archival records and situate the anthropologists in a broader intellectual context. (See, e.g., Kuklick, 1991 and Stocking, 1995.) I will say little about them here, for two reasons. First, they have filled out but not radically challenged the conventional folk history. Second, their judgments, implicit or explicit, tend to assume the superiority of the American culturalist perspective. Stocking, for instance, discussing the classical phase of British functionalism, writes that 'there are many today (especially, perhaps, in the United States) for whom the real problem for historical understanding would seem now to be: how could so many intelligent anthropologists have been so long infected by such a sterile and/or derivative viewpoint'. While fastidiously distancing himself from these crude characterisations, he suggests that the root cause of the aberration is to be found in British national

character. (Stocking, 1984: 181-182.) Perhaps for these reasons, perhaps because of a lack of historical curiosity, the professional historians have had less impact than might have been expected on the self-image of European social anthropologists.

But there are also alternative, critical histories of one kind or another, which have proved more disturbing to practitioners. Some are mainly oral accounts, which dwell on personalities. One published version is a paper by Edmund Leach, provocatively entitled 'Glimpses of the unmentionable in the history of British social anthropology'. Here Leach aimed to uncover the family secret: 'differences of social class played a critical role in what happened in British anthropology during the first 40 years of the this [the 20th] century'. (Leach, 1984: 2.) He observed that the early 20th century pioneers of British social anthropology – Rivers and Haddon – were not really gentlemen. For that simple reason they had failed to establish the discipline in Cambridge, then the headquarters of British science. When Malinowski came on the scene it was obvious that he was not an English gentleman, and so not a candidate for Oxbridge. He found a niche in the socialist and marginal London School of Economics, which Leach described as being 'a very low status institution' in the 1920s and 1930s. (Leach, 1984 11.) And his students were largely foreigners or women.

One might note in support of Leach that although the public-school and Oxford man Evans-Pritchard attended Malinowski's early seminars, he was supervised by Seligman, regarded attendance at the LSE as a shameful come-down, and detested Malinowski, who in turn informed Seligman that he would resign if Evans-Pritchard was appointed to a position at the School. (Goody, 1995: 23.) It is equally salient that Malinowski expressed disproportionate delight when Godfrey Wilson, son of the famous Shakespeare scholar Dover Wilson, became his first loyal male acolyte from the English intellectual aristocracy. On the other hand, it is worth recalling Westermarck's enthusiastic recollection that the LSE student body was 'the most international' and 'the most varied in colour of any university in the world'. (Young, 2004: 169.)

Leach remarked that in due course the LSE grew in prestige, although 'as part of its efforts to achieve respectability (which were ultimately very successful), the politics of the place were steadily moving to the right'. Similarly, and like so many *arrivistes* before them in British history, Malinowski's rag-tag army also became respectable, and also moved to the right. As Leach put it:

> With varying degrees of enthusiasm and varying degrees of success, Malinowski, Firth, Schapera, Fortes, Nadel, and the other 'foreigners' who were mainly responsible for the high prestige that was attributed to 'British' social anthropology in the 1950s and 1960s … eventually assimilated themselves into the life style and cultural conventions of Oxbridge academics, but they remained

> 'outsiders' with a highly ambivalent attitude towards the values of their adopted academic milieu. This ambivalence is both reflected in and a reflection of their approach to the study of anthropology. (Leach, 1984: 11.)

This is the crux, in Leach's view. He argued that the uncomfortable situation of the outsiders recruited by Malinowski explained their theoretical orientation. Yearning for security, they were seduced by the Oxbridge ideal of a stable British hierarchical society. Durkheim apparently believed that everything worked very nicely in properly integrated societies, where people shared the same values. So the outsiders became orthodox Durkheimians. In contrast, upper-middle class British recruits to anthropology (including E.E. Evans-Pritchard, Gregory Bateson, Camilla Wedgwood, Audrey Richards, Lucy Mair, and Leach himself) were rebels against their class. Leach speculated that they had been attracted to ethnographic research because they 'were trying to get away from a homeland which they found archaic'. (Leach, 1984: 12.) Sceptical insiders, they did not believe in any grand theories of social cohesion. They took it for granted that every establishment was out to manipulate everyone else.

An historian, Henrika Kuklick, has echoed Leach's class analysis, though without citing him (Kuklick,1991: 72-3), but these speculations, typical of the gossip about their elders and betters that anthropologists swap in pubs, after their seminars, with a mixture of reverence and *schadenfreude*, fall apart immediately they are scrutinised with any care. Were Audrey Richards and Lucy Mair insiders, because of their social class, or outsiders because they were women in the academic world of the 1930s and 1940s? Did Radcliffe-Brown's social insecurity (if indeed he did feel insecure) explain his attraction towards Kropotkin's anarchism, or rather towards Durkheim's corporate socialism? Leach's propotypical insider, Evans-Pritchard, was an orthodox Durheimian before World War II. Schapera, one of Leach's outsiders, was always sceptical of social theory. And if the analysis were true, it would be hard to understand how Leach himself became a great fan of structuralism in the 1960s. (As so often, Leach may have been generalising from his immediate situation in the small Cambridge department of social anthropology, where he and the increasingly conservative Meyer Fortes were embattled rivals in the 1960s and early 1970s.)

III

A rival alternative history emerged in the 1970s, taking a lead from Franz Fanon and Edward Said. Social anthropology was a colonial science, a form of Orientalism, 'a kind of Western projection onto and will to govern over the Orient' (Said, 1978: 95). This proposition is now taken for granted by many students (and

quite a few American anthropologists). However, a number of authors have pointed to the anti-colonialism of certain leading anthropologists, the contemptuous rejection of anthropological expertise by some colonial governors and high officials. Several of the South African anthropologists who entered British anthropology brought with them a thorough-going critique of colonialism. (Kuper, 1999(b).) A more persuasive objection is that British anthropologists often endorsed the mandarin preference for pure science, uncontaminated by policy concerns, a view expressed in particular by Evans-Pritchard and Leach but preached in the 1950s even by Fortes and Gluckman, who had been among the partisans of applied research in the 1930s and 1940s.

By and large, recent historians have resisted the designation of social anthropology as a colonial science. (See, e.g., Goody, 1995.) George Stocking's very lengthy history of modern British social anthropology, *After Tylor* (1995), devotes only part of one chapter to the colonial context, amounting to only some 20 pages in a work running to 570 pages in all. Few reviewers of Stocking's book seem to have found this at all odd. But clearly this refusal to engage with the colonial context will not do either. As BenoÓt de L'Estoile writes, 'What needs to be addressed … is precisely what is meant by anthropological knowledge being a "colonial science". We need to understand the specific historical configuration in which some discourses and practices could be held as "scientific", while at the same time unambiguously belonging to the colonial world.' (De l'Estoile, 2004 (a): 343. See also Pels and Salemink, 1994.) In his thesis, *L'Afrique comme laboratoire: ExpÈriences rÈformatrices et rÈvolution anthropologique dans l'empire colonial britannique. (1920-1950)* (De L'Estoile, 2004 (b)), De L'Estoile protests against the juxtoposition of an independent science of anthropology with another supposedly distinct entity termed colonialism. He also criticises the conventional opposition between pure science and compromised and compromising applied studies. Rather he argues that beginning in the late 1920s, British social anthropology was effectively reconstituted. This happened by way of social and intellectual exchanges between academic anthropologists, other intellectuals, missionaries, and colonial policy-makers. And de L'Estoile describes a second and more fundamental Malinowskian revolution that occurred in the 1930s, as Malinowski drew his students into a dialogue with the leading policy-makers who were concerned with Britain's African empire.

This story begins with the formation of the International Institute of African Languages and Cultures in 1926. Initially dominated by missionary societies, the IIALC concerned itself at first particularly with linguistics, which was crucial to bible translation, and to education. However, its moving spirits recast its programme as they appealed for funds. The key actor here was J. H. Oldham, secretary of the International Missionary Council, who became administrative director of the IIALC in 1931 and worked closely with

Malinowski. Oldham's plan was to establish the Institute as a privileged interlocutor of the colonial office. The main preoccupation of the colonial office was the government of Britain's African colonies. And policy-makers were casting about for social scientists who might help them to put flesh on the bones of Lugard's proposals for a system of indirect rule in Africa. The IIALC and Malinowski seized this opportunity.

Drawing on letters, minutes of meetings, and often unpublished internal reports, de L'Estoile traces the social networks that drew anthropologists, missionaries and policy-makers together – the week-end parties at Lugard's country house, the committees and meetings of the IIALC, Malinowski's LSE seminars. In these crucial years, Marjory Perham, Sally Chilver, Lucy Mair and Audrey Richards served as key intermediaries between Malinowski and the colonial grandees. (A good thesis could be written on the special role played by intellectual women in colonial affairs.)

As de L'Estoile shows, Malinowski displayed a remarkable tactical flair, which has never before been so clearly described and analysed. His broad strategy was to forge a new space for a science of African social policy centred upon the anthropology department of the London School of Economics. In the process, he refashioned the functionalist anthropology that he had propagated in the 1920s. He now conceded that his own work on the Trobriand Islands represented an inadequate model for the kind of ethnography that the circumstances of the African empire required. *Argonauts of the Western Pacific*, published in 1922, opened with a lament for the 'cruel irony that just as the importance of the facts and conclusions of ethnological research is … becoming recognised, … the material of our science is vanishing.' In a paper published in 1929 in *Africa* entitled 'Practical anthropology', Malinowski demanded an 'anthropology of the changing native'. (Malinowski 1929.)

He had his reward. With Oldham's crucial support, Malinowski was able to tap the funds of the Laura Spelman Rockefeller Memorial. An immediate consequence was a shift in ethnographic focus. The pioneering field expedition of British anthropology had been to the Torres Straits. Rivers, Haddon, Radcliffe-Brown and Malinowski himself had conducted field research largely in the Pacific. The Rockefeller fellows were now funded to undertake sociological studies in British African colonies. Malinowski and Oldham ruthlessly shut out the old guard. The Cambridge students of Haddon and Rivers, who had worked on kinship problems in the Pacific, were passed over. At Oxford, Marrett was starved of funds. Within the LSE, Seligman was side-lined, despite the fact that he was the sole African specialist at the School, and had worked closely with the government of the Anglo-Egyptian Sudan. Even after Malinowski left England in 1938 his most loyal followers were able to control the Colonial Social Science Research Council, which was founded in 1944. Firth became the secretary, seconded by Audrey Richards,

who had served as a temporary principal at the Colonial Office and worked with Hailey on post-war African policy before being appointed special lecturer in Colonial Studies at the LSE in 1944. (Mills, 2002.)

This reorientation of British social anthropology to focus on colonial policy was part of a general trend in the 1920s, as governments began to develop more comprehensive social programmes, creating a market for scientific advice. The social sciences redefined themselves as policy sciences (even if they might serve at times as sources of critical commentary). A historian of social science, Dorothy Ross, points to 'a movement toward modernist historical consciousness, the growing power of professional specialization, and the sharpening conception of scientific method', which together produced a 'slow paradigm shift in the social sciences … away from historico-evolutionary models … to specialized sciences focused on short-term processes.' (Ross, 1991: 388.)

Leach represented Malinowski's situation at the London School of Economics as a handicap. After all, Oxbridge was the base of the establishment. However, the LSE was one of the very few substantial centres for social science research in Europe in this period. Moreover, it had developed its own ideology of public service and social reform. Malinowski's inner circle, Firth, Richards and Mair, were working with Lord Hailey on proposals for colonial research, just at the moment that the welfare state in Britain was being planned by Beveridge, the director of the LSE (who married the mother of Lucy Mair, the secretary of the School).

By the early 1930s, Malinowski had effectively reinvented British social anthropology as the social science that addressed colonial policy. In practice, the Colonial Office was concerned above all with the government of African societies. (The India Office was a separate and much more prestigious institution.) It was the African colonial context which shaped the topics that were studied by the new generation of social anthropologists. But this did not dictate the stance taken by individual scholars, who could, and did, adopt a variety of intellectual and even political positions. In their public statements, Malinowski and Radcliffe-Brown tended to insist that there should be a division of labour. The anthropologist presented the facts, the colonial official decided what was to done. But both men broke this rule at times and wrote critical, occasionally intemperate, commentaries on aspects of colonial government in the 1920s and 1930s.

There was another option, however. This was to insist on the purity of scientific research. Evans-Pritchard and Radcliffe-Brown reacted to Malinowski's initiative by insisting that social anthropology should be a strictly academic pursuit. Evans-Pritchard started referring to the LSE in his correspondence as the '£.S.D', and in 1934 he wrote from Cairo to Meyer Fortes:

> The racket here is very amusing. It would be more so if it were not disastrous to anthropology. Everyone is advising government – Raymond [Firth], Forde, Audrey [Richards], Schapera. No one is doing any real anthropological work – all are clinging to the Colonial Office coach. This deplorable state of affairs is likely to go on, because it shows something deeper than making use of opportunities for helping anthropology. It shows an attitude of mind and is I think fundamentally a moral deterioration. These people will not see that there is an unbridgeable chasm between serious anthropology and Administration Welfare work. (Quoted in Goody, 1995: 73.)

De L'Estoile persuasively suggests that this purist discourse was later mobilised to promote the new institute of social anthropology at Oxford, which had been established in 1937 under Radcliffe-Brown. He and his younger associates, Evans-Pritchard and Fortes, set out to challenge the LSE. And in 1940, they published the classic statement of British Africanist anthropology in this period, *African Political Systems*. Edited by Fortes and Evans-Pritchard, and with a preface by Radcliffe-Brown, the book was presented at the time as a path-finding contribution to a new political science: political anthropology. This was not a mere smoke-screen. *African Political Systems* was among other things a transformation of the classical debates inaugurated by Henry Maine and Lewis Henry Morgan, and it perpetuated the Victorian two-stage model of kin-based societies and states. (Kuper, 1988: chapter 10.) The editors also touched on classical issues in British political theory. (See, e.g., Kuklick, 1984; de Zengotita, 1984.) But de L'Estoile argues persuasively that the book issued more immediately from the debates on indirect rule in the 1930s, which framed the issues and determined the way in which it was read in Britain at the time. *African Political Systems* addressed precisely the issues that arose within the context of indirect rule. Who were the leaders? What were the structures of administration?

Even more tellingly, the contributors accepted that the 'tribes' on which they reported were genuinely distinct societies, although, as African historians were soon to demonstrate, these so-called tribes were in very large part the product of indirect rule. But recent colonial history was written out of even the most innovative 'theoretical' ethnographies. Leach's *Political Systems of Highland Burma* (1954) challenged key features of the model that had been presented in *African Political Systems*. Leach argued that lineage systems and states were stages in one, often cyclical, process. And he represented the tribal units as unstable and intricately interconnected social fields, shaped by ambitious individuals. But like all the contributors to *African Political Systems*, with the notable exception of the South African, Max Gluckman, Leach neglected the impact of colonial overrule. Somewhat paradoxically, this was the price of abjuring 'applied' research.

While critics on the left condemn British social anthropologists for serving the Empire, functionalist

anthropology was criticised more commonly in the 1930s and 1940s for ignoring social change and failing to analyse the colonial context. Its defenders pointed out that on the contrary functionalists produced a number of studies of local government, migrant labour, land tenure, the work of the courts, and so on, and that there were several attempts at developing a theory of social change. However, such studies came to be considered as constituting a special category, termed 'applied' work. After World War II the academic purists stigmatised this sort of research as being less scientific and prestigious than what came to termed 'theoretical' studies. An ostentatious refusal to analyse the colonial context became the hallmark of theoretical contributions in social anthropology, because any acknowledgement of colonial realities would mean engaging directly, rather than implicitly, in debates on colonial policy. Yet *African Political Systems* unwittingly proved that the colonial context did in fact determine the units of study, the tribes. Colonial preoccupations also made certain topics seem naturally more important than others.

Not only were anthropologists drawn to the study of tribal political systems or law. Policies on religion, education, and the family were generally delegated by African colonial governments to missionary societies. In response, anthropologists made applied studies of Christian influences, the economics of bride-price, or the impact of migrant labour on family life. They also undertook 'theoretical studies' on the same institutional complexes, but these were ostentatiously pure in thought, dealing with initiation ceremonies, traditional religion, witchcraft, kinship taboos and lineage systems. No missionaries appeared in these 'theoretical' texts, except as straw men who took a moralising view of African practices.

IV

In the 1940s and 1950s, the Rhodes-Livingstone Institute under Godfrey Wilson, and later Max Gluckman, and the East African Institute of Social Research, under Audrey Richards, were mature embodiments of applied anthropology in Africa. They represented the apotheosis of Malinowski's project. But Africa was changing. As colonial policy-makers looked forward to the independence of African states, the anthropologists were sidelined. At the LSE Lucy Mair tried to turn applied anthropology into development studies, but applied studies were effectively extruded from departments of social anthropology in British universities after the African empire came to an end in the mid-1960s. Within the university departments, the purists won out.

At this critical juncture, the social anthropologists began to lose ground within the British universities. Britain's system of higher education entered a phase of rapid expansion in the 1960s, but social anthropology stagnated institutionally. Given the requisite political will on the part of the leading

anthropologists, it might have been possible to establish a number of new departments of social anthropology as the universities expanded and new ones were founded. A particular growth area was social science. Sociology became a popular subject and established itself in all the new universities. Social anthropology, however, remained a small elitist discipline, positioned most securely at Oxford, Cambridge and the LSE. Given Leach's thesis about the nature of British science, this might be regarded as a plus point, but it froze the institutional development of the discipline. By the 1970s, when Malinowski's students retired, there were about 150 academic social anthropologists in Britain, and the figure remained stable for another decade. Today, after a generation in which the universities have expanded enormously, there are 230 full-time academic positions in social anthropology in British universities. There are also a number of fixed-term appointments, many of them post-doctoral positions, and a few appointments in museums, which might bring the total to 300. (See Mills, 2003 (a): 22.)

As the institutional basis of the discipline within the universities stagnated, and even began to shrink in the 1980s, the decade of the 'cuts', the collective institutions of British social anthropology became bastions of conservatism, not to say reaction. (See Mills, 2003 (b).) The anthropology sub-committee of the Social Science Research Council, the Royal Anthropological Institute, the Association of Social Anthropologists, the anthropological section of the British Academy, all remained under the control of a few increasingly elderly professors. Raymond Firth was calling the shots in most of these institutions when he was well into his eighties, and his close ally Edmund Leach remained a key player until his death, although he had insisted in his Reith Lectures that in our 'runaway world' 'no one should be allowed to hold any kind of responsible administrative office once he has passed the age of 55'. (Leach, 1968.)

Having lost an empire, the social anthropologists found themselves struggling in this dismal institutional environment to find a role. And just at this moment they were challenged on their own turf, within the universities. Hamstrung by the sclerotic institutional structure of the profession, they were confronted by the rise of development studies and of sociology. The anthropologists naturally refused to be drawn into development studies, which moved into the space that had been vacated by the old colonial science. Third world development projects provided the infrastructure and ideological impetus for a fresh surge of social anthropology in Scandinavia and in the German-speaking countries, while in the Netherlands new departments of 'Non-Western sociology' split off from the old departments of ethnology in order to tap the generous funds being made available by the Dutch government for overseas 'development projects'. In Britain, however, any anthropologist who specialised in development studies would be unlikely to find encouragement, or employment, in departments of social anthropology. (See Grillo, 1985.)

The rise of sociology presented a more alarming challenge. At Oxford and Cambridge the professors were frankly terrified that their students would desert en masse to this more radical and more relevant social science. When at last the university established a chair of sociology at Cambridge, Meyer Fortes used his political skills to secure the appointment of a social anthropologist, John Barnes. Elsewhere, a few social anthropologists were placed as professors of sociology in the new departments. In Manchester Peter Worsely split acrimoniously from Max Gluckman to found a separate sociology department while Victor Turner, equally acrimoniously, turned away from the sociology of the Manchester school and took up the hermetic analysis of systems of ritual symbolism.

The problem presented by sociology went beyond competition for jobs and for students, or even for funds in the Social Science Research Council. The social anthropologists were obliged to think again about what kind of social science their discipline could claim to be. The general impulse was to redefine social anthropology in opposition to sociology. Sociology was about modern, industrial, western societies. Very well, social anthropology was defined as the science of the rest, the 'Other Cultures', even if these were now less often distinguished as 'primitive'. (There was a search for euphemisms, 'pre-literate' being popular for a time.)

The implication was that social anthropologists had no business doing research in Britain, a view that was deeply entrenched in the profession until the 1980s, and which remains the doctrine in some circles. In contrast to other European countries, there was no alternative tradition of V^lkskunde. The folklore movements of the 19th century had never established an academic base in English universities. However, Malinowski's Trobriand research had inspired ethnographic experiments at home. George Orwell's books on Wigan Pier and on being down and out in Paris and London followed in the Victorian footsteps of Christopher Mayhew, but equally they were Malinowskian experiments in participant observation. The anthropologist Tom Harrisson was instrumental in the launch of Mass Observation in 1937, which was an attempt create a popular ethnography of everyday life, carried out by amateur observers. In the 1950s came the Bethnall Green studies of Peter Wilmott and Michael Young. These men were all social reformers, attuned to the issues facing the politicians. Young had been secretary of the Labour Party's research department in the run-up to the general election of 1945, the election that opened the way for the implementation of Beveridge's welfare plans. But they also operated outside academia. As sociology consolidated its base within the universities, the leading sociologists pronounced that community studies were unreliable. Scientific research required large surveys and statistical evidence.

But nor did this tradition of urban, British ethnography attract the anthropologists. Evans-Pritchard denounced Malinowski as 'a bloody gas-bag' because he looked kindly on 'the mass-observation bilge'.

(Goody, 1995: 74.) Firth and Richards organised studies of British kinship in the 1950s and early 1960s, but they had little immediate impact on their colleagues. Few young anthropologists undertook field research in Britain in the 1960s and 1970s. As a research student in the early 1960s, I can testify that we were firmly given to understand that anthropologists should study foreigners, the more foreign the better. Young anthropologists did, however, begin to move into new areas. With the end of the African empire, India and Indonesia increasingly attracted ethnographers, and the Australian government was encouraging research in New Guinea. Anthropologists might even venture into Europe, but only to the periphery, looking for lineages in a Greek island, or dowry systems in Andalucia, or perhaps describing an isolated community in a windswept and very uncomfortable collection of rocks somewhere between Scotland, Norway and Iceland. I remember Edmund Leach returning from a holiday in Portugal in the mid-sixties and telling research students that he had seen peasants ploughing with bullocks. Someone should go out and study them. But anyone who insisted on doing fieldwork in Britain was liable to find themselves exiled to departments of sociology.

The leading anthropologists also stuck with the traditional subjects of 'pure' research: kinship and ritual. The advent of structuralism gave a fresh impetus to these fields of study, but anthropologists paid little attention to new intellectual movements in the other social sciences. And the methods of field research remained those associated with Malinowski, at least in the mythology.

And so, by and large, the British anthropologists beat a retreat in the face of sociology. They distanced themselves from issues of public interest. And increasingly they tended to redefine their project as the study of cultural variation. They chose to study isolated, traditional, if perhaps no longer 'primitive' societies, and even anthropologists working in societies undergoing revolutionary changes in India, China, Indonesia or Latin America typically concerned themselves exclusively with rituals or with kinship terminologies and rules of marriage. (LÈvi-Strauss, Dumont and Needham insisted that such studies should have nothing to do with the messy business of actual marriage choices).

Some influential figures began to argue that it had been a mistake to define social anthropology as a form of sociology in the first place. Evans-Pritchard asserted in a public lecture in 1950 that social anthropology was not a science in search of laws but 'a kind of historiography, and therefore ultimately of philosophy or art', and represented it as a complement to Oriental studies. (Evans-Pritchard [1950] 1962: 26.) He and Schapera now preferred to describe themselves as ethnographers. According to Godfrey Lienhardt, Evans-Pritchard's shift was an accommodation to the intellectual climate of Oxbridge, which was inhospitable to social science at the time (Lienhardt, 1974), but the problem was even more acute in less conservative universities, where sociology and the other social sciences flourished in the 1960s and

1970s.

Nor was this a problem only for British anthropology. Social anthropologists had to face up to the challenge of sociology in most European universities, and they either asserted their own social science credentials or drew in their skirts. In France, for instance, Georges Balandier advocated a colonial sociology in the mould of the Rhodes-Livingstone school. He was opposed by Claude LÈvi-Strauss, whose project was purist and idealist. (See Gaillard, 1990.) Some of Balandier's leading students experimented in the 1960s with a Marxist anthropology, but like the American Marxists around Julian Steward and Leslie White the younger members of the school also turned in the 1970s to a culturalist anthropology, associated in France with LÈvi-Strauss and Louis Dumont, who had taught at Oxford in the 1950s, been influenced by Evans-Pritchard, and become something of a a Parsonian. Like the American Parsonians Clifford Geertz and David Schneider, Dumont came to the conclusion that anthropologists should treat cultural systems and in particular systems of values as independent realities, without taking social processes into account.

V

As British social anthropologists fought their desperate rearguard action, they began to pay attention to developments in the USA. They had felt remote from the central scientific project of Boasian anthropology, which had spent two generations patiently assembling micro-histories of the North American Indians. They had scorned the culture and personality school of the 1930s. The four-field conception of anthropology had no prominent advocates on this side of the Atlantic after World War II, apart from Daryll Forde. But by the 1960s American anthropology was in the throes of radical change.

American anthropologists had been drawn into policy studies during World War II. This was not altogether a new development. The Boasians had addressed racism in the USA in important theoretical papers, and in popular books, although they were strangely reticent about conditions in the Indian reservations. But now anthropologists were given desks in Washington, D.C., and talked directly to administrators and politicians. Mead, Bateson and their associates drew on psychoanalysis and developmental psychology to produce profiles of enemies and allies for the benefit of government policy-makers and planners. George Peter Murdock served during the war as an officer in the US navy, and he masterminded the production of ethnographic guides to strategic Pacific islands, guides that were later to form the model for his World Ethnographic Survey. This generation of American anthropologists were not, however, interested in sociology, economics or political science. It was only when, in the aftermath of

victory, the USA was drawn into nation building in Japan, and, as the Cold War began, in the Philippines and Indonesia, that anthropologists started to collaborate with other social scientists. And now, for the first time, significant numbers began to specialise in societies beyond North America.

This entailed a redefinition of the project of cultural anthropology. At Harvard, Talcott Parsons and Clyde Kluckhohn envisaged the discipline changing its image and becoming a partner in a global social science project. Collaborating with sociologists and psychologists, anthropologists would become specialists in culture. But the culture in which they were to be the experts was not Tylor's culture. For Parsons, culture meant the ideological dimension of social life: the realm of ideas and values expressed in symbols. Anthropologists were to take culture, in this sense, as their specialism, not 'primitive peoples', and then they were to report back to the sociologists, who would synthesise an explanation of social action. (See Kuper, 1999 (a).) Meyer Fortes brought Parsons as a visiting professor of social theory in the Cambridge department of social anthropology for a year, and in 1963 Geertz, Schneider, Fallers, Sahlins and Eric Wolf were invited to present the new American fashions before a rather bemused British audience at a special decennial meeting of the ASA. (See Goody, 1995: 147.)

Clifford Geertz was in many ways the exemplary figure in this new generation. He was one of the early products of Parsons's new School of Social Relations at Harvard, and his work in Java was conceived of a part of a team effort, the anthropologists collaborating with economists and political scientists. On his return to the USA he worked with development economists at MIT (writing reports that were later elaborated and published as monographs: *Agricultural Involution* and *Peddlers and Princes*, both of which appeared in 1963). He spent the 1960s together with Lloyd Fallers as a member of the Committee for the Comparative Study of New Nations, at the University of Chicago, which was directed by a conservative ally of Parsons, Edward Shils. Other anthropologists were engaged in comparable projects in Latin America, some of which, it turned out, were bankrolled and perhaps directed by the CIA.

However, the moment of neo-imperial anthropology passed quickly. There was a scandal over the use of anthropologists and other social scientists by American intelligence in Operation Camelot in Chile in the early 1960s, but the great divide was, of course, the Vietnam War. One of the casualties of the radicalisation of the campus in the 1960s was the Parsonian project. At the end of the decade, Geertz moved from Chicago's troubled campus to the mandarin calm of the Institute of Advanced Study at Princeton. Here he began to redefine anthropology as an autonomous discipline. Its subject-matter was still culture, in Parsons' sense, the realm of ideas, values and symbols, but its future lay not with sociology but with the humanities. Cultural anthropology was to be a study of texts in action, its aim not explanation but the explication of meaning. In 1973 Geertz welcomed 'an enormous increase in interest, not only in

anthropology, but in social studies generally, in the role of symbolic forms inhuman life. Meaning … has now come back into the heart of our discipline.' (Geertz, 1973: 29.) And his essays placed anthropology within a new configuration of disciplines, linking up particularly with literary theory and linguistic philosophy.

The established alternative tradition in American anthropology, a form of evolutionism often linked with Marxism, had attracted some young radicals, and produced its new stars, Marshall Sahlins, Roy Rappaport and Eric Wolf. But Sahlins was also converted at the end of the 1960s to a culturalist position. Even Rappaport began to recast his ecological determinism to give it a culturalist edge. The new generation of American anthropologists that graduated in the 1960s and 1970s was effectively formed in an anthropology that defined its object as the realm of values, ideas, and symbols. Its members had been caught up in the campus radicalism of the time, and they remained politically engaged, but from now on radical politics on campus increasingly meant identity politics. Cultural relativism became the common orthodoxy. A number of British social anthropologists took the American line in the 1980s and 1990s, becoming culturalists, embracing cultural studies, endorsing an extreme relativism, even describing themselves as post-modernists.

VI

Practitioners notoriously write intellectual history in terms of advances or wrong turnings, where perhaps there was rather a series of engagements, or of rejections, a turning of backs. We must ask not only what anthropologists talk about, but who they talk to, and who they snub. In general hardly surprisingly, anthropologists have produced their most influential and perhaps most interesting work when they were drawn into large debates. These debates were not only about public policy. Boas and Malinowski asked questions about the application of Freudian theory in settings far from Vienna or New York; Evans-Pritchard's study of Zande magic took up classic issues of rationality and attracted the interest of philosophers; Margaret Mead revolutionised American ideas about education and adolescence by introducing comparisons with societies in the South Pacific; LÈvi-Strauss applied models drawn from linguistics to classical ethnographic materials; and, most recently, anthropologists have experimented with cognitive psychology. Political issues have also stimulated theoretical debates, drawing anthropologists into interchanges that have transformed their subject-matter, their ethnographic focus, and their ways of thinking.

It is often misleading to separate political impulses and theoretical commitments. Recent research programmes that are at once broadly political and also theoretically driven have addressed the issue of gender; the mass media, and more generally material culture and museums; bio-medicine and infectious diseases in the South or new reproductive technologies in the North; ecological problems in the tropics; immigration; the indigenous peoples movement, and other issues relevant to current debates on universal human rights. Beyond Europe and North America, notably in Mexico, Brazil, Peru, India, Japan, China, Indonesia, and South Africa, anthropologists are typically engaged in debates about pressing issues of national identity and development, engaging with specialists from other disciplines as well as administrators and politicians. This is perhaps the most obvious reason for the vitality of social anthropology in some of these countries.

The commingling of political commitment and theoretical orientation can be a recipe for politically correct but intellectually suspect work. Indeed there is a very real danger today that (as Evie Plaice has remarked (Plaice, 2003: 397)) 'anthropology is becoming the intellectual wing of the indigenous peoples movement'. This is evident in Australia and Canada, though much less so, for example, in Brazil, even among the Amazonianists. These tensions between moral commitments and intellectual independence are inevitable when social scientists address the important questions of the day. It is sometimes easy to forget that there are plenty of advocates and NGOs, and few reliable ethnographers who can present subtle analyses of particular local historical processes, conflicting forces, competing interests. On the other hand, some influential schools of contemporary anthropology have refused such engagements with public policy and have turned in on themselves, insisting that true anthropology concerns itself with the symbolic behaviour of faraway peoples.

Yet whatever the response, turning outwards or inwards, there has been a recurrent feeling that anthropologists had something distinctive to say, if only they could be sure what it was. Worrying about this question, anthropologists commonly appeal to the magic of their methods, and denounce the debasement of ethnography in cultural studies, science studies, or urban sociology. More positively, they may reposition themselves within one of the enduring anthropological paradigms that can be traced, not unchanging but remarkably stable, through a century of transformations. The discipline has its own distinctive DNA. Each gene has its prescribed place, and also its particular code word: evolution, culture or society. And each finds its fullest expression in a different disciplinary environment. It may be conceived of as a branch of biology, a discipline at home within the humanities, or as a social science.

European social anthropology traditionally conceived of itself as a social science. In the 1970s, American anthropology became polarised between a hardline biological determinism, most powerfully

manifested in sociobiology, and Geertz's paradigm of a humanistic study in pursuit of 'meaning'. Geertz's revolution was aimed against both the four-fields scientific anthropology of the old school and against the sociological anthropology of the Parsonians, to which he had once himself adhered. In his recent memoir *After the Fact*, Geertz reflect that 'the move towards meaning' had 'proved a proper revolution: sweeping, durable, turbulent, and consequential'. (Geertz, 1995: 115) In the past two decades, it has certainly been the dominant influence in American anthropology.

American influence came to predominate in all the sciences and social sciences after the War, and anthropology was no exception. The disarray of European social anthropology in the 1970s and 1980s made Americanisation more palatable, even attractive. Although American anthropologists liked to cite French philosophers, all the innovations in anthropology from the late 1960s crossed the Atlantic from west to east: the study of gender, the body, medical anthropology, the revamped field of material culture studies, the anthropology of film. And more generally, western European anthropologists absorbed the American discourse on culture. In the 1990s the post-modernist version of interpretive anthropology – which Marcus and Fischer defined as 'nothing other than relativism, rearmed and strengthened for an era of intellectual ferment' (Marcus and Fischer, 1986: 33) – seemed set for a while to sweep the board on both sides of the Atlantic. For a time it appeared that even in its heartlands, social anthropology was barely resisting translation into cultural studies. This wave has passed, but many anthropologists patently still feel that they are adrift. Will they redefine themselves once more as social scientists? Some of my senior colleagues in British social anthropology wonder whether we should not recreate a four-fields anthropology in order to rebuild our identities.

To be sure, the social science tradition in European social anthropology survived the cargo cults of the 1980s, and even flourished. But it flourished very largely outside social anthropology. Its leading figures were increasingly influential in wider debates on social theory, but at the same time they became marginal to the narrowing limits of the discipline. In France, Pierre Bourdieu tried to recast kinship studies in terms of a sociology of action, but then he chose to make his career in sociology. Mary Douglas became a pioneer of risk analysis, and won international recognition in this growing field, but few anthropologists paid any attention. Frederik Barth provided the inspiration for *microhistoria*, but found himself out of step with younger anthropologists. Ernest Gellner spent most of his career in departments of philosophy and social theory. After his retirement from his Cambridge chair, Jack Goody resigned from the social anthropology section of the British Academy and joined the sociology section.

VII

This was the context in which the EASA was founded in January, 1989. Social anthropologists came together from all over Europe and agreed that they faced similar dilemmas, confronted the same pressures, faced common choices. We set out to establish a new institutional space, and we situated our project within the European social science tradition.

How are we doing? Rereading the papers published in the first decade of our journal *Social Anthropology*, I am struck most particularly by the modesty and eclecticism of the arguments that are presented. The grand theories of the previous decades are seldom invoked, yet the authors have read widely and reflectively, and they refer as a matter of course to sociologists, historians or psychologists. Their arguments are closely tied to detailed ethnographic observations. These ethnographic descriptions themselves present a remarkable though taken-for-granted contrast to the images presented in the literature of the previous generation. Few papers describe apparently isolated, bounded, traditional, monocultural societies. Rather, even the most exotic communities are presented as part of the wider world, the site of intellectual and political cross-currents, internally riven by conflict and echoing to debates and dissension. Nor are their inhabitants mysteriously, or enchantingly, 'other'. Magic and religion often appear to be no less pragmatic than bio-medicine. Adepts of strange cults turn out to be no less reasonable than ourselves. In order to make sense of their world, even the most conservative and apparently isolated people appeal to shifting frames of reference. Nor is this all taken as a sign of modernity, or a marker of uncomfortable and ill-comprehended change, but rather as the normal state of things, everywhere, at all times. Ethnographic reports of European societies are quite often reassuringly similar to ethnographies of tropical peoples.

A recent special issue of the French intellectual review *Critique*, written largely by young French anthropologists, addressing questions of theory, gives a similar impression. (De L'Estoile and Naepels, eds., 2004.) Normal science, or at least a coherent and recognisable social anthropology, with a distinctive European character, is alive and well, and certainly in better shape than it was when we first planned the EASA. Perhaps we can claim some of the credit. But I suspect that there is still too much faith in the magic of ethnography, that not enough time is spent on following the big debates in the social sciences, and that too little serious research is directed to the public issues which absorb European intellectuals. There is a popular slogan, that we should make the unfamiliar familiar, the familiar strange. That can all too easily become just another way of saying that we are peddling exotica. If we accept this role as the specialists in strange things we condemn ourselves to the margins, and concede that anthropology must be at best the science of the marginal.

And finally, there is surely too little investment in comparison. This is the perspective that

anthropology is best placed to contribute to social science debates, and it is the most fruitful method with which to probe theories and generalisations advanced not only within anthropology but also by sociologists, political scientists and psychologists. But comparative studies are unfashionable – another casualty of the culture wars, perhaps. And in consequence anthropologists have been paying too little attention to the methodological and theoretical issues that are raised by any comparative project. (See Gingrich and Fox, 2002, for a promising fresh start.)

Towards the end of his life, Malinowski sketched the outline of a textbook for social anthropology, which he never accomplished. His theory of functionalism, he wrote, stressed 'sameness' over difference, and he urged students to dig beneath 'what appears on the surface'.

> I saw [in anthropology] a greater interest [in] diversities & I recognised their study as important, but underlying sameness I thought of greater importance & rather neglected … I still believe that [the] fundamental [is] more important than the freakish. (Young, 2004: 76.)

This does not necessarily mean that, like Malinowski, we should seek 'laws and regularities', although it would be nice to find them. But we should try to extend the range of the social sciences by testing them in other conditions, bringing back home an appreciation of social processes and views of the world that are ignored by the ethnocentric sociologists, political scientists and psychologists. For we must not only listen to other social scientists, we must talk back to them, introducing into their debates the models we have learnt in our interactions with all sorts of people around the world. And we must not just grumble about public policies on immigration and multiculturalism, or turn up our noses at projects of development. We must address them.

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