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978-0-521-45666-1 - The Expansive Moment: The Rise of Social Anthropology in Britain and Africa 1918-1970

Jack Goody

Excerpt

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Introduction

Why would one want to read or write a book about a small group of academics, whose fate might interest only practitioners in the same field? Partly because it illustrates the growth and expansion of an area of enquiry and University teaching and research which has had some influence on neighbouring fields. Partly because the accounts which are emerging of that group seem to overlook some crucial aspects of the social and intellectual situation of the 1930s and post-war period. Following an approach current in much intellectual, sociological and cultural history, they assume a homogeneity of interest between intellectuals and the government which neglects internal contradictions and oppositions. Equally intellectual positions were more differentiated, more argued about than such approaches allow, often holding within themselves the clues to the next development. That seems true not only within the group of scholars involved but also of their sponsors, whether these were foundations or governments. In looking at this situation I hope to throw light on the discussion about the relationships of these scholars to the powers that be, which I see as much more differentiated, less homogeneous, than some recent commentators.¹

As a professional field of enquiry, social anthropology emerged at the beginning of the twentieth century, though it had many earlier progenitors. This book presents an account of the period when this subject came of age in Britain, largely under the auspices of Bronislaw Malinowski, the Polish professor teaching at the London School of Economics (LSE). There he attracted a group of scholars who had already had considerable experience in other fields, who came to form the core around which the major teaching departments in the country were formed, and who founded a tight professional organisation of trained, research-orientated personnel, the Association of Social Anthropologists (ASA), in 1946. Their research consisted mainly of

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work done in British territories in Africa during the late 1920s and the 1930s, carried out under the auspices of the International African Institute which published much of their results. Although Malinowski was not an Africanist, his personal reputation for fieldwork based on observation and his association with the LSE enabled that Institute to attract money from a philanthropic body in the United States which was interested in the contemporary situation in Africa. This was the Laura Spelman Rockefeller Memorial which contributed significantly to the growth of the social sciences in Britain and had selected the LSE as a centre of excellence worthy of support as early as 1924, at which time it developed a fellowship programme in North America through the newly formed Social Science Research Council (SSRC) – which it had encouraged and financed – as well as in Europe and later Australia.²

This then is an account of the interactions of a group of scholars, with many of whom I later worked, and of the research they undertook. Inevitably it concentrates upon my own teachers, Meyer Fortes and E. E. Evans-Pritchard, and in particular on contributions made by research in West Africa where I did my own fieldwork. At the end it becomes frankly autobiographical.

How did I get into this enquiry?

The unfolding of an anthropologist's career, like that of many an academic, can be described in terms of the progression: from the seminar, to the field, to the study and to the class, although one phase does not replace another. One attends seminars as a graduate student and again later on, but in a different capacity and with different intentions, when other professional activities – writing-up or teaching – dominate one's life. For many, probably most, possibly all, academics, writing and teaching themselves gradually take a less important place than administration and politics, that is, than the participation in committees, than the round of learned societies, than the support of one candidate or one issue as against another, and than the multitude of other tasks that tend to overwhelm the pursuits that are normally identified with the profession. Distracting as these are, they form the institutional setting of teaching and research in the context of which takes place a continual hunt for funds and personnel. It is this institutional context of intellectual activity which comes out so clearly in the minutes of meetings and, increasingly over time, in the personal letters of the teachers themselves. It is the relationship between this body of material and their contributions to knowledge in the shape of

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reviews, papers and books, that lies at the centre of my interest. What I have to offer consists of two parts. The first is an account of the emergence of an academic discipline in this country, largely from the perspective of one of the participants, Meyer Fortes, and to a lesser extent, Evans-Pritchard. The second part is an attempt arising out of an invitation from Claude Tardits to lecture on the subject of 'What did we do?' to a seminar on African anthropology at the 5^{ème} Section de l'Ecole Pratique des Hautes Etudes in Paris in the spring of 1988.

It is often claimed that British social anthropology was the child of colonialism. Its work was carried out mainly within the colonial empire – Malinowski in New Guinea, Radcliffe-Brown in the Andamans, Firth in New Zealand, Tikopia and Malaysia, Evans-Pritchard in the Sudan, Fortes in the Gold Coast, Richards, Gluckman and Schapera in British East and South African territories, Leach in Burma. But the further implication of the claim is that since research had to be carried with the approval and support of these regimes, it influenced both its empirical direction and its theoretical underpinning.³

By examining a selection of documents, I want to suggest that these implications need modifying, firstly with regard to the financial base, secondly with regard to the approval of colonial governments, and finally with regard to the supposed homogeneity of the empirical and theoretical approaches. On the individual level, the participants came from differing national, social and ideological backgrounds, though many were influenced by contemporary interests in the ideas of Marx, the rise of Fascism in Europe and the ending of colonial empires. At a social level, the standard claims fail to take account of the contradictions, the conflicts and the diversity of perspective that one finds in any social formation, and specifically in the relations between research worker and the support agencies (whether government or not), and the role of human agency in interpreting, enacting or generating these conflicts. Anthropologists of the 1930s understood this well and they knew, as some have told me, that the situation was yet more complicated than the documents reveal.⁴ Nevertheless it seemed to me worthwhile to sketch out the position as it appeared to me from what I had read, even if it confirmed earlier worries about the conclusions one can draw from such textual material.

I have to add that I did not deliberately intend to get involved in this subject at all. In other words, my interest in anthropology has not been in the history of the discipline as such, although I have touched upon the development of some specific intellectual topics. In other words, my

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interest in history has usually had to do with some problem I was working on, an enterprise in which boundaries have to be breached rather than defined. How then did I come to write these pages?

As a member of the post-war generation of anthropologists, I did not know the two founders of the subject in Britain, A. R. Radcliffe-Brown and B. Malinowski, though I once met the former, and attended his very anthropological funeral; like most funerals of academics, and unlike their weddings and certainly their births, it consisted of colleagues rather than kin. But I did become acquainted with their pupils and colleagues, with Alison Davis, Evans-Pritchard, Firth, Forde, Hogbin, Kaberry, Mair, Mead, Talcott Parsons, Richards, Schapera, Srinivas, Stanner, Lloyd Warner, and with Fortes and Leach I worked closely over many years.

The historical background of social anthropology in early twentieth-century Britain may appear to be a well-worn topic discussed in the works of Jarvie (1964), A. Kuper (1973), Langham (1981), Lombard (1972), Stocking (1983, 1984, 1985) and Kuklick (1991) on the general issues, and of Douglas (1980) on Evans-Pritchard, of Firth (1957) and Panoff (1972) on Malinowski, and of others on particular aspects. I myself took up the topic partly to share the results of some archival research which I undertook when I became interested in a personal file on Fortes in the Ghana archives which related to the difficulties he had in getting to the field and the view taken of his proposal by senior colonial officers; it had been destroyed by the shredders in London, and Fortes told me he did not want to look at it. After his death I was asked to write an extended obituary for the *Proceedings of the British Academy*. I spent some time in the archives of the London School of Economics (before realising that Stocking had looked at many of these) as well as those of the International African Institute since I wanted to produce something substantial, given that these obituaries constitute one of the fullest sources that we have of immediate biographical and bibliographic data, and in some cases of intellectual achievement, especially those on A. R. Radcliffe-Brown and M. Gluckman by R. Firth, C. D. Forde by M. Fortes and E. E. Evans-Pritchard by J. A. Barnes.

For the obituary of Fortes, then, I decided to look at some of the archival materials, including the Public Record Office, and I had access to some of the papers of Meyer Fortes. These pages are a by-product of those enquiries but they originally took shape when I became dissatisfied with some comments on the conservative nature of

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an undifferentiated British 'structural-functional' anthropology with regard to colonial rule. These came mainly from Americans and Russians whose anthropologists have rarely given much effective support to self-government for the aboriginal peoples of their own immense, internal, empires, contenting themselves with limited ameliorative measures a good deal less radical than those proposed by 'colonial' anthropologists, many of whom supported freedom movements.

But I think of the result as 'notes towards' or a 'personal account' because there are other things I want to write, rather than a full-blown history. Only certain lines are followed up; I am aware there are other documentary sources, close to me, that I have not used. I am also aware of the limitations of the material. While my own background knowledge provides me with anecdotes of the past and residues in the present, I have concentrated upon the documents at my disposal. The results make one realise the limitations of a history based upon written materials, especially letters to close friends. I do not believe one can reconstruct Malinowski's persona from his letters, which express extreme attitudes to Reds and Jews, for example. Yet his behaviour to the outside world regarding Fortes and Kirchhoff, who were both, was exemplary. Equally Evans-Pritchard's letters to Fortes contain the most intolerant, reactionary statements and while he was never a political progressive in any sense, his life was marked by close friendships with two left-leaning Jews. The authors of the letters seem at times to be deliberately striking an extreme attitude as a kind of *épouwantail*, a sick joke, a shocker, to which the other is meant to respond, 'Oh B. M.!', 'Oh E. P.!'

In looking for materials, I have had much help from individuals and institutions, especially the International African Institute. But the attitude of universities and their members towards their own archives worries me. A considerable proportion of their members gain their living by archival research. Through the Public Record Office the Government provides them with excellent facilities to examine most records after a period of thirty years. When I went to the London School of Economics and asked the then Director if I could see the administrative records dating from over fifty years before, I was not allowed direct access, which seems to be one of the perks of the job and provides the basic data for their own histories of the School. I returned to my own university and made a similar request to see the archives, and was told, 'As it is you, there is no problem.' But there is one. Even

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if universities were not effectively public institutions, they have a collective interest in promoting 'openness', glasnost as we have come to call it, freedom of information. Large numbers of academics live on bits of paper written by other people. There is no excuse for not making records available on the same basis as the government does, that is, thirty years after they have been written, and I hope such access will be a future condition of grants that are made to the universities by the central authorities.

I have wondered about the propriety of using personal correspondence, especially of people I have known, since it seemed like a breach of confidence. Some of this is distasteful enough to lead some readers to want to leave it out. But I have used nothing, except for a few personal observations of my own, that does not appear in a public archive, nothing which is not available, now or in the near future, to other students. So it would be a mistake to bowdlerise their contents by selecting some extracts and deliberately avoiding others, although I do not exclude the possibility of unconscious selection. What I have done is to try and place such remarks in a wider context of understanding, the *verstehen* of the anthropologist. In any case I have not been concerned with aspects of their personal life except in so far as I considered that this affected 'the history of social anthropology'. By this I mean not only the intellectual history but their relations with organisations and colleagues, as these influenced the course of events and the situation in which I and others found ourselves. For in many ways the reader can regard this as 'une recherche du temps perdu'.

I have left untapped a large number of records of this period, partly because they were not always on open shelf, partly because I had limited intentions. But the documents are many. Some have been collected through the efforts of Raymond Firth who in 1974 wrote round to persuade his colleagues of that period to deposit their papers at the London School of Economics. That library contains those of Malinowski, Firth, Richards and Nadel. The Fortes papers are in the University Library at Cambridge. Radcliffe-Brown and Evans-Pritchard appear to have left nothing, or rather to have destroyed all that was in their hands.

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CHAPTER I

The economic and organisational basis of British social anthropology in its formative period, 1930–1939: social reform in the colonies

The role of foundations in the social sciences and parallel activities has been the subject of considerable debate. How far were they the tools of capitalism? Were the social policies of the Rockefeller Foundation ‘essential cogs in the production and reproduction of cultural hegemony’?¹ The Gramscian argument, espoused by Fisher, places its emphasis on the ‘critical-conflict’ perspective in the process of knowledge change in which causes are sought in the economy, class, ideology and hegemony. This perspective succeeds in giving to an interesting and informative paper a top-heavy superstructure which does not do justice to the subtlety of the situation, nor yet to Fisher’s own analysis. At that level the interpretation has come under attack from Bulmer (1984) and from Karl and Katz (1987). For the attempt to see the social policies as cogs in the reproduction of cultural hegemony overplays the extent of interlocking of family and foundation on the one hand and underestimates the degree of autonomy of structures and actors on the other. As this case history shows there was more conflict, contradiction, disagreement and independence than is often allowed. That conclusion seems even more true of the argument that British social anthropologists as a whole were ‘tools of colonialism’ since both their interests and those of the foundation that funded them (not to speak of a large part of the British population) rarely coincided with those of colonial governments.

Anthropology, ‘the study of man’, is a term that goes back to Aristotle and has usually meant the study of the other, the ‘primitive’ other at that. Its beginnings are lost in the interest that any people has about its neighbours but it emerged as a specific field only in the post-Darwinian era when it was closely associated with the study of the early, undocumented past of mankind (prehistory) and with its physical constitution (physical anthropology, human biology). It became established as a separate field only in the latter part of the century. Sir

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Edward Tylor, author of *Primitive Culture* (1871), first lectured in anthropology in Oxford in 1883 when he was appointed keeper of the University Museum. In the following year, the Pitt Rivers Museum was founded and Tylor was made Reader in Anthropology, with responsibility for lecturing on subjects held in the museum. He later became the first Professor of Anthropology in Britain and in 1905 a Diploma in Anthropology was established, the first course offered in a British university. Social anthropology itself was singled out by the appointment of R. R. Marrett as Reader in 1910 and a department was set up in 1914.

The other great name associated with that of Tylor was Sir James Frazer of Cambridge, author of *The Golden Bough* (1892), who was appointed as first Professor of Social Anthropology at Liverpool in 1907 but remained there for only one term. Teaching was developed in Cambridge and London, largely for the purpose of training cadets for the Colonial Service. In 1904 the University of Cambridge established a Board of Anthropological Studies which was to offer teaching in 'prehistoric and historic anthropology, ethnology (including sociology and comparative religion), physical anthropology, and psychological anthropology'.² However, a vigorous graduate programme was established only after the First World War under Bronislaw Malinowski at the London School of Economics. That was where most of the action took place during the thirties, not at the ancient universities. His intellectual status, his energy, his enthusiasm and his ability to raise funds resulted in a great expansion of field research. He was able to do this because of his connection with the London School of Economics and through them with the Laura Spelman Rockefeller Memorial which provided the large bulk of the funds.

From the 1930s to the 1960s social anthropology in Britain was marked by a commitment to periods of long and intensive fieldwork, much of it carried out in Africa. Even when the investigator was interested in one aspect more than others, the object was to grasp how social action (behaviour and norms) interacted, how to comprehend the existing community or 'society' as a whole. As such it tended to set aside the comparative and historical dimensions and to focus on the ethnographic present.

That orientation arose partly from a dissatisfaction with the results of earlier anthropology, which was now seen as engaging in doubtful reconstructions of the past and in the global comparison based upon inadequate ethnographic fieldwork. But it also coincided with a

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massive increase of funding that enabled research workers to be trained and to make intensive enquiries, as well as the arrival on the scene of highly motivated scholars. Under the inspiration of the Polish anthropologist, Bronislaw Malinowski, they formed a small group that experienced the usual friendships and enmities, but which on the intellectual level gave consideration to a number of related problems which they managed to advance and clarify by reference to field observations and to social theory.

On a theoretical level their orientation turned around the notion of 'functionalism' which some rejected or modified in favour of a more 'structural' approach. Both those approaches had their origin in the work of nineteenth-century reformers, Comte and Spencer and above all in that of the French socialist thinker, Emile Durkheim. Radcliffe-Brown had lectured on sociology in Cambridge in 1908 (as the classicist Jane Harrison attests); Malinowski encouraged his graduate students to read *Année sociologique*.³ That influence represented the important shift in British anthropology. But these new anthropologists were more concerned with concrete problems of matrilineal systems, lineage organisation, the developmental cycle, witchcraft, the nature and settlement of disputes and so forth. Current American interpretations of the development of anthropology sometimes find this approach consistent with the interests of colonial authorities, of whom the anthropologists were their pensioners if not their prisoners, even their unconscious mouthpieces. My reading of the evidence derived from archives and from personal contacts suggests that such a view needs to be heavily qualified. Firstly it neglects the motivation, origin and background of most anthropologists (many from overseas), as well as the leftward, sometimes Marxist, leanings of university life at that time, especially at the London School of Economics where Malinowski taught. Secondly, it overlooks the fact that the major source of funding lay in an American foundation with reformist rather than imperial interests. Thirdly, these two factors heightened the suspicions of administrators in the colonies towards academic research, even if those in London were more open to enquiry in the social sciences.

In the period after the Second World War, the sources of funds shifted and much research was then undertaken under the auspices of the newly founded Colonial Social Science Research Council, at a period when India and Burma had already achieved independence and when that goal had appeared on the horizon of colonial dependencies throughout the world, in other words in the twilight of colonial rule.

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The funds were distributed by a body consisting largely of academics who were under little or no pressure from the authorities, according to priorities sketched out in the reports of senior anthropologists. The recipients of the grants included students from the USA, the Indian subcontinent, from China as well as from Britain and other Commonwealth countries. The subsequent spread of the teaching of anthropology in universities gradually weakened the small-group atmosphere generated by these scholars, who had spent their earlier years in research rather than in undergraduate teaching. A diversity of interests in more historical and comparative topics (including the comparative, symbolic studies of Lévi-Strauss) began to manifest themselves. New perspectives were opened up in the work of those who constituted the third and fourth generations. But at the same time something was lost when the field became so diffuse, when the audience consisted of undergraduates rather than colleagues, when the focus on common problems (as distinct from philosophical trends) tended to disappear. That is the background to the story I present in the chapters that follow. My account dwells primarily on Africa. That is not simply the result of a personal quirk: it was the continent where the bulk of the research took place, partly because of the earlier grants by the Rockefeller philanthropies to the International African Institute and the later ones by the Colonial Social Science Research Council. If I concentrate unashamedly on work in that continent, I do not mean to underrate the importance of the research carried out in India (especially by M. N. Srinivas), in Burma (by E. R. Leach), in the Pacific (by Firth, Fortune and others) and in other parts of the globe by anthropologists associated with the British 'school'.

The development of social anthropology in Britain obviously had much to do with the position of the country as a colonial power, as was the case in Russia, in the USA and in France. In that country professional field research was of little significance until the thirties when funds became available to train and finance anthropologists. But those funds came largely from outside, as did the anthropologists themselves. Neither the givers nor the bulk of the recipients were primarily interested in propping up colonial empires. For Africa, the Colonial Office was concerned with the problems of ruling a large empire. While, unlike India, the movement to independence in that continent did not achieve much momentum until after the Second World War, with its promises of a new dispensation, many politicians, backed by a significant segment of the population, were interested not